CHAPTER 9

PILTDOWN SKULL — INSTALLMENT 2 REMARKS OFFERED TO THE RAM KHAMHAENG PANEL

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Introduction

It was with great pleasure that I received Betty Gosling's invitation to participate in the Ram Khamhaeng (RK) panel at the Asian Studies Conference, but unfortunately my newly assumed teaching duties since July 1988 prevent me from travelling to the U.S. at this time, and, together with the move out of Australia which began in February 1988, have prevented me from completing what I had projected for my second RK installment, and for which I would have found this conference an appropriate venue for its presentation.

In spite of these impediments, I fear it would be seen, and indeed I would feel it, as a cop-out if I declined to provide any contribution to the ongoing discussion, since it appears that my Canberra paper has taken the lid off long accumulated pressures and is responsible for there being an RK panel at this conference. There has now occurred a psychological breakthrough, corresponding to that which, according to an adviser of the present Thai Prime Minister, has occurred in another area in which I have been involved as a historian, the conflict in contemporary Cambodia.

I regret that I have not had time to prepare a well - organized scholarly paper, and indeed what follows may appear as stream - of - consciousness ruminations, because in the haste with

which they were set down that is indeed what they are. Still, I feel it is better to offer them in this unorganized form rather than to avoid the occasion entirely.

I must first state again clearly, perhaps more clearly, what it is that I am trying to do, and certain things that I am not attempting. Obviously my Canberra title, "...Piltdown skull...?, though ending in a question mark, implied that I considered the RK inscription to be a Piltdown skull, indeed a "...fake...," as Anthony Diller has quoted from my paper.[1]

Indeed I do consider RK a fake, and have thought so since I first began trying to read the Sukhothai inscriptions some years ago, when a graduate student at Yale. In fact I believe that if all other evidence about Sukhothai had been discovered and studied in the sequence which has prevailed, but inscription No. 1 only dug out of obscurity now, rather than in the 1830s, it would be dismissed out of hand by all historians and archaeologists as an interesting hoax perpetrated by some of the people who are skillfully 'restoring' old Sangkhalok ceramic ware.

In spite of drawing this conclusion early on, I did not try to pursue it in print, nor even for many years to try to study Sukhothai inscriptions, in the realization that the conservative nature of the Southeast Asian historical community would not permit an objective hearing. I threw out a few hints in "Guide Through Some Recent Sukhothai Historiography," having discovered that two respectable old Thai scholars whose patriotic nationalism was beyond reproach also had their doubts about RK, and then, following meetings with several Thai historians whose underground doubts about RK had long flourished, I decided that the proper time to bring anti - RK out of the closet had arrived. Indeed closet suspicion of RK within Thai circles may always have been much stronger than any of us realized, for now Sujit Wongdes has revealed that one of the old masters of Thai language and literature, Saeng Manvitoon, kept telling his students that RK had been composed by King Mongkut, but they all thought he was only joking.[2]

When I say that RK is a fake, I *do* mean that I think it was

not written when its text alleges, but at a significantly later date. I am not, however, trying to prove that it was written by King Mongkut, or by any other individual, or at any particular time. Thus counter arguments which aver that "...etymology shows the RK inscription cannot have been written in the Ratanakosin period," are irrelevant to my argument.[3]

We must also realize that until there is some foolproof scientific test to apply to the stone itself to determine the date of incision of the inscription, it will be impossible to prove absolutely that RK is, or is not, a genuine 13th century period piece, that all arguments are inferential, and that the question revolves around the relative logical solidity of the arguments as read by scholars seeking the truth. There will always be some room for disagreement. Moreover, even if fakery cannot in the end be demonstrated, or even if the better arguments prove to be on the side of authenticity, there are so many anomalies in the inscription, and even more in the linguistic work surrounding it, that the discussion will have served to advance historical knowledge.

Since I consider that the placing of all vowel signs on line is a crucial indicator of fakery, that such drastic changes of a traditional system can only result from strong external cultural influences,[3a] not from a great mind foreseeing advantages several centuries hence, I consider that RK cannot have been written before the 17th century, probably not before the reign of King Narai, when there was strong interest in Europe on the part of Thai elites. Unless, as I said in "Guide," it can be shown that there was some other Indic source for such an arrangement. To the extent I have been able to investigate such a possibility, which has been limited, the only possibility would still seem to place such influence no earlier than the 15th century.

What I wish to demonstrate, however, is that the crucial fakery of RK, regardless of when it was composed, lies in its inaccurate information about 13th century Sukhothai, and that it must be rejected as a source for Sukhothai, or Thai history. Since I am mainly interested in the status of RK as a historical source, and not in proving that it was embarrassingly faked by some later cultural hero, perhaps purists could cut a deal with

me. I will lay off 'fakery' if they will announce that RK has no bearing on, and no validity, for any question of Sukhothai history. Since contemporary sources generally contain something of value for reconstructing the history of their period, 'unhistorical fake' generally implies later compositions by authors ignorant of the true historical situation; but even those who insist on the veracity of RK's claim to composition in 1292, might still face problems regarding the veracity of its contents.

For example, did King Ram Khamhaeng invent Thai script as alleged? If this is what is claimed, but if there is good evidence that such a claim is untrue, the inscription is no less a fake, as historical evidence, than if it were written in the 1830s. Of course I know that there has been a movement, since at least the work of Coedès, to counter this difficulty by concluding that King Ram Khamhaeng only claimed to have invented *this* Thai script, which is unique, in contrast to other previously existing Thai script(s). The present most purist defender of RK authenticity will have none of this. For Dr. Prasert na Nagara, the term 'this (Thai *nī)* does not in RK mean 'this,' but serves rather as a kind of definite article, as is proved by the several occurrences of the phrase *'möan˙ Sukhodai nī ,'* when there can have been no other Sukhothai in question.[4] I agree with Dr. Prasert's reading on this point, and think there can be no way to counter it. What the RK inscription says, and what its author intended, was that King Ram Khamhaeng invented *Thai writing,* not *this* Thai writing in contrast to some other kind of Thai writing. This use of *'nī'* is extremely interesting in itself, and may serve to identify RK textual relationships, as I take up below.

There are several other RK details which seem clearly anachronistic, thus a kind of fakery, even if the text itself may not be redated to a significantly later period. I will not be able to discuss all of them here, but shall give some indication of what I mean.

I have been asked why I wished to evoke such a controversial subject, as though such challenges were outside the bounds of what a historian should do. The answer to this is that a historian's first duty is to check the validity of his sources and

criticize them. This should be most clear for historians of early Southeast Asia, where the sources, and interpretation of them made by the first generations of modern historians, abound in inconsistencies, contradictions, and the most blatant dilettantish ad hoc speculation. Since my own first introduction to Southeast Asian history, and its sources, had been in the field, in Cambodia, Laos, and Thailand, without benefit of much familiarity with the formal scholarly milieu, I was astonished on my reentrance to academia in 1967 to realize that such matters were given altogether too little attention. I found it necessary, in order to try to answer, even if only for myself, questions that arose in my own reading of primary sources, to devote my research to the analysis of primary sources to determine what portions of them merited inclusion in futher syntheses as *history.* The results were my dissertation on Cambodian chronicles and Ayutthayan chronicles relevant to early Cambodian history, and a series of articles on several different Thai sources with which most of you are familiar. What I am doing with RK is no more than a continuation of those studies which I undertook in the 1970s.

Since my major interest in RK, and my grounds for criticizing it, lie in the problem of its status as a source for 13th - century Sukhothai history, I shall begin my contribution to the present discussion with consideration of the role of RK in Thai history.

Obviously RK played no role in the writing of Ayutthaya, Dhonburi, or Ratanakosin history before it was discovered, nor apparently was it of interest to any Thai historian writing before the time of King Vajiravudh.

Neither does RK appear to have exerted much influence on the historical or political content of subsequent Sukhothai writings, even though in mid-14th century, when most of the Sukhothai corpus was composed, there must have been scholars still alive who would have known King 'Ram Khamhaeng.' The rest of the Sukhothai documents, even in their historical sections, ignore the very name 'Ram Khamhaeng,' his script, orthographical conventions, language usage, religious activities, and economic initiatives. Some of his achievements are even attributed to others. In spite of this total neglect of Ram Khamhaeng and his achieve-

ments, the later Sukhothai writers nevertheless, assuming that RK is a 13th century piece, show studied familiarity with its text, which means deliberate rejection of all that Ram Khamhaeng stood for by his grandchildren's and great grandchildren's generations. There are numerous parallel passages between RK and several later Sukhothai inscriptions, which to the extent they have been given notice have been attributed to close familiarity with RK on the part of his descendants. These passages are peculiar, however, in that exhibiting RK form they ignore RK content. They form an important element in RK studies: and those who argue for late composition of RK must show that the parallel passages are better explained by a hypothesis that the writer(s) of RK knew and copied from inscriptions 2 - 3 - 4 - 5, etc., than that the writers of those genuine Sukhothai inscriptions knew and imitated RK.

I repeat, since it received too little emphasis in my Canberra paper (p. 208), which was only a partial statement of my position, that the writer(s) of RK, if it is not genuine, knew and worked from inscriptions 2 - 3 - 4 - 5, and perhaps others, or copies of them. Some of the arguments I shall make below must be read with this assumption in mind.

By its very nature this argument, like that on any detail of RK, can never be absolute, but in the end the decision must be according to the most reasonable inferences about the evidence according to prevailing notions of scientific evidence.

RK has only played a role in history in the 20th century, and there it has influenced everything written, even caused a falsification of many historical issues, which has been realized, though the subject has never been faced squarely, nor proper conclusions drawn.

Thus Sujit Wongdes has written that he was first taught that every word of RK had been written by King Ram Khamhaeng, but now it is admitted even by purists that the work of King Ram Khamhaeng is limited to only parts of the inscription, with some of it believed by all to have been written later.[5]

This development of even purists' views on what is genuine in RK can be seen in the treatment of certain historical issues from Prince Damrong, through W.A.R. Wood, Griswold and Prasert, David Wyatt, and the discussion in *Silpavathanatham.*[5a]

This change, often insufficiently acknowledged, in the official historical line, emphasizes the necessity of criticizing sources which I noted above. Even the most simplistic king - and - battle history requires preliminary source criticism in order to establish what were genuine kings and battles and to eliminate false kings and phantom conflicts. A look at Wyatt's *Thailand: A Short History,* the latest generation of king - and - battle history, and explicitly so, apparently because the author thought that was what his public wanted, will illustrate my point.

In general Wyatt has refused to take account of advances in source criticism over that past 20 years, and for chapters 2-5, his treatment is very reminiscent of that of W.A.R. Wood. Nonexistent kings and battles are not lacking. The most glaring example concerns Ayutthayan invasions/occupations/conquests of Angkor in the 14th-15th centuries. Some years ago I demonstrated that 5 sets of a pair of Ayutthayan invasions ranged in different chronicles from 1351-52 to the late 15th century, all derived from chroniclers' misunderstandings of records about a single invasion, conquest and 15 - year occupation between 1431 and 1444; and at the time Wyatt gave every evidence of accepting the analysis.[6] In *Thailand,* however, Wyatt wants to have it all ways. The various chronicular notices of attacks on Angkor between 1351 and 1369 are subsumed under "warfare, in the course of which Angkor itself may temporarily have been occupied." Then after 1388 Wyatt feels obliged to say that Ramesuan "sent an army into Cambodia that again (sic) took Angkor," even though it is not "attested with any certainty by reliable sources," thus admitting that he is writing the history of phantom battles. Further on, moreover, Borommacha II after 1424 "inherited...armies tested against the Khmer...," in warfare which Wyatt himself plainly doesn't believe occurred. Then leading up to the single authentic record of an Ayutthayan invasion of

Angkor, Wyatt still wishes to include bad history, the invasion of 1421, which most clearly of all the phantom battles was a miscalculation from 1431, by saying "(b)y the 1420s Angkor was clearly in decline," which may have been true for all we really know, but at that time the best records show Cambodia invading Ayutthayan territory, not the opposite.

Perhaps the best example of a possibly false king is U Thong, about whom there are so many origin stories that the careful historian can only conclude that at the time the extant chronicles were compiled no one any longer knew who had 'founded' Ayutthaya in 1351.[7] Wyatt, however, picking details now from one source, now from another, does not hesitate to assert that "U Thong was born in 1314," on evidence which is not revealed,[8] moreover both of a "powerful Chinese merchant family who may have been located in Phetburi," and of "a king of 'Kamphucha,' perhaps best understood as Luphburi." Well, he cannot have been both, and if a historian wants to indulge in such antiquarian exegesis, he must choose one or another. Moreover, 'Kamphucha' *cannot* be understood as Lopburi, although 'Kamboja' might; and to accept either of these stories means denial that Ayutthaya was founded by a Thai, something which I doubt Wyatt would have wished to suggest had he thought out his scenario more clearly.[9]

Personally, I like the 'son of a king of Kamphucha' version, with 'Kamphucha' meaning precisely what it seems, Cambodia, or at that time Angkor, and early Ayutthayan royalty developing from a provincial branch of Angkor royalty.[10]

Together with the false kings and battles, at least one real battle, the unsuccessful Ayutthayan invasion of Cambodia in the 1620s, expunged from official Thai historiography, has also been expunged by Wyatt, even though, having edited one of the works of van Vliet, he was well aware of the good evidence for such an attack having really occurred.[11]

But our purpose here is not just to demonstrate that even the simplest king - and - battle history requires preliminary careful source analysis and criticism but to discuss RK in the

writing of Thai history.

Prince Damrong was influenced by it to postulate the Phra Ruang dynasty, consisting of several early Sukhothai kings called by that title.[12]

This is no longer at all acceptable. Although 'Phra Ruang' research is still in its infancy, it is certain that Phra Ruang was a Pan - Thai cultural hero, that there was probably never a living Phra Ruang, certainly not a living Phra Ruang anywhere within the limits of present - day Thailand, and whenever we encounter a source making reference to Phra Ruang, we may be certain that the reference is to a more or less distant, already legendary, past about which the contemporary writers no longer possessed factual information.

To illustrate the influence of RK on modern historiography we may take Coedès *États*, in which, although relying more on inscription 2 for Central Plains history, Coedès did accept the territorial claims of RK on the peninsula; and it was clearly RK which induced him, as Pelliot earlier, to assume that the references to *Hsien* in Yuan period Chinese chronicles meant Thai in Sukhothai, an inference not at all necessary in the nature of the sources, and almost certainly to be rejected now. Coedès was further induced, on the implicit assumption that there could have been no more than one king in 'Thailand,' to assume that the Kan-mu-ting of Phetchaburi mentioned by the Chinese was Ram Khamhaeng on campaign in the delta.

Coedès moreover accepted a number of chronicular traditions about Phra Ruang as historical fact, and identified them with Ram Khamhaeng, on the apparent grounds that even if "Siamese tradition...confuses the first kings of Sukhothai" under the name of Phra Ruang, it "designates in particular Ram Khamhaeng...." This was not even superficially true. No *source* identifies Ram Khamhaeng, nor even Ramaraja who in other Sukhothai inscriptions and *Jinakalamali* fills the Ram Khamhaeng time slot, as Phra Ruang, and the tradition of equating Ram Khamhaeng with Phra Ruang was a modern pseudo - historical tradition in the creation of which Coedès was

involved.[13]

Coedès synthesis illustrates how RK has served to falsify even crude king - and - battle history, by invoking non - existent battles (conquest of the Chao Phraya delta and peninsula), and forcing the suppression of historical kings (delta and peninsular rulers such as the Kan-mu-ting of Phetchaburi, probably a local Khmer).

The falsification of history was not solely due to RK, but it was certainly encouraged by the list of conquests and dependencies attached to the end of RK. Interestingly this part of RK was already recognized by Coedès as an epilogue, added after the rest of the inscription had been composed, and thus inherently less credible; but it could have arisen on the basis of other Sukhothai inscriptions known at the time, such as No. 2, even if RK had not been discovered. The assumptions were:

(1) 'Siam' now means, and thus has always meant, 'Thai.'
(2) *Hsien / Sien* in the Chinese records meant Siam.
(3) At the end of the 13th century when *Hsien* first appears in Chinese records the only Thai state was Sukhothai, known as Thai from RK, but which could have been inferentially identified at that date as Thai from inscription No. 2, even if No. 1 had not yet been discovered.

These asumptions are no longer acceptable. Until 19th century kings began to adopt it, all foreign observers of Thailand reported 'Siam' as a foreign appelation which the local people, who called themselves 'Thai,' did not use.[14] The origin, and original meaning of Siam and the *syām* of Angkorean and early Cham inscriptions, is unknown. Since Ayutthayan Thais as late as the 17th century did not recognize it, it probably did not mean 'Thai' in earlier times. It is acceptable to identify the *Hsien* of Chinese reports with 'Siam,' particularly since the Chinese continued to use *Hsien - lo* for the Kingdom of Ayutthaya after the date that state was allegedly founded according to Thai tradition, and until the 20th century. But literal reading of the Chinese reports from the 1280s shows they referred to a site near the delta, not far inland, obviously a predecessor of Ayutthaya, and

careful reading of the scholarly syntheses which nevertheless located *Hsien* at Sukhothai reveals that there was always some embarrassment at the identification, which had to be explained away by some such device as *Hsien* really meaning the capital, although the Chinese could only describe the port centers which they visited.

Whatever his political or cultural nationalistic motives, which I am sure most of us would reject, Phibul Songgram was historically correct in rejecting 'Siam' as official name for his country in favor of a return to the historically genuine Möang Thai, translated into English as 'Thailand.'

Because of this I shall not use 'Siamese' to designate Modern Standard, Bangkok or Central Plains Thai language, and I am convinced that if we ever discover what Siamese language was, it will prove to be non -Thai.

This once standard interpretation of RK in Thai history has undergone vicissitudes which are interesting for factual history, for the study of historiographical evolution, and for the status of RK as an historical source.

In their joint EHS Griswold and Prasert, apparently after consideration of the material difficulties of campaigns from Sukhothai to conquer Suphanburi, Phetchaburi, and the peninsula, modified the scenario to voluntary submission of those areas to the overlordship of Ram Khamhaeng. A similar modification was supported by Charnvit Kasetsiri, who argued that it was Suphanburi which in the 13th century had gained control over the peninsula, not an unreasonable hypothesis, and that Suphanburi "[f]or a brief time...was forced to accept the domination of Sukhothai." During this brief period of hegemony Sukhothai, "by controlling some dependencies situated near the sea, such as Suphanburi, Phetburi, and Nakhon Sithammarat", was able to attack the Malay Peninsula; and the fleet against whose attack the Chinese protested at one point "was stationed at Phetburi, which was then under the control of Suphanburi, at the time a dependency of Sukhothai." This should mean that the *Hsien* to whom the Chinese protest was directed was located at

Phetburi or Suphanburi, but Charnvit felt obliged to bow to convention and place *Hsien* at that time in Sukhothai, although he recognizes that the *Hsien* which initiated a similar attack in 1349 must have been a delta power.[15]

Wyatt, in *Thailand,* also repeats the modified view. "The geographical extension of Sukhothai's power...should not be understood in modern political terms. Ramkhamhaeng certainly did not raise a massive army and march over these several thousands of miles...[and] [a]lthough the Nakhon Si Thammarat chronicles claim that Ramkhamhaeng came there and ruled over the state from 1274 to 1276 it is best to regard this legend as a late insertion." Ram Khamhaeng's alleged overlordship of those distant areas would have been built upon networks of personal allegiances or marriage relationships.[16] This is not altogether bad as *speculative hypothesis,* although it is not based on any piece of evidence, but what is of most relevance for our purpose here is that it utterly contradicts RK, which says those distant areas were *conquered* ***(prap)*** enemies ***(kha soek),*** a claim which Griswold and Prasert found so ridiculous that they refused to translate it, offering a more anodyne paraphrase instead.[17]

Incidentally, the Nakhon Si Thammarat chronicles *do not* say that Ram Khamhaeng came to rule there. They say a certain *baña* Sri Saiyanaranga came to rule in 1274, and it is Wyatt who has claimed that that name "is an equivalent of the Thai name given to kings of Sukhodaya, *brah ruaṅ,* a name especially given to King Rama Gamheṅ," none of which is true, if the discussion is referred to primary sources, but which is an interesting point to take up in connection with RK and history. 'Saiyanaranga,' as I explained to Wyatt in correspondence some years ago, is a name with interesting textual relationships, but it is not equivalent, either textually or etymologically, to *brah ruaṅ*, except in one version of the Phra Sihing story, which has long been recognized as perhaps the least accurate of old chronicles for historical reconstruction. The name Phra Ruang, moreover, is not in primary sources particularly associated with Sukhothai, although its Pali translation is in *Jinakalamali. Brah ruaṅ* is a pan - Thai hero, and his identification with Ram Khamhaeng is the work of modern historians.[18]

These rationalizations of RK by scholars who formally still support its authenticity in themselves constitute a conviction that parts of it have been faked; and the effect of RK on Thai history has been to distort and even falsify the study of the lower Chao Phraya valley and the peninsula during the 13th century, an important period in which rapid political and economic changes were occurring.

The RK inscription has also served to distort interpretations in art history, the area in which Piriya Krairiksh first began to identify some of its anomalies. I shall call only one example to your attention, the dating and purpose of Wat Chang Lom at Sri Satchanalai, long believed, on the basis of RK, to have been built earlier. Now archaeological work by the Thai Fine Arts Department has proven that Wat Chang Lom could not have been built before 1370, even in its earliest phase, and it must thus be delinked from RK.[19] I must emphasize, however, that this new evidence, though showing a nefarious influence of RK on history, has no direct bearing on the question of RK authenticity, only on the authenticity of scholarly inferences made from it.

Although RK is not always mentioned, I feel that it is the RK problem which prompted some recent semi-historical ruminations by Hiram Woodward Jr. and William Gedney. That is, had there been no RK their speculations, which relate to the arrival of Thai in the Peninsula, would not have been felt necessary.

Woodward has suggested that the Thai of the peninsula were the first "wave" of Thai immigrants to reach areas now inhabited by Thai in present - day Thailand. No date of arrival is offered, but he asserts that as a "number of Dvaravati (Mon) towns may have been abandoned in the tenth century" under the impact of Khmer expansion, "it is around this time that we must suspect movements of Thai speakers," a conclusion the logic of which escapes me. Similarly vague is the further speculation that "if Mons did move from northeastern Thailand to Thaton" in the 11th century, "it seems reasonable to suppose that Thai speakers were somehow also involved." This view is made more explicit

with "The ancestor of the southern dialects was brought in the first wave — a wave which might historically be connected with the movement of Mon speakers (perhaps in the eleventh century) from northeastern Thailand to Thaton." Now apparently Woodward's need to hypothesize migrations of Thai speakers on the basis of apparent movements by Mon and Khmer, is to account for Thai in the peninsula early enough to have influenced the tone - marking system of RK, which Woodward recognizes as anomalous. In this, however Woodward is forced to deny the purist view that Ram Khamhaeng invented the Thai script, and *to suggest that he might only have meant "a feature such as his* peculiar method of vowel placement"; but were it not for RK the loose stacking up of shaky hypotheses on even weaker foundations would not have been necessary.[20]

Gedney, in "A Possible Early Thai Route to the Sea," offers speculation as to how the "Thai - speaking peoples of Northern Thailand communicated with and traveled to the Gulf and the Peninsula," "in the centuries before the founding of Ayutthaya," "with Mon - Khmer speakers in control of the Chao Phraya valley."[21]

Now for a mere historian this would seem to be a non - problem, unless there was overwhelming contemporary epigraphic evidence of Thai in the Gulf and Peninsular area at that time, which there is not — in fact there is none, and at the same time overwhelming evidence that the Mon - Khmer linguistic domination of the Chao Phraya, for which there is good evidence, would have made it impossible for, say, Thai traders to boat down the central Thai rivers, then coast down the peninsula and settle there — in short nationalistic border and passport controls such as prevail today. In general historians imagine that things were much more fluid then.

Even though there is no real evidence for such early Thai population of the peninsula, Gedney says it is certain that it occurred; and I feel that it is only because of the problems thrown up by RK that such postulation has seemed necessary.

Gedney's argument about early Thai settlement of the

peninsula is based on a view that "the Thai dialects of the southern peninsula are sufficiently different from other varieties of Thai speech to indicate that they have been in place for a long time;" and "it is hard to imagine that these groups of Thai speakers migrated through the Mon-Khmer speaking Chao Phraya valley...leaving no traces of their having passed."

Gedney then enters into unnecessary speculation about an alternative route from northern Thailand to the sea, which is not at all necessary, for there was one well-attested route through Sukhothai, Tak/Kamphaeng Phet, to Martaban and the Bay of Bengal (mentioned in Sukhothai inscriptions 4, 5, 8, and 11); and another route, attested in Sukhothai inscription No. 11 from Sukhothai via Ayodhya (sic) and then to the Bay of Bengal apparently near Tenasserim. This was used by monks travelling to Ceylon, one of the concerns which Gedney brought forward. Moreover the Tenasserim route remained the main Ayutthayan route to Western countries into the early 17th century.

Thus, *if* Thais settled the peninsula at a time when the Chao Phraya valley was still non - Thai, there need be no mystery about their routes.

But did they? There is no early epigraphic record of Thai in the peninsula; but there are epigraphic records, all in Indic or Khmer, and in particular as late as the 17th century official documents from Ayutthaya to wats in Nakhon Sri Thammarat were still being composed in a local variety of Khmer.[22]

I have occasionally seen reference to Teeuw's and Wyatt's *Hikayat Patani* as recording early Thai presence in the peninsula, but in fact those writers were extremely circumspect, stating only that "By [the 14th - 15th centuries]...the region of Patani already had felt the power of the newly - established Thai monarchies of Sukhothai and Ayudhya"; "immediately prior to the foundation of Ayudhya Thai vassals had raided Singapore;" and there are "some grounds for believing that the Kingdom of Sukhothai was active in the isthmian region as early as the last quarter of the thirteenth century, when a relationship was entered into with a state centered on Nakhon Si Thammarat;" none of which even

implies Thai *settlement* of the peninsula.[23]

Marvin Brown, followed by Gedney, and accepted by most other non - southern - Thai - specialist linguists, including James Chamberlain, has argued that the southern dialects were direct descendants of Old Sukhothai language, based on a view that modern southern tone systems are closest to the tones of RK, if the latter represented a perfectly regular system. Brown then accounted for Thai movement from Sukhothai to the south by non - linguistic historical speculations which all historians would reject. Moreover, his star southern dialect, Tak Bai, is now taken to be a late intrusion from Laos, without relevance for the question of southern evolution.[24]

Thus for Brown Old Sukhothai language was represented by RK, in which tone marks showed perfect regularity, and the descendants of that language are only found in the southern peninsula.

More recently, as students of RK have begun to examine the other Sukhothai inscriptions, it has become apparent that the tone marking system of RK, as Coedès surmised, may already have been as anomalous then as the same tone - marking system is for modern Thai. This has then been explained by a southern influence on RK. The Mahathera whom King Ram Khamhaeng invited from Nakhon Sri Thammarat, helped him devise his script, and imposed tone - mark usage fitting the southern dialect. This is in fact a negation of what Brown said, although no one seems to have noticed it.

As a matter of fact, a purist reading of RK does not require presence of Thai in the peninsula at all. What RK says is that Ram Khamhaeng *conquered* the peninsula, not that it was peopled by other Thai. Of course, after conquest he might have tried to settle it with his own people, but could this have already been effective between, say 1270 and 1292? And if so, they would certainly have spoken with the same tones as those of Sukhothai, and a tone - marking system introduced by a monk from the south would not have been so inappropriate for the Sukhothai language.

Moreover, the invitation of a monk from Nakhon Si Thammarat by RK in no way implies that the monk was Thai; important monks traveled internationally, and Thai or Burmese rulers are reported to have invited monks from Ceylon.

Thus even the most purist reading of RK can provide no evidence on Thai population in the peninsula; and the two different arguments about linguistic contacts between Sukhothai and the peninsula are mutually exclusive.

Now, moreover, the linguist specialist in southern dialects has produced work which, though he does not say so, denies all the foregoing speculations about early Thai settlement of the South, and even the special relationship between Old Sukhothai language, its tone system, and southern dialects.

In his "Tones and vowels in southern Thai: a diachronic anomaly," Anthony Diller, with respect to settlement of the south by Thais, says that "[h]istorically, a substantial Tai-speaking population has been in the peninsular area for over five hundred years," that is from early to mid - 15th century, nearly 200 years after the south might have been conquered by Ram Khamhaeng.[25] For Diller the Tak Bai dialect, as a late intrusion, is out of the picture; and as for the three - way southern tone split, which for Brown showed the special Sukhothai - southern relationship, Diller feels that "[i]t would be reasonable to suppose...that in some emergent stage of southern Thai the split [H + M] / [L] occurred as it also did to the north, and then a subsequent split to [H] / [M] / [L] originated in the lower regions of the Southern Thai dialect area from which it spread (to some extent) northward." This would mean that in Sukhothai times the tone marks of RK would have been no more apt for southern Thai, if it yet existed, than for the language of the 14th century inscriptions, Ayutthaya, or Bangkok; that in fact no special relationship between Sukhothai and Southern Thai even existed; and that probably, as a non - linguistic historian might have supposed, Southern Thai is simply an offshoot of Ayutthayan Thai which reached the peninsula during the gradual domination of the Chao Phraya valley by Thai, and which has undergone further local evolution, perhaps under the influence of non - Thai (Mon - Khmer, Malay)

substrata.[26] Although the question of such non -Thai influences on southern Thai development seems not to have received thorough study, Diller has acknowledged the possibility, and Brown has provided an example, athough not realizing the implications, in his "The Great Tone Split: Did It Work in Two Opposite Ways?", Brown notes among the special features of Southern Thai that it "...keeps initial consonant clusters far better than any of the other six branches [of Thai], and it is the only branch that has begun to lose a final (final *k* is changing to ʔ in some Southern dialects."[26a] Both these features, preservation of initial clusters and unreleased final *k* which may be realized as a glottal stop, are Khmer characteristics.

Certainly Diller's view implies that a monk invited to Sukhothai from Nakhon Si Thammarat in the 13th century would not have been a Thai.

The Lacunae and Reconstructions of B.J. Terwiel

An unexpected contribution to the RK controversy, and one which shows how it has become a trendy cause, eliciting reactions from the most diverse sources, is "The Ramkhamhaeng Inscription: Lacunae and Recontructions,' by B.J. Terwiel, who kindly gave me a copy of his paper in advance.

This is a curious piece of work, the main point of which seems to be a criticism of Bradley, particularly in comparison with a supposedly 1855 reading of RK, preserved in a small excerpt given by King Mongkut to John Bowring, and in its entirety in a document given to the Montigny mission in 1856 and now preserved in the National Library in Paris. I shall refer to this copy henceforth as the Montigny copy. The first impression is that Terwiel is beating a dead horse, for Bradley's reading, since Coedès' work of 1924 at least, has been little more than a curiosity, and since that date has not been the basis for further study of RK.[26b]

Terwiel wishes to show that Bradley may have read RK less competently than the Thai who prepared the Montigny copy some

50 years earlier, and that this discredits what Terwiel for unfathomable reasons calls the 'sequentialist' approach, which holds that scholarly work improves with each new effort.

Thus Terwiel points out a number of readings which Bradley timidly conjectured, whereas the Montigny copy shows them boldly set forth. But since we do not know how the copyists of Montigny understood what they were copying, the cases in which they correctly, in terms of modern interpretations, read what Bradley only conjectured may demonstrate that they also conjectured well, not that they had a better understanding of the text, nor that the stone was then in better shape. We must also keep in mind that Bradley was also in touch with the best Thai scholarly opinion of his time, that his readings must have depended on consultation with Thai scholars, and that if we assume the Thai copyists in 1855 to have understood RK better than Bradley, it implies a decline in Thai scholarship between 1855 and 1909.

There is, moreover, some evidence about the way in which Thai scholars read and understood RK, at least in the 1860s, if not precisely at the time the Montigny copy was done. This is *in the 'translation' by Bastian, which* Terwiel unaccountably says 'Bradley quite rightly dismissed' *because* it was "only 'a first sketch, in which the writer [Bastian] reports such impressions of the drift and import of the writing as he was able to get from Siamese sources." I say 'unaccountably,' because Terwiel comdemns Bradley for having made "some scathing comments on [other] earlier attempts to understand the inscription," such as the Montigny copy, of which Bradley had certainly seen only the small excerpt published by Bowring. Thus Bradley's neglect of Montigny, the text which Terwiel emphasizes, was because he did not know it, and as for the other early reading of RK, Schmitt, Terwiel would seem to agree with Bradley that it did not deserve great attention.

A modern student of RK who wishes to criticize the "sequential model," however, should not ignore Bastian, for it is only there, at least among readily accessible sources, that we may get some idea of mid - 19th - century Thai interpretations of RK. For example, Face 3, lines 1 - 2, translated by Griswold and Prasert

as "North of this city of Sukhodai there is the bazar *[mī talāt pasān]."* As Terwiel indicated, Bradley had trouble with *pasān,* not recognizing it as 'market,' while the Montigny copy shows unequivocally *'pasān;'* for Bradley, even though he found the stone damaged at that point, was willing to *read 'pasān,'* but he did not know any gloss which would fit the context.

Now Bastian's version is, "At the south [sic; 'north'] of the town of Sukhotay there is a market and a school-room...," indicating that in 1864 the Thai scholarly consensus was that the syllable *sān of pasān* was *sāl(a),* 'school.' Indeed, it was only Coedès with his knowledge of Khmer (the Persian connection is irrelevant and perhaps even wrong) who was able to correctly identify the term, credit for which Terwiel witholds from him.

Terwiel did give Bastian credit for one correct, in terms of Montigny, reading, "The waters are full of fish, in the fields grows rice" (face 1, line 19), a translation of the Montigny text, *nai nām thang plā, nai nā mī khaw,* whereas modern interpreters agree with the later Schmitt plate version, and direct reading of the damaged modern stone, *nai nām [mī] plā, nai nā mī khaw.* Now Terwiel's explanation is peculiar and tortuous. He says Bradley *reconstructed* the verb *mī,* which agrees with Schmitt's plate, and which "makes for a grammatically more satisfactory couplet of phrases", and "The 1855 transcribers seem to have come upon the word *thang* by pure guesswork and this may be taken as evidence that in 1855 this part of the stone already was illegible."

At this point one would like to ask, if the stone was illegible in 1855, how did the later copyists of Schmitt's plate "come upon *[mī]* by pure guesswork;" and why has *mī* 'stuck' as the correct reading in all subsequent work, since now the stone is indeed *illegible at that place?*

As to legibility, moreover, the Montigny copy gives no hint of illegibility; indeed it is a continuous text without even line breaks corresponding to the stone, whereas Schmitt copies the stone line for line, with blanks for major illegible damaged portions.

Terwiel is admittedly concerned (p. 19) to counter an objection which has long been part of RK tradition, that the Montigny

copy was never taken directly from the stone, which for iconoclasts might indicate that it represents a draft of the text before it was incised.

In at least one passage Bastian's Thai informants even read the text better than Bradley. For Face 1, lines 22-24, which has given so much trouble to all modern scholars, is rendered in Bastian as, "If death occurs, the property of the father goes to his sons, of whatever it may consist. His children, his wives, his servants, his slaves, the fruit - gardens of betel and areca, all and every thing, what the father possessed, is inherited by his son." The modern consensus on this passage, as in Griswold and Prasert, is "when any commoner or man of rank dies, his estate — his elephants, wives, children, granaries, rice, retainers and groves of areca and betel — is left in its entirety to his son." Whereas Bradley, and *his* local specialists, understood, "Among common folk of the realm, among lords or nobles, if any one soever dies or disappears from house and home, the Prince trusts, supports, aids. They are always getting children and wives, are always growing rice, [these] folk of the realm, subjects of the Thai. Their groves of areca, their groves of betel, the Prince trusts wholly to them to keep for their own children."

Although Bastian, as a general paraphrase, is closer to the best modern interpretations than Bradley, he, like Bradley, missed one important detail, the significance of *jāṅ* + *xò,* 'elephants [and] goads.' This has been dropped entirely from Bastian, while Bradley interpreted it, in terms of modern orthography, as *jāṅ khò* "they are always getting [asking for]," on which I shall comment further below. Terwiel did not touch on this passage, which would have damaged his 'anti - sequentialist' thesis, for it was Coedès who showed the probably true significance of *jāṅ* + *xò,* as well as corrected Bradley's spelling. Strangely Bradley's transcription consistently shows *jāṅ* (mai - ek) for elephant, and *jāṅ* (mai - tho) for the 'adverb - auxiliary' (Haas).

If Terwiel wishes to praise old Thai scholarship, he should not neglect Bastian, who showed, in contrast to the 1855 plates, something of what Thai scholars of the time thought. Indeed in a few details Bastian, and his Thai informants, lend support to

Terwiel's argument, with their readings which are superior to Bradley. In general, however, they have followed the method consecrated in most modern work on RK, of providing judicious paraphrases of difficult passages, a method which may disguise rather than illuminate the original text. Bradley at least tried to account for every word, sometimes unsuccessfully, and sometimes perhaps because by their very nature certain passages are not to be accounted for. I think it is quite possible that the extreme difficulty which scholars have encountered in the interpretation of some passages is because they are quite literally nonsense, but this has been hidden under the more or less felicitous paraphrases which have been presented in lieu of translation.

There may be "an ideological 'hidden' component" (Terwiel, p. 18) behind this effort to criticize Bradley, and 'sequentialists' by means of Montigny, while ignoring Coedès, whose work on RK represented a great step forward 'sequentially.' for at the end of his paper, and quite outside the context of his discussion, Terwiel suddenly brings up the question of "'weaknesses'...perceived by a relatively small, but quite vociferous group of scholars, such as Piriya Krairiksh and Michael Vickery;" and he implies that the reason for our efforts is the "real 'coup' for an iconoclast if such a revered nationalistic symbol as the first Thai inscription could be shown to be a late copy or a deliberate fake."

This insinuation, as well as the value - loaded term 'vociferous,' constitutes intellectual back - stabbing, quite out of place among students of history whose first duty is to thoroughly study their sources, and for whom no considerations of 'reverence' or 'nationalism' should put any subject out of bounds. Iconoclasm, if that is what it is, is essential in historical research, while wishy-washy iconolatry impedes intellectual advances.

If Terwiel wishes to engage in the debate, he must take up the weaknesses we have pointed out, not hide behind the nearly irrelevant differences between Bradley and Montigny. Neither may he rely on simple assertions of authority, against the arguments in my Canberra paper. If Terwiel's assertion about successful answers to those arguments were true, there would be no need

for the present AAS panel nor for any of the further seminars being planned on the subject of RK. Most of the answers to Vickery (Canberra) except Diller, to be discussed further below, have been ad hoc rationalizations, some of which contradict other such rationalizations, and which show the defenders of RK to be holding mutually inconsistent positions.

If Terwiel wishes to join in that defence, he must get directly involved with those issues, not pretend that they have already been resolved.

On one point, though, I am in full agreement with Terwiel, that we should "draw notice to the fact that the nineteenth - century transcripts are valuable documents and that they deserve to be compared with more recent transcripts" (page 6), as I shall indicate further below.

The *kh khuat* / *khai* Problem

This is the detail in my Canberra presentation which has been given the most attention. In fact it seems to be the only point which has been given very much attention, no doubt because it has permitted some people to draw the conclusion that Diller has used my treatment to prove that RK is genuine.

If the whole matter could be resolved with just one detail, those of us who wish to reject RK could just as easily cling to the archaeological *proof* of post - Sukhothai construction of the *tripura* walls to show that RK is a fake, and that none of the textual fine points matter.

Interestingly, the archaeological evidence from the walls has called forth three, contradictory, epicyclical reactions. Prince Subhadradis Diskul suggested that something might be wrong with the archaeology, since it was contradicted by RK, while Dr. Prasert, himself a scientist by training, realizing that archaeological evidence must be accepted, has now proclaimed that *tripura* does not mean what everyone since Coedès' article of 1923 has accepted that it means. That is, the triple wall of old Sukhothai was really not built in the 13th century, neither does RK claim

it was, for *tripura,* which it says was/were built, means something else, to be determined. Gedney, however, now agrees that the term in question means 'three,' or 'triple' walls, but could nevertheless have been used for a single wall. While this epicycle has more inherent plausibility than the other two, it would have been more satisfactory had it been offered years ago as part of an honest attempt to understand all the fine points of RK, rather than now when a more delicate problem has been raised.[27]

These very epicycles, casting doubt on what everyone thought had been settled, show that close attention to the textual fine points does matter, and it will take resolution of more than just one of them to settle the status of RK. Whatever the statistical probability of a later hoax-player getting the *kh khuat* etymologically correct, if too many other details argue against RK authenticity, the *kh khuat* will just have to be accepted as an improbable fluke.

Diller has developed the argument further in his "Consonant Mergers and Inscription One," where the concluding remarks imply denial that King Mongkut could have been the author, which I repeat is not my argument, though here again interesting epicycles seem to be evolving. The semi-hagiographic biographies of King Mongkut which used to be churned out by more- royalist-than-the-king *farang* scholars idolized Mongkut as very nearly the complete scholar of his time, knowing all languages and all customs; but now that such renaissance intellectuality might point to him as faker of RK, it is becoming respectable to say he just wasn't bright enough to do it. Not only may this constitute lèse-majesté, but it will not, I think, hold up. There is just too much evidence that Mongkut studied all relevant languages and loved to play with words and scripts. He was certainly inclined, *pace* Diller, to insist on orthographical uniformity, and in the words of Coedès was an 'étymologiste impénitent".

We must first try to maintain clarity about what the *kh khuat / khai* argument means and does not mean. When I raised the issue in Canberra, it was not primarily as one of the pieces of evidence against the authenticity of RK, but as an issue in Thai

historical linguistics, although it bears a relationship to the question of authenticity. With respect to historical linguistics and *kh khuat / khai* I treated RK together with the 14th - century inscriptions of Lithai as 'Sukhothai,' as Burnay and Coedès had done, and I was arguing that taken together they showed, *contra* Marvin Brown, that 'Sukhothai,' for which Brown had relied solely on RK, had not preserved ancient distinctions which had already disappeared from other Thai languages. It was not only the *khuat / khai* distinction which was at issue, but also that between *kh khon* and *kh khwāy,* which I treated together with the former. This was an argument first of all about the status of 'Sukhothai' represented by a whole group of inscriptions, not just about RK. The first substantive conclusion I drew was that, following Burnay and Coedès, Sukhothai language could not be studied from RK alone, but must be based on the 14th - century inscriptions as well, and the second conclusion was that the Sukhothai script, whether RK or Lithai, could not possibly represent the invention of Thai writing, not even of *this* Thai writing, which of course implies that RK is in this respect a fake.

What Diller then did was to use my presentation to show that RK alone had more faithfully preserved old Proto-Thai distinctions than did the rest of the Sukhothai corpus, which was evidence in favor of RK being an older variety of Sukhothai Thai, and more probably a genuine 13th century inscription. Note that at that time Diller based his argument on Proto-Thai as depicted by Fang Kuei Li with two velar consonants, *kh, *x, which have merged as / kh/ in modern standard Thai, but which seem to have been occasionally represented at Sukhothai by *kh khai, khuat* respectively.[27a]

Diller contended that my own table showed almost complete regularity in this distinction in RK, against 14th century Sukhothai, where he agreed with me that the two symbols seemed to be meaningless allographs. Part of the problem was a misreading I had made, following *Caru'k samay sukhodai,* while he had perhaps followed a better source, *Prajum sila caru'k bhag di 1* (cited further as *Phak 1*). There was, however, still another misreading which was then in my favor, and still one more which

I have only recently discovered, and as can be seen from my corrected table 1 reproduced here (pp.360-**362**), out of 15 cases, ignoring 'tamarind,' and allowing *khun,* whose Proto-Thai status is uncertain, RK agrees with Li's Proto-Thai in only 7, possibly 8 if the PT form of kha in *khasök* can be proven a *kh khai* form. In this respect RK is no more a record of real distinctions than 14th century inscriptions, in which Diller has recognized the lack of phonemic distinctiveness between the two characters.

In his recent published treatment Diller has refined the arguments, and has shifted ground. The Proto-Thai against which RK is placed for comparison is no longer Li's formulation, but a system with 5 velar terms of which 4 (represented in Li by only 2, /x/ and /kh/) merged into /x/ and /kh/ in Proto - Southwestern Thai, and are still distinct in White Thai; and RK is in accord with the White Thai distinctions. The way Diller presents it, it makes RK seem, again, like an exotic throw-back to some kind of 'Ancient' Thai, but more prosaically it means that RK agrees with White Thai in all cases but Proto-Thai *khl/ *khr > White Thai /c/, RK and modern Thai /kh/.

In addition to establishing the nature of RK *kh khai / khuat* distinctions and their relationships to White and Proto Thai, Diller has offered a few more pronouncements worthy of notice.

No doubt in reaction to my treatment, in which the evolution of *kh khwāy / khon* was related to *khai / khuat,* Diller wishes to exclude the former from the discussion, saying that they "cannot provide evidence of the sort directly relevant to establishing, in a relative way, the chronology involved in loss of a distinctive (phonemic) opposition," such as between *kh khai / khuat* in Sukhothai and later Ayutthayan, etc. languages, because *kh khwāy / khon* still represent living distinctions "in modern dialects just to the north of Sukhothai (e.g. in Lanna and Shan...)." But then, *pace* Diller, the two situations are precisely parallel. Old Sukhohai script shows regular distinctions between *kh khai / khuat* and between *kh khwāy / khon*, each of which disappeared by the end of the Sukhothai corpus, and in later stages of Thai languages in central, northeastern, and southern Thailand, but each of which has been maintained to the present day in a neighboring

language — *kh khai/khuat* in White Thai, and *kh khwāy/khon* in Lanna. Diller is correct in saying that one cannot serve to date the other, but each is relevant in situating a given language, such as Old Sukhothai, within the family of Southwestern Thai, with respect to which written records of the other distinction must be assessed. I shall illustrate this below.

Next Diller wrote that RK "shows complete *internal consistency* in its use of *kho khai* and *kho khuat* for the items in these initials occurring more than once...[i]n all such cases there is complete consistency as to initial consonant: there is no spelling variation at all in making this particular orthographic distinction." I shall demonstrate below that this is not true, though not in the way I imagined when I prepared my Canberra paper.

On the other hand Diller agrees with my Canberra position in that "for *other* inscriptions of the Sukhothai corpus, random allography is a very reasonable description for *kho khuat's* distribution." With the exception of inscription 2, and even there pending reexamination of the inscription itself, I now find from examination of plates of the inscriptions, rather than just published transcriptions, great regularity in the use of *kh khai/khuat* in the Lithai periood, which forces a restatement of the argument about RK and Old Sukhothai.

The *kh khuat/khai* issue, I have realized since Canberra, and due to Diller's criticism, is far more complex than I thought, and is even more complex than as presented by Diller. Until then I had been relying on the transcriptions of RK in standard publications, but Diller's work has made me realize that careful examination of reproductions of the stone itself is required. What is involved is not even a straighforward study of the two vowel phonemes/symbols *kh khuat* and *kh khai,* but study of sets of symbols in RK and in the other Sukhothai inscriptions which have been variously interpreted by modern commentators as *kh khuat* or *kh khai,* with each such interpretation apparently influenced by the individual scholar's preconceptions about what *kh khuat/khai* should be.

Table 1

Note jat = Jataka inscriptions of Wat Sri Jum. Transliteration is 'graphic,' with ***g*** = *kho khon, kh* = *kho khuat, v* for low class /f/ (ฟ) and z for low class /s/ (ซ) of standard Thai, and *g for *gamma* designating a reconstructed PT voiced velar fricative. Variant readings of RK are shown, separated by slashes. See comment pp. 357-358.

English	Siam	PT	White Tai	I	II 1361	III 1361	V 1361	VIII 1359+	Misc.
(title)	khun		kh/x?	*kh*un	khun	*kh*un	khun		khun (107,15)
axe	khvān	*xw							[*jat* 396 - khv̄an]
break	khāt	•kh	xāt		*kh*āt khāt			khāt	khāt (107)
broad	khwaṅ	*khw	xwāṅ			khwāṅ			
to cross	khām[2]	*x	xām		khām		*kh*ām+		khām+
drive	kháb	*khl	chap	khabb					
dig	khut	*x	khut	khut	khut				khut (no. 11)
dub				*kh*ïn+ jü'		*kh*in ji	*kh*ün jï		
egg	khai'	*khr	chai	(Li, p. 229)					

English	Siam	PT	White Tai	I	II 1361	III 1361	V 1361	VIII 1359+	Misc.
fur	khon	*kh	xun						
goad, hook	khò	*x	xo	*kh*ò					
go-in	khau2	*x	xau	*kh*au+	khau	khau	*kh*au		khau+ (106)
go-up	khün2	*x	xun	*kh*ün+	khin	khün *kh*ün	*kh*ün	*kh*in	khin (15) khin (106)
green	khiav	*x	xEv	khiav					
hang	khveeṅ	*xw	xwen	*kh*veen					
I	[khoy]	*x	*x*oy						
kill	khā[2]	*kh	x̄a	*kh*ā+/ khā			*khā* (+)		
moun-tain	khau		xau	*kh*au/ khau	khau	*kh*au khau?		*kh*au	khau (15)
place	khòk							*kh*òk	
request	khò	*khr	cho			kho			
respond	khān	*x	xān			khān			
rice	khav2	*x	khao	khau+		khau	khau *kh*au	kh_3	khau (107) khau+ (106) khau+ (102)

English	Siam	PT	White Tai	I	II 1361	III 1361	V 1361	VIII 1359+	Misc.
ride	khī'	*kh	khī	khī'		khī			
right	khvā	*khw	*x*wāa	*kh*vā/ khwā					
sell	khāy	*kh	xāy	*kh*āy		*kh*āy?		*kh*āy (15) *jat* 398 - khāy	
sing	kháb	*kh	xap	khabb	(a type of music, in Li, 'sing')				
slave	khā[2]	*khl/	sā	khā+	khā+	khā	khā		kh̄a + (102, 106, 107) *jat* 397 - khā
enemy	khā[2]			khā+ söak			khā sök; khā+sök		*jat* 401-kh̄asik
tama-rind	hmak khām	·	kham	*kh*ām					
thing	khòṅ		xoṅ			*kh*òṅ	khoṅ		
they	khau	*khl	chau		khau	khau			
white	khao	*x	xao		khāv				*kh*āv (14) *kh*āv (15)
year				khau+	khau	khau+	khau	khau	

In RK three different symbols have been recognized by most modern scholars. One of them seems clearly to be *kh khai,* and may reasonably be accepted as a representation of the ancestor of modern *kh khai* symbols. The second symbol differs from the first in a slight indentation in the vertical trait on the right-hand side of the letter. This has also been interpreted as a *kh khai,* in which case RK is held to contain 2 *kh khai* symbols which are to be regarded as meaningless allographs; but the second has also been interpreted by some readers as *kh khuat.* The third symbol has a slight indentation in the semi-horizontal curve which forms the top of the letter, like modern *kh khuat,* and, when given notice, it has been interpreted in this way. In fact this symbol often has a flat, non-indented horizontal trait and a slight indentation in the vertical trait as well. That is, beginning with 19th century Thai scholars the RK *kh khuat* has sometimes been identified as such because it incorporated the distinguishing feature of modern (at least since the 17th century) *kh khuat.* To save space I shall designate these three symbols as respectively kh_1, kh_2, and kh_3. These at least are the generally recognized -emic forms. As I shall describe below, there is also an indeterminate allograph incorporating features of both kh_2 and kh_3, and there is still another occasional allograph in which the upper segment of the character forms a complete horizontal ovoid loop. In one case, 'goad' in face 1, line 23, this has been read as *kh khuat.* It is well to emphasize that in RK the -emic features of the *kh* characters are anything but distinct, and their indistinctness cannot be explained as difficulty in engraving on stone, for the same feature, an indentation in a line, is perfectly clear and uniform in the character representing *j,* as in *jāṅ*[2] 'elephant.' Interesting too is that the representations of kh_3, seem concentrated in the first face, with kh_2 then utilized increasingly for words shown in the beginning of the inscription as kh_3 and which historically should be *kh khuat.* In faces 2 and 3 the -emic distinction between *kh khai/khuat* seems to be absence or presence of an indentation in the right side vertical trait.

The literal interpretations of these three symbols, however, have not been perfectly consistent. The transcriber of the Montigny plates ignored the distinctions, and almost uniformly wrote

kh_2. He thus did not recognize the indentations as an -emic feature. Curiously, he wrote his kh characters with a very rounded top, distinct from the rather flat curve of $kh_{1/2}$ on the RK stone, but resembling very much the kh_2 *[khuat]* of inscription no. 5 [see below].

The Schmitt plates show awareness of the problem, and it is clear from the writing of the title *khun,* the most frequent and most certain *kh khuat* word in the text, that the writer intended kh_3 as the representation of *kh khuat,* although the transcription does not show perfect regularity. For example *khau* 'rice' in line I/19 is definitely kh_3, the title 'khun' appears variously as kh_1, kh_2, and kh_3, and of particular interest, as I shall indicate further below, /khap/ 'drive' is written in I/5 as kh_3 (incorrect), in I/7 as kh_1 (correct), while /khap/ 'sing' in II/19 is kh_1 (incorrect).

Bradley in 1909 transcribed *kh khuat/khai* with near perfect regularity, and in a manner mostly consistent with present-day opinion, but made no comment on this problem, apparently considering the distinction as insignificant. Since Bradley's transcription of these two characters is so consistent and so accurate, more so than in some later publications, it would be interesting to know what he considered, at the very beginning of modern RK studies, to be the -emic feature distinguishing the two velar aspirates. He must certainly have been influenced to some degree by current Thai scholarly opinion, which, judging by the Schmitt plates, held the indented upper horizontal to be the distinguishing feature. Indeed Thai scholarly influence on Bradley, though not with respect to kh distinctions, is seen in his rendering of the word /khò/ in face 1, line 23, as 'request' with *kh khuat,* which was then Thai usage, though etymologically incorrect; and persistence in this reading of RK would have damaged its now apparent linguistic regularity, which has been redeemed by Coedès' proposal, now universally adopted, to read 'goad' *(kh khuat).*

Bradley was also familiar with the Schmitt reproductions, where kh_3 is used for *khuat*, but he appears not to have seen the full Montigny set; and thus scholarly influence of the time would have been that *kh khuat* should be represented as a type of kh_3. Now if we start from the beginning of the inscription, reading

Bradley's plates, the first relevant word is /khün/ 'go up' in line 3, with *kh* showing a flattened upper horizontal and no indentation in Bradley's plate, but a faint hint of indentation in the vertical in *Phak 1,* thus seemingly an allograph of kh_3. Next is /khau^2/, 'rice', 'year' in line 4, with a rounded upper horizontal, a clear *kh khai* feature, etymologically correct, but with a faint indentation in the right-hand vertical. In the same line the title /khun/ shows a flatter horizontal, and a slight indentation in the vertical, while in the next line the same title, now unclear in its first occurrence, shows in its second occurrence a clear indentation in the upper horizontal in Bradley but not in *Phak 1,* and a slight indentation in the vertical. Also in line 5 /khap/ 'drive' has a rounded upper horizontal and no indentation, clearly a *kh khai,* while /khwā/ 'right' has a flat horizontal and vertical indentation, and the third occurrence of /khun/ a flat horizontal and no indentation in Bradley, and the barest hint of an indentation in *Phak 1.*

Strangely, the plates of *Phak 1* render the indentations on the verticals more clearly than Bradley, but those on the top horizontal less clearly.

Up to this point Bradley would have considered, along with current Thai opinion, that the distinguishing feature between the two *kh* symbols lay in the upper horizontal trait, flat or indented for *kh khuat,* and that an indentation in the vertical was not distinctive.

The same distribution of features continues, but with gradual modifications, in the next few lines: (line 7) /khi/ *(khai)* 'ride,' unclear but no apparent indentation, /khap/ *(khai)* 'drive,' a distinctly flatter horizontal than in line 5, and no indentation, but in this single word it could be ignored as careless engraving; /khau^2/ *(khuat)* 'go in,' flat horizontal with slight indentation, and the barest hint of a vertical indentation, more prominent in *Phak 1*; (8) /khun/, flat horizontal with slight vertical indentation, and in a second occurrence a rather flat horizontal and vertical indentation; (9) /khun/ unclear, /khïn/ *(khuat)* 'raise,' flattish horizontal, slight vertical indentation; (10) /khun/ *(khuat)* roundish horizontal, hint of vertical indentation; (19) a real puzzler,

/khau^2/ 'rice,' in Bradley's plate, though unclear, seemingly kh$_3$, and it was so interpreted in the Schmitt plate, but in *Phak 1* it appears with a flat horizontal and the barest hint of indentation in the vertical, still, in terms of the foregoing, acceptable *kh khuat* features, which would make this word etymologically incorrect; (20) /khī/ 'ride,' roundish top and no indentation; and /khāy/*(khuat)* 'sell' flattish top and no indentation (poorly legible in *Phak 1*); (23) /khō/ *(khuat)* 'beg' > 'goad,' flat top, no indentations (in *Phak 1* the barest hint of indentation in the vertical), /khau^2/ 'rice', round top, unlike 'rice' in line 19, no indentations, /kha^2/ *(khai)* 'slave' flattish top, no indentations; (26) /khau^2/ *(khuat)* 'go in,' flat top, indentation on right vertical. The two occurrences of /khveen/ *(khuat)* 'hang' in lines 32 and 35 are illegible in the published Bradley plates, but in *Phak 1* they show clearly a rather flattish top horizontal and distinct, but tiny indentations in the right-hand vertical. The Schmitt plate, however, shows them with a flat, indented upper horizontal, showing how preconceptions may alter readings.

Following this, on sides 2-3, the distinctive feature clearly becomes the indentation on the vertical, whatever the shape of the top horizontal. The writer(s) of RK seem to have started with one set of conventions, but half - way through their task switched to another.

An anomaly in all reproductions of RK is that there is absolutely no difference in the kh symbols of /khab/ 'drive' in I/5, 7 (correct *khai*) and in /khap/ 'sing' in II/19, although all transliterations after Schmitt except the recent *Silpavathanatham* show the words with their historically correct initials corresponding to *kh ('drive') and *x ('sing'). *Silpavathanatham* has shown admirable fidelity to what is written on the stone, and has transcribed both words with *kh khai*. Coedès might have determined the historically correct form of 'sing' through the comparison which he made with White Tai,[28] but for Bradley, who did not make such a comparison, and who considered the two characters meaningless allographs, his transcription must have been based on current Thai usage which then would have preserved some correct historical distinctions on rare terms.

Bradley must have determined the distinctions from face 1, and regularized the spellings in his transcriptions of the following faces, where there is no new term which might cause confusion. There is, however, one irregularity in the original, which Bradley faithfully reproduced. The word /khwā/ 'right', in face 3, line 20, is written with a clear *kh khai* according to the conventions of faces 2 and 3. This is also the way it appears in *Phak 1,* and this is the way it has been transcribed by Bradley, Coedès, *Phak 1,* the Chulalongkorn memorial volume, and *Silpavathanatham.* In the Schmitt plate it is illegible.

Most commentators on RK since Bradley have treated the indented, or flat, top horizontal as the -emic trait, making kh_3 *khuat* and $kh_{1,2}$ khai. Finot in 1917, in his plates of Thai scripts, showed kh_3 for *kh khai* and kh_3 for *kh khuat* in RK, ignoring kh_2 which he must have considered a meaningless allograph of kh_3. The same choices have been made by most recent modern Thai editors, such as the National Library's new publication of Inscription 2 in 1983 where, in a table comparing the scripts of Inscription 2 and RK, for RK kh_2 is shown as an allograph of kh_1, both representing *kh khai,* while kh_3 is *kh khuat,* although their kh_3 has both the indentation in the upper horizontal stroke and on the right vertical. The Chulalongkorn University 700 - year Commemorative publication of RK has also traced their *kh khuat* with a clear indentation in the upper horizontal, although it also shows the right vertical indentation, but it is certain that the upper horizontal identation was considered the crucial one, for certain cases of *kh khai* are rendered with the right vertical indentation, but not the upper horizontal one.

Among publications by non-Thai scholars only the tables in Coedès' 1923 *Tamnan aksor Thai* differ, for they seem to show kh_1 as *kh khai* and *kh_2,* albeit with a very flat top horizontal, as *kh khuat,* but this early work of Coedès, obviously intended as a sketch of the then conventional wisdom rather than a scholarly advance, cannot be taken as decisive, for it is clearly mistaken about the *kh* symbols for "Loethai" and the period of Naray. For 'Loethai' Coedès shows kh_3, for *khuat,* which is quite mistaken, as I shall explain below, and for the Naray period he shows

kh_3 for *khai,* and no *kh khuat,* which is misleading, for although with respect to historical exactness there was much confusion in 17th century Thai, the two symbols kh_1 and kh_3 were rendered distinctly.

Diller now also has adopted kh_1 and kh_2 as paradigmatic for *kh khai / khuat,* ignoring kh_3, but if the indentation on the right-hand vertical is taken as the -emic feature, then several words on face 1 of RK must be interpreted as etymologically incorrect, destroying any picture of RK historical accuracy. The historically *kh khuat* words which must then be read as *kh khai* are (line 3) /khun/, (5) third occurrence of /khun/, (6) probably /khau^2) 'go in,' (20) /khāy/ 'sell,' (23) /khò) 'goad;' and in line 4 the historically *kh khai* word /khāu/ 'rice,' 'year,' would have to be read as *kh khuat.*

The latest approach to RK is the transcription in *Silpa-vathanatham,* Special Issue on the Ram Khamhaeng Inscription, July 1988. There is an attempt to reproduce by tracing, or drawing, the text of the inscription in the actual RK script. But the table following the inscription, p. 30, shows the editors' choice of -emic feature between *kh khuat / khai* to be the indentation on the right- hand vertical, while their representation of the entire character is inaccurately schematic showing both characters with very flat upper horizontals.

In their transcription of the text, however, they have imitated the clear cases of rounded upper horizontals, although the -emic feature for them throughout the text has clearly been presence or absence of indentation on the vertical; and indentations on top horizontals have been interpreted away.

An interesting case of complete fidelity in transcription is in the words for 'drive' and 'sing,' which they have rendered in all three cases with *kh khai,* in agreement with the original, and in refusal to be influenced by what should have been the correct historical spelling. It is not possible 'sing' and 'drive' in RK differ in any significant way, or that they can fairly be interpreted as anything but *kh khai.* The *kh khuat* which has been accorded 'sing,' at least since the work of Bradley, and including that of

Coedès, seems to indicate some survival of correct etymological spelling into the 19th century, and it damages the argument that late fakers could not have gotten their spellings correct.

From the begining of RK studies, then, there was first of all a problem of recognizing what symbols, if any, were to be understood as representing the *kh khai / khuat* distinctions.

Before returning again to confrontation of RK with Proto - Thai correspondences, I should like to illustrate the interpretative identification of *kh khuat / khai* which I have described above.

As the following table illustrating readings of [Schmitt], Coedès, Griswold and Prasert, the Chulalongkorn University commemorative text, *Caru'k samay Sukhothai, Phak 1,* and *Silpavathanatham* shows, there has not been complete agreement on all items among any two of them.

As long as some of these key words occasion disagreement among the best readings of RK, it is impossible to claim that its author(s) were working with two phonemes /x/ and /kh/. Even if the cases which may represent no more than heedless reading are sorted out in a way that shows /x/ - /kh/ regularity, there still remains the indubitable double spelling of /khwā/ 'right,' and the single spelling (kh_1) of both 'sing' and 'drive,' neither of which would have been possible had the phonemes still been distinct. What we have is an RK, just like the 14th - century inscriptions, showing just enough /x/ - /kh/ regularity to indicate a script for a language which had such distinctions, but which was not the language of Old Sukhothai.

	PT/W	Schm	Coed	G/P	Chula	Car	Phak1	Silp
kill	kh/x	kh_1	xa	xa	xa	xa	kha	xa
Mt/horn	kh/x							
1-3-6		kh_2	xau	xau	xau	xau	xau	xau
1-3-9		kh_2	khau	xau	xau	khau	xau	xau
rice	x/kh	kh_3	khau	khau		khau	khau	khau
ride	kh/kh							
1-1-7		kh_3	khī	khī	khī	khī	khī	khī
1-1-20		kh_1	khī	khī	khī	khī	khī	khī
1-1-28		n/a	khī	khī	khī	khī	khī	khī
1-3-21		kh_1	xī	khī	khī	khī	khī	khī
right	khw/xw							
1-1-5		kh_1	xwa	xwa	xwa	xwa	khwa	xwa
1-3-20		n/a	khwa	xwa	khwa	xwa	khwa	khwa
sell	kh/x	kh_1	xay	xay	xay	khay	xay	xay
sing	kh/x	kh_1	xap	xap	xap	xap	xap	khap
1-2-19								
drive	khl/ch	kh_1	khap	khap	khap	khap	khap	khap
1-1-5,7								
dig	x/kh	kh_1	khut	khut	khut	khut	khut	khut
1-4-4								

It may now be interesting to compare the *kh khai/khuat* conventions in the later Sukhothai inscriptions.

In Inscription No. 2, according to its latest publication, although no legible plates are available and the original has been set up in a position which makes further direct study impossible, a different orthographic convention from RK prevails.[29] There are two symbols, one of which, the most frequent, looks like a straightforward *kh khai,* and it has been interpreted in this way, and another with an indentation on the vertical trait at the right side of the letter, which has been read as *kh khuat.* In other words, the symbol which has been read as *kh khuat* in Inscription 2 has often been treated as one of the allographs of *kh khai* in Inscription No. 1.

The Jataka plates of Wat Sri Jum contain the same pair of symbols as Inscription no. 2, but they have been interpreted as in no. 1, allographs of *kh khai,* and no *kh khuat* has been recognized in those texts, which most students have assigned to a rather early date, closer in time to RK than the major Sukhothai

corpus. Of course there are perhaps too few meaningful occurrences of the kh symbols in the Jataka plates.

For few of the other Sukhothai inscriptions are there published descriptions of their script, nor legible plates from which to deduce much about *kh khuat / khai* conventions throughout the Sukhothai period. Finot in 1917 did not recognize any kh khuat symbol in Inscription 3, although Coedès' 1919 publication of that text did. He did not then, however, publish a plate or table to indicate what he had read as *kh khuat.* The terms in which he recognized *kh khuat* show a large degree of consistency and historical accuracy, including — not in RK — *(kh khuat) xoṅ* 'thing', in agreement with White Thai, *xān* (but also *khān*) 'respond,' *xat* 'break,' in agreement with White Thai; but one historical error in *khwān* 'broad,' which if it were to agree with White Thai and Diller's hypothesis should be written with *kh khuat.* Some of these occurrences are legible on the plates published by Griswold and Prasert in their EHS 11, Part 1, and the *kh khuat* symbols show a very rounded top and an indentation in the vertical trait, *just like the kh symbols in the Montigny plates of RK.*

For Inscription No. 5, the plates in *Phak 1* and in *Caru'k,* which are in part legible, seem to show conventions like No. 2, kh_1 as *khai,* kh_2 as *khuat* with, however an occurrence of the symbol with indented upper horizontal trait in 'rice' (side 1, line 25), but read as *kh khai,* the proper etymology. Except for this face 1, with the words for 'go up,' 'slave,' 'rice,' 'kill,' 'cross over,' also shows perfect historical accuracy. The plate of face 3 is also rather legible, but with few relevant words, and the only apparent irregularity, 'go up,' line 36, transcribed with *kh khai,* but just at a place where the plate is too illegible to check. It is noteworthy that the top of the *kh khuat* character is very round, like Inscription No. 3 and *like the Montigny plates of RK,* and that Coedès, in *Receuil,* did not recognize *any kh khuat* in inscription No. 5. Perhaps his mistaken belief that 'rice' *(khau)* was a *kh khuat* word, seen in Burnay and Coedès, resulted from that word apparently written with kh_3 in No. 5. In the publication of *Receuil* however, perhaps influenced by comparative etymological aware-

ness, he obviously treated kh_3 in that single occurrence as a meaningless allograph.

The same is true for Inscription No. 8, in so far as the plates are legible, where those symbols interpreted as *kh khuat* are provided with a distinguishing indentation, or cross mark on the right-hand vertical (side 2, lines 26, 27). and with etymological accuracy.

When the plates of these Lithai-period inscriptions, rather than just the published transcriptions, are examined, they show a kh_1/kh_2 distinction for *khai / khuat,* and a much greater degree of consistency and etymological accuracy than I earlier believed. They might well support an argument that 14th century Sukhothai writing was conscious of a distinction in the two velars, whether or not the distinction was maintained in the spoken language.

Now what does this say about the status of RK.? Strictly nothing. As I did not complete that part of my RK investigation it did not appear clearly in my Canberra presentation, but study of the text of RK reveals so many passages parallel to other Sukhothai inscriptions that it is certain either the writers of the latter knew and carefully studied RK, or the writer(s) of RK had studied at least inscriptions 2-3-4-5-8. The case for the rejection of RK must show that it is more reasonable to suppose that its author(s) knew the other Sukhothai inscriptions, than that the latter were based on study of RK. The resemblance of the *kh* symbols on the Montigny plates to Inscription 5 is a case in point. Since kh_2 with a well-rounded top had disappeared from Thai script, it is unlikely that the copiers of Montigny could have interpreted RK in that manner unless they had been influenced by other old inscriptions exhibiting that form.

Later, post-Lithai, Sukhothai inscriptions begin to show different conventions, including the appearance of clear examples of kh_3.

In Inscription No. 45 there are clear instances of kh with indented upper horizontal (formally modern *kh khuat)* in *'khun'* (title), 'mountain' (side 1, line 8, 13, 16), and a non-indented, modern-type *kh khai* in *khabon˙, khiav,* and *'khòm'* (side 1, lines

16, 20, 28, 29). It is worth noting that in the interpretation of the text in *Caru'k* the title *khun* has been both transliterated, and transcribed in modern Thai as though it were *kh khai,* while 'mountain' has been transliterated with *kh khuat,* but rendered in modern Thai with *kh khai,* all cases reflecting *interpretation* according to what modern readers thought should have been.

This may prove embarrassing for RK, which in its first face shows a kh_3 - type character for *kh khuat* at a date before that character appears in other Sukhothai work. Moreover, the distinctive features of all RK velar aspirates are so vague as to even raise doubt that the writer felt confident about the distinctions being made.

The Status of Old Sukhothai Language

Diller has now related Old Sukhothai to other Thai languages in a novel way, which is not entirely explicit, and he does not even show that he is aware of what he has done. I assume, however, that he is, but that it was not made explicit because he has chosen to base his argument, as I did, following Coedès Brown, Li, and Chamberlain, on standard rules of procedure in comparative historical reconstruction. By a number of remarks in various articles Diller has indicated that he is not entirely satisfied with this procedure, and would prefer to utilize wave-theory linguistics, but until that has been systematized with respect to comparative historical reconstruction we must hold the arguments within the old limits until the end of their applicability has been reached.

Now I would first like to make the point that the velar distinctions in Old Sukhothai *(khai / khuat / / khwāy / khon)* could in principle represent either the state of the language, *or* merely the state of an alphabet adopted from a different language where such distinctions were made. I believe this second possibility has never been considered because everyone has assumed that Ram Khamhaeng invented a script perfectly suited to his spoken Sukhothai language, or at least even if Ram Khamhaeng did not invent *the* Thai script, the writing of the inscriptions represented

the local language as then spoken. Since, however, both pairs of velar distinctions were soon neglected, and not maintained in any languages which succeeded Old Sukhothai in central, northeastern, or southern Thailand, the possibility that Old Sukhohai script represents an alphabetic structure borrowed from some other, different, language should be given serious consideration.

I would also like to re-emphasize a point I made in the Canberra paper, that the adoption in Sukhothai of an Indic-type alphabet distinguishing a series of voiced stops which are now unvoiced in Thai tells us nothing about the sounds represented by those symbols in 13th century Thai, nor about the sounds they represented in contemporary Khmer. Whether or not either Thai or Khmer had by then devoiced, the same distinctions in script were still useful for indicating the distinctions in tone or vowel which took up the load formerly borne by initial consonants. Thus I reiterate contra Gedney, "Comments," we cannot tell from inspection whether RK was a P, a PH, or a B language. Assertions that it must have been a B language are assumptions and nothing more.

Before going on with this let us recall the position of Old Sukhothai within the Thai family as hitherto reconstructed.

Li did not make a clear statement, with the exception that Sukhothai belonged to the Southwestern group. He also believed, reasoning from the script, that it had maintained ancient voiced initial stops, which would make it somewhat aberrant; but which, as I have said, is an assumption that is not necessary. Within the Southwestern group Li formulated three types of languages according to tone patterns. Within his Type III are all the languages within Thailand in which original voiced stops have devoiced to aspirates, except Tak Bai, while the Yuan or Lanna languages of northern Thailand in which the devoicing led to non-aspirates are in Type II, and the other major Southwestern languages showing devoicing to non-aspirates, such as White, Black, Red Thai, Lü, and Shan, are in Type I.

Marvin Brown considered that Sukhothai represented a special branch uniquely descending from Ancient Thai and pre-

serving many of the significant features of the proto-language, seemingly equivalent to what some other linguists might call an example of Proto - Southwestern Thai; and he held that Sukhothai was the ancestor of Southern Thai, distinct from the branches which include Lanna, Lao, or Ayutthaya. He also believed that ancient initial voiced stops had been maintained in Old Sukhothai, but his analysis as a whole is skewed through neglect of the languages of northern Laos and Vietnam, such as White Thai. All of the languages utilized by Brown were within Li's Southwestern Group, and Brown's family tree shows major distinctions between groups of languages in which devoicing produced aspirates, or nonaspirates. In Brown's system this dichotomy was produced by a split in what he called language no. 3, "1150 Chiang Saen," which split into aspirate-type languages Phuan and Ayutthaya and their descendants, and non-aspirate type languages Shan, Chiang Mai, Phrae, Nan, Lampang, Chiang Mai.

Still another scheme for classifying the Thai languages was Robert B. Jones' "On the Reconstruction of Proto - Thai."[29a] There he too showed a division into General Southern with all of the languages of Thailand in which voiced stops devoiced to aspirates, and General Northern, with the languages in which devoicing had gone to non-aspirates. General Northern was further subdivided into a Shan branch, a Kammyang branch, and an Eastern branch with Lü, White Thai, and presumably Black and Red Thai.

All of the foregoing based their categories on tone-type distinctions, not on the aspirate/non - aspirate classification which I have just emphasized, although they all acknowledge that it was change in the quality of the initial stops which produced the initial tone distinctions.

Then James Chamberlain made this aspirate/non - aspirate dichotomy a major classifier for language splits, and he clearly described how the Southwestern Thai languages, which include all those relevant here, could be divided into a P group and a PH group, depending on whether the original initial voiced stops devoiced respectively to unaspirated or aspirated voiceless stops. The former include Black and White Thai and the Lanna languages, while the PH group includes Ayutthaya, modern stan-

dard Thai, Lao and Southern Thai. Thus, for example, at the time when Proto-Southwestern Thai unity broke up, the ancestors of Ayutthaya and Lao were in a different group from the ancestor of White Thai, based on a distinction in what happened to the ancestors of *kh khwāy* and *kh khon.*

Still another classification is that of John F. Hartmann, "A model for the alignment of dialects in Southwestern Tai," again based on tone - type categories. Nevertheless, we find a division into "Lower Southwest" (all PH languages), "Middle Southwest" (Lü, Khün, Chiang Mai), and "upper Southwest" (Shan, Lü, White, Red, Black, and other P languages of northern Laos and Vietnam).[29b]

Thus although no one else has made it their major classificatory feature, Chamberlain's P/PH dichotomy, with a further split between White, Black, Red and Lanna/Yuan among the P languages, comes through clearly in the classifications of all linguists. This is not surprising, because, as all are agreed, it was this very original distinction, between voiceless and voiced initial stops which determined the ways in which ones first split. Then, according to Hartmann, there were two stages of devoicing, of which the second produced aspirates as well as the tonal development characteristic of PH languages. Although Hartmann does not express it that way, his stage 1 implicitly resulted in P languages, a stage which he says "can safely be assumed to have affected all branches and dialects of the Tai language family at some point in their history." This means that all the PH languages were at some point P languages, in contrast to Brown's grouping in which the splitting off of PH languages had priority, and different from Chamberlain's hypothesis in which devoicing into P or PH occurred from the beginning among different groups of languages.

Although most other linguists have preferred to base their classifications first of all on tones, there has been no published criticism of Chamberlain's P/PH dichotomy, which has been in print since 1972, and has gone through several more recent linguistic publications. It also, it must be noted, gives due consideration to tonal types in the analysis of further developments

after the initial P/PH split.[30] Now Gedney, apparently because of the use I made of the P/PH contrast, has attempted to knock it down. Gedney, though, like everyone else, starts with the " ...drastic changes...in these initial consonants...in some places previously voiced sets of consonants became voiceless...In fact it was these consonant changes that brought about the new tone systems..." (page 3). Further, the voiced sounds b, d, g, "have everywhere become voiceless, in some areas *p, t,* and *k,* in other areas ph, *th,* and *kh.* But then (11) Gedney claims that the P/PH distinction is no more relevant than r/h, or other distinctions; and that P/PH is no good because "it uses as its basic criterion something very late in the history of these languages" (11). Well, whatever its age, it cannot be dismissed as merely the equivalent of r/h, for P/PH is universal to the classification of the Southwestern and Central Thai languages, and, as we have seen, it comes forth willy-nilly in all Thai-language family trees, even when their inventors were working with other features, and seemingly were not even giving thought to the P/PH question.

It seems that this may not always have been Gedney's view on the importance of P/PH. In his review of Marvin Brown's book over 20 years ago, in suggestions for further work, given the little that Brown had left for others to do, he wrote, "One might also go on to plot the areas in which original voiced stops (in the Low series) have become voiceless aspirated stops as in Bangkok or have become voiceless unaspirated stops as at Chieng Mai."[30a]

I submit that Gedney's present position on P/PH simply will not wash. Gedney, in accord with all other linguists, has agreed that it was the devoicing of initial voiceless consonants which produced the tonal distinctions which they prefer to use in their classifications. By the very nature of the phenomenon, devoicing of b, d, g, which are the consonants concerned, will produce either aspirated or unaspirated stops — there is no other choice. If all of this occurred very late, it implies that several branches of Thai remained identical as to tone and initial consonant system, preserving Proto-Thai features, long after the groups of people speaking them separated and became isolated from one another. If so, then the devoicing and resultant tonal change in each

branch would have been unrelated to the others, and the reconstruction undertaken by linguists, whether based on tones, or on consonants, is invalid. Indeed, we would then expect, even within Southwestern Thai, entirely separate groups of P and PH languages, as the different sub-groups devoiced independently. Instead all evidence suggests that the division now represented by P/PH was the first split between the ancestors of, for example, the White, Black, Red Tai and Lanna languages, and the ancestors of Modern Thai and Lao. The entire logic of Thai language grouping undertaken to date depends on changes in consonants affecting tones having begun before the major dispersion of Thai peoples out of their earlier area of close intra-Thai proximity.

It is also necessary to say something about Gedney's tone and mode of discourse, which relies far too much on brow - beating and crude assertion than on reasoned argument. It is quite irrelevant that in "student days, Chamberlain liked to come up with maverick ideas;" and if Gedney regrets "not having tried to rescue him from his wicked ways" at that time, he could have at least argued against Chamberlain's P/PH categorization in print, in the linguistic publications in which Chamberlain published, or in other publications read by the profession, rather than simply engaging in *ad hominem* attacks only now when the P/PH question has come up against Ram Khamhaeng. If Chamberlain has "had some success in peddling [his ideas] among non-linguists in Thailand," it is because he has been willing to risk his arguments on paper, where the non-linguists have seen them.

One of the details this non-linguist historian picked up from a Chamberlain publication, "A New Look at the History and Classification of the Tai Languages" (p. 58), was sourced to "Gedney unpublished)", that "The age of PT [Proto-Thai] has been estimated at not older than 2000 years...and the language diversity within the family today is perhaps comparable to that of the Romance branch of Indo-European." This implies that Proto-Southwestern Thai unity may go back almost that far, that 2000 B.P. may be the approximate time of the split between PSWT and other descendents of PT. Since I continue to maintain that the logic of the linguists' arguments implies that a single

tone for each consonant series cannot have been maintained long after the splitting of languages, I also maintain the view expressed on this matter in my Canberra paper, and which Gedney has taken issue with on his page 9. If Gedney really wished to discuss these matters among scholars interested in Thai history, instead of limiting himself to cheap-shot wisecracks, he could take up the position which Chamberlain imputed to him and say (1) that he never said it, (2) he said it but it doesn't imply what I think, (3) he said it but has now changed his mind, and for what reasons; and he could explain how consonant/tone categories could remain stable after splits such as PT > PSWT, a split which by the very nature of the situation hypothesized depended on changes in tonal patterns resulting from consonant shifts. In fact we see from Gedney's conference paper (p. 2) that still, "[a]s regards date, scholars believe that Proto-Tai was spoken by a single group of people about two thousand years ago," adding now "or less." Precise dates of course are impossible, as Gedney knows, and as I knew in making my 2000 - year statement in the Canberra paper; but if there was Proto-Tai unity, and the family pattern resembles Romance, or as Gedney now puts it, "the degree of divergence seems to be about the same as in other language families whose history is better known," then the splits among languages branching off from Proto-Tai, such as Proto-Southwestern Thai, etc., must have begun not long after B.P. 2000, and the splits must have been characterized by at least signs of changes in the major significant features, such as initial consonant changes and resulting tone changes.

Thus in what follows I shall take it that Gedney's objection to the P/PH dichotomy is not proven, not even yet argued, and I shall continue to use it as a tool in discussion of RK.

As I pointed out in Canberra, it is impossible to know Sukhothai pronunciation, and thus to determine from simple inspection whether Sukhothai was a P or PH language, although it has been assumed to represent PH. And indeed the way its writing conventions evolved, it probably was PH. *Pace* Gedney again, we cannot know that "the letter now called *phoo*...was pronounced b" (11). At the moment that is only the result of a

series of *assumptions* about RK which I believe the arguments so far made cannot sustain.

The purpose of the overly long foregoing section, which had to be expanded when I received Gedney's paper just a few days before it was necessary to wrap these comments up and mail them, was as background to Diller's "Consonant Mergers and Inscription 1."

In this article Diller takes a new position on Proto - Thai (PT) based on Gedney's proposed additional series of velars,[30b] which explain the anomaly that some words which in their tones reflect PT voiceless initial consonants in one branch of Thai have cognates in another branch which reflect PT voiced initials. The White Tai distinction between /kh/ and /x/ goes back to such distinctions, not given importance by Li, and which therefore led me in my Canberra paper to emphasize apparent disagreements among White Tai, Standard Thai, and RK with respect to PT. One of Diller's goals is to show that RK is in perfect agreement with White Tai, therefore in agreement with PT, and therefore an authentic period piece because no later forger could possibly have reconstructed the correspondences required.

I agree that a late forger could not have reconstructed from nothing the correspondences presented by Diller; but now the situation with respect to RK itself has changed.

As I tried to show above, the reading of *kh* / *x* in RK is not what Diller, or I, thought last year, and there are inconsistencies within RK itself which damage the image of regularity there with which Diller worked.

Furthermore, the regularities which are evident in RK are equally evident in Lithai-period inscriptions, which does not affect my general argument about RK, since I believe it was based on knowledge of the other inscriptions, but it does relate to the position of Old Sukhothai, which Gedney also now agrees must be based on "attention to other Sukhothai inscriptions besides Inscription One" (p. 9).

Now for the moment abstracting from the clear historical

errors of RK, such as the writing of *khap* 'sing' and one occurrence of *khwā* 'right' with *kh khai,* and similar errors in the Lithai inscriptions, what we find in Diller's comparisons is agreement between RK and a P language. There is also another agreement of the same type, in the use of both *kh khwāy* and *khon* in RK, and which correspond to the same phoneme distinctions still preserved in White Tai, but which are lost in late Sukhothai. But there is no agreement between Old Sukhothai and White or Black Tai in their mergers of γ with other phonemes (in WTγ > x, BT γ > k[1],Sukh γ > kh, Lanna γ > kh),[2] with respect to velar-liquid clusters, which in Sukhothai, as well as in all PH languages, plus the P group of Lanna, have either been preserved as clusters or reduced to the corresponding P/PH velar, while in White and Black Tai some of them have become sibilant initials.

What Diller's argument suggests is that Old Sukhothai, at the time of RK, was a P language, because it maintained distinctions (kh/x, g/γ) which cannot be reconstructed from any combination of modern PH languages. But if it was a P language of the White-Black Tai type, one would expect some sign of the same treatment of velar-liquid clusters. Alternatively, one might suggest that Old Sukhothai was part of the Lanna P group, since there γ at least has also become /kh/, but the inscriptions of those languages, while still showing a few features which Li would have attributed to PSWT, do not preserve the kh/x distinction, although they still today maintain the g/γ distinction.

Moreover, the ensuing written record of Old Sukhothai, beyond the Lithai period, shows a fading away of these P - type distinctions, indicating that Sukhothai was really a P language which became a PH language, an example of what Hartmann's hyothesis about initial consonant and tone change implies — that all Thai languages were at one stage P languages, but that following the second shift of former voiced initial stops to aspirates, some of them became PH - type.

According to the linguists' own rules, such a sequence of changes is impossible. Unconditioned splits are impossible, as Diller says, and as Gedney emphasizes, "once a set of words in one box has fallen together with those in another box, it would

be impossible for speakers to sort them out again into the former pattern" ("A comparative sketch of White, Black and Red Tai," p. 11). This means in the case of Sukhothai that if *g merged with *k, as in all P languages, then /k/ < *g could not have split again to produce historically correct /kh/.

What we are left with is that Diller's comparison of RK with the new PT via White Tai has produced a hitherto totally unknown type of Thai language, perhaps a linguistic monster in the most genuine sense of that term, perhaps even a language which could not have existed.

The best way out of these difficulties, it still seems to me, is what I proposed in the Canberra paper, that the script which was used for Old Sukhothai in the inscriptions we possess was borrowed, and perhaps adapted, from another Thai language of the P type, and that therefore P - type distinctions unrelated to Sukhothai phonetics were for a time preserved, then dropped.

There can be no question that the alphabet which we see in the Sukhothai inscriptions was invented once and for all for that language in the 13th - 14th centuries. It derived from writing traditions which had been developed for Thai languages much earlier, and in its use preserved characteristics of those languages.

In that connection let me refer to Gedney's new paper again. He seems to consider (p. 11) that the worst deviltry of that maverick Chamberlain has been to suggest that Thai-speaking people "acquired their writing system far to the east, in Vietnam, very early, from the Chams," but that none of the rest of us, "usually so eager to believe everything Chamberlain says," have been able to swallow this. Passing over Gedney's again inappropriate mode of discourse, I would like to note that in fact, in my Canberra paper (pp. 197, 204 - 5), I referred sympathetically to Chamberlain's Cham hypothesis, as one way to account for some of the anomalies of early Thai script. Further, in my oral presentation there I expanded on the question of the formation of early Thai writing systems, and suggested more strongly that the Thai had indeed, several times, borrowed Indic scripts from

neighbors in Indochina before the Sukhothai period. I do not see why this idea should shock anyone, for it has always been acknowledged that at least the Sukhothai script was borrowed from one Indochinese neighbor, the Khmer, while on the basis of extant epigraphy the Cham, who must have been at some time neighbors of some group of Thai, were the first Indochinese people to use an Indic script. In my oral presentation in Canberra I demonstrated that the different ways in which the various Thai scripts have formed new letters from traditional Indic ones for sounds peculiar to Thai, or for special Thai problems, indicate that the scripts of Ahom, Black/White Tai, and Sukhothai/modern Thai are separate developments probably originating at different times among different groups of Thai while *they were still in Indochina,* not succeeding stages from a single orginal, whether RK or something else. I also suggested that Ahom descends from the earliest such Thai alphabet originating in Indochina, that Black/White Tai script is next oldest, and Sukhothai youngest, and influenced both by the earlier Black Tai script and Khmer. The ways in which Indic script structure was adapted in these Thai efforts show that it is not at all unreasonable to suggest that a Cham-type phonology exerted some influence.[31]

I regret that I have not had time to write up the arguments presented there, together with the illustrations required to make them clear, for I cannot expect serious comment, either for or against, until the entire argument is presented in writing. Pending that, which I hope will be my next RK installment, I would like to remind everyone that Burnay, Coedès, and Finot also considered that the Thai had borrowed scripts from farther east before the Sukhothai period. If they were still here, and still maintaining that view, would Gedney dismiss them as maverick devils to be exorcised rather than reasoned with?

A crucial point in the P/PH discussion, in Diller's new treatment of RK and White Tai, and in presentation of evidence on RK, is the devoicing of Proto-Thai voiced initial stops, *b, *d, *g to become unvoiced p, t, k, or ph, th, kh. As I have said above, and I think more than once, devoicing, or not, cannot be immediately determined from script, for the very nature of Thai script,

preserving the ancient Indic consonantal structure, disguises devoicing, in that original symbols for voiced consonants continue to be used to help make other distinctions. This disguise is maintained in modern Thai; and a linguist from Mars acquainted with Sanskrit and shown a modern Thai text would affirm that ancient Proto-Thai *b, *d, *g were still today maintained as such.

The only way in which devoicing may be determined through texts of earlier times is if there is confusion between high and low *kh, th,* or *ph* in a PH language, or between low and high - middle *p, t,* and *k* in a P language.

As Gedney has conveniently summarized in his current paper (5), "we cannot tell from written records when the changes took place," particularly from "dated inscriptions...usually more carefully composed, and unlikely to contain many errors..." which might be found if we had "a lot of dated casually written material...and the scribes were confused in trying to write words in which certain sound distinctions had been lost."

A corollary implied in Gedney's clear exposition of this point is that even a few examples of confusion in dated inscriptions carry much weight, and may permit conclusions for which in some other context we would wish to have quantitatively more evidence.

As I already showed in my Canberra paper, there are a very few examples in Sukhothai inscriptions of precisely that type of confusion which indicates devoicing. On my page 198, following Coedès, I pointed out a confusion of g for kh in 'threaten' in Inscription No. 3, a spelling also found 4 times in Inscription No. 38; and in Inscription No. 38 *khamoy* is spelt with g. I admit that I have been unable to discover the history of these words in material to which I have access, and there is a possibility that the usage in Inscriptions 3 and 38 is historically more correct. This panel would have been an occasion for those opposed to tampering with RK, rather than just assume airs of moral superiority, to correct such details which have direct bearing on the problems at issue.

Since then I have noticed another item in Inscription No.

3, face 2, line 44, which may indicate confusion of the high and low velars, a sign of devoicing. That line, following a lacuna in the preceding line, begins with *tai khaṃ* (ข้ำ), which Coedès translated as 'étayer', 'support', and which he took for "another form of' *gaṃ* (ค้ำ). Griswold and Prasert, with a different conjectural restoration of the lacuna, have interpreted the word differently, "equivalent to *Tai Yuan [gaṃ]* (ค้ำ), 'to tyrannize'," etc., still representing a case of confusion of initial high and low velars. Interestingly, in the comparable RK passage, of which Inscription 3 has been termed an 'echo,' the word is written with *kh khon,* and translated by Griswold and Prasert as a 'euphonic filler' or, again, 'support,' Here is a matter for serious investigation by the purists. Should 'support' historically be spelled with ข, ค, or ค ? In Black Tai, the only relevant language for which I now have an example, it is spelled with the symbol indicating *either* PT *g or *γ; but this at least indicates that the occurrence in Inscription No. 3 proves devoicing, and of the PH type.

The Sukhothai inscriptions also contain numerous occurrences of confusion between historically voiceless /s/ (ส) and voiced /z/ (ซ). Until reading Gedney's paper, I was under the impression that they were not considered sufficiently indicative of the more important devoicing of stops, but Gedney's use of non-stop voiced/voiceless pairs to make his points suggests that they are.

Some of the instances of tone-mark confusion which I pointed out may also be indicative of tone shifts and mergers resulting from devoicing.

There are also a few relevant cases from inscriptions of Lanna. In Inscription No. 68 from Lamphun, date 1489, line 19 we find *bai* for *pai* 'go,' an expected confusion in a P language, and the same occurs again in Inscription No. 73 also from Lamphun and dated 1489, line 14. These can hardly be anything but certain indications of devoicing, at least in that group of languages, although they may not prove the same thing had also happened at Sukhothai.

Together with these signs of devoicing, some of the northern

inscriptions preserve features which according to Li represent the stage of Proto-Southwestern Thai, for example *hrā* 'seek,' in No. 67, Lamphun, 1488; *hrok* 'six,' No. 63, Phrae, 1456; and *hrin* 'stone' No. 66 Chiang Rai, 1484. These are not cases, it should be noted, in which maintenance of initial h proves a proto-language unvoiced consonant, as in the example *hmā* 'dog' used by Gedney. They are old unvoiced sounds, still unvoiced in Lanna languages and in modern Thai.

These details together might indicate that devoicing and P/PH separation indeed preceded the loss of other Proto - features.

Ayutthayan Verse Forms and Sukhothai Tones

What appears at first reading as an objectively grounded refutation of the relative periodization of initial stop devoicing and ensuing tone splits evoked above is Gedney's "Siamese Verse Forms in Historical Perspective" (Conference on Southeast Asian Aesthetics, Cornell University, August 1973), by means of which Gedney dates the great sound changes "somewhere in the middle of the Ayutthaya period (1350 - 1767) ["Comments", p. 5], shall we say between 1600 and 1700."

What this would seem to imply, if the three-tone system accompanied by original voiced stops still persisted, is that after the hypothetical Thai unity began to break up around 2000 years ago, and Thai-speaking groups physically separated from one another, there were such geographically separate groups all still separately speaking Proto-Thai, in Sukhothai, Ayutthaya, Vientiane, Chiang Mai, Shan States, Northern Vietnam, until around 1600, something which I think most linguists would find *a priori* very unlikely.

I must state, before continuing further, that what I shall say on this subject is not at all linked to my own research and is solely concerned with the logic expressed or implied in the work of Gedney and other linguists. Whereas I have spent considerable time trying to learn to read inscriptions and chronicles, I have never studied Thai poetry, nor been interested in it, until I became

aware of the possible relationship to RK revealed in Gedney's "Siamese Verse Forms," which I have only read carefully while engaged in the present paper. Much earlier, however, I did spend some time looking at Khmer verse, from which I learned something of the type of rhyming exhibited in Thai poetry.

The argument in "Siamese Verse Forms" is: because old Ayutthaya verse forms require rhyming of tones, as well as vowels and consonants, it is possible to infer that tones indicated as rhyming really did rhyme, although in modern Thai the tone rhyming is artificial, based solely on correspondence of tone marks, which no longer correspond each to the same tone.

That is, according to Gedney, when the verse forms were developed Thai had only three tones, and when the writing system, represented by RK, was devised, whether earlier or later than the verse forms, there were still only three tones represented respectively by unmarked syllables, *mai-ek,* and *mai-tho.* Then as the three tones split into six, of which some merged with others to form the modern tone patterns, words carrying *mai-ek* or *mai-tho* no longer always had the same tone — the situation in modern Thai. One feature of modern Thai tones of particular relevance to the verse forms is the convergence of the *mai - ek* tone on low consonants with the *mai-tho* tone on high consonants, or in the notation I used in the Canberra paper, the complete homophony of B4 and C1 syllables. This means that words with the falling tone may be written two ways, one of which is historically correct, in comparison to other Thai languages, and the other a modern innovation. In Ayutthayan and modern Thai verse forms, such rewriting of these syllables is allowed in order to effect the required correspondence of tone-marks on syllables which are required to rhyme in that way.

Although this explanation, based on the idea that verse forms must once have meant what they say, is attractive, there are several logical objections.

First it assumes what is here the main subject of contention, that RK is genuine, that its use of Indic/Khmer voiced initial stop symbols proves that they were still voiced; and that therefore

there were only three tones denoted regularly by unmarked syllables, *mai-ek,* and *mai-tho* respectively. I have explained in some detail in my Canberra paper why the mere use of the old voiced stop symbols proves nothing; and of course if RK had never been discovered, similar inferences could not be made about tone structures from other Sukhothai, or early Ayutthayan, inscriptions in which tone marks are not used regularly.

I might comment here on one of the red herrings introduced by Gedney into his "Comments," page 10, and which serve to mislead the unwary, "the Young Turks get into terrible trouble [in] their asssumption that the great sound changes ...occurred...before the time of the Sukhothai inscriptions...they seem to be saying that Sukhothai speakers knew that certain consonant letters had formerly represented voiced sounds... ." Not at all. As I wrote for Canberra, when an Indic alphabet was adapted for Thai the same number of distinctions could usefully be made, whether based on voiced/voiceless, different tones, or different vowels. Whether *khā/*gā remained such, or changed to khā'/khā^2, or to khā/kea (Khmer), the distinction could be represented in writing by ขา/คา, as it is in different languages today. And if, as Coedès believed, and as I also do, some Thai learned to write well before Sukhothai times, they could certainly have learned their alphabets when voiced/voiceless distinctions were still active.

I am not claiming that the first Thai alphabet did not know voiced/voiceless distinctions, I am only claiming Sukhothai writing was not based on them, and that RK was not the first Thai script.

Now if devoicing and subsequent tone splitting only occurred after 1350 in different branches of Southwestern Thai, it would have occurred completely independently, without contact, in some cases, which is contrary to all reconstructions of the Southwestern Thai family tree. Even Brown, who had similar views about the nature of Old Sukhothai, was forced to postulate that it was a peculiarly isolated language which *alone* preserved some ancient features lost much earlier from other Southwestern languages.

In connection with this there is another example of Gedney's fiddling with the details (p. 11), "[a] curious notion [that] pops up from time to time in the writings of the Young Turks...is that from the proto-language... ." What I really wrote, and what Gedney obscures, was a denial of Brown's view that Sukhothai language uniquely descended virtually unchanged from Ancient Thai. There was no question of denying that any Thai language 'ultimately,' if through various stages of splits, descended from the proto-language. If Gedney agrees with Brown's family tree for Thai languages, he should just say so: but one may infer from his published remarks that he does not agree on that point with Brown either.

Gedney seems to have taken Brown's hypothesis about Sukhothai to its final logically extremist conclusion. Not only did Sukhothai evolve directly, and unusually slowly from Proto-Thai, outside the groups and splittings reconstructed for other Thai languages, but *all* Thai languages evolved separately and distinctly from Proto-Thai, not via the successive splits and subgroupings which all linguists have proposed. In the Thai - Romance analogy suggested by Gedney, it would be like saying the French, Spanish, and Italian languages only began to split from one another and from Latin around the time of Louis XIV.

Another logical difficulty in the presentation of the verse forms is that they have continued in practice, "and all of them are in current use today" (3), that is several centuries after Gedney himself realizes they are artificial, and, as he says, "in modern times only the highly literate can compose, or properly appreciate, poetry in the *khloon* and *râay* forms" (15),which now represent "highly literate pedants playing with the writing system" (18). The question is, when did this game-playing, as opposed to full genuine tone-rhyming, begin? In principle, there is no reason why such intellectual preciosity could not have been at work from the time the verse forms in question were first used for Thai. Gedney's answer, of course, is that RK proves the old regular tone patterns still prevailed, and it is assumed that the verse forms were in use, while it is recognized that the changes typical of modern Thai would have occurred by mid - 17th century.

For examples Gedney cited the *Lilit Phra Lo,* "the famous early classic," which, however his student Robert Bickner, in a dissertation on the subject, suggested was composed during the reign of Naray, when the language, as Gedney seems to agree, was no longer divided neatly into three regular tones (Robert John Bickner, "A Linguistic Study of a Thai Literary Classic," Michigan 1981, p. 28). That is *Lilit Phra Lo* was already a work of pedants playing with writing systems, and in itself is no evidence for a three-tone system with not-yet-devoiced initial stops. And if "a twentieth century poet...may impose upon himself either more or less stringent constraints than did those who first used the form in earlier times," this is simply an admission that poets may write artificially, and there is no reason to think they did not do so in the 14th century.

The specific argument made about *Phra Lo* concerns the occurrences of B4/C1 words which can now be written both ways. In *Phra Lo* there are several words written in modern Thai with low consonant and *mai-ek,* but in *Phra Lo* with high consonant and *mai-tho,* usually interpreted in standard textbooks as 'wrong *tho,*' used deliberately to satisfy tone - mark rhyming conventions. What Bickner and Gedney have demonstrated is that these words spelled in *Phra Lo* with high consonant and *mai-tho* are historically correct, whereas the modern forms are late innovations, etymologically incorrect.

In fact, this only proves that in early or mid-Ayutthayan times historically more correct forms may have been current, and it may prove that the B4/C1 merger had not yet occurred. It does not necessarily prove that the three-tone system still persisted. What would prove the contrary would be examples of modern-type non-etymological B4 words used where the rhyme scheme requires *mai-ek,* or examples of 'wrong' *ek,* historically C1 words respelled with low consonant and *mai-ek,* for that would prove B4/C1 merger, which admittedly occurred later than the split of three tones to six.

The notes I made from Bickner's thesis last year, before I was thinking of this problem, and on which I have to rely at the moment, do not indicate whether Bickner found any such ex-

amples in *Phra Lo,* but R.B. Jones has pointed one out in his *Introduction to Thai Literature,* p. 430, and note 18, the term /lau/ 'liquor', respelled with *mai ek* to satisfy the rhyme (เล่า), but both historically and in modern Thai *hlau*[2] (เหล้า) [see Li, p. 137].

This is so far a single example, against 13 words of the other so-called 'wrong' *tho* type, but it suggests that at least a Scottish verdict must be brought in on the subject of verse forms and Ayutthayan tone schemes. If B4/C1 merger had occurred when *Phra Lo* was written, it shows the 3 > 6 tone split had occurred, and before it devoicing of old voiced stops; and it seems difficult to accept that those three stages could be compressed into the Early Ayutthaya period.

A perhaps minor detail concerns the word /yaw/ 'house', now written เหย้า, but หย้าว in *Phra Lo,* which Gedney says is historically correct. In fact, Li, p. 181, says the initial was *ʔj, not *hj, and in the Sukhothai inscriptions it is written อยาว in Nos. 2, 3, 13, 38, as well as RK. This does not affect the the question of tone rhyme, although it would in a language like Luang Prabang, but it shows that when *Phra Lo* was written, h as initial member of a cluster no longer necessarily indicated an original phoneme, but was being used merely as a tone indicator.

Still another interesting detail is in an example of *râay* from *Lilit Phra Lo* ("Verse Forms," p.12). Out of 18 words on which obligatory tone rhymes are indicated, at least 10 are non-Thai, either Khmer or Indic, and thus would have originally been outside the Thai tone system. They are thus poor evidence either for three-tone regularity or non-artificiality of old *râay.* The 10 words are: /phon/ 'soldiers' (Indic), /pháhon/ 'soldiers' (Indic), /kaan/ 'action' (Indic), /pháyúhàbàat/ 'military movement' (Indic), /râat/ 'king' (Indic), /khróp/ 'complete' (Khmer), /trùat/ 'inspect' (Khmer), /sèt/ 'finish' (Khmer), /khàbuan/ 'model' (Khmer), /yâat/ 'go' (Indic). Probably /khàbèt/ 'sharp' is also Khmer, but I have not found it glossed, or glossable, in that sense. An interesting aspect of the use of such foreign words in verse is that they never have tone marks, and their use in the unmarked slot of the rhyme scheme in *Lilit Phra Lo,* whatever the real tone they eventually acquired in Thai, indicates that already at the time of the

composition of *Lilit Phra Lo* the rhyme scheme may have been "pedants playing with the writing system" without regard for true tones. Since some of them have original voiced initials, and some unvoiced, they acquired different tones in Thai, and in *Phra Lo* they represent pedants playing with the writing system.

Finally we may bring up a point about the origins of these verse forms. In his treatment of *kàap* Gedney (20) suggests "that *kàap* forms were first used in Siamese;" yet (23) he says "these *kàap* forms were borrowed *in toto* from Cambodian." This makes one wonder if *khloong* also, the very name of which is non - Thai (11), and could comfortably be considered Khmer, was not also borrowed from Khmer, and its rhyme schemes would therefore have been constructed for Khmer vowels, not Thai tones.

Gedney has indicated (19) the way to answer these questions – find examples of old *khloong* in the verse forms of other Thai languages, particularly outside of the immediate neighbors of Ayutthaya, such as Laos, where Ayutthayan influence can be expected. Given the work which has been done on Black and White Tai, it is surprising that relevant evidence has not appeared.

Perhaps this is the point for me to add a new suggestion about the way in which the tone marking system as we see it now evolved; and I certainly think it evolved from less complete and less regular use of diacritic marks for tones, and other distinctions. Gedney's ("Comments", 15) and other traditionalists' surmise that the full tone mark system could have been devised once and for all in the 13th century by someone who was "one of the brighter and more innovative among these" "teacher-scribes at work, at various schools...[s]ome...more ingenious, or more innovative and daring, than others," is to be rejected. The full tone mark system of RK is perhaps the most suspect of all its anomalies. The 14th century inscriptions, composed by leading intellectuals of the time, for example those believed to have written the *Traibhumikatha,* show much *ad hoc* use of tone - mark type diacritics to solve problems which arose in the course of composition of a particular inscription, apparently in some cases without reference to what someone else had written on another

inscription some years earlier. This is what would be expected as a writing system evolves.

Interestingly Gedney's suggestions about writers working concurrently in many places on the same problem of devising Thai script(s) implies traditions of writing Thai much earlier in some places. It is impossible to accept that suddenly, around 1300 writers in different Thai areas all over Southeast Asia, without reference to one another, suddenly all started trying to write Thai.

Furthermore, as Coedès insisted many years ago, if clear distinctions were still being made between high and low consonants, such as the voiceless/voiced distinction, Thais would have seen no need for tone marks, being perfectly capable of understanding from context what the reading should be. Thus a full tone mark system such as we have today, would only have arisen after, not only the 3 > 6 tone split which is hypothesized, but after further mergers which erased some of the tone distinctions related to opposition between high and low consonants. Otherwise it would have represented pedants playing games.

I am pleased, however, that Gedney (15) has lent his authority to my view, against Coedès, that tone marks were not invented to help non-Thais read Thai. As Gedney says, it is "most probable that tone marks, where used, were intended by the scribes to mark accurately the sound distinctions of their own speech," without thought for the problems of foreigners.

Now, as Thai scholars began to think of formulating a complete tone mark system, whether required for understanding by themselves, or as pedantry, and based on earlier incomplete tentative efforts (as suggested both by me in relation to the 14th century inscriptions, and by Gedney in "Comments"), what is it they would have seen necessary to mark? The speech distinctions that had to be indicated in writing were horizontal (with respect to the linguists' diagrams of tone categories and initial consonants), that is between, for example, syllables *khā*, *khā*, *khā* distinctions which require two marks, with one term left unmarked. This type and number of distinction was true for each

class of initial consonant, and for every Thai language, and could even have been used in Proto-Tai to mark the allotones hypothetically associated with the original consonants. Two marks originally used for *ad hoc* marking were at hand, those now represented by *mai-ek* and *mai-tho,* the latter clearly a cursive script evolution from the cross seen in early Sukhothai. The problem for the Thai scholars in question was to practically distinguish the three - fold horizontal tone differences in each consonant row, *not necessarily* to devise a system fully consistent for the entire consonant - cum - tone gamut of the language. Thus in each row of unmarked syllables, *mai-ek,* and *mai-tho* were established as signs for the triple distinction *in that row,* with the result of course, and in any language, that between rows the same sign did not always indicate the same tone.

The Content of RK

Very little of the above gets to the meat of the RK problem, which is its content, in terms of its story, relationships to other texts, isolated words, and even individual letters. As I said above, it must be examined for language which is in disagreement with Sukhothai language as seen in other Sukhothai inscriptions, and the passages which parallel sections of other Sukhothai inscriptions must be studied to determine which have priority.

What is required is continuing study of the type of detail discussed by Dr. Piriya Krairiksh in his "Towards a Revised History of Sukhothai Art: A Reassessment of the Inscription of King Ram Khamhaeng" (Siam Society 16 August 1988), though with more emphasis on comparison of RK with Sukhothai and less with Ayutthaya. For lack of time I shall not refer again to Dr. Piriya's work here, but this in no way implies lack of appreciation.

My own work on this has only begun, and, at the risk of telegraphing my punches, in the interest of scholarly collaboration, I present below some of what I imagine I have found.

Vocabulary

The RK inscription contains a number of unusual vocabulary items and spellings which should be compared with the rest of the Sukhothai corpus. I shall first consider some of them in order as they occur in the text, without regard for other contextual matters. RK contexts will be indicated by Face and line, as I/1, Face I, line 1.

- I/2 and 3, the pronouns *tu*: and *phöa*. I had written this section before receiving Gedney's "Comments," and I reproduce what I wrote, adding a response to Gedney afterward.

It would seem that these were the pronouns to which Gedney was referring in his remark in the 1987 *Pācāryasār* interview that the use of "old pronouns [which] people in the time of the Fourth Reign would have no way to know" show that RK is genuine. Well, they did know them well enough in the Fourth Reign to read them as pronouns; but whether they understood their Sukhothai usage is another matter.

According to Bradley, followed by Coedès, Griswold and Prasert, *tu:* is an "obsolete" first person plural exclusive pronoun; and this is also the view of modern linguists. Bradley said *phöa* was "the well - known sentimental first personal pronoun of the romances," a definition left unmodified by his followers. It is not listed in Li.

Bradley treated it as first person singular, and if so, comment is required on RK's use of *ku*: for first person singular other than in the two occurrences of *phöa* in lines 2 and 3. Coedès did not comment on it in his 'Critical Notes,' but in his translation of the entire inscription he implicitly treated it as plural, although his translation here is a paraphrase which obscures rather than illuminates the point at issue; and in his case comparative comment with *tu*: is required. Griswold and Prasert treated it as singular. Indeed the context is such that either interpretation makes sense, but for what follows here it is important to recall what that context is: "there were five of us born from the same womb: three boys and two girls. My/our *[phöa]* eldest brother died...." Thus if plural was intended it referred to the speaker

and three others.

Now the most recent remark by a linguist specialist, Gedney's student Robert Bickner, refers to "the now archaic pronoun *tuu* 'I,' and "the first person dual pronoun *phöa* 'we two' (Bickner, *op. cit.* above, 141, 160). These remarks, moreover, were simple asides, without any detailed discussion, as though the matter were well known to all linguists.

The usage of *phöa* evoked by Bickner is also attested in Sukhothai inscriptions other than RK, and in clearer contexts.

Inscription No. 95, which Griswold and Prasert assign to the period 1398 - 1410 (EHS 22, pp. 68 - 73), contains both words in comparable contexts, which I list with the translations from EHS.

line 5: *phöa* phvá mia, "we...man and wife"
lines 6 - *7: phöa* [ ] khau bimb, "we made...votive tablets
line 8: *phöa* dáṅ sòṅ, "the two of us"
line 9: *phöa* khau[2] pia, "(the two of us) contributed... cowries"
lines 9 - 10: *tu*: ba mee lūk"We, the father, mother and children"

In the last phrase *tu*:, contrary to Bickner, indeed fits the definition of first person plural exclusive; but *phöa* in all four contexts is used strictly as a dual.

Another example of *tu*: is in No. 49 dated 1417. Its line 34 contains *tu: khā dān dáṅ hlāy,* "all of us," clearly plural rather than dual, and possibly exclusive, though the context does not make that certain.

Inscription No. 14, dated 1536, line 10, has

sāṅ puñ pai kee *phöa* // //svan kū sai..., which in EHS 15 is translated, "...so that we shall receive the merit...As for me..." (EHS 15, pp. 141 - 2).

More literally, the first part should be "transmit merit to us," and the context shows that two persons are concerned, a man and wife who have assigned their adopted daughter to the temple in order to obtain merit. Then the kū is clearly singular, refer-

ring to the husband, who asks for "the prerequisites of Buddha hood".

It must be accepted that in later Sukhothai usage, at least, and thus implicit for the RK period, *phöa* was first person dual, contrasting both with *tū* and with *kū;* and the usage of RK, lines 1 - 3, is both internally inconsistent and contrary to Sukhothai practice.

As we see now in Gedney's "Comments," this is not his conclusion. On p. 13, commenting on interesting points skipped over by Dr. Piriya, Gedney says "The pronoun *phia* [my *phöa*] involves an even nicer subtlety...it means 'we,' dual, exclusive, that is, 'he or she and I, but not you.' (Fn. reference to David Strecker's dissertation.)...[i]n Inscription One these two pronouns are used in exactly the right meanings."

I have not seen Strecker's dissertation, but I now await eagerly a chance to consult it, in order to understand how, in the passage, "we uterine siblings five persons males three females two elder brother ours *[phöa]* eldest died from us *[phöa],*" the term *phöa* may be considered dual, since after the death of the eldest it refers to two more brothers and two sisters. Of course if three can mean one, as we saw above, why can't four mean two, but until I see a fuller explanation, I maintain my conclusion about the un-Sukhothai quality of RK in this detail.

A second detail is RK I/2, *phu: ñiṅ soṅ,* "two girls." The overall 'Lao' character of RK evoked by Bradley would lead us to expect *sāv* for 'girl,' and this is indeed Sukhothai usage in other comparable contexts. No. 2, II/2 has *lūk sāv sòṅ gan,* "two daughters," and No. 11, II/5 also *sāv sòṅ gan.*

Another RK problem is in the same context, 'two' spelled *soṅ*, rather than *sòṅ*, as in modern Thai, *and in the rest of the Sukhothai corpus.* This same way of writing medial /o/ (ใ) occurs in RK for *o*k (òk), and no*ṅ* (nòṅ), although Faces III and IV also have the same words spelled as in standard Thai.

Besides the contexts of Nos. 2 and 11, no. 5, II/19 also has *sòṅ.* The spelling *o* (ใ), where *ò* (ใ่) might be expected is found

in No. 107 in th*o*:, 'hare [year]', but this is a Mon-Khmer word, like No. 3, II/27, kar*o*m, 'below', still written with *o* in modern Thai.

It should be observed that the vowel symbol *o* (ใ), whether at Sukhothai, or in modern Thai, is most often used in words of Indic or Khmer origin. It is not required for Thai phonetics, nor was it ever adopted widely as a convention. The origin of the symbol, graphically, is from the เ x า symbol as used in Khmer. When original voiced initial stops in Khmer devoiced, a second set of vowels developed, and original vowel symbols then had two pronunciations, one following first series consonants (original unvoiced), and one following the series of original voiced consonants. For the symbol เ x า this meant /ò/ following *k,* but *o* following *g*. When Thai came in contact with Khmer and took over some Khmer script conventions, it was necessary to divide the vowel symbol เ x า into two for Thai, even when writing Khmer terms, because the consonant conventions were different. Even within Khmer, spelling conventions differed. Lithai's No. 4, for example, uses vowel *o* in several words where it would not be used today, possibly reflecting the influence of the writer's native Thai conventions; but this still requires study.

The RK usage is aberrant in terms of general Thai, and more importantly it is non-Sukhothai. As Li wrote (261), "...it is the [Siamese] words written with short *e* and *o* that have correspondences in other languages, rarely words with long *ee* and *oo*(ใ), which may be suspected to be borrowings or secondary developments in Siamese and perhaps in some other SW dialects as well;" and Gedney, "Verse forms," p. 11, "Siamese words with the long vowel *oo*...are normally not native inherited words."

The first example of still another RK anomaly appears in line I/3, the use of retroflex/alveolar dental symbols on purely Thai words where they are neither required nor helpful, and in a manner not found elsewhere in Thai epigraphy or literature.

The examples in RK and their contexts are:

*ṭ*ee' for *t*ee', I/3, II/17, II/30, but *t*ee' in IV/14
*ṭ*ò' for *t*ò, I/7

ṭaṅṅ for *táṅ*, I/17, II/7, 23, 35
ṭaṅṅ + for *táṅ* in III/12, but *taṅṅ* + in IV/7
ṭeeṅ' for *teeṅ'* in III/19

There is one occurrence of correct use of retroflex symbols, for *aṭṭhāraś,* '18;' but unexpectedly the one word which is always written with a correct retroflex in other Sukhothai inscriptions, 'tripitaka,' is written incorrectly in RK, *pitakatrai* (II/29)) instead of *piṭa*/*piṭakatrai*. In both aspects RK contrasts with the Sukhothai corpus.

When the Thai first adapted Indic writing systems, the fourth symbol of each Indic consonant group, that for voiced aspirated stops, was reserved for writing Indic words, leaving three symbols, unaspirated and aspirated unvoiced, and unaspirated voiced, for the three sounds corresponding to those categories in Proto - Thai. This was insufficient for the labial and dental series in which there was a fourth Thai sound, originally preglottalized, now pronounced /b/ and /d/. Either new symbols had to be devised, or redundant symbols had to be put to new uses. Schematically the problem was:

Thai sounds: * ʔd > d, *t > t, *th > th, *d > th (PH)/ > t (P).
Indic signs *t* *th* *d*

The adaptations were not the same in all Thai languages. In the Sukhothai script a new symbol ฏ, *t*, was derived from t ฏ; but in Black Tai the new symbol was derived from that for *d > t. What seems peculiar, considering only the Thai languages, is that the new symbols, in both Sukhothai and Black Tai were given to the original *t > t, which also corresponds to the original Indic *t* symbol, while the Thai reflexes of that Indic symbol represent the non-Indic Thai sound of *ʔd > d. In Tham script the retroflex *ḍ* > *ṭ* /d/.

The explanation probably lies in Khmer influence on Thai writing, even if the original adoption of an Indic script by Thais was not from Khmer. Thus in Khmer writing the Indic *t* symbol represented both /t/ and /d/, the difference sometimes non-phonemic, conditioned by environment. This was still the case

in Sukhothai No. 4; and no distinction was made between *t* ฎ and t ฎ when Thai was written in Khmer script. Thus the first writers of Thai script, perhaps quite arbibrarily, assigned one of the values of *t* as known to them (/d/ <*?d) to that Indic character, and invented new letters for the other value, which happened to be *t > t. The Black Tai adaptation shows that this was done after the devoicing of original voiced initials, when in that language *t and *d had coalesced as /t/. Two different symbols were still required, however, to indicate tonal distinctions. This is good evidence that Black Tai script did not evolve from Sukhothai, whether of the RK variety, or true Sukhothai.

The Khmer had a similar problem. Their original voiced initial stops devoiced, and the vowels doubled to take up the old distinctions, leaving the old consonant symbols in their places governing vowel distinctions. Thus:

vowel series /ò/ vowel series /o/
t > t th > th d > t dh > th

There was still /d/ < *?d in Khmer, however, which had traditionally been written with *t,* but eventually as the Khmer refined their script they devised a way to represent it separately, and chose the first symbol of the redundant Indic retroflex series to represent /d/, particularly initially. This had not yet occurred in the Sukhothai period, and it had no influence on the development of the Thai script.

The aberrant use of the retroflex symbols in RK seems to reflect influence from this later development in Khmer; and in fact such use of the retroflexes, as occurred in Thai, would have been one way of solving the problem which the dental series originally posed. By the Sukhothai period, however, as is seen in RK itself, if it is accepted as genuine, the problem had been solved the other way, by inventing new symbols (i.e ฏ). The writer of RK then, and this is good evidence that it is a post-Sukhothai work, seems to have been aware of the later Khmer convention, which apparently postdates 1747, and thinking it was more ancient than it was, he attempted to give RK a spurious flavor of antiquity by inserting the retroflexes in this manner.[33]

By the time epilogue 2 on face IV was composed that fantasy was given up.

This use of the retroflexes in RK shows the error of Marvin Brown's hypotheses about Thai adaptation of some features of Indic script. Brown assumed that 'Ram Khamhaeng' invented his Thai script while thinking of Indic writing with Sanskrit values, and "where Thai had sounds that Sanskrit didn't he either made up new letters...or combined old ones..." (Brown, "Historical Explanations for the Peculiarities of the Thai Writing System," p. 5). Thai also invented a 5th retroflex symbol because the original Indic *ṭ* symbol was sometimes voiced, sometimes unvoiced in Khmer/Thai usage.

Brown (p. 10) treated the dental problem entirely in terms of the relationship between Sanskrit and Thai. "[S]ome words with *t* and *p* in Sanskrit are pronounced *d* and *b*," and that is why a new letter had to be devised in each of those series. The same was true for the retroflex series. Some "words with *ṭ* in Sanskrit are pronounced *t* in Thai (and written with ฏ) and some are pronounced *d*," for which a new letter, ฎ, with "no source in either Sukhothai or Sanskrit" had to be invented. Of course Brown recognizes that graphically the evolution was the other way ฏ > ต and ฎ > ฏ (Brown, p. 17.).

As I have explained above, the invention of a new dental series character had nothing to do with Sanskrit, but was required because of the way Thais had originally adapted Indic scripts for the strictly Thai vocabulary. In the retroflex series, as Brown recognizes, priority belongs with the symbol pronounced /d/, although found in words written with *ṭ* pronounced in Indic /*ṭ*/. Moreover RK, together with its surfeit of retroflexes where they do not belong, is deficient where they are required, and in particular in a context which is almost always written correctly in other Sukhothai inscriptions, 'tripitaka.' Sukhothai did not take over the complete, perfect Sanskrit system, even in RK. Indic words were written haphazardly, with more care given to some particularly sacred terms than to others; and here also Khmer influence is apparent. The terms which Sukhothai considered most important, and which were spelled with the most

care, were those in which original Sanskrit /ṭ/, written with the first consonant of the retroflex series, had come to be pronounced in Khmer /d/, particularly *piṭak* /beidak/. Thus in Thai the first retroflex symbol, because of this Khmer - conditioned usage, was assigned the sound /d/, and when Indic orthography was more carefully studied and completely noted in Thai, a second symbol, based on original Indic *ṭ,* was devised for those contexts in which original Sanskrit *ṭ* was still pronounced /t/. The process was the opposite of what Brown inferred.

The Expression 'this Sukhothai'

As I noted above, an argument used by one of the purist readers of RK, Dr. Prasert na Nagara, for the invention of Thai writing by Ram Khamhaeng, is the occurrence 14 times of *möan sukhodai nī+*. 'This' can have only been a sort of definite article or emphatic particle, which proves that Ram Khamhaeng's claim to have invented *lai sü dai nī+* means he invented *the* Thai script, not *this* Thai script in contrast to some other Thai script *(Silpa-vathanatham,* p. 41).

This seems to mean that for Thai scholars there is an anomaly in the RK usage of *nī+,* with respect to modern Thai, which given the time difference is not surprising; and as I said above, I think Dr. Prasert's reading and interpretation of this point must be accepted.

There is another anomaly, though, not given attention by Dr. Prasert — this usage of *nī+* with *'sukhodai'* is not found in the other Sukhothai Thai - language inscriptions. In No. 2 there are at least 9 occurrences, in No. 3, 2, in No. 5, 2.

But, in No. 4, in Khmer, we find *sukhodaya neḥ,* lines I/9, II/16, II/22, II/32, IV/16.

This is perhaps one of the most important pieces of evidence for late composition of RK based on readings of other Sukhothai inscriptions. It seems to have copied a feature of Khmer usage in No. 4 which is not found in parallel phrases in other Thai inscriptions. Inscription No. 4, we will recall was 'discovered' at

the same time as RK.

Expression for 'The People'

A set of terms which may be compared with numerous contexts within RK and in other inscriptions and non - epigraphic writings is that which indicated 'the population,' 'the people.' Within RK there are three variants occurring, with the translations of EHS 9, as follows:

line

6	brai' pha: + hna: + saï	"men," "commoners;"
21	brai' fa: + hna: + saï	"men," "commoners;"
23	brai' pha: + kha: + dai	"retainers,"
32	brai' pha: + hna: + pak	"commoner"

Before commenting on these terms, the following comparable contexts should be listed:

source:	inscription number/face. line
3/2.32	brai fa: kha: dai
2.43	
5/1.16	brai fa: kha: dai
38/1.15	brai fa:
106/30	brai fa: kah: gan
45/2.3	brai dai jāṅ mā+ kha: + dai
102/1.6	...jān + mā+ kha:+ dai

Traibhumikatha

	brai' va:^{2} kha:^{2} dai
Laws	brai vā khā gan/dai/pheentin *(Three Seals)*
modern	brai' va^2 kha^2 pheentin

None of these expressions has ever been fully elucidated, although there has always been agreement that they mean in general 'the common people,' with the components glossed as *brai '* = non - slave service population; *fa:/va:/pha:* = sky, meaning "the ruler," *kha*: = slave, *dai* = free population.

As I noted in Canberra, RK writes the second term *pha:+*, while other Sukhothai inscriptions, in this expression alone, have *fa:*, while writing 'sky' in other contexts as *va:/va:²*, as in correct modern orthography. The last is also the orthography in *Traibhumi*. All but Bradley ignored these oddities. As I suggested in my Canberra paper, the anomalous *fa;+* is probably part of a set expression dating from a time when Thai script had only one symbol for /f/. The variant *pha:+* in three of the four contexts of RK, is not an anomaly in the same sense; it is the usual Ahom and Shan word for 'sky,' a regular correspondence with SWT *va:²* (Li, p. 79).

Bradley (p. 41) simply called *brai' pha+* "the antique from" of *brai' va:²*, and all of the variants as evidence of "a stock expression in rhetorical or poetic style," "ballad-forms" with "their capacity for impromptu variation;" and this "conventional character" is what accounts for the "obscurity of meaning attaching to one or more of their members." Coedès, however, realized that old Thai institutions, not poetic licence, were in question, saying "si l'on connaissait mieux l'organisation sociale des Thais de Sukhodaya, on serait sans doute capable de faire une distinction très nette entre *brai' fa:+* au visage brillant et *brai' fa+* esclave des Thais" (Coedès, "Notes critiques", pp. 5-6). Griswold and Prasert also saw institutions, and felt that within RK the group *kha:+ dai* was inferior to the group *hna:+ saï* (EHS 9, p. 207, n. 32).

Although the wide occurrence of *brai fa/va* and *kha dai*, both separately and together, leaves no doubt that the terms were institutional, designating one or more categories of commoners, the RK versions with *hna saï* and *hna pak*, 'clear' and 'covered' faces, which are found nowhere else, probably are, as Bradley thought, examples of poetic variation, but they are evidence that RK does not belong in the Sukhothai tradition. The use of *pha:+* for *fa:/va:* points in the same direction, an attempt to provide a false air of antiquity through choice of exotic terms.

In his latest "Comments" Gedney (16) has suggested that the term *fā* in the expressions discussed above should mean 'cloud,' not 'sky', although he adds that 'cloud serfs' makes no more sense

than 'sky serfs.' The trouble with this is that the evolution of the term through Sukhothai, Ayutthaya, and Ratanakosin texts indicates that for Thais in those communities it always meant 'sky.' What 'sky serfs' really were is a matter for institutional history, not linguistics alone, and the term will only be explained when old Thai institutions, all over the Thai area, have been given more attention. At least we know that in the mythology of Laos and the Tai peoples of northern Vietnam, 'sky,' 'beyond the sky,' 'below the sky,' were key concepts in their cosmology and in origin stories relating to their political culture (for example King 'Fa Ngum').

In his discussion of these expressions Coedès (p. 6) added that there was difficulty in taking *brai fā khā dai* as 'slaves', since "it is the word *pua* which in the inscription seems to have this sense." He is referring to Face I, lines 15 and 29, relating respectively to Ram Khamhaeng's raids on villages *("tai pua tai nāṅ")* and his treatment of someone who comes to him without "elephants, horses, *pua, na:ṅ,* silver, gold," which Bradley (pp.25-26) translated "got slaves, got damsels", and "no elephants, no horses, no slaves, no damsels, no silver, no gold;" and which Coedès, in his complete translation some years later, continued to render, "des garçons, des filles," and "ni éléphants, ni chevaux, ni serviteurs, ni femmes, ni argent, ni or" (*Receuil,* pp. 44-45). A third occurrence of *pua* is Face II, line 10, in a list of several categories of people who celebrate the Kathin. Bradley (27) translated it here as "servants," and Coedès (45) "les hommes."

In their work Griswold and Prasert translate all three occurrences of *pua,* with its accompanying paired term, *na:ṅ,* as "young men or women of rank," admitting that it is "conjectural" (EHS p. 205, n. 19). They justify this on the basis of the coupling of *pua* with *nāṅ,* which elsewhere in RK, as well as in dictionaries of Lao and Dioi, means 'Lady,' or 'woman of the nobility,' in Dioi being the female equivalent of *Kouang,* an honorific given to the sons of seigneurs. Condominas, op. cit., also cites *nang* as female nobility, but among his Black Tai *kuang,* in the expression *kuang nyok* are 'serfs of the interior,' while *pua* can definitely not be paired with *nang.* As Dioi is a northern Tai

language, its *Kouang* may not ipso facto be equated with Condominas' *kuang nyok,* particularly when the latter's information is so vague. William Gedney has suggested, on the basis of Black and Red Tai, that *pua* and *nang* should mean 'king' and 'queen,' but as Griswold and Prasert correctly indicated, this would only fit one of the three RK contexts (Gedney, "White, Black and Red Tai," 1964, p. 47.).

Since Gedney's explanation will not fit all three contexts, and since none of the Tai languages justify social equation of *nāṅ* and *pua,* but all agree on a high status for *nāṅ,* the justification for "young men or women of rank" is inadequate; and *pua* should still be rendered 'servant,' as Bradley did, and which fits all contexts easily.

Of course this is awkward given the pairing with *nāṅ,* unless the latter term is taken as the types of *nāṅ* who were palace serving women at the court of Ratanakosin, perhaps also of Ayutthaya, as Bradley's translation, 'damsel,' implies; and RK again shows evidence of language which may be unrepresentative of Sukhothai.

One more term of interest is face I, lines 19 - 20, *böan,* third person pronoun, in the version of EHS 9, p. 206, n. 23, "'friends,' here used as a pronoun of the third person plural, in Tai Yuan เพื่อน is still used as a pronoun of the first and third person".

This term is unique in this context, not only within RK, but in the entire Sukhothai corpus. Moreover, since it does seem to be a third person pronoun, it is probably not 'friend.' In Tai Yuan and Lao there is a distinction between *phön,* 'he,' 'they,' and *phöan,* 'friend;' and the third person pronoun /phön/ is also found in some varieties of Lue, as well as in Tai Nuea, a language related to Lue, White Tai and Shan.[34] In RK the vowel *ö* is several times written *öa,* as in *ṅöan,* 'silver' (Face I, line 21). The author probably intended *phön,* to give a Lao effect, but adopted the peculiar convention for that vowel.

This is as far as I can take discussion of the content of RK on this occasion. As you see, we have hardly gotten into Face I, treating only matters of vocabulary and script. Whatever the

consensus on RK authenticity, there is still much ink to be spilled in explanation of all its details.

In Conclusion

I hope that these hastily written comments have at least demonstrated the inconsistencies in everything related to RK. The most remarkable are those in work on Thai historical linguistics. While insisting that linguistics is based on spoken languages, those linguists who have kept an eye on RK seem to have skewed their work to fit it in, and those who have really followed the linguists' canon have come up with results which inferentially cast doubt on RK, or at least on the reconstructions of other linguists. It seems almost that if RK is to be upheld, much of what has been accepted as the standard view of Thai linguistic history must be thrown out; and if the basic premises of Thai linguistic history are maintained, then RK may only be upheld by more or less fragile epicyclical rationalizations.

There are a few more points in Gedney's paper, beyond what has been noted above, which merit some comment, both with respect to their content in relation to RK and to the form of their argument.

First, in intellectual controversy, the concept 'wrong - headed' (Gedney, "Comments", p. 9) is entirely out of place. Arguments, inferences, conclusions, may be *wrong,* just as they may also be right, partially right, dubious, or unproven, but they are not wrong - headed, unless that term were used to mean that the argument in question had ignored the standard ways of gathering and producing evidence. This was obviously not what Gedney was saying, for what he called wrong-headed was also, in his own words "thoroughly researched and clearly argued." An accusation of wrong-headedness is usually a signal that intellectual censorship is at work.

Some additional points of detail:

In support of my position on P/PH above I quoted Gedney (3) on changes in the voicing of sets of consonants and the effects

on tones. There I omitted part of Gedney's statement in order to bring it into line with the other linguists to whom I was referring. The complete citation is, "[a]nd drastic changes also occurred in these initial consonants. In some places previously voiced sets of consonants became voiceless, *or vice versa* [emphasis mine — MV]. In fact it was these consonant changes that brought about the new tone systems...." Now the "vice versa," meaning here previously voiceless sets of consonants which became voiced, is a red herring. The consonants concerned are modern voiced /b/ and /d/, which have developed from what are called preglottalized or implosive stops * b and * d, or the consonants such as once voiceless *hm, now voiced /m/, although still written *hm,* used by Gedney in one of his illustrations. These, however, *are not* the consonants whose changes are deemed by linguists to have set off the chain of tone splits. The consonants involved in the causation of tone splits are the original *voiced stops* *b, *d, *g, which in all Thai languages have devoiced to either /p/, /t/, /k/, or /ph/, /th/, /kh/. But by putting in "vice - versa," and in his examples emphasizing pairs like *hmā / mā,* Gedney is able to spring the equivalency of R/H and P/PH onto the unwary.

In another context (pp. 11 - 12) Gedney wrote "[s]ometimes this strange argument is advanced to discount identification that past scholars have made of puzzling archaic Sukhothai words *with words found in other Tai languages." Here he should have* cited an instance, for so far as I can determine, my attempts to disagree with other scholars about puzzling words were not connected to my remarks about Brown's family tree. Gedney then continues, "[t]he arrogance of this is revolting...apparently only their own appalling methods are acceptable...those of such giants of the past as Coedès are questioned."

Is Gedney saying that scholars should never question earlier work? If so, I certainly disagree. Progress in any field of knowledge only comes through doubting what predecessors have said. Such questioning can only seem 'revolting arrogance' to those reacting in panic to possibly valid new interpretations. Gedney's descent into gratuitous insult and personal invective is indeed a panic reaction, for if our methods and reasoning were as appalling

as he claims, he should have been able to demolish them with a few examples of clear intellectual exposition.

As for Coedès, he never hesitated to criticize the work of his predecessors, nor, as testified by his former students and colleagues, did he ever try to play God and forbid his followers from questioning his work. On the contrary, all who knew him say he encouraged independent thought and willingly discussed differences of opinion with his juniors, some of whom now, in France, are almost totally dismantling Coedès' reconstructions of early Khmer history, in a spirit which Coedès would have approved.

In the present context I believe, without reexamining everything I have written earlier on RK, that I have more often cited Coedès positively than critically, while Gedney, just in the short paper presented here, has gone against Coedès on four points connected with RK — according to Coedès (1) RK was not the first Thai script, (2) RK tone marks were invented for non-Thai readers (see Gedney p. 15, (3) Sukhothai language had already split into six tones, and in this respect resembled modern "le laotien, le taï-noir, le taï-blanc, et le dioi", and (4) the initial *h* of words like *hma* was strictly an orthographic convention to indicate tone and not a sign of ancient pronunciation (see Gedney, pp. 3, 5). Is Gedney claiming that it is all right for him to doubt Coedès, and on these matters for which there is as yet no certain proof, but that others must never doubt Coedès — until perhaps Gedney says it is o.k. to do so? (See J. Burnay and G. Coedès, "The Origins of the Sukhodaya Script" and "Note sur les tons et les initiales du vieux Siamois à l'époque de Sukhodaya," in *JSS* XXI/2 (1927); quotation from "Note," p. 105).

Finally, "to turn for a moment to non-linguistic matters" (p. 17), the question of whether King Mongkut composed RK (which I emphasize again is not part of my argument) cannot be settled by ruminations about his living quarters as a monk or his lifestyle, or his personal honor; and for a historian family gossip by an awe-struck granddaughter about a great man of the past has strictly speaking all the historical evidential value of George Washington's cherry tree.

Moreover, *pace* Gedney, and as I noted briefly above, there is an oral tradition in Thailand that King Mongkut was responsible for RK, and that is the reason that the controversy has attracted some of the sympathetic interest there that has been manifested in recent publications.

Whether King Mongkut, or some of his contemporaries, or some of King Narai's intellectuals or a group of monks at Sukhothai, composed Inscription I, there would have been no need for secrecy, and it would not have been viewed then as a dishonorable accomplishment. If RK is a post-Sukhothai composition, as I believe, it represents a *Tamnan* on stone, a perfectly respectable genre, but one about which historians today are extremely skeptical as a repository of accurate information about the past. There has been a prejudice, ever since Coedès at least, that everything on stone is somehow particularly worthy of credence, yet we have examples of *Tamnan* on stone in the first 6 lines of Inscription No. 76 of Wat Chiang Man in Chiang Mai, and I believe in the ancestral part of Inscription No. 2, as I commented in "Guide". Inscription No. 1 may well have been designed, whenever it was written, as a composition equivalent to the *Phongsawadan Nöa,* and who is to say that its author(s) did not have the intention to continue it on other stones to bring the story up to date?

RK is still a fascinating historical and linguistic subject meriting full discussion. It is definitely not a subject to be dropped (Gedney p. 18), least of all on the orders of someone who merely disapproves. It is particularly inappropriate for Gedney, whose professional career once suffered because he was accused of taking devilishly maverick positions and misleading young Thais, to use the same heavy methods against his professional juniors. His experience should have placed him above using that technique against people whose opposing views are entirely in the intellectual realm.

At the moment the ball is still in the court of the defenders of RK. Let them play it fairly, without whining 'foul' or demanding a new umpire. Above all let us keep the discussion on an intellectual plane, and not let it degenerate into a slanging match between, to adopt Gedney's terms and tone, Young Turks and Old

Turkeys.

Additional Note to the Piltdown Papers

I have found that on two points in my Piltdown Papers, I misrepresented positions held by fellow students of RK and I wish to correct those misstatements now.

In both papers I failed to distinguish clearly between the views of Marvin Brown and James Chamberlain on the relationships between Old Sukhothai language and other branches of Southwestern Thai, particularly the southern branch; and my statement in Piltdown 2, p. 14, that "Marvin Brown, followed by Gedney, and accepted by most other...linguists, including James Chamberlain, has argued that the southern dialects were direct descendants of Old Sukhothai language...," gives the impression that Brown and Chamberlain hold identical positions.

More precisely, however, Brown argues that all the southern dialects, which he divides into two branches, (1) Tak Bai, and (2) all the other southern dialects, developed from Old Sukhothai, which is a branch of Thai distinct from the Lao languages and from Ayutthaya - Bangkok Thai.

Chamberlain, on the other hand, in his analyses of modern Thai languages, has taken the position that Lao (Luang Prabang, Vientiane, Sisaket, etc) and Southern Thai constitute one branch distinct from 'Siamese' (Ayutthaya - Bangkok), Phu Tai, Neua, Phuan, etc. A crucial difference in Brown's and Chamberlain's analyses of Southern Thai is that Chamberlain excludes Tak Bai *from Southern Thai and considers it a late intrusion. Neverthe*less, Chamberlain has accepted Old Sukhothai as an ancestor of Southern Thai, minus Tak Bai.

The major difference, then, is that for Brown Old Sukhothai was distinct from Lao, while Chamberlain treats them as close relatives. They agree, along with most linguists, that Sukhothai was not the ancestor of 'Siamese' /Ayutthaya - Bangkok Thai.

In a personal letter B.J. Terwiel has taken me to task for

my statement in "Piltdown Skull — Installment 2," note 31 below, attributing to him the view that "the Ahom script was developed from old Mon after that Thai group reached Assam." In fact Terwiel's view, expressed in his "Ahom and the Study of Early Tai Society," Paper submitted to the Second Thai-European Research Seminar, 1982, p. 2, is "...when the Ahom conquered a small corner of the Brahmaputra Valley at the beginning of the thirteenth century, they probably brought with them their own script, apparently based upon a Mon example," which he slightly modified in his footnote 1 to "Although it is possible that the script was introduced at some time between the thirteenth and fifteenth century, the available evidence suggests that the Ahom adopted their script just prior to the thirteenth century." Terwiel does not state what the 'available evidence' is, which is intriguing, for the oldest extant Ahom document dates from the late 15th or early 16th century (Terwiel, "Ahom Documents, How to read them, and to what extent can they help us in comparative Tai studies," Paper prepared for the Thai Historical Associations's Annual Conference, Bangkok, Kasetsart University, 29 - 30 January 1989).

Notes

* This paper was originally written for the participants in the Ram Khamhaeng panel at the AAS Conference in March 1989, and because of the haste with which I had to prepare it I neglected a number of references which were not necessary for the small group for which it was destined, but which the general reader requires. Thus this second printing includes additional footnotes, all those marked 'a' and 'b', except for '5a' which was in the original.

1. See interview with M.R. Sukhumbhand Paribatra, *Khao phiset* 1 - 7 Feb 1989; and reference to it in *The Nation,* by Kavi Chongkittavorn, 27 Jan 1989.

1a. Anthony V.N. Diller, "Consonant Mergers and Inscription One," *JSS* 76 (1988), pp. 46 - 63. Diller kindly gave me a copy in advance, and attention should be called to the inaccuracies in White Tai script on p. 63 of the *JSS* version. My Canberra paper was "The Ram Khamhaeng Inscription: A Piltdown Skull of Southeast Asian History?," published in *Proceedings of the International Conference on Thai Studies,* The Australian National University, Canberra 3 - 6 July 1987, Volume 1, pp. 191 - 211.

2. Vickery, "A Guide Through Some Recent Sukhothai Historiography," *JSS* 66/2 (July 1987), pp. 182 - 246. See *Silpavathanatham,* special number on the RK inscription, 1988, pp. 145. All further references to *Silpavathanatham* are to this issue.

3. Sulak Sivaraksa, *Silpavathanatham,* p. 147, quoting from an interview with William Gedney in *Pacaryasar,* December 1987.

3a. In *Silpavathanatham,* p. 46, Dr. Prasert na Nagara cited the examples of King Mongkut's 'Aruyaka' script, and another reform by King Vajiravudh as conscious efforts at innovation resulting from foreign influences.

4. Dr. Prasert na Nagara, *Silpavathanatham,* p. 41.

5. *Silpavathanatham,* pp. 142 - 145.

5a. I regret that because of lack of time and space I will not be able to comment in this paper on all the points made in *Silpavathanatham.* I wish to thank the contributors to that volume for the very fair way in which they made my RK work known to Thai readers, and for the high standards of intellectual discourse which were maintained.

6. The details are in my Ph.D. dissertation, "Cambodia after Angkor, The Chronicular Evidence for the 14th-16th Centuries;" the main points of which were summarized in "The Composition and Transmission of the Ayudhyan and Cambodian Chronicles." *Perceptions of the Past in Southeast Asia.* See also Vickery, "The 2/K. 125 Fragment, A Lost Chronicle of Ayutthaya," *JSS* 65/1 (Jan 1977).

7. See Charnvit Kasetsiri, *The Rise of Ayudhya;* and Vickery, "A New *Tamnan* about Ayudhya," *JSS* 67/2 (July 1979).

8. David K. Wyatt, *Thailand: A Short History,* p. 65.

9. The *'kamboja'/'kambujua'* problem is discussed in Vickery, "Cambodia after Angkor," pp. 369 - 377.

10. See Vickery, "The 2/K.125 Fragment,"

11. See Jeremias Van Vliet, *The Short History of the Kings of Siam,* ed. by David K. Wyatt; and Vickery, review of same in *JSS* 64/2 (July 1976).

12. See his "Explanatory Remarks in Regard to the Period of Siamese History Antecedent to the Founding of Ayuddhya," *JSS* 1914, pp. 40 - 42.

12a. A beginning in Phra Ruang research is 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,* ed. by Robert J. Bickner, Thomas J. Hudak, and Patcharin Peysantiwong (Ann Arbor: Michigan Papers on South and Southeast Asia, Center for South and Southeast Asian Studies, The University of Michigan, Number 25), pp. 57 – 90.

13. Coedès, *États hindouisés* (1964), p. 375.

14. La Loubère, *The Kingdom of Siam,* OUP, 1986, p. 6. The same tradition is reported by Persian envoys in *The Ship of Sulaiman,* trans. by John O'Kane.

15. Charnvit, *The Rise of Ayudhya,* pp. 24, 84.

16. Wyatt, *Thailand,* pp. 54 - 55.

17. Griswold and Prasert, EHS 9, p. 218, n. 128.

18. Wyatt, "The Crystal Sands, The Chronicles of Nagara Sri Dharrmaraja," pp. 86 - 87. See my comment on 'Saiya-narang,' etc., in "Cambodia After Angkor," pp. 433 - 438.

19. Department of Fine Arts, report on Wat Chang Lom; in Thai, Kram Silpakorn, *Wat Jaň Lom,* Ekasar kòn poran-agati hmay lekh 1/2530 (1987).

20. Hiram W. Woodward, Jr., "The Movement of Thai Speakers from the Tenth through the Fourteenth Century: An Art Historian's View," in *Papers from a Conference on Thai Studies in Honor of William J. Gedney,* Univ. of Michigan 1986.

21. William J. Gedney, "A Possible Early Thai Route to the Sea," *JSS* 76 (1988), pp. 12 - 16.

22. Vickery, review of collections of Thai historical documents, *JSS* 60/1 (Jan 1972), see pp. 403 - 410 on *Kalpana.*

23. A. Teeuw and D.K. Wyatt (ed), *Hikayat Patani, The Story of Patani,* pp. 3, 5, 6.

24. Marvin Brown, *Ancient Thai*: "The Language of Suhkhothai, Where did it come from? and where did it go?," reprinted in new edition of *From Ancient Thai to Modern Dialects.* On Tak Bai see Anthony Diller, *"Tones and vowels in Southern Thai: a diachronic anomaly," in Southeast Asian Linguistic, Studies presented to André - G. Haudricourt,* p. 316, and note 5. For James Chamberlain's position see his "A New Look at the History and Classification of the Tai Languages," in *Studies in Tai*

Linguistics In Honor of William J. Gedney, ed. by Jimmy G. Harris and James R. Chamberlain, pp. 50, 63.

25. See Diller, "Tones and Vowels", p. 320.

26. *Ibid.,* 314, 332; and Anthony V.N. Diller, "Consonant Mergers and Inscription One," *JSS* 76 (1988), p. 48, on the plausibility of Mon-Khmer substratum influencing phonological development of Thai, though this statement was not made in reference to the same problem.

26a. J. Marvin Brown, "The Great Tone Split: Did It Work in Two Opposite Ways?," in *Studies in Tai Linguistics in Honor of William J. Gedney,* p. 43.

26b. Cornelius Beach Bradley, "The Oldest Known Writing in Siamese," *JSS* 6 (1909), pp. 1 - 68; Dr. A. Bastian, "On some Siamese Inscriptions," *Asiatic Society Journal* 34/1 (1863), pp. 27 - 38; A.B. Griswold and Prasert na Nagara, EHS 9, "The Inscription of Ramkamhaeng of Sukhothai (1292 A.D.)," *JSS* 59/2 (July 1971), pp. 179 - 228; George Coedès, *Receuil des inscriptions du Siam I.*

27. Report by H.S.H. Prince Subhadradis Diskul to the Thirty-First International Conference of Human Sciences in Asia and North Africa, 1983, Proceedings, pp. 80 - 81; Dr. Prasert na Nagara, *Silpavathanatham,* p. 91; Gedney in "Comments" for the present occasion.

27a. Fang Kuei Li, *A Handbook of Comparative Tai,* The University Press of Hawaii, 1977.

28. J. Burnay et G. Coedès, "ฆ et ฆ et leur origine," *JSS* 21/2, pp. 119 - 126.

29. The way in which hitherto invisible passages, entire sentences, in No. 2 were suddenly revealed in 1980 (published 1984), with the stone itself then set up anew in a position which makes further study impossible, is not an encouraging development. Let us hope that a Paranavitana syndrome is not developing.

29a. Robert B. Jones, "On the Reconstruction of Proto-Thai," *Lingua* 14 (1965), pp. 194-229.

29b. John F. Hartmann, "A model for the alignment of dialects in Southwestern Tai," *JSS* 68/1 (January 1980), pp. 72 - 86.

30. Chamberlain's analysis, as Gedney ("Comments on Linguistic Arguments Relating to Inscription One," draft of paper presented to the AAS Conference, March 17-19, 1989, p. 2) says such work should, "depend[ed] primarily on data from spoken language," not on writing systems, and not at all on any particular view of RK, in contrast to the constructions of Brown and Gedney, which start from assumptions about a writing system, that of RK.

30a. William J. Gedney, Review of Marvin Brown, *From Ancient Thai to Modern Dialects,* in *The Social Science Review* 3.2 (Bangkok: September 1965), pp. 107 - 112.

30b. William J. Gedney, "Evidence for another series of voiced initials in Proto - Tai" (Paris: Proceedings, 12th International Conference on Sino-Tibetan Languages and Linguistics, October 1979).

31. B.J. Terwiel asserts that Ahom script was developed from old Mon after that Thai group reached Assam. Although there is much resemblance among the oldest Mon, Khmer, and Cham scripts, so that a final decision may be difficult, I do not find Terwiel's view convincing from simple inspection. What Terwiel should do now, instead of spending his Ahom time on such things as comparative beer-brewing, is to publish the oldest Ahom inscription, with clear plates, and clear examples of comparison between its script and Old Mon.

32. A few examples were presented at the end of my Canberra paper – the question of tripura, and evidence suggesting parts of RK were based on reading of the Khmer inscription No. 4, in particular the use of the expression about 'knowing the *Tripitaka.*' Dr. Prasert na Nagara has kindly

commented on some of these details in *Silpavathanatham,* pp. 89 - 91, without however convincing me.

33. It does not yet appear in the so-called Modern Inscriptions of Angkor Wat, the latest of which is dated to 1747.

34. For Lao see Richard Davis, *A Northern Thai Reader,* p. 86, pien, 'friend;' pön, 3rd person pronoun; and for standard Vientiane Lao the Ministry of Education Dictionary (2505 /1962), p. 834. Tai Lue and Tai Nuea evidence is in William J. Gedney, "Notes on Tai Nuea," in *Tai Linguistics in Honor of Fang Kuei Li,* pp. 62-102, see p. 86. From Gedney's notes it is not possible to determine whether /phöan/, 'friend' also occurs. Historical dialect comparison suggests that development *ïa > ï > ö has occurred, but not the reverse. See Li, chapter 14.