

# Cœdès' Histories of Cambodia

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On the back cover of the new printing of the 1968 English translation of Cœdès' *The Indianized States of Southeast Asia* (Cœdès 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 Cœdès' 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 standards".

That English translation had been made from the 1964 'second edition' of Cœdès' *Les états hindouisés d'Indochine et d'Indonésie* (Cœdès 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 Cœdès 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 I intend to demonstrate that Cœdès' history is obsolete, and should not be given to students as a

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

basic text. Not all the obsolescence is the fault of Cœdès, 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 Cœdès' presuppositions about what constituted history, and what types of source materials should be synthesised.

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 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. My students in the beginning of my Penang period (1973-75), when the level of English at Universiti Sains Malaysia was nearly equal to Yale, were quite intelligently bored with, if not contemptuous of, history *à la* Cœdès. In the second year I tried teaching it critically, that is, for each section, demonstrating the defects in Cœdès' arguments. The reaction from the students was, quite naturally, "then, why bother?"

Perhaps they were right, and Cœdès 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 Cœdès, 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 Cœdès, I have found that is the only way to proceed. I refer to Cœdès, but teach original sources. Cœdès' *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 emphasising *Les*

*états/States*, in a discussion of Cœdès 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 Cœdès *par excellence*. This book, however, and even more so his *Les peuples de la péninsule indochinoise/The Making of Southeast Asia* (Cœdès 1962, 1969), were, for academic works, popularisations, 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.

Cœdès, 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 synthesis, some of which belongs in historical romance, not in history. Cœdès was a great synthesiser – 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.

Cœdès' 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

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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.

of inscriptions not included in *Inscriptions du Cambodge*, or discussions of particular points of the historical record.

Unfortunately, too little of Cœdès' 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 the *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 Cœdès 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. Cœdès' historical syntheses, which were the basis for most subsequent work, including Soviet studies, contain defects which 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 Cœdès' 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.

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

Let's 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 "Hinduisation" Cœdès 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 it is simply there, and was so strong that it prevented Cœdès 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 Przyluski, that "during the second European Bronze Age Indochina entered the orbit of a maritime civilisation including the Southeast Asia and Indonesia", "cleverness in navigation" was one of the features of the Austroasiatic cultural complex, and this maritime civilisation 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 sea capable of carrying 600 to 700 passengers" and constructed with a Persian Gulf technique. Cœdès 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 or the Chinese fleets were well developed, that the ships on which Chinese traveled to Southeast

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

<sup>5</sup> Cœdès 1944, pp. 9-10

Asia beyond Vietnam were Southeast Asian ships, and that the enormous ones to which Cœdès referred were built much later than the time of putative Hinduisation.

For some of this detail, Cœdès relied on Pelliot, who on this point showed the old orientalist and Indologist prejudice that backward Southeast Asia had to receive all advances in civilisation from China, India, or farther west. Writing of the large Funanese sailing ships (junks), able to carry several hundreds 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’uen-luen, 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 12<sup>th</sup> century.<sup>6</sup>

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 9<sup>th</sup> century, nor certain until the 12<sup>th</sup> century”.<sup>7</sup> This information from a technical specialist was not available in the 1940’s, but Cœdès should have integrated it into his 1964 edition.

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<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 Wheatley 1983, pp. 130, 267.

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

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, Cœdès listed the Indian contributions: Sanskrit, the alphabet, “the influence of law and the administrative organisation 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 Cœdès considered the model for law and administrative organisation. I believe no study has been conducted on this subject, and with respect to Cambodia, at least, research on its ancient administrative organisation 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 Cœdès wrote, citing Parmentier, “the monuments of the 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>

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

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

In general, an opinion of Stutterheim which Cœdès cited now seems much more accurate than the Hinduism of Cœdès, “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”. Cœdès 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 they who brought back ideas and models from India. This explains the great differences between the assumed models and results, and the differences between the ‘Hindus’ cultures of different parts of Southeast Asia.<sup>10</sup>

As Pierre-Yves Manguin put it in his contribution to the conference on “George Cœdès Aujourd’hui”, “Nothing could be more mistaken than [than Cœdès’ views about the backwardness of Southeast Asia before the arrival of the ‘Hindus’]. The archaeological research on the last 30 years has proved that the ‘Indianisation’ of Southeast Asia...happened after a millennium of steady exchanges with India, in which certain populations of Southeast Asia, who were beginning to organise 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**

Cœdès, along with everyone else in the 1940’s 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

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

<sup>11</sup> See Manguin 2000

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 13<sup>th</sup> 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 Cœdès disagreed, criticising, 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. Cœdès 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 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

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<sup>12</sup> Cœdès 1944, p. 245, Nan Chao was founded by the Thai; Cœdès 1964, the Thai “were established in Yunnan where it was long believed that they had founded... Nan Chao”. Similarly, Wyatt 1984, p. 14, forced to take cognizance of new linguistic work, admitted that Nan Chao had not been a Thai state, although asserting that there were Thais along its southern periphery; but he still insisted, p. 38, on standard conventional wisdom with the most important 13<sup>th</sup>-century development being “the movement of the Tai down from the upland valleys onto the plains formerly dominated by the major empires”, a relic of the Nan Chao theory, in spite of his recognition in earlier chapters of the gradual movement of the Thai into those areas over several previous centuries. Moreover, there is far too much about Nan Chao for a book on Thai history which denies the validity of the old theory.

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

Cœdès' treatment of Funan followed Pelliot closely except that he did not share Pelliot's scepticism about certain supposedly direct influences from India. Both Pelliot and Cœdès allowed themselves to be influenced by preconceived notions regarding 'Kaundinya', construing both the Chinese-recorded names *hun* (\*yuen)-chen(zien) or -houie or -t'ien(\*d'ien), and *Chiao Chen-ju* as Sanskrit *kaundinya* and, for Cœdès, postulating a first and second 'Hinduisation'. It should be obvious that both of those Chinese transcribed terms cannot be restored as *kaundinya*, and with the first, the least plausible, there is no sign in the story told by Funanese to the Chinese envoys of influences from India. Although Pelliot wished to see the name \*yuen-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 /kaundin/ of the name Kaundin(ya), this interpretation is rendered implausible by the title-name of his descendant, Hun P'an Houang/\*yeun b'uan *Xiwang*, of which the second and third syllables do not combine with the first to make any known Indic name or title; and /hun (\*yuen)/ alone, whether or not phonetically equivalent to /kuan/ in the 13<sup>th</sup> century, will not serve as a representation of 'Kaundinya', or anything else. We must conclude that \*yuen 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 3<sup>rd</sup> century, and Cœdès' construal of a 'First Hinduisation' 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 *kaundinya* is plausible, but it is not permissible to construe a ‘Second Hinduisation’, nor the arrival of an individual Kaundinya from India, for inscription K.5 shows that the local royalty, after several generations of contact, had simply adopted ‘Kaundinya’ as some kind of dynastic name, just as they adopted *-varma(n)* to indicate royal status. In that inscription, which Cœdès dated on palaeographic grounds to the 5<sup>th</sup> century, Gunavarman is called son of *kaundinyavamsa sasina*, ‘moon of the lineage of Kaundinya’, and later a king Jayavarman called himself Kaundinya 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 5<sup>th</sup> century, shows that the Funanese had updated their origin myth to incorporate details they had learned from contact with India since the early 3<sup>rd</sup> century. The old orientalists 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 over-synthesis by Cœdès. He tried to fit three Funan-period inscriptions into the Chinese story that Rudravarman had succeeded Kaundinya Jayavarman through assassinating a younger brother who was a more legitimate successor because his mother was queen, whereas Rudravarman was son of a concubine. Cœdès indulged here in some rather risky reconstruction, supplying names to the Chinese story, and saying that the murdered younger brother was Gunavarman, named in K.5, and his mother was queen Kulaprabhavati mentioned in the third

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<sup>13</sup> For more detailed discussion of these problems in Funan history see my forthcoming article, “Funan Reviewed: Deconstructing the Ancients”, to be published in *Seka Khmer*, Nouvelle Série No. 2, Phnom Penh.

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 Cœdès into special pleading. He recognised that the script of K.5 is earlier than that of K.875, and thus one should conclude that Gunavarman, 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>

Cœdès was again less perspicacious than Pelliot in emphasising 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 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. Cœdès provided a summary of those speculations, according to which "in 357...Funan fell under the domination of a foreigner" 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, 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

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

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

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

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>

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<sup>17</sup> For Pelliot’s full explanation see Pelliot 1903, p. 253, not 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.

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 Indianisation should be put aside, even if he was ultimately a chief of Funan.

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 Vyadhapura. The Chinese mentioned a name, T'e-mu/T'ö-mou/\**d'ek-miuk*, only once, in one of their last reports on Funan in the 7<sup>th</sup> 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 Srivijaya, may have been a maritime trading society in which different ports, or inland centres linked to ports, alternated as power centres. It is plausible, however, to accept that the centre or centres which the Chinese knew were in southern Cambodia and Vietnam, and not far into central Thailand as a few writers have suggested.<sup>19</sup>

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<sup>19</sup> For example, Boisselier 1965, who suggested Uthong; and Hoshino 1993, where Funan is localised near Sri Thep in the upper Pasak valley. Very recently a writer of superficial popularising articles in the Bangkok English-language press, Michael Wright (Wright 1999a), took up Hishino'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 5,000 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 Cœdès colloquium Wright answered

In the view of Aymonier in 1904 the physical remains at Angkor Borei suggested it as the most important centre in Funan times, and on this point Pelliot followed Aymonier. As for the name 'Vyadhapura', Aymonier was less certain. He thought it was Angkor Borei because "an ancient Khmer inscription" in Battambang "speaks of Vrai Krapas Vyadhapura, thus giving us a certain identification for the ancient Vyadhapura, the capital of Cambodia in the 6<sup>th</sup> century *saka* [578-678 AD]". That is, Vrai/Prei Krapas is still the name of the district in which

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that silting could have accounted for the filling of the Menam Basin, evidently between the 3<sup>rd</sup> the century and the 14<sup>th</sup>-century founding of Ayutthaya (Wright 1999b). A similar, but less radical, proposal has recently appeared in *ศิลปวัฒนธรรม* (*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 Dvaravati times as far north as Supanburi and beyond Ayutthaya, with Lopburi, Nakhon Nayok, Uthong, Nakhon Pathom, and Ratchaburi on the coast (สุจิตต์ วงษ์เทศ [Sujit Wongthes] and ศรีศักดิ์ วัลลิโภดม [Srisak Vallibhotama], "สุวรรณภูมิอยู่ที่นี้" ['Suvannaphum is here']). Such pseudo-scientific geography has a long history in Thailand. In *The 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 8<sup>th</sup> century about equidistant between Lopburi and Sukhothai [perhaps Nakhom Sawan?], whereas five hundred years later it was at Bangkok, said, p. 11, "The north-south difference between these two positions, approximately 220 kilometres, requires the average annual increment to have been about 400 metres, a rate 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 kilometres (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 Thosarat 1998, p. 14, describe silting in the Chao Phraya delta, which today sees "the delta advance by up to six metres 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 in *ศิลปวัฒนธรรม* proposed for the Dvaravati period.

Angkor Borei is located. In another context Aymonier said that the same information proved that Vyadhapura, “city of hunters” was the capital of Bhavavarman [I], but “was this city also the capital, possibly under other names, of his three predecessors?” (including Jayavarman and Rudravarman of Funan). The title *adhiraja* of Vyadhapura, given in the Angkor royal genealogies as ancestors of the first Angkor kings, “may indicate only the 7<sup>th</sup>-century kings, which, we know, were established in this city, and not their predecessors of the 6<sup>th</sup> century”; and Aymonier considered that Isanapura, city of Isanavarman (±616-637), was just a new name for Vyadhapura, Angkor Borei.<sup>20</sup> On the last two points we now realise that Aymonier was mistaken, and Cœdès’ treatment superior, for the location of Isanapura is firmly established at Sambor Prei Kuk in Northern Kompong Thom; and it is believed that the residence of his predecessors, Bhavavaman I and Citrasena Mahendravarman, was also in the north.

Cœdès 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’ek-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 Cœdès.

Cœdès concluded that with \**d’ek-miuk* the Chinese were transcribing a Khmer word *dmak* or *dalmak/dalmak*, ‘hunter’, equivalent to modern Khmer /*tromeak*/ ‘mahout’, which would have been the Khmer translation of *vyadha* in the Sanskrit name *vyadhapura*, 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 Cœdès to mean Funan. There are three weaknesses in this argument.<sup>21</sup>

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

<sup>21</sup> Cœdès 1942, p. 110, note 5

First, if the Khmer terms *dmak~ dalmak/dalmak* really mean ‘hunter’, it is in the sense of ‘lassooer’ or ‘trapper’, to capture alive, as an elephant, while Sanskrit *vyadha* means to pierce with a sharp instrument. Second, there is no evidence for the term *dalmak/dalmak* until the end of the 9<sup>th</sup> century, and in the 10<sup>th</sup>-century inscription to which Cœdès 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'ek-miuk*, suggest that the first syllable was the Mon-Khmer word for ‘water’, /dik, dek, dak/ (modern Khmer /tek/), 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'ek-miuk* meant are premature. Nevertheless, it should definitely be dissociated from *vyadhapura*.

As for the location of Vyadhapura, Cœdès used some of the Angkor-period references to demonstrate that it had been located at Ba Phnom, some 60 kilometres northeast of Angkor Borei. He ignored, however, several of the non-royal Angkor-period references to Vyadhapura, 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 Cœdès was his total neglect of the only pre-Angkor record of Vyadhapura, 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

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<sup>22</sup> Vickery 1998, “Appendix”, *'dalmak/dalmak'*; 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.

Ba Phnom or Angkor Borei. In that inscription Vyadhapura is seen as a provincial centre under its own chief entitled in Khmer *kurak klon* Vyadhapura, enhanced in the Sanskrit part of the inscription to *vyadhapuresvara*, literally 'king of Vyadhapura'. The full meaning of the pre-Angkor title *kurak* has not been determined, but it appears to have been only a second-level chief, lower than the ones called *vrah kamratan an...varma(n)*. Thus Vyadhapura 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 Vyadhapura in the Angkor inscriptions was because it was the home district of Jayavarman II. The 10<sup>th</sup>-century uses of Vyadhapura 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 centre 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'ek-miuk*. It is quite possible that during its existence as known to the Chinese the major power centre of Funan changed, even more than once, as has been hypothesised for another Southeast Asian maritime area, that known as Srivijaya, and now generally accepted by specialists.<sup>23</sup>

When Pelliot discussed the location of the capital he measured the 500 *li* or 200 kilometres 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 towards Angkor Borei had been discovered, and he supposed that the only water routes would have been up the Mekong or

<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 7<sup>th</sup> century (Pelliot, *ibid.*, pp. 262-63).

Bassac river. Only in that way can a distance of 200 kilometres, 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 kilometres goes far beyond Angkor Borei, itself only 90 kilometres 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 kilometres from Oc-Eo would nearly reach Kompong Cham.

One final remark on the Funan period concerns Cœdès’ acceptance of a totally speculative suggestion by Louis Finot that the kings of Funan were called *kurun bnam*, ‘king of the mountain(s).’ This title has not been found in any Cambodian document from any period, as Cœdès, who knew the inscriptions better than anyone, should have been aware. But accepting it permitted linking Funan and other areas in the type of over-synthesis which Cœdès 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 Cœdès’ 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 7<sup>th</sup>-century Khmer inscriptions are in the first two volumes of *Inscriptions du Cambodge* which Cœdès had already prepared. It shows that Cœdès was just not interested in social history or political institutions even when he was familiar with relevant sources.

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

In principle Cœdès acknowledged the importance of Khmer inscriptions, but he neglected them. Even when Cœdès 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 10<sup>th</sup>-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. Cœdès 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 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 (Cœdès 1944, p. VI; 1964, p. 8) Cœdès wrote that “much still remains to do for...political institutions and the material civilisation. 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 Cœdès 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 civilisation’ Cœdès was clearly more comfortable with the Chinese accounts of early Cambodia, which we now realise 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 Funan 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

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<sup>26</sup> Cœdès 1964c, p. 129, “Dalle de Vat Samron”

instance of this practice is in...[a work of] 1609 but still preserving unchanged material from...the 3<sup>rd</sup> 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 Cœdès 1944 (p. 89), repeated in Cœdès 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 organised administration and an entire hierarchy of officials whose titles are better understood than their functions”. Cœdès 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 Cœdès are certainly pertinent in themselves, they show that he was little interested in what the Khmer inscriptions might reveal about ‘political institutions’ and ‘material civilisation’. They also show an inexcusable negligence, even suppression, of the work of a predecessor who, 40 years before Cœdès 1944, when Cœdès 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.

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 Cambodian 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 Cœdès and Pelliot. Secondly, the syntheses later produced by Cœdès from Sanskrit inscriptions were usually superior to this aspect of Aymonier’s work, as one would expect after 20-30 years of progress in the field, but Cœdès totally ignored the

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<sup>27</sup> Wheatley 1983, pp. 120, 153 (note 12)

advances made by Aymonier in interpretation of *Khmer* texts, and which, no doubt because of Cœdès, were then ignored by the *epigone* until very recently.

In this connection I must plead guilty myself, having, under the influence of Cœdès, 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 7<sup>th</sup>-8<sup>th</sup> century 'political institutions'. I wish to take this occasion to apologise to Aymonier's memory.<sup>28</sup>

In Aymonier III (pp. 446-7) we find correct descriptions of the important chiefly titles *pon* and *mratan*, and others, never studied by Cœdès.<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 7<sup>th</sup>-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 7<sup>th</sup>-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.

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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 Cœdès' neglect, saying only that "the *pon* and the *vap* 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

That is, again because of their Marxism, they saw ancient Cambodia as feudal, that is, a private property society, and they analysed the *mratan* and *pon* as, respectively greater and lesser landowners. In my view land was not private property in ancient Cambodia, and the positions of *mratan*, *pon*, and other types of functionaries were not related to ownership or possession of land.<sup>31</sup>

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

In one context in which Cœdès 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 7<sup>th</sup>-century king Jayavarman I. From 1944 to 1964 Cœdès 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 Sivapura’”. In fact there are five inscriptions referring to a king at that time with a posthumous name relating to Sivapura.<sup>33</sup>

Then, however, in Cœdès 1968a, the English translation of Cœdès 1964a, it was said that Cœdès 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

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<sup>31</sup> Mikheev *et al* 1981. One predecessor, with respect to *pon*, 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 *kpon*, in the expanded title *kpon kamratan an* in the earliest dated Khmer inscription, K.600/A.D. 611, Cœdès said merely that “the first list of servants was offered to the god *kpon kamratan an*” (Cœdès 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> Cœdès 1944, p. 48; Cœdès 1964a, p. 138; Vickery 1998, pp. 356-64

713 calls 'the king who went to Sivapura'". This change was explained only as "Ed. note: Date corrected by Cœdès; third edition had 681". No attempt was made to revise the awkward arithmetic of a reign lasting 30 years from the 650's 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 Cœdès' discussion of the relationship of Jayavarman I and Jayadevi, 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 Sivapura. Moreover, if the rest of Cœdès' discussion is accepted, and only the critical date changed from 680-1 to 690, or later, other difficulties appear, for there would still be a Jayavarman-Sivapura whose death by 680-1 is explicit or implied in at least three texts, and he was the father of Jayadevi. We 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 Cœdès' work, comes from a hitherto neglected inscription, K.1004, from a location very near Angkor Borei, some 60 kilometres 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. Cœdès apparently did not doubt that the Jayavarman was Jayavarman I, and Claude Jacques has continued to emphasise this inscription as evidence for extending the reign of Jayavarman I and revising the meaning of Sivapura. 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 (personal communication) provided a

transcription and translation. The first line of the Sanskrit prologue is “*abde srimati dasrakevala rasai rajnas sakanam yada...*”, which Jacques has translated as “[w]hen in the year of grace counted by 612 (the six tastes, unique, the two Asvin) of the king of the *sakas*”; and it ends with the term ‘*sasna[m]*’, “promulgate”. Nowhere else in Khmer epigraphy does ‘*abda*’ (‘year’, Monier-Williams 1976: 60) alone begin the statement of a date, ‘*srimat*’ was never used in the 7<sup>th</sup> century in the expression of a date, but only before the names of kings or gods, the expression ‘*rajnas sakanam*’ is not found anywhere else in pre-Angkor epigraphy, and ‘*sasasa[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 *ajna*.

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 7<sup>th</sup>-century charter. In such a case it would be quite unreliable for any detail of 7<sup>th</sup>-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 scepticism. 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 7<sup>th</sup>-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 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 7<sup>th</sup>-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 7<sup>th</sup>-century Sanskrit, but might have badly misunderstood 7<sup>th</sup>-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 7<sup>th</sup> century, the retrospective importance accorded him by his daughters Jayadevi and Sobhajaya, and the evidence on 'the King who went to Sivapura'. Given the long break until the next recorded ruler, Queen Jayadevi 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 Eighth Century**

The next problematic period, both in Cambodian history and in Cœdès' 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.*

Cœdès, 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 Puskaraksa had taken power in Sambhupura (Sambor in Kratie Province on the Mekong), thereby causing the split. The name Puskaraksa comes from the partly legendary ancestor lists in the royal Angkor genealogies, but there is nothing in those sources about his 'taking power'. He is simply recorded as an ancient king in Sambhupura.

Throughout his three editions the details were mostly the same, with, however, some interesting changes as Cœdès re-read some of the inscriptions. In 1944 he wrote that the Chinese recorded the division in 706, and that there was a "King Baladitya [of Water Chenla] who claimed descent from Kaundinya and the Nagi Soma", an interesting illustration

of Cœdès' synthsising of history. There is no record of Baladitya '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 Puskara *or* [emphasis added] Puskaraksa became king in the State of Sambhupura and had an inscription engraved in 716". This is more uncritical synthesising. The inscription of 716 is the reason for "Puskara or Puskaraksa", 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 Puskaraksa. Moreover, none of the genealogies says Puskaraksa of Aninditapura became King in Sambhupura. There is one that makes him descend in the Aninditapura line and another that identifies him as ancestor of royalty in Sambhupura. An interesting detail which Cœdès added in 1944, and which will be discussed below, was that perhaps Puskaraksa acquired his position in Sambhupura through marriage, perhaps to Jayadevi, widow of Jayavarman I, and that he may have been the Indraloka, named posthumously in an inscription in Sambhupura.<sup>34</sup>

In 1948 Cœdès 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, Jayadevi". Nevertheless, it was the seizure of Sambhupura by Puskaraksa, 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 Cœdès believed that at the death of Jayavarman I all of Cambodia, including Sambhupura, 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

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<sup>34</sup> Cœdès 1944, pp. 106-08

<sup>35</sup> Cœdès 1948, p. 149

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

*In the latter context Cœdès 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 kings of Chenla", with a footnote reference to new work by Pierre Dupont.*

In contrast to his treatment of Vyadhapura where he had read back details from the 10<sup>th</sup>-11<sup>th</sup>-century Angkor inscriptions, Cœdès had ignored, except for Puskaraksa, what the Angkor royal genealogies said about the eighth century. These details were developed by Dupont, cited by Cœdès who, however, ignored the details in a good example of scholastic involution. Dupont analysed 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 Cœdès' work. If Cœdès disagreed with Dupont he should have at least discussed the matter, for Dupont's sources were the same which Cœdès had accepted to prove that Vyadhapura was the name of the capital of Funan. I also disagree with Dupont, but his work deserves full discussion.<sup>37</sup>

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

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

Some credit here should also be given to the neglected Aymonier. For the 8<sup>th</sup>-century division of Chenla, Aymonier already went beyond Cœdès 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/Sambhupura on the Mekong was one of the capitals, “but of which part we cannot say”. Cœdès also saw the importance of Sambhupura, but his speculations about it changed from one edition to the next (see further below on Sambhupura). In Cœdès 1964a he repeated the passage from Cœdès 1944, referring only in a note to the work of Dupont which had totally changed the picture of 8<sup>th</sup>-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 8<sup>th</sup>-century dynasties is more useful than that of Cœdès 1964a, and is no more in error.<sup>39</sup>

### **Javanese Invasion in the 8<sup>th</sup> Century**

Cœdès was not consistent in his discussions of the effect of a supposed Indonesian invasion on the internal organisation of Cambodia, and this is one of the cases which indicates a disinterest in history as a scientific discipline or field of knowledge. In Cœdès 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 (Cœdès 1964b) he treated it as a main cause of Cambodian decline in the 7<sup>th</sup>-8<sup>th</sup> centuries. Even then there was uncertainty about the precise event. Perhaps it was an attack by King Jayanasa of Srivijaya 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 Cailendras resuscitates the title of ‘king of the mountain’, given

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

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

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 Sanjaya, 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 Jayanasa inscriptions suggest that his expeditions would not have gone as far as Cambodia, although they could conceivably have touched

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<sup>40</sup> Quotation from Cœdès 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 organised 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 Bak” 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 Maharaja of Zabag, variously interpreted as Sumatra (Srivijaya) or Java, who attacked Cambodia and killed its king. Taking off from this, it was inferred that not only did the Maharaja 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 Cœdès 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, note 7, said “[t]he significance of ‘Java’ 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 Java – quel que soit le lieu que ce nom représente” (‘Jayavarman II, coming from Java – whatever the place represented by this name’).

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 Sanjaya story, it says he was attacked at 'Kemir', and even if it could reasonably be emended to indicate that it was Sanjaya who attacked, there is no claim of victory or conquest.<sup>42</sup>

The certain records of Indonesian attacks on Indochina which Cœdès 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 Jayanasa, the earliest of the potential external causes evoked by Cœdès, a new capital was probably being established 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 Campa, the incident was probably similar, and it may not be construed as the cause of political and demographic changes in Cambodia in the 8<sup>th</sup> century.<sup>43</sup>

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

<sup>42</sup> See analysis of this tale in Damais 1957, pp. 635-37. Cœdès 1964a, pp. 166-68, and 1968, p. 88 straightforwardly categorised Sanjaya's alleged conquests in Bali, Sumatra, Cambodia, and China as "incredible".

<sup>43</sup> Cœdès 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". Cœdès listed them as (1) 682 Srivijayan 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).

## Jayavarman II, the 'Founder' of Angkor<sup>44</sup> and the Devaraja

The origins and career of Jayavarman II have always caused difficulties, and they have not yet been solved. At least three biographical sketches have been produced by Cœdès and Claude Jacques, and I have offered modifications in Vickery 1998.<sup>45</sup> There is thus no special criticism implied in saying Cœdès 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 (K.235) than to the Sanskrit part, he would have spared later generations some unnecessary labour. That is, as has now been convincingly established by Claude Jacques; the Cambodians of the time did not establish a *devaraja* ('*dieu royal*'). They established, or more probably re-emphasised, a strictly Khmer pre-Indic conception, *kamraten jagat ta raja*, a type of *kamraten/kamratan* lord known to us from the earliest seventh-century Khmer inscriptions, for which the term '*devaraja*' was nothing more than a rather awkward Sanskrit translation in the Sanskrit part of the Sdok Kak Thom (and recorded *nowhere else*), which summarised the more detailed Khmer part.<sup>46</sup>

Had Cœdès given emphasis to this, rather than to '*dieu royal*', the ink which has flowed over whether it was a *lingam*, or an image of Siva, 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 raja* was not a building, nor a Siva *lingam* at the summit of a temple, or, I would assert, even an image of Siva or any other Hindu god. It was something which could easily be moved from one royal abode to another, and was

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<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, Yasovarman (889-910).

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

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

perhaps only a ceremony or stylised symbol – a sort of protective deity now known as *neak ta*.

At the end of his life Cœdès realised that the entire *devaraja* problem may have been misconstrued, but he only alluded to this in his history in a vague way. In Cœdès 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 Devaraja”, 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 *devaraja* based on the Sanskrit part of Sdok Kak Thom, and his new ideas should have pushed Cœdès to rewrite the section of his history dealing with Jayavarman II.

It seems that Cœdès had come to the conclusion that the cult of *kamraten jagat ta raja/rajya* 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 11<sup>th</sup> 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 raja/rajya* near the end of his life, Cœdès also inserted a peculiar footnote on Sambhupura and the *devaraja* 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 Sambhupura, the homeland of the ancestors of Jayavarman II”.<sup>48</sup>

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

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

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 Archæologique 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 *devaraja* 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 emphasised in his texts.<sup>49</sup> And why “Dieu-Roi” in 1944 and 1948, but “Roi-Dieu” in 1964? It seems Cœdès 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 Sambhupura shows that Cœdès realised 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 seriously to revise it. New evidence was confined to notes where most readers would not realise that the text should have been changed. Readers of his histories seeing, “Puskaraksa became king in Sambhupura [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 Jyestharya, granddaughter of Nrpendradevi 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 realise that a major detail of 8<sup>th</sup>-century history is being skimmed out.<sup>50</sup> The inscription in question is K.124, which shows an entirely local royal family in Sambhupura, from a king with a posthumous name,

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<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.

<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.

'Indraloka', through three females in direct filial descent without mention of their consorts, to Queen Jyestharya, indicating rule by that family in Sambhupura from early in the 8<sup>th</sup> century. This must be put into relation with the circumstance which Cœdès, neglecting the Khmer inscriptions, did not realise, that the rather long 7<sup>th</sup>-century epigraphic record of the Sambhupura region shows no allegiance, nor even any reference, to the mainstream kings of central Cambodia from Isanavarman in Sambor Prei Kuk to Jayadevi at Angkor.

Sambhupura was obviously an important polity in its own right, and Cœdès 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. Sambhupura 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> Cœdès, 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>

This is, however, not a matter to be squirreled away in a note, but a major datum in synthesising the information contained in the epigraphy of the 8<sup>th</sup>-9<sup>th</sup> 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 Yasovarman at Roluos, which provide other links between the family of Jayavarman II and Sambhupura, even in details of the language. These inscriptions also provide details of royal family relationships which tend to argue against Dupont in favour of close linkages among the putative 8<sup>th</sup>-century ancestors of the Angkor kings, and they show the first examples of certain titles, such as *vap*, which do

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

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

not appear earlier, but were very important later at Angkor. Cœdès' 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 Cœdès 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 Cœdès.<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 synthesised history of the Angkor period is less interesting than what he wrote about the 7<sup>th</sup> century, and it is based largely on the translation of Sanskrit inscriptions by Barth and Bergaigne. Thus there are many statements now recognised as in error. Cœdès' histories, after 30 years of further research into Sanskrit inscriptions, art, and architecture, are largely superior. On some points, however, Cœdès unwisely ignored Aymonier.

For example, Aymonier did not make the gratuitous assumption of usurpation by Jayavarman IV (928-941), with which Cœdès 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 Cœdès, 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. The close relationships of Jayavarman IV to the rest of the Angkor royalty were clearly seen by Cœdès, 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

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

proving usurpation, only indicates that rules of succession in Angkor were different.<sup>54</sup>

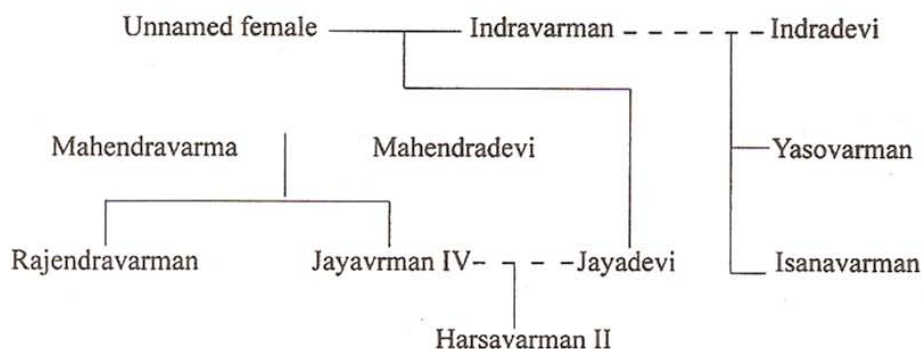
The genealogical position of Jayavarman IV is seen in the following diagram based on his inscriptions and those of Rajendravarman (944-968), who moved the capital back to Angkor. These inscriptions include the following statements in different contexts: (1) Jayavarman was son of a sister [not named] of Yasovarman [inscription K.522], (2) Jayavarman's first queen was a young sister [not named] of Yasovarman [K.905], (3) Jayavarman married an unnamed sister [half sister] of Isanavarman's father [Yasovarman], [K.286], (4) Jayavarman had a son Harsavarman [K.686, K.905], (5) Harsavarman's mother was Jayadevi, younger sister of Mahendradevi [K.806], (6) Rajendravarman was elder brother of Jayavarman, of the same mother; (7) Rajendravarman was elder brother (first cousin) of Harsavarman [K.286, K.686, K.806].<sup>55</sup> Moreover, the inscriptions of Rajendravarman 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>

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<sup>54</sup> Ayminier III, pp. 489-90; Cœdès 1944, pp. 145-46, unchanged in Cœdès 1964a, pp. 213-14, except for a remark about a change in the institution of the so-called *devaraja* (see above)

<sup>55</sup> See Vickery 1986, p. 107; Cœdès 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).



Aymonier, however, followed his historical synthesis based on the Sanskritists with a chapter on “Les institutions” in which, as he had done for the 7<sup>th</sup> century, he delineated the most important titles found in the Khmer inscriptions in a manner never surpassed by Cœdès in his later histories nor in his publications of inscriptions.<sup>57</sup> Where in one case Aymonier made an obvious error, Cœdès also failed to solve the problem, and, moreover, went into disconcerting and unhelpful hypotheses. This concerns the title *dalmak/dalmak*, about which, in one context, Aymonier said it represented a category called *dal*, “thus a Dal, named Mak, sold some land in the 10<sup>th</sup> century”. This was inscription K.158 which Cœdès had used in his interpretation of the name recorded by the Chinese for the 7<sup>th</sup>-century capital of Funan, \**d’ek-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>

<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 records of *dalmak* K.256/AD 979, and K.158/1003.

## Suryavarman I

In Cœdès' sections on Suryavarman 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 Suryavarman, even though by 1944 he had renounced an earlier theory that Suryavarman had come from the Malay peninsula via the lower Menam Basin to Angkor, he still insisted in 1944 and 1948 that One of Suryavarman's titles, *kamtvan*, derived from Malay *tuan*, 'master'. Aymonier had long before correctly identified that title with the Khmer word for 'grandmother', which Cœdès only realised in 1954. In Cœdès 1964 he finally got it right, but without giving credit to Aymonier.<sup>59</sup>

Although renouncing a peninsular connection for Suryavarman, and noting correctly the first records concerning him were in eastern Cambodia, Cœdès still found it necessary to insert the passages from the northern Thai chronicles concerning struggle among Ligor, Lavo, and Lamphun in which a certain Kambojaraja plays a role, and into which he had once inserted Suryavarman, 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 11<sup>th</sup> 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 Kambojaraja in question would have been a chief in the Ayutthaya-Lopburi area.<sup>60</sup>

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<sup>59</sup> Aymonier III, p. 496; Cœdès 1944, p. 172; 1948, p. 228; 1964, p. 248; Vickery 1985.

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

Another resort to these possibly 'imaginary' stories is in Cœdès' discussion of "The Liberation of the Thais" in the 13<sup>th</sup> century. He wrote, "in the 11<sup>th</sup> century the Khmer were installed in Lavo, and in the 12<sup>th</sup> century they had extended their domination to the border of Haripunjaya, coming into conflict with King Adityaraja", 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 60.<sup>61</sup>

There was certainly a Khmer population in central 'Siam' in the 12<sup>th</sup> century, including Lavo and even beyond, as well as a Khmer 'kingdom', but its King *marajadhirarja...kurun* Sri Dhamasoka 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 centres which were outside the political and cultural orbit of Angkor.<sup>62</sup> Neither is there any justification for Cœdès' proposal that the polity recorded in the 1167 inscription belonged to the Mon kingdom of Haripunjaya (Lamphun). Cœdès' interpretation involved the covert assumption that newly discovered inscriptions had to be related to political centres already known from literary sources.

### The 13<sup>th</sup> Century and Chou Ta-kuan

According to Cœdès, 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> Cœdès, moreover, exaggerated Pelliot's translation of the passage from Chou

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<sup>61</sup> Cœdès 1944, p. 151; Cœdès 1964, p. 355

<sup>62</sup> For the inscriptions see Cœdès 1958, pp. 132-34; and see Vickery 1979, p. 135.

<sup>63</sup> Cœdès 1944, pp. 272-73; 1964, p. 383

Ta-kuan. In Cœdès 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, Cœdès’ interpretation, at least if we follow Pelliot’s new translation published in 1951. There (p. 32), in a section on village organisation, 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 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 Cœdès 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 Cœdès pointed out that the composite name had not appeared in any extant texts until after the unification of Sien an Lo-hu, that is “after the foundation of the kingdom

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<sup>64</sup> Pelliot 1951 (Chou Ta-kuan), p. 10; Charnvit 1992, p. 76

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

of Ayutthaya 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 Cœdès did not ask whether the original might not have been just 'Sièn'. That was because he was certain that in the 13<sup>th</sup> century Sien = Thai = Sukhothai. Lo-hu was known to be Lopburi, which Cœdès 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 Cœdès 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>

In connection with this correspondence we see another example of Cœdès' 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

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

strengthen his views about the extent of the power Rama 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 ruler of Sien, may quite reasonably be restored, as Cœdès saw, as the Khmer *kamraten*, and Cœdès construed it as meaning Rama Khamhaeng. We now see, with Sien in its proper place, that *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 Sein, and placed the former upriver from the latter.

## Conclusion

I think the foregoing examples are sufficient to show that Cœdès’ 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 Cœdès’ own preconceptions. More obsolescence has resulted from new research in the 35 years since the final edition, and even from research starting just after Cœdès set the unchanging form for his work of synthesis 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.

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

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