

### Coedès' Histories of Cambodia

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On the back cover of the new printing of the 1968 English translation of Coedès' *The Indianized States of Southeast Asia* (Coedès 1968b), there are two extremely flattering testimonials by leading American historians of Southeast Asia.

O.W. Wolters was convinced that "No course on earlier Southeast Asian history should be taught anywhere for foreseeable time without frequent reference to Coedès' book"; and David K. Wyatt chimed in with, "Not surprisingly, the students upon whom this reviewer has tested this volume as a textbook appreciated its authority and admired its rigor and [Coedès] standards".

That English translation had been made from the 1964 'second edition' of Coedès' *Les états hindouisés d'Indochine et d'Indonésie* [Coedès 1964a], little changed from the first edition issued in 1948. Moreover, that first edition of *Les états hindouisés d'Indochine et d'Indonésie* was nothing more than a new edition, in fact mostly a reprinting, of an earlier title, *Histoire ancienne des états hindouisés d'extrême-orient* (1944). These three volumes should not be called separate books, but three editions of the same work. The greatest change from 1944 to 1948 was the title, something I only discovered when preparing this paper, for Coedès 1944 is very difficult to find and I had never seen it before.<sup>1</sup>

[\*The purpose of this paper is to record disagreement with the assessments of Wolters and Wyatt. I intend to demonstrate that Coedès' history is obsolete, and should not be given to students as a basic text. Not all the obsolescence is the fault of Coedès, because much new work has been done in the 30 years since his last edition, but in some places a degree of obsolescence was there from the beginning in 1944, and to a large extent because of Coedès' presuppositions about what constituted history, and what types of source material should be synthesized. [\*Text and details not in the original presentation are enclosed in brackets and asterisks, as [\*.....\*]

Having tried to teach from this book (in Penang 1973-79) after having, in a sense, been taught from it, or rather with it (Yale 1967-1970), I would agree with Wolters to the extent that, just because it exists and has been treated as a sort of Aristotelian scholastic authority, "frequent reference" is required, indeed

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<sup>1</sup> I wish to thank Louis Gabaude for procuring a copy.

I would say it is required reading for students intending to become Southeast Asia specialists, but the “frequent reference” must be critical. As for Wyatt’s students, I can only assume that they wanted to get through the course without trouble, for my students in the beginning of my Penang period (1973-75), when the level of English competence at Universiti Sains Malaysia was nearly equal to that at Yale, were quite intelligently bored with, if not contemptuous of, history *à la* Coedès. In the second year I tried teaching it critically, that is, for each section, demonstrating the defects in Coedès’ arguments. The reaction from the students was, quite naturally, then, why bother?

Perhaps they were right, and that Coedès should be consigned to the shelf, to be dug out by future Ph.D. researchers looking for exotica in the field, like Dodd 1923 on the Thais as ancestors of the Chinese, with new students introduced directly to the primary sources, rather than having to guess at what they record via the interpretations of Coedès, often via, at a second remove, from others (Finot, Krom, etc.) And in my present work, teaching early Cambodian history to Cambodian archaeology majors who can read neither English nor French adequately to absorb Coedès, I have found that this is the only way to proceed. I refer to Coedès, but teach original sources. [\* Coedès’ *Les états/States* is for Southeast Asian history what Aristotle is for Western philosophy, or Ptolemy and Copernicus for Astronomy--classics which specialists in the fields must know, but not a reliable base for beginners.\*]

It is perhaps not fair to begin by emphasizing *Les états/States*, in a discussion of Coedès as a historian, because his greatness does not lie there. Unfortunately, however, for the non-specialist, and that means those who are not specialist students of Cambodia conversant with original sources, this book has come to mean the work of Coedès *par excellence*. [\* This book, however, and even more so his *Les peuples de la péninsule indochinoise/The Making of Southeast Asia* (Coedès 1962, 1969) were, for academic works, popularizations, in French *vulgarisations*.\*] There is nothing essentially wrong with writing popular treatments of historical subjects for a non-specialist public. Indeed it is worthy and necessary. The popular treatments, though, should present, in non-specialist language for the general literate public, the results of the best specialist scientific work. They should not simply seek to entertain, or to be vehicles for speculative reconstructions which would not pass muster if presented the same way in an academic journal.

[\* Coedès, in his books, did not write as a scientist. These books are not high-standard scholarship. They are intellectual entertainment for well-read *dilletante*. They are monuments to uncritical synthesization, some of which belongs in historical romance, not in history. Coedès was a great synthesizer--indeed that may have been his greatest talent when functioning as a writer of historical accounts; and he had to find, or imagine, a connection between every detail and some other detail in another time or place.

Coedès’ real work, which, unlike these books, does stand the test of time,

is, first, his eight volumes of *Inscriptions du Cambodge*, of which the first seven volumes are texts and French translations and the eighth volume indexes to the 1005 inscriptions which had been discovered and classified by 1966.<sup>2</sup> \*] Then there are his articles, mostly in *BEFEO*, but occasionally elsewhere, which are either publications of inscriptions not included in *Inscriptions du Cambodge*, or discussions of particular points of the historical record.

[\*Unfortunately, too little of Coedès' real scientific contribution is included in his books, as though he had no respect for the subject of history as the science of the analysis of the records of man's past, and considered it as no more than what Paul Veyne proposed some 15 years later than the last edition of *Les états*, "a novel that is true", more or less, I would say.<sup>3</sup> Five of the volumes of texts of *Inscriptions du Cambodge* (3-7) were published after the first edition (1948) of *Les états*, the text of which was little changed from that of its precursor, *Histoire ancienne* published in 1944, or even for the second edition in 1964; and in the sections on Cambodia in *Les états*, there is no direct reference to any Khmer inscription except one which lists a royal genealogy not found elsewhere. Since volumes 3 to 6 of *Inscriptions* were produced between 1951 and 1954, there was ample time for them to influence a rewriting of *Les états*, but Coedès chose to ignore his own new research.\*]

The problem was in assumptions and presuppositions, not in any lack of sources or literal comprehension of sources. The defects in Coedès' historical syntheses, the basis for most subsequent work, including Soviet studies, were of course not because he was unaware of the content of the inscriptions, but because of the theoretical framework, possibly unconscious, which he imposed on them. This was a view in which history was genealogical, narrowly political, and narrative, and it would not be sufficient, in fact it would probably be impossible, to extract the additional information from the inscriptions in a coherent manner without a new theoretical framework.

What is to be found in Coedès' books then is a structure of Southeast Asian history with which he had been satisfied in 1944, and which he was unwilling to seriously revise, even with his own later work on new inscriptions, either in 1948, or when the second French edition, basis for the English translation of 1968, was published in 1964.

Let us take some specific examples as illustrations, ignoring entirely the chapter on Prehistory, which has been made quite obsolescent by new research.

[\*First is the question of Hindus and 'Hinduization'. Coedès firmly believed that all development in culture and political complexity was the result of the arrival of Hindus from India. This belief is not argued in his books, but is

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<sup>2</sup>. Since then Claude Jacques has published a supplement up to inscription number K.1050, and, according to personal communication, has registered inscriptions up to number K.1209. Still other inscriptions are being found in Cambodia. See Claude Jacques, "Supplément au tome VIII des inscriptions du Cambodge", *BEFEO* 58 (1971), pp. 177-195.

<sup>3</sup>. Veyne 1978, pp. 9-10.

simply there, and was so strong that it prevented Coedès from seeing contrary evidence which he himself was recording.\*]

In his introduction, which hardly changed from one edition to another he referred to “Hindu navigators”, and to “the navigators, merchants and emigrants, founders of the first Hindu establishments” before the arrival of priests and scholars, and that these “navigators from India” had perhaps been visiting Southeast Asian Neolithic sites from “time immemorial”.<sup>4</sup>

Along with this he apparently did not notice that he had written, following Przulski, that “during the second European Bronze Age Indochina entered the orbit of a maritime civilization including the Southeast of Asia and Indonesia”, “cleverness in navigation” was one of the features of the Austroasiatic cultural complex, and this maritime civilization was carried as far as Madagascar by the Indonesians. It should have been obvious that with Southeast Asian sailors who could travel to Madagascar, it did not require Indians to bring interesting things from India.<sup>5</sup>

Of course, there was then a general belief that the Chinese and Indian ships were highly developed, with “junks for the high seas capable of carrying 600 to 700 passengers” and constructed with a Persian Gulf technique. Coedès was not at fault for accepting conventional wisdom in a technical area which was not his. Now, however, it is known that in the early centuries A.D. neither the Indians nor the Chinese fleets were well developed, that the ships on which Chinese traveled to Southeast Asia beyond Vietnam were Southeast Asian ships, and the enormous ones to which Coedès referred were built much later than the time of putative Hinduization.

For some of this detail Coedès relied on Pelliot, who on this point showed the old orientalist and Indologist prejudice that backward Southeast Asia had to receive all advances in civilization from China, India, or farther west. Writing of the large Funanese sailing ships (junks), able to carry several hundred passengers, described by the early Chinese writers, Pelliot said they were “Persian, Indian, Chinese, but, in the T’ang period [618-906], their crews were above all Malay”. This last remark was made necessary by the Chinese writers’ insistence that the ships were *k’un-lun*/\*k’uən-luən, accepted by all as an area or ethnicity in Southeast Asia; but, of course, in Pelliot’s time it could not be accepted that Malays or other Southeast Asians were great ship builders or navigators. The techniques of construction were well described--several layers of thin planks bound with cords made of coconut husk and caulked with tree resin, a perfectly credible Southeast Asian technique, which Pelliot, however, attributed to the Persian Gulf, although referring to a source for that area dating to the 12th century.<sup>6</sup>

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<sup>4</sup>. Coedès 1944, pp. I, 17, 18.

<sup>5</sup>. Coedès 1944, pp. 9-10.

<sup>6</sup>. Pelliot 1925, pp. 257-261, 262 (K’ouen-louen). Words marked with an asterisk are ancient Chinese pronunciation as restored by Karlgren 1957 and 1974. For the English and ancient transcriptions here see

Subsequently a maritime history specialist confirmed that the very early large *k'un-lun* junks were Southeast Asian, and that they were “enormous ships sewed together like those described **later** [emphasis added] in the Indian Ocean; and that the “existence of [Chinese] junks for the high seas is thus not probable until the 9th century, nor certain until the 12th century”.<sup>7</sup> This information from a technical specialist was not available in the 1940s, but Coedès should have integrated it into his 1964 edition.

It is now a commonplace of Southeast Asian history that the great navigators of the time were neither Indian nor Chinese, but Southeast Asian, in particular the peoples speaking Austronesian languages. Their spread from the interior of Taiwan, through the Philippines and Indonesia, into the Polynesian islands of the Pacific, and as far as Madagascar, as well as the Chamic languages of the mainland, prove that they possessed great boat-building and navigation skills in prehistoric times. In fact the Chinese texts of the Funan period are unanimous in saying that when traveling beyond the coast of what is now northern and central Vietnam, they had to take Southeast Asian ships.<sup>8</sup>

On the subject of high culture, Coedès listed the Indian contributions: Sanskrit, the alphabet, “the influence of law and the administrative organization of India”, as well as the “ancient monuments [which] are connected by their architecture and their sculpture to the arts of India”. [\*Sanskrit and the alphabet, yes, but one wonders which kingdom or cultural area of India Coedès considered the model for law and administrative organization. I believe no study has been conducted on this subject, and with respect to Cambodia, at least, research on its ancient administrative organization has hardly begun, and to the extent it has been conducted it seems to show that very little derived from India\*]. As for architecture, in another context Coedès wrote, citing Parmentier, “the monuments of Farther India [that is, Southeast Asia] are so differentiated and so far from their Indian prototypes that it has been written [by Parmentier]”, “the relationship between the first of these edifices and those of India... is not striking. Without their images and their inscriptions no one would think at first sight to relate them to Hindu temples”.<sup>9</sup>

[\*In general, an opinion of Stutterheim which Coedès cited now seems much more accurate than the Hinduism of Coedès, “the whole of the Hindu culture in Indonesia was acquired from books and manuals, the Hindus themselves playing a very insignificant role, or none at all.”. Coedès did not agree with that, but at least he acknowledged that all of the sources for the process are to be found in Southeast Asia, and they “show us the result, but very rarely the chain of events which produced it”. As I would put it, Southeast Asian navigators were traveling to India far back in prehistoric times, and it was

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Wheatley 1983, pp. 130, 267.

<sup>7</sup>. Paris 1952-54, pp. 276, 277-78 (quotations).

<sup>8</sup>. Wang Gungwu 1958; Paris 1952-54; Wolters 1967; Vickery 1998, pp. 51-60; and see below.

<sup>9</sup>. Coedès 1944, pp. II, 37-38.

they who brought back ideas and models from India. This explains the great differences between the assumed models and the results, and the differences between the ‘Hindu’ cultures of the different parts of Southeast Asia.\*]<sup>10</sup>

As Pierre-Yves Manguin put it in his contribution to the conference on “George Coedès Aujourd’hui” [Manguin 1999], “Nothing could be more mistaken [than Coedès’ views about the backwardness of Southeast Asia before the arrival of the ‘Hindus’]. The archaeological research of the last 30 years has proved that the ‘Indianization’ of Southeast Asia... happened after a millennium of steady exchanges with India, in which certain populations of Southeast Asia, who were beginning to organize themselves within political systems of increasing complexity, played a decisive role, particularly in the setting up of seafaring merchant networks exporting gold and tin”.<sup>11</sup>

### **The Peoples of Southeast Asia**

Coedès, along with everyone else in the 1940s believed that the ethnic groups of Southeast Asia moved overland from North to South in successive waves, with some pockets of early waves left behind among peoples of successive waves. Thus, first came the Australoids, found now in New Guinea and Australia, but only in small pockets on the mainland (the so-called ‘Negritos’), Then came the Indonesians or Austronesians, and in two waves, the first who are now found in the interiors of the large islands, and the second, represented by the coastal Malays. The Cham on the mainland were accounted for as a remnant of the overland Indonesian spread, who were not completely submerged by the next wave--the Mon-Khmer or Austroasiatics. The Thai were the last, coming down from Yunnan, and in some treatments specifically from Nan Chao when it was conquered by the Mongols in the 13th century.

If this picture represented conventional scholarly wisdom in 1944, and perhaps even in 1948, it no longer did in 1964, and certainly not in 1968 when the English translation appeared. Those publications required at least a detailed note or appendix discussing, and if Coedès disagreed criticizing, the new linguistic work showing that the Austronesians had spread by sea, with the Cham moving to the mainland from one of the Indonesian Islands. Coedès did finally take notice that Nan Chao had not been Thai, but he still preserved, awkwardly, Yunnan as the homeland from which the Thai had moved southward.<sup>12</sup> On that point his work is obsolete, but not his fault, for the new

<sup>10</sup>. See Vickery 1998, pp. 51-60 on ‘Indianization’.

<sup>11</sup>. Manguin 1999, translated from the French,

<sup>12</sup>. Coedès 1944, p. 245, Nan Chao was founded by the Thai; Coedès 1964a, the Thais were established in Yunnan where it was long believed that they had founded Nan Chao. Similarly, Wyatt 1984, pp. 39-41, forced to take cognizance of new linguistic work, admitted that Nan Chao had not been a Thai state, but still insisted on standard conventional wisdom to the extent of trying to make the 13th century “A Tai Century”, a conception which first arose under the impact of the Nan Chao theory, according to which the Thai rushed down into Thailand and Burma following the conquest of Nan Chao by the Mongols in 1258. Thus, still, for Wyatt, the most important 13th century development was “the movement of the Tai down from the upland valleys onto the plains formerly dominated by the major empires”, in spite of his recognition in earlier chapters of the gradual

linguistic work showing that the Thai had moved westward and southwestward out of what is now northern Vietnam and adjoining areas of China was not yet published in 1964. As for the Mon-Khmer, their language splits are so ancient that it is impossible to determine when or how they reached their modern habitats, but they were certainly in place in what is now Cambodia, Vietnam, central and peninsular Thailand and Burma long before the Cham appeared on the mainland. Indeed the scattering of Mon-Khmer languages as far as northern Burma, Thailand, and Laos suggests that most of mainland Southeast Asia was once, some thousands of years ago, a solid Austroasiatic bloc.

## Funan

Coedès' treatment of Funan followed Pelliot closely except that he did not share Pelliot's skepticism about certain supposedly direct influences from India. Both Pelliot and Coedès allowed themselves to be influenced by preconceived notions regarding 'Kauṇḍinya', construing both the Chinese-recorded names *hun*(\**yuən*)-*chen* (*zǐēn*) or -*houei* or -*t'ien* (\**d'ien*), and *Chiao Chen-ju* as Sanskrit *kaunḍinya* and, for Coedès, postulating a first and second 'Hinduization'. It should be obvious that both of those Chinese-transcribed terms cannot be restored as *kaunḍinya*, and with the first, the least plausible, there is no sign in the story told by the Funanese to the Chinese envoys of influences from India. Although Pelliot wished to see the name \**yuən-tien*, which is in fact a composite of four different names (in the second syllable) in four different texts, as the Chinese rendering of the first two syllables (/kaun-din/) of the name Kauṇḍin(ya), this interpretation is rendered implausible by the title-name of his descendant, Hun P'an Houang/\**yuən b'uân Xiwang*, of which the second and third syllables do not combine with the first to make any known Indic name or title; and /hun (\**yuən*)/ alone, whether or not phonetically equivalent to /kaun/ in the 3rd century, will not serve as a representation of 'Kauṇḍinya', or anything else. We must conclude that \**yuən* was a prehistoric Austronesian or Mon-Khmer title or clan name, which in the Funan origin myth was the designation of their first, perhaps legendary, rulers. Indeed, if one reads the Chinese reports carefully, via Pelliot, Funan was not in direct contact with India at all until several generations later, early in the 3rd century, and Coedès' construal of a 'First Hinduization' falls apart, as does much of the speculation about earliest contacts with India noted above.<sup>13</sup>

In the second case, restoration of the Chinese-recorded name as *kaunḍinya* is plausible, but it is not permissible to construe a 'Second Hinduization', nor the arrival of an individual Kauṇḍinya from India, for inscription K.5 shows that the local royalty, after several generations of contact, had simply adopted

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movement of the Thai into those areas over several previous centuries.

<sup>13</sup>. For more detailed discussion of these problems in Funan history see my, "Funan Reviewed: deconstructing the Ancients", BEFEO, 90-91, 2003-2004, pp. 101-143.

‘Kauṇḍinya’ as some kind of dynastic name, just as they adopted *-varma(n)* to indicate royal status. In that inscription, which Coedès dated on palaeographic grounds to the 5th century, Guṇavarman is called son of *kaunḍinyavaṃśaśāsinā*, ‘moon of the lineage of Kauṇḍinya’, and later a king Jayavarman called himself Kauṇḍinya Jayavarman.

What is really seen in these two stories are two different versions of the Funanese origin myth, as reported by the Chinese. The first, told to the Chinese just when Funan was initiating contact with India, shows no relation to India at all, and like all origin myths harks back to a past about which there is no accurate information, while the second, recorded in the 5th century, shows that the Funanese had updated their origin myth to incorporate details they had learned from contact with India since the early 3rd century. The old orientalist could not see this because of a fixed idea that all cultural development had to originate with persons coming from India.

In this connection we should note an example of oversynthesization by Coedès. He tried to fit three Funan-period inscriptions into the Chinese story that Rudravarman had succeeded Kauṇḍinya Jayavarman through assassinating a younger brother who was a more legitimate successor because his mother was queen, whereas Rudravarman was son of a concubine. Coedès indulged here in some rather risky reconstruction, supplying names to the Chinese story, and saying that the murdered younger brother was Guṇavarman, named in K.5, and his mother the queen Kulaprabhāvatī mentioned in the third Funan-period inscription, K.875. Inscriptions K.5 and K.875, however, are too fragmentary to inspire confidence in this reconstruction, and their different scripts (K.5 is earlier than K.875) in fact forced Coedès into special pleading. He recognized that the script of K.5 is earlier than that of K.875, and thus one should conclude that Guṇavarman, at least, was not connected with the Chinese story. Moreover, there is no evidence from Cambodian history that rank of mothers played a role in choice of heir to the throne.<sup>14</sup>

Coedès was again less perspicacious than Pelliot in emphasizing another ‘Indian’ intervention in the person of a certain Chan-t’an, or Tien Chu Chan-t’an who had supposedly arrived from India and taken power in Funan. As the Chinese recorded the situation, in the year 357 “Tien Chu Chan-t’an of Funan offered tame elephants [to China]”, and in another version, “the king, Chu Chan-t’an... offered tame elephants”.<sup>15</sup>

This item was the occasion for much freewheeling speculation among the Indologists of Pelliot’s time, and he performed well in putting a damper on their diffusionist enthusiasm. Coedès provided a summary of those speculations, according to which “in 357 ...Funan fell under the domination of a foreigner”

<sup>14</sup>. Coedès 1964a, p. 117, note 4; 1968, p. 286, note 105.

<sup>15</sup>. Pelliot 1903, pp. 252-253, and 269. I have substituted English romanization for the French style used by Pelliot.

named Chan-t'an from India (T'ien Chu), and moreover, "his title Chan-t'an seems to connect him to the same stock as Kanishka", of the Indo-Scythians, or the Kushans. "We also know that the Kushans extended their domination over the Ganges, at least as far as Benares", but "in 357... all of northern India had submitted to the Gupta dynasty; the Scythian invaders had been driven back. It is possible that a branch of the Kushan family... sought its fortunes beyond the Bay of Bengal", that is, in Funan.<sup>16</sup> This is another example of chevaleresque historical romancing--the idea that some kind of royal aura enabled a few aristocrats to move and be accepted as chiefs in a new society where they had no roots or traditionally loyal retainers. It also reflects the outdated view that Southeast Asia was utterly primitive until Indians arrived.

First, Pelliot said that the literal translation of the passage in question should be "Chan-t'an of T'ien Chu of Funan"; but "what could such an appellation mean?"

Second, Pelliot noted that the other Chinese documents recording this event call him only "Chu Chan-t'an", which, according to Chinese custom in surnames or clan names for persons of ultimate foreign origin, would only mean Chan-t'an was of a family which had originally come from India. Moreover, he was not a unique example in the Chinese records of Funan and Southeast Asia. In 517 there was a Funanese ambassador named Chu Tang-pao-lao; and earlier there had been ambassadors from another "Indochinese king of P'o-houang", Chu Na-p'o-tche in 456, and Chu Siu-lo-ta in 466. In fact the first passage which mentions Chan-t'an does not even call him king, but only suggests that he was an envoy, while the second passage says he was "taking the title of king", about which the Chinese seem to have had some doubts, for they rejected his gifts. The third, however, compiled a century later, and thus inherently less accurate, called him 'king', but still noted that his mission had been rejected.<sup>17</sup>

Still another person with the same surname, Chu, was Chu Tche, from China, who had traveled in the southern seas, and wrote a book with a section on Funan. Pelliot said that not much was known about him, except that "he was of Hindu origin, as attested by his surname Chu".<sup>18</sup>

It would seem that, as in later times in maritime Southeast Asia, persons of ultimate foreign family origin were often used in diplomatic and commercial services, no doubt for their language competence, and perhaps because, as foreigners, outside powerful local family or clan networks, they were thought to be more loyal to the central authorities. Thus all the early speculations about the 'Hindu' Chan-t'an representing a new type of Indianization should be put aside, even if he was ultimately a chief of Funan.

<sup>16</sup>. Coedès 1968a, p. 46; 1964, p. 92.

<sup>17</sup>. For Pelliot's full explanation see Pelliot 1903, p. 253-3, note 4. The other two passages concerning Chan-t'an are on pages 255 and 269. I do not have sources to determine the English or ancient versions of the other Chinese names cited here.

<sup>18</sup>. Pelliot 1903, p. 277. The name of the second of the first two Chinese envoys to Funan, Chu Ying, was written with a different character, and thus had no connection with India. See Pelliot 1903, p. 275.

All of the early scholars took an interest in the ‘capital’ of Funan, and there has long been a consensus that its Sanskrit name was Vyādhapura. The Chinese mentioned a name, T’ e-mu/T’ö-mou/\*d’æk-miuk, only once, in one of their last reports on Funan in the 7th century. Thus both the location, wherever it was, for the Chinese reports do not say, and the name, may have been different in earlier centuries. Indeed, and this is why I have enclosed the word in inverted commas, there may have been no continuous ‘capital of Funan’. Funan, like Śrīvijaya, may have been a maritime trading society in which different ports, or inland centers linked to ports, alternated as power centers. It is plausible, however, to accept that the center or centers which the Chinese knew were in southern Cambodia and Vietnam, not far into central Thailand as a few writers have suggested.<sup>19</sup>

In the view of Aymonier in 1904 the physical remains at Angkor Borei suggested it as the most important center in Funan times, and on this point Pelliot followed Aymonier. As for the name ‘Vyādhapura’, Aymonier was less certain. He thought it was Angkor Borei because “an ancient Khmer inscription” in Battambang “speaks of Vrai Krapās Vyādhapura, thus giving us a certain identification for the ancient Vyādhapura, the capital of Cambodia in the 6th century śaka [578-678 AD].” That is, Vrai/Prei Krapās is still the name of the district in which Angkor Borei is located. In another context Aymonier said that the same information proved that Vyādhapura, “city of hunters” was the capital of Bhavavarman [I], but “was this city also the capital, possibly

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<sup>19</sup>. For example, Boisselier 1965, who suggested Uthong; and Hoshino 1993, where Funan is localized near Sri Thep in the upper Pasak valley. Very recently a writer of superficial popularizing articles in the Bangkok English-language press, Michael Wright (Wright 1999a), took up Hoshino’s view of Funan and added that at the time Sri Thep “was near the head of the Gulf of Siam in ancient times”, thus adding plausibility to Hoshino’s theories which ignored the clear Chinese statements that Funan was a coastal area. All writers on Southeast Asian geography, however, agree that the sea level has not changed significantly in the last 5000 years, and that in Funan times the location of Sri Thep would have been just as far from the coast as it is now. See Higham and Thosarat 1998, especially maps pp. 66 and 134. After seeing this criticism of his proposal in my paper presented to the Coedès colloquium Wright answered that silting could have accounted for the filling of the Menam Basin, evidently between the 3rd century and the 14th century founding of Ayutthaya (Wright 1999b). A similar, but less radical, proposal has recently appeared in (*Art And Culture Magazine*), 21/3, January 2543 [2000], pp. 26-45, map p. 31, showing the Tha Chin and Chao Phraya basins under water until Dvāravatī times as far north as Supanburi and beyond Ayutthaya, with Lophburi, Nakhon Nayok, Uthong, Nakhon Pathom, and Ratchaburi on the coast [Sujit Wongthes] and [Srisak Vallibhotama], [‘Suvannaphum is here’]. Such pseudoscientific geography has a long history in Thailand. In *Journal of the Siam Society* 52/1, April 1964, pp. 7-20 (with maps preceding), Larry Sternstein, “An Historical Atlas of Thailand”, commenting on Map 1, which shows the Menam Chao Phraya delta in the 8th century about equidistant between Lopburi and Sukhothai [perhaps at Nakhon Sawan?], whereas five hundred years later it was at Bangkok, said, page 11, “The north-south difference between these two positions, approximately 220 kilometers, requires the average annual increment to have been about 400 meters, a rate rarely approximated by known deltaic growth during any phenomenal year, and never even remotely approached on the average. In fact, it is ten times as rapid as the present rapid rate which has resulted in an advance of almost thirty kilometers (in a straight line) during the past seven hundred years.” As an extreme case Higham 1989, p. 6 refers to the present extension of the Mekong delta of “about 80 m per annum as recent deforestation has led to increased soil erosion”; while Higham and Thosarat 1998, p. 14, describe silting in the Chao Phraya delta, which today sees “the delta advance by up to six meters every year”; and “proceed back 5,000 years, and the land behind Bangkok was a shallow extension of the Gulf of Siam all the way up to Ayutthaya”, that is, the situation which the writers above proposed for the Dvāravatī period.

under other names, of his three predecessors?” (including Jayavarman and Rudravarman of Funan). The title *adhirāja* of Vyādhapura, given in Angkor royal genealogies as ancestors of the first Angkor kings, “may indicate only the 7th-century kings, which, we know, were established in this city, and not their predecessors of the 6th century”; and Aymonier considered that Īśānapura, city of Īśānavarman (±616-637), was just a new name for Vyādhapura, Angkor Borei.<sup>20</sup> On the last two points we now realize that Aymonier was mistaken, and Coedès’ treatment superior, for the location of Īśānapura is firmly established at Sambor Prei Kuk in Northern Kompong Thom; and it is believed that the residence of his predecessors, Bhavavarman I and Citrasena Mahendravarman, was also in the north.

Coedès imposed a change on the consensus about the capital of Funan, both with respect to its location, and, in a way, its name. The Chinese records do not offer a name for the capital of Funan until the very end, when they said it was called T’e-mu/T’ö-mou/\**d’ək-miuk*, but the distance from the sea given by the Chinese sources, if understood literally, does not fit either Angkor Borei or Ba Phnom, proposed by Coedès.

Coedès concluded that with \**d’ək-miuk* the Chinese were transcribing a Khmer word *dmāk* or *dalmak/dalmāk*, ‘hunter’, equivalent to modern Khmer /tromeak/ ‘mahout’, which would have been the Khmer translation of *vyādha* in the Sanskrit name *vyādhapura*, a name found several times in Angkor-period inscriptions as the name of one of three old kingdoms from which the first Angkor kings claimed descent, and it was assumed by Coedès to mean Funan. There are three weaknesses in this argument.<sup>21</sup>

First, if the Khmer terms *dmāk*~ *dalmak/dalmāk* really mean ‘hunter’, it is in the sense of ‘lassoer’ or ‘trapper’, to capture alive, as an elephant, while Sanskrit *vyādha* means to pierce with a sharp instrument. Second, there is no evidence for the term *dalmak/dalmāk* until the end of the 9th century, and in the 10th-century inscription to which Coedès referred (K.158), there is no indication of the occupation of the single *dalmak* who is named; and, third, where *dalmak* occurs many times, in the unpublished inscriptions of Roluos, it includes large groups of people, men, women, and children, and it is also there impossible to identify their functions. At least they do not clearly have any connection with elephant work (see further below).<sup>22</sup>

The ancient phonetics of the term, \**d’ək-miuk*, suggest that the first syllable was the Mon-Khmer word for ‘water’, /dik, dek, dak/ (Mod. Khmer

<sup>20</sup>. Aymonier I, pp. 197-98; Aymonier III, pp. 415, 430 respectively.

<sup>21</sup> Coedès 1942, p. 110, n. 5.

<sup>22</sup>. Vickery 1998, “Appendix”, ‘*dalmak/dalmāk*’; Vickery 1999. Just as a heuristic suggestion, we might propose that the *dalmak* were a hereditary occupational group of elephant specialists. This would account for the women and children among them, and for the appearance of *dalmak* in contexts unrelated to elephant work.

/tək/), a term which is often found in Cambodian place names. It is difficult, however, to propose a convincing interpretation of \**miuk* in a name together with ‘water’, and further speculation about what \**d'ək-miuk* meant is premature. Nevertheless, it should definitely be dissociated from *vyādhapura*.

As for the location of Vyādhapura, Coedès used some of the Angkor-period references to demonstrate that it had been located at Ba Phnom, some 60 km northeast of Angkor Borei. He ignored, however, several of the non-royal Angkor-period references to Vyādhapura, which indicate only that it was some place in the South or Southeast, and that it was important in connection with Jayavarman II, not Funan. A serious lapse by Coedès was his total neglect of the only pre-Angkor record of Vyādhapura, inscription K.109 dated A.D. 655, located near the ancient walled site of Banteay Prei Nokor in the Thbaung Khmum district of Kompong Cham Province, far to the east of either Ba Phnom or Angkor Borei. In that inscription Vyādhapura is seen as a provincial center under its own chief entitled in Khmer *kurāk kloñ* Vyādhapura, enhanced in the Sanskrit part of the inscription to *vyādhapuresvara*, literally ‘king of Vyādhapura’. The full meaning of the pre-Angkor title *kurāk* has not been determined, but it appears to have been only a second-level chief, lower than the ones called *vrah kamratāñ añ..... varma(n)*. Thus Vyādhapura was most probably neither Ba Phnom nor Angkor Borei, nor a capital of Funan. In an earlier study I have suggested that the importance of Vyādhapura in the Angkor inscriptions was because it was the home district of Jayavarman II. The 10th-century uses of Vyādhapura suggest that it had by then become a large province encompassing both Angkor Borei and Ba Phnom, as well as, probably, its original location. The final choice for historians between Ba Phnom and Angkor Borei, or perhaps, eventually, some other place, as the major center of Funan will have to be based on archaeology, and at present the remains at Angkor Borei give it precedence. We may, however, never be able to ascertain the ancient name of the site, nor what site the Chinese meant by \**d'ək-miuk*. It is quite possible that during its existence as known to the Chinese the major power center of Funan changed, even more than once, as has been hypothesized for another Southeast Asian maritime area, that known as Śrīvijaya, and now generally accepted by specialists.<sup>23</sup>

When Pelliot discussed the location of the capital he measured the 500 *li* or 200 kilometers from the mouth of the Mekong and found that it reached a region between Chaudoc and Phnom Penh, that is, approximately at Angkor Borei, equally close, however to Ba Phnom.<sup>24</sup> Pelliot, however, was writing before the remains of Oc-Eo and the canals running from there toward Angkor

<sup>23</sup> . See Vickery 1998, pp. 28-29, 36-37, 395-98.

<sup>24</sup> . Pelliot 1903, p. 290. The distance of 500 *li* from the sea was recorded in the *History of the Leang* (502-556), compiled in the 7th century (Pelliot, *ibid.*, pp. 262-63).

Borei had been discovered, and he supposed that the only water routes would have been up the Mekong or Bassac river. Only in that way can a distance of 200 kilometers, following all the bends in the rivers, be confined, more or less, between the river mouths and Angkor Borei. Even then the distance seems too long, and Pelliot showed some embarrassment in only alluding to “a region” “between Chaudoc and Phnom Penh”. If, as now seems more apt, a canal route from the coast near Oc-Eo is assumed, 200 km goes far beyond Angkor Borei, itself only 90 km from the coast, and we must either assume that the Chinese estimate was in error, or that they were referring to some entirely different place. In a straight line 200 km from Oc-Eo would nearly reach Kompong Cham.

One final remark on the Funan period concerns Coedès' acceptance of a totally speculative suggestion by Louis Finot that the kings of Funan were called *kuruh bnam*, ‘king of the mountain(s)’. This title has not been found in any Cambodian document from any period, as Coedès, who knew the inscriptions better than anyone, should have been aware. But accepting it permitted linking Funan and other areas in the type of oversynthesization which Coedès enjoyed (see below). During the 70 years which elapsed before this canard was put aside, it led many others astray.<sup>25</sup>

### **Pre-Angkor Society and kingship**

[\*There is no comment in Coedès' work about Pre-Angkor society beyond a listing of the kings who could be discerned in the Sanskrit inscriptions, and this is one of the least adequate parts in the three editions of his history, and in his *oeuvre* as a whole. Even in 1944 this may not be explained by lack of study of the relevant Khmer inscriptions, for some of the most important 7th-century Khmer inscriptions are in the first two volumes of *Inscriptions du Cambodge* which Coedès had already prepared. It shows that Coedès was just not interested in social history or political institutions even when he was familiar with relevant sources.

In principle, Coedès acknowledged the importance of Khmer inscriptions, but he neglected them. Even when Coedès cited inscriptions with indications of the divisions of Cambodian society, in particular chiefly groups below the kings, he apparently dismissed them as uninteresting.\*]

One of the most striking examples is in his publication of a 10th-century inscription which is of the greatest interest for its details on land possession, inheritance, and kinship relations among the Cambodian elite, including between the aristocracy and kings, Coedès felt constrained to remark that “[t]he very subject of the inscription is not of great interest; it essentially treats the history of various pieces of land obtained by this family through the liberality of

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<sup>25</sup>. Finot 1911, p. 203; Coedès 1964a, p. 74; and for the correction see Jacques 1979, p. 375.

successive Kings....[t]he only passage offering something of historical interest contains an allusion to a ceremony carried out by Jayavarman II in order to liberate Cambodia from the control of Java".<sup>26</sup>

In his introductions (Coedès 1944, p. VI; 1964, p. 8) Coedès wrote that [\* "much still remains to do for ... political institutions and the material civilization. For these matters epigraphy will be able to furnish much material when the interpretation of the vernacular inscriptions, which is not always easy and which attracts only a small number of researchers, is more advanced".\*] Seemingly Coedès considered that not much had been accomplished in this area during the 20 years from 1944 to 1964. And, as is seen in this context, for study of the 'material civilization' Coedès was clearly more comfortable with the Chinese accounts of early Cambodia, which we now realize may have combined material from incompatible sources.

As Paul Wheatley has written, "it is, unfortunately, not possible completely to isolate the several strata of information relating to Fu-nan that have been fused together in the Chinese records"; "it is difficult to be sure of the provenance of any particular item of information, for the Chinese annalists... were apt to incorporate... any apposite material conveniently to hand, however dated it might be"; and "an extreme instance of this practice is in... [a work of] 1609 but still preserving unchanged material from ...the 3rd century A.D. [where] to the paragraph on Tun-sun ... [the author] added a picture of an inhabitant dressed in a manner more suitable for Central than for Southeast Asia".<sup>27</sup>

[\*Further on, in Coedès 1944, p. 89, repeated in Coedès 1964, p. 139, we read, "the inscriptions in Khmer... are the principal source of information on the history and the institutions of the country. They reveal a solidly organized administration and an entire hierarchy of officials whose titles are better understood than their functions".\*] Coedès added, however, that "it is above all the religious life which is revealed", following this with some of the Sanskrit religious terms which are found, such as names of Hindu sects, and ignoring the panoply of Khmer administrative titles.

[\*Although these remarks by Coedès are certainly pertinent in themselves, they show both that he was little interested in what the Khmer inscriptions might reveal about 'political institutions' and 'material civilization'. They also show an inexcusable negligence, even suppression, of the work of a predecessor who, 40 years before Coedès 1944, when Coedès was still a student innocent of Khmer studies, had already laid the groundwork and produced a very useful sketch of 'political institutions' based on close reading of Khmer inscriptions.

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<sup>26</sup> Coedès 1964c, p. 129, "Dalle de Vat Samroñ", K000

<sup>27</sup> Wheatley 1983, p. 120, p. 153, n. 12.

This was Etienne Aymonier, perhaps the first of the old French administrator-scholars to learn Khmer and Khmer epigraphy well. Aymonier's work was unjustly neglected by the Sanskritists and Indologists who later took over Cambodia studies.\*] This may have been in the first place because in his chapters of historical synthesis Aymonier stepped out of his area of knowledge, Khmer, to offer suggestions for interpretation of Sanskrit and Chinese texts which were erroneous and attracted the mockery of Coedès and Pelliot. Secondly, the syntheses later produced by Coedès from Sanskrit inscriptions were usually superior to this aspect of Aymonier's work, as one would expect after 20-30 years progress in the field, but [\*Coedès totally ignored the advances made by Aymonier in interpretation of Khmer texts, and which, no doubt because of Coedès, were then ignored by the *epigone* until very recently\*].

In this connection I must plead guilty myself, having, under the influence of Coedès, given insufficient attention to Aymonier until last year when, preparing an article on the Roluos inscriptions, I was forced to turn to Aymonier as the only person who had studied them at all. I then discovered that [\*Aymonier had by 1904 anticipated some of what I thought were my own discoveries of 7th-8th-century 'political institutions'. I wish to take this occasion to apologize to Aymonier's memory.<sup>28\*</sup>]

In Aymonier III, pp. 446-7, we find correct descriptions of the important chiefly titles *poñ* and *mratañ*, and others, never studied by Coedès.<sup>29</sup> Aymonier also correctly understood the Mon-Khmer term *vrah* against Sanskritists of his time,\*] and others who have since then proposed fanciful explanations, not fully laid to rest until the work of Pierre Dupont and the Khmer linguist Saveros Pou [Lewitz].<sup>30</sup>

I also ignored other predecessors with respect to the 7th-century titles, but through excusable ignorance of the existence of their work. They were the group of Soviet scholars who produced a general history of Cambodia in 1981, which I only discovered this year, and in which the 7th-century titles are given full attention, and moreover because of their own research, not relying on Aymonier or other French scholars. Because of their Marxism the Soviets were able to develop productive theories for studying ancient 'political institutions' and to ask the right questions, even if their answers, in my view, are not always the best. That is, again because of their Marxism, they saw ancient Cambodia as

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<sup>28</sup>. Vickery 1999. I also discovered Pou 1996 at that time.

<sup>29</sup>. Vickery 1986 and 1998, chapter 6, on those titles. Sahai 1970, p. 56, continued Coedès' neglect, saying only that "the *poñ* and the *vāp* did not hold high-level positions", and in his chapter on "L'Administration territoriale" he did not mention them at all. See Vickery 1998, p. 191, note 59

<sup>30</sup>. Aymonier I, p. 49; III, pp. 446-47; Dupont 1952-54, pp. 152-53; Lewitz 1967, p. 424; Vickery 1998, pp. 450-51.

feudal, a private property society, and they analyzed the *mratañ* and *poñ* as, respectively greater and lesser landowners. In my view land was not private property in ancient Cambodia, and the positions of *mratañ*, *poñ*, and other types of functionaries were not related to ownership or possession of land.<sup>31</sup>

Surprisingly Aymonier did not notice the title *kpoñ*, probably denoting pre-Indic local goddesses in 7th-century Cambodia, but Coedès, although well aware of it because of his publications of inscriptions, also neglected its importance.<sup>32</sup>

In one context in which Coedès did integrate new information into his text he probably misjudged it, leading to confusion among his successors. This concerns the end of the reign of the late 7th-century king Jayavarman I. From 1944 to 1964 Coedès had written that “the reign of Jayavarman I, seemingly peaceful, lasted around 30 years [counted from 657, or a bit earlier] and ended after 681”, adding in 1948 and 1964, “it is perhaps him whom an inscription of 713 calls ‘the king who went to Śivapura’”. In fact there are five inscriptions referring to a king at that time with a posthumous name relating to Śivapura.<sup>33</sup>

Then, however, in Coedès 1968a, the English translation of Coedès 1964a, it was said that Coedès had revised his opinion about the death of Jayavarman I to place it “after 690”. The statement of the earlier editions suddenly changed to “the reign of Jayavarman I, seemingly peaceful, lasted around 30 years [counted from 657, or a bit earlier] and ended after 690. It is perhaps he whom an inscription of 713 calls ‘the king who went to Śivapura’”. This change was explained only as “Ed. note: Date corrected by Coedès; third edition had 681”. No attempt was made to revise the awkward arithmetic of a reign lasting 30 years from the 650s and ending after 690; and there was no advice to the reader concerning the reason for the change. This should mean that all of the earlier hypotheses, along with Coedès' discussion of the relationship of Jayavarman I and Jayadevī, are either obsolete or meaningless. Extending the reign of Jayavarman I to 690 or later raises the problem of identifying the King who went to Śivapura. Moreover, if the rest of Coedès' discussion is accepted, and only the crucial date changed from 680-1 to 690, or later, other difficulties appear, for there would still be a Jayavarman-Śivapura whose death by 680-1 is explicit or implied in at least three texts, and he was the father of Jayadevī. We

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<sup>31</sup>. Mikheev, et. al. 1981. One predecessor, with respect to *poñ*, whom I was able to acknowledge, was Jit Phumisak 1951. See Vickery 1998, p. 191, note 59.

<sup>32</sup>. In what seems to be his first remark on *kpoñ*, in the expanded title *kpoñ kamratāñ añ* in the earliest dated Khmer inscription, K.600/A.D. 611, Coedès said merely that “the first list of servants was offered to the god *kpoñ kamratāñ añ*” (Coedès 1942, pp. 21-23). So far as I have been able to determine, he never went beyond this. Vickery 1998, chapter 5.

<sup>33</sup>. Coedès 1944, p. 48; Coedès 1964a, p. 138; Vickery 1998, pp. 356-364.

have here in this conventional treatment a real example of scholastic involution, fitting ‘Jayavarman I lived until after 690’ into an unmodified context constructed around ‘Jayavarman I died in 680-1’.

The revised date, “after 690”, which was inserted at the last moment into the English translation of Coedès' work, comes from a hitherto neglected inscription, K.1004, from a location very near Angkor Borei, some 60 km south of Phnom Penh. It contains a two-line Sanskrit prologue with the name of King Jayavarman, a date equivalent to 691, and 11 lines of Khmer. Coedès apparently did not doubt that the Jayavarman was Jayavarman I, and Claude Jacques has continued to emphasize this inscription as evidence for extending the reign of Jayavarman I and revising the meaning of Śivapura. It should not be forgotten, however, that the identification of the king of K.1004 with Jayavarman I is an assumption, for nothing in the content of the inscription relates it to any of the known facts of the reign of Jayavarman I or to other inscriptions of his time. On the contrary, K.1004 is if anything overloaded with anomalies. Although Jacques has attested that the Sanskrit part is correct, and the script appears pre-Angkorean, its style and phrasing are different from all other pre-Angkor Sanskrit prologues.

Jacques kindly provided a transcription and translation. The first line of the Sanskrit prologue is "*abde śrīmati dasrakevala rasai rājñās śakānām yadā ...*", which Jacques has translated "[w]hen in the year of grace counted by 612 (the six tastes, unique, the two Aśvin) of the king of the *śakas*"; and it ends with the term '*śāśana[m]*', "promulgate". Nowhere else in Khmer epigraphy does '*abda*' ('year', Monier-Williams 1976:60) alone begin the statement of a date, '*śrīmat*' was never used in the 7th century in the expression of a date, but only before the names of kings or gods, the expression '*rājñās śakānām*' is not found anywhere else in pre-Angkor epigraphy, and '*śāśana[m]*' is not found as used in K.1004 in any pre-Angkor text; but by Angkor times it had become the most usual term for a royal edict, replacing the pre-Angkor *ājñā*.

The Khmer part, which does not mention Jayavarman nor any other named individual, is moreover so peculiar that it has defied all efforts at translation, and in my opinion is untranslatable, being in fact nonsense, the result of a late Angkor-period attempt to recopy and/or rewrite a 7th-century charter. In such case it would be quite unreliable for any detail of 7th-century history. Even if it were not incomprehensible, such striking differences from the pre-Angkor language recorded in the rest of the corpus would suggest a peculiarity requiring full investigation incorporating a strong element of skepticism. Moreover, it is not just vocabulary items which are at issue, but the very structure of the phrasing in which they occur, while the contents, in terms of subject, is not unusual. It concerns a foundation like most other 7th-century foundations. It is a familiar outline with an incomprehensible juxtaposition of phrases and terms.

Nevertheless, even if the Khmer part is to be dismissed as useless, it is in principle possible that the Sanskrit prologue accurately identifies a King Jayavarman who was alive in 691, and that even if K.1004 is in its entirety a later copy, the facts of the Sanskrit part at least were copied accurately, or rewritten by someone who knew Sanskrit but did not comprehend 7th-century Khmer. The plausibility of such an assumption lies in the unchanging nature of Sanskrit, whereas Khmer shows normal linguistic change, and late Angkor scribes would have had no difficulty reading 7th-century Sanskrit, but might have badly misunderstood 7th-century Khmer.

Assuming that there really was a King Jayavarman in 691, it is still necessary to make a decision whether he was the conventional Jayavarman I, or another Jayavarman, a successor to Jayavarman I. Such a decision must depend on the degree to which one choice or another adds to or detracts from the coherency of the other information available, such as the picture of Jayavarman I as the most important ruler of the latter half of the 7th century, the retrospective importance accorded him by his daughters Jayadevī and Śobhājayā, and the evidence on 'the King who went to Śivapura'. Given the long break until the next recorded ruler, Queen Jayadevī in 713, it would not be at all implausible to consider the Jayavarman of K.1004 in 691-2, to have been another successor of Jayavarman I, a son, or nephew, or an unrelated chief trying to assume central power.

The next problematic period, both in Cambodian history and in Coedès' treatment of it, is the difficult eighth century--difficult because there are so few inscriptions, less than 20 against over 200 for the seventh century.

Coedès, from one edition to the next adhered to the story reported by the Chinese, that around 706 Chenla had split into two, 'Land' and 'Water' Chenla, and he added gratuitously that a certain Puṣkarākṣa had taken power in Śambhupura (Sambor in Kratie Province on the Mekong), thereby causing the split. The name Puṣkarākṣa comes from the partly legendary ancestor lists in the royal Angkor genealogies, but there is nothing in those sources about his 'taking power', implicitly from some other ruler or dynasty. He is simply recorded as an ancient king in Śambhupura.

Throughout his three editions the details were mostly the same, with, however, some interesting changes as Coedès re-read some of the inscriptions. In 1944 he wrote that the Chinese recorded the division in 706, and that there was a "King Bālāditya [of Water Chenla] who claimed descent from Kauṇḍinya and the Nāgī Somā", an interesting illustration of Coedès' synthesizing of history. There is no record of Bālāditya 'claiming' anything. He is just a name in a list of ancestors, indeed claimed, by one or another of the early Angkor kings. "At about the same time a prince of Aninditapura named Puṣkara or [emphasis added] Puṣkarākṣa became king in the State of Śambhupura and had

an inscription engraved in 716”. This is more uncritical synthesizing. The inscription of 716 is the reason for “Puṣkara or Puṣkarākṣa”, because it records an act of a person with the former name, but it does not identify him as royalty or with any connection to the King Puṣkarākṣa. Moreover, none of the genealogies say Puṣkarākṣa of Aninditapura became King in Śambhupura. There is one text that makes him descend in the Aninditapura line and another that identifies him as ancestor of royalty in Śambhupura. An interesting detail which Coedès added in 1944, and which will be discussed below, was that perhaps Puṣkarākṣa acquired his position in Śambhupura through marriage, perhaps to Jayadevī, widow of Jayavarman I, and that he may have been the Indraloka, named posthumously in an inscription in Śambhupura. [\*These guesses by Coedes, however were wrong\*]<sup>34</sup>

In 1948 Coedès changed the story slightly, saying “the origin of the secession [split in Chenla] was apparently the anarchy which followed the reign of Jayavarman I who died without a male heir. In 713 the country was governed by a queen, perhaps the widow of Jayavarman I, Jayadevī”. Nevertheless, it was the seizure of Śambhupura by Puṣkarākṣa, who was perhaps Indraloka, which marked the beginning of the secession. His marriage, in this edition, was “a mere hypothesis, and we may just as well imagine a *coup de force* as a result of the vacancy of the throne”. What, or which throne? Obviously Coedès believed that at the death of Jayavarman I all of Cambodia, including Śambhupura, had been firmly united under one throne which became vacant because Jayavarman had no male heir.<sup>35</sup> In fact, we know nothing about the last point, nor even whether Cambodian custom of the time was patrilineal. From 681, or 691 if we accept K.1004, until 713 there is no mention anywhere of the heirs of Jayavarman I, nor of any other royalty.

By 1964 Coedès had discovered that Jayadevī was not the widow, but a daughter, of Jayavarman I, who was perhaps the king with the posthumous name Śivapura, adding “he does not seem to have left an heir for in [713]... there was a Queen Jayadevī”. Now the apparent determining cause of the secession of the eighth century was “the fall” of Jayavarman I, although in another context Coedès repeated what he had written in 1948 about Puṣkarākṣa.<sup>36</sup>

In the latter context Coedès added a new detail, that while the Chinese noted a division of Chenla into two, it in fact “returned to the anarchic state which existed prior to its unification by the last kings of Funan and the first

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<sup>34</sup>. Coedès 1944, pp. 106-08.

<sup>35</sup>. Coedès 1948, p. 149.

<sup>36</sup>. Coedès 1964a, pp. 138, 161-63.

kings of Chenla”, with a footnote reference to new work by Pierre Dupont.

In contrast to his treatment of Vyādhapura where he had read back details from the 10th-11th-century Angkor inscriptions, Coedès had ignored, except for Puṣkarākṣa, what the Angkor royal genealogies said about the eighth century. These details were developed by Dupont, cited by Coedès who, however, ignored the details in a good example of scholastic involution. Dupont analyzed carefully the putative ancestor lists of the royal genealogies and concluded that during the eighth century Cambodia had been divided into several small states each under its own dynasty and that a new unity was not achieved until well after the reign of Jayavarman II. If accepted this would mean a completely new rewriting of the eighth century, different from what appears in Coedès’ work. If Coedès disagreed with Dupont he should have at least discussed the matter, for Dupont’s sources were those which Coedès had accepted to prove that Vyādhapura was the name of the capital of Funan. I also disagree with Dupont, but his work deserves full discussion.<sup>37</sup>

Some credit here should also be given to the neglected Aymonier. For the 8th-century division of Chenla, Aymonier already went beyond Coedès 1944, and anticipated Pierre Dupont in writing “it could even be supposed that there were, not two, but several different states”; and he saw clearly that Sambor/Śambhupura on the Mekong was one of the capitals, “but of which part we cannot say”. Coedès also saw the importance of Śambhupura, but his speculations about it changed from one edition to the next (see further below on Śambhupura). In Coedès 1964a he repeated the passage from Coedès 1944, referring only in a note to the work of Dupont which had totally changed the picture of 8th-century Cambodia, and which had been foreseen by Aymonier.<sup>38</sup> In fact, Aymonier’s discussion of the retrospective Angkor-period royal genealogies purportedly recording 8th-century dynasties is more useful than that of Coedès 1964a, and is no more in error.<sup>39</sup>

### **A Javanese invasion and the 8th century**

Coedès was not consistent in his discussions of the effect of a supposed Indonesian invasion on the internal organization of Cambodia, and this is one of the cases which indicates a disinterest in history as a scientific discipline or field of knowledge. In Coedès 1964a he seemed unconvinced of the importance of any such invasion, or even that it was a historical fact; but in another publication of the same year (Coedès 1964b) he treated it as a main cause of Cambodian decline in the 7th-8th centuries. Even then there was uncertainty about the

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<sup>37</sup>. Vickery 1986; Vickery 1994; Vickery 1998, pp. 321, 324, 391-92, 399.

<sup>38</sup>. Aymonier III, p. 459; Coedès 1944, pp. 107-08; Coedès 1964a, pp. 162-63, with note 1, p. 162 on the work of Dupont.

<sup>39</sup>. Aymonier III, pp. 461-65.

precise event. Perhaps it was an attack by King Jayanāśa of Śrīvijaya at the end of the seventh century, reflected in an Arab seamen's tale; but he also saw some connection in the fact that "In Java...the Buddhist dynasty of the Çailendras resuscitates the title of 'king of the mountain', given previously to the kings of *Fu-nan*", and that "In the middle of the eighth century [there were] conquests in Cambodia and in China by the Javanese king Sañjaya, mentioned in a late text".<sup>40</sup>

As has been noted earlier, with respect to Funan, the title 'King of the Mountain' (imagined as Khmer *kurun bnam*) is fictitious, and no connection between Cambodia and Java may be related to it. Studies of the Jayanāśa inscriptions suggest that his expeditions would not have gone as far as Cambodia, although they could conceivably have touched a Khmer area on the Malay peninsula, which is probably the Khmer kingdom concerned in the Arab story, if that is at all based on fact.<sup>41</sup> As for the Sañjaya story, it says he was attacked at 'Kemir', and even if it could reasonably be emended to indicate that it was Sañjaya who attacked, there is no claim of victory or conquest.<sup>42</sup>

The certain records of Indonesian attacks on Indochina which Coedès mentioned in the same context were all ephemeral coastal assaults, without conquest, and apparently without any political effect. They cannot imply the overthrow of an inland kingdom; and by the end of the reign of Jayavarman I, the time of the expeditions of King Jayanāśa, the earliest of the potential external causes evoked by Coedès, a new capital was probably being established

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<sup>40</sup>. Quotation from Coedès 1964b, pp.5-6. Unfortunately, standard historical work on Cambodia took too literally the statement in the Sdok Kak Thom inscription (K.235), the most detailed record of the establishment of a capital in the Angkor region, although written over two hundred years later, that the first Angkor king, Jayavarman II, came, or returned, from Java, and that he organized a rite to insure Cambodia's independence from Java. Apparent confirmation is also found in K.956, which refers to another ceremony to "prevent Kambuja from being taken by Java", and in a much more recently published third inscription, that of "Sab Bāk" in northeastern Thailand, recording a foundation "to prevent Cambodia from being attacked/occupied by Java" (see Chirapat 1990).

These statements from the Khmer side have been related to a vague Arab seamen's tale of a Mahārāja of Zabag, variously interpreted as Sumatra (Śrīvijaya) or Java, who attacked Cambodia and killed its king. Taking off from this, it was inferred that not only did the Mahārāja of Zabag kill the reigning king, but that he also took a Cambodian prince back to Zabag, and that this was the future Jayavarman II, who even "lived at the court of the Sailendra...until shortly before his return to Cambodia about 790". See Coedès 1964a, pp. 175-7, 184; 1968a, pp. 92-93, 97; 1964b, note 6; Briggs 1951b, pp. 65-69; Dupont 1952-54, pp. 152-57; Groslier 1962, p. 90 (the quotation about "court of the Sailendra"); Mabbett 1969, p. 208.

Subsequent work has been more circumspect. Wolters 1973, p. 21, n. 7 said "[t]he significance of 'Jav ā' in this context is still unknown"; and in his discussion of Jayavarman II in Wolters 1982, p. 7, he did not even mention this detail. Similarly, Claude Jacques 1972b, p. 208 wrote "...Jayavarman II, venant de Javā--quel que soit le lieu que ce nom représente" ('Jayavarman II, coming from Javā--whatever the place represented by this name').

<sup>41</sup> Coedès 1964b, pp. 155-59; 1968, pp. 83-4.

<sup>42</sup> See analysis of this tale in Damais 1957, pp. 635-7. Coedès 1964a, pp. 166-8, and 1968, p. 88 straightforwardly categorized Sañjaya's alleged conquests in Bali, Sumatra, Cambodia, and China as "incredible".

near Angkor, much too far inland to be endangered by a sea force from Indonesia. If Cambodia was also attacked at the same time as Champa, the incident was probably similar, and it may not be construed as the cause of political and demographic changes in Cambodia in the 8th century.<sup>43</sup>

### **Jayavarman II, the ‘founder’ of Angkor.<sup>44</sup>**

The origins and career of Jayavarman II have always caused difficulties, and they have not yet been resolved. At least three biographical sketches have been produced by Coedès and Claude Jacques, and I have offered modifications in Vickery 1998.<sup>45</sup> There is thus no special criticism implied in saying Coedès failed to solve all the problems. I do think, however, that had his indological predisposition not prevailed, and had he given more weight to the Khmer part of the Sdok Kak Thom inscription (K.235) than to the Sanskrit part, he would have spared later generations some unnecessary labor. That is, as has now been convincingly established by Claude Jacques, the Cambodians of the time did not establish a *devarāja* (‘*dieu royal*’). They established, or more probably re-emphasized, a strictly Khmer pre-Indic conception, *kamraten jagat ta rāja*, a type of *kamraten/kamratān* lord known to us from the earliest seventh-century Khmer inscriptions, for which the term ‘*devarāja*’ was nothing more than a rather awkward Sanskrit translation in the Sanskrit part of Sdok Kak Thom (and recorded nowhere else), which summarized the more detailed Khmer part.<sup>46</sup>

Had Coedès given emphasis to this, rather than to ‘*dieu royal*’, the ink which has flowed over whether it was a *lingam*, or an image of Śiva, or part of the towers of central temples could have been spared. In the Khmer text it is quite clear that the *kamraten jagat ta rāja* was not a building, nor a Śiva *lingam* at the summit of a temple, nor, I would assert, even an image of Śiva or any other Hindu god. It was something which could easily be moved from one royal abode to another, and was perhaps only a ceremony or a stylized symbol--a sort of protective deity now known as *neak tā*.

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<sup>43</sup> Coedès 1964b, p. 6: "During the same period a series of naval raids occur which bring ruin and destruction to the Indo-Chinese peninsular coast from north to south". "For more than a century, from 682 to 802, a series of reliable documents describe these maritime incursions, Javanese conquests on the east coast of Indo-China and Javanese domination over Cambodia". Coedès listed them as (1) 682 Śrīvijayan expedition; (2) 767 invasion of Tongking by Javanese (Maspero 1928, p. 97); (3) 774 incursion into Nhatrang by terrifying black men (Barth and Bergaigne 1885, p. 252); (4) 787 attack on Nhatrang by Javanese (Barth and Bergaigne 1885, p. 217).

<sup>44</sup> The name ‘Angkor’, of course, dates from much later, nearly modern, times, and the urban area now considered as ‘Angkor’ was not founded by Jayavarman II, who always resided elsewhere, but by his third successor, Yaśovarman (889-910).

<sup>45</sup> Vickery 1998, pp. 393-96.

<sup>46</sup> It cannot be too strongly emphasized that wherever there are parallel Khmer and Sanskrit texts it is the former which are more detailed.

At the end of his life Coedès realized that the entire *devarāja* problem may have been misconstrued, but he only alluded to this in his history in a vague way. In Coedès 1964a, discussing the reign of Jayavarman IV at Koh Ker, he said, laconically, that his foundation there represented “without doubt an innovation in the conception of the Devarāja”, with a footnote to an unpublished article, “The real founder of the cult of divine royalty in Cambodia”, which only appeared in 1970 as “Le véritable fondateur du culte de la royauté divine au Cambodge”.<sup>47</sup> In fact, that article completely demolished the construal of the *devarāja* based on the Sanskrit part of Sdok Kak Thom, and his new ideas should have pushed Coedès to rewrite the section of his history dealing with Jayavarman II.

It seems that Coedès had come to the conclusion that the cult of the *kamraten jagat ta rāja/rājya* had been developed by Jayavarman IV. If so, this means that Sdok Kak Thom may no longer be taken as a reliable guide to the career of Jayavarman II, but is only what it is in the first place, a claim to status by a particular family who by the 11th century had amassed wealth and high positions through close associations with several kings.

Besides this doubt about the origins of the cult of *kamraten jagat ta rāja/rājya* near the end of his life, Coedès also inserted a peculiar footnote on Śambhupura and the *devarāja* in all three editions of his work. It refers to “a stele at Sambor on the Mekong dated 803... which mentions Indraloka, great-grandfather of the inscription’s author.... I have already indicated that the appearance of a posthumous name at Sambor and the mention of the God-King [1964a “Roi-dieu”; 1944 and 1948 “Dieu-Roi”] in another inscription [K.125] of the same locality tend to place the origin of these rites in the Kingdom of Śambhupura, the homeland of the ancestors of Jayavarman II”.<sup>48</sup>

The place in which he had “already indicated” this must have been a 1911 article to which he refers published in the *Bulletin de la Commission Archéologique d’Indochine* (which I have not seen), for there is no such suggestion anywhere else in the texts of his history. But to have preserved this in a note until 1964, when his allusion to Jayavarman IV also showed doubt about the standard interpretation of the *devarāja* and Jayavarman II, shows he had long had serious doubts, in fact one could even say he did not believe in the conventional wisdom on this subject which he emphasized in his texts.<sup>49</sup> And why “Dieu-Roi” in 1944 and 1948, but “Roi-Dieu” in 1964? It seems Coedès

<sup>47</sup>. Coedès 1964a, p. 214; Coedès 1970.

<sup>48</sup>. Coedès 1944, p. 130, note 3; 1948, p. 176, note 3; 1964a, p. 193, note 2.

<sup>49</sup>. In the English translation of 1968, this note is number 41 on page 307, and it gives the title of the 1911 article, “Note sur l’apothéose au Cambodge”, and the page number 48.

sometimes preferred to conceal his most original thought in corners of difficult access, while maintaining a Standard Total View for the general reader, including scholars.

The reference to Śambhupura shows that Coedès realized the importance of this place, but did not wish to deal with it in his histories, suggesting that he had established his Standard View even before 1944, and refused to seriously revise it. New evidence was confined to notes where most readers would not realize that the text should have been changed. Readers of his histories seeing, “Puṣkarākṣa became king in Śambhupura [around 716]... and possibly received at death the title Indraloka, mentioned in an inscription of Sambor as great-grandfather of a queen reigning in 803”, followed by “an elder queen Jyeṣṭhārya, granddaughter of Nṛpendradevī and great-granddaughter of King Indraloka, made a foundation at Sambor in 803”, one year after the conventional date for the beginning of the reign of Jayavarman II, would not realize that a major detail of 8th-century history is being skimmed out.<sup>50</sup> The inscription in question is K.124, which shows an entirely local royal family in Śambhupura, from a king with a posthumous name, ‘Indraloka’, through three females in direct filial descent without mention of their consorts, to Queen Jeṣṭhārya, indicating rule by that family in Śambhupura from early in the 8th century. This must be put into relation with the circumstance which Coedès, neglecting the Khmer inscriptions, did not realize, that the rather long 7th-century epigraphic record of the Śambhupura region shows no allegiance, nor even any reference, to the mainstream kings of central Cambodia from Īśānavarman in Sambor Prei Kuk to Jayadevī at Angkor.

Śambhupura was obviously an important polity in its own right, and Coedès may not have been completely off the mark in his peculiar footnote calling it the homeland of the ancestors of Jayavarman II, although I would say that Jayavarman II began his career farther south. Śambhupura was at least an important stage in the accumulation of power by Jayavarman II, and the connection is seen in the first period of ‘Angkor’ epigraphy, the long inscriptions of Roluos, mostly in Khmer.<sup>51</sup> Coedès, with two exceptions for a Sanskrit inscription and one linked to royalty (K.713 and K.806), ignored them, but he did record, to be sure in a note, that Indraloka was also “mentioned in a stele of the Bakong”, the major temple in Roluos of Indravarman (877-889).<sup>52</sup>

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<sup>50</sup>. The quotations are respectively in (1) 1944, p. 118; 1948, p. 149, 1964a, p. 163; and (2) 1944, p. 118; 1948, p. 163; and 1964a, p. 178.

<sup>51</sup>. See the discussion of Śambhupura in Vickery 1998, chapter 8, and of the Roluos inscriptions in Vickery 1999.

<sup>52</sup>. Coedès 1944, p. 108, n. 5; 1948, p. 149, n.5; 1964a, p. 163.

This is, however, not a matter to be squirreled away in a note, but a major datum in synthesizing the information contained in the epigraphy of the 8th-9th centuries, at least if the objective is more than just a list of kings. It requires study of the long Khmer inscriptions of Indravarman and Yaśovarman at Roluos, which provide other links between the family of Jayavarman II and Śambhupura, even in details of the language. These inscriptions also provide details of royal family relationships which tend to argue against Dupont in favor of close linkages among the putative 8th-century ancestors of the Angkor kings, and they show the first examples of certain titles, such as *vāp*, which do not appear earlier, but were very important later at Angkor. Coedès' neglect of these texts is a real weakness in his work, and must be attributed to his indological preference for Sanskrit, and contempt for the mere "lists of serfs" which he too often saw in the Khmer texts.

[\*This is another instance when Coedès did not try to build on the work of an important predecessor, Aymonier, whose analysis of the Roluos inscriptions, had it been continued and developed, could have led to better understanding of that period than is found in the work of Coedès.<sup>53\*</sup>]

### **The Angkor Period**

Here too one must recall Aymonier and regret that later scholars too often refused to build on what he had accomplished.

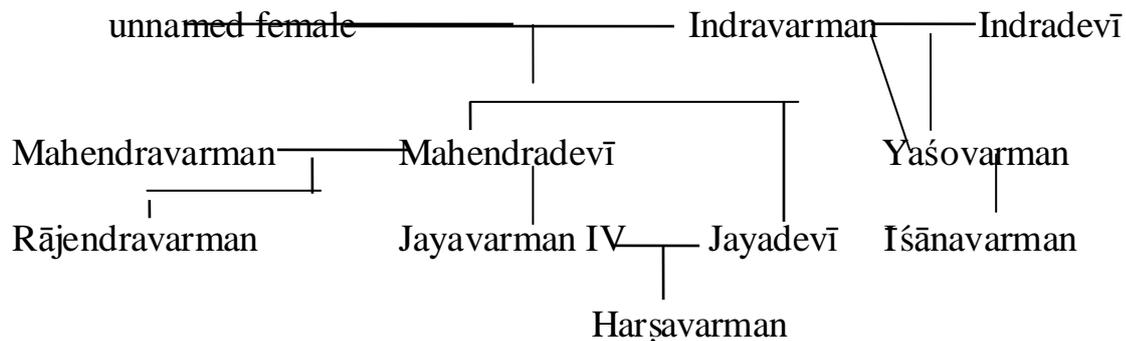
Aymonier's synthesized history of the Angkor period is less interesting than what he wrote about the 7th century, and it is based largely on the translation of Sanskrit inscriptions by Barth and Bergaigne. Thus there are many statements now recognized as in error, and Coedès 1944, and the later volumes, after 30 years of further research into Sanskrit inscriptions, art, and architecture, are largely superior. On some points, however, Coedès unwisely ignored Aymonier.

For example, Aymonier did not make the gratuitous assumption of usurpation by Jayavarman IV (928-941), with which Coedès unnecessarily put his followers off the rails, just because Jayavarman IV built a new capital city at Koh Ker. Here is a good example of the guiding theories of Coedès, with which Aymonier was not burdened, that Cambodian institutions should be interpreted according to what would have been the theoretical norms in ancient Hindu India, or in this case feudal Europe, in particular legitimate royal descent through patrilineal primogeniture, for the close relationships of Jayavarman IV to the rest of the Angkor royalty were clearly seen by Coedès, who did not draw the right conclusions, that is, Jayavarman IV was squarely within the main branch of the royal family, and his succession, rather than proving usurpation, only indicates that rules of succession in Angkor were different.<sup>54</sup>

<sup>53</sup>. For further study of the Roluos inscriptions see Vickery 1999.

<sup>54</sup>. Aymonier III, pp. 489-90; Coedès 1944, pp. 145-146, unchanged in Coedès 1964a, pp. 213-14, except for a

The position of Jayavarman IV is seen the following diagram based on his inscriptions and those of Rājendravarman (944-968), who moved the capital back to Angkor. These inscriptions include the following statements in different contexts: (1) Jayavarman was son of a sister [not named] of Yaśovarman [inscription K.522], (2) Jayavarman's first queen was a younger sister [not named] of Yaśovarman [K.905], (3) Jayavarman married an unnamed sister [half sister] of Iśānavarman's father [Yaśovarman], [K.286], (4) Jayavarman had a son Harṣavarman [K.686, K.905], (5) Harṣavarman's mother was Jayadevī, younger sister of Mahendradevī [K.806], (6) Rājendravarman was elder brother of Jayavarman, of the same mother; (7) Rājendravarman was elder brother (first cousin) of Harṣavarman [K.286, K.686, K.806] (8) Rājendravarman's mother was Mahendradevī and his father was Mahendrarvarman [K.806].<sup>55</sup> Moreover, the inscriptions of Rājendravarman speak of Jayavarman IV with the greatest respect, contrary to what one would expect if the latter had been considered an usurper, or if, as Claude Jacques has gratuitously put it, it had been a situation in which Rajendravarman “had...helped Harshavarman II in his bid for power and , after the latter's death, decided to seize power for himself”.<sup>56</sup>



Aymonier, however, followed his historical synthesis based on the Sanskritists with [\*a chapter on “Les institutions” in which, as he had done for the 7th century, he delineated the most important titles found in the Khmer inscriptions in a manner never surpassed by Coedès in his later histories nor in

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remark about a change in the institution of the so-called *devarāja* (see above).

<sup>55</sup> See Vickery 1986, p. 107; Coedès 1937, p. 74. In Khmer usage, both ordinary and royal, half-siblings and first cousins are often termed ‘brother’ and ‘sister’.

<sup>56</sup> Freeman and Jacques 1999, p. 11. This is an example of Jacques’ habit of introducing new historical conclusions in non-scholarly texts without the necessary references to full studies of the sources and the reasons for his interpretations. Both “bid for power” and “decided to seize power” require such scholarly apparatus. They ultimately go back to the conception of Jayavarman IV as an usurper, which Jacques no longer supports (*ibid.*, p. 10).

his publications of inscriptions\*].<sup>57</sup> Where in one case he made an obvious error, Coedès also failed to solve the problem, and, moreover, went into disconcerting and unhelpful hypotheses. This concerns the title *dalmak/dalmāk*, about which, in one context, Aymonier said it represented a category called *dal*, “thus a Dal, named Māk, sold some land in the 10th century”. This was inscription K.158 which Coedès had used in his interpretation of the name recorded by the Chinese for the 7th-century capital of Funan, \**d'ək-miuk*. Strangely, Aymonier made no connection with the term *dalmak* which he had read in the Roluos inscriptions, although there too he was in error, construing it as a place name.<sup>58</sup>

### Sūryavarman I

[\* Coedès' sections on Sūryavarman I there are good examples of scholastic involution, refusing to allow new information to displace obsolete conceptions. First, with respect to a special title characteristic of Sūryavarman, even though by 1944 he had renounced an earlier theory that Sūryavarman had come from the Malay peninsula via the lower Menam Basin to Angkor, he still insisted in 1944 and 1948 that one of Sūryavarman's titles, *kamtvan*, derived from Malay *tuan*, ‘master’. Aymonier had long before correctly identified that title with the Khmer word for ‘grandmother’, which Coedès only realized in 1954. In Coedès 1964 he finally got it right, but without giving credit to Aymonier.<sup>59</sup>\*]

Although renouncing a peninsular connection for Sūryavarman, and noting correctly that the first records concerning him were in eastern Cambodia, Coedès still found it necessary to insert the passages from the northern Thai chronicles concerning struggle among Ligor, Lavo, and Lamphun in which a certain Kambojarāja plays a role, and into which he had once inserted Sūryavarman, imagining that he had come from Ligor on the Malay peninsula. For this he gave the excuse that “even if the conflicts between Cambodia and the Mon kingdom of Haripunjaya... are imaginary, it is nevertheless true that Cambodian expansion into the lower Menam in the 11th century is attested by a group of Khmer inscriptions in Lopburi”. Historians should exclude what they believe to be imaginary, and as I have explained in detail elsewhere, those conflicts, even if not imaginary, did not involve Cambodia, and the Kambojarā

<sup>57</sup>. Aymonier III, pp. 539-549.

<sup>58</sup>. Aymonier III, p. 545; II, p. 447, Vickery 1998, pp. 36-37, 421-22; Vickery 1999, pp. 56-57, 74-75, where, p. 74, I missed two records of *dalmak*, K.256/AD 979, and K.158/AD 1003.

<sup>59</sup>. Aymonier III, p. 496; Coedès IC II [1942], p. 46, n. 7, "équivalence *kamtton* [pre-Angkor] = inscrip. angkoriennes *kamtvan*, titre d'origine malaise"; Coedès 1944, p. 172; 1948, p. 228; p. 9, n. 3, giving credit to Aymonier]; 1964, p. 248; Vickery 1985.

ja in question would have been a chief in the Ayutthaya-Lopburi area.<sup>60</sup>

Another resort to these possibly “imaginary” stories is in Coedès’ discussion of “The Liberation of the Thais” in the 13th century, the very concept of which is no longer in accord with current scholarship. He wrote, “in the 11th century the Khmer were installed in Lavo, and in the 12th century they had extended their domination to the border of Haripunjaya, coming into conflict with King Adityarāja”, a conflict which in fact could not have involved the Angkor kingdom of Cambodia at all, for the reasons explained in Vickery 1977 and 1985 cited in note 57.<sup>61</sup>

There was certainly a Khmer population in central ‘Siam’ in the 12th century, including Lavo and even beyond, as well as a Khmer ‘kingdom’, but its King *mahārājādhirāja...kurūṇ śrī* Dharmāsoka who had his inscription of A.D. 1167 in Nakhon Sawan engraved in Khmer and Pali, unlike Angkor usage of the time, was obviously not part of the Angkor political orbit. As I have written elsewhere, there is ample 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>62</sup> Neither is there any justification for Coedès’ proposal that the polity recorded in the 1167 inscription belonged to the Mon kingdom of Haripuñjaya (Lamphun). Coedès’ interpretation involved the covert assumption that newly discovered inscriptions had to be related to political centers already known from literary sources.

### **The 13th century and Chou Ta-kuan**

According to Coedès, just before 1296 “a disastrous war with the Thais of Sukhothai had ravaged the country”, as reported in the book by the Chinese envoy to Cambodia Chou Ta-kuan.<sup>63</sup> Coedès, moreover, exaggerated Pelliot’s translations of the passages from Chou Ta-kuan. In Coedès 1964a, p. 373, we find, “in the recent war with the Siamese, all the Khmer people were required to fight, and the country was entirely devastated”, with reference to Pelliot’s first edition of Chou Ta-kuan in 1902, pp. 173, 176. That was, however, Coedès’ interpretation, at least if we follow Pelliot’s new translation published in 1951. There, on p. 32, in a section on village organization, we find, “recently, during the war with Siam [the villages] were entirely devastated”, ‘the villages’ being an emendation by Pelliot, where perhaps just ‘villages’ or ‘some villages’ would

<sup>60</sup>. Coedès 1944, pp. 174-75; 1948, pp. 231-32; 1964a, pp. 251-52. For ‘Kambojarāja’ see Vickery 1977, pp. 369-77, 378-80; and for a full explanation of the Sūryavarman I problem, Vickery 1985.

<sup>61</sup>. Coedès 1944, p. 251; Coedès 1964a, p. 355.

<sup>62</sup>. For the inscription see Coedès 1958, pp. 132-134; and see Vickery 1979, p. 135.

<sup>63</sup>. Coedès 1944, pp. 272-73; 1964a, p. 383; the latest edition of Chou Ta-kuan’s work is Pelliot 1951.

have been better. Then on page 34 another context has, “in the war with the Siamese the entire population was required to fight. In general, these people know neither tactics nor strategy”. ‘Disaster’, ‘ravaging’, and ‘devastation’, moreover, are belied by Chou Ta-kuan’s description of the wealth of the country and its impressive constructions. It would seem that war damage had been restricted to the western border regions.

It seems more likely now that the war with Hsien/Sien/Sien-lo recorded by the Chinese envoy Chou Ta-kuan was not with Sukhothai, and I would propose that it did not even involve the Thai. Chou Ta-Kuan said that Sien was “towards the Southwest, 15 day-stages (*15 jours d’étapes*)” from Angkor, which eliminates any locality very far north of the Gulf coast. This fits very well with the latest treatments of ‘Hsien/Sien’, which make it a port state near the coast.<sup>64</sup>

In this connection some newly revealed correspondence between Coedès and Pelliot tends to confirm the new consensus on the location of Hsien/Sien, even if that was not the conclusion which they reached.<sup>65</sup> In two passages concerning ‘Siam’ Chou ta-kuan had used the name Sien-lo. In his letter to Pelliot Coedès pointed out that the composite name had not appeared in extant texts until after the unification of Sien and Lo-hu, that is “after the foundation of the kingdom of Ayudhyā in 1350”. He thus asked Pelliot if the original text had not said only ‘Lo-hu’, and that the form ‘Sien-lo’ “is not a correction of editors in Ming times” (1368-1644). Note that Coedès did not ask whether the original might not have been just ‘Sien’. That was because he was certain that in the 13th century Sien = Thai = Sukhothai. Lo-hu was known to be Lopburi, which Coedès accepted as independent of Sukhothai, but which had been reported by an earlier Chinese writer, Chau Ju-kua in 1225, as a vassal of Cambodia, and had sent its own first envoy to China in 1289. Thus Coedès suggested that the war reported by Chou Ta-kuan might have been a war by Lo-hu to gain independence from Cambodia.

Pelliot answered that indeed the original text had ‘Sien-lo’ in two passages, and that it was certainly an anachronism due to post-1350 editors. Nevertheless, in other passages in which Chou Ta-kuan spoke of ‘Siamese’, including the two in which reference was made to the war, he used only the term ‘Sien’, and thus a correction to ‘Lo-hu’ would not be possible. The correct conclusion to draw from this is that in his own original text, now lost, Chou Ta-kuan had written only ‘Hsien/Sien’, and said that it was southwest of Angkor, giving further support to the new consensus that Sien and Sukhothai were quite separate polities, with the former located somewhere near the Gulf coast.<sup>66</sup>

<sup>64</sup>. Pelliot 1951 (Chou Ta-kuan), p. 10; Chamvit 1992, p. 76. For a summary of other evidence and further references see Vickery 1995, p. 118 and notes 67-68.

<sup>65</sup>. “Une correspondance de 1928 entre George Coedès et Paul Pelliot à propos de Sien-lo”, presented to the public at the conference on “George Coedès aujourd’hui”, Bangkok, 9-10 September 1999.

<sup>66</sup>. The two passages with ‘Sien-lo’ are in the Introduction and in Section 3 (pages 10, 13 of Pelliot 1951),

In connection with this correspondence we see another example of Coedès' refusal to integrate new information if it would force a change in his preconceptions. In his histories he referred to this correspondence with Pelliot, but only to a different detail which he could manipulate to strengthen his views about the extent of the power of Rāma Khamhaeng. That is, Pelliot had informed him about a detail in the history of the Yuan (Mongol) dynasty saying that in 1294 the "Kan-mu-ting of the city of Pi-ch'a-pu-li sent an envoy to offer tribute [to China]". The city was of course Petchaburi, and the title 'kan-mu-ting', which the Chinese had also used for the ruler of Sien, may quite reasonably be restored, as Coedès saw, as the Khmer title *kamraten*, and Coedès construed it as meaning Rāma Khamhaeng. We now see, with Sien in its proper place, that the *kamraten* of Petchaburi, was either the *kamraten*, ruler of Sien, or the ruler of another Gulf polity, and that he/they was/were probably Khmer.<sup>67</sup> As noted in Vickery 1995, p. 118, the Yuan history had clearly distinguished Sukhothai from Sien, and placed the former upriver from the latter.

## Conclusion

I think the foregoing examples are sufficient to show that Coedès' history books are too often obsolete to serve as major basic texts for beginning students as lauded by the two historians cited at the beginning. Some of the obsolescence was there from the beginning, due to Coedès' own preconceptions. More obsolescence has resulted from new research in the 35 years since the final edition, and even from research starting just after Coedès set the unchanging form for his work of synthesization in 1944. These books, like Aymonier's *Le Cambodge*, must be studied thoroughly by specialists, but should not be put in students hands without a warning and professorial guidance.

Coedès' great work on Cambodia, which permits the further work on which we must be engaged, was the publication, translation and indexing of inscriptions in the eight volumes of his *Inscriptions du Cambodge*, and other work on inscriptions published in various journals, in particular *Bulletin de l'École Française d'Extrême-Orient*.

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concerning respectively the direction and distance from Angkor to Sien-lo and importation of cloth from Sien-lo and Champa. Sections 10 concerning the language, 29 concerning silk, and 34 and 39 concerning the war (Pelliot 1951, pp. 19, 30, 32, 39), have only 'Sien'. In the "Correspondence of 1928", as reproduced in the conference paper, Pelliot wrote 'IX' in error for 10, and 'XXIV' in error for 29. This was no doubt because he had not completed his final text.

<sup>67</sup>. Coedès 1944, p. 266, n. 3; Coedès 1948, p. 343, n. 5; Coedès 1964a, p. 373, n. 5; Coedès 1968a (English), p. 205, and n. 104, from which I have taken the English romanizations of the Chinese transcriptions.

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