

# Piltdown 3

## Further Discussion of The Ram Khamhaeng Inscription

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This paper was originally presented in October 1991, at a panel on the Ram Khamhaeng Inscription for the International Conference on Sino-Tibetan Languages and Linguistics in Chiang Mai. Some changes have been made to take account of comments by participants at that conference and of other published work which has subsequently come to my attention.

The first part of this paper contains answers to the responses or criticisms which have been elicited by my "Piltdown Papers", 1 and 2.<sup>1</sup> Some of these answers involve presentation of new material, and this forms the second part of the paper, to the extent that the two parts may be separated. The nature of the material involves some overlap. The third part is concerned with the origin of Thai writing systems. Some of it was presented orally together with "Piltdown" 1 in Canberra, but it cannot be fully understood nor criticised until presented in written form.

There are certain questions and criticism, which I shall not attempt to answer, and which I think are unanswerable, not because they are weighty, but because they are outside the realm of scientific discourse within which historians and linguists must work.

For example, I shall make no attempt to counter arguments of the type, "why couldn't a great genius, such as 'Ram Khamhaeng' devise from nothing a perfect writing system?" This question is unanswerable. We cannot say in a scientifically provable way that a great genius could not have done that, but all we know about the development of such cultural items suggests that if not impossible, it is extremely improbable.

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Though JSS has a policy of following the Chicago Style Manual, that manual of style has limitations when applied to philological work; therefore, this policy has been disregarded for the present article; only minor editorial changes from the author's manuscript have been made (ed.).

Writing in general must be assumed to have evolved because of perceived needs to record information, which presupposes a certain level of development in society, and it was preceded by other means of recording, pictures, mechanical devices like the Inca *quipu*, etc. In the entire history of humanity there are perhaps only three or four known independent inventions of writing; all of the current scripts of Europe, the Near East / West Asia, and Asia, except those of the Sinitic type, are considered to have been derived from a single origin.

Changes in script have occurred when an existing script was adapted to a new language, in which there were more or fewer, or different, phonemes, or when change within a single language made certain conventions obsolete (inaccurate with respect to the spoken language—this is the position of contemporary Thai and English). When a script moves to a new language, new features may be added for vowels, consonants, or other phonological features which did not exist in the source language, or conversely, script features may disappear if the borrowing language does not have use for them.

Both types of change can be demonstrated in the languages of Southeast Asia as scripts from India spread to languages of different types, and hypotheses about script origin and change must be based on such materialist considerations, not on what some king or great sage may have thought.

I shall also ignore, unless they are important in other respects, such questions as “If RK was faked in Bangkok, why did it not include?”, such as details from Nañ *Nabhamās*, or other features, or vocabulary items known to Bangkok literati.<sup>2</sup> We cannot know why the writers of RK, at whatever date, did not write something, and we must devote our study to what they did write.

All questions or suggestions based on assumptions of what someone might have thought in the past will be ignored, because we cannot know anything about such past thoughts, and attempts to speculate about them in historical reconstruction inevitably lead to results which cannot be distinguished from historical fiction.<sup>3</sup> It is nevertheless difficult to avoid some consideration of what the writers of RK, if it is a late composition, believed they were doing.

I shall also, unless I consider them substantively important, not answer criticisms of attempts to revise the history of RK which are not related to what I have said about it, or which seem designed to distract readers from the real controversies, or which demonstrate mere denigration without attention to what I or someone else actually said or wrote.<sup>4</sup>

Among the distractions I must mention some remarks in Anthony Diller’s “Consonant Mergers—A Closer Look”.<sup>5</sup> It is unfortunate that Diller, whose work may be singled out among the upholders of RK authenticity as including the highest quality criticism of my own, and which has stimulated much rethinking and improvement in my own work, chose to preface his study of

consonant mergers with a number of statements which are strictly red herring obfuscations, or straw men. Thus, no one among those of us trying to revise the status of RK has ever tried to argue that “those responsible for these ‘traditional readings’ of Inscription One were somehow influenced externally by White Tai or by a similar dialect”, or that, in the 19th century, there was “interest ... in the intricacies of the comparative method as applied to the Tai languages”, or that putative 19th-century writers of No. 1 were interested in “serious comparative or descriptive study of remote and ‘uncouth’ local dialects like White Tai, with a view to elucidating anything in the Central Thai language”, or that “King Mongkut and his associates ... had any interest at all in details of the Proto-Southwestern-Tai ‘etymological’ distribution of kho’khuat” (๓).<sup>6</sup>

As I shall try to demonstrate more clearly than in my earlier “Piltdown Papers”, the writers of No. 1, at whatever time it was written, and I believe the evidence points most probably to the late 18th or early 19th century, simply believed that what they were writing was correct in terms of other documents with which they were familiar. They believed they had done careful research, and they were trying to record what they believed to be true history in the form of an imitation of an ancient document. Of course they were influenced by what they believed to be correct or normal Thai practice, and they may have had a propaganda purpose in giving ancient authority to a new type of script with all characters on the line. There is no question that they “‘rigged’ the ‘traditional readings’ of Inscription One to conform to the comparative evidence represented by the White Tai correspondences”,<sup>7</sup> and I fully agree with Diller that they were not concerned with White Tai–RK–Bangkok comparison at all.

Nevertheless, we may assume that, among the Bangkok literati of the third and fourth reigns, there were persons familiar with White Tai, Black Tai, Lao, the Lanna dialects, and Shan, for since the reign of Taksin, at least, Bangkok had been deeply concerned with those regions, at times trying to conquer them, and at other times trying to influence and control local politics. It is even more certain that a knowledge of Khmer was rather common in those circles.<sup>8</sup>

One more general comment. Both Dr. Piriya Krairiksh and I have questioned whether certain terms in RK represent genuine Sukhothai language and practice. Defenders of RK have shown that some of these terms are found in various old Ayutthayan literary sources. This proves nothing. Those Ayutthayan works were familiar to early Bangkok literati, and could in that way have been used in composing RK. The important comparison is of RK with other Sukhothai inscriptions, to discover usage differing from the main Sukhothai corpus. Also significant are details which are not Ayutthayan, but seem to be 18th or 19th-century innovations.

I would first like to review some of the achievements of the movement against the authenticity of RK in the area of Thai history and historiography.

In "Piltdown 2" I emphasized that it had not been my purpose to try to prove that RK was written at a particular time by a particular person, only that it is not a genuine historical source for 13th-century Thailand. I even said that I would stop speaking of fakery, if other historians would reject RK as a source for early Thai history, although it seems that there will not be a general rejection of RK until it is demonstrated convincingly to be a later composition.

Nevertheless, a significant group of historians has taken up my position on the value of RK as a historical source. Dr. Elizabeth Gosling's paper for the 1989 AAS conference in Washington, published with some changes in *The Ram Khamhaeng Controversy*, falls into this channel of revisionism. In order to render RK architecturally comprehensible, and credibly authentic, Dr. Gosling has concluded that late 13th-century Sukhothai was "not ... a highly developed Buddhist 'Kingdom' ", but "at a cultural level anthropologists sometime[s] label 'formative' or 'chiefdom' ", perhaps, although "obviously ... not just another thirteenth-century mü,ang ... but 'a sort of super-mü,ang' as David Wyatt has described it".<sup>9</sup>

The following year Prof. Srisakra Vallibhotama agreed that Sukhothai "reached its zenith ... under the reign of King Mahadhammaraja Lithai.... It was during this period that Sukhothai developed a unique art and culture, which later [my emphasis—MV] dominated surrounding communities"; he considers that the authenticity of the RK stone is of lesser significance. "Even though it might not have been created in the reign of King Ramkhamhaeng, the inscription itself has high historical and linguistic value", just as Piriya Krairiksh has emphasized, and which justifies the ongoing investigation into its details.<sup>10</sup>

Prof. Chai-anan Samutrawanich [sic] added that "King Ramkhamhaeng the Great was perhaps a less important ruler than his successors"; and Prof. David K. Wyatt, for nearly thirty years the most faithful western defender and imitator of Thai traditional history, now urges that "Thai historians come out to propose that the Sukhothai kingdom was not the greatest kingdom in the area", and only "in the late period did [Sukhothai] become the centre of Buddhism, culture and trade".<sup>11</sup>

The most recent dismissal of RK of which I am aware was Craig Reynolds' remark in his speech at the London Thai Studies Conference that the attention devoted to RK in the last few years was an elitist preoccupation. Reynolds was proposing that historians should focus on a new theme in Thai history, gender, implying that those of us interested in RK and early Sukhothai should perhaps work on a biography of Ram Khamhaeng's mother, Nang Söang, or on Ram

Khamhaeng's seduction by the wife of Ngam Möang, Lord of Phayao. Perhaps the new 'employment' of the Ram Khamhaeng story, to adopt another of Reynolds' preoccupations, would be to relate his wide conquests, political and personal, and new alphabet, if the RK Inscription is taken as genuine, to the influence of a domineering mother, making him a kind of medieval Thai Max Weber.<sup>12</sup>

Now Reynolds has discovered another angle from which to knock down Ram Khamhaeng research, which also keeps him safely out of the controversy itself and out of the way of criticism, whichever way the argument about Ram Khamhaeng's authenticity is eventually settled. Reynolds complains, "[t]he debate about the authenticity of the first Thai language inscription of A.D. 1292 ... has so far failed to provide what one would expect from historians, namely, an account of how rulership in the kingdom of Sukhothai came to be identified as paradigmatic of good government in the modern period".<sup>13</sup> What Reynolds proposes is, of course, an interesting and valuable subject for investigation, but it is quite unconnected with the question of authenticity of Ram Khamhaeng; the one may, and I would say should be, studied without reference to the other. Although more can be done, we already know when "the kingdom of Sukhothai came to be identified as paradigmatic"; much has been written *how*, and *why* is almost self-evident, although there is no doubt more to be dug out of the writings of the modern royal nationalists in Thailand.<sup>14</sup>

None of these revisions of Thai historiography would have yet been possible were it not for the work of those who since 1986 have been criticising RK publicly.

I agree with Dr. Gosling's conclusions as an accurate picture of the Sukhothai of Rāmarāj, to use the true recorded title of the late 13th-century king, but I do not agree that this is what RK says. Its intent is to portray Ram Khamhaeng's Sukhothai as a great kingdom, with control over extensive territory, and as a center of highly developed Buddhism. Dr. Gosling's study is a welcome advance in Sukhothai history, but it does not, as she imagines, contribute to the support of either RK or 'Ram Khamhaeng'.

In spite of the value of Dr. Gosling's suggestions about the historical status of 13th-century Sukhothai, there is an uncomfortable circularity in her method. She assumes that RK, and its dates, are genuine, then uses a monument which has been hypothetically identified with one of the vague indications in RK, Wat Saphan Hin = RK's Araññik, to demonstrate the historical accuracy of No. 1. Some of her argumentation against other studies is also regrettable, as she sets up straw men or inaccurately describes what others have said. Thus, I have, never expressed disagreement "with Dr. Piriya that it was King Mongkut who wrote Inscription One", but stated only that the identification of the author was not among my purposes. Neither have I based my arguments on mere assertions

“that such and such a word is untypical or highly unusual for the thirteenth century”. I have compared words and contexts of RK with other Sukhothai inscriptions to show parallels with or divergences from recorded Sukhothai language, in particular of the 14th century, which I consider significant.<sup>15</sup> Real slyness creeps into her argument that I once “labelled the Ram Khamhaeng period ‘historic’ ... defined as a period for which contemporary documentation is available”.<sup>16</sup> What I really called “the first historical, as opposed to protohistorical, period of Sukhothai history”, was the reign of the king known from a contemporary source, at least contemporary in the sense it was established by someone who could have been his contemporary, as Rāmarāj, but who “has come to be known as Rām Gāṃhaen”. The source is Inscription No. 2, by Śrīś raddhārājaculāmuṇi, who as a child or youth could have observed the end of the reign of Rāmarāj, whose date of death is unknown.<sup>17</sup> “Rāmarāj” occurs in other later Sukhothai records for the king of that time slot, and was known to the compiler of *Jinakālamālī* as the name of an early king of Sukhothai. f139<sup>18</sup> There can hardly be doubt about his historicity, even though hardly any detail of his time has been preserved.

In her own contribution, Gosling says that eight other monuments with the same architectural features as Wat Saphan Hin represent what is left of Ram Khamhaeng’s ‘Seminal’ period construction, following which there was little architectural development for a half century, ending apparently around 1345, from which date she begins her second period labeled “Early, From c. 1345”. Except for a “ground level floor” in the first group and a “12” – 24” Base” in the second, the architecture of the ten structures of the ‘Early’ group is virtually identical to that of the ‘Seminal’ group, and includes parts of Si Chum (Wat Sri Chum), Saphan Hin, Ton Chan, Ton Makham, Thonglang, and Phra Pai Luang, which figured in the first group.<sup>19</sup> For a non-specialist in architecture it seems hazardous to date those monuments to two different periods fifty years apart, particularly since none of them is securely dated by an inscription.

In particular, recent work on Wat Sri Chum, although not its *wihan* but its *mondop* which Gosling does not discuss in this context, might be construed as casting doubt on the attribution of Wat Sri Chum to either of Gosling’s first two periods; and if so, then “ground floor level” may no longer be accepted as a diagnostic of early construction. In previous writing, Gosling adamantly, even intemperately, defended her view that the Jataka illustrations of Wat Sri Chum and their inscriptions should be “dated to the mid-fourteenth century or earlier”.<sup>20</sup> Now, however, there seems to be a new, rather wide, linguistic and art historical consensus that the Jataka plates of Wat Sri Chum were inscribed at the end of the 14th century, and were designed for the ceiling of the stairway of the *mondop* where they are now found, not produced in early or mid-14th

century and placed at Wat Mahathat—Gosling's position.<sup>21</sup>

One of the unfortunate aspects of the RK controversy which I noted in "Piltdown 2" is the tendency of some defenders of tradition to exercise their authority to stop the discussion. In the *Discussion*, Dr. Prasert implied that heavyweight opinion should be respected and Dr. Vinai Phongsriphien also resorted to this boxing metaphor in his criticism of Dr. Piriya Krairiksh.<sup>22</sup> But when there are at least four points of disagreement between heavyweights Gedney and Coedès with respect to RK, other weights may legitimately intervene.<sup>23</sup>

I would like to begin by reviewing the discussion of one detail with respect to which the heavyweight defenders of RK would seem superficially to have won—the significance of *trīpūra*.

Some of the heavyweights, including Prasert, Griswold, Maha Cham Thongkhamwan, and the compilers of the *Royal Institute Dictionary*, agreed for years that '*trīpūra*', or '*trīpūṇ*' as in the Wat Chiang Man Inscription, meant a triple wall.<sup>24</sup> This consensus held until I insisted in "Piltdown 1" that it reflected negatively on the authenticity of RK because archaeology proved that two of the walls were built much later. Immediately there was a scramble to demonstrate, or just assert, that '*trīpūra*' meant something else, not on any solid evidential ground, but as an epicycle to keep RK in stable orbit.<sup>25</sup>

The latest such effort was the statement that at Wat Chiang Man, it cannot mean 'triple wall' because the walls of Chiang Mai are obviously not triple.<sup>26</sup> The answer to this is that, as A.B. Griswold and Dr. Prasert carefully noted in their EHS 18, the inscription, written in 1581, does not refer to the visible walls of Chiang Mai built in the 18th century, but to walls allegedly built in 1296 around an early city area with Wat Chiang Man at its center, and of which no traces exist. Thus for all we now know the walls to which reference is made might have been triple. Moreover, in EHS 18, Griswold and Prasert agreed that '*trīpūṇ*' meant 'triple walls', the original walls, not the the one extant.

It is surprising, therefore, that in 1990 Dr. Prasert wrote, "In Inscription No. 76 [Wat Chiang Man], dated 1581, *trīpūra* was built on all four sides of Chiangmai, which has only one wall", rejecting his and Griswold's clear reasoning that Inscription No. 76 cannot provide evidence on the matter. He further referred to the *Kāṃsrual śrīprāj* of the Ayutthaya period which "says also that Ayudhya is *trīpūra*, while it has only one wall"; but the *Kāṃsrual śrīprāj* is one of the references given in the *Royal Institute Dictionary* as evidence that *trīpūra* meant three walls.<sup>27</sup>

It is peculiar, as Griswold and Dr. Prasert noted, that Wat Chiang Man and its related constructions are not mentioned in *Jinakālamālī*, the *Chiang Mai Chronicle*, or other literary sources, and one may ask if that part of the Wat Chiang Man

Inscription is not a 16th-century fiction, based on a local legend. If so '*trīpūṇ*' would be a 16th-century word, and there is no way to ascertain what it meant unless it is found in other sources of that time within clear contexts.

One of the '*trīpūra*' epicycles is at least plausible; that '*trīpūra*', although literally 'triple wall', was a general term for a city wall of any type.<sup>28</sup> The question is now moot, although I would still argue that the compilers of RK believed it to mean 'triple wall' referring to the three walls of Sukhothai visible to them.

At least, the discussion of '*trīpūra*' has demonstrated the value of lightweight iconoclasm in stimulating greater rigor in heavyweight textual study of early Thai epigraphy.

Another example of heavyweight consensus which has now been shaken is the pronoun *phōa* (พี่อ).

As I wrote in "Piltdown 2", heavyweights Bradley, Coedès, Griswold and Prasert had agreed that *phōa* "is the well-known sentimental first personal pronoun of the romances", and in their translation Griswold and Prasert construed it as singular, 'my', referring to the eldest brother who had died.<sup>29</sup> Then in 1981, one of Gedney's students, Robert Bickner, discovered that *phōa* was a first person dual pronoun, which I illustrated for Sukhothai with citations from Inscriptions No. 95 and No. 14. Gedney has accepted that *phōa* was dual, but inexplicably has argued that it is used as such in RK, although the context clearly refers to four persons—Ram Khamhaeng, a brother, and two sisters—following the death of the eldest brother.<sup>30</sup>

In response to my "Piltdown 2", Dr. Prasert has proposed that *phōa*, which he also now recognizes as dual, refers only to Ram Khamhaeng and his brother, because "in the past, we differentiate sons from daughters in grouping", or perhaps "[i]f two daughters not yet born ... [w]ould we say that the eldest brother dies, leaving four siblings, including the two not yet born?"

With respect to the first remark, no linguist of Thai has proposed that the ancient Thai pronouns were gender specific, and the context of RK does not permit that inference. Moreover, Inscriptions No. 95 and No. 14, in which *phōa* refers to a man and a woman, prove that it was not gender specific. The RK Inscription lists "three boys [and] two girls". This is immediately followed by "our [*phōa*] eldest brother died from us [*phōa*] when he was still young", which grammatically refers to all the preceding whether they were all born at the time of the eldest brother's death or not. Dr. Prasert's explanation is a conjecture based on a supposition contrary to what the inscription says, and, as such, is an unacceptable epicycle. I maintain that the writers of RK did not know the Sukhothai use of *phōa*, and that lightweight close reading has proven superior to heavyweight, perhaps overweight, tradition.



I shall continue with specific criticisms of my “Piltdown Papers”, beginning with the most specific and continuing on to the more complex, or matters which have attracted the attention of different persons.

### Vocabulary Items

In addition to *‘trīpūra’* and *‘phōa’*, Dr. Prasert has commented on my treatment in “Piltdown 2” of certain words in RK which I considered peculiar.

In the expression *‘phū nīn son’* (‘two girls’), I suggested that *‘nīn’* was anomalous and that the Sukhothai expression was *‘lūk sau son’* as in Inscription No. 2. Dr. Prasert’s answer is that *‘lūk hñīn’* and *‘phū yīn son’* are found in the Inscriptions of Nāy Dit Sai and No. 48.<sup>31</sup> The latter rather confirms my argument. Not only is it from 1408 when conventions may have been different, but it is from Chainat, where a group of several 15th-century inscriptions seem to be records of Ayutthayan, or at least not Sukhothai, Thai. The former inscription is of even later date, 1422, when there had been undoubted Ayutthayan political and linguistic influence in Sukhothai, and it is not decisive for the matter in question.<sup>32</sup>

In the same context, Dr. Prasert has answered another point I made about the same phrase from RK, that the vowel sign for /o/ [ŏ] in *‘son’*, is anomalous. It represents a vowel found only in borrowed words, usually from Indic or Khmer. Dr. Prasert says it “may be just a special characteristic of a dialect”, but the linguists are explicit that the vowel represented by that character is not found in Thai dialects.<sup>33</sup> Some early examples of this borrowing process are found in Sukhothai Inscription No. 5, of Lithai. There the Khmer word ‘favor’, ‘grant’, is written in Thai as *โพรส* (mod. *โปรด*), and Khmer *astrologer* is written *โพร*. Thai was forced to adapt a new vowel sign, , to represent Khmer *ə* in certain contexts, because the Khmer vowels had already split into two series, with two different phonemes represented by each vowel sign, and the vowel symbol *ə* in Thai was used for Thai words in which the vowel was different.<sup>34</sup>

As an indication of later composition of RK, I pointed out its abusive use of the alveolar / retroflex dental symbols where they are neither helpful, nor found in other Thai writing and I showed that such use reflected a Khmer practice which began after 1747 in answer to a real phonological need as that language changed.<sup>35</sup>

Dr. Prasert’s answer, also in the same context, is that Sukhothai writers “may suspect that these words are borrowed from either Khmer or Mon, and want to give some clues for the borrowing”, like *hāñ* ‘brave’, written with a final *ñ*, as in

Khmer rather than with *n*, in accordance with Thai pronunciation. Again the example is not pertinent. *Hāñ* is truly a Khmer loan word and, like many such, it preserves Khmer spelling. Even if the four words I cited from RK were loan words, and probably only one of them, *teen*, is, that particular spelling with initial alveolar was not in use in Khmer before the 18th century. There is no evidence that any Sukhothai writers wished to indicate loan words (in fact all nationalist treatments of RK have emphasized its pure Thai aspect), and all four words are found in other Sukhothai inscriptions spelled in the usual Thai manner, that is, without the peculiar use of alveolar/retroflex initials.

With respect to 'expressions for the people', occurrences of *brai fā hnā sai* in a literary work of 1482 and in the "Yuan [Lanna dialect]-Thai-English Vocabulary" in no way demonstrates that it was part of the Sukhothai tradition. The first, at least, rather points to the rhetorical or poetic style, which Bradley thought permeated the text and the second might indicate that the expression denoted a genuine northern institution. If there is really a genuine tradition behind the category *brai fā hnā sai*, it would have been helpful if Dr. Prasert and Griswold had given it some attention in their publication of RK, rather than simply treating the expression as 'commoners' or 'commoners with bright faces', since it, and *brai fā hnā pak*, which Dr. Prasert has apparently not found in the work of 1482 or the "Yuan-Thai-English Vocabulary", constitute a major institutional puzzle.<sup>36</sup> If it was a northern institution, one would expect to find it in the *Mañrāysāstr*, and in Griswold's and Prasert's study of that text, but apparently it does not appear in those "old" descriptions of the Chiang Mai society. If *brai fā hnā sai* was a genuine northern expression, but *brai fā hnā pak* was not, it is another indication of late compilers of RK arbitrarily utilizing an exotic term, and moreover, supplying it with a counterpart.<sup>37</sup> In the epigraphic corpus and *Three Seals Code*, there are literally dozens of examples of *brai fā vā khā dai*, but nowhere except in RK *brai fā hnā sai/hnā pak*.<sup>38</sup>

Dr. Prasert's comment on *pua* and *nāñ*, that they are "equivalent to 'bau' and 'sau', a young man and lady", where "'Bau' ... means 'servant' " supports, rather than contradicts, my concluding observation on those terms. I did not say that they "should not form a pair in RK", only that the treatment of the contexts in which they occur has so far been inadequate.<sup>39</sup>

Another interesting feature is the misuse of the vowel /*öa*/ (ᵛᵛ) where it is etymologically and historically incorrect. I commented on *böan* (Face 1, lines 19–20), used as a third person pronoun, and *nöan* 'silver' (Face 1, line 21). The first is really 'friend', while the third person pronoun in question, in those languages where it occurs, is *bön* /*pön*, *phön*/ (vowel ᵛ). 'Silver' in all languages is /*nön*/, never /*nöan*/; and linguists are in agreement that the original Proto-

Tai vowel /*öa*, *üa*/ became /*ö*/ in some languages, but that the opposite never occurred.

Dr. Prasert maintains that “friend” and the pronoun are in origin the same word, and he refers to Ahom as an example. This is not true, for both occur in some languages, as I indicated. Moreover, contrary to Dr. Prasert’s illustration, the vowel /*öa*/ (his /*üa*/) does not occur in Ahom.<sup>40</sup> As for /*ṛṇān*/, instead of /*ṛṇn*/, Dr. Prasert says this “is still used in Nan dialect”, which is contrary both to the historical analysis of Li Fang Kuei and to the descriptive work of Marvin Brown.

My argument is based on standard linguistic history. Li, in his chapters 14 and 15, indicates that while in some Thai languages the diphthong in question (*öa*, *üa*, *ia*) becomes the short vowel, the opposite, i.e., /*ṛṇn*/ > *ṛṇān* has apparently never been attested, and in his chapter 10, section 4, the original form for ‘silver’, perhaps a Chinese loan word, is given with the simple vowel.

Likewise Marvin Brown, whose phonetic and phonemic recordings are considered by all linguists to be of unparalleled accuracy, lists no language or dialect, including Nan, in which the proto-Thai vowel /*i*,*ö*/ became the diphthong /*ia*, *öa*/.<sup>41</sup>

Thus, Dr. Prasert’s argument in this case is not against me, but against the authors of the best Thai descriptive and historical linguistic work to date, whom I am only following.

Still another case of misused /*öa*/ is *söak* ‘war’, in the expression *khā söak khā söa* ‘enemy soldiers’ (RK, 1.31). This word is found with the simple vowel in both modern Thai and in Black Tai.<sup>42</sup>

A way out of Dr. Prasert’s dilemma was offered by Phasit Chitraphasa.<sup>43</sup> He said that several terms containing these vowels have been misread by everyone who has studied RK, beginning with King Mongkut. He pointed out that some words, which in modern Thai contain the /*öa*/ vowel, are written in RK with only one vowel support (i.e., the independent vowel *a* symbol [a]), while others show two such vowel supports side-by-side [aa]. Examples of the first are *söaṇ*, the name of Ram Khamhaeng’s mother, and *möaṇ*. Khun Phasit said that these terms should in fact be read as /*söṇ*/ and /*möṇ*/, as in certain northern and northeastern languages, and that only those words with a double vowel support, such as *phöaa*, the dual first person pronoun, and *möaa* ‘when’, should be read with the vowel /*öa*/.<sup>44</sup> If he were correct, then my objections to *böan*, *ṛṇān*, and *söak* would be invalid, for the writer of RK would have intended that they represent the etymologically correct pronunciations with /*ö*/. It would, however, mean that Dr. Prasert’s explanations are from another point of view invalid.

Khun Phasit's proposal does not hold up. First, the problem is not one of arbitrary variation in individual words within any single Thai language, but well attested historical vowel changes which affect entire sets of words in all Thai languages. In some languages, such as Ayutthaya and modern Bangkok both vowels /*ö*/ and /*öä*/ have been maintained, as in /*ṇön*/ and /*möaṇ*/. In other languages the latter vowel has coalesced with the former. Nowhere has /*ö*/ become /*öä*/. If in the RK language /*öä*/ had become /*ö*/, so that /*möaṇ*/ > /*möṇ*/, then the vowel in all such words as /*böan*/, /*ṇöan*/, /*söak*/ would also have been /*ö*/.<sup>45</sup>

As for the RK writing system, Bradley noted long ago that the writer of RK tended toward the convention of using a single vowel support when the word terminated in a consonant, but a double vowel support in open syllables.<sup>46</sup> Perhaps, to indulge in an *ad hoc* hypothesis, this was to make the open and closed syllables symmetrical, or to add clarity needed because of other vowel symbols being placed in front of the initial consonants.

There are also a few contexts in RK which substantiate the view that the vowel intended in all such words was /*öä*/. In Face 4, line 8, there is *möä*, 'when', which is spelled *möäa* in other contexts. Clearly the author intended the pronunciation /*möä*/ however it was written. Another key example is one instance of the /*ö*/ vowel which is never replaced by /*öä*/ in any language. This is the word /*thön*/, 'up to', written เ็ง, which is no longer the standard spelling.<sup>47</sup>

Finally, comparison with other Sukhothai inscriptions and post-Sukhothai linguistic development in central Thailand indicates that Sukhothai, like Vientiane, Ayutthaya, and Bangkok, was a language which preserved the distinction between /*ö*/ and /*öä*/ and the writers of RK, whether at Sukhothai, Ayutthaya, or Bangkok, would never have intended their *söaṇ* or *möaṇ* to represent the pronunciations /*sön*/ and /*mön*/.

Thus my comment about these terms in "Piltdown 2" still holds, and they represent an artificiality which argues against authenticity.

### Möaṇ Sukhodai nī

In "Piltdown 2" I took up a point which Dr. Prasert had made with respect to RK use of the word *nī* 'this', in the phrases *lāy sū daiy nī* and *möaṇ sukhoday nī* in RK, and indicated that I agreed with the logic of his opinion that in RK '*nī*' in these contexts should not be construed as the pronoun 'this', but as a sort of definite article. Thus, RK could not be construed as saying Ram Khamhaeng invented this script in contrast to some earlier existing Thai script, just as there was no other Sukhothai to contrast with this Sukhothai.<sup>48</sup>

I further pointed out that in the other Sukhothai inscriptions of the Lithai period, namely Nos. 2, 3, and 5, Sukhothai is mentioned at least 13 times, but never followed by *nī*, whereas, in Lithai's Khmer-language No. 4 the phrase *sukhodaya neḥ* occurs 4 times; I suggested that this was evidence for late composition of RK based on No. 4.

Now Dr. Prasert has found the expression *sukhodai nī* in No. 106, which I had not noticed, and says that this refutes my argument. In fact the phrase *mōaṇ sukhodai nī* also occurs in No. 102.<sup>49</sup>

Before continuing, we should review the treatments of Nos. 1, 102, and 106, and the controversy over '*sukhodai nī*'. In their EHS 9 on the Ram Khamhaeng Inscription, Griswold and Prasert made no comment on what Dr. Prasert has since explained as an anomalous use of '*nī*'. Each occurrence of *sukhodai nī* was translated 'this Sukhothai', and the phrase *lay sū dai nī* was translated "these Dai letters", although it was clear in their introduction that they considered there had been no other Thai letters, saying "By giving an account of the invention of *Tai* writing, it explains how it was possible for these inscriptions to come into being."<sup>50</sup>

Only later, in answer to the conjecture, which started with Coedès, that Ram Khamhaeng meant only that he had invented these Thai letters, improving on some kind of earlier Thai letters, Dr. Prasert advanced the opinion that *nī* in that context was not 'this', but a definite article, supported by the multiple occurrences of *sukhodai nī* which could not mean 'this Sukhothai' as opposed to another Sukhothai, for there was no other Sukhothai.

The argument is less strong than first appears for in RK there are other occurrences of *nī*, which seem clearly to mean 'this'. There are at least 7 occurrences of *mōaṇ nī* ('this *mōaṇ*'), three occurrences of *mai/pā tān nī* ('this / these sugar palm trees / forest'), and the phrase *khḍār hin nī* ('this stone slab').<sup>51</sup>

In their studies of Nos. 102 and 106 as well, Griswold and Prasert translated "lord of this Mōaṇ Sukhodai" and "this [land] of Sukhodaya" without comment about the anomalous 'this'<sup>52</sup> and in each inscription there is at least one other context in which *nī* must certainly be construed as 'this'.<sup>53</sup> Just as in No. 1, it is impossible to affirm that *nī* should be generally construed as a definite article rather than as "this", and its use with the name Sukhothai must be without literal significance.

Inscription No. 102 is of special interest, though, because in addition to *cau mōaṇ sukhodai nī*, there is a broken context--*mōaṇ sukhodai an--*, which Griswold and Prasert rendered, reasonably, as "Mōaṇ Sukhodaya, which". This would indicate that, for the writer of No. 102, the word *nī*, in *cau mōaṇ sukhodai nī* was

not just a filler, but had some definite significance. Perhaps a different translation should be tried, not “lord of this Möaṅ Sukhodai”, but “this lord of Möaṅ Sukhodai”, because the passage contrasts the rebuilding of the monastery in his reign with the neglect into which it had fallen under an earlier lord of Sukhothai.

Taken all together, these inscriptions weaken Dr. Prasert’s conjecture about the special significance of *nī* in certain passages of RK. I would still agree with him, though, that the intention of the writers of RK was that ‘Ram Khamhaeng’ had invented Thai script, not just this Thai script.

The real anomaly, which I pointed out in “Piltdown 2”, persists. The Lithai period inscriptions, the first Sukhothai writings after the dates contained in No. 1, do not use *nī* following “Sukhothai”; whereas, the Khmer-language No. 4 uses the Khmer equivalent, *neh*, RK resembles Lithai’s Khmer more than Lithai’s Thai. I still consider that it is one of several features of RK which are best explained as resulting from the influence of No. 4 on the writers of No. 1.

The irrelevance of No. 106 is even more certain if the arguments of Anthony Diller about rapid changes in Sukhothai Thai are accepted. Inscriptions No. 102 and No. 106 are later than Lithai’s reign and are not entirely pertinent. As Diller has emphasized, there was considerable change in Thai beginning after Lithai’s reign, and undoubtedly much influence from Ayutthaya which, itself, was under heavy Khmer influence.<sup>54</sup> Griswold and Prasert believed that Sukhothai had been conquered and occupied by Ayutthaya.<sup>55</sup> If the use of *nī* in Inscriptions No. 102 and No. 106 was not intended as ‘this’, as seems to be the case in No. 102, then they may show signs of that Khmer influence. Indeed Griswold and Prasert pointed out a Khmerism in No. 102, the word *nai* (ใน) used as ‘of’ (Thai / khòṅ / ของ).<sup>56</sup> Characteristic Khmerisms, although not noted as such by Griswold and Prasert, in No. 106, are *bannlu*, “an expanded form of *blu*”, and *bannlapp*, “the expanded form of *blap*”.<sup>57</sup> If Nos. 102 and 106 represent a new style of Ayutthayan or Khmer-influenced language, then they are not relevant for ascertaining fine points in the writings of the Lithai period.

## Cowries

In “Piltdown 1” I noted discrepancies between the descriptions of a *kathin* in No. 1 and a great festival which might have been a *kathin* in Lithai’s Nos. 4 and 5. I found it strange that the number of cowries among the offerings was 2 million in RK, but 10 million in Lithai’s records. More important was that RK spoke of ‘heaps’ of cowries using the Khmer term *bnam* (‘mountain’), an anomaly

because “inscriptions of Sukhothai, and of northern Thailand, in the 14th–15th centuries contain many references to cowries, from which it is certain that cowries were not just ornamental, but were a currency used for purchase and sale, as well as serving as a store of wealth ... [t]hey are always mentioned in precise quantities ... never in ‘heaps’ or even ‘large quantities’ .... This passage of RK seems to indicate a person unfamiliar with Sukhothai economic life”.<sup>58</sup>

Dr. Prasert objects, saying that no one knows whether 2 million cowries in Ram Khamhaeng’s time was worth less than 10 million in Lithai’s time or not; he brings in the analogy of modern inflation and the great increase in the price of noodle soup in the 20th century. There are, however, ample records showing that the value of cowries was held constant over centuries, until they went out of use in the 19th century, probably an example of administered prices as emphasized in the works of Karl Polanyi.<sup>59</sup>

More significant, however, is the anomalous use of Khmer *bnam*. It is not relevant that the “arrangement of money in the form of trees (*bum*) or attached to tree branches has been practiced up to the present”, or that “*bnams* of flowers” can be seen in the example of flowers that King Mongkut had placed around a certain chedi.<sup>60</sup> With respect to the use of ‘*bnam*’ we need to know if King Mongkut’s floral arrangements were called ‘*bnam*’, and even if they were, it proves nothing with respect to cowries. What matters is that in Sukhothai and Lanna inscriptions of the 14th and 15th centuries, in which there are numerous, references to cowries, they are never described as in ‘heaps’, but always listed in precise quantities.

This matter was taken up again in the *Discussion* by M.R. Supavat who referred to the record in the *Luang Prasöt Chronicle* of a great offering by King Maha Cakrabartiraj at which there were “*kòn jòn 4 dau jān*”, “equal to 1,600 baht”, which “at that time was equal to around 10 million cowries”, and thus ‘*kòn jòn*’ was ‘*bnam*’.<sup>61</sup> It is not certain precisely how M.R. Supavat understands *kòn jòn*. It would seem that he construes it in the modern Thai sense as ‘pile’–‘tray’, because he writes that there was “a stand below as a supporting tray”. In the context of the *Luang Prasöt Chronicle*, however, the king offered a white elephant with silver *kòn jòn* [on] 4 feet (4 *dau*) of the elephant. Thus the *kòn jòn* are probably to be construed as something on the elephant’s feet, for example in the Khmer sense, as anklets (*kòn*) around the elephant’s legs (*jòn* ‘foot’).<sup>62</sup> But whatever the exact meaning of this passage, M.R. Supawat’s argument is another irrelevancy. There is no doubt that there were piles or trays full of valuable offerings at royal ceremonies. What is at issue is the anomalous use of the word *bnam* in RK.

**Copy of the passage from *Luang Prasöt***  
(the phrase under discussion is underlined).

ศักราช ๙๑๙ มะเส็งศก (พ.ศ. ๒๑๐๐) วัน ๑๑๔ เกิดเพลิงไหม้ในพระราชวังมาก  
 หนึ่งในเดือน ๓ นั้นทำการพระราชพิธีอาจาริยาภิเษก และทำการพระราชพิธี  
 อินทราภิเษกในวังใหม่ หนึ่งเดือน ๔ นั้นพระราชทานสัตสดกมหาทาน และให้  
 ข้างเผือกพระราชทานมี กองเซ่งเงิน ๔ เท่าข้างนั้น เป็นเงิน ๑๖๐๐ บาท.

### Siam

This is not a detail which has any connection with the authenticity of RK, but since Dr. Prasert has included it in a critique of my papers, some readers may mistakenly think it is relevant, and I shall therefore run through it again.

In "Pilltdown 2", in an explanation of why I did not like to use 'Siamese' for the modern standard Thai language, I cited the evidence indicating that until the 19th century, no Thai people used 'Siam' as the name for their own country or ethnicity. This term was only used by outsiders to designate some part of the Menam Chao Phraya basin and its original meaning is unknown, but probably did not at first mean 'Thai'. Dr. Prasert has countered with *Jinakālamālī*, which uses 'Syāmaḍesa' for the Sukhothai area.<sup>63</sup> This, however, rather proves my point. *Jinakālamālī*, a Lanna work, only used the term 'Siam' for another area. If its authors thought 'Siam' meant 'Thai' we would expect to find it used for their own country, or to find it in the Sukhothai inscriptions. I repeat, however, that this is of no relevance for RK authenticity.<sup>64</sup>

In "Pilltdown 2" I gave some attention to the location of the polity which the Chinese called *Hsien*, and the pre-1350 references which had usually been interpreted by modern scholars as meaning Sukhothai. My argument was that *Hsien*, for the Chinese, had always meant an area in or near the Menam Chao Phraya Delta, not Sukhothai. Moreover, there was at least one Yuan Dynasty record in 1299 which recorded envoys from both *Hsien* and Su-Ku-tai at the same time. My attention has now been called to an even more explicit Yuan period record which states that *hsien* [xian in the article in question] controlled, or was the link to, "upper water" or "go upriver" Su-gu-di, meaning that not only were Sukhothai and *Hsien* different places, but that Sukhothai was upriver from *Hsien*, implicitly placing the latter downstream.<sup>65</sup>



## Treatment of Ram Khamhaeng in Other Sources

In "Piltdown 2" one of my arguments against the historical accuracy of the content of RK was that the true king of the 'Ram Khamhaeng' period, Rāmarāj, is given little notice in other Sukhothai inscriptions, which "ignore the very name 'Ram Khamhaeng', his script, orthographical conventions, language usage, religious activities, and economic initiatives".<sup>66</sup> In particular, Inscription No. 2, authored by a man who was perhaps old enough to have remembered Rāmarāj, passes him off with a brief remark on his *dharmic* qualities and nothing about his heroics and administration which are given such attention in RK. Those comments of mine have elicited a number of reactions.

First Dr. Prasert said the reason why Inscription No. 2 only referred to Rāmarāj as a religious man and not as a fighter was because the author of No. 2 had renounced the world and only referred to Sukhothai kings in *dharmic* terms.<sup>67</sup>

That is not at all accurate. The author of No. 2 describes battles of early Sukhothai kings, and his own participation in warfare in lengthy detail. This point was also raised by Michael Wright, to whom Dr. Prasert answered that the author of No. 2 was of a different lineage, and he only recorded the heroics of his own lineage, while kings of the other Sukhothai lineage, such as Rāmarāj, were described in *dharmic* terms.<sup>68</sup> Even this epicycle is not quite correct, for among the war heroes of No. 2 is the father of Rāmarāj, Indrāditya.

Another answer was offered by Dr. M.R. Suriyawut. He said that No. 2 referred to Rāmarāj in *dharmic* terms, but not as a fighter, because its author had not known Rāmarāj and did not want to refer to details of his life before he became king.<sup>69</sup> This argument does not hold up, because a large part of No. 2 is devoted to the early history of Sukhothai, including the lives of several ancestors before they became kings. The treatment of Rāmarāj, if considered in comparison to RK, is a special case which seems anomalous.

The latest reaction, again from Dr. Prasert, is that the author of Inscription No. 2 was a monk "who should refer to Ram Khamhaeng as a Dharma supporter only", against which one must raise the same objection as above, that No. 2 is full of battles and politics involving early Sukhothai kings. In addition, according to Dr. Prasert, the author of No. 2 "may not want to talk about him [Ram Khamhaeng] as a warrior who conquered his grandfather's land".<sup>70</sup> Such a conquest is pure speculation, not justified by any inscription, and it is the type of explanation, speculation about past thoughts, in which historians should not indulge.

In the same article Dr. Prasert argues that Ram Khamhaeng was not ignored in other records by citing several inscriptions or literary sources which refer to Rāmarāj or Braḥ Ruan. The Sukhothai inscriptions which refer to Rāmarāj, like

No. 2, do not accord him any special importance, and they support my position rather than that of Dr. Prasert.<sup>71</sup> As for Braḥ Ruaṇ, I explain below why he is to be considered a mythical character, not to be identified with Rāmarāj, or any other historical king.

### The Evidence of *Cintāmaṇī* / จินตมานฉิ

In several contexts Dr. Prasert and Dr. Thawat Punnotak have referred to one version of *Cintāmaṇī* as providing corroborative evidence for the fact of Ram Khamhaeng's inventing Thai writing at the date stated in RK.<sup>72</sup> The passage in question says that Braḥ Ruaṇ in Sri Satchanalai devised a Thai writing system in the year 645, presumably the Chula Era, equivalent to the *śaka* year 1205 (A.D. 1283) found in RK. Dr. Thawat makes the point that if RK is a late composition, its writers would have known *Cintāmaṇī* and thus 'known' that Thai writing was invented by Braḥ Ruaṇ. Why then would they have attributed it to 'Ram Khamhaeng'? For him, this is evidence for the authenticity of RK. More pertinently, his argument casts doubt on the authenticity of this passage of *Cintāmaṇī*, for if Thai writing was really invented by Ram Khamhaeng, why did *Cintāmaṇī* attribute it to Braḥ Ruaṇ and how did *Cintāmaṇī* get the name 'wrong' but the date 'right'?

In another context, Dr. Prasert indeed notes that the extant *Cintāmaṇī* may have suffered tampering. In a discussion of tone marks, he noted that *Cintāmaṇī* describes the marks *mai trī* and *mai catvā*, which in his opinion did not come into use until the Thonburi or Bangkok periods.<sup>73</sup>

The possibility of a doctored *Cintāmaṇī* was taken up more forcefully in the March 1989 *Discussion*. Dr. Piriya Krairiksh noted that the *Cintāmaṇī* in question was a 19th-century copy.<sup>74</sup> Then Pitya Bunnag emphasized that of hundreds of copies of *Cintāmaṇī*, three in the National Library and one in London state that Braḥ Ruaṇ invented Thai writing, and that only the one in London contains the date. It is called the King Boromakot Version, but was not in fact written in that king's reign. Khun Pitya went on to demonstrate why the London version must have been written in the Bangkok period, and suggested that the insertion of the date attributed to Braḥ Ruaṇ was a deliberate effort to back up RK Inscription.<sup>75</sup> This, I think, goes beyond what a historian may speculate, but there is indeed objective evidence that the date in question was inserted into that version of *Cintāmaṇī* by a late compiler. As written, the date is not 645, equivalent to RK 1205 (A.D. 1283), but 655. The 655, however, is labelled 'goat year', the animal synchronism of 645, whereas 655 was a dragon year. The name of the legendary

author is written *Roñ* (รอง), not *Ruañ* (รง), and he is associated with Sri Sajjanalay, not Sukhothai. More precisely, he is called พระญารองเจ้า who had, apparently in that year, 'obtained' (ได้) *möaṇ* Sri Sajjanalay.<sup>76</sup> This does not reflect the story told in RK, but seems to belong to one of the other stories of the *Ruañ* cycle.

This leads to the subject of Brah *Ruañ* whom I have treated as a mythical pan-Thai hero, who probably never existed. At least there was never a living Brah *Ruañ* within the area of present-day Thailand. Among feats attributed to him by tradition is the invention of writing. According to the พงศาวดารเหนือ ('Northern Chronicle'), which all historians recognize as an unreliable composition,<sup>77</sup> in B.E. 1000 equivalent to Chula Era 119 [sic], Brah *Ruañ* had the Thai, Chiang, Mon, Burmese, Thai, and Khom scripts made.<sup>78</sup>

Dr. Prasert has taken issue with my characterization of Brah *Ruañ* as a mythical hero, but his arguments merely restate standard assumptions. Prince Damrong "equates Brah *Ruañ* with King Ram Khamhaeng", although as I noted Prince Damrong postulated a whole dynasty of 'Brah *Ruañ*'; and *Yuan Phai* refers to Rāma and Lüdai "which correspond to Brah *Ruañ* and Phra Lü of Sukhothai", although the latter two are not mentioned in that sequence except in dubious sources such as the "Northern Chronicle". Dr. Prasert's third example is Inscription No. 13 dated 1510, and which refers to *pū brañā ruañ*. Dr. Prasert thinks this is "the name of one important person", but it more likely refers to an imagined ancestor believed to have constructed irrigation works, the true origin of which was no longer known.

The proof that *Ruañ* was not a specific Sukhothai king, but a pan-Thai hero is his appearance at the head of the Nan ancestor list in Inscription No. 45 and in the form Khun Lung in the Ahom chronicles where he had a son called Leu. In *Jinakālamālī*, Brah *Ruañ* (Rocarāja) is the father of Rāmarāj at a date which would correspond to King Indrāditya of the Sukhothai lineage. In the "Northern Chronicle" Brah *Ruañ* Arunakumār had the Thai, Mon, Burmese and Khmer scripts created in 1000 B.E. (A.D. 457), or Chula Era 119 (A.D. 757). The Lao hero Tao Hung seems also to be a version of *Ruañ*.<sup>79</sup>

One more detail arising from the discussion of *Cintāmaṇī* is its *cula* era date for the invention of writing, contrasted with the *śaka* era date of RK. Dr. Thawat says this is proof that RK was not written in the 19th century, for 19th-century fakers would have used their *cula* era instead of the genuine Sukhothai *śaka* era. Dr. Prasert also stated in another context that if RK had been written in the Fourth Reign its authors would have used *cula* era.<sup>80</sup> This is a non-problem. Any early 19th-century intellectual would have understood the *śaka* era and would have known that it was common in earlier times. On the assumption that RK was faked in the Bangkok period, its writers would have known at least Inscription No. 4 which uses the *śaka* era, and they would have been familiar with the *Three Seals Laws* which also contain examples of *śaka* era.

## The Dispersal of Thai Populations and Script

This is a matter on which there can be no absolutely precise knowledge for the time before the first appearance of Thai written records. In recent years, there has been general agreement among linguists that the Thai began moving westward and southwestward from northern Vietnam about 2000 years ago and the content of the Sukhothai inscriptions legitimizes a hypothesis that they may have reached the central Menam Chao Phraya Basin by the 12th century. The time of their occupation of the lower Menam Chao Phraya area and the peninsula is controversial, for there is written evidence that the language in use in those areas until the 15th or 16th centuries was Khmer and, until the 9th or 10th centuries, Mon, as well.

In my earlier "Piltdown papers" I emphasized the Khmer presence in those areas and also said that there had been at least three independent developments of Thai script in Indochina before the Sukhothai period, based on Khmer or Cham or both. I shall take this up again in more detail below.

Dr. Prasert has set forth his own views on these matters, in part in answer to my statements, and in part as a general exposition, the purpose of which was to demonstrate that the Ram Khamhaeng script must be the invention of Thai writing, with the exception of Ahom and Tham, which Dr. Prasert recognizes as independent, in his opinion somewhat later, developments.<sup>81</sup>

Dr. Prasert assumes that by the 13th century, Thai occupied a wide area from Assam to Vietnam, southern China and Malaya and, of course, all of what is now Thailand and Laos; he says that the absence of any discovery of Thai writings in those areas before the 13th century indicates that there was no Thai writing.

A difficulty with this argument, aside from the probability that there was earlier writing on perishable material which has not survived, is that in the lower Chao Phraya area and on the peninsula there are several examples of stone inscriptions, but all of them are in Mon, Khmer or Indic, which suggests that there was no significant Thai population.

In answer to the section of my "Piltdown 2" citing Anthony Diller's work on southern Thai linguistics, in which I argued that there had not been a Thai population on the peninsula in Sukhothai times, Dr. Prasert in another context answered that Diller's conclusion that Thai had been on the peninsula for 500 years really meant, according to Diller, "at least 500 years, and it may be 700 years", that is, perhaps, since late 13th century.<sup>82</sup> This does no damage to the points I was making that: any Thai language on the peninsula 500 to 700 years ago would not have had the features of modern southern Thai; the supposed influence of the monk from Nakorn Sri Thammarat on Ram Khamhaeng's tone marks would not have been as Dr. Prasert conjectured; and the purist reading

of RK, according to which Ram Khamhaeng conquered the peninsula, implies that Thai only settled there at that time, and would have spoken the Sukhothai language.

Dr. Prasert says that when Thai populations were not the ruling group they had to use their rulers' scripts rather than devise their own, and thus, there was no development of Thai script until they became the dominant group. Then they adapted the scripts of their rulers to make their own, a Chinese-type script in southern China, a Mon-based script in Lanna, and Ram Khamhaeng's writing adapted from Khmer. If there had been an earlier old Thai script, Ram Khamhaeng would have adapted his script from it.<sup>83</sup> My position is that Sukhothai writers did continue the use of an older Thai script.

Dr. Prasert's list omitted one type of writing which is significant for the controversial lower Chao Phraya area—the use of pure Khmer script to write Thai, a practice seen in Ayutthaya as late as the 16th century. Thus, had there been a significant Thai population in that area in earlier times we would expect to find Thai inscriptions in Khmer script, along with the Khmer and Mon inscriptions which have been discovered. On the peninsula, there was also a Khmer alphabet based on an Indonesian script, and 17th-century documents show both Khmer and Thai written in peculiar scripts quite different both from standard Khmer and from Sukhothai Thai.<sup>84</sup>

Dr. Prasert says that Inscription No. 62 (Wat Braḥ Yün in Lamphun) shows that the Sukhothai script spread to Lanna. In fact, there is no information about this in No. 62 and Dr. Prasert is simply stating an article of faith based on the traditional interpretation of RK. He also considers that the Sukhothai script spread to the White, Black and Red Tai and other groups in Tongking, whose scripts would thus have developed from the writing of RK.<sup>85</sup>

I shall attempt below to show that this is not true, that the Tongking scripts are independent, and that there is even some evidence that the *fak khām* (ฝักขาม) writing of Lanna may have been devised earlier than the Sukhothai script, and thus, might have been the origin of the latter. But at least if Sukhothai script spread, it was the script of Lithai, not that of Ram Khamhaeng.

Dr. Prasert acknowledges that the Ahom script is a separate, but slightly later development than Sukhothai, as is the Tham script of Lanna, which he asserts was devised by King Mang Rai from the local Mon script.<sup>86</sup>

The last is purely conjecture, for there is absolutely no evidence to justify it. The Lamphun Mon script, rather square, with most letters differing hardly at all from Old Khmer, is unlikely as the origin for the round Tham script of Lanna, which probably derives from a Mon model, much later than the Lamphun inscriptions or the reign of Mang Rai.<sup>87</sup>

## Tone Marking

One of the features of RK which is most suspect is its complete tone mark system, virtually identical to modern standard Thai, in comparison to the rest of the Sukhothai corpus in which tone marks are incomplete and seemingly erratic.<sup>88</sup> They seem to show writers searching for signs to mark distinctions which they felt necessary and which would eventually lead to a complete system. It is contrary to what we believe we know about development of cultural systems, in particular the invention of scripts, to suppose that a great genius invented the perfect system in the beginning; if he did, why did his descendants, within two generations, lose control of it?

In "Piltown 1", I illustrated the use of tone marks in the Sukhothai corpus, indicating that *mai ek* was most often a vowel marker, not a tone indicator, and that *mai tho*, even though often used where *mai tho* is used today, was erratic in all inscriptions, and sometimes used, apparently for an *ad hoc* contrast, where it would not be used today. Rarely did *mai tho*, within any inscription, make useful contrasts between or among words which except for tone are perfect homonyms with identical spellings. I also pointed out that the earliest Ayutthayan Thai inscriptions show precisely the same characteristics. We must assume that the writers of those inscriptions felt a necessity for the sign we call *mai tho*, but it was certainly not in order to make the same distinctions as in modern Thai. I suggested that the signs *mai ek* and *mai tho* must have originated as something other than tone marks and this is seen in the wide use of *mai ek* as a vowel indicator.

Marvin Brown had already given attention to a different type of discrepancy, not between RK and the rest of the Sukhothai corpus, but between a supposed rational system of 'Ram Khamhaeng', the inventor, in which each tone mark always indicated the same tone, and the irrational marking of later Ayutthaya and modern standard Thai in which each tone mark does not always indicate the same tone. The explanation of Brown, who accepted the traditionalist interpretation of Ram Khamhaeng, was that in his language the splits and mergers of tones which characterize modern Thai dialects had not yet occurred, so that all words in the A, B, or C Columns of the linguists' diagram still bore the same tone. Thus *mai ek* on B words or *mai tho* on C words always meant the same tone.<sup>89</sup>

Then, according to Brown, teachers from Sukhothai went to Ayutthaya, where the tones were different and imposed Sukhothai writing in which the tone marking system did not fit the tonal distinctions of Ayutthaya.

Dr. Prasert, who did give heed to the discrepancy between RK and the Sukhothai corpus, hypothesized that the monk whom Ram Khamhaeng invited from Nakhon Sri Thammarat imposed on Ram Khamhaeng a tone marking system suitable for the dialect of the south, but inaccurate for Sukhothai and it decayed after the end of Ram Khamhaeng's reign. Thus, Dr. Prasert's view is, in a crucial point, diametrically opposed to that of Brown, but it helps to account for the otherwise embarrassing discrepancy between Inscription RK and the Sukhothai corpus, a problem to which Brown gave too little attention.<sup>90</sup>

Dr. Prasert's explanation also opens up another problem. If the tone marking system was inapt for Sukhothai, both in the time of Ram Khamhaeng and of his successors, inapt for the Ayutthayan dialect, and for Bangkok, why did this system eventually prevail to become the system of modern standard Thai?

In "Piltdown 2", I cited the work of Anthony Diller, a linguist specialist on southern Thai, to show that the tonal structure of southern Thai had not been what either Brown or Dr. Prasert had supposed, but in the beginning was of the same type as Ayutthayan Thai. More importantly, I believe, I insisted that diacritics were used by writers who felt the necessity to indicate certain speech distinctions in writing, and that, with respect to Thai tones, the important distinctions (in terms of the linguists' diagram) are horizontal, between Columns A, B, and C, without respect to the vertical, where in writing most distinctions are indicated by initial consonants. It is necessary to mark, for example, the differences among /khau/ (เข) 'mountain', /khau/ (เข) 'knee', and /khau/ (เข้า) 'enter' (respectively: A–no mark; B–mai ek; C–mai tho), without concern whether /khau/ 'enter' bears the same tone as /khau/ (เข้า) 'origin', also C–mai tho, but with a different initial consonant, and indeed different tone.

Thus, there may never have been a Thai writing system in which each tone mark always indicated the same tone, nor is there any need to hypothesize such. A concern with horizontal distinctions only, which is in fact the modern standard system, is applicable to any Thai dialect.

Dr. Prasert has confused tones and tone marks. He has assumed that each tone mark must have indicated a particular tone height and contour, whereas such marks probably, in the beginning just as now, only indicated distinctions among terms which might otherwise have been confused in reading.<sup>91</sup>

The three-term tone marking is particularly apt for the branch of Thai including Ayutthaya and Bangkok, in which A, B, and C Columns are for the most part distinct. In the southern dialects, as hypothesized by Brown, there was, and is now, a great deal of merging between A and B Columns, which means that a two-term system might have been adequate, if the purpose was to indicate tones. Thus indeed a tone marking system based on the southern

contrasts of today or as hypothesized by Brown in the past, would have been inapt for 13th-century Sukhothai, but it would not have produced the system we see in RK.<sup>92</sup> On the other hand, even with convergence of tones in the A and B Columns, a language could still use a mark to distinguish between, for example, homonyms 'mountain' and 'knee', but it would not be a tone mark. This supposition is in part confirmed by the idiosyncratic tone marking system which David Wyatt found in one text of the Nakhon Sri Thammarat chronicles. The use of *mai muan* (๒) only for words which in standard Thai carry the "high or falling tone", and *mai malai* (๓) for words with "the mid, low, and rising tone", marks a significant difference in southern Thai where the original A and B tones have largely coalesced, but are distinct from C ('high' and 'falling') tones. Another isolated example of southern Thai idiosyncrasy is 'mountain' with *kho kuat* and *mai tho* (๒๓), contrasting with 'enter' with *kho kuat* and no tone marker (๒๓), the same distinction as made in standard Thai, but with opposite use of tone markers. This shows that the 'tone' marks were not to indicate tones in themselves, but to mark contrasts between two sets. There does not seem to have been a marker to distinguish total southern Thai homophones of the 'mountain'/'knee' type; and on page 42B of manuscript 'A' *kha* (๒๓ is written identically, for both 'I' and 'leg', although in southern as in standard Thai (๒๓/๒๓ respectively) they bear different tones. Neither, except for *mai muan/mai malai*, is there the regularity which Wyatt claimed for tone marking in version 'A'. 'My father' (*pho khā*, พ่อข้า in standard Thai) is written four different ways on a single page (พ่อข้า พ่อข้า พ่อข้า พ่อข้า).<sup>93</sup>

Another treatment of the problem was by Dr. Pranee Kullavanijaya.<sup>94</sup> On one point she seems to agree with me that the reason why tone mark distinctions in RK are the same as in modern standard Thai is because the problem is the same—to mark differences among the A, B, and C Columns. She does not see any problem with respect to the complete system appearing in RK, but she neglects what is the most important consideration, the difference between RK and its immediate successors. Why, if RK is genuine, was its writing system so quickly ignored?

Dr. Pranee also called attention to another feature which she believed indicated the antiquity of RK, its use of the *hn*, *hm*, *hr*, *hl*-type consonants which are neither compound consonants nor tone markers, as the initial *h* is in modern Thai. In origin, they served to indicate voiceless nasals and liquids, as opposed to their voiced pairs written *n*, *m*, *r*, *l*.

Again, Dr. Pranee should have looked at the entire Sukhothai corpus in which, at least in the Lithai-period inscriptions, one observes the same use of *h* before nasals and liquids. Inscription No. 1, then, in this respect merely shows



a feature which is present in written Thai from the Sukhothai period to the present and which does not indicate any special antiquity for RK. Modern fakers would have had no trouble with this detail.

Anthony Diller has now come forward with a new proposal, that from the Sukhothai period until the 19th century there were two competing systems of superscript marking, one in which *mai ek*, for example, marked a tone, and another in which it marked vowels. He also hypothesizes "rather rapid diachronic sound change, especially relating to tone", as (1) Khmer speakers became assimilated to Thai, and (2) because of "substantial Tai dialect mixing, especially in the Chao Phraya valley during the 14th and 15th centuries".<sup>95</sup>

In that paper, Diller assimilated Ayutthayan Thai and an unknown number of other dialects with Sukhothai to make an argument about the status of the tone marking system in RK. He was no doubt right about both the influence of Khmer speakers and dialect mixing in the Chao Phraya Valley, particularly beginning in the 15th century. He may, however, be mistaken about this being important in the time of Lithai, whose inscriptions are the crucial evidence in the discussion of tone marking in Sukhothai.<sup>96</sup>

In his paper, page 15, Diller, assuming RK is genuine, attempted to show that in the inscriptions after RK there was a gradual decline in the correct use of the tone mark, *mai ek*, and gradually increasing use of a *mai ek* type mark, which Diller has baptized 'ฝหนอง (*fon thòn*)' to indicate vowels, in particular the vowel /ɔ/.<sup>97</sup> To illustrate this he has set up a bar graph showing the use of the two types of symbol in the following seven inscriptions in the order—Inscription No. 1 [A.D. 1292]; No. 93 [1399]; No. 9 (2) [1369]; No. 5 [1361]; Dit Sai Inscription [1422]; No. 62 [1370]; and No. 49 [1417]. Inscription No. 1 is shown with 100% correct use of tone marker *mai ek*; No. 93, just over 50%; No. 9 (2), about 30%; No. 5, 20%; Dit Sai and No. 62, hardly any; and No. 49, 1-2% correct use. As for the *mai ek* marker as a vowel sign, Diller's '*fon thòn*', the bar graph shows virtually no use in his first five items, then a take off to around 30% in Inscription No. 62, and 60% in No. 49, and still more frequent use in written records of the 17th century.

This bar graph, however, violates the first principle of such illustrations, that the items should be in a regular chronological series. That is Diller's items 1-7 should be in the order 1 [Inscription 1-1292]; 4 [5-1361]; 3 [9 (2)-1369]; 6 [62-1370]; 2 [93-1399]; 7 [49-1417]; 5 [Dit Sai-1422].

Then we would see, abstracting from Inscription No. 1, RK, an increase in correct *mai ek* from a rather low 20% in Lithai's Inscription No. 5, to 30% in Inscription No. 9, followed by a drop to almost nothing in No. 62, and then a dramatic increase to over 50% in Inscription No. 93. In fact, No. 62, from a

different area and polity, Lamphun, should be excluded from the comparison, which would then show gradual increase in correct modern tonal usage from a low in the time of Lithai to the end of the 14th century. Thereafter, whether in the Sukhothai or Ayutthaya areas, tonal marking was erratic.

Diller has also been careless in his illustration of *mai ek* as a vowel sign. His bar graph indicates virtually no use of it in his first four examples, then a slight takeoff in the Dit Sai Inscription, followed by a great increase in items 6 and 7, Inscriptions No. 62 and No. 49. The problem here is that both the two latter predate Dit Sai. Moreover, the Dit Sai Inscription contains no *mai ek*-type signs at all, including five words which should have a *mai ek* tone mark in modern usage, and the other inscriptions in Diller's graph which predate Dit Sai all have some examples of a *mai ek* vowel sign. In this respect the graph is simply erroneous.

It is also peculiar that Diller neglected the Lithai corpus, except for Inscription No. 5. It is these inscriptions, the responsibility of 'Ram Khamhaeng's' grandson, who would have learned Thai literacy from teachers who had known his grandfather, which are the telling evidence against the authenticity of the 'Ram Khamhaeng system'.

Diller has badly failed to make his case for two competing systems of marking, one which declined from Ram Khamhaeng's script until sometime in recent centuries, and another which began to develop in the 14th century and gained in usage thereafter. All Sukhothai inscriptions, except RK, exhibit a confusing mixture of *mai ek* as tone and vowel marker, and at the same time, some of the same words without any mark. This is also true for *mai tho*, which, as I indicated in "Piltdown 1", is a better test. There was no *fon thòn* system, nor any system at all. There were apparently competing ideas about how certain diacritical signs should be used, but in all the texts which show enough examples to be useful, the use is erratic and tentative.

The status of RK in this respect turns on comparison with the inscriptions of the Lithai period, Nos. 2, 3, 5, 8, in particular, written in the 1350s–1370s, at the behest of princes only one and two generations from 'Ram Khamhaeng', who learned their language from parents living at the time their father, uncle, or cousin had No. 1 composed, assuming it is genuine. I cannot accept that Diller's considerations of language change and dialect mixing are valid for the relevant period, and we may not hypothesize that Rāmarāj, śrīśaddhā (with his contemporary King Löthai), and King Lithai each spoke, and tried to write, a different dialect.

It requires an act of faith to maintain that the epigraphic record may be interpreted as showing a decline in a full tone marking system devised for RK. I maintain that the tone marking system of RK is an anomaly, and that no explanation so far proposed, except the hypothesis of late composition, accounts for it.

## Ram Khamhaeng and The South

There are three issues here relating to the authenticity of RK:

1. The language of Nakhon Sri Thammarat, and the influence of its language on the Sukhothai script.
2. The dominant religion in Nakhon Sri Thammarat in the 13th century and its influence on Sukhothai.
3. The political relationship between the two areas, that is, did 'Ram Khamhaeng' conquer the peninsula as stated in Inscription No. 1?

The first is a relatively new issue, resulting from a hypothesis by Dr. Prasert to explain the 'dissolution' of the complete tone marking system of RK in the later Sukhothai inscriptions. In "Piltdown 2", I cited the extant written evidence from the peninsula to show that it was probably not yet Thai in the 13th century, and Anthony Diller's work on southern Thai in which he surmised that Thais had only been living there for somewhat over 500 years, not as long ago as the reign of 'Ram Khamhaeng'. Above I indicated evidence that even if the early southern dialects showed those tonal features now considered typical, they would not have produced the tone marking system seen in RK and in modern Thai.

Now Dr. Prasert says Diller really meant Thai had been on the peninsula as long ago as 700 years, which would still mean only the beginning of Thai settlement at the end of the 13th century. The characteristic features of the southern dialects would still not have developed at the time of 'Ram Khamhaeng', and the influence of southern tones on the Sukhothai script would not have occurred as postulated by Dr. Prasert. Moreover, if the conquest of the peninsula by 'Ram Khamhaeng' is accepted, just at the time when Thai may have been first settling there, then we might suppose that most of those new settlers were Thai from the central plain in Ram Khamhaeng's army, and their dialect would have been that of Sukhothai, or close to it. From this angle as well, it is unlikely that a monk from the south would have skewed the RK tone marks via the influence of his own Thai dialect.

In his latest contribution, Dr. Prasert seems to deny that 'Ram Khamhaeng' conquered the peninsula, as he and Griswold had already implicitly denied it in their EHS9. In his Kunming paper, he said, "the expression may be translated as capable of conquering". The expression 'capable of conquering', *āc prāp*, (face 4, line 16), which Griswold and Prasert then rendered "he was able to subdue", precedes "a throng of enemies", but with respect to named localities including Nakhon Sri Thammarat the text says simply *prāp* 'conquered' (line 17), which Griswold and Prasert rendered "whose submission he received" (p. 218), because they realized then, as Dr. Prasert does now, the implausibility of

conquest of the peninsula by Sukhothai. It is clear, however, that the author(s) of RK intended to say that Ram Khamhaeng conquered, and the phrase *āc prāp* preceding the statement that he conquered certain territories should probably be construed as, 'he was able, and in fact he did'.<sup>98</sup>

In answer to my allusion to evidence that the dominant language of the Nakhon Sri Thammarat region may still have been Khmer, and that Khmer was important as late as the 17th century, Dr. Prasert drew an analogy with the Pope using Latin which does not mean that his listeners are Latin.<sup>99</sup> For the analogy to be pertinent, the documents from Ayutthaya should have been in Pali, which, not Khmer, was the language of religion. It was not Khmer language which was used for religious purposes in 17th-century Ayutthaya, but Khmer script, used to write religious texts both in Pali and in Thai.<sup>100</sup> Moreover, the Khmer texts in question, found in the region of Pattalung and Nakhon Sri Thammarat, are not strictly religious, being grants of land and slaves, and they are not in the standard Khmer of Cambodia and Ayutthaya, either in language or in script, but in a script and dialect peculiar to the peninsula, proving, I would say, that it was still a spoken dialect in the region.<sup>101</sup>

Is it likely that a Mahathera from Nakhon Sri Thammarat would have been invited by a late 13th-century Sukhothai king to bring, or strengthen, orthodox Singhalese Buddhism? A careful reading of Griswold and Prasert's EHS reveals that they did not explicitly attribute the introduction, or a re-introduction, of Sinhalese Buddhism into Sukhothai by the Mahathera, but their treatments of Inscriptions No. 1 and No. 2, and those of Lithai, in EHS 9, 10, 11, imply that they considered the Mahathera of RK to be a representative of Sinhalese orthodoxy, and that when Lithai invited his famous monk, who was explicitly of the Sinhalese persuasion, it was to renew the faith of his grandfather.<sup>102</sup> This also seems to be the tenor of Dr. Prasert's latest comment, that "Ram Khamhaeng invited a monk to bring to Sukhothai a new Buddhist sect."<sup>103</sup>

In "Piltdown 2", I cited several works of art history which indicate that the archaeological and art historical evidence of the south does not indicate Sinhalese orthodoxy.<sup>104</sup>

Then, at the conference in Washington, D.C., for which my "Piltdown 2" was prepared, Hiram W. Woodward, Jr. presented a paper which assumed that the authenticity of RK was so certain that it needed no defence.<sup>105</sup> Much of Woodward's discussion concerned religious art and architecture and its meanings. He considered that "[i]n both Siam and Cambodia the dominant Buddhist sect for the greater part of the thirteenth century was a sect that can be called Lopburi Hinayana. Its roots lay primarily in Burma. The sect started to challenge the dominant Mahayana of Cambodia toward the end of the twelfth century; it emerged victorious, and it persisted until the middle decades of the fourteenth

century when it was finally supplanted as a result of new ties with Sri Lanka."<sup>106</sup>

That is, Sinhalese orthodoxy only began to dominate in Siam in the time of Lithai.

Furthermore, "the dominant type of Buddha image in Ram Khamhaeng's time", with a lineage of Buddha images lying behind it, was the 18-cubit type mentioned in RK at the Araññik monastery now identified with Wat Saphan Hin. "The concept of the eighteen-cubit Buddha should be considered part of the bundle of Lopburi Hinayana beliefs", and the posture of Ram Khamhaeng's eighteen-cubit image derives from earlier images which Woodward associated with Thai speakers to the east of Sukhothai in Laos where there is an early example apparently dated A.D. 1006. Examples become rather numerous around the final decades of the 13th century, and "[t]he interest in this posture I take as a feature distinguishing Ram Khamhaeng's Buddhism from earlier Lopburi Hinayana traditions."<sup>107</sup>

Thus, 'Ram Khamhaeng' was a religious innovator, but not in importing Sinhalese orthodoxy, rather in adding a northeastern Thai tradition to Lopburi Hinayana. In that paper, Woodward totally ignored the problem of the Mahathera from Nakhon Sri Thammarat and everything he wrote about late 13th-century Sukhothai Buddhism would tend to undermine that part of RK.

Even when Woodward, in earlier work, explicitly stated his belief in a literal reading of RK, he seemed uncomfortable with 'Ram Khamhaeng's' Mahathera.<sup>108</sup>

At that stage Woodward was not yet using the concept 'Lopburi Hinayana'. In the 11th and 12th centuries, he identified "three distinct iconographical complexes, Pimai's Vajrayana, Angkor's Mahayana, and a Hinayana in central Siam", which descended from Dvaravati. "During the 13th century a fourth iconographical system came to dominate Siam ... became more or less joined to the local Hinayana." Its "features are ones also found in Burma, and ... the new iconographical complex will be called 'Mon'". The Mon iconography "was eventually replaced by Sinhalese orthodoxy, first proclaimed in Sukhothai perhaps [my emphasis—MV] by the patriarch" of Ram Khamhaeng, "and then strengthened by direct ties during the reign of King Lö Thai (?1298–1346/47)".<sup>109</sup> The 'perhaps' is because Woodward saw no iconographical evidence for Sinhalese Buddhism in Sukhothai, and apparently not in Nakhon Sri Thammarat, in the 13th century, but he had faith in RK. The tentative reference to Ram Khamhaeng is repeated a few pages later, and again with the emphasis "it is only with the inscription of ... Lö Thai ... that there is solid evidence of religious intercourse with Ceylon"; in the evidence to which Woodward alluded, Inscription No. 2, the Sinhalese influence was not via Nakhon, but directly from Ceylon through lower Burma.<sup>110</sup>

Of course Woodward noted that Nakhon Sri Thammarat seems to have had contact with Ceylon in the 13th century, citing Coedès' *Les états hindouisés d'Indochine et d'Indonésie* for the details. Those details, and most of the sources in which they are found, are anything but satisfactory. Between about 1230 and 1270, a peninsular king, Chandrabhānu, was in some kind of relationship, often bellicose, with Ceylon, and the Ceylonese sources claim he was interested in Buddhist relics. Thus, his reign may have been the very beginning of Sinhalese Buddhist influence in Nakhon, not yet a place from which Mahatheras would set out as missionaries. Coedès also considered that Nakhon, at that time, was not Thai, and that it was only after Candrabhānu's death that it was conquered by the Thai, that is, by 'Ram Khamhaeng'.<sup>111</sup>

This is an appropriate place to discuss a suggestion by Dr. Prasert that the *Jinakālamālī* provides evidence in support of the implication of RK that Sinhala Buddhism came to Sukhothai from Nakhon Sri Thammarat in the time of 'Ram Khamhaeng'. Dr. Prasert referred to a section of *Jinakālamālī* dated A.D. 1518/9, which has been construed as saying that a king named Rāmarāj brought the religion to Siam from Sri Lanka, and Dr. Prasert, preceded by other scholars, has interpreted it as a reference to Ram Khamhaeng.<sup>112</sup> In his own words, "in 2026 B.E. [sic 2062=A.D. 1518/1519] the monks of Chiang Mai gave thanks for the Buddhist religion which the king named Rāmarāj (Pho Khun Ram Khamhaeng) had brought from Lanka."<sup>113</sup> George Coedès considered that the Pali of the passage in question was so corrupt that it was untranslatable. "These stanzas", he wrote, "have been sabotaged by copyists who undoubtedly did not understand them.... They are not translated in the Siamese version [Coedès was writing in 1925], and the best Pali scholars of Cambodia and Siam whom I have consulted have had to admit their inability to reestablish the correct text."<sup>114</sup> Since the rest of *Jinakālamālī* is apparently in fairly good Pali, the state of these stanzas suggests that they are an interpolation, not a part of the original.

In his English translation of *Jinakālamālī*, N.A. Jayavikrama also called attention to the problem of these stanzas, noted Coedès' remark, and warned that "the translation given here is merely tentative".<sup>115</sup> The relevant section of that translation is "A pre-eminent sage was honoured ... and unto him who had accrued merit not found in others they gave the name whose first part is Rāma; and both of them (were determined) to illuminate the Word of the Noble Sage which had been brought from the Island of Lanka."

This is quite different from Dr. Prasert's proposal. There is no question here of a certain Rāma having brought the religion from Lanka. Moreover, following the difficult Pali stanzas, and still in connection with the religious celebration

recorded in that section of *Jinakālamālī*, it is stated that in the year 2062 (A.D. 1519) “[f]rom the time of its introduction to the city of Nabbisi [Chiang Mai], the Sihala dispensation had been in existence there for eighty-eight years”, that is since A.D. 1431.<sup>116</sup>

In spite of this, Jayavikrama seemed to accept that the Rāma mentioned in the difficult context was Ram Khamhaeng.<sup>117</sup>

In this, he was influenced by certain Thai scholars. Dr. Saeng Manavidura in “Some Observations on the *Jinakālamālīpakaraṇa*” preceding Jayavikrama’s translation, wrote “[f]rom his [Ratanapañña, author of *Jinakālamālī*] work, we can gather that there were three sects (Nikāya) of the Order of Saṅgha in Thailand”: (1) Naggaravāsigaṇa, “the native sect ... since the time of the Ven. Soṇa Thera and the Ven. Uttara Thera”, repeating a Southeast Asian myth that Buddhism was first introduced by missionaries from King Asoka; (2) Pupphavāsigaṇa, “the Rāmañña Sect established by the Ven. Phra Sumana Thera”, who “first of all stayed at Sukhothai and was later invited to Chiangmai by King Kuenā”, events that seem well established and dated to the 1360s–1370s, the reign of King Lithai;<sup>118</sup> (3) “[t]he Sinhalese sect headed by Ven. Phra Mahā Dhammagambhīra Thera and Ven. Phra Medhaṅkara who both went to Ceylon and were ordained there.” This is dated in *Jinakālamālī* in A.D. 1430.<sup>119</sup>

Note that for Dr. Saeng, these were the only introductions of Theravada Buddhism into either Sukhothai or Chiang Mai.

Dr. Saeng continued, saying the people of Chiang Mai received the Ceylonese system from Sukhothai, and he is referring to the last-mentioned mission, for he repeats that in *Jinakālamālī* this “Buddhism belonging to the Sinhalese Sect had been established at Chiangmai for eighty-eight years ... [t]he year when this statement was made was 2062 B.E. ... [s]o it means that the Buddhism of the Sinhalese Sect was introduced into Chiangmai in 1974 B.E. (1430/31 A.D.).”

Then, surprisingly, Dr. Saeng continued, “...and the statement ‘Rāmādinā mam Laṅkāḍī pāgatantam munivaravacanam ... [of the controversial Pali section] shows that the Buddhism which was introduced by King Rām (i.e., Khun Rām Khamhaeng....) was further introduced into Chiangmai”. This contradicts Dr. Saeng’s previous exposition of the stages of Buddhism’s implantation in Thailand. It is also incoherent. In no text is there justification for identifying the movement which reached Chiang Mai in the 1430s with 13th-century Sukhothai, for the monks who went to Ceylon in the 1420s for reordination were from Chiang Mai, not Sukhothai. Of course, before reordination, they may have represented what Dr. Saeng called the Ramañña Sect, introduced into Chiang Mai around 1370 from Sukhothai, but that is associated with Lithai, not Ram Khamhaeng.<sup>120</sup>

Given Dr. Saeng's well-known scepticism about Ram Khamhaeng,<sup>121</sup> he may, after setting out his own opinion, based on a close reading of *Jinakālamālī*, have deferred to conventional views in the rather tortuous rationalization about reintroduction of the Buddhism associated with Ram Khamhaeng.

For *Jinakālamālī* is an embarrassment to the RK faithful. In its history of Buddhism in Thailand there is no mention in its treatment of the 13th or 14th centuries of the role of Ram Khamhaeng, or Rāmarāj, in any phase of the introduction or development of the religion.

There is, to be sure, the legend of the Sihala, or Sihing, Buddha image, interpolated after the account of Sumana bringing a type of Buddhism (Dr. Saeng's Rāmañña sect) to Chiang Mai in 1369. But it is recognized as legend, not history, and does not at all help the case for Ram Khamhaeng. According to this legend, in 1256 a king of Sukhothai named Rocarāja, Pali for Braḥ Ruaṇ, went to Nakhon Sri Thammarat to get the Sihing statue which had miraculously survived a shipwreck on the way from Ceylon and floated for three days until reaching Nakhon.<sup>122</sup> Thus, 'Braḥ Ruaṇ' acquired a magic statue, but there is no question in that story that he introduced or developed the religion itself. This Braḥ Ruaṇ, moreover, was not Ram Khamhaeng / Rāmarāj. Rāmarāj was his son, and in *Jinakālamālī* he is credited with no special religious activity except continuing to worship the Sihing image, nor is he given any political importance.<sup>123</sup> That is, the treatment of the Sukhothai King Rāmarāj in *Jinakālamālī* is precisely like that in Inscription No. 2, a one-line acknowledgement of his existence.

It is no wonder that "Prince Dhaninivat observe[d] ... that Ratanapañña's knowledge of the political history of the dynasty of Sukhodaya-Sajjanālaya Kingdoms is rather meagre compared with his greater familiarity with the events connected with the Ayudhya Kingdom"; that Dhanit Yupho told Jayavikrama the list of Sukhothai kings in the Sihala Buddha story was erroneous, for of course the Braḥ Ruaṇ who got the image should have been 'Ram Khamhaeng', as Dhanit Yupho informed Jayavikrama. Jayavikrama's translation of the difficult passages, moreover, was influenced by notes and translations of Dhani Nivat and Dhanit Yupho, who may have been strong believers in Ram Khamhaeng.<sup>124</sup>

We may all agree that the Sihala Buddha story is not accurate history, but then each of its details is suspect unless precisely supported by better evidence, and no detail may be lifted and inserted into another historical frame. Certainly the reception of an image from Ceylon via Nakhon Sri Thammarat may not be reworked as the introduction of Sihala Buddhism to Sukhothai via Nakhon in the form of a famous Mahāthera, as the writer(s) of RK may have done.



*Jinakālamālī* probably contributed to the composition of that part of RK together with Inscription No. 4.

What is most important is that there is nothing in the difficult passage which justifies association with Ram Khamhaeng. It is clearly placed in a context which dates the relevant arrival of Buddhism to the 1430s, brought by monks who went through the Mon country of Lower Burma, where, in the 1430s the king was named Rām, or Rāmarājādhirāt.<sup>125</sup> Even this may not be significant. The poor quality of the Pali suggests it may be a very late interpolation such as the '*Gāthānamaskār*' ('Stanzas of Homage / Words of Praise') of *Traibhūmikathā*, which the scholar-princes Damrong, Naris, and Vajirañān all recognized as a spurious inclusion, not part of the original text. In that case we cannot know whom the writer intended as 'Rāma', nor on what grounds.<sup>126</sup>

### All Vowels on The Line

With respect to this problem, Dr. Prasert has made a plea of the type "why could Ram Khamhaeng not have...?", which is unanswerable, but he has nevertheless evoked an interesting detail which deserves treatment. Dr. Prasert has asked, if RK put all consonants on the line, i.e., without using conjunct or compound / subscript consonant symbols ('foot' consonants in Khmer), as most Indic scripts do to represent consonant clusters, why could he not have decided to put all vowels on the line, too?<sup>127</sup>

The circumstance that the earliest known Thai writing, whatever the status of RK, in contrast to other Indic scripts, both in India and in Southeast Asia, ignored conjunct consonants to express clusters, requires explanation, but a materialist explanation, not one based on speculation about what a great sage might have thought. I believe there is a materialist explanation, which provides a clue to the origin of Thai writing in general, and I shall take it up below in the section on the history of Thai scripts.

A new argument which has emerged is that some of the mid-14th-century Inscriptions, No. 2 and No. 3 of Lithai, No. 62 in Lamphun, and perhaps others, continued the practice of putting some vowels on the line, following the system of RK, thus proving that such a system had been devised earlier, and therefore RK is genuine.

I believe the first person to make this argument was Dr. Thawat Punnotak who found one example of vowel *i* (ඞ), on the line in No. 2. and one case each of *i* (ඞ) and *ü* (ඞ) in No. 3.<sup>128</sup> Then Dr. Prasert repeated this in a quantitatively less precise manner, saying that after the RK period writers returned to placing

vowels above and below according to habits they had learned from the Khmer. But they sometimes slipped up and put vowels *i* and *ü* on the same line as seen in Inscriptions No. 2 and No. 3; he added that this also occurred in Inscriptions No. 8 and No. 102 in Sukhothai, and No. 62 in Lamphun.<sup>129</sup>

These examples are special cases which prove nothing. The vowels in question consist of the vowel support, or independent vowel (in Thai 'floating vowel'), which in most (perhaps all) Indic scripts is identical to the independent vowel *a*, with the marks for *i*, *ü*, etc., added above it. What we see in the examples cited by Drs. Thawat and Prasert is best explained as carelessness, or isolated experimentation in placing the signs for *i* and *ü* within the roughly circular *a* vowel rather than above it. These idiosyncracies show the type of experimentation which might have eventually led to an RK-type writing system, but at a date just over half a century after the alleged RK period, they cannot be accepted as relics of it.

This seems also to have been the view of Griswold and Prasert when they reedited and published Inscriptions No. 2, No. 3, No. 8, No. 62, and No. 102. Nowhere did they call attention to the few cases of *i* or *ü* written on the line. In their study of Inscription No. 2, they wrote, "[t]he script devised by Rāma Gaṃhèn ... has changed considerably in No. 2 ... several of Rāma Gaṃhèn's innovations have been abandoned or modified. No longer are all the vowels written in the same line with the consonants; such vowels as *i* and *ī*, *u* and *ū* have resumed their places above and below the consonants."<sup>130</sup> One orthographic detail to which they did give attention and which argues for late composition of RK is that the inscriptions of Lithai do not distinguish the vowels <sup>◌̣</sup> and <sup>◌̤</sup>, which are represented by <sup>◌̣</sup> and <sup>◌̤</sup>. RK has a full set of these vowel signs, like modern Thai.

### The *kh khai* (๒)/*kh khuat* (๓) Problem

Phase 1: In "Piltdown 1", I used only published Thai transcriptions of the inscriptions and did not look at the plates. The result was that RK and all 14th-century Sukhothai inscriptions seemed erratic in their use of (๒)/(๓), and I concluded that the two symbols were meaningless allographs.

Anthony Diller then said in the discussion at the Canberra conference that RK agreed with White Tai (WT), but the other inscriptions did not, and he has written that there is complete consistency in the use of (๒)/(๓) within RK, but not within the other inscriptions. Thus, the RK writing system preserved ancient distinctions which are still found in WT. This is where Diller gets the fifteen out of fifteen correspondences between RK and WT which he says could not happen by chance.<sup>131</sup>

It should be noted, however, that not every example of RK–WT identity is significant, but only those cases of RK identity with WT in the use of (๑) in agreement with Proto-Tai (PT) \*x, that is, RK apparent use of (๑) corresponding to WT /x/ where PT shows \*x which should produce (๑) in a PH language, such as Sukhothai, Ayutthaya, and modern Bangkok. The agreement of RK with WT in the use of (๑) against a proto-Thai \*kh is a quite different problem, as I shall illustrate below; the cases of agreement among RK, WT and PT in the occurrences of respectively (๑), /kh/, and \*kh contribute nothing to the discussion, for it is the normal reflex of \*kh whether in Sukhothai, Ayutthaya, or Bangkok.

We should note that there has been general agreement among linguists about this status of RK and Sukhothai. Whether they are of the faction who see Old Sukhothai as a language of the Lao–Sukhothai–Southern Thai group, or of the faction who wish to take Sukhothai as ancestor of Ayutthaya and Bangkok Thai, they agree that Old Sukhothai was PH, or at least B about to become PH. *Pace Diller*, “Consonant Merger 1”, p. 165, it is not just within the Chao Phraya Basin that \*/x/ and \*/kh/ merged, perhaps, as he wrote, under Mon–Khmer influence or as the result of Thai dialect mixing. Linguistic comparison and reconstruction, as outlined by Diller [“Consonant Merger 1”, pp. 166–8. (II.)], must lead to the conclusion that the merger of \*/kh/ and \*/x/ was a general characteristic of the PH (or B becoming PH) branch of SWT, predating Sukhothai, and predating the dispersal of those dialects/languages over the areas of Laos and Thailand.<sup>132</sup> The vocabulary and linguistic features of the Sukhothai corpus, and of RK, show that they are all quite normal representatives of the PH group of Southwestern Thai, and that they are not exotic offshoots of some other group, as is Saek.<sup>133</sup>

I said in “Piltdown 2” that the PH/P distinction was significant because it appears throughout solid contiguous areas, meaning that the split between Proto-PH (or Proto-B>PH) and Proto-P (or Proto-B>P), whether or not both devoiced at the same time or at different times, was established before they spread out from a rather small original area. Otherwise, if the distinction was not established, which means, if devoicing, or some significant allophonic distinction, had not occurred, we would expect a leopard spot pattern of PH/P differences, such as occurs within Mon–Khmer (for example between Mon and Monic Nyah Kur, respectively P and PH).

It seems to me also that we should accept that any feature shared by all PH languages against some or all P languages represents a feature which was already distinct at the time of the Proto-PH/Proto-P split. Otherwise, just as with respect to PH/P itself, we would find leopard-spot variations of that feature within PH rather than areal and typological solidity. Only PT \*kh may be reconstructed from the /kh/ of extant PH languages.

Of course, the /kh-x/ issue is not a question of PH/P devoicing. Both those velars are voiceless, but the PH/P grouping is significant because voiceless velars have different reflexes in P languages, but are identical throughout the PH group.

To be specific, all PH languages share identical treatments of Proto-PH velars, whereas within the P languages there are at least four types with respect to realizations of Proto-Tai velars, including velar clusters (1) Chiang Mai (with Shan), (2) White Tai, (3) Lü, and (4) Black Tai. In all PH languages, Proto-Tai \*kh, \*x, \*G, \*Γ, \*khl, \*khr have coalesced in /kh/, which should indicate that this was already a characteristic of Proto-PH at the time of the split between Proto-PH and Proto-P.<sup>134</sup> The divergent patterns among P languages indicate that they preserved more of the Proto-Tai distinctions at the time they diverged from Proto-PH and from one another, and as a result they show different realizations today. Contrary to Diller, "details of this derivational path [do] need ... concern us here".<sup>135</sup>

**Phase 2:** For my "Piltdown 2" I examined all available legible plates and made, I believe, two discoveries. First, the Lithai Inscriptions, especially No. 3, No. 5 and No. 8, show such consistency in use of (၁)/(၂) that their writers must have been conscious of meaningful regularity. They are in this respect like RK.

Inscription No. 2, of the same period, is not, which poses a problem worthy of attention, but it cannot be treated exhaustively until No. 2 has been taken out of its closet for close study and detailed legible plates produced.<sup>136</sup> There seems, however, to be sufficient evidence to conclude that No. 2 was really defective in its use of (၁), and this shows that the Sukhothai language did not make the (၁)/(၂) distinction.<sup>137</sup> The high regularity, but not perfection, of Lithai's inscriptions in this respect should be interpreted as strictly conventional, reflecting a writing system which had originated much earlier, or which had been borrowed from another language.

The comparison of No. 2, written by the monk Śrīśraddhā, a cousin of the Sukhothai kings, who might have been born in the reign of Rāmarāj and who wrote in the time of Lithai, with Lithai's inscriptions, proves that the (၁)/(၂) distinctions found in those inscriptions were no longer phonemic.<sup>138</sup> Given the short time span, we may assume that the same was true toward the end of the reign of Rāmarāj when Sukhothai was speaking the language which Śrīśraddhā and Lithai learned from their parents, and that even if RK was written then, its use of (၁)/(၂) was a learned spelling convention, inherited from an earlier period of the script.

We may not hypothesize, as I said above, that Rāmarāj, Śrīśraddhā (together with King Lōthai), and King Lithai, each spoke, between the 1290s and the 1350s, and attempted to write, in the latter two cases somewhat later, a different dialect.

I also found from plates (Bradley and *Śilā cārūk bhāg 1*) of RK that the distinction between the letters which are purportedly (ᵛ)/(ᵛ) in RK was very uncertain. I concluded that there were three relevant symbols, which I called kh<sub>1</sub> (the usual [ᵛ]), kh<sub>2</sub> (indentation on the vertical), and kh<sub>3</sub> (the usual [ᵛ]), and that even the expert readers of the past had been influenced by what they thought the symbols should have meant. I also believed that my new interpretations of RK script diminished the number of cases of peculiar agreement between RK and WT to 7 or 8.<sup>139</sup>

Dr. Prasert has commented that, "Vickery ... fails to take heed of Bradley's warning that the Schmitt transcript was ... inaccurate.... Based on Schmitt plates and other inaccurate data, Vickery makes a wrong conclusion."<sup>140</sup> Dr. Prasert has seriously misread what I wrote. I used the Schmitt and Montigny transcriptions of RK to show how others had read RK in the past. For my own readings of RK, I relied on the plates accompanying Bradley's article and on those in the Thai-language publication of *Śilā cārūk bhāg 1*, which Diller recommended to me shortly after the Canberra conference as the best available.

Dr. Prasert continued, "one may conclude that only kh<sub>1</sub> and kh<sub>2</sub> appear in RK. Kh<sub>3</sub> probably appears for the first time in Inscription No. 45 of 1392 to replace kh<sub>2</sub>." Dr. Prasert is perfectly correct that kh<sub>3</sub> appears for the first time within the Sukhothai corpus, except for RK, in Inscription No. 45. Another interesting feature of No. 45, to which I did not give much attention when writing my first two "Piltdown papers", is that its use of (ᵛ)/(ᵛ) corresponds completely to WT, and to PT, including 'mountain', written in No. 45 with (ᵛ), assuming Diller's suggestion that /khau/ 'mountain' is an extended meaning of /khau/ 'horn'.<sup>141</sup> That is, the PT initial for 'horn' is \*kh which regularly produces /x/ in WT and /kh/ in PH languages. Here, again assuming 'horn' > 'mountain', No. 45 shows a reflex which is regular in WT, but 'wrong' for a PH language.

Could this mean that Inscription No. 45 is White Tai? No, because in White and Black Tai 'mountain' is /pu/ and /pou/, PT \*buu.<sup>142</sup> If /khau/ ᵛ 'Mt.' is an extension of 'horn', it is only within the PH branch of Sukhothai, Ayutthaya, etc.; the *bhū* of Ayutthaya is a hyper-Sanskritized misconstrual of old Tai /bu/ >P /pu/, PH /phu/. Perhaps /khau/ 'Mt.' with (ᵛ) in Sukhothai, whatever its etymology, was to distinguish it from 'horn' in writing; this is more evidence that in Sukhothai the distinction between (ᵛ) and (ᵛ) was not phonemic.

Inscription No. 45 is thus further support for my suggestion that the Sukhothai corpus maintains the written distinction after it was lost in the spoken language, and that some of the correspondences with WT do not represent Sukhothai pronunciation, but were only spelling conventions. One example of a rather rare term in No. 45 is *xok*, 'edge, border', also written with (๑) in Lithai's No. 8, and showing the same initial phoneme in WT.<sup>143</sup>

Phase 3: In his two papers on consonant mergers, Diller has made two important contributions. He has introduced a new element to which I had previously paid no attention, Gedney's hypothesis of two more velar consonants in PT, and he has offered new readings from apparently better plates of RK. A conclusion of his first paper was "[t]he crucial point here is that the consistent *system of contrasts* in White Tai involving ... (kh) ... and (x-) ... incorporates *exactly the same system of contrasts* that one finds on Inscription One."<sup>144</sup> shall show below that this is not entirely true, and even to the extent it is true it is not so crucial to the RK problem as Diller thought.

With respect to Diller's new readings (CM-2), I accept that his characterization of /khap/ 'sing' is better than what I proposed, and that the intention was probably to write it with (๑). I also acknowledge an error in my description of the two occurrences of /khwaa/ 'right side'. What I should have written ("Piltdown 2", p. 27) was, "the word /khwaa/ 'right', in face 3, line 20, is written with a clear (๑) according to the conventions of face 1" (not "according to the conventions of faces 2 and 3"). I would maintain, however, that even in Diller's better illustrations, the two occurrences of *khv* are written differently. By Diller's criteria the one on face 1, line 5 is written with (๑), and that on face 3, line 20 shows (๑). They thus support my main point, that the word for 'right' is written both ways and is evidence for lack of phonemic distinction. On this point see further below.

I consider that the first occurrence of *khau* 'enter' in Diller's illustrations must either be taken as unclear or as showing both traits, a horizontal and vertical indentation, and both traits may be read from the same word on face 2, line 22. His first illustration of *khün* 'ascend' is illegible, and no distinctive trait may be read from the second occurrence of 'hang'. Indeed because of the way the two consonants *kh* and *v* are compressed, it could be argued that the distinctive vertical indentation on the first occurrence of 'hang' is uncertain. The spelling of this word must, therefore, be judged either uncertain, or showing both spellings, with (๑) and with (๑).

Another case of clear double spelling both by Diller's criteria and mine in "Piltdown 2" is /kha/, which I called 'slave' and Diller glosses as 'upland group'.<sup>145</sup> Finally the word for 'sell', which most readers of RK, including

myself, have treated as a (ᨶ) word, is clearly written with (ᨶ) by Diller's conventions, and in rereading my own description of it in "Piltdown 2", p. 26, I find that I should have interpreted it that way then, unless the very flat top horizontal is taken to represent a quasi-indentation. This is an example of what I criticized in others, allowing myself to be influenced by earlier readings.

In spite of these discrepancies, Diller maintained his conclusion of CM-1 cited above and insisted again on the fifteen out of fifteen cases of correspondence with White Tai.

I must emphasize that Diller did not try to describe, as I did, all of the relevant terms on face 1, but chose a selection which best illustrated the point he wished to make. And after writing CM-2, Diller wrote to me personally, saying, "maybe there are really four possibilities" (followed by drawings of respectively  $kh_1$ ,  $kh_3$ ,  $kh_2$ , and a  $kh$  with indentations on both vertical and top horizontal). Then, "In the present note, I have opted for the right upper jag on the vertical as definitive in making the White Tai correlation, with no attention to a notch on the top [upper horizontal], —but you may be right—certainly the top thing is important by the end of the 14th century."<sup>146</sup> Thus, after further thought, Diller seems to have come around to agreement with my general argument that the (ᨶ)/(ᨶ) written distinctions are not always clear in RK, even if the plates he used were better than those available to me and infirm some of my particular readings.

There is one more observation by Diller in his CM-2 which merits attention, both with respect to (ᨶ)/(ᨶ), and to the question of what early Bangkok authors might have conceived or written. Diller says that on King Mongkut's "hand-transcribed extract of Inscription One presented to Sir John Bowring ... in just the first seven lines, he [King Mongkut] has misread the *kho' khai* versus *kho' khuat* distinction at least twice", i.e., /*khi*/ 'ride' and /*khai*/ 'rice' written with *kh khuat* (ᨶ) instead of the correct *kh khai* (ᨶ).<sup>147</sup>

In this, Diller is mistaken about two details. The transcription in question was not the work of King Mongkut—at least no one has ever attributed it to him. It has been described as "First page of lithographed copy of the transcript prepared by the Commission", that is "a Commission of scholars under the direction of Prince Rkṣa", which in 1836 undertook the task of decipherment.<sup>148</sup> One of the most interesting details in this transcript is that all velar aspirates, all the initial consonants read as either *kh khai* or *kh khuat*, are written as *kh khuat* (ᨶ) according to Diller's criterion. They all show a rounded top with no indentation and a very clear jag on the right vertical. From this transcript it is impossible to know what the writer(s) considered the consonant he/they wrote to represent, but since in 1836 all such consonants had merged as /*kh*/ in Bangkok Thai, it is likely that they imagined it to be *kh khai* (ᨶ).





As I said in "Piltdown 2", the  $kh_2$  symbol of the Montigny plates is precisely the (๓) symbol of the Lithai Inscriptions, not the (๓) symbol used in RK, and this proves that the early Bangkok writers were familiar with at least one Lithai Inscription in Thai. This and the different line arrangement suggests that the Montigny text represents a draft before the inscription was put on stone.<sup>149</sup> In two salient details the Montigny text is not a copy of final RK, but a different version of an RK text.

The identifiable contribution of King Mongkut, in the copy which he sent to Bowring, consists of only 11 words in lines 1 and 2, all of which he read correctly except 'Surindrady' for 'Śrī Indrādity', the name of Ram Khamhaeng's father. This single error in an important name may not be ignored, however. If the inscription were not so full of anomalies, it might be considered sufficient evidence that King Mongkut and his contemporaries had no hand in it. As it is, I view it as evidence that the final stone inscription was preceded by more than one draft, with indecision about the name of the father of 'Ram Khamhaeng'. In annotating one draft for Bowring, in which the name is clearly written as 'Śrī Indrādity', Mongkut slipped up and wrote a name which may have been in another draft.<sup>150</sup>

I am quite in agreement with the Gedney hypothesis of two more velar consonants, and find it very relevant for the RK problem, but am surprised that Diller still thinks RK shows the magic fifteen out of fifteen improbable correspondences with WT, for with Gedney's hypothesis, the number is reduced to only three or four.

Gedney's hypothesis was not at all a result of the RK problem, but of strange correspondences between Northern Tai (NT) and SWT, in which the former showed evidence of original (PT) voiced initials and the latter unvoiced initials.<sup>151</sup>

An empirical result, which directly affects the RK problem is that those PT consonants reconstructed by Li as \*kh and \*x, each produce reflexes in WT with both initial /x/ and /kh/. Those with initial /kh/ in WT all have cognates in NT showing original voiced initials, while those with initial WT /x/ have NT cognates with voiceless initials. In PH languages they all result in /kh/.

Gedney's solution was to postulate two new proto consonants \*G, and \*Γ, of which the modern reflexes, in both WT and PH languages, are /kh/. Thus the new set of PT initials which are relevant to our subject are: \*kh, \*x, \*G, and \*Γ. The new lists of \*kh and \*x words are the lists of Li (10.2 and 10.6) minus those words which in WT begin with /kh/ rather than /x/. Those words are respectively \*G words, from the \*kh list, and \*Γ words, from the \*x lists. I emphasize that further work on this aspect of RK must use these new lists, One may not at one moment refer to Gedney's new series, and at another moment re-utilize the \*kh and \*x as first established by Li.

I think that some people may not have perceived the full extent of the implications for the history of Southwestern Thai of the new series of proto-Thai consonants which Gedney proposed, especially the \*G and \*T. I repeat, the lists of \*kh and \*x words, such as found in Li, must be reworked, with some words removed from them and put into new lists of \*G and \*T words.

In the Table below, Column 1 shows the fifteen words in RK, plus 'horn' for comparison with 'mountain', which are at issue, written with the initials which I read in that inscription. Column 2 shows the reconstructed PT initials as established by Li. Column 3 shows the initial consonants in White Tai; Columns 4–8 show how the 14 words (minus /(x)khun/) are distributed among 5 PT initials as established by Gedney and utilized by Diller in his latest papers.

1	2	3	4	5	6	7	8
Old List							
RK	*PT (old)	WT		*PT (new)			
			*G	*T	*kh	*x	*khl
x/khun (a title)	?	?					
khapp 'drive'	khut	ch					khlap
xapp 'sing'	kh	x			xap		
khut 'dig'	x	kh		khut			
xo 'hook'	x	x				xo	
xau 'enter'	x	x				xau	
xün 'ascend'	x	x				xün	
x/khween 'hang'	xw	xw				xwen	
x/khā 'kill'	kh	x			x/khā		
x/khau 'Mt.'	?				x/khau		
[khau 'horn']	kh	x					
khau 'rice'	x	kh		khau			
khī 'ride'	kh	kh	khi				
x/khw 'right'	khw	xw	khvā				
khāy 'sell'	kh	x			x/khāy		
kh 'slave'	khl	s					khlā

It can no longer be said that 'White Tai preserves the Proto-Tai distinction between \*kh and \*x'. White Tai does not preserve that distinction any more than does Bangkok. Both \*kh and \*x collapse into /x/ in White Tai, just as they collapse into /kh/ in the Southwestern PH languages. Thus, with respect only to Proto-Tai (\*kh and \*x), Southwestern PH, and White Tai, /kh/ and /x/ as reflexes of \*kh and \*x in the two latter are allophones.

This explains the apparent anomaly in White Tai script that Diller noted, that the White Tai equivalent of (၁) is used for /x/ and the White Tai (၂) equivalent is used for /kh/, the opposite of what seems to have been the case in Sukhothai, Ayutthaya, and Bangkok.<sup>152</sup> That is, White Tai, just like the ancestor of Sukhothai and Ayutthaya, took the original Indic kh symbol for the dominant velar, /x/ in White Tai, /kh/ in the PH languages; then in each type of script a special marked variant of the original kh symbol was devised for the other velar. This is one of the details pointing to an independent origin for White/Black Tai script. The distinction that White Tai preserves is between Proto-Tai \*G/\*T (White Tai /kh/) and Proto-Tai \*kh/\*x (White Tai /x/), all of which in Southwestern PH languages have coalesced into /kh/.<sup>153</sup>

Now where does that leave us with the list of fifteen RK words in question? First, two terms, /(x)-khun/ a title of nobility, and /(x)-khween/ 'hang' must be removed from the comparison for lack of relevant evidence. The term /(x)-khun/ is apparently not found in WT, nor do we know what the NT cognate, if any, is. It exists in Ahom, but written with *kh*, which is the Ahom reflex of both \*kh and \*x. Thus, the source of the spelling with (၁) in Inscriptions No. 1, No. 3, and No. 45 (but not, apparently in Inscriptions No. 2, No. 5, No. 15, and No. 107, where we find [၂]) cannot be explained. The source is not the *Three Seals Law Code* (see below) where *khun* is always written with (၁). Diller's suggestion that in WT it is represented by the term for 'hair' or 'fur' ('khhun') on Dieu and Donaldson's p. 174 is farfetched ('the hairy noble'—a Sampson syndrome?), as is the example of 'ai khhun hó' on their page 2.<sup>154</sup>

The term for 'hang' must be removed because of its uncertain readings in RK. Not only are the two examples slightly different at the crucial point, but the second element, /v/ is so closely attached to the /kh/ that the presence or absence of a jag on the vertical cannot be seen. It may not be argued that the writer must have meant (၁), for the lack of clarity in definition of these characters throughout RK means that nothing may be taken on faith. I also still maintain doubt about the spelling of /kh/ 'kill'; the two examples of /khau/ 'mountain' may reasonably be read as showing different initial consonants, which would mean that the writer of RK was unsure, and if it is genuine, indicates that the distinction was in writing only, not phonemic. The Lithai Inscriptions No. 3, No. 5, and No. 8 show examples of both terms with (၁).

Then, as should have been emphasized earlier, any RK words which are written with /kh/ representing a regular reflex of Proto-Thai phonemes (\*kh, \*khl, \*khr) must be removed from the magic fifteen for their comparison with WT is irrelevant. Such are /khap/ 'drive', /khii/ 'ride', and /khaa/ 'slave/enemy' in my version (Diller's 'upland group'). The first and last, moreover, are

not comparable with WT because in that language they have palatal initials, respectively /chap/ and /chaa/.<sup>155</sup>

Next, any \*G or \*T words which show /kh/ in RK evidence a normal PH reflex, and here comparison with WT is irrelevant. The terms for 'rice', a \*G word, with initial /kh/ in both RK and WT no longer represent unexpected agreement against a proto phoneme, but are the expected reflexes in each case. Another example is the \*T word for, 'dig', *khut*, which no longer goes back to a \*x initial.

Another surprising result of the new \*G/\*T hypothesis is seen in the terms for 'right' (*khw* or *xwā*), 'sell' (*khāy* or *xāy*), and 'sing' (*xapp*). In "Piltown 2", I treated the two occurrences of 'right' as spelled respectively with (ᨶ) and (ᨶ), in accord with the readings of Coedès, the Chulalongkorn transcribers and those who prepared the transcriptions in *Silpavatthanatham*. I would agree now with Diller that they may both be (ᨶ), if it is accepted that on Face 1 the -emic feature is a notch in the top horizontal, not the vertical, which is not Diller's position. Otherwise, there seem to me to be two spellings of /khwaa/ as I noted above. 'Right' is treated by Gedney and Chamberlain as a \*G word, in spite of the surprising reflex in WT, /x/ instead of /kh/, and Li's reconstruction of PT \*khw.<sup>156</sup> In any case, the expected reflex for PH languages is /kh/, and RK shows correspondence with WT, against the proto-phoneme. The case of 'sell' is the same, and here there is no question of \*G or \*T. The proto-phoneme is old-fashioned \*kh, resulting in /x/ in WT and normally /kh/ in PH languages. As we shall see, whether "sell" is written with (ᨶ) or (ᨶ) in RK, it makes no difference for the present discussion.

When Diller was using the RK-WT correspondences to prove RK authenticity, the important consideration was that the RK-WT agreement was also agreement with proto-phonemes, and evidence that RK maintained old phonemes which are now only known from WT, and lost in modern PH languages.

Now we see in RK three terms, 'right', 'sell', and 'sing', in Diller's reading, which show RK-WT agreement, but which could never have been pronounced that way in Sukhothai, a PH language, or at least a B language about to become PH.<sup>157</sup> The second of those terms, moreover ('sell' *xāy/khāy*) is spelled with [ᨶ] in Inscription No. 3, perhaps, and in Inscription No. 15. They represent, if deliberate, and if RK is genuine, a mere spelling convention, and are evidence for what I shall argue below, that the Sukhothai alphabet was taken from a WT-type language, and some of the WT conventions maintained for a time although they did not represent faithfully the Sukhothai language. To maintain the contrary, that the RK spelling represents Sukhothai pronunciation, means that \*kh, one of the most stable phonemes throughout SWT, showing /kh/

everywhere except in WT, at one time split, unconditioned, in the branch of SWT represented by Sukhothai, into /kh/ and /x/, which later recombined to /kh/. So far as I know, this is something which linguists consider impossible. Another such example would be the *xāt* (๒๑๓) 'break' of Inscription No. 3, although "break" seems also to be written *khāt* (๒๑๓) in another context of Inscription No. 3, which may indicate that, for the scribe in question, the two characters were allographs.

With respect to the plausibility of Bangkok fakery, these three terms represent either scribal carelessness, literati game-playing, or direct copying of another document, because their agreement with WT is not at the same time agreement with a proto-phoneme, which in all cases should have produced /kh/. With respect to 'sell' the documentary evidence is certain, as I shall explain.

Now what are the residual words which show RK–WT correspondence of ๒ and /x/, which is also in agreement with a proto-phoneme, and thus inappropriate for a PH language? They are 'hook', 'enter', and 'ascend', only 3 out of 15, and once we know that two other RK words show a (๒) which could never have represented a phoneme in that language, it is possible to assume the same for 'hook', 'enter', and 'ascend'. Moreover, the evidence of Bradley's reading is that in the 19th century /xo/ ๒๑ was not understood as 'hook', but as 'request', which was very frequently written then with (๒), even though that is not historically correct. This is relevant for the argument that a faker could not have gotten it right. He could have, as I shall demonstrate.

If RK is the work of early 19th-century fakers, they were writing 'hook' (xo, with [๒]) according to conventions of their time, or at least what they could have considered as conventions of an earlier language they wished to imitate.

In modern Thai, and as a normal development in SWT PH languages, at the latest, I believe everyone would agree, by 14th century in Sukhothai, the two words 'hook' and 'request' (/kho/) are perfect homonyms. The proto-initial in 'request' was /khr/, but that cluster, and the other PT velar-liquid clusters, seem not to have been preserved at Sukhothai.<sup>158</sup> In the old Ayutthayan laws of the *Three Seals Code*, recodified by Rama I in 1805, there are altogether 294 occurrences of those terms, 218 with (๒) and 76 with (๒). Of these 4 are 'hook' the remainder 'request'; of the contexts meaning 'hook' 3 are written with (๒). They are found respectively in the Palatine Law in a section on elephantry; in the Law on Witnesses describing a form of torture that involved tearing out eyes with a type of hook; on two occurrences, one with each consonant, in the Law on Treason in a section describing how execution by slow death should be conducted. There, a type of hook was used to force open the victim's mouth.<sup>159</sup>

Thus, for early Bangkok writers, (๗) was the predominant conventional way of writing 'hook' and 'request'. The same is true for /khaay/ 'sell', with 315 instances of (๗) and 161 of (๗) in the *Three Seals Code*; for those who might refuse to accept that "mountain" is written both ways in RK, the *Three Seals Code* shows 5 examples with (๗) and 8 with (๗), indicating that the former was an acceptable spelling in the early Bangkok period. We may for the present ignore the hypothesis that 'mountain' was a semantic extension of 'horn'. Whether that is correct or not, the spelling with (๗) was simply an acceptable early Bangkok convention, which, within the Sukhothai corpus, is also found in Inscriptions No. 8 and No. 45.

Now the remaining two words, 'enter' and 'ascend' are the only items requiring a faker to guess correctly (their occurrences in the *Three Seals Code* show overwhelming preponderance of [๗]), and the odds have improved to plausibility. Even if 'sing' and 'right' are included here, the number is increased to only four out of fifteen. It is probably not, however, necessary for us to rely on even improved odds. On the assumption that RK is genuine, they represent the adapted, but partially non-phonemic alphabet which Sukhothai had taken from a WT-type language. On the assumption that RK is a fake, and with my corollary that the fakers had to have been acquainted with some Lithai-period inscriptions, we have examples of both 'enter' and 'ascend' in Inscription No. 5, and of 'ascend' in No. 3 and No. 8.

### **'Echoes' of RK in The Sukhothai Corpus, or vice versa**

In this section I compare contexts of RK which closely resemble in their content, or in their language, other Sukhothai inscriptions. Traditionally, these contexts have been explained as 'echoes' of RK in the work of his followers, who must have studied his work. The comment also includes discussion of controversial terminology which did not appear in the earlier Piltdown papers.

Dr. Piriya Krairiksh has given much attention to these matters, and I have noted some of them in "Piltdown 1 and 2", but I wish to evoke them again in a different arrangement in an effort to better make the case that they are evidence for use of the Lithai inscriptions by the writer(s) of RK, and not echoes of the latter in the former.<sup>160</sup>

If each instance of similarity between RK and another inscription were considered in isolation, it would not be objectionable to assume that it was because the various inscriptions, all records of kings of the same polity and culture within a fairly narrow time span, less than one hundred years, treat

similar subjects from an identical point of view. But the cumulative effect of so many similar passages which yet differ in surprising ways is an impression that the similarities result from copying by persons who did not completely understand their sources, and that can only mean that RK is later than the others. If the copying were in the other direction, an assumption of incomprehension by Śrīsraddha or Lithai would be difficult to sustain.

First, No. 2 says in its discussion of Sukhothai royalty that King Śrī Indrādity had a son named Rāmarāj, which identifies that name with Ram Khamhaeng, whose father in RK was also named Śrī Indrādity. "Ram Khamhaeng, son of Śrī Indrādity" in RK also parallels part of the genealogy of the protagonist of No. 2, whose father is named Khamhaeng Phra Ram (คำแหง พระ राम / *gāṃheeṇ braḥ rām*), son of Śrī Indrapatīndrādity, an unhistorical expansion of 'indrādity'.<sup>161</sup> In No. 2 Rāmarāj is said to have built a great relic monument (*braḥ śrī ratnamahā dhātu*) in Śrī Sajanālai, an action ascribed to Ram Khamhaeng in No. 1. Both Ram Khamhaeng and the hero of No. 2 engage in heroic elephant duels with enemies, the details of which are rather similar.

In each case an enemy attacked the protagonist's father in an elephant duel, and the son intervened heroically to save his father from embarrassment or defeat. In each case the son stabbed the enemy's elephant. In No. 2 the term *deen* 'stab' is used, and in No. 1 *būñ* (modern spelling *buñ*), in the sentence "*kū būñ jāñ xun sām jan*", 'I stabbed the elephant of Khun Sam Jan'.

Strangely, Griswold and Prasert preferred to emend *būñ* to *rap buñ*, making the sentence 'I fought the elephant....'; and their reason cannot have been the 'incorrect' long vowel in *būñ* in RK, for their emendation also requires a short vowel.<sup>162</sup>

Another contextual similarity is the suspect list of vassals in Epilogue II of RK, which seems to be an effort to duplicate in more detail the area roughly claimed for the ancestor of No. 2's hero.

Even more indicative of copying are those passages in which nearly the same language is found. The left Column below is RK, and the right Column contains passages from Inscriptions No. 3, No. 4, and No. 5 of Lithai. The contexts are numbered by face and line in RK (1.22=face 1.line 22), and by inscription number/face.line in the right hand Column.

1.19–20 böan cūoñ[?] vvva pai gā khī' mā<sup>+</sup>  
pai xāy grai cakk grai' gā jāñ+ gā<sup>+</sup> grai  
21 cakk grai' gā mā<sup>+</sup> gā<sup>+</sup> grai cakk grai' gā<sup>+</sup>  
nōan gā<sup>+</sup> doñ gā<sup>+</sup>

3/2.32–brai fā khā dai  
khī rōa pai gā khi mā pai (khāy)

The passage from No. 3 has already been emphasized by scholars to show Lithai's fidelity to the ideas of Ram Khamhaeng. The phrase is translated, "the people go by boat to trade or ride horses to sell" (more literally, "ride boat go trade ride horse go sell", which as Griswold and Prasert wrote is "a sort of echo of Rāma Gāṃheñ's statement" in line 1/1.19-20, "they lead their cattle to trade or ride their horses to sell."<sup>163</sup> There is indeed a 'sort of echo', but which is the original, which the echo? The certain sense of No. 3 is that both boats and horses were means of transport for traders, while in RK it seems rather that the cattle being led were the objects of trade, and the situation of the horses is uncertain. The supposed masterwork is much vaguer than the assumed copy.

Even more questionable is the use in RK of *grai*, 'who, whoever'. In his comparison of White, Black and Red Tai, William Gedney showed that the equivalent terms in those languages were aberrant in comparison with 'khray<sup>1</sup>', 'who', because standard Thai /khr/ (\*gr) is cognate with /ch/ (c) in White, Black and Red Tai, a regularity seen in the near homonym in RK, *grai*, 'wish, desire'. Gedney explained the word for 'who' as "usually believed to be a contraction of khon<sup>1</sup> day<sup>1</sup> or khon<sup>1</sup> ray<sup>1</sup>", and, moreover, the "difference in tone [among the languages in question] suggests recent invention", although it would be interesting to know what Gedney then had in mind for the subjective concept 'recent'. Fang Kuei Li gives the same explanation.<sup>164</sup> This explanation, and the anachronism of RK, is not negated by the appearance of *grai* (ไกร) in inscriptions Nos. 45 and 15, where it has been glossed in "A Glossarial Index of the Sukhothai Inscriptions" as "(pro. who, whom, whose)." In Inscription No. 45 *grai* appears twice in the context *phū tai grai*. Griswold and Prasert rendered the first instance as "if either of us" "is untrue" (/bo si/), which lacks precision in not accounting for *grai* apart from *phū tai*. They construed the second context, *phū tai grai codanā*, however, as "If anyone [*phū tai*] wishes [*grai*] to complain [*codanā*]", revealing the true significance of *grai* in that late 14th-century Sukhothai text as 'wish', not 'who'. The first context would be more completely translated as "if either of us [*phū tai*] wishes [*grai*] to be untrue". Inscription No. 15 indeed seems to show *grai* meaning 'who', but it may not be relevant for it is dated in the 16th century. Its relevant context, however, is interesting. It says, in reference to a young woman who had been consecrated in a *vat* by her master, *grai grai lee ao ī keev nī pai xā xāy* (ไกร ไกร แล เอา อี้แก้ว นี้ ไป ข่าขาย), "whoever [*grai*] / wishes [*grai*] to take I Kees and sell [her]", which is a real echo of the passage in RK, or is it?

Another set of parallels which has received attention is RK lines 21-24 (following the statement about free trade which concludes the section discussed above).



phrai' fā hnā sai

22 lūk cau lūk xun phū+ tai leē lām tǎy hǎy kvā' yāv rōan ba' jōa+

23 sōa+ yām (คำ) mann jān xo lūk miyya yyia

khau+ phrai fā khā dai pā' 24 hmāk pā' phlū

ba' jōa+ mann vai+ kee' lūk mann sīn

3/2.43-5-brai fā khā dai

lūk cau lūk xun phū tai-----

tai khām ao yāv nāv ao rōan khau ba tǎy\_

hai vai (kee lūk bī tǎy hai vai kee) noñ

//5/1.18-19 ba tǎy vai kee lūk bī tǎy vai kee noñ

This section of RK has been rendered, "when any commoner (*brai fā hnā sai*) or man of rank (*lūk cau+ lūk khun*) dies his estate [enumerated] ... is left in its entirety to his son."<sup>165</sup>

This is to be compared with Nos. 3 and 5, translated in EHS 11-1, "when commoners or men of rank [die] ... he {the king} must not seize their estates; when a father dies, (the estate) must be left [to the sons; when an elder brother dies, it must be left] to the younger." The long bracketed passage is interpolated from No. 5.<sup>166</sup> This is an extremely loose rendering, and not only should the first ['die'] be in brackets, but ['he must not'] as well. Griswold and Prasert 'interpreted' this passage to conform to their ideas of what Sukhothai society must have been, as they understood it from RK. The words preceding 'estate' in No. 3 really mean 'has oppressed', perhaps 'seized'. Of course the lacuna in the stone could have contained an expression permitting a translation such as "he has not seized", but it is not legitimate to assume that.

The first question at hand, however, is not the translations of EHS, but parallels between the text of RK and other inscriptions.

In the present case, the passages contain enough identical or near identical elements to permit an inference that one of the authors (of Inscription No. 1, No. 3, or No. 5) must have studied the work of another, perhaps not entirely understanding what he read; with this in mind, it is noteworthy that Nos. 3 and 5, aside from the lacuna in the former, have caused no translation difficulties, while the passage from RK has been nothing but a headache.<sup>167</sup>

It is not even certain that the two versions imply the same institutions. The RK text, if translated completely and literally (ignoring some vocabulary difficulties), implies entailed primogeniture: when a man dies "his estate—his

elephants, wife(s) [*miyya*], child(ren) [*lūk*], granary(ies) [*yiya*], rice [*khau*], retainers [*brai fā+ khā+ dai*] and grove(s) of areca and betel—is left in its entirety to his son.”<sup>168</sup>

Coedès’ less literal translation confuses the issue. He subsumed wife(ves) and child(ren) under “sa famille”, and says all was left to “ses enfants”. It is impossible, though, that the children of the defunct could be both heritage and inheritors, and it must be assumed that the author’s intention was that all surviving dependents of the dead man were left to a single inheriting child, presumably the ranking son. An incoherency, once it is established that *brai fā+ hnā+ sai* is a fantasy calqued on *brai fā+ khā dai* (see above) is that a class of people who were “retainers” (EHS 9) or “esclaves” (Coedès), could have enjoyed the same property rights as nobility.

Inscription No. 3 and No. 5 reveal quite a different situation. They indicate that property passed through siblings of one generation before going on to the next, something much more in conformity to Southeast Asian institutions as they are understood from other sources. Of course institutions change, but one may wonder if they changed so completely during the seventy years separating ‘RamKhamhaeng’ and a grandson who, assuming RK’s authenticity, so assiduously studied his grandfather’s records.

It should be emphasized that succession through members of the same generation was an ancient institution in Southeast Asia, including Ayutthaya, and the primogeniture of RK is a detail which is suspect.<sup>169</sup>

Moreover, there is another possible translation for No. 3, which is more in line with a subordinate position for *brai fā khā dai*. Their juxtaposition with, but preceding the designation of the upper classes, *lūk cau lūk khun*, suggests that the phrase might be construed as “*brai fā khā dai* of the *lūk cau lūk khun*”; and that it was perhaps those ‘retainers’ who are passed in inheritance from father to son and from elder to younger brother.

In fairness though, it must be recorded that Inscription No. 10, possibly from Phitsanulok and dated 1404, just 45 years after Lithai’s writings, contains near its end a phrase, *bo tǎy vai kee lūk lūk tǎy vai kee hlān hlān tǎy vai kee hleen*, “father dies leave to child, child dies leave to grandchild, grandchild dies leave to great grandchild”. The damaged condition of the stone does not permit a conclusion as to whether this is a statement of general legal principles, whether it refers to personal property or to a position, or even whether it is relating what happened in a particular case.<sup>170</sup>

Besides the institutional problem, there are vocabulary difficulties in the passages in Nos. 1 and 3. Below, I juxtapose the translations of Bradley, Coedès, and Griswold / Prasert, in order to show that some of this passage in RK may in fact be incomprehensible.<sup>171</sup>

First, RK, face I, line 22, *lam<sup>+</sup> t̄āy h̄āy kvā*. Bradley translated it as “dies (*lam<sup>+</sup> t̄āy*) or disappears from” (*h̄āy kvā*).

Coedès (“Notes critiques”, p. 2), thought that *kvā* should be considered equivalent to the Dioi /*kva*/, which is equivalent to /*sia*/ in Standard Thai. In Dioi *té[t̄āy] kva lew*=‘he is dead’; and the phrase of RK should simply mean ‘tombe malade et meurt’.

In EHS 9 Griswold and Prasert (p. 206, n. 27) accepted Coedès’ version, adding that in Shan *kvā* is ‘go’ and RK *lam<sup>+</sup> t̄āy h̄āy kvā*=mod. Thai *lom h̄āy t̄āy cāk*. Indeed, *kva/ka* is ‘go’ in Shan and in Ahom, where it is also a post-verbal particle indicating past tense, as Coedès reported for Dioi.<sup>172</sup>

For the entire passage there has been considerable difference of opinion. In the juxtaposition below B=Bradley, C=Coedès, and E=EHS (Griswold and Prasert).

RK: *phū<sup>+</sup> tai lee lam<sup>+</sup> t̄āy h̄āy kvā ?yāv<sup>+</sup> rōan*

B: if any one soever dies or disappears from house and home

C: si [anyone] tombe malade et meurt, | la maison... (Coedès considered that *?yāv rōan* belonged with the following phrase as part of the estate).

E: When any [person] dies, his estate [items listed: *?yāv<sup>+</sup> rōan ba jōa<sup>+</sup> sōa<sup>+</sup> yām<sup>+</sup> mann jān xo lūk*]. Thus E followed C on this point.

The segmentation in the three versions is different, as is the significance of the terms interpreted as ‘house’.

RK: *ba jōa<sup>+</sup> sōa<sup>+</sup> yām<sup>+</sup> (𑜀𑜢𑜤𑜰𑜫)mann*).

B the Prince trusts (*jōa<sup>+</sup>*), supports (*yām<sup>+</sup>*), aids (*sōa<sup>+</sup>*) him (*mann*)

C (continuing from ‘la maison’ ... de ses pères (*ba jōa<sup>+</sup>*), ses vêtements (*sōa<sup>+</sup> gā*) [Coedès read *yām* as *gā* ‘stick to’—see comment below]

E The deceased (*sōa<sup>+</sup> yām<sup>+</sup>*) father of the family (*ba jōa<sup>+</sup>*) himself (*mann*). This translation is in their note 28, p. 206, but left out of their running translation because they considered it, obviously, as redundant [not to say incoherent—MV]. Note that they did not accept Coedès’ reading of *gā* for *yām*.

After this we do not need to consider Bradley’s version, for Coedès showed that it was certainly inaccurate.

RK: *jān<sup>+</sup> xo lūk miyya yyia khau<sup>+</sup> phrai fā khā dai pā<sup>+</sup> hmāk pā<sup>+</sup> phlū*

C. ses éléphants, ses enfants et ses femmes, ses greniers de riz, ses serviteurs ... plantations, etc.

E. elephants, wives, children, granaries, rice, retainers ... groves, etc.

RK: *...ba jōa mann vai<sup>+</sup> kee lūk mann sin<sup>+</sup>*

C ...de ses pères [*ba jōa<sup>+</sup> mann*], (le roi) les conserve en totalité aux enfants

E is left in its entirety to his son.

If one thing seems certain from the enormous differences in the three

translations, and the justificatory comment accompanying them, it is that this passage is anything but straightforward Thai. This did not trouble Bradley because of his belief in 'poetic' language which did not have to have a clear meaning. Bradley certainly went wrong after his "trusts, supports, aids"; it was unreasonable to translate *ba* as 'prince', even if Coedès had not shown the correction of *jōa*, 'trust', to *jōa*<sup>+</sup> 'lineage'.

Coedès' construal of *sōa yām*, however, which he read *sōa*<sup>+</sup> *gā*<sup>+</sup>, is less satisfying. His explanation of it as 'clothing', by derivation from a Lao expression about 'clothing attached to the body' (*gā*, /*khaa*/ = to stick to), is weak because it is not shown that the doublet means 'clothing' in any dialect. Bradley had read the word as *yām*, which he translated 'support'; this reading, with *g*<\**γ* (𑜀) has been retained in all modern versions of RK. EHS 9, p. 206, n. 28, explains it as "a euphonic filler, or else for *gām*, 'support' ". It must be noted that the word in question is written with *g*<\**γ* (𑜀) *kh khon*), which, of the two obsolete velars, is the one most likely to have been significant. Certainly it was used with some regularity in early Sukhothai. Thus *yām*<sup>+</sup> in this context should not be interpreted as either 'stick to' (*g* < \**g* / 𑜀) *kh khvāy*), or 'support', which as a Khmer loan word ( 𑜀 ), would never have been written with *kh khon* (𑜀).<sup>173</sup> The only word written 𑜀 which fits the context is 'gold', and it is strange that no one seems to have thought of it, for it fits very well in a list of property left by a dead man to his heir(s), particularly in the translation of Coedès who treated *sōa*<sup>+</sup> as 'clothing'. This was impossible for Griswold and Prasert, however, for they glossed *sōa*<sup>+</sup> as 'a deceased person' by analogy with the term *phī sōa*, a type of ghost, an *ad hoc* guess which requires textual support from other contexts to come at all close to plausibility. Their proposal also meant that *yām* had to be treated as a nonsense word, and their solution is very unsatisfactory.

Both Coedès and Griswold/Prasert acknowledged that there were problems in the organization of this passage of RK, which are reflected in the three translations, and I suggest that the reason is the composition of RK based on poorly understood readings of partly damaged older texts which were hypothetically reconstructed by the author(s) of inscription RK.

In "Piltdown 2" (p. 43) I commented that the poorly understood word *khām* (𑜀) in No. 3, whether interpreted as 'support' (Coedès) or 'to tyrannize' (Griswold and Prasert), represented a case of confusion of voiced and voiceless velars indicating that the former had devoiced. The comparable word in No. 1, written there with *kh khon* (voiced / \**γ*/) was construed by Griswold and Prasert as either a euphonic filler or as 'support'.

Dr. Prasert has denied the relevance of this. He says that in No. 3 the term may not be translated as 'support', which would mean that No. 3 is less an 'echo'

of RK than previously assumed. He maintains that the correct translation is 'tyrannize', assimilating ขำ to a Chiang Mai dialect word คำ written with a voiced consonant, adding that "as Diller shows this letter [*"kho khon"*, in fact *kh khvāy*] merges with *kho khai* in WT and it is written with *kho khai* in Inscription No. 3". Seemingly, Dr. Prasert has confused letters with phonemes. The letters in question did not merge. The PT phoneme /*\*ɣ*/ has in WT merged with /*x*/, not /*kh*/. Merger with /*kh*/ is characteristic of PH languages, such as Sukhothai, Ayutthaya, and Bangkok. But whatever the merger, it would represent devoicing.<sup>174</sup>

RK continues, lines 25-27, with a passage on disputes which is conventionally translated in its entirety, "when commoners and men of rank differ and disagree, [the king] examines the case to get at the truth and then settles it justly for them."<sup>175</sup> The phrase translated as 'differ and disagree', *phit pheet seek vān* bears some resemblance to line 19 of No. 5, *zū phū tai phit vān*, which Griswold and Prasert have translated, "when anyone quarrels with him [i.e., the king]".<sup>176</sup>

phrai' fā<sup>+</sup>

25 lūk cau<sup>+</sup> lūk khun phī<sup>+</sup> lee<sup>+</sup> phī<sup>+</sup> pheet  
zeek vān<sup>+</sup> kann svān tū 26 dee<sup>+</sup> lee<sup>+</sup> cīn leen  
gvām k(ee)' khā tvy<sup>+</sup> sū' pa' xau<sup>+</sup> phū<sup>+</sup> lakk  
makk 27 phū zon'

5/1.19-21 zū phū tai

phit vān .nīrām dau tai ka tī pa hon khā vann  
sakk jāp

In their detailed explanation of the RK passage, Griswold and Prasert identified '*vān<sup>+</sup>*' as 'wide' "or else equivalent to *pān* 'to tear' " and in their translation of No. 5 they were influenced by their interpretation of RK, which led them to assume that *vān* (วาง) in No. 5 really meant *vān<sup>2</sup>* (วาง).<sup>177</sup> This is not necessary, and probably wrong.

A look at the *Three Seals Law Code*, which would have been well known to Bangkok scholars, might suggest something else. In its *pet srec* laws the expression *phit pheet* (ผิดแพ่ง), apparently to be construed as 'quarrel', occurs 4 times, twice followed by *zeek ān* (แซก อ้น).<sup>178</sup> In each of those cases the question is of quarrels among people (*dvay rāṣṭar phū<sup>2</sup> tai* [art. 139] / *ph<sup>2</sup> tai* [art. 167/146]), who are accusing (*phit pheet zeek ān*) one another (*kee kán / kán*) of something. In art. 139, it follows a case of a claim relating to wages owing to persons who were

murdered and in which circumstances it was difficult to determine against whom a case (*phit pheck*) should be laid. In art. 167 / 146, it is a question of “whoever (*phū<sup>2</sup> tai*) in a quarrel (*phit pheck zeek āñ kán*) hires [and] requests (*cāñ vān* / จ้าง วาน) a specialist in magic (*hmo*) to do (*hai<sup>2</sup> kra:dām*) ... something.”

The meaning of *phit pheck* as ‘quarrel’ comes through clearly in its other two contexts. The first is in the same art. 139, in the explanation of the case, “it is impossible to determine whom the evil persons who stabbed and killed the three dwarfs had a quarrel with” (ผู้ร้ายแทงสามเทีรตายจะได้อมีผิดแพกด้วยผู้ใดหามิได้). The other context, in art. 140, is, “whoever (*phū<sup>2</sup> tai*) ... quarrels (*phit pheck tā<sup>1</sup> kán*)” followed by instructions concerning the investigation.<sup>179</sup>

The term *zeek* also occurs once more in the laws in a passage which reveals its meaning, too. In “Crimes Against the State” (*ājñ hlvañ*), art. 65 deals with falsification of documents in terms of removing parts of the genuine text and inserting other wording.<sup>180</sup> In the first statement, the terms used are *lap* (‘cut out’) ‘the words, evidence’ (*gāram* [การณ]) and ‘*siat*’ (‘put in’) ‘other words’ (*gāram ün*). This is followed by *lap* (‘cut out’) *sāmnvan bicāraṇā nai kra:lākār sia* (‘the text of the discussion in court’) and *zeek* (‘insert’) *kho<sup>2</sup> gvām ün<sup>1</sup> sai<sup>1</sup>* (‘other material’). This context, in which the standard usage of *zeek* is clear, immediately reveals its etymology, from Khmer *jrek* (‘intervene’).<sup>181</sup> The term *āñ* is also Khmer, meaning, as in Thai, ‘to claim, allege’, and the expression ‘*zeek āñ*’ may be understood as ‘intervene with a claim against someone’.

These are the only contexts of either *phit pheck* or *zeek* in the entire *Three Seals Code*, and if their recorded dates, equivalent to A.D. 1362 / 63, are even approximately accurate, they are probably old legal terms no longer used in late Ayutthaya or Bangkok.<sup>182</sup> The term *zeek* seems no longer to be part of standard Thai vocabulary, except in the expression *zok zeek* (ซอกแซก), which So Setaputra glossed as ‘to be’ (of an investigative nature), with *diav* ‘travel’ (to little known places), and with *rū<sup>2</sup>* “well-informed on little-known things”, in all of which the old sense of *zeek* < *jrek* comes through. In Wit Thiengburanathum’s glosses it is even clearer, “to edge one’s way through”, “prying”, “devious”.<sup>183</sup>

The RK context, which bears a superficial resemblance to the law contexts, differs in two terms, *seek* (แสก) for *zeek* and *vāñ<sup>+</sup>* (ว้าง) for *āñ*. In modern Thai, *seek* and *zeek* are both pronounced /saek/, but literati would be unlikely to confuse them if both were still in current use. Whether confusion would be considered possible in 13th-century Sukhothai depends on respective linguists’ views on sound change. I believe most linguists, particularly those who defend Ram Khamhaeng, would consider it unlikely, but in this case that problem may be skirted.

The term '*seek*' is also found once in the old laws, in the expression *seek hnā<sup>2</sup>* (แสกหน้า), one of the killer points in the body which if struck may cause death, and this agrees with the modern dictionary definition, 'the median line of the skull or of the face'.<sup>184</sup>

Thus the phrase in RK, '*phit pheck seek vān<sup>2</sup>*' seems to be nonsense, and the translation of Griswold and Prasert, with '*seek*' construed as 'to part' and *vān<sup>2</sup>* as 'wide' is strictly guesswork out of mystification without reference to other possibly helpful contexts. As for No. 5, the context is probably totally different, and *vān<sup>2</sup>*, should not be amended to *vān<sup>2</sup>*, but construed in its normal sense as 'to lay a plan', 'to plot', and the entire phrase, to the extent comprehension in spite of damage is possible, should be interpreted, 'if anyone' (*zū phū tai* ชี้ผู้ใด) 'does wrong / commits a fault (*phit* / ผิด) [and] 'plots' (*vān* / วาง)---[damage] ? (---*hrām* / งาม) 'to whatever extent' (*dau<sup>1</sup> tai* เท่าใด).

There is nevertheless, I believe, a connection between No. 5 and RK. It would seem that Bangkok literati had seen No. 5, and they of course were familiar with the old law code, though perhaps not all old terms were completely understood. They conflated the *phit* followed by *vān* of No. 5 with the *phit pheck zeekān* of the laws, and created the *phit pheck seek vāān<sup>2</sup>* of RK.<sup>185</sup> Even if the Khmer character of *phit pheck zeek ān* might not be unusual in 13th to 14th-century Sukhothai, it is equally plausible as Ayutthayan terminology.

The continuing contexts of RK and No. 5 differ, the former, after a phrase irrelevant to the present discussion, going on (lines 27-28) with, "when he sees someone's rice he does not covet it, when he sees someone's wealth he does not get angry"; paralleled exactly in No. 5.16-17.<sup>186</sup>

hen khau<sup>+</sup> dān' pa' grai' bīn hen sīn dān' pa'  
grai tōat

5/1.16-17 rū prānī kee brai fā khā dai daññ  
hlāy hen khau dān pa grai bīn hen sīn dān pa  
grai tōt

In No. 5, however, this is followed by the passage on inheritance. The RK then continues with a long passage on other rulers who come to seek asylum; this seems to be paralleled in No. 3 by a much shorter and badly damaged passage, which cannot be completely interpreted, but which at least contains, *tān pān tān mōaṅ cakk m bö mā in tan*, "...other villages and mōaṅ come to rely and lean on him".<sup>187</sup> Inscription No. 5 also contains a similar statement, but which is there within a different context, as illustrated further below.

28 gan tai kh'ī jān+ mā hā bā möaṅ mā sū joy'  
 hnōa fö 29-a kū+ mann pa' mī jān+ pa' mī mā+  
 pa' mī pvva' pa' mī nān pa' mī nōa 30-n pa'  
 mī doṅ hai+ kee' mann joy' mann tvaṅ pen bā  
 n+ pen möaṅ

3/2.40–42 phi vā ... n tñn pān tñn möaṅ cakk  
 mā bōṅ mā in tan ph.....vā sai tan pai kha bōṅ  
 möaṅ dān...

5/1.23 ñòm ao mā lyaṅ mā khun pa hai thōṅ  
 dī chip dī hāy

Then RK continues with a passage nearly identical to the continuation in No. 5 from the passage on conflict discussed above, “[if] he gets [enemy] soldiers, however aggressive, he does not kill or beat [them]”; while No. 5 first says of the people in dispute, “he never kills them at any time”, and goes on, “if [he] gets [enemy] soldiers, however aggressive, he does not kill or beat [them], [but] is willing to keep and care for [them]”.<sup>188</sup> These passages are as follows:

31-n. tai+ khā+ sōak khā+ sōa hvva bu'ñ hvva  
 rap ka' tī pa' xā+ pa' tā

5/1.21–23 zū tai kh sōk khā sōa hvva buñ  
 hvva rap ka tī pa khā+ pa tī ñòm ao mā lyaṅ  
 mā khun pa hai thōṅ dī chip dī hāy

In EHS 9, p. 208, n. 47, Griswold and Prasert followed Bradley in stating that *khā+ sōa* “is only an alliterative pendant”. This is probably inaccurate. In Black Tai there is an expression ‘*sūk sūa*’ which means ‘enemy’ and in Ahom there is a term *sū* ‘army’.<sup>189</sup> There is no reason for the hesitancy of Griswold and Prasert over *hua buñ hua rap* ‘fighters’ in EHS 9, n. 47, suggesting that the *hua* were of higher rank than those designated as *khā*, for there is still in modern Thai a perfectly good expression, ‘*rap buñ*’ (รบพุ่ง), to wage war, while *hua* in this context is much better explained as “one whose attitude is” [i.e., *hua kau* (หัวเก่า) ‘old-fashioned’, *hua kheen* (หัวแข็ง) ‘obstinate’, *hua khamoy* (หัวขโมย) ‘habitual thief’; by analogy *hua buñ hua rap* (หัวพุ่งหัวรบ) ‘those who are aggressive, fighters’], rather than ‘leader’ in contrast to *khā*.



I submit that the 'echoes' discussed above are better explained as familiarity of the authors of No. 1 with Inscriptions Nos. 3 and 5, than as imitations of No. 1 by writers of the Lithai period, particularly when the latter avoided all the special script and spelling conventions found in No. 1.

There is one more case of textual oddities which I have emphasized in my earlier papers, but which deserves attention again now. These are the references to the monks allegedly invited by 'Ram Khamhaeng' and by Lithai, respectively from Nakhon Sri Thammarat and from Ban in lower Burma's Mon region.

2.29 ...oy dān kee mahāthera saṅgharāj prāj  
ryan cab bitakatray hlvak kvā pū grū nai mōaṅ  
nī,

5/2.20-21 ...añjeñ mahāsāmī saṅgharāj mī  
sīlācār lee rū brah pitakatray ... nakk fūn mahā  
sāmī ann ayū nai ... laṅkāḍib...,

4/2.12 [khmer] ... añjeñ mahāsāmī saṅgharāj ta  
mān sīl ryyan cab brah pitakatray ta siṅ nau  
laṅkāḍvib ta mān sīlācāryy

The passage from RK has been translated, "gift to the Mahāthera Saṅgharāja, the sage who has studied the scriptures from beginning to end", but it could be more literally, "gift to the Mahāthera Saṅgharāja the sage who has completely learned the Tripitaka better than the monks in this country".

Number 5 can be rendered, "to invite a mahāsāmī saṅgharāj virtuous and who knew the Tripitaka ... the crowd of mahāsāmī who were in ... Laṅkāḍvip...."

And No. 4 is, "he invited a mahāsāmī saṅgharāj who had virtue and had completely learned the Tripitaka, and who had lived in Laṅkāḍvip".<sup>190</sup>

The versions of Nos. 4 and 5, not surprisingly, are virtually identical, although in different languages, being contemporary records of the same occasion. It is less expected to find RK imitating the Khmerism *ryan cab*, "learn completely".<sup>191</sup>

This is not just a case of a common use of Khmer in Sukhothai and Ayutthaya for religion and ritual, as Dr. M.R. Suriyavut suggested.<sup>192</sup> It is extremely peculiar that RK resembles Lithai's Khmer more than Lithai's Thai. Under the traditional assumption, that the 'echoes' in Lithai's inscriptions were because he, or his scribes, studied the work of his grandfather, we would expect him to imitate this phrase from No. 1 in his Thai-language No. 5, perhaps in Khmer in No. 4, too, but at least in Thai. Finding this correspondence with RK only in the Khmer No. 4, which King Mongkut had removed to Bangkok from Sukhothai, is very strong evidence for late composition of No. 1.

Betty Gosling has misrepresented the argument, which is not about the date of Khmer influence in Sukhothai, but about the relationship of Lithai's texts to that allegedly formulated by his grandfather. Moreover, Gosling's "the late thirteenth century ... ['RK'] is much closer to Sukhothai's period of Khmer political and cultural domination ... than are Inscriptions Four and Five", is, with respect to 'Khmer domination', still a hypothesis, although generally accepted. If true, then the RK inscription is in this respect, too, peculiar, in showing so few Khmerisms, far fewer than in later Sukhothai and Ayutthaya work. One of the few is the parallel with Inscription No. 4 evoked above. Inscription No. 4 proves the cultural and administrative importance of the Khmer language in Lithai's time, even when there is no longer any question of political hegemony from Cambodia.

Another detail of the same type is that in No. 1 the monk is called a mahāthera, but in the passages cited above from Nos. 4 and 5 the monk in question was a mahāsāmī. In No. 4, however, he was also called mahāthera in two other contexts, but never in No. 5.<sup>193</sup>

Another item in this passage which deserves attention is 'kvā', used in the above passage from RK in its modern Thai sense of 'more than', whereas in the previous passage on inheritance it was construed in the Ahom or Northern Tai sense as 'pass', 'away', or final particle of completed past time. Investigation is required as to whether a language with 'kvā' in one of these senses would also have it in the other. Standard Thai, and Ahom material available to me suggests not, and if this is the general situation the unexpected use of kvā discussed above might be considered as an artificial exoticism introduced to give an air of spurious antiquity. In Ahom the comparative, 'more than', 'better than' is expressed by *khüñ* / khün; in White Tai the post-verbal particle is / cá /, 'to be past' is / cai /, and 'more than' is / ho'n /.<sup>194</sup>

Some interesting contextual comparisons between No. 1 and Nos. 4 and 5 may also be made. To receive his monk, King Lithai had a monastery built in the Mango Grove to the west of Sukhothai; Ram Khamhaeng installed his monk west of the city in the Araññika. In Lithai's inscriptions a bronze statue the size of the Buddha was installed in the middle of the city, and in No. 1 "[i]nside this city ... there is/are statue(s) 18 cubits in height" (the size of the Buddha); this/these is/are clearly distinct from the 18-cubit statue in the Araññika.

In Nos. 4 and 5 the king was ordained as a monk in the Golden Pavilion (hemaprāsāda) of the palace, and a golden statue was installed in another palace building, the *rājamandīra* 'royal palace'. This passage suggests a source for the two *sālā* 'pavilion' of No. 1, the 'Golden Sacred Image [*brah*] Pavilion' and 'the Buddha Pavilion', as well as the "golden statue(s) of the Buddha", which were "inside this city".

The great festival described in Lithai's inscriptions at the close of the rainy season retreat, with gifts for the monk, corresponds to a *kathin*, and there are clear resemblances between it and the *kathin* described in No. 1. In this connection another Khmer element in No. 1 which may be suspect is "*krān kathin*", literally 'spread the *kathin*' (*krān* < *krāl* 'spread [a cloth, mat, etc.]' in Khmer), found twice in lines 2.13, 14. I had not noticed this in my previous papers, and attention was called to it by Michael Wright.<sup>195</sup> I considered that this expression in No. 1 would seem to be another 'echo' of Lithai's Khmer No. 4/2.26, *krāl nu bastrā* 'spread out cloth' for the *mahāsāmī* to walk on, at which time there was another great festival, equivalent to a *kathin*.

Then, in personal correspondence, Betty Gosling called my attention to the fact that *krān kathin* is a particular ceremony in which the cloth for monks' robes is stretched out preparatory to cutting and sewing.<sup>196</sup>

Given this circumstance it is peculiar that in their translation of No. 1 Griswold and Prasert, who must have known the dictionary definition of *krān kathin*, translated *krān kathin* in the first instance as 'celebrate the *kathin*', and they omitted the second entirely.<sup>197</sup> It is also surprising that in his answer to Wright, Dr. Prasert did not cite the genuine ceremony of *krān kathin*, but instead argued that there was no problem with such use of Khmer in relation to Sukhothai Buddhism, because Jayavarman VII of Cambodia, whose rule may have extended over Sukhothai in the late 12th century, was already Buddhist. Did Griswold and Prasert consider that the RK context referred to the entire *kathin* celebration, and not just to the preparation of the cloth? That would seem to me a reasonable argument, and evidence that in RK the expression was misused.<sup>198</sup>

I might add that there is no mention of *kathin* at all in the inscriptions of Angkor; the absence of the term 'kathin' in Nos. 4 and 5, even when a festival like that now called 'kathin' was being celebrated, might suggest that it was not yet in use in Sukhothai Buddhism, in which case No. 1 is guilty of an evident modernistic anachronism.<sup>199</sup>

Finally, RK and No. 3 each have a passage listing other inscriptions which had been set up. In RK, three inscriptions are listed, in Chaliang beside the Sacred Relic, in the cave of Brah Rām beside the Sāmbāy River, and in the cave of Ratnadhār. Of these locations only Chaliang is known at all, and it is considered to be the temple site in the bend of the river just east of the old city of Sri Satchanalai.<sup>200</sup> Inscription No. 3 apparently gives quite different locations for the four inscriptions, said to contain more detailed treatment of the matters recorded in No. 3: one in Sukhothai beside the Mahādhātu, one in Mōaṅ ..., one in Mōaṅ Fāṅ, and one in Mōaṅ Sralvaṅ.

3.22 carik ann nūn mī nai möaṇ jalyaṇ  
 sthabak vai+ tvay+ braḥ śrī ratnadhātu  
 carik ann nūn mī nai thām+ jū thām+ braḥ  
 rām ayū faṇṇ nām sāmbyā  
 cārik ann nūn mī nai thām ratnadhār

2.48-50 cārik ann mi nai möaṇ  
 sukhodai—nakk braḥ mahādhātu būn lee  
 cārik ann niṇ mi nai möaṇ—  
 ann niṇ mi nai möaṇ fāṇṇ  
 ann niṇ mi nai möaṇ sralvaṇ

The two locations in the last two lines of No. 3 are considered to be near Uttaradith and west of Phitsanulok, respectively. None of the inscriptions mentioned in either RK or No. 3 has ever been found. If we consider these passages from the point of view of copying, the writers of RK intended that their inscription be from Sukhothai, and thus they substituted Jalyaṇ where Lithai, 'writing from' Kamphaeng Phet, said the first of his more detailed inscriptions was in his seat of government, Sukhothai.

I submit that these similarities are better explained by the hypothesis that the writer(s) of No. 1 had examined originals or copies of Inscriptions No.2, No. 3, No. 4, and No. 5, and perhaps others, had not entirely understood them, and composed parts of No. 1 as restorations of the details they read in the others.

It is certain from the script of the Montigny plates discussed above, in particular the shape of the *kh khai/kh khuat* [(๓)/(๓)] used there, that they had seen and imitated at least one Lithai-period inscription, and that detail cannot be attributed to No. 4, because the special form of that symbol is found in Lithai's Thai script, but not in the Khmer of No. 4.<sup>201</sup>

There is no particular mystery about their access to those inscriptions. King Mongkut's grandfather, King Rama I, had over 1200 Buddha images brought to Bangkok from the northern *mōaṇ*, including Sukhothai and Srī Satchanalai.<sup>202</sup> Some of those images had inscriptions written on them, and it is reasonable to assume that curiosity about them, if not already present, would have been awakened. It is likely that copies, more or less accurate, were made, and the palace scholars of early Bangkok probably had access to them. There was also some tradition of copying and trying to read old inscriptions among the monks at Sukhothai, as described at the time of Prince Vajiravudh's visit in 1907.<sup>203</sup>

One more 'echo', which has not previously been evoked as such, and the RK occurrence of which has been brought out as evidence in favor of RK authenticity, is *braḥ khbuṇ* (พระ ขบุ่ง [RK spelling ข.บุ่ง]). In his "The Efficacy of the P/PH Distinction", James Chamberlain wrote, in connection with early emigration of the Thai/Tai from what is now northern Vietnam and their relations with Austroasiatic groups, "it is appropriate to mention here Ram Khamhaeng's most powerful spirit of the mountain at Sukhothai named Phra Khaphung / Kha? /

phung/, spelled with the initial high class /Kh/ as if it were originally /khaa<sup>2</sup> / ‘Austroasiatic’.” Chamberlain implies that this designation would have derived ultimately from the Austroasiatic folk hero Cheuang or Hu’ng, who “became an ancestral spirit of the Tai peoples as well as the Austroasiatics in northern Southeast Asia”, via an ethnonym ‘Kha Phong’, designating peoples “still found in Sam Neua, Xieng Khwang, Khammouan, and Nghê An provinces ... [s]ome speak[ing] a Viet–Muang language and others apparently speak[ing] a dialect related to Khmu”.<sup>204</sup> In preserving this element of prehistoric inter-ethnic contact, RK would show its authenticity, for such a detail could not have been imagined by fakers.

It is not, however, that straightforward, once it is realized that early 19th-century literati had access to some Sukhothai inscriptions, not to speak of Chamberlain’s chain of hypothetical identifications which violate historiographical acceptability (that is, why would a Tai people take ‘khaphong’ rather than the name of the hero, as designation for their spirit?). The name *braḥ khbañ*, presumably a variant of *khbuñ*, for some kind of deity or spirit occurs in two other Sukhothai inscriptions, proving, at least, that whatever its origins, it remained for some time a part of Thai belief. It is found in inscription No. 45, face I, lines 15–16, as *pū cau braḥ khba ñ xau* [Mt.] *yannyañ braḥ śrī* near the end of a list of spirits; in No. 98, dated 1519, from Vat Chetuphon, Sukhothai, where the last line says “this stone was brought from *khau braḥ khbañ hlvañ*”, which seems to confirm the traditional identification of the site of Ram Khamhaeng’s *braḥ khbuñ* as on the hill known today as *khau hlvañ*.<sup>205</sup>

I am grateful to Chamberlain for calling attention to this passage, to which I had given insufficient notice, but which represents still another peculiarity in the content of RK, and the treatment of which shows efforts to impose preconceived notions on a passage lacking in straightforward sense. Lines 3–5 of Face 3, just before the passage in question, are without controversy, listing several natural features, kus, vihras, monks, a dam, and groves of several types of trees. Then in line 6 it says, transcribed in modern Thai, มี น้ำ ไคก มี พระขฟง ผี เทพดา ใน เขา อนนนั้น followed by the non-controversial “the greatest of all the spirits in this country”.

Coedès translated the Thai phrase in question as “there is a spring (spurting) from a hill (*colline*). There is the Brañā [sic!] Khabuñ, spirit and deity of this hill”; and in his footnote 4 said the hill was “probably the Khau Hlvañ”. Griswold and Prasert rendered it as “there are mountain streams and there is Bra Khabu. The divine sprite of that mountain”, with footnote 95 explaining that ‘Braḥ Khabuñ’ “is apparently a variant of Khmer **braḥ khbañ** ‘holy and exalted’”, and noting that *with their rendering* of น้ำไคก as ‘mountain streams’, “Braḥ Khabaṇ [although]

... generally taken to be the name of the sprite, ... the syntax here shows that it is the name of the mountain".<sup>206</sup> That is, in Coedès' construal 'this/that mountain' referred to โดก and *brah khabun* was its deity, whereas in Griswold's and Prasert's translation โดก merely qualifies a type of water source and can no longer be taken as the referent of 'that mountain', which then grammatically refers to *brah khabun* as the mountain on which there was a spirit, the ผีเทวดา. The difference in syntax lies in the translations, not in the Thai. Griswold and Prasert probably did discern an incongruity in Coedès' version, but preferred to deal with it surreptitiously.

The first problem is น้ำโดก. Coedès construed โดก as an elevation of the ground from which water spurted, while Griswold and Prasert preferred to take โดก adjectivally as an attribute of the water, 'mountain streams'. But are those legitimate construals of โดก? In modern Thai, โดก is glossed as 'mound, hill, hillock, a dry place', and as 'raised earth', with a note that in Khmer it means 'waterless place'.<sup>207</sup> If โดก is taken as a raised mound or hill, does it represent one that is high enough to be called 'mountain', or to have a spring spurting from it? Some traditional and revealing contexts are found in the *Three Seals Code*. Excluding place names, โดก is found in two contexts, referring to raising earth to make a plot for planting trees or to mark off pieces of land, thus hardly 'mountain', or even 'hill'. This agrees with D.B. Bradley's 1873 dictionary, in which โดก is described as a man-made elevation.<sup>208</sup> It would seem that Coedès, taking *brah khabun* as a spirit, and recognizing it as a variant of Khmer *khbari*, 'high place', was influenced by this and by the phrase 'that mountain' (เขาอันนั้น), on which it was located, and was forced to discover a 'mountain' earlier in the text to which reference was made. This led to his forced construal of โดก. Although realizing that in their translation *brah khabun* was the name of a mountain, not a spirit, Griswold and Prasert maintained the essence of Coedès' translation, which forced them also to construe โดก artificially. Of course, if *brah khabun* is taken as the name of the mountain, there is no need to construe โดก as 'mountain', either nominally or adjectivally, but the problem of its precise meaning in this context remains.

In the first scholarly work on RK, Bradley at least avoided problems of logic by translating, "there are upland waters", probably the source of Griswold's and Prasert's 'mountain streams'. "In yonder mountain is a demon-spirit, Phra Khaphung." Here the allusion to mountain is sufficiently vague that no previous referent is required, but the rendering of โดก is still controversial.<sup>209</sup>

It is interesting to examine the earliest known translation, that of Bastian, who must have depended on opinions of Thai scholars of mid-19th century. He and his informants seem not to have been troubled by the missing referent for

'that mountain', and his version reads "there is water in a cistern [โลก]. There is also the lord Khaphung, the demon-angel [ผีเทพดา], who is the mightiest in that mountain." How did โลก come to be translated as 'cistern'? Perhaps from the fact that when earth is raised around a field the enclosed area may fill with water in the rainy season, or possibly from Mon, in which /kok/ is 'kiln', that is a hole in the ground.<sup>210</sup>

I wish to suggest that the problem lies in the composition of RK by late writers familiar with Sukhothai inscriptions in which *braḥ khbañ* occurred, and perhaps even comprehending it as a spirit on a high place. In No. 98, as I have noted, that name is simply the designation of a mountain, apparently the *khau hlvañ*, but in No. 45, *braḥ khbañ* is a spirit designated in that text as พระขพงเขายนียง (*braḥ khabaṇ [of] xau yannyan*), the last term of which might have been as unfathomable for early 19th-century scholars as it was for Grisiwold and Prasert, and they rewrote it in RK as *annnan*.<sup>211</sup> The expression น้ำโลก must also be attributed to the influence of some written record which the writers of RK did not understand, since it appears to be a nonsense expression. At least that is what is suggested by current dictionaries and by the varying translations of RK scholars. Those who wish to defend its authenticity must discover some genuine usage of น้ำโลก in Thai literature which can fit the context of RK.

### Old Thai Administration

In "The Efficacy of the P/PH Distinction", James Chamberlain brought out another detail of the content of RK of relevance to its authenticity—its depiction of administrative structure. He sees "divergent political structures [in] Ayutthaya and Sukhothai"; which parallel the different branches of Thai languages which he identifies; "[w]hile Inscription One portrays a system of benevolent patriarchy, the Ayutthayan evidence provokes images of a highly ordered and codified (Sakdina) society". Even more highly organized societies are found among P language peoples, the Black, White, and Red Tai, the Lue, and the Shan, which have "the most rigid hierarchical social structures". In the past "this has been interpreted as ... isolation and hence a more original preservation of an older common Tai administrative and religious order", but Chamberlain has "recently come to believe that this may not have been a Tai system, but a Chinese one".<sup>212</sup> At the time some Tai/Thai groups adopted the Chinese structures, the branch which became Ayutthayan Thai, like the P language groups, was still close to Chinese influence, while the ancestors of Lao and Sukhothai Thai were already farther west.

I fully agree with Chamberlain's notice of the divergent political structures and their historical importance; I have also indicated that the highly structured systems, at least some institutions, including the Ayutthayan *śaktinā*, probably derived from China. Rather than a rigid distinction between a Chinese system borrowed by some Tai/Thai groups, and which is reflected in Ayutthaya, as well as in Black and White Tai societies, I suggest that the 'Chinese' features of Tai/Thai systems result from very ancient proximity, at least from Han times and perhaps earlier, and that the 'Chinese' traits may just as well be treated as ancient Tai, to the extent it may be reconstructed. Some of the same institutions, such as a declining descent rule for royalty, were part of traditional Vietnam as well.<sup>213</sup>

Contrary to Chamberlain, however, I do not think this helps make a good case for the authenticity of RK. The system of "benevolent patriarchy" which may be inferred from RK is too different from the hierarchy portrayed in the 14th-century Lithai corpus, and suggests rather modern writers with some awareness of Sukhothai titles, (I do not say the Sukhothai system), idealizing the past.<sup>214</sup> Inscription No. 1 itself, moreover, in spite of proclaiming a benevolent patriarchy, shows a rather complex panoply of ranks with obvious relationships to the rank structures of Black and White Tai and Lue. In RK we see the following ranks:

*ba khun* ('king', Ram Khamhaeng, Śrī Indrāditya=*cau möa-*)

*dāv, brañā* (rulers, Ram Khamhaeng)

*braḥ* (prince, Ram Khamhaeng)

*khun* (chief of major town, Ram Khamhaeng, *cau möaṇ*)

*nāṇ* ('lady', mother of Ram Khamhaeng)

*lūk cau lūk khun* (nobility?, officials?; see below, Inscriptions No. 4 and No. 5)

*brai/khā* (commoners, restricted rights)

*pova* (?), paired with *nāṇ*<sup>215</sup>

A curious feature of No. 1 is that King Ram Khamhaeng is called variously, and randomly, *ba khun*, *dāv brañā* (4.12), *braḥ, khun* (4.2), and *ba khun braḥ* (4.1), as though all those titles were of equivalent status rather than following one another as seen in the hierarchies of other sources. Although it might be argued that this indicates a loose, free rank structure, I find rather that such *ad hoc* attribution of titles is one of the features casting doubt on the authenticity of RK.

The titles found in No. 2 differ between its contemporary part of the 1360s and its historical beginning, relating events of perhaps mid-13th century. Taken all together they are:



*brañā* (king, Srināv Nām Tham, Phā Möaṅ)  
*ba khun* (king, Nām Tham, Phā Möaā, Bān Klān Hāv, Śrī Indrādity, Rāmarāj)  
*kamrate añ* (Phā Möaṅ)  
*dāv brañā* (1.39)  
*brañā* (Khamhaeng Brah Ram)  
 Dharmarāja (no rank title) Loethai or Lithai  
*cau, cau rājakumār* (prince, Śrīsraddha)  
*khun* (chief of a town)  
*cau möaṅ braḥ, samtec*  
*braḥ* (Śrīsraddha)

These terms are less confusing, even in the possibly semi-legendary period. *Brañā* and *ba khun* can be understood, respectively as Mon and Thai equivalents at a time when Mon and Thai groups were probably of equal importance. The title *ba khun*, however, is restricted to dead kings of the past, while in the contemporary real world of No. 2, *brañā* was used both for a king and for a prince who was not king, Khamhaeng Brah Ram, father of the inscription's protagonist, Śrīsraddha, a Thai prince who spent most of his life as a monk. He is called *cau*, Thai for 'prince', *rājakumār* 'king's son', and *samtec braḥ*, probably here an ecclesiastical title, although the terms, both khmer, are also found in secular titles. *Kamrateñ añ* was a high Angkor official title. Although the hierarchy is not absolutely precise, the ranks in No. 2 do not show the same ad hoc character as No. 1. Terms indicating commoner ranking are not found in No. 2.

The hierarchy in Lithai's Inscription No. 3 is:

*brañā* (Lithai, his father Löthai, and grandfather Rāmarāj)  
*dāv brañā* (fellow rulers who consecrated Lithai)  
*dāv + brañā* (Lithai after consecration)  
*brañā + royal titles* (Lithai further on in the body of the text)  
*cau/khun* (chiefs of major *mōaḥ*)  
*lūk cau lūk khun*  
*khun bī khun nòn lūk hlān* (family of Lithai?)  
*brai fā khā dai* (commoners)

The aristocratic titles which appear here are equivalent to the usage of No. 2, but interestingly *bakhun* has been dropped, even for grandfather Rāmarāj who received it in No. 2. The expression *lūk cau lūk khun*, found in RK and in the Lithai inscriptions, and over which too much speculative ink has been spilled, is revealed in the Khmer / Thai pair of Inscriptions (No. 4 and No. 5) as equivalent

to *amātya mantri rājakula* in the Khmer text.<sup>216</sup> These Indic terms, used in Khmer, may be translated 'officials', 'ministers', 'royal family', which must also be the referents of *lūk cau lūk khun*.

	No. 4	No. 5
Lithai, grandfather Rāmarāj	<i>braā pād kamraten añ</i>	<i>brañā</i>
other rulers who consecrated Lithai	<i>kṣatra</i>	<i>dāv/brañā</i>
Officials who welcomed the monk	<i>amātya mantri rājakula</i>	<i>lūk cau lūk khun</i>
commoners		<i>brai fā khā dai</i>

Whereas the inscriptions discussed above were the work of kings, and mention only the highest royal or noble ranks, after the end of Lithai's reign in the 1370s there are several inscriptions commemorating works of lesser nobility, along with mention of other levels of upper-class rank. The first of these is No. 102, of A.D. 1380, the work of a woman who has been identified as a princess.<sup>217</sup>

*brah srī rāja-oras*, 'the eldest brother', 'king's son', and king himself  
*pā nāñ*, 'aunt princess' (inscription's founder)  
*luñ khun*, 'uncle khun', or 'uncle of the khun' possibly husband of 'aunt princess'. Griswold and Prasert say he was the uncle of the ruler of Sukhothai.  
*nāy* 'chief', named Ay Ind  
*jī nva+ jāñ*, 'craftsman named nva'  
*gan teen*, 'assigned personnel', denominated as 'houses', assigned by princess to take care of temple  
*khā dai*, 'servants'

The passage about *gan teen* assigned to work at the temple shows a class of unfree persons totally subservient to the nobility, yet different from another class of commoner, the *khā dai*.

Another inscription of the same period, No. 106 of A.D. 1384 shows a few more such titles.<sup>218</sup>

*samtec* (King Mahadharmaraja, Lithai)  
*dāv brañā*, 'kings', who may reign in Sukhothai  
*brañā* (an otherwise unknown person named *brañā srī debāhūrāj*)  
*cau bram jai*, (another mysterious, apparently royal, figure)  
*bnañ* [or *ba nañ*] *sai tam* (พณีสถา), 'foster father' [?]; name of inscription's author,  
 a member of the royal family<sup>219</sup>  
*gan*, people assigned by house for service to temple  
*brai fā khā gan bal*, commoners  
*khā dai*, 'slaves', 'servants', whom the protagonist caused to become monks.

Inscription No. 93 of 1399 is a record of the founding of a *stupa* by a Sukhothai queen. There is mention of several high-ranking monks whose titles begin with *cau*, perhaps indicating that they were of the aristocracy. Commoners are also mentioned—Nāy Jyañ Srī Cand, overseer of fifty families of *gan tee* assigned, along with rice fields for the support of the temple.<sup>220</sup>

These inscriptions show that Sukhothai society had a ruling class divided into several strata, and that there were at least two levels of unfree, or partly unfree commoners at the disposal of the aristocracy. Even if the Lithai inscriptions do not show the lower levels clearly, it is obvious that wherever there is a titled nobility, there must be clearly distinct lower strata providing service and labor.

Thus, the overt proclamation of a 'loose structure' in RK is one more feature marking it as a 'sport' within the total Sukhothai corpus.

The Sukhothai structure, moreover, is not very different from that of the Black Tai and Lue, two of those P language societies cited by Chamberlain as 'rigid'. Much of the Black Tai and Lue rank terminology is very similar, and also clearly related to titles known from Sukhothai epigraphy.<sup>221</sup>

	<u>Black Thai</u>	<u>Lue</u>
legendary clan chief	<i>khun, po</i> [ฟ๑]	
chiefs, princes,	<i>cau, /puu cau</i> <sup>2</sup> /	<i>cau</i>
ruling feudal aristocracy		<i>cau</i>
Lord of the Land / king,		<i>cau pheendin</i>
hereditary chiefs of <i>mōaṇ</i>	<i>fia / phia</i> [b(r)añā] <i>tao</i> [ກາ]	
chief of lower <i>mōaṇ</i>	<i>khun, tao</i>	<i>cao mōaṇ</i>
lower aristocracy		<i>phyā</i> [b(r)añ]
nobles	<i>tao</i>	[tāv]
descendents of nobility,		
who had become free peasants		<i>lūk lān tāv phyā</i> <sup>222</sup>
community headmen		<i>tāv khun</i>
common people		<i>khā phai / brai</i>
free peasants	<i>pay</i> [brāi]	<i>dai</i> (thai) <i>mōaṇ</i>
taxed and subject peasants, and		
war prisoner servants	<i>kuoṇ</i>	
serfs or slaves /		
Thai, non-Thai	<i>kuoṇ ñòk</i>	
'interior'	<i>kuoṇ</i>	inner <i>kun</i> [คณ] <i>hōn</i> [เรื่อง]
'completely dependent'	<i>ñòk</i>	outer <i>kun hōn</i> [?]
new <i>kuoṇ</i> ; debtors, condemned,		
vagrants, White Thai for		
Black Tai <i>kuoṇ</i>	<i>ñòk</i>	

house people, servants, inferiors lord's slaves domestic slaves	<i>kon</i> [คน] <i>hūñ</i> [เรื่อง]	<i>kun hñ</i> <i>lek noy</i> <i>khā cau</i> <i>khā hñ</i>
<i>kuo</i> given land	<i>pua' pai</i> [พวกไพร่]	<i>lek noy</i>
serfs, slaves/non-Thai	<i>pua' pai</i>	

If Sukhothai and Lao society were in fact less rigid than Black and White Tai and Lue, I propose that the reason was not because their ancestors migrated earlier and escaped Chinese influence, but because they moved into areas of Mon society. The Thai who settled in the lower Menan Basin and who became part of Ayutthaya came under Khmer influence, that is a society which was just as rigid, if not more so, than ancient Chinese. Note that the most rigid reign of all, as recorded in extant documents, was that of Naresuan, a Sukhothai prince in Ayutthaya.<sup>223</sup>

### The Development of Thai/Tai Scripts

Much of what follows was presented orally at Canberra in connection with "Piltdown 1", but now I wish to get it into print in order for it to be adequately studied and criticized. There are several points for which the evidence is incomplete.

The conclusions which I have drawn are (1) the Sukhothai/modern Thai, Black/White Tai, and Ahom type scripts each represent a separate development from previous Indochinese Indic scripts, (2) they were all originally adaptations by Thai/Tai peoples before they left Indochina, (3) Ahom may represent the earliest Thai/Tai borrowing and the Sukhothai type the last, and (4), a matter not discussed in Canberra, the source alphabet may have been Cham rather than Khmer.

Table A shows the relationships in script form of consonants among Old Khmer, Old Mon, Old Cham, Sukhothai, Black Tai (BT), and Ahom.

Note first the very close similarity among the first three languages, which means that an argument about the relative importance of Old Mon or Old Khmer in the development of Thai cannot be sustained.<sup>224</sup>

Nearly all the Sukhothai symbols clearly belong in that tradition, and the long-standing assumption that Sukhothai Thai was strongly influenced by Khmer script is well-founded. Even the Sukhothai *m mā* (ม), which at first looks bizarre to anyone familiar only with modern Thai, can be seen as a development from old Khmer-Mon prototypes. Some important exceptions are the Sukhothai

Table A

md. Khm/Thai	ဂ/ဂ	ဓ/ဓ	ဂ/ဂ	ယ/ယ	ဒ/ဒ
Khmer	က	ခ	ဂ	ဃ	င
Mon	က	ခ	ဂ	ဃ	င
Cham	က		ဂ	ဃ	င
Sukhothai	က	ခ	ဂ	ဃ	င
Black/White	က	ခ	ဂ		င
Ahom	က	ခ	ဂ	ဃ	င
Md. Khm/Thai	ဓ/ဓ	ဓ/ဓ	ဓ/ဓ	ယ/ယ	ဒ/ဒ
Khmer	ဃ	င	ဃ	ယ	ဒ
Mon	ဃ	င	ဃ		ဒ
Cham			ဃ		
Sukhothai	ဃ	င	ဃ	ယ	ဒ
Black/White	က		ဃ		ဒ
Ahom		ဃ			ဒ
Md. Khm/Thai	ဂ/ဂ	ဓ/ဓ	ဓ/ဓ	ယ/ယ	ဒ/ဒ
Khmer	က	ခ	ဂ	ဃ	င
Mon	က	ခ	ဂ	ဃ	င
Cham	က		ဂ		င
Sukhothai	က	ခ	ဂ	ဃ	င
Black/White	က	ခ	ဂ	ဃ	င
Ahom	က	ခ	ဂ	ဃ	င
Md. Khm/Thai	ဃ/ဃ	င/င	ဃ/ဃ	ယ/ယ	ဒ/ဒ
Khmer	ဃ	င	ဃ	ယ	ဒ
Mon	ဃ	င	ဃ	ယ	ဒ
Cham	ဃ		ဃ		ဒ
Sukhothai	ဃ	င	ဃ	ယ	ဒ
Black/White	က	ခ	ဂ	ဃ	င
Ahom	က	ခ	ဂ	ဃ	င
Md. Khm/Thai	ဃ/ဃ	င/င	ဃ/ဃ	ယ/ယ	ဒ/ဒ
Khmer	ဃ	င	ဃ	ယ	ဒ
Mon	ဃ	င	ဃ	ယ	ဒ
Cham	ဃ		ဃ		ဒ
Sukhothai	ဃ	င	ဃ	ယ	ဒ
Black/White	က	ခ	ဂ	ဃ	င
Ahom	က	ခ	ဂ	ဃ	င
Md. Khm/Thai	ဃ/ဃ	င/င	ဃ/ဃ	ယ/ယ	ဒ/ဒ
Kh. M, Ch	ဃ	င	ဃ	ယ	ဒ
Sukhothai	ဃ	င	ဃ	ယ	ဒ
Black/White	က	ခ	ဂ	ဃ	င
Ahom	က	ခ	ဂ	ဃ	င

symbols for *ch chān* (ឃ), *th thuñ* (ត), *ph phān* (ផ), *kh rakhañ* (ក), and *ch chō* (ឆ), which cannot be related to those Indo-Khmer prototypes. Neither are they related to any kind of late cursive Khmer and the last two seem in Sukhothai to be peculiarly related to each other. In modern Khmer, the first, *c<\*j* ( ជ ) is easily explained as a development from the ancient form, and the Khmer *p<\*b* ( ប ) maintains until now one of the ancient forms.

In Black Tai / White Tai, among the symbols relevant to the discussion, those equivalent to standard Thai *k kai* (ก), *kh khvāy* (ค), *c cān* (จ), *d dek* (ด), *th thahān* (ท), *b bai mai* (บ), and all the nasals except *ng ngū* (ง), show derivation from the corresponding Indic symbols. The Black Tai letters which are not from that source are, like Sukhothai, *c<\*j* (equivalent to *ch chān* [ឃ]), *th* (equivalent to *th thuñ* [ត]), *p<\*b* (= *ph phān* [ផ]), and also *t* (= *t tau* [ត]), *r*, and *s* (= *s sōa* [ส]). Black Tai *th* resembles Sukhothai *th thuñ* [ត], which is not surprising to traditionalists who consider that BT developed from Sukhothai, but as we shall see, the nature of the relationship is equivocal. We should note that BT did not adopt three of the Indic voiceless aspirate symbols, *kh*, *ch*, *th*.

The Ahom symbols with clear similarities to the Khmer-Mon-Cham prototypes are those equivalent to Thai *k kai* (ก), *kh khvāy* (ค), the single Ahom palatal / c, ch / series symbol, and the symbols equivalent to Thai *t tau* (ต), *ph phōn* (ผ), *y yaks* (ย), *v ven* (ว), and *h hīp* (ห). The Ahom *s* (= Thai *s sōa* [ส]), at first appears unlike other Thai, or relevant Indic scripts, but when we consider the Cham forms, we see that a smaller lower buckle led to a form resembling W, to which Ahom bears a clear similarity. There is also resemblance to Mon. Ahom also ignored the old voiceless aspirates *kh* and *ch*, but seems to have adapted the Mon *th*. This is the only Ahom symbol, it must be emphasized, which has a clear Mon prototype.

Table B illustrates the way a prototypical Indic script was borrowed and adapted by the three types of Thai.

The topmost division of this Table illustrates the Proto-Tai phonemes which are relevant to the discussion, and the resolutions of the old voiced series in PH / P languages. Also illustrated are the so-called pre-glottalized *\*?d* and *\*?b*, which have become voiced / d / and / b /. It must be understood that historically they are not the original voiced *\*d* and *\*b*.<sup>225</sup>

**Table B**

PH/P			
k kh/x	*g>kh/k	kh<*	n̄
c ch	*j>ch/c	s<*z s	n̄
t th	*d>th/t		n̄
p ph	*b>ph/p	f<*v f	m
w	y	h r l	
PH			
k kh	g	gh	n̄
ch	j	jh	n̄
t th	d	dh	n̄
p ph	b	bh	m
w	y	h r l	
P			
k x/kh	k<*g		n̄
c			n̄
t th	t<*d		n̄
p ph	p		m
w	y	h r l	
P			
k	k<*g		n̄
c/j		s	n̄
t th	t<*d		n̄
p ph	p		m
w	y	h r l	

**Sukhothai/  
Ayutthaya**

**Black/  
White**

**Ahom**

d< \*?d  
b< \*?b

t th  
p ph  
w

t th  
p ph  
w

t th  
p ph  
w

The next three sections of Table B show the ways in which Indic symbols were borrowed by the Thai languages, respectively, Sukhothai/Ayutthaya (modern Thai), Black/White Tai, and Ahom. In each section the roman script symbols indicate Indic consonant symbols, and the various Thai symbols adapted from them are juxtaposed.

The encircled symbols represent Indic letters which were not adopted by the first Thai scripts, at least for use in representing Thai vocabulary. They comprise the entire range of voiced aspirate stops, the alveolars, *j*, and *b*. Note that the Sukhothai alveolars, shown in Table A to the right of a slanted line following the dentals, are quite different from the Indic forms.

From wherever the Thai borrowed their script, they did not make full use of the possibilities of an Indic script as used in Khmer or Mon. In those scripts each class of consonants, velar (*k*), palatal (*c*), alveolar (*ṭ*), dental (*t*), labial (*p*), contains 4 symbols for (1) voiceless unaspirated, (2) voiceless aspirated, (3) voiced unaspirated, (4) voiced aspirated (*k-kh-g-gh*). When this type of script was taken over by Thai, however, the fourth member of each series seems to have been reserved for Sanskrit and Pali words, as they still are today in general, and were not utilized to help represent the several consonant features of Thai.

In Khmer, in contrast, the full range of Indic was used from the beginning to represent phonemes of Khmer.

As a result, in Thai, in each series of consonants there were too few symbols available for Thai phonemes, and new consonant symbols had to be devised. The new symbols are adaptations of existing consonant symbols which were phonetically similar. Thus, in Sukhothai and related scripts including modern Thai, *kh khon* (๓) was created by adding a notch to *kh khvāy* (๓), and *kh khuat* (๔) is *kh khai* (๔) with an added notch. The form of *s so* (๕) is clearly an adaptation of *ch chān* (๕), which probably indicates that they were closer in pronunciation than now; *t tau* (๖) is certainly based on *d dek* (๖).

Examination of the ways new consonant symbols were devised reveals both the independent developments of the three types of script, and perhaps something of their phonological histories. The dental and labial series are the most interesting.

In Sukhothai and Black Tai, the Indic voiceless *t*, which was used for the same voiceless consonant in Khmer, Mon, and Cham, became in Thai the symbol for /d<?d/ (๓), now a voiced consonant. Then in Sukhothai the new symbol which was required for voiceless /t/ (๓) was devised from the old *t*. In Black Tai, however, the new symbol for voiceless /t/ was based on their adaptation of Indic *d*, corresponding to Sukhothai *th thahān* (๓). The Black Tai device was possible there, but not in Sukhothai, because in Black Tai, a P



language, original \*d>/t/, whereas in Sukhothai it became /th/. These different treatments show (1) that the Black/White Tai script did not come from Sukhothai, and (2) when the Black/White Tai scripts were devised, the voiced stops had already devoiced. The Sukhothai device in this case cannot reveal anything about devoicing, because its *d* (*th thahān*), being aspirated, could not in any case have provided a vehicle for the new dental symbol which was required. In both Black and White Tai the symbols for *th* are innovations, not adaptations of the original Indic, and, moreover, they are independent innovations, not mutually related. The basic Black Tai form is obviously related to that of Sukhothai and modern Thai (ᨾ), but the form shown by Coedès as standard White Tai *th* is a modification of White (and Black) Tai *d* by adding a vertical stroke below it. Finot showed a low series White Tai *th* nearly identical to Black Tai *th*. This is also shown in Diêu and Donaldson, but as high series. For Black Tai, Diguët also provided a second *th* symbol, presumably low series, and bearing some resemblance to the first White Tai *th*, but with only two words ascribed to it, one of them *thān* 'pail', the same as modern Thai *น้ำ*.<sup>226</sup>

It must be admitted that published examples of Black and White Tai scripts show some variety in the rendering of certain symbols, and I am uncertain about which should be considered paradigmatic. For example, depending on the type of *t* symbol (equivalent to Thai *t tau*) chosen for comparison, we might wish to conclude that BT *d* (/d<?\*d/) and *t* (/t<?\*t/) both developed from the original *d* symbol.

In the labial series Sukhothai and Black Tai show identical procedures. Indic *p*, voiceless, was assigned to the perhaps only emerging /b/<?\*b, and for /p/ the tail of the original symbol was extended. Both of them also ignored the Indic *b*, but they adopted *ph*. From it they constructed new forms for their reflexes of *b*, /ph/ in Sukhothai and /p/ in Black Tai. They also devised their two fricative (/f/) symbols from the same base. This suggests that the voiced initial stops had devoiced, because the symbols devised for the reflexes of \*b are based on *ph*, an original voiceless consonant. Here also White Tai has a low series *ph* formed by adding a short vertical stroke below *p*<\*b.

The neglect of original Indic *b*, which they needed, is intriguing. The possible reasons are (1) it was still voiced in Khmer and Cham, but devoiced in Thai, and there was no place for it, but then one may ask why they did not take it for /b<?\*b/, (2) in the language from which the Thai borrowed their scripts /b/ and /v/ had converged, and all words were written with a *v* symbol. The relevance of this will be seen below.

Now let us consider the velar and palatal series in Black Tai. Black Tai's *kh* *khai* equivalent, representing original *kh*, is based on that for *k* (*kkai*). If this neglect

of original *kh* is not a freak incident; it seems only to be explained as a result of the source alphabet being already deficient in symbols for aspirates. This also will be taken up below. In Table B I have included two varieties of the WT *kh*, to illustrate how it was formed from WT *x*, the symbol which in BT represents *kh*. As I noted above, White Tai, Black Tai, and Sukhothai each established a basic voiceless aspirated velar symbol for its own dominant aspirated velar phoneme (Sukhothai adapted the original Indic form), but in Sukhothai and Black Tai the dominant velar voiceless aspirate was /*kh*/, whereas, in White Tai it was /*x*/ . Thus, in White Tai the new symbol which had to be constructed was for /*kh*/, while in Sukhothai it was /*x*/ (Sukhothai *kh khuat* [๓]).<sup>227</sup> In the palatals, the symbol for BT /*c*/ seems to show real, though vague, adaptation from Indic, but in this series, too, BT ignored Indic *ch*, and also *j*, and developed a new *j* symbol (/ *c*/) by turning their *c* upside down and extending its tail. Their *s so* equivalent (\**z*) seems also to have developed from *c*, but true or not, it is of no concern here. As I noted above, the Sukhothai *ch chān* (\**j*) is quite aberrant in terms of Indic, but its later development bears a clear resemblance to the BT equivalent, and the resemblance is even closer if WT forms are considered. This is a clue that the Sukhothai line of script development may be later, even an offshoot, of that now represented by Black and White Tai.

The adaptations of original Indic by Ahom are even less complete; more new symbols were invented, and where the same problems were faced as in BT and Sukhothai the procedures were different, proving that Ahom, like BT, was a script separate in origin from the others. Ahom's *kh* is not clearly related to anything, although it might have been adapted from *k*, or even from *h*. The single Ahom palatal, used for words which in other languages are written with both *c* (Thai ๑) and *c/ch* (\**j*) (Thai ๓), shows some relationship to Indic *j* forms. The Ahom dental series is quite different from the other scripts. The Ahom symbol which in form is derived from Indic *d* (Thai ๓๒ *thahān* [๓]), is used for words beginning with /*d*<?*d*/; thus, in fact, preserving the original voiced value of that symbol.<sup>228</sup> Ahom *t*, which derives from Indic, is used both for words originally beginning with that consonant, but also for words beginning with original \**d*>/*t*/ (Ahom is a P language). The source of Ahom script was thus a language in which \**d* was still voiced. In the labials Ahom shows coalescence between the symbols for /*b*<?*b*/ and /*v*-*w*/, and that symbol is used for words beginning in other Thai languages with both. The Indic *p* has been maintained for the same phoneme in Ahom.

Another interesting feature are the written symbols corresponding to Indic originally voiced aspirate stops. Ahom did not adapt the Indic symbols, but invented a new set, based in three cases, *jh*, *dh*, and *bh*, on already existing Ahom symbols. The *dh* is *th* with a small circle added at the bottom, and the same feature was added to *ph* to make *bh*. This type of adaptation is also characteristic of White Tai in its invention of *kh* from *x* (Black Tai *kh*), and the low series *th* and *ph*; that is one bit of evidence that Ahom derives from a prototype in the east. Ahom's *jh* is also interesting. It is derived from the *y* symbol, which in modern Ahom grammars is listed among the palatals, even though all the words in which it is found are *y*-words, illustrating the fricative quality often found in Thai, and Khmer /y/.

One more detail which may link Ahom with Indochinese scripts is the vowel sign /e/ placed to the left of the consonants, and which is used in compound vowel symbols for /au/, /o/, etc. In both Ahom and BT/WT, and different from Sukhothai, it is √.

Now we may consider the type of source alphabet which might have influenced the characteristic adaptations of the Thai scripts. They ignored all or some of the voiced aspirates, and their borrowing of the unvoiced aspirates was incomplete, particularly in Ahom. There is also the interesting question, evoked above, of the absence of conjunct consonants in the Sukhothai and Black Tai types, although Terwiel has shown evidence of a few in Ahom.<sup>229</sup>

The problem of conjunct consonants merits attention. They are characteristic of the scripts of India, for languages which are rich in consonant clusters; when an Indic script was taken by Khmer and Mon speakers, that feature was adopted and fully utilized, for those languages are perhaps even richer in consonant clusters. The conjunct consonant device is an extremely efficient way of writing clusters, and did not cause scribal problems until it came up against mechanical writing devices such as printing presses and typewriters.

The variety of Thai represented by Sukhothai and modern Thai would also have found conjuncts useful for the frequent clusters with /r/, /l/, /v/ as second element, and for other clusters in Khmer or Sanskrit loan words, and their neglect does not represent any kind of progressive innovation, but a real defect.<sup>230</sup> The reason must have been that the language from which the first Thai scripts were borrowed had already neglected conjuncts because they were less useful for its phonology than in Indic or Khmer. Therefore, neither Khmer nor Mon was the original source for the Tai scripts, even though Sukhothai, as we see it now, must have been reformed on the basis of Khmer.

Within Southeast Asia the languages which fit this hypothesis are of the Austronesian family, with few or no clusters or aspirates, either voiced or

unvoiced. In Indochina the important representative is Cham, in which the earliest extant example of a Southeast Asian written language is found.<sup>231</sup>

Early Cham inscriptions, for example of the 8th century, with many Sanskrit terms, show full use of the Indic-type alphabet, and the most frequent conjunct is *r* in Indic words, but also in Cham words such as *vriy* (Malay *beri* 'give'). In Cham of this type, /*b*/ was assimilated to /*v*/, and all words beginning with Austronesian /*b*/ are written with the same Indic *v* symbol found in words with original /*v*/, a situation I evoked above in connection with Black Tai and Ahom. Other conjunct consonants found often in Old Cham words were *l* and *y*. Voiceless aspirate symbols are very rare in Cham of that period.

Modern Cham texts show *b* where expected, which would seem to indicate that their language has developed from a dialect other than that of the old inscriptions. Another feature of modern Cham is that conjunct consonants, the second element in clusters, *j*, *l*, *v*, are written on the line. Only conjunct *r* persists in the classical manner as a curve around the left side of the initial consonant (as also in Khmer រក្រ /*kr*).<sup>232</sup>

More than a tentative exposition of this hypothetical relationship between Cham and old Thai scripts requires examples of the full sequence of Cham scripts from the 8th to 15th centuries, which I do not have. A. Cabaton, in his *Nouvelles recherches sur les Chams*, p. 90, wrote, "after the 8th-century Cham script lost its archaic appearance and began to resemble the scripts of Cambodia and Java. Beginning with the 9th century it disarticulated and became overburdened with flourishes."<sup>233</sup>

Cham influence could account in a materialistic way for the lack of conjuncts in Thai. The first Thai scripts would have been adapted from Cham either at a time and place where Cham had ceased to use them because they were not required, or from a Cham script which already placed them on the line. Cham influence can also account for the defective voiceless aspirate series in Thai; it can account for the few conjuncts found by Terwiel in Ahom, *v*, and *l*, which were among those with some continuing importance in Cham.

These features would have been accentuated in Thai if the borrowings had occurred first in Tai languages which had lost even those few clusters occurring in proto-Tai, with *r* and *l*. Such languages would have been those of the Black / White Tai type. Then, when Thai languages which maintained those clusters took over the alphabet, it had no special device for clusters, and all consonants were naturally written on line. The evidence that I alluded to earlier for precedence of *fak khām* is the occurrence there of conjunct *r*, prominent in the inscriptions in *bra* (𑜋𑜨𑜧𑜨𑜫𑜨𑜧), a Khmer loan. At least this is evidence that *fak khām* is independent of Sukhothai, not a derivation from it.<sup>234</sup>

## Notes

Note on transcription: titles of works in Thai and citations of Thai texts or from the inscriptions will be in the standard Indic, or “graphic” transliteration; citations in Thai script represent modern spellings; names of places, historical sites and persons, except in citation, are in common *ad hoc* phonetic form.

1. “The Ram Khamhaeng Inscription: A Piltdown Skull of Southeast Asian History?”, *Proceedings of the International Conference on Thai Studies*, The Australian National University, Canberra, 3-6 July, 1987, Volume I, pp. 191-211; and “Piltdown Skull-Installment 2”, sent to the 1989 Annual Meeting, Association for Asian Studies, Washington, D.C., March 17-19, 1989. Both have been published in *The Ram Khamhaeng Controversy*, Collected Papers, Edited by James R. Chamberlain, Bangkok, The Siam Society, 1991, which will be the source of citations here. The abbreviation ‘RK’ means the Rām Khamhaeng Inscription, Inscription No. 1 of the Thai corpus.
2. นางนพมาศ (‘Nang [princess] Nabamāṣī’), sometimes romanized Nang Nophamat, pretends to be the memoirs of a Sukhothai princess, but most scholars believe it was written in the early Bangkok period. See Vickery, “A Note on the Date of the Traibhūmikathā”, *JSS*, Vol. 62, Part 2 (July 1974), p. 281, n. 26.
3. See my remarks in “A Guide Through Some Recent Sukhothai Historiography”, *JSS*, Vol. 66, Part 2, pp. 184-185.
4. As an example see the remark of M.R. Supavat Kasemsri, in the March 4, 1989 discussion of Inscription No. 1 presided by HRH Princess Galyani Vadhana, published by the Siam Society as ค่ายอภิปรายเรื่องศิลาจารึกสุโขทัยหลักที่ ๑ (‘Discussion of Sukhothai Inscription No. 1’), p. 45, that I denied there were both /khap/ ‘drive’ (ขับ) and /khap/ ‘sing’ (ขับ) in RK. What I said about those two words concerned their initial consonants in the script of RK, not their presence or absence. The published record of the March 1989 discussion will be cited further as ‘Discussion-author’s name, p...’.
5. I shall refer to this paper as Diller, “Consonant Mergers 2”, (CM-2), “Consonant Mergers 1” being his “Consonant Mergers and Inscription One” (CM-1), *JSS*, Vol. 76 (1988), pp. 46-63. Both have been published in *The Ram Khamhaeng Controversy*, pp. 161-192 and 487-512, which will be the source of citations here. I wish to thank Diller for providing me with a pre-publication draft of CM-2.
6. Diller, CM-2, pp. 491-3.
7. Diller, CM-2, p. 493.
8. According to Charnvit Kasetsiri, “Each Generation of Elites in Thai History”, *Journal of Social Science Review*, Vol. 1, No. 1 (March 1976), p. 201, as late as the reign of King Chulalongkorn, “ministers [then nearly all princes] were well-educated persons in the traditional manner...knew Pali, Khmer languages...”. The original publication of this article, in Thai, was in วารสารธรรมศาสตร์ / *Thammasat University Journal*, Vol. 3, No. 3 (May B.E. 2517), pp. 94-115.
9. Elizabeth Gosling, “Architecture at Sukhothai Prior to the Mid-Fourteenth Century and Its Relation to Data in Inscription I”, paper for the Asian Studies conference, Washington, D.C., March 1989, expanded and published as “Sukhothai Religious Architecture and its Relevance to the Authenticity of Inscription One”, in *The Ram Khamhaeng Controversy*, pp. 227-256; quotation from pp. 240-241. Gosling opened “

Sukhothai Religious Architecture" with a most peculiar footnote about the distinction between 'authenticity' and 'genuineness', of which she says the latter means an original document, not a forgery, while the former "refers to the veracity of the document", even if forged. A "good forgery [not genuine] preserves...the contents of the original", and "My [Gosling] concern is with authenticity only". Gosling implies the possibility that the extant RK Inscription is an authentic [true contents] copy of a genuine inscription. I wish she had elaborated on this, for although, as she noted, "Dr. Piriya and Dr. Vickery contend that Inscription One is both unauthentic and ungenuine", I believe that in addition to the genuine and still extant Sukhothai inscriptions known to the writers of RK and influencing their text, there may have been still other genuine Sukhothai inscriptions known to them which have been lost or have not been noticed. A possible example of the latter is Inscription No. 285, จารึกพ่อขุนรามพล / Inscription of *pho khun rāmabal*, published in ประชุมศิลาจารึก ภาคที่ ๗ ('Collected Inscriptions, Part 7'), Bangkok, Office of the Prime Minister, Thai History Revision Committee 2534/1991, pp. 3-10. My linguistic and palaeographic arguments, however, are intended to demonstrate that RK cannot be authentic for the 13th or 14th centuries.

10. All quotations in this and the next paragraph are from "Historical expedience or reality?" *The Nation* [Bangkok], *Focus*, Section three, 8 February 1990, p. 25. Proper names have in this case been spelled as in that article, and may not be the same as those persons have used in other contexts. See also Wyatt's views in "Cornell historian defends stone inscription's authenticity", in *Bangkok Post*, 17 March 1989; and my response in "Ramkhamhaeng Inscription", "Post Bag", *Bangkok Post*, 30 March 1989.
11. I should add that Wyatt, in the same *Nation* article, adopted a position which I have argued since 1973, that "the early Ayutthaya period (U-Thong) might have been ruled by Khmers"; he hopes "one day to see some Thai historians with courage enough to say" it. There is no hint of this in Wyatt's *Thailand: A Short History*, and it cannot be a discovery which Wyatt has made independently since 1984. Let us hope that students whom Wyatt encourages in this line of research acknowledge what has already been achieved.
12. Craig Reynolds, "Predicaments of Modern Thai History", Third Conference Lecture, The Fifth International Conference on Thai Studies, SOAS, London, 9 July 1993; Craig J. Reynolds, "The plot of Thai history: theory and practice", in Gehan Wijeyewardene and E.C. Chapman, eds., *Patterns and Illusions, Thai History and Thought*, published by the Richard Davis Fund, Singapore, Institute of Southeast Asian Studies, 1992, pp.313-332. For the alleged womanizing of Ram Khamhaeng, see G. Coedès, *The Indianized States of Southeast Asia*, Kuala Lumpur, University of Malaya Press, 1968, p. 206; with respect to Weber, see Arthur Mitzman, *The Iron Cage*, New York, Grosset & Dunlap, 1969.
13. Craig J. Reynolds, "A Look at Old Southeast Asia" *Journal of Asian Studies*, Vol. 54, No. 2 (May 1995), pp. 419-446, see p. 421. Note that Reynolds' Formulation, "the first Thai-language inscription of 1292", assumes an answer to the controversy.
14. And not only 'royal'. In spite of his barbs directed against royalist scholars, Jit Phumisak shared their preconceptions about the greatness of Thai states in the past, and he had no doubts about the authenticity of Ram Khamhaeng. This convergence of Jit's radicalism with Prince Damrong's modernist conservatism is another subject about which historians have "failed to provide what one would expect".

15. Gosling, p. 228. A particular example of Gosling's confusion in this respect, concerning Khmerism (her note 10), is treated below, p. 00.
16. Gosling, p. 231, citing p. 203 in Vickery, "Guide".
17. The name of the protagonist of Inscription No. 2 is spelled here in the corrected Indic of Griswold and Prasert. Spelling of this name in the inscription is not consistent, but a characteristic example is 'Śrīsradhārājaculāmunī'. I shall follow Griswold's and Prasert's convention of referring to him as Śrīsraddhā.
18. Coedès, "Documents", p. 99.
19. See Gosling, p. 250, Fig. 3.
20. Quotation from Gosling, "On Michael Vickery's 'From Lamphun to Inscription No.2 [published in *Siam Society Newsletter*, Vol. 3, No. 1 (1987 March), pp. 2-6]'", *Siam Society Newsletter*, Vol. 4, No. 1 (1988 March), pp. 5-7, where she was also unable to get the evidence straight. I did not rely on "evidence from the Thai chronicles", where dates, as Gosling correctly notes, "have long been recognized as unreliable". In "From Lamphun", I demonstrated relevant cases in which the chronicles were "in contradiction with epigraphic evidence" (p. 5), but these contradictions are damaging for the points which Gosling wishes to make. See also Betty Gosling, "Once More, Inscription Two: An Art Historian's View", *JSS*, Vol. 69, Parts 1&2 (1981), pp. 13-42.
21. Christian Bauer, "The Wat Sri Chum Jataka Glosses Reconsidered", *JSS*, Vol. 80, Part 1 (1992), pp. 105-25; see p. 105, and notes 1, 4, 28. This, incidentally, substantiates my argument in "From Lamphun". It also negates Gosling's arguments about the section of Inscription No. 2 concerning a Mahathat and some jataka illustrations. They may no longer be attributed to the Sukhothai Wat Mahathat, but are more reasonably to be situated in Sri Lanka, as some historians have argued. Dr. Prasert a Nagara has continued to publish work insisting that the jataka glosses and Inscription No. 2 date from late in the 14th century, 1392-1427 and "1371 or later" respectively. See Prasert na Nagara, "Comments on Arguments Relating to Inscription One", *Proceedings of the 4th International conference on Thai Studies*, Kunming, 11-13 May, 1990, Volume IV, pp. 278-289 (p. 287). Further citations to this source will be abbreviated, "Prasert-Kunming, p....". Bauer, *op. cit.*, note 1 refers to still later work of Dr. Prasert which I have not been able to consult.
22. *Discussion-Phongsriphien*, p. 61.
23. See "Piltdown 2", p. 409.
24. Griswold and Prasert, EHS 9, p. 209, and EHS 18, "The Inscription of Vat Jyañ Hman (Wat Chieng Man)", *JSS*, Vol. 65, Part 2 (July 1977), p. 127, 'triple rampart'; Maha Cham, Inscription No. 76, in *ประชุมศิลาจารึกภาคที่ ๓* ('Collected Inscriptions Part 3'), pp. 210-218, n. 7; *พจนานุกรมฉบับพระราชบัณฑิตยสถาน* ('Royal Institute Dictionary'), edition of 2525, p. 319; C.B. Bradley, "The Oldest Known Writing in Siamese", *JSS*, Vol. VI, Part I (1909), pp. 27, 51, was only tentative, but read 'three' and thought it was related to the wall; Coedès, however, "Nouvelles notes critiques sur l'inscription de Rama Khamhaeng", p. 115, said only that *trīpūr* was 'mur d'enceinte'.
25. *ศิลปวัฒนธรรม* ('*Silpavatanadharrrm*'), Special issue on "Cārūk Pho Khun Rām Khamhaeng", Dr. Prasert na Nagara, p. 91; and Dr. M.R. Suriyawut Sukhsawat, p. 121, who considered that the *trī* of *trīpūra* is from Khmer-Sanskrit *giri* 'mountain' in the term *jayagiri* found in an inscription of Jayavarman VII. I assume that discussion of this particular epicycle is unnecessary. All further citations of this source will consist of

- author's name-*Śīlp*, page number (Prasert-*Śīlp*, p. 91). In some cases I am forced to render Thai personal names in *ad hoc* phonetic spelling, because I have not seen them in print in English, and I apologize for any errors which may result.
26. Suriyavut-Silp, pp. 91, 121; and Prasert-Kunming, p. 280.
  27. EHS 18, pp. 111, n. 2, 112, 113, n. 6.
  28. Prasert-Kunming, p. 280.
  29. EHS 18, p. 114, n. 7. The suggestion that it may be a legend is mine, not theirs.
  30. William J. Gedney, "Comments on Linguistic Arguments Relating to Inscription One", paper for the Washington AAS conference, March 1989, published in the *Ram Khamhaeng Controversy*, see p. 219.
  31. See "Piltown 2", p. 395; EHS 9, p. 203 and note 3.
  32. "Piltown 2", p. 396; Robert John Bickner, "A Linguistic Study of a Thai Literary Classic" pp. 141, 160; Gedney, op. cit., and interview in *อาจารย์สาร* ('Senior Teachers' Journal'), Bangkok, 1987.
  33. "Piltown 2", pp. 397-8; Prasert-Kunming, 287.
  34. This does not contradict the suggestion made above that early Ayutthaya was Khmer. By the middle of the 15th century there must certainly have been Thai influence in Ayutthaya, and Chainat, midway between Ayutthaya and Sukhothai would have been subject to Thai influence even earlier. The Inscription of *nāy dit sai* is published in *จารึกสมัยสุโขทัย* ('The Inscriptions of Sukhothai'), pp. 135-137. It was in fact discovered in Bangkok, but marked with the word '*saṅgalok*' in early Bangkok period script, an indication that it was probably one of the pieces brought down from the northern provinces by King Rama I (see The Chronicle of the First Reign, *พระราชพงศาวดารกรุงรัตนโกสินทร์ รัชกาลที่ ๑*, National Library Edition, 1962, p. 236.).
  35. Fang Kuei Li, *A Handbook of Comparative Tai*, Oceanic Linguistics Special Publication No. 15 (cited hereafter as Li, *Handbook*), p. 261; William J. Gedney, "Siamese Verse forms in Historical Perspective", conference on Southeast Asian Aesthetics, Cornell University, August 1973, p. 11.
  36. See also "Piltown 2", p. 398, where I believe I was in error in writing that "Lithai's No. 4...uses vowel *o* [ ໐ ] in several words where it would not be used today, possibly reflecting the influence of the writer's native Thai conventions". I was misled by the Thai transcription of No. 4 in *จารึกสมัยสุโขทัย*, where [ ໐ ] is used to represent a Khmer phoneme. In fact, the adaptation of [ ໐ ] for Thai may have been much more complex, involving habits associated with transcription of Indic, as well as three, or even four Khmer phonemes, but detailed discussion is not required for the RK problem.
  37. "Piltown 2", pp. 398-402.
  38. EHS 9, p. 206 and n. 26, p. 208, n. 49.
  39. Griswold and Prasert, EHS 17, "The Judgements of King Mañ Rāy", *JSS*, Vol. 65/1 (January 1977), pp. 137-160; the *มังรายศาสตร์* ('*Mañrāyśāstr*'), edited by Dr. Prasert a Nagara, printed as a cremation volume, Bangkok 4 April 2514/1971; this raises a new embarrassment. If the *Mañrāyśāstr* is a genuine old text, why is the institution of *brai fā hnā sai* not mentioned; if it may be decided from other records that there really was such an institution, does its omission from *Mañrāyśāstr* prove that work to be a modern composition? Indeed, another dictionary of the northern language, Fu Attasivamahather, *หลักภาษาไทยพยางค์* ('Principles of Phayap Thai'), Chiang Mai, 1991, p. 298, gives *brai vā hnā sai*, along with *brai pān daiy möān* as glosses for the entry *brai vā khā pheentin*, which does



not help at all in understanding the first. One might suspect that the compilers of the northern dictionaries had been influenced by RK.

40. กฎหมายตราสามดวง ('Laws of the Three Seals'), Guru Sābh edition. Occurrences of vocabulary in the *Three Seals Code* may be determined with the *KWIK Index of the Three Seals Law*, Osaka, National Museum of Ethnology, 1981.
41. "Piltdown 2", pp. 405-6; Prasert-Kunming, p. 288.
42. Ahom does not have vowel /öa/, original /öa/ has become /ü/. Examples from *Linguistic Survey of India*, *lüt* 'blood', *mü* 'time and hand', *müng* 'country', *ngün* 'silver', *phük* 'white', *rü* 'boat', *rün* 'house'.
43. See Li, *Handbook*, chapter 14, and Brown, p. 63, section 4.31, par (2), p. 80. I insist that the results of methodical linguistics must be preferred to anecdotes.
44. In modern Thai it is written สึก with an unhistorical initial ś.
45. *Discussion*, pp. 80-81.
46. He said that Dr. Piriya had already acknowledged the reading /söŋ/, which is inaccurate. Dr. Piriya only said that /söaŋ/ was equivalent to Isan /söŋ/. See Piriya Krairiksh, จารึกพ่อขุนรามคำแหง ('The Rām Khamhaeng Inscription'), Bangkok 2532 [1989], p. 68.
47. Li, *Handbook*, p. 204, item 14, "silver", comment on it on p. 206, and remarks, p. 281, section 15.3. Li's treatment of words now written with vowel ɛ̃, however, is unclear.
48. Bradley, pp. 37-38.
49. Inscription No. 1, face 2, line 16. The preferred modern spelling is สิ่ง although the *Royal Institute Dictionary* also gives เสิง as an alternate spelling.
50. "Piltdown 2", p. 402; Prasert-Silp, p. 41.
51. Prasert-Kunming, p. 288. The two inscriptions were studied by A.B. Griswold and Dr. Prasert in their EHS 8, "The Inscription of Vat Jān Lòm", and EHS 7, "The Inscription of Vat Tabān Jān Phöak", respectively, both published in *JSS*, Vol. 59, Part I (January 1971), pp. 189-208 and 157-188.
52. EHS 9, p. 196.
53. Respectively faces/lines 2/3, 2/4, 2/5, 3/7, 3/8, 3/11-12, 3/25, 3/26.
54. EHS 7, p. 168; EHS 8, p. 208.
55. Inscription No. 102, *brah nī* (p. 165, line 9), "this holy statue" (p. 167); and *-rañik nī* (p. 166, line 22), "this Forest Monastery" (p. 168). Inscription No. 106 there is *nai saṅsārā brabuddha nī* (p. 197, lines 34-5), "this Buddha's saṅsāra" (p. 203) and *fu n* (p. 200, line 18), "this throng", referring to monastic buildings and sites, not literally translated by Griswold and Prasert.
56. Anthony Diller, "Sukhothai superscript [']: tone mark or vowel sign?", abstract of his paper for the Chiang Mai conference in October 1991. Prof. David K. Wyatt now agrees with me that early Ayutthaya was Khmer (see note 11 above).
57. EHS 1, 4.
58. EHS 7, p. 169, n. 28.
59. EHS 8, p. 204, nn. 29-30. Such expanded nasal-infixed forms are a typical Khmer feature not found in Thai, although the two terms in question, as glossed by Griswold and Prasert, are not found in Khmer dictionaries. Perhaps they are examples of syntactic borrowing, or loans from an extinct Khmer or Mon-Khmer dialect.
60. "Piltdown 1", pp. 32-33.
61. Prasert-Silp, pp. 89-90.

62. I was quite aware, *pace* Dr. Prasert, that cowries were used in Thailand until the reign of King Mongkut. See Karl Polanyi, et. al., *Trade and Market in the Early Empires*, Glencoe, The Free Press, 1957; *Dahomey and the Slave Trade*, Seattle, University of Washington Press, 1966; George Dalton, ed., *Primitive, Archaic and Modern Economies, Essays of Karl Polanyi*, Boston, Beacon Press, 1968.
63. Prasert-Silp, p. 90.
64. *Discussion*, pp. 44-45.
65. The passage is found in พระราชพงศาวดารกรุงศรีอยุธยาฉบับหลวงประเสริฐ/ *The Luang Prasöt Chronicle*, under the date *cula* era 919. In modern Khmer *kòh jôn* ( កង ជ័ន , in Thai script กอง เจริง) is perfectly clear as 'anklet'. This is another example of Khmer in early Ayutthaya which I had not previously noticed, and I thank M.R. Supavat for calling my attention to it.
66. Prasert-Kunming, p. 283.
67. Charnvit Kasetsiri agrees that in the 14th century the Chinese intended *Hsien*, their rendering of 'siam' / *syām* (as it was written in Old Khmer and Cham), as a name for the lower Menam basin, including Ayutthaya, not Sukhothai. See his "Ayudhya: Capital-Port of Siam and its Chinese Connection in the Fourteenth and Fifteenth Centuries", *JSS*, Vol. 80, Part I (1992), pp. 75-81. Craig Reynolds, "Introduction:", p. 4, *National Identity and its Defenders*, Chiang Mai, Silkworm Books, 1993, has added to the confusion, saying "Siam had been a term used from ancient times by...Champa, China, and Cambodia to designate the kingdom dominated by the Thai-speaking peoples of the Chaophraya River valley". We cannot know what the Cham and Khmer meant by '*syām*' in the 10th-11th centuries. When the term first appears in their inscriptions with a clear ethnic meaning, and as David Wyatt now agrees (note 10 above), the Ayutthayan region, which the Chinese called 'Hsien' in the 13th-14th centuries, was probably not yet Thai. More peculiarly, Reynolds, p. 4, adds that the "kings until the end of the absolute monarchy encouraged the use of Siam", which was true only from Mongkut on, yet in his note 3 says, "[i]t is not unlikely that *sayam* was coined during Mongkut's reign". This is justified by scepticism that "*sayam* in Thai is a translation of 'Siam' in English (or its equivalent in other foreign languages)". Whichever direction the translation, there can be no doubt that the term in Thai and foreign 'Siam' are equivalent. Reynolds seems to have forgotten that the transcription '*sayam*' is no more than an arbitrary convention for a Thai spelling (สยาม) which could just as legitimately be transcribed *syām*, for modern Thai equally arbitrary but more faithful to the early occurrences in Khmer, Cham and Pali (as in *Jinakālamālī* produced in Chiang Mai) where the *y* was a subscript indicating a cluster *sya*, not *saya*. What King Mongkut invented was the use of this ancient term of uncertain meaning as an official name for Thailand, *mōaṅ dai*. Note further, with respect to the use of '*syām*' in Khmer inscriptions, that it occurs at least seven times as a proper name, of both common workers and a high official, as early as the 7th century, but there is no indication that they were not local Khmer and an ethnic identity may not be imputed. That term, moreover, has remained as a rather common proper name until the present. It may be useful to call attention to a commonly proposed etymology among the faithful who insist that *syām*/Siam means 'Thai', Sanskrit *śyāma* 'dark'; Saveros Pou, *Dictionnaire vieux khmer-français-anglais An Old Khmer-French-English Dictionary*, p. 514, has allowed chauvinism to obtrude on science to the extent of glossing *syām* as 'dark-complexioned', 'barbarian', and 'Thai of

- Siam'. If one must raise the matter of complexion, it is far better to hypothesize that the Thai of the 10th-14th centuries within the area of modern Thailand were light-complexioned, as are the Thai/Tai of northern Thailand, Laos, and Vietnam today.
68. "Piltdown 2", pp. 342-343, Tatsuro Yamamoto, "Thailand as it is referred to in the Da-de Nan-hai zhi at the beginning of the fourteenth century", *Journal of East-West Maritime Relations*, Vol. I (1989), pp. 47-58; Geoff Wade, "The Ming Shi-Lu as a Source for Thai History 14th to 17th Century", paper presented at the 5th International Conference on Thai Studies-SOAS, London, 1993, p. 25. I wish to thank Dr. Wade for reminding me of this information. Charnvit Kasetsiri agrees that in the 14th century the Chinese intended Hsien, their rendering of 'siam' / syam (as it was written in Old Khmer and Cham), as a name for the lower Menam basin, including Ayutthaya, not Sukhothai. See his "Ayudhya: Capital-Port of Siam and its Chinese Connection in the Fourteenth and Fifteenth Centuries", *JSS*, Vol. 80, Part I (1992), pp. 75-81.
  69. "Piltdown 2", p. 337.
  70. Prasert-Silp, p. 89-90.
  71. Michael Wright, in *Sinlapa-Wathanatham*, Vol 9, No 3, cited in Dr. Prasert's response, Prasert-Silp, p. 92.
  72. Suriyawut-Silp, pp. 107-108.
  73. Prasert-Kun, p. 289.
  74. In addition to Inscription No. 2, they are Nos. 3, 4, 5 (of Lithai), Nos. 38, and 45.
  75. Prasert-Silp, pp. 41, 89; Thawat-Silp, p. 136. See *จินตามณี เล่ม ๑ และ ๒ กับ บันทึกเรื่องหนังสือ จินตามณี ฉบับพระเจ้าบรมโกศ*, Bangkok, 2512 [1969].
  76. Prasert-Silp, p. 50.
  77. *Discussion*, pp. 31-33.
  78. *Discussion*, pp. 70-73.
  79. *จินตามณี*, p. 173.
  80. Prasert-Silp, p. 41.
  81. *พงศาวดารเหนือ/ Bañśāvātār hnōa* ('Northern Chronicle'), Guru Sabhā edition, p. 11
  82. Vickery, "Guide", pp. 194-5; *พงศาวดารเหนือ*, p. 11; and on Tao Hung see James R. Chamberlain, "Remarks on the Origins of Thao Hung or Cheuang", in *Papers from a Conference on Thai Studies in Honor of William J. Gedney*, edited by R.J. Bickner, T.J. Hudak, Patcharin Peyasanitwong, Center for South and Southeast Asian Studies, University of Michigan.
  83. Thawat-Silp, p. 136; Prasert-Silp, p. 91.
  84. The following is from Prasert-Silp, pp. 42-3.
  85. "Piltdown 2", p. 349; Prasert-Kunming, p. 281.
  86. Prasert-Silp, p. 42. It is uncertain what Dr. Prasert meant by a Chinese-type Thai script; but later, pp. 87-8, he modified this statement to say that if the Thai Chuang in southern China had had a script, it would have been based on Chinese.
  87. The sixteenth-century example of an official Ayutthayan Thai-language document written in Khmer script is the Dansai Inscription, the subject of A.B. Griswold and Prasert na Nagara, EHS 24, "An inscription of 1563 A.D. recording a treaty between Laos and Ayudhya in 1560", *JSS*, Vol. 67, 2 (July 1979), pp. 54-69; the Khmer-Indonesian script is illustrated by the Grahi inscription (*Receuil des inscriptions du Siam*, II, pp. 29-31); on the special peninsular scripts see Michael Vickery, Review of *Prachum phra tamra baram rachuthit phu'a kalpana samai ayuthaya phak 1*, in *JSS*. Vol. 60, Part I (January 1972), pp. 403-410.

88. Prasert-Silp, p. 43.
89. Prasert-Silp, pp. 43, 87-8; Dr. Prasert's arguments were summarized again in Prasert-Kunming, p. 283
90. I owe this information about Lamphun Mon script to Dr. Christian Bauer.
91. It is not pertinent to deny the near identity of RK and modern tone marks on the ground that the former lacks *mai trī* and *mai catva*, introduced, according to Dr. Prasert, in Thonburi or early Bangkok times, and mainly used on foreign loan words.
92. See Marvin Brown, "Historical Explanations for the Peculiarities of the Thai Writing System", pp. 5-16 in Brown, *From Ancient Thai to Modern Dialects*, 2nd edition; for clear examples of ABC tones see the charts in James R. Chamberlain, "A New Look at the History and Classification of the Tai Languages", in *Studies in Tai Linguistics in Honor of William J. Gedney*, pp. 49-66.
93. Prasert-Silp, pp. 50, 88, 92, Prasert-Kunming, p. 284-5.
94. Prasert-Silp, p. 88.
95. See Brown, *op. cit.*, and the charts in James R. Chamberlain, "A New Look at the History and Classification of the Tai Languages", pp. 49-66.
96. *The Crystal Sands, The Chronicles of Nagara Sri Dharmmaraja*, translated by David K. Wyatt, Data Paper: Number 98, Southeast Asia Program, Cornell University (April 1975), pp. 16-17, 189 and 191 ('enter' / 'mountain'), 193 (*pho kha*).
97. *Discussion*, pp. 66-70.
98. Quotations from Anthony Diller, "Sukhothai superscript [']: tone mark or vowel sign?", abstract of paper for the conference panel in Chiang Mai. His paper, in Thai, was entitled "ไม้เอกมาจากไหน (Where did *mai ek* come from?)".
99. The results of these influences are very clear in, for instance, Inscription No. 49 of 1417. But one Khmer feature which is prominent there, and in contemporary inscriptions from Chainat, the connective particle *da* (transcribed in modern Thai as *dha*, originally written in Khmer as *ta*) is not found in the Thai Inscriptions of Lithai, although it occurs normally in his Khmer No. 4. Here is a perfect example of the process Diller has evoked, and evidence that it had not affected literate Sukhothai at the time relevant to the RK controversy.
100. It seems from Diller's paper, and from dictionaries at my disposal, that the term '*fon thòn*' has not been traditionally used by Thais to designate the *mai ek*-type sign used as a vowel marker. '*Fon thòn*' is the small vertical mark which turns the vowel sign for short /i/ ( <sup>^</sup> ) into long /ii/ ( <sup>="</sup> ).
101. Prasert-Kun, p. 281.
102. David Wyatt's treatment of Sukhothai-Nakhon Sri Thammarat relations in the 13th century, in *Thailand: A Short History*, pp. 50-56, constitutes historical fiction, but defies critical analysis because of the author's refusal to indicate sources for his speculative constructions.
103. Prasert-Kunming, p. 282.
104. Khmer script is still used today in Thailand to write Pali.
105. See Vickery, Review of *Prachum phra tamra baram rachuthit phu'a kalpana samai ayuthaya phak 1*, in *JSS*, Vol. 60, Part I (January 1972), pp. 403-410.
106. See particularly EHS 11-1, pp. 120-121.
107. Prasert-Kunming, p. 289.

108. See Piriya Krairiksh, *Muang Boran* 12/1 (January-March 1986; Piriya Krairiksh, ประวัติศาสตร์ศิลป์ในประเทศไทย ฉบับคู่มือนักศึกษา ('History of Art in Thailand, A Student Handbook'), Bangkok 1985; Piriya Krairiksh, *Art Styles in Thailand: A Selection from National Provincial Museums, and an Essay in Conceptualization*, Bangkok 1977; reviews of Art Styles in Thailand by H.G. Quaritch Wales and M.C. Subhadradis Diskul, both of whom, even though critical of Piriya's conceptualizations, nevertheless emphasized the Mahayana and Hindu character of peninsular art well into the 13th century; Stanley J. O'Connor, "Tamburlinga and the Khmer Empire", *JSS*, Vol. 63/1 (January 1975), 161-175.
109. "Ram Khamhaeng's Inscription: The Search for Context", published in the *Ram Khamhaeng Controversy*, which is the text cited here.
110. Woodward, p. 424.
111. Woodward, p. 427.
112. The work in question is Woodward's dissertation, "Studies in the Art of Central Siam, 950-1350 A.D.", Yale University, 1975.
113. Woodward, "Studies", pp. 128-130.
114. Note that Woodward ascribed a date, circa 1345, to Inscription No. 2, his 'Lö Thai's Inscription', which is no longer accepted by Dr. Prasert. Dr. Prasert subsequently dated it to around 1361, and with the monk protagonist of its story, not King Lö Thai, as author, and most recently as late as 1371 [Prasert-Kunming], pp. 287, 289.
115. Coedès, *Les états*, pp. 336-337. Note that David Wyatt's treatment of the religion of Nakhon in the 13th century, in *Thailand: A Short History*, is quite at variance with all of the above, and, p. 51, he inserts the amazing claim that "it was from Nakhon that monks carried the new Buddhism to the Angkorean empire".
116. I first heard Dr. Prasert mention this in the Canberra conference. Since then he has repeated it in Prasert-Silp, p. 89-90.
117. Prasert-Silp, p. 89.
118. G. Coedès, "Documents sur l'histoire politique et religieuse du Laos occidental", *BEFEO*, 25 (1925), p. 131, n. 1.
119. N.A. Jayavikrama, *The Sheaf of Garlands of the Epochs of the Conqueror*, being a translation of *Jinakalamalipakaranam*, Pali Text Society, 1968, quotation from p. 168, n. 1.
120. Jayavikrama, p. 171, Coedès, p. 131.
121. Jayavikrama, p. 168, n. 5.
122. Some writers call this a Sinhalese Sect, but that is of no import in the present discussion.
123. Saeng, in Jayavikrama, pp. xlv-xlv (further reference to Dr. Saeng's comments below are from the same location); Coedès, p. 106; Jayavikrama, p. 131. In fact Dr. Saeng interpolated from Thai and Mon tradition. The Asokan missionaries Sona and Uttara are not mentioned in *Jinakālamālī*, nor is any connection between Asoka and Southeast Asia; the only mention of the Nagaravāsi sect is in an interpolation attributed to Dhanit Yupho, p. 108, n. 7.
124. Dr. Saeng also made the identification with Ram Khamhaeng in his Thai translation of *Jinakālamālīpakarn*, printed as a cremation volume for Mr. Phongsawat Suriyothay, Bangkok (2518/1975), p. 148, and n. 3.
125. *Discussion*, p. 18, remarks by H.R.H. Princess Galyani Wadhana, contradicting Dr. Prasert's [Prasert-Kunming], p. 289, opinion that Dr. Saeng only appeared to question the authenticity of RK as a pedagogical device.

126. Thus, moreover, floating around the peninsula from west to east, for at that time ships from the west landed on the west side.
127. Coedès, p. 98-99; Jayavikrama, pp. 120-122.
128. Jayavikrama, pp. xv (Dhani Nivat), 120, nn.2, 3 (Dhanit Yupho); Jayavikrama, pp. ix; 121, nn. 2-3; 168, n. 4.
129. H.L. Shorto, *A Dictionary of the Mon Inscriptions From the Sixth to the Sixteenth Centuries*, p. 317, where the regnal dates, based on Mon inscriptions, are given as 1426-46.
130. See the letters of Princes Damrong and Naris, *Sān samtec*, Guru Sabhā edition, Vol. 11, p. 320; Vol. 13, pp. 1-3; 18-19. I have discussed them in "On Traibhūmikathā", *JSS*, Vol. 79, Part 2 (1991), pp. 24-36.
131. Prasert-Silp, p. 89 and Prasert-Kunming, 278: "King Ram Khamhaeng places all consonants in line, while Indian, Khom, and Mon write some consonants as subscripts ... why can the king not place all vowels on line too?"
132. Thawat-Silp, p. 138.
133. *Discussion*-Prasert, p. 35; Prasert-Kunming, p. 278, "The placing of i and ü on line is practiced in the north (Ins 62) and in Sukhothai (Ins. 2, 3, 8, and 102) at least as late as 1379". It is important to know the frequency of these occurrences. If, as in Nos. 2 and 3, they are isolated, they are hardly significant, and since Dr. Prasert and Griswold never mentioned them in their studies of these inscriptions, written before defence of RK had become an issue, we may assume they are all isolated cases.
134. EHS 10, "King Lödaya of Sukhodaya and his Contemporaries", *JSS*, Vol. 60, Part 1 (January 1972), p. 83. See also their treatment of No. 3 in EHS 11, Part I, "The Epigraphy of Mahadharaj I of Sukhodaya", *JSS*, Vol. 61, Part 1 (January 1973), p. 79, No. 8 in EHS 11, Part II, "The Epigraphy of Mahadharaj I of Sukhodaya", *JSS*, Vol. 61, Part 2 (July 1973), p. 102; No. 62 in EHS 13, "The Inscription of Wat Pra Yün", *JSS*, Vol. 62, Part 1 (January 1974), p. 125; No. 102 in EHS 7, "The Inscription of Vat Traban Jan Phöak...", *JSS*, Vol. 59, Part I (January 1971), pp. 157-188. In all but the last, they gave careful attention to orthographic peculiarities, but ignored the few cases of i or ü vowels written on the line.
135. See Diller's papers cited in note 4 above.
136. William J. Gedney, in his "Comments on Linguistic Arguments Relating to Inscription One", p. 209, insisted that Sukhothai in the time of 'Ram Khamhaeng' was a B language in which the voiced stops had not devoiced. Marvin Brown treated Sukhothai as a uniquely bizarre throwback to 'Ancient Thai', but I believe few linguists of Thai now accept his proposal. Diller, however (CM-1, p. 171), has now proposed that "[a]ll three of these languages [RK, White Tai, and modern Central Thai] have presumably derived from Proto-Southwestern Tai...but the exact details of this derivational path need not concern us here".
137. See William J. Gedney, "The Saek Language of Nakhon Phanom Province". *JSS*, Vol. 58 (1970), pp. 67-87.
138. Proto-Thai \*g, \*ɣ, \*gr, and \*ɣr have also merged as /kh/ in the PH languages, but this may be a separate problem.
139. Diller, "Consonant Merger 1", p. 171.
140. See my remark in "Piltown 2", note 29.
141. Examples of defective use of the two velar symbols may be read from some existing plates, and even if transcriptions such as those by Coedès and Griswold / Prasert may

not be perfect, it is safe to say that they took sufficient care to provide us with many useful readings.

142. For hypotheses about the life span of Śrīśraddhā, see EHS 10, pp. 72-74, 146. The date of Inscription No. 2 was first estimated at around the 1340s, and its author Lōthai. There now seems to be consensus that the author was Śrīśraddhā, and its date between 1361 and the 1370s. See A.B. Griswold, *Towards a History of Sukhodaya Art*; EHS 10; Prasert-Kunming, p. 287; comments above, note 15 and associated text.
143. te that Diller's "Consonant Merger 1" predates my "Piltdown 2".
144. Prasert-Kunming, p. 279.
145. Seen clearly in จารึกสมัยสุโขทัย (*Crūk samáy sukhodáy*), plate of face 1, p. 119, line 13.
146. Diêu Chính Nhìem and Jean Donaldson, *Tai-Vietnamese-English Vocabulary*, Saigon, Bô Giáo-Dục Xuất Ban (1970) p. 299; Edouard Diquet, *Étude de la langue tai* [Black Tai], Hanoi, 1895, p. 170; Li, *Handbook*, pp. 65-66.
147. Diêu and Donaldson, p. 169, *khók*. In the table in "Piltdown 2", p. 361, I glossed this inaccurately as 'place'. In จารึกสมัยสุโขทัย (*Crūk samáy sukhodáy*) the plates of faces 1 and 3 of No. 45 are sufficiently legible, and face 2 does not contain any controversial terms.
148. Diller, "Consonant Merger 1", p. 171.
149. The gloss is not at all significant for the problem at hand. For more precision, see James R. Chamberlain, review of *Historical Dictionary of Laos*, JSS, Vol. 80, Part 1 (1992), p. 155, "this word appears to derive from an ancient ethnonym for 'Austroasiatic', vestiges of which are found in the terms *Khmu*, *Khmer*, *Khom*".
150. Letter dated 12th Oct 1989. I am treating it at present as a sketch for CM-3, assuming that Diller will publish his new findings. Note that No. 45, at the very end of the 14th century, still showed almost perfect congruence with WT in the two velar terms in question.
151. Diller, CM-2, p. 493.
152. Griswold and Prasert, EHS 9, respectively figure 4 and page 183. The full text of the lithographic copy was given to Montigny, and I shall refer to it as the 'Montigny Plates'.
153. "Piltdown 2", pp. 363-64, 371.
154. Griswold and Prasert, EHS 9, p. 184. Thus those who have argued that because King Mongkut made 'many' or 'several' mistakes in his interpretation of RK it cannot be a product of his reign are off the mark.
155. 'Northern Tai' here follows the usage of Li, *Handbook*, and means a group of Tai languages most of which are found in China.
156. Diller, "Consonant Merger 1", pp. 183-4, n. 33 (This anomaly was not illustrated correctly in Table 2 of the publication of his "Consonant Merger 1"). Diller hypothesized that the reason was that WT script had been borrowed from the script of a Lao dialect in which /kh/ and /x/ were in non-distinctive free variation. Diller also suggested that the WT/BT scripts bear a distinct resemblance to Lao inscriptions from about 1600 described by Pierre-Marie Gagneux, in "Les écritures lao et leur évolution du XVe au XIXe siècles", *ASEMI* XIV, 1-2 (1983), pp. 75-95, but this is not supported by any of the examples of script illustrated by Gagneux.
157. There are still a few WT exceptions in the new structure, the terms for 'log', 'joint', 'year (of age of children)', 'to open', 'guest', with WT showing /x/ when /kh/ is expected, and *vice versa* (see Li, *Handbook*, pp. 194, 209). James Chamberlain informs me that these forms "are problematical ones in more than just WT so there may be other things going

- on....In situations like this we wait for more data"; he cited the Mène language for examples (letter 11 August 1991).
158. Diller, CM-2, p. 501
  159. See Li, *Handbook*, pp. 227-228 for "drive" (chap); Diêu and Donaldson, p. 373 /tsap/, and p. 369, /tsa/ "ethnic minority groups of the highlands of North Vietnam".
  160. Gedney, "Evidence for Another Series of Voiced Initials in Proto-Tai", 12th International Conference on Sino-Tibetan Languages and Linguistics, Paris, October 1979, p. 18; Li, *Handbook*, p. 238; James R. Chamberlain, "Mène: A Tai Dialect Originally Spoken in Nghê An (Nghê Tinh), Việt Nam", draft, 8 January 1991, p. 25.
  161. There is no third possibility. Sukhothai was either PH/Proto-B>PH or P/Proto-B>P. All records of the area indicate that it must have been of the PH type. No one has ever proposed that it was a P/B>P language, and such a hypothesis, particularly that it was a P language of the White Tai type, would lead to even more difficulties than the ones we now face. David K. Wyatt, *Thailand: A Short History*, p. 53, seemed to suggest this in his remark that "[t]he language of their [early Sukhothai kings] earliest inscriptions...suggests an affinity with the White Tai", but Wyatt clearly did not understand what is at issue. Wyatt, even more incongruously, was referring to Pha Müang and Bang Klang Hao, mentioned in Inscription No. 2, but who lived, and wrote if they did, a century before there is any evidence for the language of Sukhothai. In fact, Wyatt contradicts himself in the following sentence by noting correctly that "[t]hey themselves...left no record of their background or early careers". Nor is there any evidence whether or not, *pace* Wyatt, they "believed that certain spirits--dwelling in caves or on mountains located to the north up the Nan River valley and the Nam U valley of north Laos--would protect them".
  162. A reflex of another old cluster, \*xr, was preserved in the writing of some Lanna P languages as late as the 15th century. For examples see No. 76 from Phrae dated 1456, line 2 *hrok* 'six'; No. 67 from Lamphun dated 1488, lines 10, 13, 14, *hra* 'seek' and lines 14-15 *hrok* 'six'. No. 66 from Chiang Ray dated 1484, line 14, has *hrin* 'stone' (PT\**thrin*).
  163. กฎหมายตราสามดวง ('Laws of the Three Seals'), Guru Sabha edition, Vol. 4, p. 86, Vol. 2, p. 71, and Vol. 4, p. 118. I wish here to emphasize the great utility of the Japanese KWIK computerized index for this type of research. Diller, "Consonant Merger 1", p. 165, notes that 'request' was already being spelled with the incorrect (๓) in the 17th century. Diller made much of the erratic use of (๓)/(๓) in the rather large corpus of extant 17th-century writings to show that there was already then complete confusion and therefore no modern faker could have reproduced so many historically correct spellings. Those considerations are less relevant than the evidence in the *Three Seals Code* for actual early 19th-century usage among Bangkok scholars with respect to particular terms, whether or not there was global consistency throughout the entire vocabulary.
  164. Piriya Krairiksh, จารึกพ่อขุนรามคำแหง ('The Ram Khamhaeng Inscription'). Dr. Piriya has made valuable comparisons with citations from Ayutthayan and Ratanakosin literature which I have not consulted. For lack of space I have not cited details of Dr. Piriya's work, but in general I agree with the points he has made about the influence of other inscriptions and literature on the composition of RK.
  165. See Vickery, "A Guide through Some Recent Sukhothai Historiography", *JSS*, Vol. 66, Part 2 (July 1978), pp. 197-98. In his สารัตถคดี ['Fundamental Works'], p.65, Dr. Prasert ๓ Nagara objected to my dismissal of Indrapatindrdity as unhistorical, and suggested



that it represented the name of the Cambodian capital, Indrapat, plus the king's title 'indrādity'. This is impossible for two reasons, one historical and one linguistic. 'Indrapat', sometimes written *indrāprasth*, or in a more colloquial manner "Inthapat", as name of the old Cambodian capital did not evolve until its history had become lost in legend, and its earliest recorded use is late in the 16th century (see Michael Vickery, "Cambodia After Angkor, The Chronicular Evidence for the Fourteenth to Sixteenth Centuries", Ph.D. thesis, Yale University, 1977, p. 237; and Saveros Pou, *Dictionnaire*, p. 36, citing 'indrāprastha' as a "name given to the city of Angkor" in "l'Ep[oque] my. [moyenne]", which for Pou, p. II, began in the 15th century). The 'sruk *indrāparāss*', which Pou, p. 36, cites from an eleventh-century inscription, may not be assimilated to 'indrāprastha', and in that context it does not refer to the capital. The linguistic objection is that had that name been used as suggested by Dr. Prasert, the full title would have been 'Indrapatindrādity', with short /i/ in the fourth syllable, for the long /ii/ of "Indrapatindrādity" represents sandhi of *pati+indra*. Nevertheless, I may have been too hasty in my original statement, for 'Indrapatindrādity' might be interpreted as 'Lord of Kings', with 'Indra' taken as 'king' rather than 'god'. It would still be a retrospective enhancement of title, not a title borne by the person in question.

166. The Thai contexts are respectively in EHS 10 (Inscription No. 2), p. 96, line 69; and EHS 9, p. 197, line 8. Their translations are EHS 10, p. 116, and EHS 9, p. 204.
167. EHS 9, p. 206; EHS 11, Part 1, p. 109 and n. 125.
168. William J. Gedney, "A comparative Sketch of White, Black and Red Tai", *The Social Science Review*, Special Number, 14 December 1964, p. 42; Li, *Handbook*, p. 148, n. 39. In White Tai and Lao the equivalent term is 'phai', perhaps from *phū tai*, and in Black Tai it is *fai*.
169. Inscription No. 45 is in EHS 3, "The Pact Between Sukhodaya and Nān", *JSS*, Vol. LVII, Part I (January 1969, pp. 57-108, and the phrases in question are from Face I, lines 17-18 and line 32; Inscription No. 15 is in EHS 16, "The Inscription of Vāt Brah Stec, near Sukhodaya", *JSS*, Vol. 63, Part I (January 1975), pp. 143-160, with the relevant phrase in Face III, lines 12-13. "A Glossarial Index of the Sukhothai Inscriptions" is by Yoneo Ishii, Osamu Akagi, and Noriko Endo, The Center for Southeast Asian Studies, Kyoto University, November 1972.
170. EHS 9, pp. 206-207 and n. 28.
171. EHS 11, Part I, p. 110, n. 140. {...} indicates my explanatory interpolation.
172. In addition to the long notes in EHS 9, p. 206, nn. 27-28, see Bradley, pp. 48-49, Coedès "Notes critiques", pp. 3-6.
173. EHS 9, pp. 206-7, modified to show the uncertain status of plurals.
174. As the Dutch representative in Ayutthaya, Jeremias van Vliet, reported in the 1630s, one of the controversies surrounding the struggles preceding the enthronement of King Prasat Thong was whether legitimate royal succession was from brother to brother or father to son. The Bangkok kings tried to firmly establish father to son succession, as seen in their Palatine Law, which of all the *Three Seals Code* shows the most evidence of rewriting. See Vickery, "Prolegomena to Methods for Using the Ayutthayan Laws as Historical Source Material", *JSS*, Vol. 72, Parts 1&2 (January and June 1984, pp. 37-58; and Vickery, "The Constitution of Ayutthaya", paper presented at the Fifth International Conference on Thai Studies, SOAS, London, 4-11 July 1993.
175. Coedès, in *Receuil des inscriptions du Siam*, did not offer a translation; and it has not been treated by Griswold and Prasert.

176. Bradley, p. 26; Coedès, "Notes critiques", pp. 4-6; Griswold and Prasert, EHS 9, pp. 206-207.
177. *Linguistic Survey of India*, "Ahom", pp. 97, 124; Li's entry, *Handbook*, pp. 236-37 suggests the gloss 'pass' is rather widespread.
178. Khmer, as recorded since the seventh century, has only velars /k/, /g/, /kh/, and possibly /gh/ in ancient Khmer, with written forms corresponding to Thai ก, ฦ (but in Khmer unaspirated), and ข, ฦ respectively. There is no symbol in Khmer corresponding to Thai kh khon (ฦ).
179. Griswold and Prasert, EHS 11, Part I, "The Epigraphy of Mahdharmaraj I of Sukhodaya", JSS, Vol. 61, Part I (January 1973), pp. 71-182, p. 110, n. 140; Prasert-Kunming, p. 282, where in contradiction to his own spelling in EHS 11/1, note 140, and to the Phayap Dictionary, *หลักภาษาไทย* p. 440, ฦ, he said that the Chaing Mai ('Tai Yuan') word was written with kh khon; for the merger see Li, *Handbook*, p. 214.
180. EHS 9, p. 207.
181. EHS 11-1, p. 154.
182. EHS 9, p. 207, n. 34; EHS 11-1, p. 155, n. 17.
183. *Three Seals*, vol. 3, "braḥ āyakār pet srec", pp. 164 (art. 139), 165 (art. 140), 178 (art. 167/146). These are the only occurrences of these expressions in the entire *Three Seals Code*, and this probably indicates that they were special legal terms at a particular time.
184. The transcription of the sentence from art. 139 is, phū<sup>2</sup> rāy<sup>2</sup> deen sām diār t̄āy ca: tai<sup>2</sup> mī phit p̄heek tvay<sup>2</sup> phū<sup>2</sup> t̄ai hā mi tai<sup>2</sup>. The passage contains other terms needing further elucidation too, but not relevant to the present subject. At least the meaning of 'phit p̄heek' seems certain.
185. *Three Seals*, Vol. 4, p. 55.
186. This type of Khmer loan word is also found in zok/ซอก 'a narrow passage' < Khmer jark ( ជ្រក ), zp/ซัป 'soak, imbue' < Khmer jrp ( ជ្រក ) 'soak up water', zrau/ ซรา 'crevice' < Khmer jrau ( ជ្រៅ ) 'deep', also reflected in Thai drau/ เถรา, as in the name Cha Choeung Sao/ ฉะเชิงเทรา 'deep river' [cha choeung < Khmer stūn, ស្ទឹង. ].
187. The principal date of the *pet srec* law, and under which the first context of *phit p̄heek zeek* occurs, is Buddhist Era, and is one which I have questioned in Vickery, "Prolegomena to Methods for Using the Ayutthayan Laws as Historical Source Material", JSS, Vol. 72, Parts 1&2 (January and June 1984, pp. 37-58); but the content of the law, and its true date in aka era, could conceivably be from the 14th century. Chit Phumisak, *สังคมไทยลุ่มแม่น้ำเจ้าพระยา ก่อนสมัยศรีอยุธยา* ('The Society of the Chao Phraya Basin Before the Ayutthaya Period'), p. 45, claimed that the date of the royal preface preceding the second context of *phit p̄heek zeek*, which he read as 1156, instead of 1146, should be understood as aka, equivalent to A.D. 1234. For him this was evidence of a pre-1351 Thai kingdom in the vicinity of Ayutthaya. I would agree that there was a state in that location at that time, that post-1351 Ayutthaya was built on it, and perhaps even represented a direct continuing phase. I do not, however, believe that the earlier state, or even 14th-century Ayutthaya, was Thai; I consider that the preface in question is by King Rama I of Bangkok, in cula year 1146, A.D. 1784, but with a content which was already part of an old Ayutthayan law, perhaps with some modifications, but with old Ayutthayan terminology, such as *phit p̄heek zeek ān*.

188. SoSethaputra, *New Model Thai-English Dictionary*, Vol. I, p. 315; Wit Thiengburanathum, *Thai-English Dictionary*, p. 340. Similar glosses in Thai are listed in the *Royal Institute Dictionary*, 2525 Edition, p. 277.
189. Three Seals, Vol. 3, p. 197, *Braḥ aiyakār lākṣana vivād*, art. 29.
190. I now reject the interpretation I offered in the first version of this paper, presented in Chiangmai, “perhaps *phit vāñ* in No. 5 is a misreading, by all readers, of what was intended as *phit āñ*, a type of misreading, confusion of the independent a vowel and consonant v symbols, that is rather easy in many types of Thai script”; although I maintain, from another point of view, my continuing explanation, “we might suspect that late authors of RK had seen No. 5, misunderstood it, and interpolated what they read into a legal phrase they knew from the Three Seals Code”.
191. EHS 9, p. 207; EHS 11-1, p. 154, for translations.
192. See EHS 9, p. 207 and EHS 11-1, p. 110, where the interpretation, given the size of the lacunae, is fanciful.
193. See EHS 9, p. 208 and n. 47; EHS 11-1, p. 155.
194. See Jay W. Fippinger, “Black Tai Sentence Types, A Generative Approach”, in *Studies in Tai Linguistics In Honor of William J. Gedney*, edited by Jimmy G. Harris and James R. Chamberlain, p. 157, (130), “*hau pa:i sūk pa:i sūa*”, “we flee enemy flee enemy”, or “We fled from the enemy”. For Ahom, see *Ahom Primer*, Gauhati, 1968, p. 12.
195. See respectively EHS 9, p. 212; and EHS 11, Part 1, pp. 156 for No. 5 and 139 for No. 4. In discussion of No. 4 I have suppressed the ś in śāmī, incorrect both in Thai and in Sanskrit (*svāmī*).
196. The title *pū grū* is also unusual in Sukhothai, and Griswold and Prasert found it awkward to explain. EHS 9, p. 212, n. 81 and p. 211, n. 77.
197. Suriyavut-Silp, p. 117.
198. Betty Gosling, “Sukhothai Religious Architecture and its Relevance to the Authenticity of Inscription One”, p. 244, note 10. The Inscriptions of Lithai’s time show less admixture of Khmer in Thai than do later Thai inscriptions, particularly after the end of the 14th century, probably influenced from Khmer Ayutthaya rather than from Cambodia. If the RK Inscription were taken as genuine, and studied from this point of view, it might be considered evidence against the hypothesis of Khmer domination of Sukhothai.
199. Betty Gosling has opined (personal letter 25 October 1991) that on the basis of thirteenth-century Sri Lankan usage, in which “mahathera appears to have been an early classificatory title, whereas mahasamin...was bestowed on especially notable monks, or mahathera, in the thirteenth century”, “at Sukhothai I can see a mahathera being honored as mahasami in the Luthai period but probably not in RK’s...”. I do not find this argument convincing, and it requires more precise demonstration. The important detail, for me, is still that RK resembles Lithai’s Khmer No. 4, which King Mongkut possessed, more than his Thai-language No. 5.
200. *Linguistic Survey of India*, “Ahom”, pp. 93, 127. It is stated there that the same feature is found in Khamti and Shan; Diêu and Donaldson, pp. 23, 24, 135. Note that in Diêu and Donaldson the letter ‘c’ represents the consonant /k/.
201. I have not seen Wright’s first presentation, and became aware of it through Dr. Prasert’s rejoinder in Prasert-Silp, p. 92. Wright mentioned it again in Wright-Silp, p. 99.

202. Letter, 25 October 1991, citing McFarland's dictionary, p. 51 (also in the Royal Institute Dictionary, p. 50).
203. EHS 9, pp. 209-210.
204. Without any attempt to comment on its significance, it may be worth noting that the authority for the definition of *krn kahin* in the Royal Institute Dictionary is the *อุปมบทวิธิ* ('Ordination procedures') written by Prince Patriarch Vajirayana Varorot, a son of King Mongkut. The term *กรรม* in connection with *kahin* is also found in one passage of the Law on the Sangha of King Rama I (*Three Seals Code*), Vol. 4, p. 197.
205. I have discussed the gifts presented at these festivals with reference to 'cowries'. See above.
206. See Vickery, "The Old City of 'Chaliang'--'Sri Satchanalai'--'Sawankhalok': A Problem of History and Historiography", *JSS*, Vol. 78, Part 2 (1990), pp. 15-29, and note 6 on the orthography of this name.
207. See Inscription No. 4, face 4, lines 7 and 16 which are visible on the plate published in *Cārūk samáy sukhodaya*, p. 240.
208. พระราชพงศาวดารกรุงรัตนโกสินทร์รัชกาลที่ ๑ ('Chronicle of the First Reign'), National Library Edition, Bangkok (2505/1962), p. 235, when King Rama I was having Wat Bra. Jetuban (Wat Pho) constructed in 1789, he had 1248 damaged images brought from Phitsanulok, Savarrgalok, Sukhothai, Lophburi, and Ayutthaya to be repaired and placed in Wat Pho.
209. One inscription on a Buddha image from 'Sagalok' is that of Ny Dit Sai, to which Dr. Prasert called attention (see above). See EHS 7, p. 158, on the "primate of the monkhood in the Sukhodaya region, who had had some experience in reading Old Siamese", and who tried to help Prince Vajiravudh read an inscription which Griswold and Prasert believe may have been No. 102.
210. James R. Chamberlain, "The Efficacy of the P/PH Distinction for Tai Languages", in *The Rām Khamhaeng Controversy*, Chapter 12, pp. 453-496, see p. 475.
211. Inscription No. 1, Face 3, line 6; EHS 9, p. 214; EHS 3, "The Pact Between Sukhodaya and Nān", p. 83, n. 24; Vickery, "The Old City of 'Chaliang'--'Sri Satchanalai'--'Sawankhalok'", note 19; and No. 98 in จารึกสมัยสุโขทัย, pp. 361-367.
212. Coedès, French translation of No. 1 in *Receuil des inscriptions du Siam*, première partie, p. 46, from which I have made an English translation. Coedès, with his knowledge of Khmer, saw that *khbu* was Khmer *khbañ/khpon* ( ( ឃ្នង ) ), 'top or ridge of a mountain' (The gloss here is from Robert K. Headley, et. al., *Cambodian-English Dictionary*, Volume I, p. 101). In fact, the context of Inscription No. 98 indicates that *khba* might have been no more than a name of a hill. George Coedès, "Les premières capitales du Siam aux XIIIe-XIVe siècles", *Arts asiatiques*, III/4, pp. 264, ff., cited in EHS 9, p. 214, n. 95, but which I have not been able to consult; Griswold and Prasert, EHS 9, p. 214, and their Thai transcription of RK, p. 200, Face 3, line 6, where *kbu* appears as
213. From respectively Wit Thiengburanathum, *Thai-English Dictionary*, p. 288, and *The Royal Institute Dictionary*, p. 198.
214. *Three Seals*, 3/109/15, [1/156/02, ] 3/115/18; D.B. Bradley, *Dictionary of the Siamese Language*, Bangkok (1873), p. 94, 'แปล ชื่อ ที่ ดอน ฤ ที่ สูง, คือ คน ขน เอา ดิน มา ภูน ขึ้น, ให้ แปล เนิน สูง นั้น'.
215. C.B. Bradley, "The Oldest Known Writing in Siamese", *JSS*, Vol. 6/1 (1909), p. 28.

216. Dr. A. Bastian, "On some Siamese Inscriptions", *Journal of the Asiatic Society of Bengal*, 34/1 (1863), p. 34. Bastian was not mistakenly referring to the dam (สระ) which he had paraphrased in his previous clause as "pretty lake with plenty of fish". On *kòk* see Vickery, "Some New Evidence for the Cultural History of Central Thailand", *The Siam society's Newsletter*, 2/3 (September 1986), pp. 4-6. Mon /*kòk*/, like Thai and Khmer โคน, is written with original initial g.
217. Griswold and Prasert, EHS 3, "The Pact Between Sukhodaya and Nān", p. 83 and note 24.
218. Chamberlain, "The Efficacy", pp. 472-73.
219. Michael Vickery, "The Constitution of Ayutthaya", paper presented at the Fifth International Conference on Thai Studies, SOAS, London, 4-11 July 1993.
220. Note that one Thai scholar, Jit Phumisak, would have none of such idealization, and for him RK showed an exploitative feudal society not long after the transition from slave society. See จิตร ภูมิศักดิ์ สมสมัย ศรีสุทธรพวรรณ โฉมหน้าศักดินาไทย ('The Face of Thai Feudalism'), Bangkok 2518/1975 pp. 126-28.
221. See discussion above, pp. 11-12.
222. See Griswold and Prasert, EHS 9, footnotes, 20, 26.
223. See Griswold and Prasert, EHS 7, "The Inscription of Vat Trabān Jān Phōak", *JSS*, Vol. 59, Part I (January 1971), pp. 189-208. I do not agree with Griswold and Prasert that this inscription is a record of Ayutthayan intervention in Sukhothai.
224. See Griswold and Prasert, EHS 8, "The Inscription of Vat Jān Lòm", *JSS*, Vol. 59, Part I (January 1971), pp. 189-208.
225. I am not convinced by the argument that พ่อ should be construed as 'breast father' (and therefore 'foster father') by analogy with แม่ แม่หม่อม, 'breast mother', that is, 'wet nurse', but the point is not relevant here.
226. See Griswold and Prasert, EHS 2, "The Asokma Inscription of 1399 A.D.", *JSS*, Vol. LVII, Part II (January 1969), pp. 29-56.
227. Information on Black Tai is from Georges Condominas, *From Lawa to Mon, from Saa to Thai*, on which, however, see my review in the *Thai-Yunnan Project Newsletter*, 13 (June 1991), pp. 3-9, Edouard Digue *Étude de la langue tai...*, and corrections concerning the terms *kuo* and *ñòk* from James Chamberlain, personal correspondence. The Lue data are from Jacques Lemoine, "Tai Lue Historical Relation with China and the Shaping of the Sipsong Panna Political System", in *Proceedings of the International Conference on Thai Studies*, The Australian National University, Canberra, 3-6 July 1987, Volume 3, pp. 121-134. In the following table I have regularized transcriptions in accordance with standard conventions, retaining some of the Black Tai and Lue features. Where confusion might result, I have inserted standard Thai spellings.
228. The Lue category *lūk lān tāw phyā* suggesting equivalence with Sukhothai *lūk cau lūk khun*, indicates low-ranking descendants of nobility, who are free peasants. The Sukhothai rank should probably also be interpreted as lower nobility and royalty (*rājakula*) who occupied lower levels of the administration, such as going to meet honored official guests as they did in Inscriptions No. 4 and No. 5.
229. See the description of Naresuan's reign in Jeremias van Vliet, *The Short History of the Kings of Siam*, translated by Leonard Andaya and edited by David K. Wyatt, and my review article in *JSS*, Vol. LXIV, 2 (July 1976), pp. 207-236.

230. In what follows 'Indic' means the early Khmer, Mon, and Cham scripts, not scripts used earlier in India. My Mon examples, in so far as possible, are taken from the Mon inscriptions of upper Burma published by G.H. Luce in *Old Burma-Early Pagan*. This is the variety of Mon considered relevant by B.J. Terwiel who believes that Ahom script originated in Mon (personal correspondence with Terwiel). The Black and White Tai examples are based on George Coedès, *ตำนานอักษรไทย* ('Story of Thai writing' [Black and White Tai]), Bangkok, Guru Sabh, 2507/1964; Louis Finot, "Recherches sur la Littérature laotienne" [Black and White Tai], *Bulletin de l'École Française d'Extrême-Orient*, Tome XVII (1917), Pl. I; Diêu Chính Nhâm and Jean Donaldson, *Tay-Vietnamese-English Vocabulary* [White Tai], Saigon, 1971; Edouard Diguët, *Étude de la langue tai* [Black Tai], Hanoi, 1895; and James R. Chamberlain, "The Black Tai Chronicle of Muang Muay Part I: Mythology", typescript, December 1984, kindly supplied by Chamberlain. As noted below there is some variation in forms among these sources.
231. Whether they were really pre-glottalized or something else is not significant here, and that question is ignored.
232. In Diêu and Donaldson the Tai script is illustrated in the introduction, but the Dictionary entries are in romanization, without distinction of high and low series consonants.
233. Coedès and Finot show the second White Tai *kh* symbol as representing the low consonant; Diêu and Donaldson do not explain it, but it is listed with the other low series consonants; Diguët also shows it as a second series *kh* in Black Tai, but lists only two words, neither of which have cognates in modern Thai. Thus the standard Black Tai *kh* is that shown to the left of the slash in Table A.
234. Dr. Prasert [Prasert-Silp, p. 88] has objected to this analysis, saying that originally Ahom did not have a *d dek* (original Indic *t*) symbol, and that only in the Ratanakosin (Bangkok) period did they adapt their *n* to make a symbol for their /d/, because /n/ and /d/ are phonetically similar in many Thai languages. He did not explain how words with initial /d<?\*d/ were originally written in Ahom. Dr. Prasert's view is contrary to everything I have seen written about Ahom; and one person now undertaking special studies of Ahom, Dr. B.J. Terwiel, has written that "the consonants 'd' and 'n' seem originally to have been separate letters, distinguished only in that the 'd' possessed a markedly larger loop at the lower right-hand side of the letter ... but in manuscripts the two are usually indistinguishable..". (B.J. Terwiel, draft of "Ahom script: Its Age and Provenance"). A glance at Table A shows that a certain similarity between symbols for *d* and *n* goes back to the Old Khmer, Mon, and Cham scripts.
235. Terwiel, op. cit.
236. As evidence that such a suggestion has really been made see Craig J. Reynolds, "The Plot of Thai History", in *Patterns and Illusions Thai History and Thought*, Edited by Gehan Wijeyewardene and E.C. Chapman, Singapore, Institute of Southeast Asian Studies, 1991, p. 323, Luang Vichit Vadakarn "even likened the Thai writing system of AD 1283 to the European one, because it placed all vowels and consonants on a single line, a sign of Thai liberation from Cambodia and the *khom* writing system, as well as of equality with Europe".

237. The superfluous character of aspirate consonants as perceived by Austronesian speakers is seen in their use in Javanese as a sort of capital letter; and there are examples in Cham of their use, not to represent original aspirate consonants, but phonetically aspirate situations where a vowel had been elided, as *tahun* 'year' > *thun* in Cham.
238. This is illustrated in A. Cabaton, *Nouvelles recherches sur les Chams*, Paris, Leroux (1901), pp. 73, 76 for two varieties of early 20th-century Cham.
239. This statement is somewhat unclear, for after the 8th century there are important differences between Khmer and Javanese script. Eighth-century Cham is rather close to Cambodian script.
240. For a clear example see the illustration of inscription พร. 1, in Chiang Mai, on p. 208 of นางสาวกรรณิการ์ วิมลเกษม (Miss Kannika Vimolkasem), *อักษรฝักขามที่พบในศิลาจารึกภาคเหนือ* ('Fak Kham Script Found in Inscriptions of Northern Thailand') Silpakon University, 2527 / 1974.

