
Before *The Early State* and After: An Introduction

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BEFORE THE EARLY STATE

About thirty years ago the collection volume *The Early State* appeared. The book did not appear out of the blue, but had had a long period of gestation. A period during which my ideas about the existence of centralized socio-political formations gradually took form. The basis was laid during long conversations (in 1959) with my supervisor, Professor André J. F. Köbben of Amsterdam University. These finally led me to writing a Ph. D. thesis in which a number of non-literate centralized societies would be described and compared with respect to their political organization. To one who studied Anthropology in Amsterdam the comparative perspective was quite natural – Köbben being one of the leading scholars in the field of comparativism (e.g., Köbben 1955, 1967, 1970). This approach necessitated on the one hand to develop definitions that could be used in intercultural comparisons, and on the other, to produce systematic descriptions of the selected societies to make such comparisons possible. Many of the definitions had to be invented on the spot, for there had not been much intercultural research in this field to build on. Among these works should be mentioned those of Julian H. Steward, whose views influenced my approach deeply (Steward 1949), and *African Political Systems*, edited by M. Fortes and E. E. Evans-Pritchard (1940). During the years in which I prepared my thesis I was a teacher of geography and history in a secondary school in Wassenaar in the Netherlands. I thus had not the opportunity to do fieldwork, and so the thesis became based upon the analysis of literature – travelogues, journals, letters, memoirs, and similar sources. I am thus a genuine armchair anthropologist. Apart from some considerations about comparison, there was not much explicit theoretical analysis in the thesis. In retrospect I think that this is one of its main shortcomings. In 1970 the thesis, called *Van vorsten en volken; een beschrijvende en functioneel-vergelijkende studie van de staatsorganisatie van vijf schriftloze vorstendommen* ('About Princes and Peoples; A Descriptive and Functional Comparative Study of the State Organization of Five Non-

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Literate Principalities') was defended at Amsterdam University. The 'principalities' examined were those of eighteenth century Tahiti, and Tonga in Polynesia, nineteenth century Dahomey, and Buganda in Africa, and fifteenth century Inca empire, chosen because of their presumably independent historical trajectories. It had then not been possible to find traces of cultural contacts or borrowing – diffusion – between the five principalities, so it seemed reasonable to assume independent developments. Later research would show that matters were more complicated. It was demonstrated, for example, that the Polynesian cases shared – in a distant past – a common origin (Claessen and van Bakel 2006d). Contacts between the Incas and the other cases are non-existent and the same holds for contacts between the African and Polynesian cases. Contacts between Dahomey and Buganda seemed improbable at that time: both principalities have a completely different background and history (cf. Connah 1987)¹.

In *Van vorsten en volken* a great number of comparisons was made, divided into three *central themes*: the ruler, the elite and the state organization, and the people. The central themes were sub-divided into a number of *topics* such as 'tasks of the ruler', 'the court', 'role of the dignitaries', and so on. In the topics I analyzed as many *aspects* as I could find. Those aspects that scored four times positive (or negative), I called *general aspects*. Of the altogether 241 aspects no less than 158 appeared to be general, which means about 65.5 %. More important was that the general aspects clustered more or less around a limited number of topics, namely: tasks and obligations of the ruler, the political organization of the principalities, tasks and obligations of the (elite) dignitaries, obligations of the people, and the like. Great differences were found in topics such as rules of succession, and position of military leaders. Clearly several aspects appeared to be constants in the principalities I had analyzed, while others showed great variation (for a discussion: Claessen 1991: 25–27).

Shortly after the defense of my thesis I was appointed an associate professor of anthropology at Leiden University, where I was to teach Political Anthropology². I remained interested in the non-literate principalities, but changed that cumbersome name into 'primitive states'. I used this designation in the paper I presented at the I.U.A.E.S. Congress held in Chicago in 1973 (Claessen 1979).

Here I met a number of anthropologists, several of whom would become involved in later years in what might be called 'The Early State Project'³.

THE EARLY STATE

The most crucial of these meetings was with Peter Skalník, from – as it was called then – Czechoslovakia. We soon detected a mutual interest in primitive states, and 'alternately drinking coffee and beer' (Claessen and

Skalník 1978: v), we considered the possibility of bringing together a number of articles on centralized polities. To quote Skalník (2004: 79):

‘The term “early state”, which I suggested to Hans Claessen instead of his “primitive state” when we first met in 1973 was a reaction to the inadequacy of the Russian Marxist term “early class state”’.

Clearly, Peter was better schooled in theories about the state than I was, so I had no objections to his ‘early’, and thus, from that moment on, we studied the ‘early state’. In subsequent meetings in Prague and Leiden we developed our plans for the book. It would consist of about twenty case studies, and some theoretical chapters, and it would be closed with several comparative chapters – in many respects an enlarged version of *Van vorsten en volken*. First we made a list of the cases we wanted to include and set out to find competent authors to write the chapters. This appeared no easy task. We were unknown to the scientific community, and, moreover, we pressed upon our authors a long list of instructions about data to be included and problems to be discussed. This list was inevitable in view of the comparisons we had in mind. Though we tried to spread our net as wide as possible, the majority of our authors were recruited from our networks; Friends, and Friends of Friends, as Jeremy Boissevain (1974) would say. Several of the experts we approached flatly declined cooperation, not being inclined to follow ‘writing directives’, as one of them stated. In this way we ‘lost’ chapters on Sparta, and Ancient Rome. Yet we succeeded in bringing together the desired number of chapters and the operation started.

We aimed at bringing together descriptions of cases of veritable early states, and with the help of the data we would try to construct the structure of early states, and look for the factors decisive for their development. Some twenty years later these decisions would give rise to the accusation of applying a simple, unilinear evolutionism (Korotayev, Kradin, de Munck and Lynsha 2000; Bondarenko, Grinin and Korotayev 2002; Grinin 2003). In a sense that reproach is correct, but when a number of early states is brought together, and it is pointed out that they all had followed a course leading to state formation this is not so much the application of a crude evolutionary model, but only a simple conclusion. More important is that it appeared that the stage of the early state could be reached along various paths.

The strategy to urge our contributors to follow as detailed as possible our guidelines (which most of them scrupulously did) made it possible for us to make extensive comparisons at the end of the volume indeed (chapter 25, Claessen 1978; and chapter 26, Skalník 1978). Chapter 25 in particular followed the approach applied in *Van vorsten en volken*. The construction of the comparisons was a delicate task, for the data came from widely varying cases, and many times – efforts of our authors notwith-

standing – they were inconclusive or vague. This situation strongly underlined the necessity of clear definitions. ‘Only if the categories are defined as clearly as possible will the risk of wrongly bringing together different phenomena be reduced to an acceptable minimum’ (Claessen 1978: 536). This approach has become characteristic for the whole Early State Project. The phenomena under study were clearly defined, and these definitions – for better or worse – were then applied in all our works following *The Early State*. They became the building blocks of our constructions (not reconstructions – one cannot really know the past). On the one hand we constructed and applied definitions to enable a comparison of the twenty-one cases in *The Early State* and in later works to characterize the various traits of new cases. On the other hand we held that when a certain phenomenon showed the characteristics mentioned in one of our definitions, then that phenomenon could be subsumed under that heading. A good example of this procedure is found in Albert Trouwborst's essay *Kende men in Afrika staten?* (‘Was the State Known in Africa?’ 1980). In it he stated that whether or not the Africans realized that they lived in a state (or a chiefdom, for that matter), when the polity in question presented the characteristics of our definition, we were justified to call it an (early) state⁴. But – as is only natural – it is all in the eye of the beholder, for the definitions are our constructs, and the same holds for the concepts and typologies we developed. They ‘are analytical tools that are related to specific theoretical views and research premises and are not inherent in the phenomena studied’ (Claessen and van de Velde 1987: 3).

The data furnished by the case studies were grouped around a number of *key concepts*, such as territory, sovereignty, stratification, functionaries, etc. In each of these *clusters* there were distinguished a number of *aspects*, from which as many *characteristics* as could be extracted from the descriptions were included in the *tables*. Incidentally missing data were added from other sources.

Where possible *structural characteristics* were indicated. To be so declared, specific characteristics had to be present in at least sixteen cases, and absent in no more than two, while in the remaining three cases they were placed under the heading ‘no data’ (Claessen 1978: 537). Many years later, in 2003, in an article by Dmitri Bondarenko and Andrey Korotayev, it was concluded after complex statistical analyses that ‘Claessen's data set is extremely valuable, and we think that further statistical analysis of it will bring new and interesting results’ (2003: 124).

With the help of the data, made accessible in 20 tables, and the processual analyses of chapter 26, we set out to formulate conclusions. As there had been a very long period of time during which no early states existed we looked for factors that had caused the emergence of the early state, and as there do not exist early states any longer we also paid some

attention to the end of the early state. This last subject was covered in only two pages, which was – certainly in the light of later publications – a rather insufficient analysis⁵.

The factors leading to the emergence of the Early State were summarized by us as:

a) population growth and population pressure; b) war, the threat of war or conquest, raids; c) conquest; d) progress in production and the promotion of a surplus, tribute, affluence; e) ideology and legitimation; and f) the influence of already existing states (Claessen and Skalník 1978: 625). The order, in which these factors appeared, as well as their intensity, could vary. At the end of the chapter we stated: ‘Summarizing the above, the existence of an ideology, as well as of a surplus, appears to have been a necessary condition for state formation. The elaboration of social inequality was found to be a consequence rather than a cause of such formation’ (*Ibid.*: 629).

Finally we distinguished three types of early states, the inchoate, the typical, and the transitional one. The inchoate early state (later we thought the term ‘incipient’ had been better) was used to indicate the first phase of the early state. This demanded to draw a borderline between chiefdom and (inchoate) early state. We suggested that the qualitative differences between them lay mainly in the development of legitimized power, ‘as manifest from (a) the power – whether consensual or coercive – to enforce the decisions of the central government; (b) the power – whether consensual or coercive – to prevent fission. These two powers, of course, are no more than the top of an iceberg’ (*Ibid.*: 630)⁶. We suggested that after the transitional early state the mature state would emerge. We neglected, however, to pay serious attention to this phenomenon; only in later publications (*e.g.*, Claessen 1991: 184–185; Bargatzky 1987) this type of polity was somewhat elaborated. We also realized that a more general discussion of the emergence of states was needed, which we set out to do in later publications. In these publications we also addressed the phenomenon of the evolution of political organizations in a general way (see below, section *After The Early State*). In this way the findings in *The Early State* were not the end of research, but rather a beginning! (*E.g.*, Claessen 2000a, 2006b.)

REVIEWS

The appearance of *The Early State* in the fall of 1978 produced a lot of reviews and comments, of which I will mention a few shortly here. Some reviews were rather positive, others were – as could be expected – less so. In *American Ethnologist* (1979: 201–202) Morton Fried expressed his doubts about the concept of the Early State as such, but praised the many interesting case studies. In *Historisch-Politike Bücher* we learned from

Eike Haberlandt's rather short review that 'Such a point of departure, aimed at the formulation of laws which are independent of place and time, of necessity will go astray'⁷. Interestingly in the *Revue française de Science Politique* a rather different view was found: 'The *Early State* will delight historians, ethnologists and political scientists alike'⁸. The next year, 1980, also brought a number of reviews. Michał Tymowski presented a long, positive review in *Przegląd Historyczny* (71: 801–807). In *American Anthropologist* (82: 426–427) David Webster pointed out that we had not included the states mainly known archaeologically, such as, for example, the Maya. He criticized the 'hap-snap' collection of states, the lack of instructions to our authors (!), and so on. He was content, however, with many of the case studies. In *Bijdragen tot de Taal-, Land- en Volkenkunde* (136: 160–162) Jarich Oosten wrote a positive evaluation, but stressed – typical for Leiden – that we had neglected the 'vision of the participants'. Jean-Claude Muller wrote in *Anthropologie et Société* (4: 181–185) a detailed and positive review of the book. Also in 1981 some reviews did appear. Christian Guksch informed us in *Tribus* (30: 278–280) that he was in general content with the book, but reproached us for the limited attention we paid to evolutionism (in which he was right). Irmgard Sellnow wrote in 1982 a lengthy review article, 'Zur Konzeption des frühen Staates' ('On the conceptualization of the Early State') in *Ethographisch-Archäologische Zeitschrift* (22: 443–452). It was a strongly Marxist based essay in which a number of sound comments were made. Finally, in 1983 there appeared a lengthy review by Ronaldo Tamayo y Salmorán in *Boletín Mexicano de Derecho Comparada*, which on the whole was very positive. It will not come as a surprise that several of the reviewers, having demonstrated their interest in problems of early states, contributed chapters to later publications.

AFTER THE EARLY STATE

In a way the reviews, summarized in the previous section, could equally well have been brought under this heading. It is, however, my intention to present in this section some of the publications in the Early State Project that followed the 1978 book. First, however, I must mention the rather informal Dutch Early State Society, which formed shortly after the first conference on *The Early State* in Leiden in 1979. This society, with shifting numbers of participants from various departments and different disciplines met more or less yearly in one of the Dutch universities, and ended a day of lively discussions with drinking beer or – to remain in style – coffee. Usually such a meeting was followed by the publication of the (reworked) papers. Of such books no less than fourteen did appear, and there were also published in the same series some related books. In these publications various aspects of early states were discussed, which formed

a welcome addition to our data set⁹. In 1995 these meetings came to an end. Several of its members had retired by then, others were heavily burdened with management tasks in the bureaucratic machinery of Dutch universities, and yet others found new research interests.

While reviews of *The Early State* were still appearing Peter Skalník and I organized a three day symposium under the title ‘The Study of the State’ during the World Congress of the International Union of Anthropological and Ethnological Sciences (ICAES) in New Delhi in December 1978. The publication of its results took quite some time, but in 1981 *The Study of the State* appeared. New names and new regions of interest were added in this volume to the Early State Project. In a review of the book by Christian Guksch in *Tribus* (1983, vol. 32: 175–177) it was remarked again that the evolutionist foundations of the project were weak. Though Guksch still was right, in the final chapter we had seriously tried, to bring more order in the theoretical framework, in particular in the six factors that we held responsible for the development of the Early State. Some of these, we reasoned, were in need of reformulation. The ideological factor gained more emphasis (1981: 479, 484), while war and conquest were reduced to more secondary roles as a corollary of economic, demographic or ideological competition (see on war and state formation especially Claessen 2006a; also Lewis 1981; Cohen 1985). Also some new variables came to the fore, among them irrigation, management, the role of trade, and endogamy. From this it became clear that the factor surplus – as formulated in 1978 – had been conceived too narrow (1981: 484). It was suggested therefore, to reformulate this factor as ‘dominance and control of the economy’, instead of surplus. In this way lower-order variables such as management, trade or irrigation could be brought under a more encompassing heading. The factor endogamy we shoved together with the whole phenomenon of kinship under the heading of ideology (1981: 484). In retrospect I think we were right in doing this, for in the last analysis kinship, in whatever form it manifests itself, appears to be rather a matter of ideology than of biology (cf. Claessen 1989; Godelier 1989, following a different approach, also considered kinship a matter of ideology).

Though we were not aware of it at that moment, the basis of the Complex Interaction Model was laid here. ‘A limited number of relevant factors in the development of Early States is selected, but not yet satisfactorily interconnected’ (Oosten and van de Velde 1994: 7). In these years in the Leiden Department of Anthropology a growing interest in evolution and evolutionism had emerged, greatly stimulated by the late Peter Kloos, the effects of which were discussed during some Dutch Early State Conferences, and followed by the publication of several volumes in which the papers were brought together (see note 5). A year at the Netherlands Institute for Advanced Studies (NIAS, at Wassenaar) made it possible for me to intensively study – with Pieter van de Velde – the problems of evolu-

tion and state formation. In *Development and Decline* (Claessen, van de Velde and Smith 1985) we constructed a general *model* of the evolution of socio-political phenomena on both the basis of our earlier findings and the contributions to that volume, which we christened the ‘Complex Interaction Model’ (CIM). The original factor ‘population growth’ appeared to be rather unsatisfactory: not only was the number of people relevant, but also the number of people in relation to the means of production and the spatial distribution of the population. We covered these complex relations with the term ‘societal format’ (format of the society), which includes the number of people, possible population pressure, and the spatial distribution of people. In view of the fact that war and conquest had been reduced to merely incidental influences, and that for a general model the influence of already existing states is too specific, we were left with three main factors, namely *ideology*, the *dominance and control of the economy*, and the *societal format*, which in a complex interaction direct the process of socio-political evolution. As also the emerging socio-political forms have a momentum of their own, this was taken as the fourth factor in the model (summary based on Claessen and van de Velde 1987: 6–7).

This approach made it clear that the development of the Early State was not a simple matter. On the contrary, early states were often the result of gradual and lengthy processes, and it was added that they also could decline or disappear. They were preceded by other forms of polities – chiefdoms, big men confederations, heterarchies and the like. And sometimes there did not develop a state, but an alternative form (Bondarenko, Grinin, and Korotayev 2002). Indeed, one might ask oneself ‘Was the state inevitable?’ (Claessen 2002) As Patricia Shifferd had made sufficiently clear: ‘continued centralization was the least common outcome in the sample at hand’ (1987: 47). Yet, early states did develop, and remained an important field of study. In subsequent international conferences we concentrated on the factors of the CIM. So we held a conference in Montréal (Canada) in 1983 on the theme of ‘The Early State and After’, which resulted in 1987 in *Early State Dynamics* (Claessen and van de Velde [eds.], referred already to above). In 1988 we met in Zagreb (then Yugoslavia) where we made the political economy the central focus, resulting in *Early State Economics* (1991, edited by Claessen and van de Velde). Based mainly upon African, American, and Polynesian cases the problems of early state governments to generate an income were discussed – and of course also how it was spent. Also redistribution, prestige goods, trade, taxation and tribute were discussed in detail. The third factor of the CIM, ideology, stood central during the international conference in Mexico City in 1993. The volume, based upon this conference, *Ideology and the Formation of Early States* (Claessen and Oosten [eds.]) did appear in 1996. Many aspects of ideology were discussed in this volume. To mention a few: the crucial role of the sacred ruler who, when not succeeding in his main obligation, producing fertility to women, cattle and land,

could easily lose his position – and sometimes also his head. Then there was the important problem of legitimacy, connected with matters of consent and coercion. The ideological character of kinship and gender was discussed. The contents of this volume underlined the crucial role of ideology in the formation and maintenance – but also in the decline – of early states.

The ideology-volume was the last of the voluminous books on the Early State I edited – together with Peter Skalník, Pieter van de Velde, M. Estellie Smith, and Jarich Oosten. Also the series of early state books in Dutch had come to an end. I retired and the possibilities of organizing substantial conferences had come to an end; the infrastructure necessary for such projects had fallen away. My major scholarly activity became the writing of articles and reviews, with as an exception, the book *Structural Change; Evolution and Evolutionism in Cultural Anthropology* (Claessen 2000a). In it I presented my views on the evolution of different socio-political forms, not only of early states. The CIM was its leitmotiv, and the components of the model each got separate chapters. Here I also introduced (inspired by Hallpike 1986) the idea of *evolutionary streams*, according to which evolutionary developments in different regions run different courses leading to different cultural systems in the formation of which ideological principles played a dominant role (Claessen 2000a: 169–185).

NEW VISTAS

In the summer of 2000 I visited Moscow, where a conference on the theme of ‘Hierarchy and Power in the History of Civilizations’ was organized by Dmitri Bondarenko and his colleagues. Here I met a number of Russian friends and colleagues, interested in problems of the Early State. Already before that meeting I had contacts with some members of this group which had formed around Dmitri Bondarenko, Andrey Korotayev, Nikolay Kradin and Leonid Grinin. These contacts had resulted in the incorporation of an article of mine on evolution and evolutionism in *Alternatives of Social Evolution*, edited by Kradin, Korotayev, Bondarenko, de Munck and Wason (Claessen 2000b). One of the papers I presented during that conference was published later in the new journal, *Social Evolution & History* (Claessen 2002). This new journal soon became one of the leading journals in the field, and several former participants in the Early State Project now found themselves publishing articles in the new journal (among them Donald Kurtz and Edward van der Vliet). In the years that followed I had the pleasure of contributing some more articles to *Social Evolution & History* (Claessen 2005, 2006b).

In between I conducted over the year 2006 a lively correspondence with Dmitri Bondarenko, who with great care, and an amazing knowledge, probed the value of several tenets of Early State orthodoxy. Among the bones of contention belonged the matter of civil servants, bureaucrats or servants (Bondarenko was supported in this question by Nikolay Kradin).

Their point of departure was the Weberian definition of bureaucracy (Weber 1964), and they held the opinion that as long there were not found real bureaucrats there was no real state. Many letters, and arguments were exchanged debating the pros and contras of these (and many other) views. The requirements of Weber were, in my opinion, too demanding to be found in *early* states. In our publications we had used therefore the term ‘administrative apparatus’, which we deemed sufficiently vague to cover the various office holders in early states. Bondarenko pointed out rightly that the term was insufficiently specific and suggested ‘incipient bureaucracy’ in its place, and stated that such functionaries should act as ‘professionals’. Meaning that regardless of the matter of loyalty to the sovereign, their position within the apparatus was decided by their capacities and not by their belonging to a certain family (it seems appropriate here to refer to Kurtz’s recent article [2006] in which he emphasizes that it is people, functionaries, who in fact ‘are’ the state; the state is not something impersonal). On this solution we reached agreement, and continued our discussions with regard to other problems, some of which were caused by the necessity to draw borderlines between *The Early State, Its Alternatives and Analogues* – as a substantial Russian publication was called (Grinin, Carneiro, Bondarenko, Kradin and Korotayev 2004). One of the problems was the definition of the Early State. After considering various alternatives, we decided upon the following, slightly adapted version:

The early state is an independent three-tier (national, regional, local level) centralized socio-political organization for the regulation of social relations in a complex, stratified society, divided into at least two basic strata or emergent social classes – viz. the rulers and the ruled – whose relations are characterized by political dominance by the former and the obligation to pay taxes of the latter, legitimized by a common ideology of which reciprocity is the basic principle (Bondarenko, in a letter of 28.09.2006).

We moreover agreed that several of the notions used in the definition might ask for annexes with clarification, such as, for example, the matter of reciprocity (*Ibid.*).

Several of the problems we discussed were also discussed in Bondarenko’s substantial book *Homoarchy: A Principle of Culture’s Organization* (2006) in which he brought together and analyzed an amazing mass of data on early states, their alternatives, and especially on Benin. A book in which he quotes more publications of mine than I can remember ever to have written...

The dominant role in Early State studies and evolutionism no longer is played in the Netherlands. Our Russian friends and colleagues have taken the lead and set themselves with great diligence at this task, producing numerous publications and organizing several conferences.

It is not only in Russia where interest in early states is found. Also in the Peoples Republic of China an interest in matters of Early States is developing. Because of political reasons the literature available was for quite some time limited to Friedrich Engels' *Origin of the Family, Private Property and the State* (1884), and Lewis H. Morgan's *Ancient Society* (1877). The more knowledge was gathered the application of the theories of these old masters became difficult, however. At the end of the 1970s and in the beginning of the 1980s the situation changed somewhat. Somehow several copies of *The Early State* arrived in China. Two of its chapters (chapter 1 and 3) were translated by Lin Zhichun, who included them in his book *Problems of City States in the Ancient World* (1985). Also during this period Elman Service's *Origins of the State and Civilization* (1975) became known there. In 1995 Xie Weiyang of Shanghai University published *Chinese Early States* in which he compared the theories of Engels/Morgan, Service and Claessen/Skalník. In 2006 my wife and I had the privilege to be invited by Yi Jianping of the Institute of World History of the Chinese Academy of Social Sciences at Beijing. Here I presented three lectures: 'When do we Speak of Early States?', 'How and Why did Early States Emerge?', and 'Further Developments, or the Early State and After'. These lectures have been translated into Chinese, and appeared recently in *Bulletin of the Research Center for Ancient Civilizations* (vol. 12, 2006, pp. 44–53, and vol. 13, 2007, pp. 61–72 and 73–82; see also Claessen 2006c). According to recent letters of Yi Jianping interest in early states and the evolution of political systems is still growing in China's academic circles.

This here seems a good point to end my summary of the checkered history of *The Early State*. In the thirty years of its existence the book has reached – and hopefully influenced – many scholars and occasioned positive as well as negative reactions. What more can a book wish for?

NOTES

¹ In a later publication (Claessen 2000a: 169–186) I proposed the idea of evolutionary streams, based on the fact that certain cultural phenomena are found only in specific regions. It seemed possible thus to distinguish separate evolutionary streams, such as a Polynesian stream, an African stream, an American stream, and so on. Cf. Hallpike 1986. Eventual similarities between Dahomey and Buganda then can be explained as remnants of a shared background in a distant past.

² See on differences between the anthropology curricula of Amsterdam and Leiden Universities: Oosten and van de Velde 1994.

³ Among them Donald V. Kurtz (then at Milwaukee, USA), M. Estellie Smith (then at Oswego, USA), Rolando Tamayo y Salmorán (UNAM, Mexico), Myron Aronoff (then at Tel-Aviv, Israel), S. Lee Seaton (then at Bowling Green, USA), and Peter Skalník (then at Bratislava, Czechoslovakia).

⁴ There is still no communis opinio on this matter. Where Michal Tymowski speaks about 'the African pre-colonial state', (2004), Peter Skalník informs us

(also in 2004: 79) ‘Why chiefdoms and kingdoms in Africa are neither states nor empires’.

⁵ See for example Tainter 1988; Yoffee and Cowgill 1988. In our later publications, Claessen, van de Velde and Smith 1985, and Claessen and van de Velde 1987 more attention to problems of stagnation and collapse was given. See also Claessen (ed.), 1992, and Claessen 2000a.

⁶ In a recent article, Peter Skalnik (2004) returns to the older custom of speaking about kingships and chiefdoms, finally bringing them all under the heading of chiefships. I am not so sure that this approach will ‘open doors to new vistas of knowledge’ (Skalnik 2004: 82).

⁷ ‘Eine solche Betrachtungsweise, die unabhängig von Zeit und Raum grundsätzliche Gesetze aufstellen möchte, muss notgedrungen auf Irrewege Führen’.

⁸ ‘The Early State passionera historiens, ethnologues et politistes’.

⁹ These books were, in order of appearance, their titles roughly translated into English: 1980. Renée Hagesteijn (ed.), *Romping with States*; 1981. Renée Hagesteijn and Edward van der Vliet (eds.), *Legitimacy or Lies?*; 1983. Henri Claessen, ed. *Territoriality*; 1984. Henri Claessen (ed.), *The Power and the Glory*; 1985 no book; 1986. Henri Claessen, (ed.) *Mighty Mothers*; 1987. Henri Claessen and Jarich Oosten (eds.), *Sources of Power*; 1988. Martin van Bakel and Edward van der Vliet (eds.), *Divide et Impera*; 1988. Henri Claessen (ed.), *Layers between Prince and People*; 1989. Henri Claessen (ed.), *Help or Hinder – on Models*; 1990. Henri Claessen (ed.), *Protection or Threat? – the Role of the Military*; 1991. Henri Claessen and Pieter van de Velde (eds.), *Discussion of Dynamics*; 1992. Henri Claessen (ed.), *The Disappearance of the Early State*; 1993. Henri Claessen, ed. *Law and Justice in Early States*; 1994, no book; 1995. Henri Claessen (ed.), *Infrastructure in Early States*. In this catalogue also fit the volumes *Evomatica* I, and II, ed. by respectively Pieter van de Velde (1981), and Peter Kloos (1984), and the volume *Sacred Kingship*, ed. by Henri Claessen (1988). All books were published by the Leiden Department of Anthropology in the series ICA Publications, with the exception of the 1991 and 1992 volumes which were published by the Leiden CNWS Press.

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