

“A New *Tāmnān* About Ayudhya”, *JSS* LXVII, 2 (July 1979), pp. 123-86

**The Rise of Ayudhya: A History of Siam in the Fourteenth and Fifteenth Centuries**  
by Charnvit Kasetsiri  
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In his foreword to Charnvit Kasetsiri's *The Rise of Ayudhya*<sup>1</sup> David K Wyatt calls that work “a startling new interpretation of Ayudhya's early history”, which represents “a major new hypothesis intended to explain Ayudhya's relations with its predecessor states”; and he implies that it is “a revision of an historical orthodoxy that had stood virtually unchallenged since early in this century”.<sup>2</sup>

Apparently others have been equally impressed, for *The Rise of Ayudhya*, written as a Ph D dissertation, was published without change from its original form<sup>3</sup>.

Of course, a writer need not be held responsible for the statements in another person's foreword, but when that person was supervisor of the dissertation which became the book in question, and when one of the major themes of the book is very close in conception and content to a paper which the dissertation supervisor was writing at about the same time<sup>4</sup>, it is legitimate to assume that the ideas expressed in the foreword are also the author's own. That is, Charnvit intended his book to be an entirely new interpretation, and in particular that it would reveal the “complex internal dynamics” which would replace the traditional “dreary succession of kings and battles”<sup>5</sup>.

This is the very least one expects from a historian today at a time when dreary successions of kings and battles are no longer considered interesting history at all, and turning attention to the study of society and its internal dynamics is the basic task of a modern historian. The test of quality will be whether the author has made proper

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1. In my own commentary I generally use the graphic transliteration of Thai, but in citations from Charnvit and in other contexts where dual forms would otherwise occur in contiguous statements, I follow his transcription, particularly for proper names.

2. Charnvit, *The Rise of Ayudhya*, pp vii-viii.

3. PhD dissertation, Cornell University,

January 1973. See also the review by R B Smith. *BSOAS*, XLI, 1 (1978), 202-03.

4. David K Wyatt, “Chronicle traditions in Thai historiography”. In spite of the date of publication and references in notes 40 and 44 to two of Wyatt's own publications of 1975, the content of Wyatt's essay seems to indicate that it was written before or during the preparation of Charnvit's dissertation and probably influenced the latter. Moreover, Wyatt calls Charnvit's thesis “recent”, with no mention of the book, which, judging by the date of Wyatt's Foreword, was already being prepared in 1974. Had Charnvit's thesis been completed first, Wyatt would necessarily have referred to it on several points, and would not have emphasized, on his p 121, the ‘Ayudhyā *phongsāwadan*’, since Charnvit's important innovations come mainly from the *tāmnān*.

5. Wyatt's Foreword in Charnvit, p vii.

use of sources, first to identify the problems neglected by earlier historians, and then to explain them; and this includes his identification of assumptions made by earlier generations which may have to be rejected or at least questioned.

As examples of the traditional treatment of Ayudhyan history both Charnvit and Wyatt probably had in mind the writings of George Coedès, Prince Damrong, Prince Chula Chakrbongse, Rong Syamananda, and W.A.R Wood; and indeed their histories of early Ayudhya consist largely of kings and battles.<sup>6</sup> The reasons are, first, that they all grew up with scholarly traditions which accepted kings and battles as the essence of history, and second, because all Ayudhyan history was based on the official chronicles which contain little else. If pressed, they would apparently have answered with good conscience that they could write about nothing other than kings and battles because of the limitations of their sources, or that they first had to establish a chronological and genealogical framework on which to hang the results of subsequent societal research. Such at least was the tenor of Coedès' response to critics who reproached his generation "for not showing sufficient interest in 'economic and social' questions".<sup>7</sup>

It is interesting first of all to compare Charnvit's story with the earlier version of Ayudhyan history. In the broadest outline, according to Charnvit, Ayudhya, a Thai state from the beginning, was suddenly founded in 1351 and rapidly emerged from obscurity thereafter<sup>8</sup>. Its first king, Uthong, came from somewhere else, settling in Ayudhya because of its favourable economic situation and soon thereafter Ayudhya began its expansion at the expense of Sukhothai to the north and Cambodia to the east.<sup>9</sup>

At this level then, there is no difference between Charnvit's story of Ayudhyan beginnings and that of the traditional writers, and each of the above statements is either an explicit detail of the traditional chronicle histories, or an assumption of traditional historians working from them. That is, they are statements which we would expect the author of a "startling new interpretation" to at least question, and then either to reaffirm with more methodical reasoning or convincingly disprove.

The near convergence of Charnvit's treatment with conventional history continues through the fifteenth century, the story of which is almost entirely a paraphrase of Prince Damrong's work of 60-odd years ago<sup>10</sup>. In fact, the only important new details in Charnvit's outline are (a) the affirmation that Uthong came to Ayudhya from Petchaburi, and (b) some attention to the economic background of

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6. See G Coedès, sections on Siam in *The Indianized States of Southeast Asia* and *The Making of Southeast Asia*; Prince Damrong Rajanubhab, commentary to *The Royal Autograph Chronicle* (RA) [in Thai], of which the 1968 one-volume edition will be cited here; HRH Prince Chula Chakrabongse, *Lords of Life*; Rong Syamananda, *History of Thailand* and two Thai-language works cited in Charnvit; W.A.R. Wood, *A History of Siam*. Bangkok, n.p., n.d.

7. George Coedès, "Some problems in the ancient history of the Hinduized states of South-East Asia", pp. 2-4.

8. Charnvit, pp xi, 51. Wyatt also, in Charnvit, p vii, says, "founded so suddenly in the middle of the fourteenth century".

9. Charnvit, pp 78-79.

10. Charnvit, chaps, VI, VII. See also Prince Damrong, RA, pp 248-79, and discussion below.

Ayudhya's foundation, a subject really neglected by the older historians. There are also, on a more detailed level of description, some interesting, if unprovable, hypotheses about the political background to Ayudhya's formation based on sources not generally used by other historians of Ayudhya and it is here, in the protohistory of the Ayudhyan area and the sources dealing with it, that Charnvit's treatment does show some originality, although the relative success of his new approach is something which merits close examination.

Charnvit divides the Thai historical sources into two basic types, *tāmnān* and Phongsawadan/ *bañśāvatār*, the first of which is universal history of the Buddhist world with one or another of the Thai states as its culmination in the author's present, and the second of which is royal dynastic history<sup>11</sup>. The latter category is well known and requires no special explanation. The first such history of Ayudhya is the chronicle of *Hlvaṇ prasro th* (*LP*), extant in an apparently eighteenth-century copy, the preamble of which states that the original version was written in 1681 and was based on archival records. Although such archives have long been lost, the style of *LP* lends credibility to the assertion, and where its information can be checked against external records its chronology seems rather accurate, which has given it a reputation as a reliable outline of the king-and-battle history of Ayudhya from 1351 to 1605<sup>12</sup>.

All the other *bañśāvatār* histories of Ayudhya derive from *LP* in their chronology, which has been skewed, but contain more detailed narrative, the accuracy of which must be investigated by careful internal analysis<sup>13</sup>.

As examples of *tāmnān* history Charnvit cited *Bañśāvatār Hno'a* (*PN*), *Bañśāvatār Yonak* (*PY*), *Cāmadevīvanś*, *Gāñhaikār jāv krun kau*, *Jinakālamālī*, *Nidān brah buddhasihing*, *Sangūtiyavanś*, *Tāmnān mūlaśāsanā*, the chronicles of Nakhon Si Thammarat (*CS*), and the introductory section of the so-called British Museum Chronicle (*BM*)<sup>14</sup>.

The first significant thing to note about these *tāmnān* is that almost all of their stories are centered in old Thai polities other than Ayudhya, and thus they are irrelevant in a typology of

**Ayudhya** historiography, even though, since they do at times mention Ayudhya, they might be of some use in the reconstruction of events in Ayudhyan history<sup>15</sup>.

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11. Charnvit, chap I, and pp 54-56.

12. In Prince Damrong's commentary to *RA* he generally opted for *LP* dates against those of other chronicles, and Griswold and Prasert have done the same.

13. On the chronology see Prince Damrong, "The story of the records of Siamese history" *JSS*, XI, 2 (1914), pp 9; Michael Vickery, "Cambodia after Angkor," chap IX, and see *Ibid*, chap X, for investigation of the narrative sections concerning Cambodia.

14. For bibliographic details see Charnvit, pp 163-74. Note that my citations from *PN* are from *Pra:jum bansavatar/Prachum Phongsawadan* (*PP*), Guru sabha edition, vol I; *CS* refers to Wyatt, *The Crystal Sands*.

15. *Cāmadevīvanś*, *Jinakālamālī*, *Bañśāvatār Yonak*, and *Mūlaśāsanā* are chronicles of northern Siam; *Nidāna Brah Buddha Sihing* is the story of the peregrinations of that statue all over the Thai

Also significant is that those *tāmnān* which eventually merge with Ayudhya as their main concern, and on which Charnvit particularly relied, such as *Gāmhaikār*, *Saṅgītiyavañś*, and in particular *PN*, do not fit Charnvit's definition of *tāmnān* history, best typified by , *Jinakālamālī*, as a form which begins at a point when the Buddha in an earlier incarnation made a vow to attain Enlightenment, passes through the history of Buddhism until it reaches Siam, and then describes the development of Buddhism in Siam up to the time of the writer.

*Gāmhaikār* and *PN* do refer briefly to the Buddha in his incarnation as Gautama, but then their content concerns chiefly the old cities of Sukhothai area, not Ayudhya. *PN* includes Uthong and the foundation of Ayudhya, and *Gāmhaikār* merges with an Ayudhyan history which continues up to the eighteenth century, but not in any special Buddhist framework. Thus to the extent that they are *tāmnān* in Charnvit's sense they do not concern Ayudhya, and in their treatment of Ayudhya they are not *tāmnān*. The same is true of *Saṅgītiyavañś*, which was written as a vast history of Buddhism, but its section on Ayudhya is in no way Buddhist more than dynastic<sup>16</sup>.

Charnvit's treatment of another old chronicle, *Culayuddhakāravañś*, is also equivocal. He says it 'deals with the origin of Prince Uthong in the *phongsawadan* historical tradition', and 'set the style of *phongsawdan* historiography on the question of Ayudhya and Uthong<sup>17</sup>'. But the story of *Culayuddhakāravañś* also begins in a legendary Buddhist past, then skips to Sukhothai, including some of the same stories found in *PN*, and finally merges with early Ayudhya<sup>18</sup>. In its structure it is just as much a *tāmnān* as *PN* or *Gāmhaikār*, and the fact that early Bangkok writers chose its version of Uthong's family background does not thereby make it a *baṇśāvatār*.

What all of these works have in common in their treatment of Ayudhyan history is that they are clearly **not** based on archival records and appear rather to be oral traditions of varying accuracy gathered together at as yet to be determined dates. *Gāmhaikār*, *PN*, and *Culayuddhakāravañś* contain many of the same stories, but reign sequences and chronologies differ, and where comparable, are often in startling disagreement with the as yet unassailable *LP*. Even *Saṅgītiyavañś*, which is the least fantastic for the Ayudhya period, contains a chronology which is at times self-contradictory, and which shows its author to have been influenced by three different

area; and the area of *CS* is obvious from its title. The nature of *PN*, *Saṅgītiyavañś* and *Gāmhaikār* are discussed below.

16. *Saṅgītiyavañś*, Bangkok, 1923, pp 373-421; G Coedès, "Une recension palie des Annales d' Ayudhya"; and for some analysis of *Saṅgītiyavañś* see Michael Vickery, review of *The Short History of the Kings of Siam* by Jeremias van Vliet; and "Cambodia after Angkor", chapter VIII.

17. Charnvit, pp 164 and 56, respectively.

18. The first part of *Culayuddhakāravañś* is in *PP*, part 66, whereas the 1920 edition, cited by Charnvit, begins with its second part. The beginning of the story of part two falls at approximately pp. 76-78 of *PP*, part 66, original edition.

models<sup>19</sup>.

The weaknesses of the *tāmnān*, especially *PN*, were already recognized by Prince Damrong and have been emphasized by every historian since;<sup>20</sup> and while judgements of an older generation often have to be modified in the light of later research, Charnvit's first task, if he wanted to use *PN*, should have been to analyse it thoroughly to prove its worth rather than to use it straightforwardly as an unjustly neglected source with hardly so much as a warning to the reader.

Indeed, in areas of historical study other than Southeast Asia the criticism of sources as a preliminary to their use in the writing of history is considered indispensable, and a text such as *PN* could not hope to be used without such investigation. As one noted philosopher of history puts it, “the first requirement of historical method is to determine the context of your evidence. When your evidence includes texts ... one of the first steps ... must be textual criticism”.<sup>21</sup>

In earlier publications I have analysed some of the texts dealing with early Southeast Asian history, including one used by Charnvit, and have demonstrated conclusively, I believe, that they are of very little value in the reconstruction of the factual past<sup>22</sup>.

Although space does not permit a thorough analysis of *PN*, since it was so important for Charnvit's study it is necessary to describe a few sections of the text in order to demonstrate that *PN* is a late composition which displaces events to impossible time periods, or mixes up recognizable events of different dates, that therefore Prince Damrong's and Griswold's judgements are still valid, and Charnvit's use of *PN* generally unjustifiable.

Among the most striking features of *PN* are the anachronistic statements concerning the legendary events of the old Sukhothai kingdom. Thus, early in the Buddhist Era a certain *rishi* named Sājanālāy was instrumental in the founding of Sawarrgalok, which reflects historical fact to the extent that the name ‘sājanālāy’

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19. Michael Vickery, review of Jeremias van Vliet, *The Short History of the Kings of Siam*.

20. Damrong *RA*, p 3; Damrong, “Story of the Records”, p. 3. A. B. Griswold in particular, in “Thoughts on a Centenary”, p 32, supported Prince Damrong's judgement, and added, “since no one can put [PN] ... to any use at all without making large assumptions as to where this or that incident should be fitted in, it is all too easy to come to almost any conclusion one wishes.” Coedès also, in “Some problems”, referred to Siam before 1350, “about which there existed nothing more than legends which had no foundation in reality”.

21. Morton White, “Historical method in the study of religion”, pp 10-11. See also Edward J Thomas, *The Life of the Buddha as Legend and History*, p. xxii: “the Pali chronicles of Ceylon do not ‘stand on their own tottering feet’”; and any standard manual of historical method, such as Gilbert S. Garraghan and Jean Delanglez, *A Guide to Historical Method*, Westport, Connecticut, 1973, and its intellectual predecessors, the classical European works of Ernest Bernheim, Langlois and Seignobos, and Alfred Feder.

22. Vickery, “Cambodia after Angkor”, treats the Cambodian chronicles for the period up to AD 1600; and Vickery, “The Lion Prince”, is a critique of Manit, *Tāmnān siñhanavatikumāra* (*TS*). I am assuming of course that the first interest of historians is to discover the factual past preliminary to interpreting it, an assumption apparently shared by Morton White, *loc cit*, p. 9; “historical investigation is any sort of investigation intended to determine just what did happen in the past”.

seems to have preceded ‘*sawarrgalok*’, and it illustrates the widespread phenomenon of eponyms whereby the origin of a polity is attributed to legendary heroic figure.<sup>23</sup>

Later on, at a date expressed both as BE 500/BC 43-44, and as *cula* 86/AD 724, we find the reign of King Ruan, traditionally identified with Indrādity, Rām Gāmhaen, Lo’daiy or Lidaiy<sup>24</sup>; and the intention of *PN* to identify him with Rām Gāmhaen is seen in the existence of a younger brother, rather than son, named Rddhikumār (i.e. Lidaiy) and their trip to China which resulted in knowledge of pottery being brought to the Sukhothai area.<sup>25</sup> The writer, in stating that “at that time the sea came up to Sājanālāy”, showed both his awareness of the difficulty, or impossibility, of such a sea voyage in actual riverine conditions, and his ignorance of the fact that the seacoast has not varied nearly so much within historical times.<sup>26</sup>

Shortly after King Ruan’s death in 956-7 AD, the fortifications of Sājanālāy were rebuilt with artillery incorporated into them<sup>27</sup>; and at approximately the same time, BE 1500/AD 957, the famous statues of the Jinarāj, Jinaśih, and Śri Sāsdā are said to have been cast<sup>28</sup>.

Brah Ruan appears again later in another story which relates the sending of tribute water by Sukhothai to Kambujādhipaṭī or Lahvaek. This story may contain a kernel of fact in the subjection of central Siam to Angkor which could have prevailed at approximately the date of the *PN* story<sup>29</sup>, but ‘*lahvaek*’ was not used for Cambodia

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23. *PN*, pp 3-6, ‘Sājanālāy’ is the name found in the first Sukhothai inscriptions, Nos I, II, III, IV.[\*] For analysis see Vickery, “The Old City of ‘Chaliang’--‘Śri Satchanalai’--‘Sawankhalok’, pp. 15-29. **Pp.in this book.**

Examples of eponymism elsewhere are ‘Brut’, cited below, ‘Romulus’ for Rome, ‘Ion’, ‘Achaeus’, ‘Aeolus’, and ‘Pelops’, for locations in Greece. See G W. Cox, *General History of Greece*, I p 16: “each town had its founder or heroic *Eponymos*, whose name it bore”, cited in Funk and Wagnalls, *Standard Dictionary of the English Language*, 1963, under ‘eponymism’, p 840.

24. See Mom Chao Chand Chirayu Ranjani, *A Guide to the Inscriptions of Sukhothai*, p 1; and Vickery, “A guide through some recent Sukhothai historiography”, pp. 193-95.

25. *PN*, p 13.

26. E.H.G Dobby, *Moonsoon Asia*, London, 1961, p. 27; Charles A Fisher, *South-East Asia: A Social, Economic and Political Geography*, London, 1964, p. 27: “at the present moment, marine inundation is probably more extensive than at any time during the last million years”; and for the formation of deltas by alluviation, see his pp. 414-17. Larry Sternstein, “An historical atlas of Thailand”, p. 11, also recognized that the coastline shown on his map I for AD 748 was impossible.

27. The use of the terms, ‘big gun’, (ປຶກຫາຍຸ), ‘gun port’ (ຫອເປົ້ນຫາຍຸ), and ‘casting of guns’ (ຫລວມຫັນ), in *PN*, pp. 17-18 certifies that the writer, anachronistically, intended ‘artillery’, not ‘arrows’. See Hans Penth, “A note on Pün”, *JSS* LIX, 1 (Jan 1971, pp. 209-10. But note, as described in Vickery, “The Old City of ‘Chaliang’--‘Śri Satchanalai’--‘Sawankhalok’, p. 18, the fortifications of the old city in question do have ports for big guns, probably noticed by those who compiled this story.

28. *PN*, pp 24-27; Vickery, “Guide”, pp 217-18.

29. The conquest of Sūryavarman I, in the first half of the eleventh century. [\*Since writing this, I have become less convinced that the relations between Angkor and the central Menam basin were relations of conquest and subordination rather than assimilation of two areas of similar ethno-linguistic identity and culture. See Vickery, “The 2/k 125 Fragment, a lost chronicle of Ayutthaya”, **in this book**, pp. 00-00\*]

until the sixteenth century AD<sup>30</sup>, which serves to date the composition of the story and indicates the writer's probable ignorance of Angkor-period political details.

Brah Ruan is also made the creator of various alphabets - Thai, Thai *chian*, Mon, Burmese, Khom, etc, at a date described both as BE 1000/AD 456 and *cula* 119/AD 757, at a time when he is said to have cut the Buddhist Era to establish a new one<sup>31</sup>.

In connection with Brah Ruan, Chiang Mai history is shifted to a past more venerable than claimed by its own *tāmnān*, and with a story not found at all in northern sources. Briefly, there was no male heir in Chiang Mai, so the officials asked for Rddhikumār to marry the late king's daughter, Nān Mālikā. After the marriage, he was reconsecrated as Brahyā Lu'a which again illustrates confusion of genuine Sukhothai genealogy, and "ever since Lao women have had the custom of asking for husbands"<sup>32</sup>.

One more example of the total confusion of *PN* as history is an incident used by Charnvit as evidence for the factual background of Ayudhya, saying "in 1307, Phraya Kraek became king in Ayudhya"<sup>33</sup>. The *PN* story of Phraya/Brahyā Kraek starts approximately 102 years after the expressed date BE 1502/AD 959, with Brah Mahā Buddhasagar reigning in *Hnōn sano*. He is said to have died in '336', which at the latest would have been *cula* era, or AD 974. He was followed by Brahyā *godama* for 10 years, then by Brahyā *gotratahpòn*. The latter was eventually replaced by a certain Brahyā Kraek, in whose reign the date BE 1850/AD 1307, appears. However, the impossibility of the time span in the total story means that no single date is acceptable. Furthermore, although Kraek is said to have died in BE 1857 plus 59 years, in further stories of his antecedents and reign we find the dates BE 1600, *cula* 100<sup>34</sup>; while the date AD 1307, expressed as *cula* 669, comes up again later in an entirely different story about different individuals<sup>35</sup>. Quite apart from the question of dates, the Kraek stories are particularly risky as a basis for historical reconstruction, since Kraek, as a crippled child who eventually becomes king, fits an almost world-wide pattern of mythological heroes<sup>36</sup>.

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30. See Vickery, "Cambodia after Angkor", pp 68, 82.

31. *PN*, p. 11. [\*On the cutting of eras, see Vickery, "The Lion Prince", a review article on Manit Vallibhotama, *Tāmnān siñhanavatikumāra*, pp. 326, 377.\*]

32. *PN*, pp. 14-15. Chiang Mai is called '*mo'an bijāy jianhmai*', but the general location is placed beyond doubt by the ethnic identification 'Lao'. [\*That is, the Chiang Mai region was considered by Bangkok Thais and westerners as 'Laos' or 'Western Laos' well into the 20<sup>th</sup> century, as demonstrated by Coedès famous study of the *Jinakalamālī*, entitled "Documents sur l'histoire politique et religieuse du Laos occidental" ['western Laos', emphasis added],\*]

33. Charnvit, p 46.

34. *PN*, pp 34-43.

35. *PN*, p 54.

36. See Stith Thompson, ed *Motif-Index of Folk-Literature*, vol. V, L100-L199, "Unpromising hero (heroine)", and with respect to medieval Europe see Emmanuel Le Roy Ladurie, *Montaillou, village occitan de 1294 à 1324*, p. 428, n 3; "the limbless, lame, and blind as cultural heroes...".

Thus even when the stories of *PN* have some connection with historical fact, it is only through our knowledge of the facts from better sources that we can discern that aspect of *PN*, and the latter on its own is simply not admissible as evidence for the facts of central Siamese history either for the period explicitly designated in its text or even when replaced in the proper Sukhothai or Ayudhya temporal context.

A moderate acquaintance with the historiography of other parts of the world should also impose a critical attitude toward such collections of oral tradition in Southeast Asia. *Tāmnān* are not unique to Thailand. The same sort of things were written in the West in earlier times. “The Romans had set an example in faking origins: “Virgil brought Aeneas and his Trojans to Latium to win a kingdom, so as to glorify the early Romans”; and the Roman myth was further extended by the Franks who claimed descent through Frankon, son of Hector in a story with several variants. The Celts of the British Isle also invented an eponymous ancestor, Brut, or Brutus, who was “grandson of the Trojan Aeneas and the founder of the royal race of Bretons”, which, along with ‘Frankon’, illustrates the same phenomenon as the name *sājanālāy* cited above.<sup>37</sup> Each town or principality which boasted any history at all had to have its share in antiquity. The Latin king Turnus was said to have founded Tournai, and a certain school of Polish historiography believed that ‘Cracow’ derived from ‘Greek town’, since the Poles in origin were Greeks whose ancestors had defeated Alexander the Great and then fought their way north to settle in Poland.<sup>38</sup>

The same sort of confusion occurred in the Middle East. After the Arab conquest the Persians forgot almost all of their ancient history and they had to “fall back on mythology, which forms the basis of the great national epic of Firdawṣī, the *Shāhnāma*”. The communal memory retained only two historical names of kings from antiquity, one of whom, Darius ‘was remembered in a confused and **conflated form, based on three monarchs of that name**’ (emphasis added). Moreover, Alexander, the foreign conqueror, was made into a native and presented in the myths as a Persian prince claiming his own. Muslim Spain was also forgotten by Muslims; and the “work, indeed (the) name” of Ibn Khaldun, “one of the greatest historians who ever lived”, were “virtually forgotten among the Arabs.”<sup>39</sup>

Just as in the case of Angkor or Sukhothai, the factual history of early Europe and the Middle East has been reconstructed in modern times through the use of sources neglected by, or unknown to, the traditional writers. Today no one would give the least attention to a history of Europe which seriously evoked Brut as forefather of the Bretons, the Trojans as ancestors of the Franks, or a Greek origin for the Poles<sup>40</sup>; but one of the fascinating aspects of Southeast Asian historiography is

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37. Beryl Smalley, *Historians in the Middle Ages*, , pp 50-51; Leon Poliakov, *The Aryan Myth*, pp 18, 38-40.

38. Beryl Smalley, *loc. cit.*

39. Lewis, *History, Remembered, Recovered, Invented*, pp 40, 71-78.

40. Although the Arthurian myth apparently still has its scholarly adherents. See Donald A White, review of two books on King Arthur, *American Historical Review*, LXXX, 2 (1975), pp. 380-81. It is interesting to note that in *Roman Britain and the English Settlements*, vol. I of *The Oxford History of England* (2nd ed, 1937), R. G Collingwood, pp. 320-24, attempted a *tāmnān*-type synthesis for

that all of the stages of development of European history writing can be found compressed into a period of the last 100-500 years.

To be sure, Charnvit is not alone, even among historians of the Western tradition, in trying to use oral traditions as straightforward sources or records. One of his mentors, O.W. Wolters, apparently believed in long collective memories, even in the absence of written records, basing his belief on an ‘oral tradition’ of the late Tom Harrison, who is reported to have said “that Iban family memories, even when they extend back to time as much as three hundred years, can be regarded as reliable in matters of concern to the families”<sup>41</sup>. Unfortunately, Harrison is no longer around to carry the investigation further, but one wonders what evidence could be brought forward to support the accuracy of Iban genealogies; and E. R. Leach, who conducted more careful investigation in a similar situation, discovered that although “some chiefly genealogies purport to record history for the last forty generations or more”, and “every Kachin chief is prepared to trace his descent back to Ninggawn Wa, the Creator”, there “could be disagreements .... even with regard to persons as near as the great grandfather’s generation.”. Leach concluded that “Kachin genealogies are maintained almost exclusively for structural reasons (that is, relative seniority in present-day political relations) and have no value at all as evidence of historical fact.”<sup>42</sup> This conclusion agrees with the results of research in the oral historical traditions of medieval Europe, where the inability to preserve much historical fact beyond the grandfather’s generation has long been common knowledge among specialists<sup>43</sup>.

I hope this digression has shown that the weight of the evidence from European and Middle Eastern historiography plus the analysis of Southeast Asian chronicles and oral traditions so far undertaken is against the treatment of *Tāmnān* as **historical records**, and that it is incumbent on writers who wish to use them to demonstrate their worth.

## Ancient Thai history and the pre-Ayudhya Menam Basin

Charnvit used *PN* and the Ayudhyan sections of other *tāmnān* for two main purposes, as evidence for the nature of pre-Ayudhyan society and for an analysis of the origins of Uthong, which latter subject will be discussed below in connection with Charnvit’s chapter IV.

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the period of ‘King Arthur’, while his co-author, J.N.L. Myres, who adopted a source-critical approach to the same period, apparently found it unnecessary to postulate the existence of ‘King Arthur’ at all.

41. O.W. Wolters, *The Fall of Srivijaya in Malay History*, chap VI, n 55.

42. Leach, *Political Systems of Highland Burma*, pp 127, 167.

43. For remarks on the confusions in European oral traditions see Ladurie, *op. cit.*, pp 428-29; Bernard Knox, “Triumph of a heretic”, *New York Review of Books*, 29 June 1978, pp. 4-8; E. K. Chambers, *The English Folk-Play*, New York, 1966; and A. van Gennep, *La formation des légendes*, where confusions very similar to those of Southeast Asian *tāmnān* are recorded: for example, a German legend of Lutheran Swedes in conflict with Charlemagne, who was equipped with large cannon (pp. 166-67).

The first subject was part of Charnvit's discussion in chapter I, "The nature and concept of ancient Thai history", and it is also an important element in the arguments of chapters II and III. As Charnvit emphasized, the authors of *tāmnān* history were more concerned with religious than administrative or economic history and thus religious figures, rather than warrior kings, were given attention. However, these *tāmnān* were admittedly written long after the periods they purport to discuss, their details are often fantastic, and it does not necessarily follow that in reality "in the early stages of Thai history it was religious men, either monks or people who led a different way of life from ordinary laymen, who were the most important leaders of the society"<sup>44</sup>. It could well be that the stories show an idealized portrait of an ancient golden age, and they must be carefully analyzed internally before drawing factual conclusions from them.

Charnvit writes, for example, "another Thai **record** (my emphasis; the 'record' is *PN*) shows that the building of the city of Phitsanulok was led by a religious man named Ba Thammarat"<sup>45</sup> without telling us that the event is placed by *PN* in approximately BE 500, long before 'Phitsanulok' could possibly have existed, and even before the earliest date scholars give for the beginnings of Phitsanulok's predecessor, Sukhothai. Moreover, the story of 'Ba Thammarat' in *PN* concerns, not explicitly Phitsanulok, but Savarrgalok and "Kambojanagar or *mo'an/muang Dun Yān*"<sup>46</sup>, which some historians might wish to **interpret** as being in the general Phitsanulok **area**.<sup>47</sup> The explicit founding of Phitsanulok is placed by *PN* slightly before BE 1500/AD 956-7, also fantastic<sup>48</sup>.

It is likely that both these stories are muddled traditions dating from a time after Phitsanulok had become the principal city of the old Sukhothai area, and also after the 'Thammarat' (Dhammarāja) kings of that area had become a vague and confused memory<sup>49</sup>. In his chapter III Charnvit amplifies the discussion and draws wider conclusions concerning the "nature of the institution of kingship in the Menam Basin".<sup>50</sup> He seems to believe that these vague and misdated traditions show literally that kings were chosen from among informal religious leaders, that "there was no tendency for royal families to rule for a long period of time, establishing dynasties", and that the "situation was highly fluid, permitting contenders or challengers to take over".<sup>51</sup>

44. Charnvit, p 5.

45. *Ibid.*

46. *PN*, pp 3-8.

47. Charnvit, however, in other contexts, pp 63, 65-66, and n 35 to chap V, interprets 'Kamboja' as meaning the area of Lopburi, Suphanburi, Ratburi, and Phetburi. For a full discussion of the Kamboja problem see Vickery, "Cambodia after Angkor", pp 369-77.

48. *PN*, pp 21-27.

49. Prince Damrong, *RA*, p 235, accepted that the name 'Phitsanulok' did not come into existence until after the reign of Uthong, that is, at the earliest, in the last quarter of the fourteenth century; and no source discovered since Prince Damrong wrote has yet shown an earlier occurrence of the name.

50. Charnvit, p 44.

51. Charnvit, p 45, for quotation.

Of course, since we have no other written ‘history’ concerning those places at that time, it is not possible to rely on other textual evidence to disprove such stories conclusively, but the vagueness of the *tāmnān* with regard to time and place, and the internal analysis which has been carried out to date, should make one extremely circumspect in dealing with them. Their stories conflict with Charnvit’s own description of the economic background of Ayudhya, and the most genuine *tāmnān*, *Jinakālamālī*, which is perhaps closest of all to the founding of a real kingdom, emphasizes the importance of hero kings, not religious figures, as far as political events are concerned<sup>52</sup>.

There is no objection to the main point of chapter II. “The Menam Basin before 1351”, that before the foundation of Ayudhya that area had lacked unity and contained a number of small *mo’ān/muang*. So much is clear from the extant inscriptions and the contemporary reports of Chinese diplomats and traders, but some of the details which Charnvit wishes to elaborate within the larger picture are highly tendentious.<sup>53</sup>

The first example involves questions of both historiography and fact. Charnvit wonders if his description of a fragmented Menam Bas in “ignores the importance and power of Sukhothai”<sup>54</sup> and is contrary to a certain interpretation of pre-Ayudhyan history which would have Sukhothai ruling nearly the entire Menam Bas in the thirteenth century. He then answers that “this view of Sukhothai is a rather recent development”, that “no such regard for Sukhothai existed in the minds of the men of Ayudhya”, and he refers to one *tāmnān* story, an “Ayudhyan historical record”, which omits any mention of Sukhothai at all<sup>55</sup>. He seems to feel that since Ayudhyan historiography, as he reads it, more or less ignores Sukhothai, we may conclude that Sukhothai was not very important. This seems to be an ultra-Collingwoodism carried to the extreme - history is the rethinking of past thought, and if the Ayudhyan historians did not think about Sukhothai, then we cannot think our way through them to it.<sup>56</sup>

Some relevant facts about this subject are (a) the study of Sukhothai history is, as Charnvit notes, a modern undertaking, and (b) Ayudhyan writers knew much less about Sukhothai than we believe we know now, although (c) the *tāmnān* which ultimately merge with Ayudhyan history devote most of their pre-Ayudhyan attention

52. See below for discussion of the economic background. *Jinakālamālī* relates the founding of Chiang Mai, Chiang Saen, and Chiang Rai.

53. For some of the epigraphic evidence, see G Coedès. “Nouvelles données épigraphiques sur l’histoire de l’Indochine centrale”, JA, CCXLVI, 2 (1958), pp 125-42. The Chinese records mention, in the twelfth to fourteenth centuries, at least seven: Chēn- li-fu, Hsien, Lo-hu, Ming-t’ai, Petchaburi, Sukhothai, and Su-mēn-pang (possibly Suphanburi). See T Grimm, “Thailand in the light of official Chinese historiography”, JSS, XLIX, 1 (July 1961), pp 1-20; G H Luce, “The early Syām in Burma’s history”, JSS, XLVI, 2 (Nov 1958), pp 139-40; O W Wolters, “Chēn- li-fu”, JSS, XLVII, 2 (Nov 1960), pp 1-36. (for ‘Su-mēn-pang’, see Wolters, n 70).

54. Charnvit, p 13.

55. *Ibid*, p 14.

56. R G Collingwood, *The Idea of History*, p 228; Vickery, “Guide”, p 185, n 9.

to the Sukhothai area, even if they give more prominence to Sājanālāy or Phitsanulok than to Sukhothai itself<sup>57</sup>, and (d) the *bañsāvatār*, which Charnvit seems to be downgrading in his early chapters, show how important Sukhothai was well into the Ayudhya period<sup>58</sup>.

The relative neglect of Sukhothai by Ayudhyan historians is because their histories, including the *tāmān*, were written long after Sukhothai's decline, and the writers were ignorant of the details of its important earlier history. The modern reconstruction of Sukhothai history is due to the rediscovery and study of original Sukhothai records in the form of stone inscriptions which the Ayudhyan writers ignored; and the *tāmān* are more hindrance than help in their interpretation<sup>59</sup>.

Nevertheless, Charnvit has, in an awkward way, put his finger on an aspect of Menam Bas in history which is important and still controversial - what precisely was the political importance and territorial extent of Sukhothai from mid-thirteenth to early fifteenth century?<sup>60</sup> All the best records, indigenous and foreign, indicate political fragmentation in the lower Menam Basin, and, as Charnvit wrote, the main centers seem to have been Dvaravati, Suphanburi, Ayodhya/Ayudhya, and Lopburi. Sukhothai, judging from most of its own records, would have been just another *mo'an/muang*, again as Charnvit wrote, were it not for the final 'epilogue' of the Rām Gāmhaen inscription and the identification of Sien/Hsien with Sukhothai. I have earlier indicated that I consider both these arguments to be very weak, and if Sukhothai is to be put into its proper place it must be through a reworking of the records dealing with those two points, and not with the argument that certain historians of Ayudhya ignored Sukhothai.<sup>61</sup>

Another important point which Charnvit touches in this and later chapters is

57. Including *Gāmhaikār* which Charnvit misused in his n. 4 to chap II. Although *Gāmhaikār*, p. 177, has a summarized history of the Ayudhya background which omits Sukhothai, on p. 178 its king list includes 'Brah Cau Ruañ', and 'Brah Cau Lū'a', who have never been associated with any place but Sukhothai; the body of the text has a section on 'Brah Ruañ Sukhodaiy' (pp 11-29); his younger brother Brah Lū'a succeeded him and moved to Nagar Savarrgpuri, where he was followed by two more kings (pp 29-36); and then we find King Śrī Dharrmarāj of Bisñulok (pp 36-40), and King Anurāj of Jāynād (pp 40-46).

58. See the northern campaigns of the Ayudhyan kings and the Ayudhyan involvement in the affairs of the Sukhothai area between 733/1371 and 800/1438, in *LP*, any edition, and the corresponding entries, with skewed dates, of *RA*.

59. As an example of such hindrance see Prince Chand, *Guide*, p 31; and a comment in Vickery, "Guide", pp 217-18.

60. A B Griswold and Prasert na Nagara (G/P), "On kingship and society at Sukhodaya", pp 39-43; G/P, "Epigraphic and Historical Studies (EHS) No 10", 26-47; Vickery, "Guide", pp 207-08.

61. [\*On Rām Gāmhaen see my 'Piltdown papers in Chamberlain, *The Ram Khamhaeng Controversy*, and "Piltdown 3" here. It is now accepted that Sien/Hsien was not Sukhothai, but the coastal area which probably included Ayutthaya. See Yoneo Ishii, "A reinterpretation of Thai history with special reference to the pre-modern period", Paper presented at 8<sup>TH</sup> international conference on Thai studies at Nakhon Phanom, Thailand, January 2002; Chris Baker, "Ayutthaya Rising: From the Land or From the Sea", pp. 41-62; and Charnvit Kasetsiri, "Ayudhya: Capital-Port of Siam and its Chinese Connection in the Fourteenth and Fifteenth Centuries", 75-80. \*] Vickery, "Guide", pp 207-08, 215-16, 204-05 respectively, and below, pp. 28-29.

the economy of the lower Menam Basin. He assumes that the economy of the entire lower Menam, the area of “the central region of the kingdom of Dvaravati”, was a center of rice production, and he also makes a point which has been neglected in most previous studies of Ayudhya, that the Dvaravati-Ayudhya area was also important for maritime trade and had a “mixed hinterland-maritime”economy which “made Ayudhya different from all the other important Thai kingdoms”<sup>62</sup>. This is an extremely important subject which needs to be developed, but I shall argue below, in connection with Charnvit’s chapter IV, that a good opportunity was spoiled by his efforts to force economic analysis into the *tāmnān* framework.

Two statements of less value, at least without detailed substantiation, are (a) that part of Lopburi’s rice production was sent to Sukhothai, which supposedly lacked rice, and that (b) Sukhothai’s rice deficiency was such that it eventually could not feed its population and lost military ascendancy<sup>63</sup>. The first is based on the equation of Hsien with Sukhothai, but it should be clear from even casual observation of the geography of the two areas that Sukhothai, particularly with the irrigation works built by its kings, would never have needed rice from Lopburi, and it is doubtful that the transport of the time would have been adequate to carry bulk foodstuffs such a long distance. In fact, the export of Lo-hou (Lopburi) rice to Hsien, recorded by the Chinese<sup>64</sup>, is another piece of evidence, if the latest views on the ecological history of the Menam Basin be accepted, that Hsien was probably somewhere in the delta to the south of Lopburi. That is, the delta area, before the improvements of the nineteenth century, would have been unsuitable for large-scale agriculture, would only have developed due to a favourable situation for trade, and would always have been a rice-deficit area<sup>65</sup>. As for the second statement, about Sukhothai’s lack of food to feed its population, Charnvit cites no evidence and I have no idea what the basis for it was.

[\*There seems now to be a new consensus (see note 61 above), including Charnvit, that Hsien was on the coast , not Sukhothai, and that the coastal region, including what became Ayutthaya, was not in ancient times a rice granary. In fact, the location should have been clear from one of the first Chinese reports concerning Hsien, that of Zhou Daguan, an envoy to Angkor at the end of the 13<sup>th</sup> century, and who said, in connection with a recent war between Hsien and Angkor, that Hsien was southwest of Angkor.\*]

Besides these major points, several statements of chapter II, resulting from Charnvit’s uncritical use of disparate secondary sources, need mention. First, it is not possible to speculate on when Suphanburi came into existence, and we certainly have no information that it “was the main center of manpower and military strength”, an inference which may derive in part from a statement by Prince Damrong, probably based entirely on the laconic chronicle entry of 712-715/AD 1351-53, which merely

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62. Charnvit, pp 19-20 and p 23 for similarly apt remarks about the situation of Suphanburi.

63. Charnvit, pp 18-19.

64. For the Chinese remark see W.W. Rockhill, “Notes on the relations and trade of China”, 99-100. It is not clear from Charnvit’s statement, p 19, and his note 19, that he was aware of what the original source said or of the interpretive nature of the material he was using.

65. Yoshikazu Takaya, “An ecological interpretation of Thai history”, 190-95.

says the governor of Suphanburi was sent to aid the king's son in a war with Cambodia<sup>66</sup>.

Although it is reasonable to believe that Suphanburi had advantages in foreign trade similar to the other lower Menam cities, it is not admissible to assume that the Thao Uthong of the Nakhon Si Thammarat chronicles was from Suphanburi or that Suphanburi paved the way for the putative conquests of Rām Gāmhaen.<sup>67</sup> The Nakhon Si Thammarat story could well be a displaced legend and that text must be thoroughly analyzed before attempts are made to integrate its details into further syntheses. In any case, the story says Thao Uthong was “ruler of Kruṇ Śrī Ayudhyā”<sup>68</sup>, and we may not assume that it ‘it must have meant’ Suphanburi. In fact, if we must emend, why not say Uthong came from Ayodhya<sup>69</sup>?

It is also premature to state that the Nakhon Sawan inscription of AD 1167 records Lopburi’s attempt to gain independence.<sup>70</sup> All it records, and all that may yet be inferred from it, is the existence of a political center and a royal family unknown from other sources. Coedès’ interpretation involved the covert assumption that newly discovered inscriptions had to be related to political centers already known from literary sources<sup>71</sup>. Furthermore, Lopburi’s assertion of independence, for which there seems to be good evidence, was not necessarily from **Khmer** domination (an effort by a non-Khmer group), but only from that of Angkor. The inscription of 1167 is in Pali and Khmer, and there is ample other epigraphic evidence to show that the central Menam Basin and Malay Peninsula, both before and after that date, were partly occupied by Khmer centers which were outside the political and cultural orbit of Angkor<sup>72</sup>.

### “The emergence of the Thai in the Menam Basin”

Chapter III, like chapter II, is based on uncritical acceptance of the details of various *tāmnān*, “neglected Thai sources”, which Charnvit persists in calling “records”; and the same general objection, that until such stories have been critically analyzed their details are unacceptable, still prevails. This means that Charnvit’s story of the emergence of the Thai is no more than speculation.

It is nevertheless interesting and useful, and only fair to the reader, to provide

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66. Charnvit, pp 16, 22; Prince Damrong, *RA*, p 241, says that King Paramarājādhirāj, formerly of Suphanburi, was skilled in warfare.

67. Charnvit, p 24. For the doubtful nature of Rām Gāmhaen’s conquests see the latest views of G/P, cited above, n 60; and Michael Vickery, “Piltdown 3--Further discussion of the Rām Khamhaeng Inscription”.

68. CS, pp 90-94, 150.[\*See Vickery, review of David Wyatt, *The Crystal Sands-The Chronicles of Nagara Sri Dharmaraja*, in *Journal of Southeast Asian Studies*, Vol. VIII, No. 1 (March 1977), pp. 118-120. \*]

69. See below, the discussion of chap V, in which Charnvit devotes considerable effort to proving the existence of Ayodhya as predecessor of Ayudhya.

70. Charnvit, p 20.

71. Coedès, “Nouvelles données”, pp 133-39. Coedès, contrary to Charnvit and his sources, preferred to attribute the inscription to the Mon kingdom of Lamphun.

72. Michael Vickery, “The Khmer inscriptions of Tenasserim”, *JSS*, LXI, 1 (Jan 1973) 51-70.

some criticism in detail, particularly since the important *tāmnān* here are mostly from the North, and *PN*, treated above, plays only a minor role.

One of these northern *tāmnān*, *TS*, was analyzed by this writer about three years ago<sup>73</sup>, and while Charnvit cannot be criticized for not taking note of what had not yet been written, the chronological confusion of *TS*, its impossible details such as reigns of 120 years, and certain attempts already made to revise it, should have convinced even the most casual reader that no detail can be taken with confidence as a historical fact<sup>74</sup>.

On the positive side Charnvit began by emphasizing that the ‘Nanchao theory’ of early Thai history must now be rejected, and this is presumably what he means by “a ‘revolution’ in ideas about early Thai history”.<sup>75</sup> It turns out, however, that Charnvit only rejects the extreme version of the Nanchao theory, that the Thai had only moved into the area of present-day Thailand after the conquest of Nanchao by the Mongols in the thirteenth century; but that had already been rejected by Coedès 14 years ago<sup>76</sup>. Charnvit still accepts that the heartland of the Thai before their move into Thailand was “in the area between Chiangmai and the mountains of Yunnan”,<sup>77</sup> even though the whole point of exploding the Nanchao theory was to question whether the Thai had ever occupied that area at all before their appearance in Thailand. The sources on which Charnvit based his ideas are themselves based either on a modified Nanchao theory or on the legends found in *TS* and related chronicles. So far as I know, the only serious research on the origin of the Thai before they entered Thailand is linguistic, and it tends to show that they entered the areas of present-day Thailand from what is now Laos and northern Viet Nam after having spread from the Kwangsi-Viet Nam border area<sup>78</sup>.

Nevertheless, even while denying that the sources used by Charnvit are valid, or that his conclusions follow from any solid evidence, I would like to state that I have no objection to the idea that “the appearance of the Thai in the Menam Bas in occurred well before the thirteenth century”, and that they “had taken many centuries to gain ascendancy”<sup>79</sup>. But this is an *a priori* notion which cannot be proved by any extant evidence in the present state of research. At most it could be argued that the amount of Sanskrit and Khmer already assimilated by the Sukhothai language at the time of the first inscriptions would have required a long period of acculturation which

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73. Vickery, “Lion Prince”.

74. The revised version is that of Manit Vallibhotama, analyzed in “Lion Prince”. Charnvit, by citing only this version of *TS*, has implicitly accepted Manit’s revisions.

75. Charnvit, p 30.

76. G Coedès, *Les états hindousés d’Indochine et d’Indonésie*, 1964, or its English translation, *The Indianized States of Southeast Asia*, chapter 13, section 1.

77. Charnvit, p 36.

78. James R Chamberlain, “The origins of the southwestern Tai”, *Bulletin des amis du Royaume Lao*, No 7-8 (1972), pp 233-44. Note that the better known work of J Marvin Brown, *From Ancient Thai to Modern Dialects*, did not use linguistic evidence to prove the Yunnan origin of the Thai, but merely accepted the then traditional ideas. See also William A. Smalley, review of Marvin Brown, in *JSS*, LV, 1 (Jan 1967), p 127.

79. Charnvit, p 36.

could only have occurred in central or northeastern Siam.

I suppose an immediate objection that might be offered to my contention that no proof is available is the mention of *syām*, assumed to mean Thai, in the eleventh-twelfth century Cham and Angkor inscriptions<sup>80</sup>. However, since the first Europeans to visit Siam were impressed by the fact that ‘siam’ was a name used by foreigners, not by the Thai themselves<sup>81</sup>, we are not obliged to assume that *syām* meant Thai, and it is possible, maybe even probable, that *syām*, and Chinese *hsien*, referred to the Menam Basin irrespective of ethnicity. On the other hand, if it could be proved that *syām* always meant ‘thai’, and if the Thai were all over the Menam Basin before the thirteenth century, there would no longer be any reason to identify Hsien with Sukhothai, a matter to which I shall return later<sup>82</sup>.

Among the specific points concerning early Thai settlement which require comment and correction are the following.

The cula era.<sup>83</sup> Charnvit has used a story of the founding of the *cūla* era (AD 638-39) by ‘Laochakkarat’ in northern Siam as the “first recorded appearance of the Thai in local history”, and he feels it “is difficult to disregard this legendary episode” because it “is frequently reported” and because of “the eventual acceptance and widespread use of the Lesser (*cūla*) Era”. This is one of the episodes I treated in my analysis of *TS*<sup>84</sup>, where I showed that the early part of *TS*, including the story of the *cūla* era, is fictional; and the occurrence of the story in “a great number of northern Thai documents” only demonstrates a relationship among the texts.<sup>85</sup> In any case, it is now known that the first Southeast Asian use of the *cūla* era was in Burma, from where it later spread to Siam<sup>86</sup>, and the Thai (my emphasis) did not “continue to use (it)” from any single date or, as far as extant evidence shows, from any date as early as the seventh century. The Thai of Ayudha, judging from extant inscriptions, did not

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80. Coedès, *Indianized States* (Kuala Lumpur), pp 140, 190-91.

81. Donald F Lach, *Southeast Asia in the Eyes of Europe*, p 524, for reference to the Portuguese. Seventeenth-century Iranian visitors to Ayudha also understood that “the Iranians the Franks call the natives of Shahr Nāv [Ayudha] Siamese, but the natives themselves trace their stock back to Tai”. The Iranians also considered most of the inhabitants of Pegu to be Siamese, which might mean that the term was originally applied to the Mon, who, as we know, occupied the lower Menam area (Dvāravati) before the Thai. See John O’Kane, trans, *The ship of Sulaiman* (London, 1972), 88, 198. [\*Shahr Nāv or Shahr-i Naw is ‘new city’ in Persian, not Arabic, *pace*/Charnvit, “Origins of a Capital”, p. 59; Andaya, “Ayutthaya and the Persian and Muslim Connection”, pp. 121, 136. See n. 159 below.\*]

82. See also my remarks above, and Vickery, “Guide”, pp 204-05.

83. Charnvit, chap III, n 12.

84. Vickery, “Lion Prince”, pp 365-66.

85. As David K Wyatt has stated, in Wyatt and Dian Murray, “King Mangrai and Chian Rung”, *JSS*, volume 64, part 1 (January 1976), pp. 378-81, “The northern chronicles in particular often give the impression of having derived from a single, almost circular tradition: and if ... two different chronicles are both based on a single source, it is no proof of reliability to say that the two check against one another”.

86. Roger Billard, *L’astronomie indienne*, Publications de l’Ecole Française d’Extrême Orient, Tome LXXX-II (Paris, 1971), 74, 124.

use it until the late sixteenth century,<sup>87</sup> the Thai of Sukhothai first used the *śaka* era and only switched to *cula* in the late fourteenth century;<sup>88</sup> and even if the northern Thai never used any other era, the oldest extant record dates only from 1369<sup>89</sup>.

Moreover, since the northern Thai have always used the common Chinese-Vietnamese 60-year cycle of dates, which with respect to the numerical designation of years within the decade is different from the *cula* era<sup>90</sup>, we might conclude that the *cula* era only came to the north from Sukhothai along with the religious influence marked by inscription no. 62. This impression is reinforced by the fifteenth-century inscription from Keng Tung which has several dates both in the 60-year cycle and the Mon-Khmer-southern Thai animal cycle, but no dates in any **named** era.<sup>91</sup>

It is necessary to add that Charnvit's treatment of this matter, with respect to historical method, is particularly inconsistent. On the one hand he cites the Nan chronicle and *PY* for the founding the *cula* era<sup>92</sup>, but on the other hand he accepts Manit Vallibhotama's version which places the beginning of the *cula* era about 560 years earlier than 'Laochakkarat' and omits the latter story altogether<sup>93</sup>. If Manit is correct, then the Nan chronicle and *PY* should also be revised, and if the story of 'Laochakkarat' is accepted, Manit's version must be ignored. In another context Charnvit also makes use of *TS* via Kachorn Sukhabanij's "Thai beach-head states"<sup>94</sup>, which is based, not on Manit, but on the original *TS* whose chronology follows the same system as the Nan chronicle.

### The Thai beach-head states.

Charnvit seems to accept the suggestions of Kachorn Sukhabanij, which Kachorn himself acknowledges as being based on "legendary facts", concerning several early settlements of the Thai within the boundaries of present-day Thailand.<sup>95</sup> Some of these suggestions are based on *TS*, which in its present condition is not at all usable.<sup>96</sup> One more proposal is based on the AD 1167 inscription of Nakhon Sawan, which would show, according to Kachorn, that "a Thai chief, Kamrateng Añ Śrī

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87. See the Ayudhyan inscriptions in Vickery, "The Khmer inscriptions of Tenasserim"; in particular the Dansai inscription (*Ibid*, n 14), the last Ayudhya document before the Sukhothai royalty were enthroned there. We may perhaps assume that the *cula* era became official in Ayudhya under the Sukhothai kings after 1569, although there are no contemporary documents from that period.

88. The Rām Gāmhaen inscription uses *śaka*, as do all the inscriptions of Lidaiy. The earliest use of *cula* seems to be the small gold plate of 746/1384 (G/P, *EHS* No 11-1, *JSS*, LXI, 1, July 1973; pp 124-28). *Sāka* was still favoured by some later Sukhothai writers, as in No XLIX of the early fifteenth century.

89. The first dated northern Thai document is inscription No. 62 from Lamphun.

90. See explanation in Vickery, "Lion Prince", pp 341-43.

91. See G/P, *EHS* No 19, "An inscription from Keng Tung (1451 AD)", *JSS*, LXVI, 1 (jan 1978) 69.

92. Charnvit, p 48, n 12.

93. Charnvit's bibliographic references to *TS*, and Vickery, "Lion Prince", pp 334-37, 339-40.

94. Charnvit, pp 34-35, and p 48, n 14.

95. Charnvit, p 35; Kachorn Sukhabanij, "The Thai beach-head states in the 11th-12th centuries", *Silpañkon*, I, 3 (Sept. 1957), 74-81, and I, 4 (Nov 1957), 40-54 p. 46 for the quotation.

96. Vickery, "Lion Prince".

Dhammadokarāja, was ruling at Sawankaloke at this period".<sup>97</sup> But since the inscription is in Pali and Khmer, and contains no details which can be identified with any other document, it cannot bear evidence on the subject of Thai settlement. The mere mention of the title 'Dhammadokarāja', there and in *CS*, is not sufficient to postulate a Central Menam **Thai** dynasty which later moved to the Peninsula. In fact, that name is also prominent in Khmer chronicles of Cambodia where shows some clear similarities to the *CS* story.<sup>98</sup>

Still another beach-head state would have arisen near Sakon Nakhon, since an early Khmer inscription there contains an official title *loñ*, which Kachorn would assimilate to *hlvan*, and a place name *jralen*, which he thinks should be *jralieng*, or the "Chalieng Luang" from where Uthong, according to *PN*, started his journey toward Ayudhya.<sup>99</sup> As for the first point, *loñ*, as a status or rank title, is found throughout the Angkor inscriptions, and if it were identifiable with *hlvan*, it would mean either that there were Thai in Cambodia during Angkor times or that *hlvan* is not a Thai word, a point which Coedès at least denied.<sup>100</sup> As for *jralen=jralieng=Chalieng*, this needs detailed linguistic proof of the sound changes proposed, and in any case the story of *PN*, at least in the published version, has *chajianhlvan* (Chachieng), not Chalieng.<sup>101</sup> The beach-head states then are just what Kachorn concluded, "at best ... legendary stories ... whose potentialities as historical facts remain to be developed".<sup>102</sup>

The Grahi inscription. This Khmer-language inscription of AD 1183 does not, as I pointed out earlier, contain any Thai words, and does not, therefore, indicate "the influence of the Thai" or that "the Thai had penetrated .. (to) the area surrounding Nakhon Si Thammarat". Of course, this was not Charnvit's own idea, and he perhaps thought the point had been proven, but it is the sort of detail which needs to be checked when writing such a dissertation, and at Cornell competent linguists should have been available to point out the risks of such conclusion. [\*A similar blooper has found its way into a new book on the *Prehistory of Thailand*, where it is stated that the A.D. 1167 Pali and Khmer inscription from Nakhon Sawan "is said to be the earliest evidence for the use of Thai in Thailand, as it includes two words, *Phra* (cleric or royal prefix) and *nam* (bring)"; but those two words in Thai are loans from Khmer, and the inscription is entirely in Pali (one section) and in Khmer (the longer section).\*]<sup>103</sup>

97. Kachorn, *op cit*, p 75; Coedès, "Nouvelles données", pp 134-41.

98. Kachorn, *op cit*, pp 40-41; *CS*, pp 73-95. I have discussed the similarities between *CS* and the Cambodian chronicles in Vickery, "Cambodia after Angkor", pp 281, 284-85, 286-91.

99. Kachorn, *op cit*, p 45; C Coedès, *Inscriptions du Cambodge*, (textes), VI, 281-83.

100. Coedès, *Inscriptions du Cambodge* (textes), III, p 7, n 2, Coedès was not referring to Kachorn's study. for occurrences of *loñ*, see Sakamoto, Yasuyuki. nd. *Kodai Kumērugo: KWIC sakuin* (Old Khmer: KWIC index of the Khmer inscriptions in Coedès, *Inscriptions du Cambodge* VIII).

101. *PN* p 72.

102. Kachorn, *op cit*, p 46.

103 Charnvit, p 35, basing his statement on an article by Manit Vallibhotama, to which I referred in Vickery, "The Khmer inscriptions of Tenasserim", pp 52-53, n 8. The fact that Coedès, *Prahjum ū ilā cāru'k II*, found no Thai words in the Grahi inscription should have been sufficient to settle the

The investiture of a Thai chief in 1135<sup>104</sup>. This ‘Thai’ chief, ‘Khun Chuang’, who “was invested by the Emperor of China”, is a cultural hero of several northern peoples, including the non-Thai Khmu<sup>105</sup>, and his existence as a real person at a definite date cannot yet be determined from the evidence. In *TS* the Chuang episode is found in the legendary, probably fictional, part of that chronicle, and, like the version of the Nan chronicle, which Charnvit noted, is not a record of Chinese investiture.<sup>106</sup> Even with respect to “*chotmaihet hon*”, the astrologers’ records, Charnvit draws illegitimate inferences, since that ‘record’ only says Chuang received a Chinese envoy.

Contrary to what Charnvit implies, the ‘Chotmaihet Hon’ are not necessarily more reliable than the *bañśāvatār* or *tāmnān*. As he seems to be aware,<sup>107</sup> none of the extant astrologers’ records were copied before the eighteenth, or possibly the seventeenth, century, and whatever original earlier information they contain has gone through the same process of recopying, editing and reinterpretation as the chronicles. For example, pre-sixteenth century astrologers’ records would have been dated, like inscriptions, in the standard Ayudhyan system of the time, the *śaka* era; and the *cula* dates now found for earlier events must represent in themselves efforts at recalculation or interpretation. Furthermore, had Charnvit looked for the “probably ... great number of such records located in the Bangkok National Library (which) still await study by historians”,<sup>108</sup> he would have seen their dubious nature for himself.<sup>109</sup> To take just the published version to which Charnvit refers<sup>110</sup>, it contains the erroneous dates of the 1157 and *RA* chronicles for the sixteenth-century Burmese invasion of Ayudhya and for the death of King Naray<sup>111</sup>, showing that this ‘astrologers’ record’ was in fact composed after the writing of the 1157/AD 1795 chronicle and copied some of its incorrect dates. The original *Cathmāyhet hor* 157 manuscript also shows some editing at the dates 714 and 771. At the former the original scribe wrote of Tavoy falling to the Thai and at the latter Pegu, but then those

matter. [\*See Charles Higham and Rachanee Thosarat, *Prehistory of Thailand*, p. 206, citing Weeraprajak, *Inscriptions in Thailand IV* [in Thai], 1986, Bangkok, Fine Arts Department.\*]

104. Charnvit, p 35.

105. William A Smalley, “Ciaŋ: Khmu culture hero”, *Siam Society Felicitation Volumes of Southeast-Asian Studies Presented to Prince Dhaninivat* (Bangkok, 1965), 41-54; Smalley, “Khmu”, p 114, in Frank M Lebar, Gerald C Hickey, John K Musgrave, *Ethnic Groups of Mainland Southeast Asia* (HRAF, 1964).

106. Charnvit, p 49, n 18; Vickery, “Lion Prince”, pp 337-38.

107. Charnvit, p 163.

108. *Ibid.*

109. See the list of manuscripts, perhaps not complete, attached at the end of this text.

110. *Cathmāyhet hor*, in *PP*, part VII. It is based on Ms No 157 with some ‘corrections’.

111. By ‘1157 chronicle’ I mean the chronicle written in *cula* 1157/AD 1795, of which the oldest extant version is *Băncandanumāś* (Co’m). Its dates between 1388 and about 1630, as well as some in the latter part of the seventeenth century, are known to be wrong. For the two events in question it has 1556 and 1682, instead of the correct 1569 and 1688. See Prince Damrong’s commentaries on *RA*, *passim*, and Vickery, “Cambodia after Angkor”, chaps VII and IX, for discussion of the Ayudhyan chronicle dates.

names were crossed out and replaced with ‘Pegu’ and ‘Sahtòy’ [Thaton?] respectively. Which represents the original ‘astrologers’ records’? No such events are mentioned in the Ayudhyan chronicles.

There is still more evidence of editing and late composition among the astrologers’ records in the National Library. For example, manuscript No 159.2 reports the coronation of Rama I in *cūla* 1144 [1782] using the title “... brah̄ ayakā”, ‘grandfather’, showing that the record was not composed until the reign of Rama III. In No 159.1, at the end of entries for the year *cūla* 1129 [1767], one finds the note, “there was an intercalary day (*adhikavār*), but the astrologers did not record it”, thus apparently leaving one of their main tasks to be completed by a layman. Manuscript No 158, for the year *cūla* 1124 [1762], explicitly quotes a long passage from a chronicle, and at the date 1176 [1814] inserts a comment that the information found there about the appointment of 22 *kram* is not correct and that one should check a certain book published in [B.E.] 2457/1914<sup>112</sup>.

Furthermore, the astrologers, even when they were really drawing up genuine astrologers’ records, were quite capable of calculating dates and entire calendars back to *cūla* era 1, and indeed did so. Among the documents in the National Library is a calendar for the year *cūla* era 1, which was never in use in Siam, if indeed anywhere. There is another one for the year *cūla* 712, traditional date of the founding of Ayudhya<sup>113</sup>. Moreover, the dates of these calendars are calculated by methods in use in recent times, whereas there is some evidence that in earlier centuries different rules for the calculation of calendars prevailed<sup>114</sup>. As for the accuracy of even apparently genuine dates, the extant records contain two different dates for the death of King Paramkoś<sup>115</sup>, three dates for the death of King Rama I of the present dynasty<sup>116</sup>, and two dates for the death of his son Rama II<sup>117</sup>. In view of all this it is impossible to give any special weight to the evidence of these documents for dates in the early Ayudhya period.

### Early ‘Thai’ practice in recording dates.

112. The title given for the book is *Rōan tan brah̄ paramvanśanuvañs* (‘Story of appointments of royal family members’).

113. *Patidin*, MSS Nos 7 and 8 respectively.

114. Roger Billard, “Les cycles chronographiques chinois dans les inscriptions thaïes”, *BEFEO*, LI, 2 (1963), 401-3. On p. 409 he speaks of difficulties in calculating certain dates and attributes the complicated system to a “reform, certainly that on which Lut’ai prides himself, later obscured ...” and in a personal communication (letter dated 26 Feb 1973) mentioned that “in the last centuries in Indochina, there arose in the luni-solar calendar an inconsistency...”. For evidence that the compilers of the astrologers’ documents used the modern system of calculation, even for early times, one may note the basic elements for the year 712, given in *Patidin* No 8, which agree with those found in Billard’s table, p 418.

115. No 158, 1120/1758, Tiger Year, Monday, first of the waning moon, eighth month; No 159 (1), 1120/ 1758, Tiger Year, Tuesday, fourth of the waning moon, fifth month.

116. No 159 (2), 1170/1808, Snake Year, Thursday, thirteenth of the warning moon, ninth month (1170 was a Dragon, 1171 a Snake Year); ,..., Thursday, fourteenth of the waning moon, ninth month.

117. No 158, No 159 (2), 1186/1824, Monkey

There is one more bit of ‘evidence’ for early Thai presence which Charnvit did not use, but which has since appeared in print, and it would be well to lay it to rest immediately before it takes on the character of established fact and begins to support further inferences.

In a generally excellent attempt to search for new interpretations in early Southeast Asian history which has been published by the University of Michigan<sup>118</sup>, we find statements to the effect that the animal dating cycle of a Phimai inscription of AD 1041 is “typical of later Thai practice and nowhere else encountered in Khmer epigraphy”<sup>119</sup> and this, together with the Grahi inscription of AD 1183 which has the same cyclical dating system, is therefore “early evidence of Thai-speaking peoples who were administratively incorporated into the Khmer government of Sūryavarman I”, showing that “Thai-speaking peoples had reached the lower Chao Phraya valley and the peninsula by the late twelfth century”, apparently via “a communication network connecting Phimai and Lopburi”.<sup>120</sup>

[\*Since Charnvit in later work has confused the situation of Sūryavarman I and Lopburi, it may be helpful to insert a comment on this matter here. There is indeed an inscription of that king in Lopburi, but it is not “the earliest evidence of Khmer influence in the central plain”. It may be the earliest sign of Angkor influence, but there are earlier Khmer inscriptions from that area. It is absurd to evoke old legends of a King of Nakhon Sithammarat capturing Lopburi in 922, and the source, *Jinakā lamālī*, is not ‘Thai’ in the proper sense, but Pali of Chiang Mai; and to evoke the old canard of Sūryavarman I of Angkor as son of that peninsular king before he became king in Angkor shows lack of attention to the relevant literature, already 15 years old when Charnvit wrote the article in question.\*]<sup>121</sup>

The phrase of the Phimai inscription which contains the mention of an animal cycle is “953 śaka masañ nakṣatra śukravāra”, or “year 953 of the śaka era, snake nakṣatra, Friday”<sup>122</sup>. Presumably ‘Thai practice’ refers to the Sanskrit term *nakṣatra* used incorrectly, if Sanskrit usage is taken as the norm, for a year in the animal cycle, and indeed in a way later typical of Thai, as well as Cambodian, inscriptions and chronicles. The rest of the Phimai inscription is in Khmer; and the animal terms of the cycle (here *masañ*), even in later Thai usage, are Mon-Khmer<sup>123</sup>. Thus to say that this usage of *nakṣatra* was Thai in origin makes no more sense than to say that since ‘Dvaravati’ was later incorporated into the official names of Ayudhya and Bangkok, it

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118. Kenneth R Hall and John K Whitmore, eds, *Exploration in Early Southeast Asian History: the Origins of Southeast Asian Statecraft*.

119. *Ibid*, “An introductory essay ...”, by Kenneth R Hall, 4-5.

120. *Ibid*, “Southeast Asian trade ...”, by Kenneth R Hall and John K Whitmore, 317-18.

<sup>121</sup> Charnvit, “Origins of a Capital”, p, 60; and for the history of Sūryavarman I, whose origins were certainly within Cambodia, Michael Vickery, “The Reign of Sūryavarman I and Royal Factionalism at Angkor”.

122. G Coedès, *Inscriptions du Cambodge*, VII, 124-26.

123. G Coedès, “L’origine du cycle des douze animaux au Cambodge”, who said they were Muong, borrowed by the Khmer in the eighth to eleventh centuries; but the occurrence of most of the terms in other Mon-Khmer languages as well means that they may go back to common Mon-Khmer.

also must have originally been used by the Thai.

In addition to the Phimai and Grahi inscriptions *nakṣatra* in this sense is also found in the fifteenth-century Ayudhyan gold-plate **Khmer** inscriptions of Tennasserim, Ayudhya, Pichit, and Suphanburi, which are evidence of continuing **Ayudhyan**, but not necessarily Thai, practice<sup>124</sup>. There is also one inscription in Thai of the same type from Phitsanulok, but it dates from AD 1565 when Khmer and Thai usage would have been thoroughly mixed.

As for the contention that the Phimai-type usage is “nowhere else encountered in Khmer epigraphy”, I have found it in the following strictly Khmer inscriptions of Cambodia, and the list makes no attempt to be exhaustive.

- K.351, *IC* IV, p 191, “914 śaka [992 AD] .... **ron**[‘dragon’] nakṣatra”.
- K.618, *BEFEO* XXVIII, P 56 AND *IC* III, p. 151, n.3  
“948 śaka [1026 AD] ... **khāl** [‘tiger’] nakṣatra”.
- K.470, *IC* II, pp 187-89, “...**thoh** [‘hare’] nakṣatra”, probable date 1327 AD.
- K.830, 1028 śaka [1106 AD]... **co** [‘dog’] nakṣatra
- K.465, Phnom Bakheng, unpublished, “1505 sak [1583 AD] **mame** [‘goat’] nakṣatra”
- K.39, Vat Bati, “1496 sak [1574 AD] **ca** [‘dog’] nakṣatra”.
- K.261, Siemreap, “1561 sakkha [1639 AD] **thoh** [‘hare’] nakṣatra<sup>125</sup>.

It is also instructive to take a look at indubitable Thai usage, in the early inscriptions of Sukhothai, where there is no dispute about the ‘Thainess’ of language and culture.

The allegedly earliest dated Thai inscription, that of Rām Gāmhaen, does not use the term *nakṣatra* at all. Thereafter, in No III, we find “1279 [AD 1347] pī rakā to’ an pet... **purbaphalguni** nakṣatra”. Here the place of the term ‘nakṣatra’ follows Sanskritic usage, designating a sign of the zodiac, *purbaphalgunī*, not the animal year (here *rakā* [‘cock’]) as in the Phimai-type usage. The same thing is found in No V, of AD 1361, face III, lines 23-24, “buddhabāra dai rvan plau **punarbbasu** nakṣatra,”<sup>126</sup> and the Phimai-type usage only appears in Sukhothai later on in the fourteenth century when there seems to have been strong Ayudhyan influence there<sup>127</sup>.

124. Vickery, “The Khmer inscriptions of Tennasserim”, in particular the table of inscriptions. following p 62, in original publication.

125. The readings of K 465, K 39, and K 261 were made from the rubbings now kept in the Bibliothèque Nationale in Paris. The animal terms are respectively *ron* (dragon), *khāl* (tiger), *thoh* (hare), *mame* (goat), *ca* (dog), and *thoh*.

126. See these inscriptions in *Prahjum śilā cāru ’k syām* I.

127. See the gold plate inscription published in *JSS*, LX, 1 (Jan 1972), p 147; and note here the use of the 60-year cycle (*rvan plau*), here called *dai/thai*. See also numbers 37, 46, 49, 93; and at even later dates numbers 13, 14, 15, all in *Prahjum śilā cāru ’k* I, III, IV.

If we look even farther north, to an area which was perhaps even more purely Thai, we find that a fifteenth-century inscription from Keng Tung shows in one passage Sanskritic usage of the zodiac plus the Mon-Khmer animal term, while in another passage the word ‘*nakkhatta*’, is coupled with *vaisakh*, one of the signs of the zodiac, again reflecting proper Sanskrit practice, not the system found at Phimai<sup>128</sup>.

The proper interpretation of all this, it seems to me, is that the Phimai-type usage of *naksatra* has nothing to do with the Thai, but was a style developed in a provincial Khmer region outside Angkor proper and including pre-Thai central Siam, from where it later spread both to real Thai areas and to the central part of Cambodia.

In addition to the lack of specific evidence for the presence of Thai speakers in any part of Siam before the Sukhothai period, there is a rather impressive amount of documentation showing that Ayudhya and part of the Peninsula remained linguistically Khmer until much later. Nearly the entire, admittedly small, corpus of Ayudhyan inscriptions from the fifteenth century and earlier is in Khmer, Khmer was still used in official documents of the Phattalung area as late as the seventeenth century, and these, together with the several Khmer inscriptions from apparently non-Angkorean polities in the Menam Basin in earlier times, and the evidence of local styles in Khmer writing, show that the persistent Khmer usage was due to local traditions rather than influence from Cambodia<sup>129</sup>.

It must be emphasized in conclusion that at the present stage of research there is no single piece of acceptable evidence which shows a specific Thai presence in any part of Siam before the thirteenth century, although their presence in Sukhothai and farther north one or two centuries earlier is a reasonable *a priori* supposition, and one which I would support.

Nevertheless, all of Charnvit’s statements about interregional relations which depend on the presence of Thai speakers,<sup>130</sup> at least before the Sukhothai period, are nothing but more or less useful hypotheses of the sort that normally precede historical investigation but which are not acceptable as conclusions at the present time; and for the Ayudhyan and peninsular areas they are contradicted by such contemporary evidence as exists.

A critical discussion of all such hypotheses would encumber too much space, but there are two which should finally be laid to rest, namely, “Sukhothai... in the second half of the thirteenth century, had used Nakhon Sithammarat as a maritime outlet”, and “Nakhon Sithammarat depended upon Lamphun’s rice which was brought down by relatives”, presumably before the thirteenth century<sup>131</sup>.

Charnvit gives no sources for the first statement, but it is probably based on the territorial epilogue to the Rām Gāmhaen inscription, a passage from the Sihing

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128. See G/P, note 91 above, face II, lines 39-30, and face III, lines 8-9.

129. Coedès, “Nouvelles données”; Vickery, review of *Prachum phra tamra ... phu'a kalpana ...*”, in JSS, LX 1 (Jan 1972), 403-406; and Vickery, “The Khmer inscriptions of Tenasserim”, including further bibliography; [in this volume, pp. 00-00](#).

130. In his sections on “The nature of the Thai movement”, 36-39, and “Marriage relationships”, 39-41.

131. Charnvit, pp 39 and 49, n 22 respectively.

Buddha story, and perhaps certain statements from the chronicles of Nakhon Si Thammarat<sup>132</sup>. Whatever the worth of these sources proves to be, a glance at a map shows that there was no possible direct connection between Sukhothai and Nakhon, that water routes from Sukhothai could only descend the Tha Chin or Chao Phraya Rivers through Suphanburi or Ayodhya/Ayudhya, which latter ports, or some other center on the same routes, must have been Sukhothai's maritime outlets. Nakhon Si Thammarat would only have been reached by sea from one of the northern gulf ports and at best could have been a regular secondary port of call for Sukhothai ships using those ports . Moreover, it is well to emphasize '**could have been**', for nothing we believe we know about the trade and economy of Southeast Asia in the thirteenth century shows that regular and important contact between Sukhothai and Nakhon was likely<sup>133</sup>.

The second hypothesis is even less credible. Nakhon Si Thammarat has a larger rice area than Lamphun<sup>134</sup>, and even with the better transport facilities of today it would be difficult to feed the former with rice from the latter. Even if such a story is included in the *Tāmnān mūlaśāsanā*, cited by Charnvit, belief in such sources, however well they may resist general criticism, must always be tempered by consideration of the objective geographical and economic probabilities<sup>135</sup>.

### The birth of Ayudhya

In his fourth chapter Charnvit initiates an interesting investigation of sources, but then draws tendentious conclusions. Besides a discussion of historiography, he also makes decisions about the factual background of early Ayudhya, proposes a political and dynastic explanation of its foundation, and at the end of the chapter attempts to justify his choices with reasoning from the realm of the philosophy of history.

The greatest merit of this chapter is in showing that there are at least six different stories concerning the origins of Uthong, rather than the single version

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132. On the Rām Gāmhaen epilogue see G/P, *EHS* 9, *JSS* , LIX, 2 (July 1971), 193-96, 218-20; *EHS* 10, *JSS*, LX, 1 (Jan 1972) 26-47. See the *Jinakālamālī* version of the Sihing Buddha story in Coedès,, “Documents sur l'histoire politique et religieuse du Laos occidentale”, 98-99; and see Wyatt, “Crystal Sands”, 86-87, 142, 186.

133. For the patterns of trade in early Southeast Asia see Wang Gungwu, “The Nanhai trade”; O W Wolters, *Early Indonesian commerce*; Wolters, *The Fall of Srivijaya in Malay History*; Kenneth R Hall, “Khmer commercial development and foreign contacts under Suryavarman I”, *JESHO*, XVIII (1975)

134. See Frank J Moore, *Thailand* (HRAF, 1974), table 15, p 560 for comparative rice areas in regions of Thailand; W.A. Graham, *Siam*, vol II, (London, 1924), p 6 describes evidence that the Chaiya area, bordering Nakhon Si Thammarat, shows signs of a much larger area of rice cultivation in the past.

135. G/P, who have used *Mūlaśāsanā* for their own historical reconstructions, admit that it is “based on a very defective manuscript”, and that the printed edition “added several mistakes of its own” (*EHS* 10, *JSS*, LX, 1, Jan 1972, 53-54). It is likely that in the section concerning Lamphun and Nakhon Si Thammarat the old name for Nakhon Si Thammarat (Sridhammanagara) has been confused with the old name for Thaton (Sudhammanagara or Sudhammapura). See Coedès, “Documents”, pp 80, n 3 and 160.

accepted by official historiography. These versions are: (a) Uthong came to Ayudhya from the area of Sawankhaloke; (b) Uthong, son of a wealthy man, married the daughter of a king of Ayodhya, forerunner of Ayudhya; (c) Uthong came from Petchaburi, where he was already king; (d) Uthong was son of a wealthy man in Kamboja or Kambuja where he married the king's daughter and then moved south to found Ayudhya, a story which, except for the geographical location, is the same as (b); (e) Uthong was a Chinese who landed at Pattani, built the major cities of the Peninsula, and finally founded Ayudhya; and (f) Uthong descended from a ruling family of northern Siam<sup>136</sup>.

Among these stories Charnvit has opted for Petchaburi as the place from which Uthong came to Ayudhya, a decision which is as acceptable as any other, and certainly preferable, given what we now seem to know about the organization of Sukhothai, to the story of his extreme northern origin. However, Charnvit has again failed to criticize his sources, and has assumed the first thing which needs proof, that there was ever a Prince Uthong at all, or that the first King of Ayudhya must have come from somewhere else in the middle of the fourteenth century. I intend to discuss this in some detail at the end of this study, and only wish to note now that given the extreme disagreement among the sources it would be equally legitimate to conclude that no factual information has been preserved about Ayudhyean origins, and that all such stories are legend. Even the "undisputed facts" concerning his birth found in the 'astrologers' records' may be no more reliable than any other information<sup>137</sup>.

Together with discussion of Uthong's origins, much of chapter IV concerns his marriage alliances, which are used as part of an explanation of Ayudhyean emergence "as the result of the prevailing political situation .... it was the result of the decline in military power of the two earlier dominant states of the area, Sukhothai ... and Angkor".<sup>138</sup> Uthong, taking advantage of the favourable situation, enhanced his influence by contracting marriages with the two leading families of the central and lower Menam area.

I intend now to investigate these marriage stories carefully, both for their relevance as factual history and also for their historiographical interest in connection with Charnvit's use of sources.

In the first sketch of his argument, Charnvit says, "one of his wives was a princess of Suphanburi", and he had apparent, but uncertain connections with the ruling house of Lopburi<sup>139</sup>. Later Charnvit says that *PN* and *BM* show Uthong marrying into a local family in Ayodhya or Kamboja Pradeśa, and he "also married

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136. These versions are found respectively in *PN* (1 and 2), *Gāmhaikār*, *BM*, van Vliet, and the Bangkok *bañśāvatār* tradition as represented by *Culayuddhakāravaṇś* and *Saṅkhep* (see David K Wyatt, "The abridged royal chronicle of Ayudhyā of Prince Paramanuchitchinōrot", *JSS*, LXI, 1 (Jan 1973), 25-50).

137. See discussion of astrologers' records above, pp. 15-16.

138. Charnvit, p 52.

139. *Ibid.*

into the ruling house of Suphanburi”.<sup>140</sup> A few pages later this theory becomes more definite with, “his marriages to two princesses, one from Suphanburi and the other from Ayodhya which was connected to the ruling family of Lopburi”,<sup>141</sup> which latter ‘connection’ is a gratuitous supposition by Charnvit. On page 70 he is again less definite speaking of “his marriage alliance with the houses of Suphanburi and Ayodhya, Lopburi or Kamboja Pradeśa”, but in a subsequent chapter he definitely “married a princess of Ayodhya, as described by” *PN* and *BM*, even though earlier on *PN* had only “hinted” at this and *BM* had spoken of Kamboja Pradeśa<sup>142</sup>.

Let us first take the case of Uthong’s ‘Suphanburi princess’. Scholars who accept this as fact usually refer to *RA*, which says the first king of Ayudhya “let *khun hlvan banua*, who was the elder brother of his queen, and whom he called ‘elder brother’”, be ruler in Suphanburi<sup>143</sup>.

*RA*, however, is the latest version of a chronicle tradition beginning in 1795 and of which several earlier versions are extant. The earliest, represented by the *Bāncāndanumāś* chronicle, has nothing like the passage just quoted. Neither does the *Brah Bānarat* text or Bradley’s original two-volume publication<sup>144</sup>. But all of these texts, from the earliest to the latest, contain in a later passage describing war with Cambodia in about 713-14/AD 1351-52, a statement that “Samtec Braḥ Paranarājā dhirāj Cau, who was the royal elder brother” was called from Suphanburi to aid Prince Rāmeśuor in battle<sup>145</sup>. This apparently genuine passage of the entire chronicle tradition thus has an intriguing and troublesome detail about an elder brother of Ayudhya’s first king who was only a provincial governor. This cries out for explanation, and is ‘explained’ in *RA* with the earlier inserted statement that he was really the queen’s elder brother, even though the king also called him by those terms.

This insertion is also found in the somewhat earlier *Sankhep* (‘summarized’) chronicle written by Prince Paramanujitjinoras in 1850<sup>146</sup>, and we are entitled to wonder whether it was simply **his own** attempt to explain a family situation which seemed anomalous to nineteenth century royal eyes.

If so, it was an explanation which he did not pull out of thin air, but which represented an interpretation of certain passages in works of his mentor, Samtec Braḥ Bānarat of Văt Braḥ Jetuban. In the latter’s *Saṅgūtiyavaṇīś*, written about 1788-89, there is one brief statement that Paramarājādhirāj was the maternal uncle (*mātulā*) of Uthong’s son, Rāmeśuor, and in his contemporary *Culayuddhakāravaṇīś* he wrote that Paramarājādhirāj was indeed brother of Rāmādhipati’s wife<sup>147</sup>. That these statements

140. *Ibid*, 66.

141. *Ibid*, p69.

142. *Ibid*, pp 88 and 66 respectively.

143. *RA*, p 67.

144. For a discussion of these chronicles and their filiation, see Vickery, “Cambodia after Angkor”, chaps VII, IX, X.

145. *RA*, pp 67-68.

146. See Wyatt, “The Abridged Royal Chronicle”.

147. *Saṅgūtiyavaṇīś*, p 375; *Culayuddhakāravaṇīś*, pp 27, 30.

are interpretations, not part of older written records, seems clear from the fact that Samtec Braḥ Bānarat, unlike later less careful writers, did not try to include them in his version of the chronicle, which he is believed to have written about 1807, and which “became, in a sense, definitive”.<sup>148</sup>

I have earlier shown that Samtec Braḥ Bānarat’s version of Ayudhyan history included in *Sangītiyavaniś* was not entirely original with him, but was an adapted version of the Thai-language chronicle, probably dating from the reign of Prāśād Dōn and preserved in van Vliet’s Dutch translation<sup>149</sup>. There it is explicit that Rāmeśuor was deposed by his **father’s** brother, and “the uncle was declared and crowned king ... [and] since that time there has been a law in Siam that at the death of a king his brother inherits the crown”.<sup>150</sup>

Of course one could argue that van Vliet misunderstood one of the Thai kinship terms (*mātulā, lun*) and that the Thai original from which he worked really had ‘maternal uncle’ rather than ‘paternal uncle’. This is unlikely, since van Vliet insists on the point, undoubtedly because, as he wrote elsewhere, the custom of succession from brother to brother was considered important in seventeenth-century Siam<sup>151</sup>, and he would probably have checked this detail of translation rather carefully. We must conclude that the evidence for Paramarājādhirāj being brother of Rāmādhipati’s queen is not very strong.

However that may be, the argument for Rāmādhipati’s queen to be considered as a member of Suphanburi royalty is an entirely different matter. The oldest text which refers to this question at all is *Culayuddhakāravaṇś*, pages 27 and 30. Although it says that Paramarājādhirāj was brother of Rāmādhipati’s wife, they did not originate from Suphanburi. After the foundation of Ayudhya, Paramarājādhirāj was brought from *Kruñdeb mahānagar*, an earlier residence of Uthong, and only then appointed to rule in ‘Suvarṇabhūmī’. The *Sankhep* and *RA* chronicles in fact also say only that Paramarājādhirāj was appointed to Suphanburi after the founding of Ayudhya. There is thus no indication that Paramarājādhirāj, or his putative sister, Rāmādhipati’s queen, had any connection with Suphanburi or its royalty until he was appointed to that post after Ayudhya’s foundation; and any interpretation which argues that in order to receive such an appointment he must have been from old Suphanburi royalty goes beyond the limits of legitimate inference.

We should perhaps nevertheless take a look at Prince Damrong’s interpretation of these events, which was used as support by Charnvit and has probably influenced

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148. Wyatt, “The abridged royal chronicle”, p 27; although not so definitive as to escape drastic alteration by the compilers of *RA*.

149. Vickery, review of van Vliet, *The Short History of the Kings of Siam*, in *JSS*, LXIV, 2 (July 1976), 207-36..

150. Van Vliet, p 60. In n. 35 to van Vliet, Wyatt wrote: “all other sources indicate that his (Ramesuor’s) successor was the elder brother of Ramathibodi’s queen”: and he cited *RA*, ignoring “all the other sources” preceding *RA*, which contain no such information.

151. Jeremias van Vliet, “Historical account of Siam in the 17th century”, *JSS*, XXX, 2 (1938), 96.

other writers as well<sup>152</sup>. Prince Damrong accepted that Paramarājādhirāj was both brother of Rāmādhipati's queen and originally from Suphanburi. His reasoning, however, shows he was aware of the lack of real evidence which I have demonstrated above. First, he argues, Paramarājādhirāj was son of a former 'Uthong', lord of *mo'an/muang* Uthong, and Paramarājādhirāj himself had governed Suphanburi as a province under his father's capital<sup>153</sup>. This was based on Prince Damrong's belief that Uthong had come from *mo'ari* Uthong, the earlier capital, to found Ayudhya, an interpretation now obsolete<sup>154</sup>, and on his own construction of a dynasty of 'Uthong' kings leading up to the one who founded Ayudhya. This construction was a way to account for the widely differing dates associated with Uthong in various *Tāmnān*, but it is an example of the 'epicyclical' fallacy and cannot be accepted<sup>155</sup>.

Evidently Prince Damrong was not entirely satisfied with this explanation, for he also proposed, or rather hinted at, another. According to this one Paramarājādhirāj might not have been appointed until after King Uthong founded Ayudhya. At that time he wished to appoint Paramarājādhirāj to govern Uthong, but since there was trouble in Uthong, Paramarājādhirāj was appointed in Bāndhumpurī, which according to the 'old *Traibhūmi*' and certain other old texts was the former name of Suphanburi<sup>156</sup>. Now the *Traibhūmi*, as a Sukhothai composition, may not be the best source for such details of Ayudhyan history, but another text which includes a story such as the one hinted at by Prince Damrong is *PN*, accepted by Charnvit, but elsewhere rejected by Prince Damrong<sup>157</sup>

One of the tales of *PN*, entitled "The Story of Brahma Cau Uthong", starts with a certain ruler of the lineage of 'Nareśuor of Hanśāvafī', who had several *vāt* built, one of them being Vāt Brahma Pālelaiyk in *mo'an* Bāndhumpurī, which was a bit later renamed *mo'an* Sònbanpurī<sup>158</sup>. Since a Vāt Brahma Pālelaiyk is a famous site in Suphanburi it is possible that Bāndhumpurī in this story was really intended as Suphanburi, and that the change of name, due to the ordination of 2,000 (*sòn bǎn*) men, prefigured a series of tortuous folk etymologies leading to 'Suphanburi'.

However, the expected next step is missing from *PN* which continues, just after the date 563/AD 1203, with the story of Uthong, who with his elder brother (*jeṣṭhā*) and son(s) and family came from *mo'an* Chajianhlvan to Savarrgadevalok and on down to the *kruñ*, presumably Ayudhya, or Ayodhya, or an immediate predecessor in the same region. Uthong then appointed his elder brother to rule in *mo'an*

152. Charnvit, p 72, n 2.

153. Damrong, *RA*, p 240.

154. On the obsolescence of the '*mo'an* Uthong theory' see Charnvit, pp 56-58.

155. See postscript II below, 'The epicyclical fallacy'.

156. Damrong, *RA*, p 240.

157. See above. I have been unable to find mention of 'Bāndumpurī' in the currently published *Traibhūmikathā*; and it is not listed in the Index to the French translation by G Coedès and C Archaimbault, *Les Trois Mondes*, Publ EFEO, Vol LXXXIX, Paris, 1973.

158. *PN*, p 72.

Sugandhagirī and appointed Braḥ Cau Dòn Lăń Rāj, son of the elder brother, as the first succeeding ruler, apparently after Uthong's death. Now since Dòn Lăń /Cāndr is the name of Paramarājādhirāj's son in all the *bañśāvatār*, we could reasonably argue that Sugandhagirī is to be understood as the next phase of Sònbānpuri and a precursor of Suphanburi, although such is not stated, and later on Braḥyā Sugandhagirī is also called 'Cau Mo'an Jian Hmai', or 'Bijāy Jian Hmai'. Nevertheless, at Uthong's death he was succeeded by Cau Jaiyasen, son of the ruler of Bijāy Jian Hmai<sup>159</sup>, apparently the elder brother mentioned earlier<sup>159</sup>.

This seems to be the story which Prince Damrong had in mind, but there is no way, when the story is read as a whole, to argue for an old connection between Paramarājādhirāj and Suphanburi, and the entirely fictitious character of the previous names of Suphanburi is revealed by the twelfth-century Phra Khan inscription at Angkor which uses 'Suvarṇapura' for Suphanburi<sup>160</sup>.

Uthong's second important marriage, according to Charnvit, was into the royal family of Lopburi. This is first suggested by the appointment of his son Rāmeśuor to rule in Lopburi, which must have been done because of family connections. Charnvit also interprets two *Tāmnān*, *PN* and the introductory section of *BM*, as support. The latter says unequivocally that Uthong married a princess of Kamboja, which Charnvit says, reasonably, was some part of the central Menam Basin<sup>161</sup>. The story in *PN*, however, which Charnvit considers, again quite reasonably, as another version of the story included in *BM*, says Uthong married a princess in the city of Phraya Kraek,<sup>162</sup> which Charnvit believes was Ayodhya. Now it is true that in one of *PN*'s tales Phraya Kraek did appear as king in Ayodhya in about AD 1307,<sup>163</sup> but there are several stories of Kraek in *PN* and he is also found in the legendary history of Cambodia.<sup>164</sup>

In the story under consideration here Uthong married a princess in the city of Phraya Kraek at a time about three generations after Kraek's death<sup>165</sup>. Then he

159. *PN*, p 80. We should note here that 'Jian Hmai' ('new town' or better, 'new burg') does not necessarily mean the northern town of that name, and is not necessarily anomalous in the lower Menam region. In the early sixteenth century Ayudhya was known to Arab traders as *Shahr-I Naw*, "Persian for 'new town'" (Paul Wheatley, *The Golden Khersonese*, Kuala Lumpur, 1961, p 235, n3); and over 150 years later the same name was still used by Persians, but had become corrupted to 'boat town', 'shahr nāv' (John O'Kane, trans, *The Ship of Sulaimān*, pp 4, 88) [\*One might speculate that in fact the oldest variant was 'boat town', given Ayutthaya's origins as a port (see Ishii and Baker, above n. 61), and that the 'corruption', a hypercorrection, was in the other direction.\*].

160. The latest statement in support of this view is MC Subhadradis Diskul, "Notes on recent excavations at Prasat Muang Singh", *JSS*, LXVI, 1 (Jan 1978) p. 110.

161. Charnvit, p 66. See also the discussion of 'Kamboja' in Vickery, "Cambodia after Angkor", pp 369-77.

162. Charnvit, pp 91-92, n 35.

163. *PN*, p 38; Charnvit, p 46.

164. E Aymonier, "Chronique des anciens rois du Cambodge", *Excursions et reconnaissances*, IV, 2 (1880) 151, 154, 172.

165. *PN*, pp 72-82.

moved his city to a place 15 days travel southward. He ruled there, in Ayodhya, and sent his three sons to govern *mo'an* Nagar (perhaps Nagar Hlvañ, Angkor), *mo'an* Tahnāv (Tenasserim), and Petchaburi. At Uthong's death an image of Phraya Kraek was brought from *mo'an* Indapatnagar to be set up in Ayudhya.

From these details it is possible to explain both the story of *PN* and that of *BM*. The name '*indapat*' was a well-known post-Angkorean name for Angkor, and other Cambodian capitals, and it has never been found clearly applied to any non-Cambodian location.<sup>166</sup> Thus this story is one of the Cambodian tales of Kraek, and according to it Uthong is to be understood as having married a princess in Cambodia before founding Ayudhya. Of course this does not fit the detail that Uthong then moved southward, but as I have explained in detail elsewhere '*kambuja*' was eventually confounded with '*kamboja*', now recognized as part of central Siam, resulting in confusion and conflation of originally distinct stories<sup>167</sup>. Conflation was particularly easy for Thai writers, since Angkor Vat was also known as *bisnulok*<sup>168</sup> and another Biṣṇulok [Phitsanulok] was one of the main cities of Kamboja, in north central 'Thailand'. The *BM* story in fact still preserves the name '*kambuja*',<sup>169</sup> although the southward direction of Uthong's move indicates that the scene has shifted to Kamboja.

These stories, then, are tales, which even if originally having some basis in fact, have been misplaced from another context and they in no way support a theory of Uthong's marriage into the ruling family of Lopburi. Furthermore, if we accept, as Charnvit does, the other story of Kraek ruling in Ayodhya in 1307, it is impossible to also accept a story situated three generations after Kraek's death, which begins just after c. 565/AD 1203 and which ends with Uthong's death, aged 100, in BE 1600/AD 1057, without at least some very detailed source criticism to systematically explain such impossible chronology. It is also a serious methodological error for Charnvit, who accepted the story of Uthong's Petchaburi origins, to lift details from one of the stories of Uthong's northern origins which he otherwise rejected.

There is thus no serious textual support for the hypothesis of dynastic marriages, as stated, although such may in fact have occurred, and they cannot be used as part of a political explanation of Ayudhya's early development.

The chapter ends with an awkward argument in support of his use of *tāmnā n.*<sup>170</sup> They all, says Charnvit, discuss the founding of Ayudhya in connection with Buddhist mythology, and he quotes Eliade on the necessity for primitive men to

166. Saveros Lewitz, "La toponymie khmère", *BEFEO*, LIII, 2 (1967) 417, 430; Vickery, "Cambodia after Angkor", 214, 237, 291. Note also Wyatt's remarks in *CS*, p 87, n 9.

167. See note 161 above

168. Saveros Lewitz, "La toponymie khmère", 429-30, '*brah viṣṇulok(a)*'; *Inscriptions modernes d'Angkor*, No 2, line 12 and No 3, where the name is spelled 'bisnulok' (in *Silā cārik nagarvatt*, Phnom Penh, Institut Bouddhique, 2501).

169. Recognized by Charnvit with his 'Kamphut Prathet', p 62; but he is in error, p 65, with "the Khmer ... referred to their own country as Kamboja Desa". Khmer usage is '*kambuja*', in origin quite different from '*kamboja*' (see Vickery, "Cambodian After Angkor", pp. 369-377).

170. Charnvit, pp 70-72, from which all the following citations, unless otherwise noted, are taken.

justify important actions as imitations of celestial archetypes. In this way the *tāmnān* strengthened Uthong's legitimization. True, but they may nevertheless be pure fiction. Charnvit states further that "these *tāmnān* versions ... seem to fit with the concept of ancient Thai historiography as described in Chapter I", which is a useless tautology since it was precisely the same *tāmnān* which were the object of discussion in the earlier context. A further daring inference is "it is possible that the events of the origin of Uthong ... took place at a time when a *tāmnān* type of world view dominated and this would direct the writing of the history of Uthong and Ayudhya", which implies, without any attempt at proof, something no one else had dared suggest, that the *tāmnān* stories are somehow contemporary with Uthong, an idea which even the cursory analysis attempted here shows entirely untenable.

Finally Charnvit argues that if "historical action should be seen in the light of its contemporary world views, the action of Uthong and the foundation of Ayudhya must be considered in the light of the *tāmnān* traditions". But first we need some demonstration that the *tāmnān* are contemporary with Uthong, and second that they are to any extent factual. In the writing of history fact must come before interpretation, not the other way around, and if, for example, "Ayudhya history" was not seen "in fact as the successor of Sukhothai" by the *tāmnān* writers,<sup>171</sup> it was not simply because of a different world view or interpretation, but because a whole body of fact concerning Sukhothai, now rediscovered, was unknown to them. The *tāmnān* are certainly interesting as documents of a particular world view and philosophy of history, but they are nearly worthless as direct testimony for the facts of early Ayudhya, which is what we now need and what Charnvit has failed to discover.

### **'Ayodhya: the forerunner of Ayudhya'**

Chapter V is devoted to the problem of an important pre-1351 settlement, 'Ayodhya', which would have partly occupied the site of post-1351 Ayudhya.

Readers unfamiliar with the sources might wonder why the question arises at all and why some Thai writers have devoted so much attention to it<sup>172</sup>. What Charnvit should have explained first of all in this chapter is why Praya Boran/Porāṇ Ratchathanin--who for all his intelligence and familiarity with the site did not have, in 1907, the means to determine methodically that "a pre-Ayudhyan city was situated immediately to the east of the location of Ayudhya"<sup>173</sup>--was concerned with such a problem, and why Prince Damrong seven years later decided a city called Ayodhya "was founded by the Khmer who were ruling at Lopburi" at the point where the

171. Charnvit, p 60. [\*In fact, Ayudhya "as the successor of Sukhothai" is a modern royalist-nationalist myth\*]

172. In addition to the remarks of Brahyā Porāṇ and Prince Damrong, cited below, see Srisak Vallibhotama, *Ayudhya in history* [in Thai], *Saṅgamśāstr paridaśn*, Special Vol 3 (June 1966), 58-87; No ṇa Paknam, three articles cited in Charnvit's bibliography plus "Five months among the ruins in Ayudhya" [in Thai].

173. Charnvit, p 76; Bra:ya porāṇ rājadhānindr, "Story of the old Capital" [in Thai], *PP*, part 63, vols 36-37. See vol 37, pp.1-2, 26.

“three rivers, the Pasak, the Lopburi, and the Menam Chaophraya meet”, that is, at the very place where traditional Ayudhyan chronicles place the founding of Ayudhya in 1351.<sup>174</sup>

The most probable reason for Phraya Boran’s and Prince Damrong’s conclusions about an earlier city is that in several of the early *tāmnān* -type stories ‘Ayodhya’ is mentioned as an important center in the south a good many years before 1351, the traditional date for Ayudhya’s founding<sup>175</sup>. This posed a real dilemma for traditional scholars who accepted the chronicular<sup>176</sup> statements that Ayudhya had been founded once and for all in 1351 as the result of a royal decision. It looked very much as though the ‘Ayodhya’ of those provincial *tāmnān* had been intended to mean Ayudhya, and if so, it meant that the authors, some as early as the fifteenth-sixteenth centuries, showed no awareness of the importance of ‘1351’.

Outright acceptance of the northern chronicles at face value would have meant the rejection of the opening statements of the official chronicles of the Ayudhya and Bangkok dynasties. If Ayodhya/Ayudhya had existed long before 1351, it could not have been founded at that date by ‘Uthong’. Such a solution was not possible for traditional scholars for whom the stories of 1351 were very nearly sacred dogma.

Why not then assume that the Ayudhya/Bangkok chronicles were correct and the northern ones a tissue of legend? This solution was also unacceptable because other sections of the northern chronicles were essential to the Bangkok scholars’ reconstructions in other respects<sup>177</sup>, and in addition to that, the methodology of traditional historians did not encourage questioning the veracity of sources at all. If two chronicles seemed contradictory, instead of trying to choose between them, a rationalization (epicycle) was devised to cover both.<sup>178</sup>

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174. Charnvit, p 76, quoting an article by Thep Sukratni, who does not cite any of Prince Damrong’s writings; and it is not at all clear from this that such was really Prince Damrong’s opinion, although he did believe in the existence of pre-Ayudhan Ayodhya. See Damrong, *RA*, p 222.

175. Such pre-1351 mention of ‘Ayodhya’, and even of ‘Ayudhya’, is found in *CS*, *Mūlaśanā*, *PN*, *PY*, the Sihing Buddha story, and *Jinakālamālī*, the first five of which were cited by Srisak Vallibhotama, *op cit*, pp 57, 71, 71, 75 who together with No ณ 152 Paknam, ท้าเตื่อน, pp ณ, ณ, ff, posed the Ayodhya problem in terms of its mention in the chronicles. Likewise, in ร่องกรุงเก่า *Brah्या Porān* specifically cited *PN*, but then used ‘Ayudhya’, indicating that for him ‘Ayodhya’ and ‘Ayudhya’ were just variants of a single term.

176. In a communication to Yale University dated 27 Oct 1977, David K. Wyatt complained that ‘chronicular’ did not appear in any of the dictionaries he had consulted. This is true, but I find that A.B. Griswold, in *EHS* 10, p 72, considered the word permissible; and in a communication to Yale dated 8 Nov 1977, on the same subject as Wyatt’s, John W. Hall was able to use ‘chronicular’ without embarrassment. It would seem high time that a word accepted in such distinguished company find its way into the *Oxford English Dictionary*.

177. For example, the northern genealogy of Uthong, which, beginning with *Culayuddhakāravāṇī*, became official doctrine, as emphasized by Charnvit; and the Ayoja-Kamboja warfare of *Jinakālamālī*, which apparently influenced one section of the Bangkok *Baiśāvatār*, as described in Vickery, “Cambodia after Angkor”, pp 377-81.

178 On such epicycles see Michael Vickery, “A guide to some recent Sukhothai historiography” *JSS*,

The problem eventually went beyond a mere choice among chronicles with the realization that certain remains, not in the center of Ayudhya, but a bit to the east, really antedated 1351<sup>179</sup>, and they served to support the explanation that ‘Ayodhya’ meant an earlier city supplanted by Ayudhya after 1351.

It seems to me that the problem has been posed in the wrong way and that the solution is nothing but an ‘epicycle’. The question that should have been asked is whether the statements about the founding of Ayudhya in 1351 did not need reinterpretation and whether Ayudhya, under whatever name, had not been an important center since much earlier.

For a historian not committed to any particular tradition, the important considerations are the following.

(a) The only perfectly certain evidence is the archaeological remains which prove the existence of a pre-1351 city in part of what was later Ayudhya (assuming that the remains have been correctly dated - if closer study by competent archaeologists proves they are really post-1351, my proposed explanation may be false, but then the whole Ayodhya problem fades away, the chronicle references to pre-1351 ‘Ayodhya’ must be accounted legend, and the general value of such chronicles is thereby diminished).

(b) The next best evidence is in the old northern chronicles, such as *Jinakālamāli*, composed by monks with contacts all over the Menam Basin and neighbouring areas, who apparently gave no importance to ‘1351’, and who moreover used the name ‘Ayodhya’ both for pre-and post-1351 events, showing that for them there was only one such city which had existed without break from before that date.<sup>180</sup>

(c) ‘Ayodhya’, with various spellings, is found in a few post-1351 inscriptions, indicating that writers not long after that date considered it a variant of the name now written ‘Ayudhya.’<sup>181</sup>

The conclusion to draw from this evidence, then, is that a center called Ayodhya/Ayudhya began to develop sometime before the fourteenth century, and it became the city known in early modern times as Ayudhya. Against this there are only the opening statements of the Ayudhya and Bangkok chronicles, and without dismissing them we could hypothesize that 1351 marks an important event, the shifting of a palace, or the city center, or even the establishment of a new dynasty, but not the physical foundation of an entirely new city.

Even if one wishes to insist, against the better evidence, that there ‘must have been’ an earlier Ayodhya and a later Ayudhya, the question is of little importance for

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LXVI, 2 (July 1978), pp.182-246

179. See the works of Srisak Vallibhotama and No ṇa Paknam cited above. Note also that the absence of ‘Ayodhyān’ remains from the center of Ayudhya proves nothing about the area of Ayodhya, since they could have been obscured by post-1351 construction.

180. See *Jinakālamāli* in Coedès, “Documents”, pp 95, 100, ‘Ayoja’.

181. Inscription No XI, from the late fourteenth century or early fifteenth, and Nos XLVII and XLVIII from the early fifteenth. See *Silā cāru’k III*; G/P, EHS 10, EHS 11, and “A Pali inscription from Vat Śrīratnamahādhātu, Art and Archaeology in Thailand (Bangkok, BE 2517).

Charnvit's subject, or for the interests of most modern historians, who are concerned with the economic, social, ethnic, and political development of the Menam Basin, not merely with the identification and personalities of kings whose very names may be open to doubt.

What everyone now apparently agrees on is that even if 1351 was an important date, Ayudhya was not founded on wild land, but partly on, or beside, another developed center, thereby continuing a developmental process already begun, perhaps centuries earlier. The stories of Uthong in a literal sense are already falsified by the pre-1351 archaeological remains. The development of Ayodhya/Ayudhya, then, was a single, continuing process and must be studied as such, and the truth about 'Uthong', or '1351', or the dynastic relationships of the time may be undiscoverable.

In his chapter V Charnvit seems to agree essentially with the above analysis and this chapter, resuming a discussion begun in chapter II, could have been a valuable contribution to the study of early Ayudhyan history had he not found it necessary to force his socio-economic hypotheses into the framework of traditional tales.

He starts off by saying that "the founding of the kingdom was probably the culmination of a long process of social and political change",<sup>182</sup> adding later that the region "may have enjoyed still another economic advantage (in addition to abundant food) ... in the field of trade with the outside world, and especially with China",<sup>183</sup> and as a result growth may have been quite rapid.<sup>184</sup> He emphasizes further that 'Ayodhya', the pre-1351 location, because of its favourable situation, could have controlled a large area and its communications, and that naval power may also have been developed<sup>185</sup>. He postulates that the kingdom came into existence as early as the eleventh century, and that "its emergence reflected a wider pattern of political rivalry ... in South-East Asia", and that Uthong's kingdom "was structurally continuous with the old political system of the area".<sup>186</sup>

So far, so good, but then, in spite of describing a center which had been doing very well in its development for about 300 years, which was wealthy and powerful and controlled a large region through a judicious combination of trade, shipping and favourable geography, Charnvit nevertheless felt forced to state that in mid-fourteenth century Ayodhya/Ayudhya needed "a leader to exploit the advantages it offered", that "Ayodhya could become Ayudhya only by a series of political acts under particular circumstances",<sup>187</sup> a statement I find devoid of meaning, but which for Charnvit apparently means "the new Kingdom of Ayudhya was born as a result of coalescence between two old rival *muang* [Lopburi and Suphanburi] engineered by Uthong",<sup>188</sup> even though Charnvit's own previous analysis had demonstrated that the center to be

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182. Charnvit, p 76.

183. *Ibid*, p 79.

184. *Ibid*, pp 81.

185. *Ibid*, pp [82-83].

186. *Ibid*, pp 86 and 88 respectively.

187. *Ibid*, pp 86 and 88 respectively.

188. *Ibid*, p 87.

named ‘Ayudhya’ was already in existence and powerful, and was thus not born of any political act in mid-fourteenth century. This is what I meant about forcing socio-economic analysis into the framework of old tales, for all of Charnvit’s good discussion of the Ayudhyan economic background negates, or at least makes irrelevant, the whole collected body of Uthong stories.

It is unfortunate the Charnvit did not pursue his socio-economic analysis further, for the necessary framework was already in existence and we will not be guilty of criticizing him for neglect of material not yet available.<sup>189</sup> He touched on the crucial details in a brief description of the nature of Chinese trade, based on Skinner,<sup>190</sup> but unaccountably neglected the work of one of his mentors, O.W. Wolters, who in *The Fall of Srivijaya* carefully described the patterns of Chinese trade over the twelfth-fourteenth centuries, and even noted the relevance of those patterns for Ayudhyan history<sup>191</sup>.

According to Wolters, and apparently no one has been able to challenge his analysis, in pre-Sung times China preferred to deal with one principal entrepot in Southeast Asia, a position long held by Srivijaya, then in the Sung and Yuan periods Chinese shipping increased and the Chinese themselves traded directly with a variety of ports, which contributed to the weakening and ultimate destruction of Srivijaya and at the same time encouraged the development of many other ports, including the one which became Ayudhya. Then when Chinese policy, after 1368, reverted to a preference for a single favoured entrepot, several new ports competed for this favoured status, eventually gained by Malacca, but Ayudhya by that time was already strong enough to go its own, different, way, which I shall discuss later.

Any discussion of a pre-1351 city on the site of Ayudhya is complicated by the problem of Hsien, to which I have alluded above and in an earlier article<sup>192</sup>, and which Charnvit has treated in an equivocal manner. [\*Although the work cited with note 61 above shows that the problem is now solved, and that *Hsien* could not have been Sukhothai, but was near the coast, possibly the precursor of Ayudya\*] I shall review here some of its complexities and implications, as presented in the original publication of this article\*].

The original identification of Hsien with Sukhothai, which has persisted until today, was based on the unnecessary assumption that Hsien/*syām*/Siam must mean ethnic Thai and the further assumption that in the twelfth-thirteenth centuries there would have been no other Thai center but Sukhothai<sup>193</sup>. If it were once proven or

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189. It would be unfair to criticize him for mistakenly emphasizing early Ayudhyan agricultural output (pp 77-78), shown unlikely by the latest work on the historical ecology of Thailand, and which in turn reinforces the argument that Ayudhya developed in response to favourable circumstances for international trade. See Yoshikazu Takaya, “An ecological interpretation of Thai history”.

190. Charnvit, pp 8-81.

191. Wolters, *The Fall of Srivijaya*, chaps III, IV, V, and p 67. Charnvit’s bibliography shows he was familiar with this work.

192. Above, note 61; Vickery, “Guide”, pp 204-05.

193. Paul Pelliot, “Deux itinéraires de Chine en Inde à la fin du VIII<sup>e</sup> siècle” *BEFEO*, IV (1904), 131-143; Coedés, *Indianized states*, 190-91. [\*Jit Phumisak based much of his argument in *Gvām*

accepted that there were other Thai centers farther south, or possibly non-Thai centers in an area which could have been known to foreigners as Hsien, then there is no longer any logical necessity to equate Hsien and Sukhothai at all. Thus the rejection of Hsien = Sukhothai should have been easy for Charnvit, and for other writers whom he cites in evidence that there were Thai in the Menam Basin and the Peninsula as early as the eleventh century. [\*It is now clear that this is not true. See Vickery, “Tenasserim”, “Guide”...\*]

A modification which was introduced into the original Hsien theory is that by 1349 Hsien meant Suphanburi, since it is clear that the Chinese were dealing with a center near the coast. The choice of Suphanburi is again based on an assumption--that no other appropriate center existed in the lower Menam Basin; but even *LP*, as Charnvit emphasizes, shows ‘Ayodhya’ to have been rather wealthy as early as 1324<sup>194</sup>.

Moreover, if Ayodhya, as I think Charnvit has convincingly hypothesized, had developed from the eleventh century as an important economic and trading center, then there is no reason why the displaced Hsien of the early fourteenth century (if we accept that theory) could not have been Ayodhya rather than Suphanburi;<sup>195</sup> and since all we believe we know about Chinese contacts with Southeast Asia indicates that they were mainly interested in maritime trade centers, then Hsien must always have been such, and if Ayodhya fulfilled that function from the eleventh to fourteenth centuries, there is no place for an identification of Hsien with Sukhothai. Conversely, if it were convincingly established that Hsien really was Sukhothai, then Hsien/Sukhothai must have dominated the maritime trade of the entire Menam Basin and there is no place for the postulated pre-1351 Ayodhya. It will not do to claim that Hsien was Sukhothai and Suphanburi was its port, for the Chinese wrote of the port with which they dealt, not a distant inland overlord, and the latest research on Sukhothai shows that its control over distant theoretical vassels was very tenuous<sup>196</sup>.

As with so many other problems which arise in Charnvit’s book, he wants to have it all ways, refusing, like traditional historians, to thoroughly criticize his sources. Thus, he accepts the old theory that when Hsien attacked Malayu in the thirteenth century this referred to Sukhothai, but that it was “likely that this fleet was stationed at Phetburi, ... under the control of Suphanburi, ... a dependency of Sukhothai”<sup>197</sup>. At least Charnvit recognized the difficulty of Sukhothai itself sending a fleet to Malayu, but the relationship between Phetchaburi and Suphanburi is pure speculation, forced by Charnvit’s decision about the original Hsien. Later, in mid-

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pen mā khòn gām syām, daiy, lāv, lee: khòm ('Origins of the terms *syām*, *daiy* [thai], *lāv*, and *khòm*'). Bangkok, 1951. DK Books,

on the equation of Thai-Tai and Hsien-Siam, but he thought the original area was farther north.\*]

194. Charnvit, pp 84, 85, 87, and his bibliographic references on the subject.

195. If *Su-mēn-pang* was the Chinese name for Suphanburi, as Wolters at one point would have it, *Chén-li-fu*, p 20, n. 76), then it is was already doubtful that ‘Hsien’ ever meant Suphanburi.

196. The putative relationship between Sukhothai and Suphanburi is implied by Charnvit’s remarks, pp 83-84. The new view of Sukhothai territorial control is in G/P, “On kingship and society”, pp 39-43. See also note 61, above, Ishii and Baker.

197. Charnvit, p 84.

fourteenth century, when Hsien attacked the Singapore area, he accepts that Hsien must mean Suphanburi because of the “maritime nature” ascribed to its people<sup>198</sup>. If that is a decisive argument, then what more proof of maritime nature does one need for the thirteenth-century record than the ability to mount a naval attack against Malayu?

Charnvit then goes on to speculate that the Suphanburi naval power would have passed to Ayudhya as result of Uthong’s marriage connection with Suphanburi, and that the naval power enabled Ayudhya to become powerful rapidly<sup>199</sup>. Apart from the speculative nature of such a marriage, which I have demonstrated above, the statement conflicts with Charnvit’s own analysis of Ayodhyian growth, which, if accurate, implies that Ayodhya was already an important naval power before 1349.

Charnvit has failed to resolve the contradictions between his economic and geographical hypotheses about the origin and growth of Ayodhya/Ayudhya, and the political theory of its foundation, which ignores and negates the former. He tried to combine these contradictory theories, and in this illustrates the phenomenon which I have previously called ‘scholastic involution’ in Southeast Asian history, meaning that a certain framework is taken as given and unassailable, and new discoveries are fitted into it without considering whether the new discoveries may destroy the validity of the original framework<sup>200</sup>.

[\*It is now possible to make a stronger statement on this matter. Let us start with a peculiar statement which Anthony Reid made on two occasions since I wrote the original of this article, that Ayutthaya “does not really appear in the external record as an international trade center...until a maritime Chinese embassy reached the new port in 1370, and recognized it as the Hsien-lo of earlier periods. In particular Sukhothai had conducted a busy tribute trade to China in the period 1280-1323, after which Siam had disappeared from Chinese records for half a century”].<sup>201</sup>

[\*Operating out of his depth here Reid has made contradictory statements. If the Chinese in 1370 recognized Ayudhya as the Hsien-lo of earlier periods, that was precisely the place which traditional Thai historians, followed by traditionalizing western historians of Thailand had identified erroneously with Sukhothai. From about 1280 Yuan-dynasty Chinese records report on, first, *Hsien*, then after 1349, *Hsien-lo*, and in two contexts mention *Su-ku-tai* as a separate entity farther upstream (see note 303 below and associated text)

[\*It is generally accepted now, including by Charnvit Kasetsiri, that *Hsien* and *Hsien-lo* always referred to the area near the gulf of Thailand, if not always specifically to the site of Ayudhya. Charnvit agrees that in the 14th century the

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198. *Ibid.*

199. *Ibid.*

200. Vickery, review of H L Shorto, *A Dictionary of the Mon Inscriptions*, in *JSS*, LXI, 2 (July 1973) 105; and review of Robert B Jones, *Thai Titles and Ranks*, in *JSS*, LXII, 1 (Jan 1974), p. 165.

<sup>201</sup> Anthony Reid, “Documenting the Rise and Fall of Ayudhya as a Regional Trade Center”. In (1) *Proceedings for the International Workshop Ayudhya and Asia*, Core University Program Between Thammasat University and Kyoto University, Bangkok, 18-20 December 1995, pp. 5-14; (2) Anthony Reid, *Charting the Shape of Early Modern Southeast Asia*, Chiang Mai, Silkworm Books, 1999, chapter 5, pp. 85-99. See p. 86.

Chinese intended *Hsien*, their rendering of 'siam'/*syām* (as it was written in Old Khmer and Cham), as a name for the lower Menam basin, including Ayutthaya, not Sukhothai.<sup>202</sup> The first *Hsien*, for instance, could have been at Suphanburi, or some other riverine or coastal port. Reid unfortunately relied on the outdated treatments of Suebsaeng Promboon, and David Wyatt, who asserted boldly that *Sien* (*Hsien*) was Sukhothai.<sup>203</sup> This is Wyatt's only reference to that term, but in citing Zhou Daguan's contemporary report on Angkor, where *Hsien* is prominent, Wyatt used 'Siam'. Nevertheless, when describing Rāmādhipati's settlement of Ayutthaya, p. 66, he called it "a port City of some antiquity", which fits precisely the *Hsien* described by Chinese writers since the 1280s, and which name the Chinese would continue to use for Ayutthaya until modern times. Note also that Zhou Daguan, who was at Angkor in the 1290s, wrote clearly that *Hsien* was **southwest** of Angkor, thus somewhere near the gulf, a detail which caused problems for Coedès and Pelliot.<sup>204\*</sup>]

## Ayudhya after its foundation

Most of chapter VI is a paraphrase of the traditional textbook history of the fourteenth-fifteenth centuries based on the *banśāvatār* and laws, plus some speculative embellishments which are not easily accessible to proof or disproof.

In the very beginning Charnvit is guilty of some serious misuse of evidence with respect to the extent of Ayudhyan territory and the characteristics of the Ayudhyan state.

He starts by saying Ayudhya "claimed control over a vast area of Siam", including 16 major *mo'an*, from Sukhothai in the north to Malacca and 'Chawa' (= Johore?) in the south, even though he admits such claims to be exaggerated<sup>205</sup>. In fact, as Wyatt and Griswold have shown, Malacca did not yet exist<sup>206</sup>, and the Sukhothai kingdom--including Phitsanulok, which was probably not yet so named,

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<sup>202</sup> Charnvit Kasetsiri, "Ayudhya: Capital-Port of Siam and its Chinese Connection in the Fourteenth and Fifteenth Centuries", *Journal of the Siam Society*, Volume 80, Part 1, 1992, pp. 75-80.

<sup>203</sup> Suebsaeng Promboon, "Sino-Siamese Tributary Relations, 1282-1853", Ph.D., Michigan, 1971; David Wyatt, *Thailand: A Short History*, p. 58.

<sup>204</sup> See Michael Vickery, "Coedès' Histories of Cambodia", Colloque George Coedès aujourd'hui, Bangkok, Centre d'Anthropologie Sirindhorn, 9-10 September 1999, forthcoming [2002] in *George Coedès aujourd'hui*, Louis Gabaude & Pensiri Charoenpote, eds. See also Tatsuro Yamamoto, "Thailand as it is referred to in the Da-de Nan-hai zhi at the beginning of the fourteenth century", *Journal of East-West Maritime Relations*, Vol. 1 (1989), pp. 47-58; Geoff Wade, "The Ming Shi-Lu as a Source for Thai History 14th to 17th Century", paper presented at the 5th International Conference on Thai Studies-SOAS, London, 1993,-p. 25. I wish to thank Dr. Wade for reminding me of this information. In his lecture to the 2002 International Thai Studies Conference in Nakhon Pathom, Prof. Yoneo Ishii, confirmed the new consensus that the area the 13<sup>th</sup>-14<sup>th</sup> century Chinese knew as Hsien/Sien/Siem was near the coast, not at Sukhothai; and the controversy should now be considered settled.

205. Charnvit, pp 93-94.

206. David K Wyatt, "The Thai 'Kata Mañḍiarapāla and Malacca'", *JSS*, LV, 2 (July 1967), 279-86; G/P, *EHS* 11-1, p 74.

Phichai, Phichitr, and Kamphaengphet--was still independent.

Moreover, the territorial list to which Charnvit refers is only found in the Bangkok chronicles, not in the Ayudhyan *LP*, nor in the even earlier van Vliet chronicle. It is thus surprising evidence for the situation of fourteenth-century Ayudhya, and may not even represent an early Ayudhyan claim.

Even while admitting the poor credibility of this list, Charnvit still seeks to defend some of its claims in general terms, for example, arguing that Suphanburi, inherited by Uthong, controlled Nakhon Si Thammarat, although this is a hypothesis which is based ultimately on an arbitrary rewriting of *CS*<sup>207</sup>.

His final conclusion on this subject, that Ayudhyan territory was bounded by Chainat, Chanthaburi, Tenasserim, and Nakhon Si Thammarat is acceptable as a hypothesis<sup>208</sup>, but then it was not necessary to give serious consideration to the 16-*mo'ah* list, which has been rejected by historians for several years.

A bit of risky speculation, not entirely Charnvit's fault, concerns Uthong's "shaky claim to territory in Lower Burma", perhaps explained by his connection with Suphanburi. Charnvit has taken this from Griswold and Prasert, who combined the Mon chronicle *Rājādhīrāj*, Phayre and their own speculations to make the following story<sup>209</sup>.

Martaban, under its first Mon dynasty, was subordinate to Brahma Ruan in Sukhothai for three generations of kings, from 1287 to about 1319. A few years later 500 Thai from Phetchaburi, "a town under the control of Suphanburi",<sup>210</sup> arrived in Martaban as volunteers, eventually killed the Mon king, and replaced him with their own leader who was then assassinated by the local royalty. After this, the "King of Siam" sent troops, who were defeated<sup>211</sup>. Griswold and Prasert say the 500 Thai were a fifth column sent by the king of Suphanburi and that the second Siamese force was also from Suphanburi. However, the incident of the second Siamese force, in which there is no mention of Suphanburi, is only found in Phayre's translation of a Burmese version of Mon history, and it corresponds to a statement in *Rājādhīrāj* that "ties of friendship between Martaban and Sukhodaya were severed".<sup>212</sup> Thus all mention of Suphanburi is speculation by Griswold and Prasert, and the Mon sources, correct or not, seem only to be concerned with Sukhothai.

In that case, though, the mention of Phetchaburi is troubling, if it is taken to

207. Charnvit, pp 94-95. The rewriting of *CS* is in saying that the Uthong who supposedly came from Ayudhya in the late thirteenth century to defeat the ruler of Nakhon must really have been king of Suphanburi (*CS*, pp 90-93 and Wyatt's note 1, p 90). Charnvit does not cite *CS*, only Prince Damrong, who, *loc cit*, merely wrote that there were grounds for believing that Nakhon was subject to Ayudhya in 1351. Thus is it not certain where Charnvit obtained his information about Suphanburi and Nakhon.

208. Charnvit, p 97.

209. *Ibid*, p 95; G/P, *EHS* 10, pp 41-47, and table, p 23.

210. Charnvit, p 95.

211. *Rājādhīrāj*, pp 18-23, 44-55, 57, 58 (edition of Glang Vithaya), Bangkok, BE 2513).

212. G/P, *EHS* 10, p 47; *Rājādhīrāj*, p 58; Sir Arthur Phayre, *History of Burma* (London; 1883, 1967), p 66, Phayre used 'Siam' indistinctively for Sukhothai and the lower Manam area.

mean the present town of that name, and it is no wonder that Griswold and Prasert sought an explanation, even if unduly speculative and epicyclical. A much better explanation is found in the circumstance that some Sukhothai inscriptions give the name ‘Bājrapurī/Bejrapurī’ to Kampaengphet, one of the Sukhothai-area towns; and since the Mon histories specifically refer only to Sukhothai, it is far better to assume their *bejrapurī* to mean Kamphaengphet rather than to erect an *ad hoc* story involving Suphanburi and Uthong.<sup>213</sup>

With respect to territorial administration within Ayudhyan boundaries Charnvit says Ayudhya “was conceived to be the magical center of the kingdom, with an important city at each of the four cardinal points”; and at first the four cities were Lopburi, Phrapradaeng, Nakhon Nayok and Suphanburi. They were called *mo'an lūk hlvanī*, ‘literally “cities of royal sons”’.<sup>214</sup>

The very concept of these cardinal cities called *mo'an lūk hlvanī* is a highly speculative reconstruction, and the evidence, which no one seems to have examined systematically for 50 years or so, is as follows:

(a) In the Ayudhyan Palatine Law (*kaṭ mantierpāl*), article 3, says princes born of *lūk hlvanī* govern in *moán ek*, first class provinces; and the term *lūk hlvanī* here refers to the status of the mothers, the third rank of royal consorts. In article 8 of the same law the *mo'an lūk hlvanī*, which was **perhaps** meant to indicate towns governed by sons of *lūk hlvanī* consorts, are enumerated as “Biṣṇulok, Savarrgalok, Kām̄baenbej, Labpurī and Singpūn”.<sup>215</sup>

(b) The date in the preamble of this law is *cūla* 720/AD 1358, and it was first assumed that it was a law of Rāmādhipatī which described the situation of his time. By 1914, however, Prince Damrong, realizing that the name *biṣṇulok*/Phitsanulok could not yet have been in use, had decided that the law really belonged to the reign of Trailokanath and should be redated as 820/1458. He nevertheless spoke of the appointment of Indarājā’s three sons to govern *mo'an* Suphanburi, *Sarrg*, and Chainant in 1409 as *lūk hlvanī*-type appointments, and it is clear that he was referring to the institution mentioned in the Palatine Law.<sup>216</sup> Later he elaborated a theory that Ayudhya, already in the reign of Rāmādhipatī, had been surrounded by four *mo'an lūk hlvanī* at the cardinal points, namely Lopburi, Nakhon Nayok, Phrapradaeng, and Suphanburi.<sup>217</sup>

(c) But this, however, is an *ad hoc* modification of the Palatine Law to fit a preconceived notion without any evidential basis. If the law should really be redated, then the institutions it describes may not be projected back to Rāmādhipatī’s time with any certainty, particularly so long as Trailokanath is believed to have been a

213. Inscription 46, lines 10-11, *Bajrapurīśrīkām̄baenbej* in *EHS* 1, *JSS*, LVI, 2 (July 1968), 225, 227; and inscription 38, line 5, *kām̄baen bejrapurī* in *Cāru'k* 1983, p. 356; *EHS* 4, *JSS*, LVII, 1 (Jan 1969), 128.

214. Charnvit, pp 97, and 97-98 respectively.

215. *Kaṭhmāy trāsāmtvaṇ* (“Laws of the Three Seals”), Guru Sabhā edition, vol 1, pp 7-71, 72.

216. Damrong, *RA*, pp 254-55, 265.

217. H.G Quaritch Wales, *Ancient Siamese Government and Administration*, p 104, based on an earlier work of Prince Damrong.

great reformer. Furthermore, even if the list of *mo'an lūk hlvan* of the law fits the extent of Ayudhyan territory at the time of Trailokanath, they in no way fit a system of cardinal points surrounding Ayudhya, and thus Prince Damrong's modification is arbitrary from two different angles. In short there is no evidence in any source that Rāmeśuor's appointment to Lopburi, or Cau Sām Brahyā's appointment to Chainat, or any other appointment, had anything to do with the institution of *mo'an lūk hlvan*, or that *mo'an lūk hlvan* were intended as cardinal cities. In addition to this, it is methodologically impermissible for modern scholars who accept Prince Damrong's revision of date, or David K. Wyatt's different revision (1468)<sup>218</sup>, to talk about *mo'an lūk hlvan* in the reign of Rāmādhīpatī. And if they do not accept the chronological revisions, their case must be argued, not assumed, since both Prince Damrong and Wyatt adduced serious evidence for their hypotheses<sup>219</sup>.

Charnvit's use of *mo'an lūk hlvan* comes ultimately from Prince Damrong, and he no doubt felt that was sufficient authority, although one would expect an announced critic of the *bañśāvatār* tradition to look more closely into modifications of sources undertaken within that tradition. Strangely Charnvit does not cite Prince Damrong, but only refers to two works by Rong Syamananda and to Heine-Geldern<sup>220</sup>. Professor Rong, in the work which I was able to consult, does not mention *lūk hlvan*, but calls those same cities *mo'an pòm prākār*, 'citadel cities'.<sup>221</sup> As for Heine-Geldern, although citing no sources, he obviously relied on Quaritch Wales and Prince Damrong; and he cannot be used to substantiate their suppositions.<sup>222</sup> On the contrary, if the *mo'an lūk hlvan* theory does not hold up on its own, Heine-Geldern's general argument is thereby weakened.

Following his discussion of *lūk hlvan* Charnvit continues by describing the area beyond the cardinal cities and occupied by the "muang phraya maha nakhon" and "muang prathetsarat". He is apparently still describing the kingdom in the time of Uthong and seems unaware that historians since Prince Damrong have considered the laws outlining these institutions to date from the reign of Trailokanath.<sup>223</sup>

In one respect Charnvit cites important evidence which he was unable to use properly due to his effort to force contradictory reasoning into a unified argument.

218. Wyatt, "The Thai Kata manḍiarapāla".

219 I accept neither revision of the date, although I agree that 720/1358 is mistaken and that the law is not from the time of Rāmādhīpatī. The argument is irrelevant here, and will be presented after the completion of research in progress. See Vickery, "The Constitution of Ayutthaya", in *New Light on Thai Legal History*, Edited by Andrew Huxley, Bangkok: White Orchid Press, 1996, pp. 133-210

220. Charnvit, pp 27, n 27, and 115, n 16.

221. Rong Syamananda and Wilatwong Nopparat, "Prawatsat", cited by Charnvit, p 115, n 16, and in his bibliography. Neither does this work contain any of the other details about those cities attributed to it by Charnvit.

222. Robert Heine-Geldern, *Conceptions of State and Kingship in Southeast Asia*, Cornell University, Southeast Asia Program Data Paper No 18, 1956, p 5. Here Heine-Geldern has also confused the princes of the cardinal cities and the ministers who were known as the 'four pillars'.

223. Charnvit, pp 98-99; Damrong, RA, p 229; Quaritch Wales, *op cit*, pp 34, 75, 171, 173; Vickery, review of Yoneo Ishii et al, *An Index of Officials in Traditional Thai Governments*, in JSS, LXIII, 2 (July 1975), p 425.

This concerns Thai, or Menam Basin, efforts to control the entire Peninsula, something attested by the Chinese since the thirteenth century and by the Portuguese in the sixteenth. Such evidence fits very well with the hypothesis of the eleventh to fourteenth-century development of Ayodhya as a maritime trading center and aids in the integration of Ayodhya/Ayudhya into the framework of Wolters' Srivijaya thesis. However, Charnvit fails to follow this train of thought, dismissing it with the remark that "Southern Malaya was too far away and of too little real concern to Ayudhya for a major effort to be made to subdue it", in spite of the fact that the Chinese and Portuguese records show that major efforts were indeed made. Charnvit here feels that "the major concern of (Ayudhya) .... was directed towards the east and the north", the point of view of the extant *baṇśāvatār*, which ignore the earlier concern with the Peninsula, probably because they were written after the peninsular policy had been given up.<sup>224</sup>

In the last part of this chapter Charnvit discusses two interesting subjects connected with the expansion of Ayudhya, the major theme of the following chapter. These are the Ayudhya-Suphanburi conflict and Ayudhyan relations with China, both of which he treats almost exclusively as political questions, ignoring the economic aspect which he had touched on earlier. This leads him to explain the resolution of the problems as due to relationships among individuals, or as political actions for vague, undefined purposes, a procedure which is here particularly risky and inevitably speculative since there is no real information about individuals in the extant sources. This procedure also leads him to ignore the pre-Ayudhyan Ayodhya, whose existence he had worked so hard to establish.

For instance, he notes that in the apparent struggle among Ayudhya, Suphanburi, and Lopburi, the other centers were not trying to secede from Ayudhyan control, but to gain power over Ayudhya which had become the most important center of the region; and he argues that the reason for Ayudhya's preeminence was the long reign of Uthong, who "provided common ground where local *muang* leaders met and interacted".<sup>225</sup> Although this is the purest speculation, it may very well be true, but is it even then the right conclusion? Surely the pre-eminence of one city over another in such a small area depends on certain objective economic, geographical, or strategic considerations, and if we must speculate, it would be better to look at these latter areas and to suggest that Ayudhya's central place was probably due to a riverine situation more favourable for the trade which Charnvit realizes was important for Ayodhya/Ayudhya. That is, the continued growth of Ayudhya rather than a return to political fragmentation may have resulted, not from a "new style of politics",<sup>226</sup> but from the silting of Suphanburi's river,<sup>227</sup> which destroyed its own port status, and

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224. Charnvit, p 97. for evidence of Thai efforts see two references to Chinese reports on Charnvit's pp 83-84; the summary of Portuguese information in Donald F. Lach, *Southeast Asia in the Eyes of Europe*, p 520; Wolters, *The Fall of Srivijaya*, pp 108-09, 154-55, 169; Vickery, review of van Vliet, pp 232-34.

225. Charnvit, p 105.

226. *Ibid.*

227. E.H.G. Dobby, *Southeast Asia*-(London, 1967), pp 273-74.

from Ayudhya's riverine situation which controlled Lopburi's access to the sea.

The growth of relations with China fully justifies the attention given it by Charnvit, but he considers it too much as a political matter, speculating on its connection with personal plays for power among the Ayudhya-Suphanburi elites.<sup>228</sup> The economic aspect is much more interesting, especially at the level of abstraction made necessary by the nature of the sources, and Charnvit should have seen how this would fit Ayudhya into the theory developed by Wolters in his *Fall of Srivijaya*.

Chapter VI, then, shows great potentialities not properly developed due to uncritical reliance on earlier speculations and the inability to distinguish contradictory theories.

### **The expansion of Ayudhya and its attempts to lead the Thai world**

In chapter VII, dealing with Ayudhya after the reign of Uthong, Charnvit has to a serious extent lost control of his material. The subject of the chapter is Ayudhya's expansion against, or rivalry with, Sukhothai and Angkor, and even with the most conservative reading of the sources we must admit that such expansion really occurred.

Nevertheless, since all sources of whatever reliability, show threats by Ayudhya against its neighbours, not the opposite, Charnvit should show precisely what he has in mind as evidence that "Ayudhya would have felt vulnerable to threats from the two earlier established kingdoms"; and as for "capture of enemy populations", the only evidence consists of dubious passages from the Cambodian chronicles.<sup>229</sup>

The expansion against Sukhothai can hardly be disputed at all. Ayudhya eventually did absorb that area, and even if the Sukhothai evidence itself cannot be integrated into the story as has been attempted<sup>230</sup>, a credible outline is found in the LP chronicle which has so far proven resistant to any attempt to discredit it.

[\*In 1979 this still seemed to me an adequate summary. Later work, however, some by myself and especially by Chris Baker, emphasizing that Ayutthaya began as a seaport and trade area known as *hsien/xian/sien* which eventually became a land power attempting to expand northward. Although militarily sometimes successful

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228. Charnvit, pp 111-114. Most of Charnvit's details about Thai individuals and their relations with China come from a dissertation which I have not seen; but the relevant extracts from the Chinese sources have been published by T. Grimm, *op cit*, where the Chinese information has been forced into the framework of the Ayudhyan chronicles. The Chinese notices themselves suggest that the identities and relationships among the Thai mentioned are not nearly so clear as Charnvit believed.

229. Charnvit, p 119. On that part of the Cambodian chronicles see Vickery, "Cambodian after Angkor", for a demonstration that the passages concerning fourteenth-century invasions of Cambodia are either fictional or misplaced; [\*and for a theoretical discussion of the "capture of enemy populations" in early Southeast Asian warfare, see Vickery, "Two Historical Records of the Kingdom of Vientiane", in Christopher E. Goscha and Sören Ivarsson (eds.), *Contesting Visions of the Lao Past Lao Historiography at the Crossroads*, Richmond: Curzon Press, 2002, 352 pp., ISBN 8791114020\*]

230. Vickery, "Guide".

Ayutthaya never conquered or absorbed the important northern *muang/moan*, Sukhothai, Chaliang, Phitsanulok, Kamphaengphet whose rulers remained kings and princes developing their territories, and who, taking advantage of the Burmese attack of 1568-9 became rulers of Ayutthaya under whom its language and high culture finally changed from Khmer to Thai. They also gained wealth from exports of ceramics from the rich kiln fields of Chaliang, which may have been an objective of some of the Ayutthyan attacks northward, which were unsuccessful. Unification of what became modern Thailand was under Phitsanulok and Sukhothai, not under Ayutthaya.\*] <sup>230a</sup>

Ayudhya's expansion against Cambodia, however, is an entirely different matter. As I have noted briefly in this journal on other occasions, there are several conflicting stories about this expansion which long resisted resolution,<sup>231</sup> and while Charnvit cannot be blamed for ignoring work not yet published at the time he wrote, it was incumbent on him to seek a coherent explanation once he decided to tackle the problem.

Charnvit accepted Wolters' analysis, which seemed to show that Angkor was captured twice by Ayudhya, in 1369 and 1389, and while his acceptance *per se* cannot be severely criticized, what must be discussed is his attempt unsystematically to force documents not used by Wolters into Wolters' framework.<sup>232</sup>

Wolters' date '1369' was based on Chinese records plus the Cambodian *Ang Eng Fragment* and he did not deal with the Ayudhyan chronicles, although he seemed to feel that the earlier invasions of Cambodia in those sources were misplaced records of the '1369' campaign.<sup>233</sup>

Charnvit makes this explicit with, "in the 1369 campaign, Uthong appointed his son Ramesuan ...." which is the story found in the Ayudhyan chronicles in 1351-53.<sup>234</sup> Even here, however, he is not faithful to any of the original sources, for he says that after Angkor was taken, "a son of the deceased [Cambodian] king [was appointed] to rule....". The story of such an appointment is found **only** in the Cambodian chronicles, and there the new king is son of the **Ayudhyan**, not the Cambodian king. Charnvit's version comes from Prince Damrong, who, together

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<sup>230a</sup> The most thorough treatment is Chris Baker, "Ayutthaya Rising: From the Land or From the Sea", *Journal of Southeast Asian Studies* 34/1 (2003), pp. 41-62. See also Vickery, "The 2/k.125 Fragment, a Lost Chronicle of Ayutthaya", *Journal of the Siam Society* 65/1 (1977); "A Guide Through Some Recent Sukhothai Historiography"; a review article, *JSS* Volume 66, Part 2, July 1978, pp. 182-24; "The Old City of 'Chaliang'--'Srī Satchanalai'--'Sawankhalok', a Problem in History and Historiography" *Journal of the Siam Society*, Vol. 78, Part 2 (1990), pp. 15-29;

231. Vickery, review of Jones, *Thai Titles and Ranks*, pp 164-65; review of van Vliet, pp 229-31; "The 2/k 125 Fragment, a lost chronicle of Ayutthaya", *JSS*, LXV, 1 (Jan 1977), 55-56.

232. Charnvit, p 122. [\*We must recognize that, working in Cornell at the time, where the atmosphere did not encourage critical treatment of mentors or old masters, Charnvit could not have avoided acceptance of Wolters' interpretations\*]

233. O W Wolters, "The Khmer King at Basan". See pp 79-83 for the relevant remarks about the Ayudhyan chronicles; and for my own analysis of the same material see "Cambodia after Angkor", chaps IV, V, and pp 218-23.

234. Charnvit, p 123; *RA*, pp 67-68.

with other Thai writers, altered the story, presumably because Ayudhyan chronicles made no mention of such a son.<sup>235</sup>

Charnvit has thus piled synthesis upon synthesis without paying attention to any of the original chronicles. Prince Damrong's own synthesis was an arbitrary assimilation of details from Ayudhyan and Cambodian chronicles, and Charnvit has removed it to Wolters' date. Wolters attempted a systematic treatment of the Cambodian dates and concluded that the Cambodian chronicles' invasion of 1351-52 should be moved to 1369. However, since the date 1351-53 is also found in the Ayudhyan chronicles, and cannot itself be revised systematically to 1369, Charnvit's treatment (although not Wolters') implies that the Ayudhyan chronicles were written later than, and in this passage blindly copied from, the Cambodian, a conclusion which is the opposite of all earlier studies and which requires demonstration, not simply assumption.<sup>236</sup>

Charnvit has also fitted the events of *Jinakālamālī* into this scheme in unusual ways. In a modification of Wolters' bipolar theory, which he generally accepts, Charnvit says, "Uthong ... waged expansionist wars against both Sukhothai and Angkor", and this claim for war against Sukhothai is surprising, since it is not found in any source. Apparently Charnvit is drawing again upon Prince Damrong, who is supposed to have written that "Uthong invaded Sukhothai and captured Phitsanulok".<sup>237</sup> If we look closely at Prince Damrong's writing, however, we find that he only credited Uthong with an attack on *mo'ai Sarrg* in modern Chainat Province, which he equated with the 'Jayanāda' of *Jinakālamālī* and the 'Dvisakhaganara' ('confluence city') of *Cāmadevīvañś*.<sup>238</sup> Even accepting *Sarrg* - Chainat as a dependency of Sukhothai, as Prince Damrong did, an attack on it is not equivalent to an attack on Phitsanulok. However, Griswold and Prasert have since then asserted that 'Jayanāda' means Phitsanulok and Charnvit is apparently following them;<sup>239</sup> but this procedure should be made explicit, and in any case it is not possible to follow both Prince Damrong and Griswold and Prasert on this point.

Charnvit is also forced into difficulties on the question of date. As he says, if the first capture of Angkor took place in 1369 (following Wolters), then the attack against Sukhothai must have come earlier (since Uthong died in 1369), but this contradicts all of the sources, which clearly place the campaign against Jayanāda, or Dvisakhaganara, **after** the event which in those sources had been interpreted as

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235. Charnvit, p 123; Damrong, *RA*, p 236. I am imputing the reason for the alteration, having found no explicit statement concerning it.

236. G Coedès, "Essai de classification des documents historiques cambodgiens conservés à la Bibliothèque de l'Ecole Française d'Extrême-Orient", *BEFEO*, XVIII, 9 (1918), p. 18; Lawrence Palmer Briggs, "Siamese attacks on Angkor before 1430", *FEQ*, VIII (1948), PP 9, 30-31; Wolters, "The Khmer King at Basan", p 79.

237. Charnvit, p 121 for both quotations.

238. Damrong, *RA*, pp 233-34.

239. G/P, *EHS* 3, p 63; *EHS* 11-2, p 108; "On kingship and society", p 64, n 22; "A fifteenth-century Siamese historical poem", in *Southeast Asian History and Historiography*, ed by C D Cowan and O W Wolters (Ithaca, 1976), p 64, n 22.

indicating Uthong's attack on Cambodia.<sup>240</sup>

Furthermore, Charnvit's synthesis here implicitly accepts 'Kamboja' as meaning Cambodia, whereas in an earlier section he agreed with the present scholarly consensus that 'Kamboja' meant central Siam<sup>241</sup>. In that case, though, *Jinakālamālī* contains no story of an invasion of Cambodia at all in the reign of Uthong, and it cannot be fitted into a synthesis after the manner of Charnvit.

According to Prince Damrong, the attack on Cambodia, following both the standard Cambodian chronicles and *RA*, occurred in 1352 and the attack on Jayanāda in 1354; Wolters equated the Kamboja story of *Jinakālamālī* with the campaign of 1352 and said the true date was 1369; Griswold and Prasert ignore all invasions of Cambodia before *LP*'s 1431 and they recognize that the story of *Jinakālamālī* does not refer to Cambodia.<sup>242</sup> Charnvit apparently wishes his sources to mean all things for all occasions, and he tried to agree now with Prince Damrong, now with Wolters, and then again with Griswold and Prasert.

[\*Charnvit repeated the same confusion over twenty years later, writing that following its founding in 1351 Ayutthaya began to attack both Sukhothai and Angkor. "Sukhothai was invaded for the first time in 1354/5, and it fell to ... Ayutthyan forces in 1378/9 [citing Prince Damrong]". Then, "in the following century, the territory once ruled from Sukhothai was successfully incorporated into the expanding new kingdom, and in 1419/20 Sukhothai became a vassal state of Ayutthaya". As for Angkor, it was captured "for the first time in 1369 and for the second time in 1388/9 (Wolters...)", and "in 1431, Angkor was captured again and depopulated..."]<sup>243</sup>

I have provided my own analysis of these events and sources elsewhere and have concluded that there is no extant evidence for an Ayudhyan attack on any part of Cambodia before 1431, or possibly 1409<sup>244</sup>, and I only wish to repeat here, with respect to Charnvit's sources, that if 'Kamboja' in *Jinakālamālī* means central Siam, then that work provides no evidence for an attack on Cambodia at any date; and if, as Wolters thought, the *Jinakālamālī* campaign against 'Kamboja' was identical to the *RA* attack on 'Kambujā' in about 1352, then neither of these stories may be shifted to his '1369'.

Another point of confusion concerns Uthong's marriage to a princess of

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240. See *Jinakālamālī*, in Coedès, "Documents", pp 99-100.

241. Charnvit, p 65.

242. Damrong, *RA*, pp 235-36; Wolters, ; "The Khmer King at Basan", pp. 79-83; G/P, *EHS* 8, *JSS*, LIX, 1 (Jan 1971), 207, n 49; *EHS* 11-2, p 108.

<sup>243</sup> Charnvit Kasetsiri, "Origins of a Capital and Seaport: The Settlement of Ayutthaya and its East Asian Trade", in *From Japan to Arabia: Ayutthaya's Maritime Relations with Asia*, edited by Kennon Breazeale prefaced by YOneo Ishii and Charnvit Kasetsiri, Bangkok 1999, pp. 55-79, see p. 55. There is no evidence that Angkor was depopulated.

244. Vickery, "Cambodia after Angkor"; and in more detail on the question of '1409'. see "The 2/k 125 Fragment, a lost chronicle of Ayuttaya", pp 56-61. [\*These remarks concern the post-1350 period treated in the Ayutthayan and Cambodian chronicles. Zhou Daguan, visiting Angkor in 1296, reported an attack on Cambodia's western border a few years earlier by *Hsien*, which he described as southwest of Angkor, and which must have been some site along the gulf coast or in the Ayodhya/Ayudhya-Suphanburi region. \*]

Kamphaengphet, which led to a period of peace between Sukhothai and Ayudhya following Uthong's, attack northward<sup>245</sup>. First of all, accepting this story means a literal acceptance of the *tāmnān* of the Sihing Buddha, which purports to relate the peregrinations of a miraculous Buddha image and is thus, more than strictly political chronicles, subject to distortions. Furthermore, there are several versions of the Sihing Buddha story, and in the one inserted into *Jinakālamālī*, the princess from Kamphaengphet marries Uthong's successor, after Uthong's death.<sup>246</sup> Given such contradictions, no political inference may yet be made from any version.

From the early fifteenth century the major route of Ayudhyan expansion in all sources was northward, which Charnvit clearly outlines, and he again refers to the *mo'an lūk hlvanī*, arguing, along with Prince Damrong, that the status of northern cardinal city was shifted from Lopburi to Chainat shortly after 1409.<sup>247</sup>

Even then Charnvit is not in control of his sources. He accepts 'Chainat' as in the "area of modern Chainat", whereas earlier he implicitly accepted Griswold's and Prasert's contention that 'Chainat' in the fourteenth and fifteenth centuries meant Phitsanulok.<sup>248</sup> Of course, in 1409, so far as we know, there was no way for an Ayudhyan king to appoint his son to rule in Phitsanulok and thus 'Chainat' in the early Bangkok chronicles must have been intended as Chainat.

This means either that Griswold and Prasert are wrong about 'Chainat' meaning Phitsanulok, or the chronicle entry for 1409 is an inaccurate later interpolation. In any case Charnvit cannot have his evidence both ways, and before the theory of a northward removal of the cardinal city may be accepted, all the conflicting evidence must be sorted out.

Charnvit goes on to say that the cardinal city was shifted again, to Phitsanulok, when Prince Rāmeśuor was appointed as ruler there in 1438. I have elsewhere called attention to certain weak points in the theory and here will only add that

*Culayuddhakāravānī*, the forerunner of the Bangkok *banśāvatār* according to Charnvit, says clearly that Rāmeśuor only went to Phitsanulok for three days to perform ceremonies.<sup>249</sup>

After this Charnvit proceeds to some careless secondary synthesizing about Trailokanath. He says "in 1438, Ayudhya took the opportunity to put its own candidate, Trailok, on the throne of the northern kingdom, claiming that as a lineal descendant of the Sukhothai family through his mother he was qualified to rule over Sukhothai. Then only a boy aged fifteen (thus born 1423?), Trailok was sent north....". Then, one page later, he adds, Trailok "was born in 1431... At the age of seven (1438) he was given the title Ramesuan, the *uparāja*. After spending the first

245. Charnvit, ppp 122, 126.

246. *Jinakālamālī*, in Coedès, "Documents", p 100; and for G/P's evaluation of the miraculous Buddha stories, *EHS* 11-1, p 73, n 6; *EHS* 12, p 117.

247. Charnvit, pp 127-128.

248. Charnvit, p 128; and see above concerning Uthong's attack on 'Chainat'.

249. Charnvit, p 131; Vickery, "The 2/k 125 Fragment", pp 75-76; *Culayuddhakāravānī* part 2 (Bangkok, 2463/1920), p 59;

fifteen years of his life in Ayudhya, he was sent to rule over Phitsanulok [1438 or 1446?]<sup>250</sup>”, but as Baker pertinently noted, it was more likely a pilgrimage than to rule.

The basis for these remarks is first, *LP*, which at the date 1438 says, “... Samtec brah Rāmeśuor, the royal son, went to Phitsanulok. At that time he saw that the eyes of the Buddha Jinarāj were emitting blood”.<sup>251</sup> Out of this cryptic entry has grown the whole scaffolding of assumptions supporting a story that Rāmeśuor was sent to rule in Phitsanulok, that therefore Mahādhammarāja IV must have died shortly before, and that Sukhothai was finally taken over by Ayudhya.<sup>252</sup>

An initial complication to the reconstruction comes from *Yuan Phai*, which says Rāmeśuor/Trailoc was born while his father was preparing to attack Cambodia, that is, as accepted now by historians, in 1431<sup>253</sup>. This has not troubled Griswold and Prasert, who assume that princes were introduced early to public life in those days;<sup>254</sup> but for Prince Damrong age seven was too young for someone to have been sent to rule Phitsanulok, and he thought that such an appointment must have been made when the prince was 15, in 1446, and that in 1438 he would only have been given the formal rank of *uparāja*.<sup>255</sup> However, *LP* says nothing about appointment as *uparāja*, and is specific that he went to Phitsanulok in 1438, although not necessarily as ruler. [\*or did the writer of *YP* think the war with Cambodia was in 1421, as is stated in most of the chronicles, which would mean revision of the date of composition of *YP* to late 18th or early 19<sup>th</sup> century?\*]

Prince Damrong’s supposition that he did not go to Phitsanulok until 1446 simply negates the evidence of *LP*, and also negates the speculation of other scholars that Mahādhammarāja IV died in 1438. On the other hand, we could say that *LP* is accurate and that, in agreement with Prince Damrong, Rāmeśuor must have been at least 15 years old, but that negates the evidence of *Yuan Phai*. This is the sort of difficulty that traditional scholars get into when they refuse to criticize their courses. Charnvit has refused to criticize, not only sources, but even syntheses of sources, and thus he has been led to say on one page that Trailoc went to rule in Phitsanulok at the age of 15 in 1438 and on the next that he was only sent to Phitsanulok in 1446.

I have earlier criticized and explained the notion that the mother of Trailoc was a Sukhothai princess<sup>256</sup>, and will do no more here than indicate how

250. Charnvit, pp 131, 132; Baker, op. cit., p. 10.

251 *LP*, at date *cula* 800.

252 Damrong, *RA*, pp 258-59, 262-63; A.B. Griswold, *Towards a History of Sukhodaya Art*, pp 55-56; G/P, “A fifteenth-century Siamese historical poem”, pp 130-32.

253. Damrong, *RA*, p 262; G/P, “A fifteenth-century Siamese historical poem”, pp 130, 143.

254. Griswold, “Notes on the art of Siam, No 6, Prince Yudhiṣṭhira, p 221, n 3; and G/P, “A fifteenth-century Siamese historical poem”, p 130, mention his age with no comment.

255. Damrong, *RA*, pp 262-63.

256. Vickery, “Guide”, pp 189-90. In connection with this I added that there was no evidence for Prince Chand’s statement that the Bañā Rām of *LP*’s date 781 was subsequently appointed to be Bañā Chalieng. Since then I have noticed that Prince Damrong, in *Nidān porāṇagatī*, *Nidān* No 19, section 3, suggested this synthesis, and that was probably Prince Chand’s source. Nevertheless, Prince Damrong’s suggestion is only a hypothesis, and there is no evidence to support it.

Charnvit has arbitrarily embellished it. His embellishments are the remarks that Ayudhya claimed that as a lineal descendant of Sukhothai royalty Trailok was qualified to rule there, that his mother “may have played a major part in her son’s success among her relatives”, that he “brought his mother with him when he went north”, that “she probably helped smooth relations between her son and her family”, and that “some members [of Sukhothai royalty] were seeking support from Ayudhya .. and it was logical for them to join the Ayudhyan prince and his mother”.<sup>257</sup> It is necessary to emphasize, for readers unfamiliar with the Thai sources, that there is absolutely no evidence for any of these statements in any extant source, and Charnvit’s construction here amounts to historical fiction, even if he has happened to hit on some of the truth.<sup>258</sup>

Charnvit also follows Prince Damrong in stating that Trailokanath “adopted many customs of the northern kingdom”, which are “evidenced in his later acts as ruler in Ayudhya”.<sup>259</sup> Although not *a priori* unreasonable, this is a surprising statement, since the very laconic chronicle entries for Trailokanath’s reign, which, together with certain law texts, are the **only** extant sources, show nothing that can be based on Sukhothai custom as that is revealed by the Sukhothai sources.

The first example of such speculations concerns Trailokanath’s transforming the royal palace into a temple. In *RA*, but not *LP*, there is a statement at the beginning of his reign to the effect that “he made the palace into Wat Phrasisanphet and himself went to reside near the river.” As Prince Damrong interpreted this, Trailokanath did not really move, but just built a *vāt* on part of the palace grounds within the palace walls. Prince Damrong further guessed (คาด) that in doing this he imitated the practice in Sukhothai where *Vāt Mahādhātu* had been built on the *jān* (ฐาน) ‘platform, veranda’) of the royal palace.<sup>260</sup>

Now this does not seem to be the view of modern historians of Sukhothai art. Both in textual description and on maps it seems clear that although *Vāt Mahādhātu* and the palace site are rather close, the *Vāt* was distinctly separate from the palace grounds<sup>261</sup>. Even less is there evidence that it was a “residence (turned) into a temple”,<sup>262</sup> and in fact there is even doubt about the date of the *Mahādhātu*.<sup>263</sup>

In any case, temples were built very close to, and within, the precincts of the palace at Angkor, whose influence on Ayudhya, and also on Sukhothai, is beyond doubt, and thus whatever Trailokanath really did, it cannot with any certainty be attributed to Sukhothai influence. One might also cite the story of Uthong turning his

257. Charnvit, pp 131-33.

258. Hitting on the truth by chance, or with a lucky guess, is not sufficient to write history. The difference between history and historical fiction which is close to the truth lies in a consistent methodology and systematic use of sources.

259. Charnvit, pp 131-32; Damrong, *RA*, p 263.

260. Charnvit, pp 136-37, *RAF*, p 73; Damrong, *RA*, p 264.

261. See remarks on the *Mahādhātu* and map, in A B Griswold, *Towards a History of Sukhodaya Art*; see also the maps attached to G/P, *EHS* 2, *JSS*, LVII, 1 (Jan 1969).

262. Charnvit, p 136.

263. See the discussion in Vickery, “Guide”, pp 211-12.

residence into a temple as evidence that Trailokanath was following old Ayudhyan customs, but although Charnvit notes this, he preferred to follow the reasoning of Prince Damrong.<sup>264</sup>

As further evidence of Sukhothai customs adopted by Trailokanath, Charnvit cites his religious activity in general. “He built and restored many Buddhist temples in the new capital [Phitsanulok]”, which emulated old Ayudhyan custom just as well as Sukhothai practice (in fact the chronicles mention only two temples); and as for entering the monkhood, Charnvit’s interpretation goes far beyond the evidence. According to him, “Sukhothai kings were famous for becoming Buddhist monks … whereas Ayudhyan kings before Trailok never entered the monkhood”. But the only Sukhothai king whom we know for certain to have become a monk was Lidaiy, and the Ayudhyan chronicles for the fourteenth-fifteenth centuries are so sparse in detail that we may draw no conclusions as to whether kings became monks or not. Furthermore, there is no ground for assuming that “Trailok’s ordination … was designed to emulate … the great Sukhothai king, Maha Thammaracha I [Lidaiy].”<sup>265</sup>

Finally, it is utterly beyond the realm of proper inference to say that Trailok’s composition of a Jataka edition was in order to emulate Lidaiy’s writing of the *Traiphum/Traibhūmi*, an entirely different type of work; and there is absolutely no evidence that “his new version of the tale was now used in Buddhist sermons and drama, replacing the old version formerly in use in Sukhothai”.<sup>266</sup>

Thus Charnvit has uncritically followed Prince Damrong’s speculations and even more uncritically added to them, and even then all of the acts adduced as evidence, except one, were performed by Trailok in Phitsanulok and do not fit Charnvit’s original claim that Trailok “adopted many customs of the northern kingdom, as evidenced in his later acts as ruler in Ayudhya”.<sup>267</sup>

On the contrary, there is some evidence that Trailok, far from borrowing Sukhothai customs, was trying to force his own Ayudhyan usages on the north. This evidence is also too sparse to be conclusive, but once the subject has come up for discussion the reader’s attention should be drawn to it.

Charnvit claims that Trailok, for his ordination, invited a Ceylonese monk, and “thereby passed over the Sukhothai *Sangha* for this important event”, which in itself is already contrary to the theory that Trailok wanted to emulate Sukhothai customs. In addition, the “ordination took place in a rather insignificant temple which he had restored [Vāt Culāmaṇī] … rather than at the temple of the Buddha Jinarāja”, which, given that view of the relative importance of the two sites, seems strange for a king whose “ordination… was planned in order to penetrate and take hold of the Sukhothai *Sangha*”. Moreover, an interesting feature of Vāt Culāmaṇī, which Trailok ‘restored’ ('built' according to *LP*, and whose ‘insignificance’ would come as a surprise to

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264. Charnvit, pp 136-37.

265. *Ibid*, p 138.

266. *Ibid*, p 140. [\*For discussion of the supposed Sukhothai *Traibhūmi*, see Vickery, “On *Traibhūmikathā*”, in this volume, pp. 00-00.\*]

267. *Ibid*, pp 131-32, my emphasis.

anyone who had visited it), is the apparently ‘Khmerizing’ style of its architecture.<sup>268</sup>

These details of Trailok’s religious activities, together with the *jataka* images he apparently had made in 1458 and which were embellished with Khmer inscriptions<sup>269</sup>, and the extant inscriptions from the Sukhothai period of his reign - including one possibly issued during his monkhood - which were also in Khmer,<sup>270</sup> could just as well be used to argue that his policy was to impose Ayudhyan practices on the north. [\*And the Khmer magnificence of Văt Culāmaṇī would fit into this interpretation showing that Trailok’s religious policy was not just to ‘penetrate’ the Sukhothai *Sangha*, but to impose Ayutthayan Khmer customs\*].

There are still other examples of misused evidence in this chapter. At the beginning of his section on Trailok Charnvit quotes a passage from the *Arthaśāstra*, and later attempts to argue that this work was known in Ayudhya and might have been studied by Trailok. The chain of reasoning starts with *Yuan Phai* which says Trailok knew “Vedic literature, the *Tripiṭaka*, the *Rājadharmā*”; and in addition, the *Mahābhārata* and *Rāmāyaṇa*, which contained “guidance for kings” were also known in Ayudhya. It is also accepted that forms of the *Dharmaśāstra* and *Rājaśāstra* were known, and Charnvit cites a writer who apparently suggested that the *Arthaśāstra* was an ‘offshoot of the *Dharmaśāstra* [sic]’. “This confirms the hypothesis that Trailok had available one form or another of the *Arthaśāstra*.<sup>271</sup> Not at all. Even if the suggestion of the *Arthaśāstra* deriving from the *Dharmaśāstra* be true, this would have been from an Indian version of the latter, whereas the *Dhammasutta* of Ayudhya were in a long line of descent from the Indian *Dharmaśāstra* and had no logical connection to the *Arthaśāstra*.<sup>272</sup>

Charnvit goes on to speak of the “adoption of the *deva-raja* cult” in Ayudhya, ignoring available research which tends to show that the *devarāja* was probably not just ‘god-king’, but a very complex institution. In the absence of any mention of *devarāja* in Ayudhyan sources, it is quite improper to say “the ruler was more or less proclaimed a *deva-raja*.<sup>273</sup> In fact, no Angkor king was ever ‘proclaimed *devarāja*’. Interestingly, Charnvit cites only Akin Rabibhadana who relied on Prince Dhani’s description of Thai coronation ceremonies which tended to show that “the person of the king was assimilated with the god”.<sup>274</sup> Whatever the accuracy of that conclusion, it does not prove that Ayudhya had adopted the Angkorean *devarāja* complex, and it might be evidence that the Ayudhyan and Angkorean royal ceremonies derived from

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268. *Ibid*, p 139. The Khmer style was already recognized by Prince Damrong, *RA*, p 273.

269. Apparently only one of the images survives. See *Śilpakarrm samāy ayudhyā*, Kram Śilpākar, printed for National Children’s Day BE 1514, and fig 5; Vickery, review of Jones, *Thai Titles and Ranks*, p 165, n. 9.

270. Vickery, “The Khmer inscriptions of Tenasserim”.

271. Charnvit, pp 133-45.

272. Lingat, R. “Evolution of the conception of law in Burma and Siam” *JSS*, XXXVIII, 1 (1950), 9-31.

273. Charnvit, pp 135, and 45-56, n 40.

274. Akin Rabibhadana, *The Organization of Thai Society in the Early Bangkok Period*, p 40.

different sources.<sup>275</sup>

There remains just one final matter. In discussing Trailok's war with Chiang Mai Charnvit says, "in 1456, the army of the Chiengmai king ... invaded ... and threatened to capture Chainat".<sup>276</sup> Here I would agree with him that such is what the sources say, but it was already impossible when he wrote to leave the matter at that, since Griswold and Prasert, whom Charnvit often cites approvingly, claim that 'Chainat' at that time meant Phitsanulok, and Charnvit, in another context, has implicitly accepted their claim.<sup>277</sup>

This entire chapter shows almost total loss of control over the sources. Charnvit has tried to combine the formulations of Prince Damrong, Wolters, and Griswold and Prasert, without taking note that they are sometimes contradictory, and this has resulted in some statements which cannot reasonably be based on any of the evidence. The entire section on Trailokanath is hardly anything more than a paraphrase of Prince Damrong's treatment of that reign in his commentary to *RA*, but without adequate indication of this to the reader. Such is not sufficiently original work for a dissertation or book, and in the rare instances where Charnvit adds an original interpretation it is too speculative to be proper in a work of history.

## Conclusion

It should be obvious to the reader that I have found *The Rise of Ayudhya* very disappointing. For the factual history of Ayudhya after its founding Charnvit has

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275. I have discussed the question of Angkorean influences on Ayutthaya in Vickery, review of Jones, *Thai Titles and Ranks*. Research on 'deva-rāja' which was available to Charnvit includes J. Fillozat, "New researches on the relations between India and Cambodia", *Indica* 3 (1966); Hubert de Mestier du Bourg, "A propos du culte du dieu-roi (deva-rāja) au Cambodge", *Cahiers d'histoire mondiale*, XI (1968-69); I.W. Mabbett, "Devarāja", *JSEAH*, c, 2 (1969); Sachchidanand Sahai, *Les institutions politiques et l'organisation administrative du Cambodge ancien* (Paris, 1970). [\*Since then a study by Claude Jacques has noted that in Angkorean Cambodia the term 'devarāja' is found only in the Sanskrit part of the Sdok Kak Thom inscription (K.235/AD 1052) where it seems to be, not the expression of the original conception, but merely a translation of the Khmer title *kamrateñ jagat ta rāja*, perhaps to be translated literally as 'lord of the world/universe [*kamrateñ jagat*] of/for the king'. The literal translation, however, may not be very significant, because the

Angkorean *kamrateñ jagat* were a special class of Khmer deities which included Brahmanical and Buddhist identities as well as apparently deified former living persons. They most resemble the

spirit cults known in Cambodia today as *neak tā*. At least the *kamrateñ jagat ta rāja*, poorly translated already in the 11<sup>th</sup> century, was not the king and was not Śiva, which is not to deny that Cambodian kings enjoyed a semi-divine quality. See Claude Jacques, "Les *kamrateñ jagat* dans l'ancien Cambodge"; Paper presented at the Thirty-First International Congress of Human Sciences in Asia and North Africa, Tokyo, 31 August-7 September, 1983, and published in Proceedings of the conference, pp. 1025-27; Claude Jacques, "The Kamrateñ Jagat in Ancient Cambodia", in *Indus Valley to Mekong Delta Explorations in Epigraphy* edited by Noboru Karashima., Madras 1985: New Era Publications, pp. 269- 286; Michael Vickery, *Society, Economics and Politics in Pre-Angkor Cambodia: The 7th-8th Centuries*; Tokyo. The Centre for East Asian Cultural Studies for Unesco, The Toyo Bunko, 1998, pp. 144-46, 423-25 \*]

276. Charnvit, p 137.

277. *Ibid*, p 121, and see discussion of this above, pp. 35-36 (in original publication

hardly gone beyond his predecessors at all, and in some respects has further confused their already uncritical syntheses. Concerning Ayudhya's founding, his attention to the conflicting stories about Uthong and to early Ayudhya's economic situation is useful, but by forcing the latter, which should have helped situate Ayudhya in a wider context, into a synthesis with the former, he spoiled a good start and has not provided a useful basis on which either he or other historians can build.

In ignoring the international situation of early Ayudhya Charnvit has remained within the traditional historiographic tradition, and this has prevented him from saying anything very helpful about why a kingdom of Dvāravatī flourished, why it disappeared, why the Menam Basin may then have been broken up into small *mo'an*, or why the situation was favourable for new developments in the days of Ayodhya and early Ayudhya.

Judging from some of Charnvit's remarks, including his 'conclusion',<sup>278</sup> he may feel that his major contribution lies not in factual history, but historiography, in his distinction between *tāṇṇān* and *baṇśāvatār* historical traditions. There are no doubt many weaknesses, including a very narrow world view, in the latter, but the former are even less reliable from whatever point of view. Charnvit argues that it is "necessary to consider the concepts and idea of history of a particular time before venturing into historical facts and constructing a new history".<sup>279</sup> I fail to understand what he is trying to say. Facts are not something one ventures into; they are the basic material out of which all history is formed, and they must be **established** with care. The "concepts and idea of history of a particular time" are among the facts of that time and when established may help us understand why certain other facts were treated in particular ways by contemporary writers. But the latter facts, the actions of real individuals and groups of people, as well as broader trends which may not even have been visible to contemporaries, must be discovered through close study of all the evidence. We cannot "know the purpose of certain types of history writing"<sup>280</sup> until we know the underlying facts about which the historians were trying to write. History is not principally past thought, *pace* Collingwood, it is first of all past action and activity. Careful study may eventually reveal an idea of history for a given period, but assumptions about an ethnic or political 'ethos' rather than attention to "the accuracy of all the details of ... reconstruction",<sup>281</sup> will only lead to more of the misty, speculative, personalizing syntheses which have for so long hindered the development of early Southeast Asian historiography to the level expected in the study of other parts of the world.

Whatever our own idea of history we must recognize that the *tāṇṇān* are

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278. Charnvit, pp 148-53.

279. *Ibid*, p 151.

280. *Ibid*.

281. Frank Reynolds, review of *The Rise of Ayudhya*, in JAS, XXXVIII, 1 (Nov 1978), 216-17. [\*In mitigation, it is necessary to emphasize that Charnvit was producing a Ph.D. thesis in an academic environment where the 'Moguls', accepting Collingwood as a sort of deity, insisted on the type of history writing which I criticize here. See my further remarks on this subject in Vickery, "The Composition and Transmission of the Ayudhya and Cambodia Chronicles",

dateless traditions which may not with any certainty be attributed to any period earlier than the date at which they were recorded. There is no evidence that they represent the idea or ethos of history of Ayodhya or early Ayudhya; and a much better working hypothesis, pending the full analysis that must be carried out before they are used at all, is that they, like European *tāmnān*, are a confused mixture of fact and fancy due to people who were grossly ignorant of the facts of the past. An exception to this judgement is *Jinakālamālī*, but as I have already pointed out, it does not support Charnvit's use of *tāmnān* to reconstruct early history. Another point worth noting is that certain *tāmnān*, such as *Gāmhaikār*, continue well into the middle and late Ayudhya periods, for which they have always been recognized, in comparison with the *bañśāvatār*, as aberrant and erroneous both as to fact and chronology. Charnvit seems to accept this judgement, for he no longer relies on *tāmnān* for the period covered by the post-1351 *bañśāvatār*. Why are the *tāmnān* suddenly less valuable after that date? And if they are so inaccurate for periods nearer to the writers' present, how can we assume any special validity for a time many centuries earlier?

Far from being the revolutionary work which Wyatt and Charnvit himself envisioned, *The Rise of Ayudhya* is rather reactionary in the sense of trying to return to an ethos of history writing which is outmoded and in failing to build on the methodological and factual progress which had already been achieved.

I cannot help but wonder if Charnvit's enthusiasm for *tāmnān* as neglected, revolutionary sources did not stem from an enthusiasm for the work of Jit Phumisak (จิต พูมิศักดิ์) who apparently used *tāmnān* as a basis for his theory that Thai society has passed from the primitive communal stage to the slave society stage about 400 years before the establishment of Sukhothai, or in the middle of the ninth century.<sup>282</sup> Although I do not wish here to question enthusiasm for Jit Phumisak, I think that any of his theories which are based on *tāmnān* must certainly be questioned, if not rejected.

Since my reactions to Charnvit's attempted reconstructions are so negative, it is only fair that I suggest certain positive reconstructions which I consider superior. In this connection I shall take up two points, the origins of Uthong as an individual, and an outline of early Ayudhyan development. It must be understood that I still consider both to be hypothetical, as prolegomena to any detailed definitive study, and that such definitive treatment will result from critical exchange of views on all the evidence by all interested scholars.

Generally speaking I am convinced that it is utterly impossible to try to write the history of early Ayudhya, or Sukhothai, or pre-modern Cambodia, or any other part of early Southeast Asia in terms of individual kings and precise political events.

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282. Charnvit Kasetsiri, "Judging the Work of Jit Phumisak" [in Thai], in Charnvit Kasetsiri and Sujati Savasdisri, eds, *Thai History ad Historians* [in Thai] (Bangkok, 2519/1976). Here Charnvit approvingly cites the use of these 'important documents', which most people had neglected. The work of Jit Phumisak in question is โฉมหน้าศักดินปักษย ('The Face of Thai Feudalism').

The sources are just too sparse and insufficiently clear. It will be much more productive to devote our attention to more abstract structural history, such as was outlined for Southeast Asia by Harry J Benda and has been undertaken in much more detail for other parts of the world by Barrington Moore and Perry Anderson.<sup>283</sup>

I hope that the remarks on Uthong, below, will serve as support for the first point, and that the proposed outline of Ayudhyan development will illustrate the second.

*Uthong/udòn*. As Charnvit has clearly demonstrated, the stories of Uthong's origins are multiple, and he has attempted to choose among them. However, when faced with such multiple stories we may also hypothesize that none of them is true and that the multiplicity is because of lack of knowledge at the time they were written.

An intriguing detail about the stories is the name 'Uthong' itself (written ຖ ນອງ *udòn*). The official etymology of the Bangkok *bañavatār* tradition shows it to mean 'cradle of gold', but there are also alternative etymologies, 'source of gold', and 'plenty of gold'.<sup>284</sup> Still another etymology is implied in the van Vliet chronicle, where the prince was originally named 'Ou-e' or 'Ui', simply a plausible Chinese name, and acquired the *dòn* element through marriage to a Chinese princess named Pacham Thong, which of course is not plausible Chinese.<sup>285</sup>

As for the official etymology, *ū* (ູ) is not the central Thai word for cradle, which is *ple* (ຟຳ). In Vientiane Lao *ū* is the common word for cradle, and perhaps it is also in other northern dialects.<sup>286</sup> Although one might argue that it then fits the story, since he was of northern origin, the fact remains that the story is an Ayudhyan concoction in which it appears that a traditional element, *udòn*, had to be explained, and chroniclers searched around for meanings, finally hitting, in one case, on the northern word for cradle. This is just the sort of thing that typically happens in the formation of a folk etymology based on a foreign term of forgotten meaning.<sup>287</sup>

In addition to the various and conflicting stories about Uthong in the chronicles of old Siam, other interesting parallels to the use of *ū* as a ruler's personal name can be found in certain chronicles and quasi-historical tales from neighbouring countries.

In the Mon chronicles of lower Burma a certain Bañā Ú was ruler in Martaban and moved from there to establish a new dynasty in Pegu just about the same time as Uthong was active in Ayudhya. Just like the Uthong of Ayudhyan history, he is

283. Harry J Benda. "The structure of Southeast Asian history", *JSEAH*, III, 1 (1962), 106-38; Barrington Moore, *The Social Origins of Dictatorship and Democracy*; Perry Anderson, *Passages from Antiquity to Feudalism*; and *The Lineages of the Absolutist State* (both by New Left Books, 1974).

284. David K Wyatt, "The abridged royal chronicle", p 31; G/P, *EHS* 10, p 34.

285. Van Vliet, *The Short History of the Kings of Siam*, pp 55-57.

286. See *Văcananukram bāsā lāv khòn ka:suoni su'ksādikān* (Vientiane, BE 2505), p 1070.

287. Jan Vansina, *De la tradition orale*, pp 43-44, 135; Mary R. Hass, *The Prehistory of Languages*, p 79, says that when a word lacks a clear etymology in language B, but has one in language A, then the latter is the original language, something which will be seen below as relevant for 'ūdòn'.

supposed to have come from a provincial town, or former capital, to found what would henceforth be a new political center for his people. According to one Mon chronicle,<sup>288</sup> his reign was 19 years (1364-1383), like that of Uthong, and he was also followed by a king entitled *rājādhīrāj*, although a son, rather than a brother or brother-in-law, who, like the first Param Rājādhīrāj of Ayudhya, was involved in a long series of campaigns against rivals to the north.

Farther afield, in the shan States of Burma there are *u-tòn* stories which are in fact creation myths. For instance, in Male it is related that a female *naga* became pregnant by the sun *nat* (Burmese spirit deity) and laid three eggs. The mountain where she laid them is called ‘U-Daung’ (*u-ton* in transliteration), literally ‘egg-mountain’ in Burmese. Later the eggs were washed away and one went to China to hatch U-Dibwa, the emperor, etc. In Lai Hka the story is reported with variations. One of the eggs became king of birds, the second hatched Pyu Sawt, a king of Pagan, and the third produced a girl who later married U-Dibwa, king of Wideha (China)<sup>289</sup>.

Now in Burmese the etymologies are based on the common words for egg and mountain. Thus if we follow the rules for analyzing folk etymologies we should say that the *u-ton* creation myths came to Ayudhya via the Shan States where Burmese terminology had been assimilated, and, the Burmese words being incomprehensible in Ayudhya they were given new meanings while the creation theme was changed to that of foundation of a kingdom.

An alternative explanation, that the Burmese borrowed an *ūdòn*, ‘golden cradle’, story from northern Siam and reworked it is less likely because no such *ūdòn* story is attested in the north, while egg-origin stories are found over the whole area. In addition to the examples cited above, one might mention the account of the birth of the Thai folk hero, Brahma Ruon, from a *naga* on top of a mountain;<sup>290</sup> the inclusion of the Lai Hka story in a truly Burmese context in the Glass Palace Chronicle;<sup>291</sup> and the creation legend of the Ahom, in which a goddess laid four eggs containing the ancestors of all the creatures in the world.<sup>292</sup> It is a cardinal rule in studying folk traditions that when a story is spread over a wide area including different linguistic groups it may not be assumed true for any single place,<sup>293</sup> and all the stories involving

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288. R Halliday, “Slapat Rājāvan Datow Smiñ ron - a history of kings”.

289. Sir James George Scott, *Gazeteer of Upper Burma and the Shan States*, II, 2, pp 134-35, 4-5 respectively.

290. PN, pp 8-9.

291. Pe Maung Tin and G H Luce, trans, *The Glass Palace Chronicle of the Kings of Burma*, pp 34-36.

292. Rai Sahib Golap Chandra Barua, trans, and ed, *Ahom Buranji* (Calcutta, 1930) p 2.

293. This has been most explicitly stated by the practitioners of ethnohistory, for example, Gaston van Bulck, “Beitrage zur Methodik der Völkerkunde”, *Wiener Beitrage zur Kulturgegeschichte und Linguistik*, II (1931), p. 195. Although more recent anthropological study looks with disfavour on much of the ethnohistorical school, the same principle seems to be followed in studying folk tradition in recent years. For example, H Deschamps, “Traditions orales au Gabon”, *The Historian in Tropical Africa*, eds, J Vansina, R Mauny, L V Thomas (Oxford, 1964) p. 173, speaks of the problems of “an assimilation of traditions to that of neighbouring peoples”; and J Vansina, *De la*

a man named Ū or Ūdòn, are just that--folk tales which need much more analysis over a much wider area than just Siam. I should think that a profitable line of investigation would be to examine whether the egg-mountain myth was not a common Thai creation myth, or perhaps a myth common to the Thai, Burmese and other neighbouring peoples, and that the Burmese terms *u*, 'egg', and *ū-tòn*, 'egg-mountain', for a hero born from the egg, passed on to the Mon and Thai at a time when Burma was the dominant power in the areas, and were then reinterpreted in the local languages. That sort of thing would have been particularly easy in multilingual early Ayudhya.

All that emerges with any certainty from the various stories surrounding the names Ū, Uthong, etc, and the origins of the founder of Ayudhya is that (a) when the extant records were first compiled no one knew how or by whom the city had been founded; (b) these stories may not be used directly for the reconstruction of Ayudhyan history; and (c) there was probably never an Ayudhyan ruler known to contemporaries as 'Uthong'.

*Ayudhyan origins.* The study of early Southeast Asian history seems to show that the states which developed there in early historical times belong to one or the other of two broad, but significant, socio-economic categories: (a) inland agrarian states, and (b) coastal trading states; and that these categories have analytical utility at least until the fifteenth century.<sup>294</sup> Among the second category one may also distinguish further between entrepot states and those which exported their own products.

It also seems clear that there were certain rhythms, or patterns, in the development of the maritime states, and also in the transition from one type to the other in certain areas, and that these rhythms depended to a large degree on the nature of external demand for Southeast Asian products, or products transited through Southeast Asia, in particular demand by China, and Chinese government policy in connection with such demand.<sup>295</sup>

Along with the recognition of these categories and rhythms has come an awareness of certain immediately observable characteristic features of each category. The inland states are characterized by large numbers of impressive temples of stone and brick of a high level of architectural and artistic achievement and by an enormous corpus of stone or metal-plate inscriptions concerned with the establishment of such buildings or with the control of land, status of officials, organization of population, etc. There is also rather clear indication, either in inscriptions or on the ground, of

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*tradition orale*, p 66, also notes that oral tradition can be influenced by diffusion and may thereby lose whatever historical value it possessed. Within the context of Southeast Asian history this principle has been most clearly stated by Louis Damais. "Une mention de l'ère *saka* dans le *Ming Che*", BEFEO, L. 1 (1964), 31-32.

294. Benda, *op cit*; Bennet Bronson, "Exchange at the upstream and downstream ends: notes toward a functional model of the coastal state in Southeast Asia", pp 39-41, 51, in *Economic Exchange and Social Interaction in Southeast Asia*; Kenneth R Hall, *loc cit*, nn. 118-119 above.

295. Wang Gungwu, "The Nanhai trade", JMBRAS, XXXI, 2 (1959); O W Wolters, *Early Indonesian Commerce*; and *The Fall of Srivijaya*.

considerable attention to irrigation. On the other hand, the pure type of coastal trading centers are almost completely devoid of all such material remains or indigenous records, and knowledge of those places is derived mainly from the writings of foreigners, Chinese, western Asians, and finally Europeans.

It is clear from the records of the first category that strict control of the population was an important feature of that type of state, while considerations of the nature of trading, plus the descriptions of later port states such as Malacca or Aceh, lead to the belief that in polities of the latter category the population was less rigidly organized and more cosmopolitan, the hinterland peoples were left alone, and the channels of authority and control were much more diffuse.<sup>296</sup>

One would expect, and indeed the evidence shows, polities which were transitional or intermediate between the two extreme types, the best-known being the states of eastern Java between the tenth and fifteenth centuries. They were oriented toward foreign trade, mainly as exporters of rice, but some of the monumental institutions of the earlier, agrarian, central Javanese period were maintained. Temples were built, but fewer, smaller, and of different function. Many inscriptions were left, but their form and content reflect the changes in socio-economic structure. This intermediate type might result, in theory, from the influence of an immediate.....e predecessor which was typically inland-agrarian, as in Java, or from the structural requirements of a state exporting its own products, as in East Java, as opposed to the entrepot states, like Srivijaya, which have hitherto been considered as the pure type of the coastal trading category.<sup>297</sup>

Some recent research has also been devoted to the development of trade within and among the agrarian states; and the existence of such trade should occasion no surprise, for at the very least some luxuries which could not be obtained or manufactured locally always had to be imported.<sup>298</sup> Nevertheless, the dominant mode of production in classical Angkor or Pagan was intensive agriculture, just as the dominant mode of production, or of economic activity if production is denied, in Funan, Srivijaya, and Malacca, was maritime trade. We might note in this connection the importance of coinage in Oc-Eo (presumably Funan), Dvāravāṭī, Sriksetra, and Arakan at a time, fifth to eighth centuries, when those polities sat astride an important maritime trade route, the disappearance of coinage during the time of classical Pagan and Angkor, and its reappearance in the fourteenth century when the theory of

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296. Benda, *op cit*, p 113; Denys Lombard, *Le Sultanat d'Atjeh au temps d'Iskandar Muda 1607-1636*, 49-60; J. C. van Leur, *Indonesian trade and Society*, pp 66-67, 78, 104-07, 354, n 47; and for a general theoretical suggestion about the relative freedom of maritime societies, see Gerhard E. Lenski, *Power and Privilege*, p 192.

297. On the economy of Java, see Jan Wisseman, "Markets and trade in pre-Majapahit Java", especially p 206.; John K Whitmore, "The opening of Southeast Asia: trading patterns through the centuries", pp 143-44. See also Slametmuljana, *A Story of Majapahit*, p 115.

298. Kenneth R Hall, "Khmer commercial development and foreign contacts under Suryavarman I", *JESHO*, XVIII (1975) 318-36; Kenneth R Hall and John K Whitmore, "Southeast Asian trade and the Isthmian struggle, 1000-1200 AD", in *Exploration in Early Southeast Asian History*; articles by Wisseman and Whitmore cited in n. 297 above. [\*See also Michael Vickery, Review of Kenneth R. Hall *Maritime Trade and State Development in Early Southeast Asia.*\*]

rhythms in international trade shows some of the mainland areas again being drawn into the international maritime trade network<sup>299</sup>.

Do these categories and rhythms have any relevance for the study of early Thai or Siamese centers, and if so, how do Ayodhya, Ayudhya, and Sukhothai fit into the larger Southeast Asian pattern?

We may first of all state definitely that Sukhothai belonged to the pure type of inland agrarian state existing in a largely self-sufficient manner on a dominant mode of production which was intensive rice agriculture. In its temples, inscriptions, lack of currency, attention to irrigation, implied existence of a state-supported artisan class and restricted peasantry, it shows the characteristic traits of Angkor or Pagan.<sup>300</sup> [\*Since this article was first written there is an increasing controversy as to whether the impressive hydraulic works at Angkor were mainly for agricultural irrigation, and that rice then, as now, depended on rain and small-scale local storage.\*]

Furthermore, Sukhothai's geographical location is such that in no circumstance could it have depended on maritime trade for its existence, although as in other inland centers some trade, in certain specialized products, inevitably existed. [\*After this article was published in *JSS* in 1979 I spent 1982-1988 working with the Thai Ceramics Archaeological Project at the University of Adelaide whose project was study of the kilns and ceramics of the Sukhothai area, mostly near the town now named Sri Satchanalai, about 50 km north of Sukhothai. Because of TCAP it is now known that there were thousands of kilns some producing high quality ceramics which were exported, some via Burma and perhaps some via Ayutthaya, although there is nothing about this in either inscriptions or chronicles.<sup>300a</sup> See Vickery, "The Old City of 'Chaliang'--'Srī Satchanalai'--'Sawankhalok' a Problem in History and Historiography", in this volume, pp.00-00\*]

Ayudhya, in the standard treatment of its history, has also been assumed to fit into the same category. Its supposedly Brahmanical, despotic, presumably Angkorean, heritage has been emphasized<sup>301</sup>; and although the writers who produce, and reproduce, this picture rarely show much concern with economy or modes of production, the features emphasized are those which elsewhere consistently accompany the development of inland agrarian states. Skinner first gave some prominence to another aspect of Ayudhyan development, its maritime activities;

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299. Pamela Gutman, "The ancient coinage of Southeast Asia" *JSS*, LXLI, 1 (Jan 1978), 8-21.

300. Evidence for irrigation is found both in inscriptions and in remains still visible; and the very layout of the old city (see map, G/P, *EHS* 2, *JSS*, LVII, 1, Jan 1969) reflects an effort to copy the city plan of Angkor, with large artificial ponds to the east and west and a smaller one to the north. Restrictions on the populace are implied by inscription No 38, as I read it (see Vickery, "Guide", pp 230-32, in this volume, pp. 00-00).

<sup>300a</sup> Don Hein, "'Bullet' Coins Excavated at Sisatchanalai, Thailand" p. 1, the ceramic industry "operated from about the tenth century AD for...about six hundred years....The site contains the ruins of about one thousand kilns...also remains of about one hundred metal furnaces"; Don Hein and Mike Barbetti, "Sisatchanalai and the Development of Glazed Stoneware in Southeast Asia".

301. Charnvit, pp 22, 100-03; G/P, "On kingship and society", pp 68-72.

Wolters briefly noted it;<sup>302</sup> and Charnvit, in what might have been a major contribution, again emphasized it; but he was ultimately unable to separate it from the more traditional picture.

It is obvious now that the history of Ayudhya must begin with the history of the entire lower Manam Basin, for which the first relevant documentary (as opposed to archaeological) evidence is the Chinese reports about 'Hsien'. Although the real meaning of *hsien* at that time is still of interest, it can be ignored for the present. The Chinese were interested in ports, and even if Hsien was somehow politically subservient to Sukhothai, the latter was an inland center, whose ties with its dependencies, according to recent research, were weak, whereas the Hsien which in Chinese eyes began its development in the 1280s consisted of one or more ports in the lower Menam area. Moreover, the Chinese also knew 'Su-ku-t'ai' separately, and evidently gave it little importance, since it is only mentioned twice.

Not only is there at least one Yuan dynasty record in 1299 which recorded envoys from both *Hsien* and *Su-ku-tai* at the same time, but there is an even more explicit Yuan period record which states that *hsien* [*xian* in the article in question] controlled, or was the link to, "upper water" or "go upriver" *Su-gu-di*, meaning that not only were Sukhothai and *Hsien* different places, but that Sukhothai was upriver from *Hsien*, implicitly placing the latter downstream<sup>303</sup>

The most interesting thing for us now is that the development of Hsien and later Ayudhya from the 1280s and on through the fourteenth century fits into the general Southeast Asian pattern of trade rhythms and alternating development and decline of states.

As Wolters has written, Srivijaya, already weakened by Javanese competition after the tenth century, was further weakened, and finally destroyed, by changes in Chinese trade policy under the Southern Sung and Yuan dynasties from the late twelfth through mid-fourteenth century. During that period the Chinese, rather than depending on foreign shipping, sent out increasingly large fleets of their own to trade with Southeast Asian ports, depriving Srivijaya of its privileged position and

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302. G William Skinner, *Chinese Society in Thailand*, pp 1-5; O W Wolters, *The Fall of Srivijaya in Malay History*, pp 66-67.

303 G H Luce, "The early Syām in Burma's history", *JSS*, XLVI, 2 (Nov 1958), p 140; [\*Tatsuro Yamamoto, "Thailand as it is referred to in the Da-de Nan-hai zhi at the beginning of the fourteenth century", *Journal of East-West Maritime Relations*, Vol. 1 (1989), pp. 47-58; Geoff Wade, "The Ming Shi-Lu as a Source for Thai History 14th to 17th Century", paper presented at the 5th International Conference on Thai Studies-SOAS, London, 1993 p. 25. Charnvit Kasetsiri now agrees that in the 14th century the Chinese intended *Hsien*, their rendering of 'siam'/syām (as it was written in Old Khmer and Cham), as a name for the lower Menam basin, including Ayutthaya, not Sukhothai. See his "Ayudhya: Capital-Port of Siam and its Chinese Connection in the Fourteenth and Fifteenth Centuries", pp. 75-81. On the contrary, David K. Wyatt, ignoring new work, asserted boldly in his *Thailand: A Short History*, p. 58, that *Sien* (*Hsien*) was Sukhothai. This is his only reference to that term, and in citing Zhou Daguan's contemporary report on Angkor, where *Hsien* is prominent, Wyatt used 'Siam'. Nevertheless, when describing Rāmādhipaī's settlement of Ayutthaya , p. 66, he called it "a port City of some antiquity", which fits precisely the *Hsien* described by Chinese writers since the 1280s, and which name the Chinese would continue to use for Ayutthaya until modern times.\*]

encouraging the growth of competitors. Among the new ports taking advantage of the new opportunities were several along the coast of Sumatra, and of interest to us, Hsien, first noticed by the Chinese in 1282. Probably the missions to China from Lavo and Chén-li-fu were also related to the same process.<sup>304</sup>

When Chinese policy again changed in the late fourteenth century, with private Sino-Southeast Asian trade made illegal and the tribute system falling into disuse,<sup>305</sup> many of the new ports suffered, but Ayudhya (still Hsien [-lo] for the Chinese) was able to try and fill the vacuum because of its ‘unsullied record’ as an obedient vassal. Another such favoured port was Pasai, and as the old tributary trade was reinvigorated they, and a few other ports, competed for the position of favoured Southeast Asian entrepot, a reward finally won by Malacca.

Hsien, or Ayodhya, that is the lower Menam area, thus began a new development with the change in Chinese policy of the twelfth-thirteenth centuries and was able to continue its development when the Chinese policy changed again at the end of the fourteenth. Moreover, recent work on historical ecology indicates that the area of Ayudhya, as a result of excessive flooding, was unfavourable for agriculture before modern times, and could only have developed as a commercial center.<sup>306</sup> This also fits very well with the idea that ‘Hsien’ always meant some place in the general Ayudhyan area, since the Chinese reported that the soil of Hsien was infertile because of its dampness.<sup>307</sup>

Because of lack of sources we can know nothing of the rulers of the Hsien’ Ayodhya area before 1351; and it is clear, both from the early sections of *LP* and from the Ming records, that other competing centers, such as Suphanburi, still existed in the last half of the fourteenth century. The Chinese remark of 1349 concerning conflict between Hsien and Lo-hu<sup>308</sup> may reflect a first effort at merger of these states, and it may well have been effected by the rulers of Lopburi who began to date a new dynastic period, that seen in the Ayutthayan *bañśāvatār*, from shortly thereafter. In any case it seems certain that none of the Uthong stories are very helpful in studying early Ayudhya, and it would probably be well for historians to ignore them. Furthermore, in view of the close Chinese interest in Hsien, which had been developing steadily since the 1280s, and their attention to a political change in 1349, it is difficult to believe that an ‘Uthong’ from Petchaburi, or any more distant place, taking power in Ayudhya, which would represent a sort of conquest, would not have been noticed by the Chinese. We are forced to assume that the rulers of Ayudhya after 1351 were strictly local people, descended from families who had gradually accumulated power in the Ayudhya-Suphanburi-Lopburi triangle over the previous century.

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304. Coedès, *The Indianized States*, pp 221-22; O W Wolters, “Chen-li- fu, a state on the Gulf of Siam at the beginning of the 13th century”.

305. Wolters, *The Fall of Srivijaya*, pp 66-67.

306. Takaya, “An ecological interpretation of Thai history”.

307. W W Rockhill, “Notes on the relations and trade of China ...”, *T'oung Pao*, XVI (1915), p 101;e [\*and see work by Ishii and Baker in note 61 above\*]

308. W W Rockhill, pp 99-100.

From the remarks of the first Portuguese writers in Southeast Asia, the van Vliet chronicle, and the Malay histories, it seems that a major interest of Ayudhya in the fourteenth and early fifteenth centuries was control of the Malay Peninsula, perhaps in order to dominate the entrepot trade of the Malacca Straits. When this policy failed, Ayudhya turned to the conquest of its hinterland and the export of local products. The new policy succeeded, and Ayudhya was known to Chinese, Japanese and finally Europeans as a source of many valuable trade goods.

The process of expansion was also accompanied by the adoption of some of the characteristics of inland monumental Angkor and Sukhothai: the construction of large permanent temples, strict control of the population, a complex hierarchy of officials, but **not**, interestingly, the habit of writing all manner of permanent records on stone. Ayudhya became, like Eastern Java, and possibly contemporary Pegu, a mixture of the two major types of Southeast Asian state, controlling a large hinterland through agrarian bureaucratic institutions, but deriving a significant portion of its revenue from international trade. The authoritarian, despotic character of Ayudhya which was clear to foreign observers from the sixteenth century onward, was probably not part of its origins, ‘inherited from Angkor’, but something which developed later along with its territorial expansion, and perhaps through direct Sukhothai influence. A probably near-contemporary account of the Sukhothai style of rule as it was imposed on Ayudhya is to be found in the van Vliet chronicle’s description of the reign of Narešuor.<sup>309</sup>

This is as far as I intend to pursue this sketch, which I present as an alternative way of considering early Ayudhyan history. There is much room for refinement and filling in of details. For example, since it appears that the Sukhothai pottery industry must be redated,<sup>310</sup> to what extent were the wars among Ayudhya, Sukhothai, and Chiang Mai in the last half of the fifteenth century directly related to control of that valuable export? This is a problem which gets no attention from either the *tāmnān* or *bañśāvatār* schools of history-writing, but which is of primary interest to historians now.

## POSTSCRIPT I

### Note on the work of Prince Damrong

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309. In earlier writing I have noted some evidence that part of the bureaucratic hierarchy and legal system may have been borrowed from Sukhothai, and that such borrowing from Angkor may have been less than hitherto believed. See my reviews of Jones, *Thai Titles and Rank*, [in this volume](#), pp.00-00; Yoneo Ishii, *et al*, *A Glossarial Index*, n 4; and van Vliet, *The Short History of the Kings of Siam*, [in this volume](#), pp. 00-00, [\*See also Michael Vickery, "The Constitution of Ayutthaya", in *New Light on Thai Legal History*, Edited by Andrew Huxley, Bangkok: White Orchid Press, 1996, pp. 133-210; and once more see the comments on this in Chris Baker, "Ayutthaya Rising: From the Land or From the Sea"]

310. Hiram W Woodward, Jr, "The dating of Sukhothai and Sawankhalok ceramics: some considerations", *JSS*, LXVI, 1 (Jan 1978), pp 1-7.

In my criticism of Charnvit's reliance on Prince Damrong, I have often been led to criticism of Prince Damrong's work itself, but I do not wish the reader to feel that I am denigrating Prince Damrong's scholarly activity. Given the intellectual atmosphere of his time, his busy administrative career, and the sources and previous historical work at his disposal, Prince Damrong's historical work represents a truly impressive achievement in methodology, critical standards of source analysis, and historical synthesis. Nevertheless, his conclusions do not always represent ultimate historical truth, either because (a) later discoveries force new conclusions; or (b) critical methods have become more refined; or (c) Prince Damrong, like other people, occasionally made mistakes. For (a) compare Prince Damrong with Griswold and Prasert on the identities of Prince Yuddhiṣṭhira and Brahma Jalian,<sup>311</sup> and for (c) see my remarks on the *rājādhirāj* evidence for the Sukhothai origin of Trailokanath's mother.<sup>312</sup> On the question of (b) some discussion is necessary, particularly since many of the statements which I have criticized above as too speculative derive from Prince Damrong's reconstructions.

When Prince Damrong wrote his commentaries on the reigns of the early Ayudhyan kings he was concerned first with explaining conflicting evidence and then with filling in plausible details for events only briefly mentioned in the chronicles. Where *LP* and *RA* were in conflict he almost always preferred *LP*, a choice still supported by historians today. In the second instance, however, it is generally recognized today that a merely plausible story is not sufficient for history--historical fiction may be equally plausible; and what the historian must do is determine the most **probable** explanation of the evidence within the limits of the generally accepted rules for the logical construction of arguments. As an example of the problem, let us take Prince Damrong's hypothesis that Trailok's construction of a temple within the grounds of the Ayudhyan palace was an attempt to emulate Sukhothai practice, which is a **plausible** reconstruction. This would represent a diffusion of Sukhothai practice; but it is a solid principle of modern archaeological method that diffusion may not be argued unless the things to be compared are formally and functionally identical, in this case if the two Buddhist *vāt*, which **are** functionally identical, were built in precisely the same relationship to the nearby palaces, which, even from Prince Damrong's description, is clearly not the case<sup>313</sup>. The argument would also require that there be no other plausible model, such as Angkor. Likewise we could only argue for Sukhothai literary influence on Trailok if he had composed a new edition of the *Traibhūmi* instead of a *jataka* collection.

In defence of Prince Damrong as a historian it must be emphasized that he often qualified his own reconstructions as guesses or suppositions, showing thereby a greater critical awareness than many writers of later generations.

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311. Damrong, R A, pp 170-71; Griswold, "Yudhiṣṭhira", pp 223-24, and n. 8; G/P "A fifteenth-century Siamese historical poem", pp 134-37.

312. Vickery, "Guide", pp 189-90.

313. Charnvit, p 136; Damrong, RA, p 264.

## POSTSCRIPT II

### The ‘epicyclical fallacy’

On two earlier occasions<sup>314</sup> I have used the term ‘epicycle’, by analogy with the Ptolemaic system of astronomy, in referring to a certain fallacious manner of reasoning in historical synthesis. Since at least the fourteenth century one of the basic principles of logical reasoning, known as ‘Ockham’s razor’, after William of Ockham, c 1285-1349, has held that “the principle of parsimony [should be] ... employed as a methodological principle of economy in explanation”. This means, for our purposes, that “plurality is not be assumed without necessity”, and “what can be done with fewer [assumptions] is done in vain with more” - (brackets in original). Ockham’s intention was “the elimination of pseudo-explanatory entities”, and his principle requires that “nothing is to be assumed as necessary in accounting for any fact, unless it is established by evident experience or evident reasoning, or is required by the articles of faith”.<sup>315</sup> Today, of course, Ockham’s principle would be modified to remove the sacred character of the “articles of [Christian] faith”.

Although failure to observe Ockham’s principle is recognized as a fallacy, the fallacy apparently has no general name; but since a well-known example of neglect of the principle is found in Ptolemy’s epicycles, I have decided to call it the ‘epicyclical fallacy’ and to characterize unnecessary, or illegitimate, assumptions in historical reconstruction as epicycles.

In his “Remarks on ‘The Lion Prince’”,<sup>316</sup> Prince Chand Chirayu Rajani criticized my analysis on two principal points, the existence of the princes Nām Duom and Jaiy Sangrām, and the genealogy of Manrāy’s descendants. I responded to the first in “Guide through some recent Sukhothai historiography”,<sup>317</sup> but did not have a suitable context for answering the second criticism until now, when it is appropriate because Prince Chand’s explanation is an excellent example of a historical epicycle. I argued in “Lion Prince” that the contemporary evidence of inscription No 62, which shows only four generations, made it necessary to remove two generations from the chronicle genealogies of Manrāy’s descendants.<sup>318</sup> Prince Chand agreed to the extent that there were two superfluous generations in the stories of *Jinakālamālī* and the *Chiang Mai Chronicle (CMC)*,<sup>319</sup> but instead of dismissing them as fictitious, he invented a second ‘Kam Fu’ in order to divide the family into two branches with four generations in each. This is the purest type of epicycle, in which the assumption is not embedded in the evidence, and in fact violates it. Besides this, Prince Chand confused the issue with the quotation: “in inscription (62) [Kam Fu] was a son of Mangrai and therefore a younger brother of Jaya Songkram”.

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314. Vickery, “Lion Prince”, p 269; and “Guide”, p 185.

315. All citations are from *The Encyclopaedia of Philosophy*, vol VIII (1967), p 307.

316. In *JSS*, LXV, 1 (Jan 1977), pp 281-91.

317. Vickery, “Guide”, pp 198-203.

318. Vickery, “Lion Prince”, pp 371-76.

319. Chand, “Remarks”, p 288.

In fact, there is no mention of ‘Jaya Songkram’/Jaiy Sangrām in inscription 62, which was one of the reasons I declared him fictitious. Prince Chand wrote “it is better to follow epigraphic evidence”,<sup>320</sup> but after that he presented a table partially illustrating his epicycle and quite contrary to the epigraphic evidence, that is inscription 62.<sup>321</sup>

Prince Chand next brought up another interesting point, that in No 62 the controversial name appears to be written, not ‘Kam Fu’, but ‘Kam Bhu’ thus more accurately ‘Gām Bū’, which I continue to use here), and I fully agree that such is what the published illustration of No 62 shows. This means, according to Prince Chand, that there were two Gām Bū’s or one Gām Bū reigning in Chiang Mai (No 62), and one Gām Fū, a king of Chiang Saen.<sup>322</sup>

However, it was not just “the editor of the inscription [in *Silā Cāru’k III?*]... with the chronicles in mind”, who produced ‘Gām Fū.. The sixteenth-century Jinakālamālī called the prince of that generation ‘Hariipyava’, which apparently means ‘floating gold’, that is, *gām fū*,<sup>323</sup> all published versions of the *CMC* have ‘Gām Fū’,<sup>324</sup> and this reading was accepted by Coedès in 1925.<sup>325</sup> Thus, if there has been corruption in the texts it occurred less than 200 years after the events in question, perhaps under the influence of the old Thai mythical Gām Fū, who appears in the Nan ancestor list of No 45.<sup>326</sup> Either that, or there is some merit in Prince Chand’s contention that there were two persons, Gām Bū and Gām Fū.

The obvious weak point in this theory of a double genealogy is that the chronicle list of ‘Chiang Saen kings’, who in fact also took turns reigning in Chiang Mai, continues very explicitly with Gām Fū’s son Hrayu/Phāyu, then his son Kilana/Kū’nā (Prince Chand’s Guna); and we must then assume, not just two Gām Bū/Fū’s, but also two Hrayu/Phāyu’s and two Kilana/ Kū’nā’s.<sup>327</sup> But the details of the reign of Kilana/Kū’nā of the chronicles are so close to the details of the reign of the Sòn Saen Nā of No 62 that the protagonists of each must be identical, as has so far been assumed by all historians.<sup>328</sup> Thus, if the last individual of this segment of the genealogies is a single person, there can be only one line of immediate paternal

320. *Ibid*, p 290.

321. The inscription of Vat Jiañ Mān (Prince Chand's 'Chieng Mun', 'Remarks', p 283) is not reliable epigraphic evidence because, written in 1581, it is of no more value for the Māñrāy period than the chronicles, whose story it incorporates.

322. Chand, “Remarks”, p 291.

323. Coedès, “Documents”, p 93, n 2.

324. The two best-known published versions are *Tāmnān phünmöañ Jian Mai* Bangkok, B E 2514); and the French translation by Camille Notton, *Annales du Siam, III, chronique de Xieng Mai* (Paris, 1932).

325. Coedès, “Documents”, pp 195, 197.

326. See inscription No XLV, in *Prachumśilā cāru’k III*, and in G/P, *EHS 3, JSS, LVII*, 1 (Jan 1969), 80-81.

327. Coedès, “Documents”, pp 93-95.

328. Coedès, “Documents”, pp 95-97, 102; G/P *EHS 13*, “The inscription of Wat Pra Y:un”, *JSS*, LXII, 1 (Jan 1974), pp 123-42.

ancestors--his single father Phāyū and the latter's father Gām Bū. When we look at the entire genealogy from a point at which there is no doubt about direct comparability Prince Chand's theory of a double line of descent from Mānrāy breaks down, and my proposal for dealing with the ambiguous generations of Jaiy Sangrām and Saen Bhu is not only logically preferable, but the only way to account for the evidence, unless we wish to invoke, as the medieval scholastic Europeans did, certain 'articles of faith' which must not be challenged.

There is still more evidence of interest to the study of these genealogies. Prince Chand noted that in the *Mūlaśāsanā*, presumably an older text than *Jinakālamālī*, "the son that Saen Bhu sent to be king of Chieng Saen was called Mun Jedtra".<sup>329</sup> However that may be, the point is not very useful for our purposes, since the standard *Mūlaśāsanā*, with the exception of some interesting differences in spelling, also has the same genealogy as *Jinakālamālī* and CMC: Mānrāy - Grām-Saen Bū (note) - Gām Bū (note) - Phāyū - Kilana.<sup>330</sup> A name with some resemblance to 'Jedtra' is found in another version of *Mūlaśāsanā*,<sup>331</sup> which has Mānrāy - Phāyū-Āy Cet Bāntu – Ku'nā, and which makes Saen Bhū a son of Mānrāy and brother of Phāyū. Gām Bū/Fū is missing, as is the dubious Jaiy Sangrām; and there are only four generations between Mānrāy and Ku'nā, as in No 62 - and as Prince Chand has accepted as the most reasonable picture of reality. There may be still more versions of this genealogy in as yet unexplored texts. Coedès reported a tradition that 'Phan Tu' (*bāntu*) was another name of Ku'nā,<sup>332</sup> which would mean that the second version of *Mūlaśāsanā* mistakenly dropped the real Gām Bū, and split his grandson's identity in order to make the required four generations. If one is going to build epicycles and multiply identities, then all of these names have to be treated as equally real, and I think it is obvious that the picture created would be impossible.

It is much better to apply Ockham's razor, accept the list of No 62 as a base, and judge the chronicles according to how well they agree with it. Prince Chand accepts that there was only one Mānrāy and at the other end the singularity of Kilana/Ku'nā (Guna)/Sòn Saen Nā cannot be doubted. For the two generations in between, Phāyū and Gām Bū (preferable to Fū)<sup>333</sup> are the best choices since they are found in three major chronicle traditions as well as the inscription. The only way to

329. Chand, "Remarks", p 291; and I am assuming Prince Chand means the standard *Mūlaśāsanā*, of which the edition I have used is Cremation volume for Jum PholThor, 3 December 2482, with the imprimature of Krom Šilpākor, 23 August 2482.

330. *Mūlaśāsanā*, pp 222-24.

331. *Mūlaśāsanā*, version of Vat Padaeng. Transliteration Series IX, by Sommai Premchit, Department of Sociology and Anthropology, Chiang Mai University, January 1976. An English translation by Sommai Premchit and Donald K Sweareer has been published in *JSS*, LXV, 2 (July 1977), 73-110.

332. Coedès, "Documents", p 95, n 1.

333. Since *Mūlaśāsanā* and inscription No LXII agree on this point. Credit goes to Prince Chand for calling our attention to the writing, '*bū*', on No LXII.

rehabilitate Jaiy Sangrām and Saen Bū (Bhū) would be to postulate that they descended from Mānrāy in an entirely different line, but then one would be writing historical fiction, not history, since there is no evidence for it in any of the better sources.

*Cathmāyhet hor* ('Astrologers' records'): the following were catalogued in the National Library, Bangkok, in 1971-72:

*Paṭidin* (yearly calendars):

- No 1, Pig Year, *cula* era 1
- No 8, Tiger Year, *cula* 712
- No 8/k, Tiger Year, *cula* 712
- No 9, Dragon Year, *cula* 1146
- No 9/k, Dragon Year, *cula* 1146
- No 22, Dragon Year, *cula* 1170
- No 22/k, Dragon Year, *cula* 1170
- No 23, Snake Year, *cula* 1171
- No 38, Monkey Year, *cula* 1186
- No 38/k, Monkey Year, *cula* 1186

*Pūm* (calendars covering several years):

- No 163, Bull Year 1071 - Snake Year 1087
- No 164, Snake Year 1051 - Snake Year 1128
- No 166, Rat Year 1094 - Horse Year 1244
- No 166/k, Rat Year 1094 - Horse Year 1244
- No 168, Bull Year 1143 - Horse Year 1220
- No 169, Pig Year 1045 - Tiger Year 1156

*Cathmāyhet nai pūm* (calendar with events recorded in it):

- No 202, for reigns I-V of the Bangkok period

*Cathmāyhet hor* (astrologers; record of events):

- No 157, a modern manuscript; the version published in *PP*, part 8
- No 158, for the years *cula* 1087-1218
- No 158/k, nearly the same as No 158; some of the commentary is different
- No 159, part 1, for the years *cula* 1120-1188; part II for 1144-1257, with a gap for the years 1215-1236.
- No 160, for *cula* 1218-1236
- No 161, a pencilled manuscript