Book Reviews

- Henri J.M. Claessen, Ross H. Cordy, *A study of prehistoric social change: The development of complex societies in the Hawaiian Islands*, New York: Academic Press, 1981, 274 pp., Maps, ills., index, Appendices.
- Th. van den End, C. Guillot, *L'affaire Sadrach. Un essai de christianisation à Java au XIXe siècle*. Editions de la Maison des Sciences de l'Homme, Paris 1981. 374 pp. *Etudes insulindiennes/Archipel* 4.
- Renée Hagesteijn, A. Milner, *Kerajaan: Malay political culture on the eve of colonial rule*. The Association for Asian Studies, Monograph no. XL, University of Arizona Press, Tucson, 1982.
- P. van Hees, Emile Henssen, *Gerretson en Indië*. Boumas Boekhuis/Wolters Noordhoff, Groningen 1983. 231 blz. + los register.
- M. Hekker, H.D. Kubitschek, *Geschichte Indonesiens. Vom Altertum bis zur Gegenwart*, Akademie Verlag, Berlin, 1981. xiii + 266 pp., I. Wessel (eds.)
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- Gerrit J. Knaap, R.Z. Leirissa, *Maluku Tengah di masa lampau. Gambaran sekilas lewat arsip abad sembilan belas*, Penerbitan Sumber-Sumber Sejarah no. 13, Arsip Nasional Republik Indonesia, Jakarta 1982. XIV + 218 pp., Z.J. Manusama, A.B. Lopian (eds.)
- S. Kooijman, Tibor Bodrogi, *Stammeskunst, Vol I, Australien, Ozeanien, Afrika*, ed. by Tibor Bodrogi, 306 pp., 396 photographs (16 coloured), 13 sketch maps. Vol. II. *Amerika, Asien*, ed. by Tibor Bodrogi and Lajos Boglár, 274 pp., 238 photographs (16 coloured), 8 sketch maps. German translation of *Tözsi Művészet*, Budapest, Corvina Kiadó, 1982., Lajos Boglár (eds.)
- H.M.J. Maier, R.G. Tol, *Raja Ali Haji Ibn Ahmad, The Precious Gift (Tuhfat al-Nafis)*. An annotated translation by Virginia Matheson

BOEKBESPREKINGEN

Ross H. Cordy, *A Study of Prehistoric Social Change: The Development of Complex Societies in the Hawaiian Islands*, New York: Academic Press, 1981, 274 pp. Maps, Ills., Index, Appendices.

HENRI J. M. CLAESSEN

In the book under discussion Ross Cordy, of the Historic Preservation Office of the Trust Territory of the Pacific Islands, presents an analysis of the development of complex societies in the Hawaiian Islands. The book, a development from his Ph.D. thesis, is based on more than 10 years' fieldwork and a thorough knowledge of the literature. Over the years Cordy has published numerous articles and reports discussing aspects of Hawaiian prehistory. This book is the mature synthesis of these earlier works. It contains a general discussion of evolutionary anthropology, a number of testable hypotheses about the Hawaiian past, and his archaeological and anthropological data on Hawaii and their evaluation.

In his discussion of evolutionary anthropology Cordy follows two lines of approach. The one is a critical evaluation of the "traditional neo-evolutionists", and the other is an attempt at a more satisfying classification of sociopolitical forms. I personally have some problems with Cordy's approach to the "traditional neo-evolutionists" (p. 25ff.). In discussing only some of the works of Service and Fried he presents a picture of this "school" that is not very convincing. The said works should not be discussed separately from those of, e.g., Sahlins, Carneiro, Price, Sanders, Renfrew, and others, who all worked with similar, or narrowly related concepts. Admittedly these scholars are mentioned several times, but their views hardly receive any attention. Moreover, the summary of Service's and Fried's views does not seem well-balanced. Why say, for example, that these scholars based themselves mainly on the state/non-state dichotomy (p. 25), while even a cursory reading of their works shows that such is not the case? Fried as well as Service establish various general types of sociopolitical organization and try to indicate how these types differ from each other. This does not imply that they are unaware of the fact that not all cases fit their classification, or that they exclude the possibility of the existence of intermediate types, as Cordy seems to suggest (p. 28).

In this connection Cordy's own attempts to present a better, or more convincing classification of complex societies deserves our full attention. Even in the first chapter he proposes to classify the more complex kind of chiefdoms (as, e.g., Hawaii) together with (early) states and civilizations in one large category, and the simpler type of chiefdoms in another, lower, one. At first glance there seem to be no objections to this specific way of categorizing the phenomena in question. As social evolution can be imagined as a more or less uninterrupted sequence of minor changes culminating in qualitative differences between the beginning and the end phase, every attempt at grouping or classification is bound to be arbi-

trary and artificial, and based on the specific prejudices and objectives of the scholar concerned (cf. Claessen, Smith and Van de Velde 1984).

Cordy's proposal should be considered as an attempt at grouping his data. The theoretical considerations that are needed to provide the foundation of a *classification* are either lacking or rather meagre. Apparently the author assumes that differences between simple and complex chiefdoms can be easily established archaeologically (cf. Renfrew 1974), while those between complex chiefdoms and early states are more difficult to determine. Recently Schaedel (1984) has suggested that differences between the economy of an early state and that of a chiefdom might be used by archaeologists to distinguish between the two (cf. also Van de Velde 1984). Surprisingly, Cordy states on p. 225 that the Hawaiian polities were (early) states. This makes his whole grouping exercise rather superfluous. His additional observation that there are always borderline cases – of which the Polynesian polities are excellent examples – is correct, but hardly contributes to a solution of the problems.

In his attempts to find a better method of determining the nature of a polity than the "outdated" methods of the neo-evolutionists, Cordy first discusses the *decision-making hierarchy* as advocated by Wright and Johnson (p. 33ff.). In this approach the number of decision-making levels is taken as an indication of the degree of hierarchy in a given society. Chiefdoms are accorded two levels, early states three or more. As this approach makes use of the same categories as those of the "traditional neo-evolutionists", and no archaeological criteria for identifying the number of decision-making levels could be found, Cordy rejects the method. He does, however, retain the idea of structural differentiation in relation to societal size, making it the basic hypothesis for his further research (Cordy 1981:38).

Before following him in his attempts to disentangle the intricacies of Polynesian prehistory, a *caveat* seems in place. The hypothesis that there ought to be a positive correlation between population size and social ranking structure does not hold in all cases. For instance, the Tiv, or the Nuer, are very numerous, while their level of sociopolitical complexity is fairly low. The factor of societal size thus should be qualified with the additional observation that the correlation is only found where the population concerned forms a coherent, well-organized unity (cf. Claessen, Smith and Van de Velde 1984).

A first elaboration of his basic hypothesis is given by Cordy with the aid of Sahlins' well-known classification of Polynesian societies (Sahlins 1958). It is not clear here why Cordy leaves out Sahlins' type III, the atoll culture (Cordy 1981:39). In the analysis of the Hawaiian data this category might have been of use, as we shall see below. Moreover, there is no *a priori* reason for expecting that developments in Hawaii started at the second level, without a prior level III period. Besides, I am puzzled by Cordy's contention that Sahlins' ethnographical data were gathered mainly in the 1920-1930s (Cordy 1981:40). Most of Sahlins' data are based on journals and notebooks of early European visitors to the Pacific dating back to the last quarter of the 18th century.

Be that as it may, inspired by Sahlins' classification, Cordy tries to

evolve a classification of Polynesian societies in which the number of rank echelons, the population size, territorial size and social distance are used as criteria. All this results in Table 5 (p. 43). No explanation is given of why island societies such as those of Mangareva or Tonga (included in Sahlins' lists) are excluded. I further wonder whether the qualification "moderate social distance" really applies to the Tahitian principedoms (cf. Claessen 1978). On the whole, however, the classification is plausible. Data from Hawaiian oral tradition are used for a further refining, and in the end Cordy distinguishes (p. 45ff.):

- *simple-rank societies* (two rank echelons, less than district size, and minimal social distance);
- *complex-rank, no. 1 societies* (three rank echelons, district size, moderate social distance);
- *complex-rank, no. 2 societies* (four rank echelons, whole islands and great social distance).

The remainder of the book is devoted to the empirical testing of the Hawaii-specific hypothesis that these political types evolved consecutively in the Hawaiian Islands.

In Chapter 3 Cordy explains how archaeological findings can be linked to social variables. For this purpose he postulates that (p. 51):

"In the Hawaiian Islands at Contact (1778-1820), house construction and burial practices differed between each social rank echelon, and rank insignia and temple size differed between each chiefly social rank echelon".

For the reconstruction of the population size Cordy uses two different criteria. The one is based on cross-cultural data, assuming an average of 10 m² of roofed floor area per person, while the other is a Hawaiian-specific one, estimating an average of 6 persons per sleeping-house. Though the resultant differences in population size are not great, Cordy prefers the Hawaii-specific method, which yielded more satisfactory results for the individual communities (p. 92). To establish the territorial size of the former Hawaiian societies, Cordy looks for important boundary features such as, e.g., cairns, buffer zones, or fortifications.

Chapter 4 (pp. 111-175) contains the detailed analysis of two sets of data. One consists of data from Lapahiki, Kaloko and Anaehoomalu, the other of data gathered in North Kona.

With the aid of these data Cordy in Chapter 5 (pp. 175-184) attempts a reconstruction of Hawaii's past and tries to match the results with the hypotheses formulated in Chapter 2. The wide buffer zone between the Kona District and Anaehoomalu leads him to suppose that here two major areas were separated. The Kahala District, in which Anaehoomalu is located, appears to have had a similar buffer zone in the northeast, which separated the district from Hamakua. After ca. 1400 A.D. a rapid expansion occurred in the settlement of the buffer zone, and the formerly separated areas came to form one uninterrupted whole. Cordy interprets this development as a sign of the formation of a more encompassing sociopolitical unit (p. 181). The absence of indications in sites such as Anaehoomalu of the existence of a higher rank echelon in his view points to the circumstance of an overlord dwelling elsewhere (pp. 180, 192; also 203).

The interpretations given tend to support the known fact that in Hawaii more complex and more encompassing sociopolitical units developed in the course of time. They do not, however, exhaust the range of possible interpretations. The occupation of the buffer zone may have been occasioned by a simple population growth, and sites such as Anahoomalu may have been occupied by people possessing the social structure of atol dwellers. Another *caveat* is in place here. Cordy seems inclined to interpret his data in the light of his foreknowledge of the end results. As a consequence his conclusions remain hypotheses.

The additional data brought together in Chapter 7, which were gathered after the completion of the author's Ph.D. thesis, do not contradict the general conclusions of the book. In a number of cases more specific statements are made, as, e.g., on p. 225, where Cordy contends that the pre-contact Hawaiian polities had reached the state level (cf. Bargatzky 1984). The problem of landownership remains unclear (p. 223ff.), while the introduction of the word "feudal" does not contribute to a better understanding here. Data from Tahiti, for example, suggest that "landless" people either had claims to land that were so weak that these were overruled by those of people higher up in the ramage structure, or had land in less congenial parts of the islands, which made them prefer a position as tenants on the land of more fortunate ramage members to working their own plots (Claessen 1978).

In summary it may be concluded that, these weaknesses notwithstanding, Cordy's is an interesting book, in which sophisticated archaeological and anthropological methods are applied to Hawaiian data, resulting in a series of highly plausible hypotheses regarding the development of complex societies in this archipelago.

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C. Guillot, *L'affaire Sadrach. Un essai de christianisation à Java au XIX^e siècle*. Editions de la Maison des Sciences de l'Homme, Paris 1981. 374 pp. Etudes insulindiennes/Archipel, 4. Price c. f 50,—.

TH. VAN DEN END

The author was at one time a teacher of French in Yogyakarta (Central Java) and at present has an assignment to conduct research on Javanese history at the CNRS in Paris. For this study of one of the movements representing a transition to Christianity which originated in Central Java in the 19th century he has used Javanese and Dutch sources. He felt attracted to the Sadrach movement (which in the 1890s counted about 7,500 followers) as a product and illustration of contact between two cultures in the colonial era, and moreover because it was a religious movement, i.e. an expression of longing for radical change. He sets out to study it as a social movement, with the object of finding out why in a short space of time thousands of Javanese became Christians, and thus discovering an answer to the question of why the Javanese have changed their religion with seemingly so much ease in the course of history (6).

In the introduction and Chapter I (9-99), Guillot outlines the political, economic, social and religious context. He notes in this connection the position of the planters, who had opened up the forests, as middlemen between European and Javanese society (20f.); the role of orthodox Islam with its "Western" world-view paving the way for Christianity (25, 92); and the fact that a Western-type Christianity (Emde) held more attraction for the Javanese than the Javanese variety propagated by Coolen (61).

In chapter II (108-285), the author attempts a careful reconstruction of the events described earlier by Adriaanse and others. His description focuses on "the affair", that is, the relation between Sadrach and the Europeans. He sees Sadrach as the middleman mediating between the European rulers and the poor Javanese peasants who had become his followers (148f., 390ff.). As such, he protected his people against the vexations of oppression by the indigenous *elite* and even against the European authorities. He further founded schools and gave his following a Western-style organization. At the same time, as a "village-founder" and a *guru ngèlmu* (religious teacher), he conformed to Javanese patterns. However, he did not lead his peasants into a hopeless *jacquerie*, but at their head met the Europeans as an equal. This attitude was at the same time the cause of the long series of conflicts. The Europeans, least of all the missionaries (with the notable exception of Wilhelm), were far from ready to accept a Javanese as their equal. Their refusal to let Sadrach sit on a chair is symbolical of their eagerness to

replace him as chairman of the Javanese Christian congregations. In 1891 an emissary of the Dutch Churches completed the process of estrangement: from then onwards there was no possibility of Sadrach's cooperating with the official Dutch missions, while the Dutch Reformed Churches (themselves the product of a secession) were starting their own missionary work with a large deployment of forces in the field of evangelization, education and medical care.

In the third chapter, Guillot summarizes the results of his researches. His exposition is full of ironies, depicting a colonial government that was more tolerant towards this independent Javanese religious movement than the Protestant clergy and the missionaries (and the established Church in turn more so than the missions); the attempts of the Dutch Reformed Churches, having themselves separated from the established Church of the Netherlands some years before to become a Free Church, to bring under their control a movement which, under the name of Javanese Free Church, had beaten off attempts of the local established Church officials to annex it; a so-called rebellious Sadrach being prepared to the very end to cooperate with Dutch people who showed him respect (even after 1891); and, the most bitter irony of all, Sadrach's people leaving him *en masse* when the Dutch missionaries, who were Sadrach's enemies, appeared to be more effective "middlemen" than their old leader and their schools better than his.

Guillot's study gives the reader much opportunity to reflect on the twists of colonial history. The book is remarkable for its extremely objective tone, writing history in a matter-of-fact way without moralizing, even about the missionaries. It is one of the better products of the modern French historical school. Unfortunately it lacks an index and an English-language summary (or an Indonesian one, for that matter).

A. Milner *Kerajaan; Malay Political Culture on the Eve of Colonial Rule*. The Association for Asian Studies, Monograph no. XL, University of Arizona Press, Tucson, 1982.

RENÉE HAGESTEIJN

The author sets out to use native historical sources (*hikayats*) instead of European colonial sources for explaining nineteenth century Malay political events. In his opinion European models of Malay political developments have limited explanatory value. 'I wanted to understand Malay political activity in Malay terms . . . The structure of this book records the process by which I became disillusioned with European sources and turned to Malay writing' (p. xiii). And: 'The present work explains why and how I explored Malay writings in order to discover Malay perceptions of their political motivation. . . . The book is a case study in *political culture* [my italics]' (p. x).

The book is divided into eight chapters. It is based on two historical case studies: the rise and expansion of the Sumatran state of Deli and a civil war in the Malay state of Pahang.

The author is struck by the fact that nineteenth century Europeans tend to view Malay political motivation in economic terms. In an obvious colonial framework, the attention is focused on fertility of the soil, natural resources, possibilities of exploitation of products such as pepper and tobacco, commercial monopolies, piracy and smuggling. Milner asks whether this is the correct approach for studying political activity. In his opinion: '... indigenous correspondence and literature throw a different light on the perspectives of the rulers whom the Europeans thought they understood' (p. 18). The concept of monopoly does not appear in indigenous legal papers. Besides, in Malay terms there is little difference between trade and piracy. It is obvious that different perceptions are involved. According to Milner, *rajas* (rulers) were not concerned to distinguish between commerce and 'piracy' because, as we shall see, trade was not their object. The Malay *rajas*, as becomes apparent from the Malay writings, were not concerned with commerce but with wealth. The way in which this wealth was obtained, be it through the use of force, 'legitimate trade', monopolies, or even through gambling or magic, was a matter of relative indifference. The accumulation of wealth was not an end in itself, but wealth was used to attract and keep political followers. The position of the *raja* was dependent on this accumulation of wealth. Wealth among other Malays was discouraged, because this might threaten the position of the *raja*. Therefore 'non-royal' trade was in the hands of other ethnic groups, such as Arabs and Chinese.

In chapters three to five indigenous written sources (the *Hikayat Pahang*, *Hikayat Deli*) are analysed to illustrate this point. One of the themes in the *Hikayat Pahang* is that of the ruler, the *raja*, giving presents to those who join him and admire him. According to Milner, this royal gift-giving presents an important clue to the nature of Malay political culture. The gifts of the *raja* hardly have economic value, and their purpose is first and foremost *ceremonial*. Milner suggests that the role of the *raja* himself 'is ceremonial rather than practical' (p. 47). It also appears from 19th century European sources that Malay courts are 'obsessed' with formalities. These ceremonies are only considered to be effective if performed according to strict rules. 'The ruler's duties, then, were concerned with etiquette, and he was expected to exemplify the highest norm of the Malay culture; ideally he was the most cultured of men. Refined manners, therefore, are a requirement for a just and good ruler as they imply a knowledge of customary arrangement ...' (p. 51). *Real* power was in the hands of the 'district chiefs'. 'The chief was responsible for such hard facts' of government as local administration, justice, defence, revenue collection and general leadership' (p. 51). According to Gullick – a modern observer of the 19th century Malay world cited by Milner – the chief, and not the *raja*, is the key institution in the political system.

In chapter six the conclusion is drawn that the *raja* was the 'organizing principle of the Malay world' (p. 94). Milner here discusses the meaning of the *raja* for his Malay subjects. Service to the *raja* can influence a subject's *nama* or reputation. 'The interaction between the subject and the *raja* provided both participants with opportunities. The subjects were able to improve their *nama*, or position in this world and the next.'

Reward in this world and the next depended on the devoted service they offered their *raja* (p. 108). Social mobility was linked to this *nama* system, in which titles and ceremonies played important roles. They were no 'empty rewards', as has been suggested by European observers. Without the institution of the *raja*, the Malay world would have fallen into confusion. There would have been no meaning to political action.

In the last chapter, the Conclusion, the findings of the book are summarized, with an explanation of the concept of *kerajaan* as 'the condition of having a raja', rather than a state or a government. Once Western colonizers penetrated the Malay world, the *kerajaan* system eroded and was replaced by a western-style government.

With his book Milner makes an important contribution indeed to the understanding of Malay culture. Although it is curious that he omits to mention the work of the Dutch Leiden School, he has a point when he complains that indigenous sources like the *hikayats* have been insufficiently used in historical and anthropological analyses.

It is to be regretted that the author does not give a clarification of his frequently used concept 'political culture'. Besides, I am not so sure that the author has really succeeded in giving a picture of Malay political culture from the Malay point of view, in the way he promises to do in his preface (p. x). After all, he has made a selection from the sources for his own interpretation. And he uses a functionalistic approach rather than a cognitive one in his analysis. Therefore we are still confronted with a historian's rather than a participant's model.

On the other hand, it might be argued that Milner's presentation of the Malay political culture is not typically Malay. Recent publications in the political anthropological field have revealed that patron-client relations between rulers and ruled, legitimized by symbolic and ceremonial redistribution and gift-giving, are common characteristics of so-called Early States (cf. Claessen & Skalnik 1978, 1981). They cannot be defined and described in Western terms (a problem that Milner deals with throughout his book), because they depend on personal relations, with the ruler as focal point, and they lack territorial awareness and a-personal bureaucratic institutions. Quite possibly the nineteenth century precolonial Malay political structures as described by Milner can be viewed as Early States.

The overall impression is that although his point of departure and the questions raised by him are commendable, Milner becomes bogged down in a comparison between western and local sources. He pushes his argument on the opposition between the nature of western and indigenous sources too far, at the expense of his actual analysis of Malay political culture. An issue that could have been dealt with in the Preface, is too dominant in the remainder of the book.

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Emile Henssen, *Gerretson en Indië*. Bouma's Boekhuis/Wolters Noordhoff, Groningen 1983. 231 blz. + los register, prijs f 32,—.

P. VAN HEES

Er valt de laatste tien jaren een toenemende belangstelling waar te nemen voor de Utrechtse hoogleraar en letterkundige dr. F. C. Gerretson ((ps. Geerten Gossaert) 1884-1958). In het begin van de jaren zeventig verschenen zijn *Verzamelde Werken* (letterkunde en geschiedenis) en in de jaren tachtig zijn *Briefwisseling* met zijn vriend en collega prof. dr. P. Geyl en in 1984 met zijn vriend, de dichter prof. mr. P. N. van Eyck.

Terecht werden leven en werk van deze "koopman, dichter, historicus en politicus" ook onderwerp van verder wetenschappelijk onderzoek. J. de Gier schreef in 1982 een dissertatie over Geerten Gossaerts *Experimenten* en in 1983 promoveerde Emile Henssen in Groningen op de dissertatie *Gerretson en Indië*.

Nederlands Oost-Indië heeft in Gerretsons leven een grote rol gespeeld. Als hartstochtelijk nationalist zag hij het koloniale rijk als een onlosmakelijk met het moederland verbonden deel van het Rijk en onmisbaar om deszelves grootheid te handhaven. Tijdens zijn leven heeft hij ook daadwerkelijk met de Oost te maken gehad. Hij was ambtenaar op het Departement van Koloniën (1913-1917) en directie-secretaris bij de Koninklijke Shell (1917-1925), waar hij o.a. te maken had met het Djambi-contract; van 1925 tot 1951 was hij bijzonder hoogleraar aan de Indologische faculteit te Utrecht en last but not least van 1939 tot 1954 buitengewoon hoogleraar in de constitutionele geschiedenis van het Rijk in Europa en overzee. Tenslotte heeft hij zich ook als politicus in het interbellum en na 1945 niet onbetuigd gelaten inzake de strijd tegen de herziening van de Indische Staatsinrichting in de jaren twintig en, na de oorlog, de liquidatie van het Imperium en de vormgeving van onze verhouding tot de West.

In een aangenaam leesbaar geschreven werk heeft Henssen ons een beeld gegeven van Gerretsons activiteiten rond de betrekkingen tussen Nederland en Oost-Indië. Pièce de résistance is Gerretsons intensieve bemoeienis met de oprichting van het Fonds voor Indologische Studiën aan de Rijksuniversiteit te Utrecht. De in 1925 gestichte bijzondere leerstoelen moesten dienen als tegenwicht voor de zgn. "ethische richting", die in Leiden verkondigd werd en die, zo dacht men in behoudende en commercieel ingestelde kringen, al te snel tot de volledige onafhankelijkheid van de Oost zou leiden.

Over de strijd tegen Leiden en over de oprichting van de Utrechtse tegenhanger, waarbij ook M. W. F. Treub een grote rol speelde, geeft Henssen belangwekkende informatie. De spottend genoemde "olie-faculteit" dreef op heel wat minder oliekapitaal dan men wilde geloven. De Koninklijke Shell was zelfs niet eens een voorstander van deze faculteit.

Over de betrekkingen tussen Nederland en Indië had Gerretson ook zo zijn ideeën. In Indië wilde hij federalisme invoeren en geen unitair staatsbestel scheppen. De Volksraad was in zijn ogen een onding. Het

gaf halve democratie en dat werkte niet. Hij trok dan graag een parallel met de Nederlands-Belgische verhouding in 1830. De band met Nederland diende zo lang mogelijk gehandhaafd te blijven, al zag hij wel als einddoel een politiek zelfstandig Indië. Deze conservatieve standpunten heeft hij met verve verkondigd en, meeslepend redenaar en scherp polemist als hij was, in woord en geschrift verdedigd. Henssen heeft deze activiteiten, de oprichting van de bijzondere faculteit, de strijd tegen de herziening van de Indische Staatsinrichting, de professorale taken in Utrecht en de strijd tegen de soevereiniteitsoverdracht na 1945 bestudeerd en er verhelderende opmerkingen over gemaakt en de nodige mythen opgeruimd.

De keuze van het onderwerp "Gerretson en Indië" brengt echter beperkingen met zich mee. Het is niet de meest succesvolle activiteit van Gerretson geweest, al is de oprichting van de Utrechtse Indologische faculteit geen geringe prestatie geweest, en zo'n ingeperkte optiek leidt gemakkelijk tot enige vertekening. Henssen is er niet geheel aan gekomen Gerretson af te schilderen als een geniale zonderling die eenmaal op zijn strijddros gezeten er niet voor terugschrok om zijn tegenstanders met soms weinig fraaie middelen te attaqueren. Het beeld is m.i. te negatief gekleurd, ook al heeft Gerretson door zijn polemieken en door zelf veel in dissertaties te schrijven of ze in een enkel geval geheel zelf te schrijven het beeld van een niet geheel au sérieux te nemen hooggeleerde mee opgebouwd.

Niettemin mogen zijn wetenschappelijke prestaties er zijn en ondanks niet-voltooiden plannen, heeft zijn oeuvre hem een bijzondere plaats onder de Nederlandse historici bezorgd.

Een enkele kritische opmerking nog.

Het is jammer dat de schrijver de controversie Utrecht-Leiden niet wat meer heeft uitgewerkt. Waren de verschillen werkelijk zo groot en verschilden de opleidingen van de bestuursambtenaren in spe wezenlijk van elkaar?

Op blz. 40 schrijft Henssen dat Gerretson in 1922 al schriftelijk tegen Leiden ageerde, maar dit blijkt nergens. Is dat een verschrijving voor 1923? Ook over het Djambi-contract, waar Gerretson op koloniën en bij de Shell aan werkte, worden we nauwelijks ingelicht, terwijl Gerretson er in zijn *Geschiedenis der "Koninklijke"*, dl. V, p. 204 e.v., uitvoerig over schrijft, ook over zijn persoonlijke betrokkenheid, waar Henssen op blz. 31 zonder bewijs aan te voeren wat schamper over schrijft.

Ook over de relatie tussen Gerretson en Colijn worden we weinig gewaar. Er is tussen hen beiden over Indië toch vrij wat contact geweest en in de brieven van Gerretson aan Colijn, waarvan niet blijkt dat de auteur deze heeft geraadpleegd (Universiteitsbibliotheek Vrije Universiteit, Amsterdam), moet toch wel wat te vinden zijn. Hetzelfde geldt voor Gerretsons activiteiten in de jaren twintig tegen de herziening van de Indische Staatsinrichting. Hoe heeft hij de CHU-fracties in de Tweede en Eerste Kamer bewerkt? Toch niet alleen via B. C. de Savornin Lohman. Naar aanleiding van Gerretsons na-oorlogse politieke artikelen over het Indië-beleid van de regering in bladen als *Ons Vrije Nederland*, *De Nieuwe Eeuw* en *Nieuw-Nederland* zou wat meer

informatie over redactie en achterban van deze bladen ook welkom geweest zijn.

Op blz. 120 stelt Henssen dat tussen de Ondernemersraad voor Nederlands-Indië en de Utrechtse Indologenfaculteit na 1945 geen contacten zijn geweest. Formeel misschien niet, maar informeel hadden Gerretson en bijv. G. A. Ph. Weyer wel contacten met die kring. (Vgl. *Briefwisseling Gerretson-Geyl*, dl. V, brieven 934 en 938.)

De kritiek op Gerretsons houding aan het begin van de Duitse bezetting in Nederland en zijn streven naar een NSB-loze regering die met de Duitsers zou moeten onderhandelen, zou gewonnen hebben, wanneer deze kwestie tegen het licht van de gebeurtenissen in België en Frankrijk geplaatst zou zijn. Hetgeen niets afdoet aan de door mij met de auteur gedeelde mening, dat het geen gelukkige gedachte van Gerretson is geweest.

Is de op blz. 162 genoemde De Loor, die daar niet te plaatsen is in Gerretsons fulminaties tegen "Romeins *De Eeuw van Azië* en de masochistische 'exhibities' van De Loor", niet een verschrijving van Gerretson voor Van Leur, wiens werk na 1945 bekendheid kreeg?

Uit de door Henssen geraadpleegde briefwisseling Gerretson-Van Eyck (brief van 9-8-1945, Letterk. Mus., Den Haag) zou nog te vermelden zijn dat Gerretson in 1945 een ogenblik met de gedachte gespeeld heeft om in Leiden te gaan doceren als opvolger van H. J. Krom. Zag hij zichzelf een moment als een soort wrekende engel? Immers Leiden en de Leidse liberale historici konden in zijn ogen geen goed doen. Colenbrander was zijn "bête noire", ook al was deze, zoals Henssen aantoonde (blz. 62), helemaal niet zo "ethisch". Trouwens, in Gerretsons ogen hadden de Nederlandse historici altijd een negatief beeld van het beleid van Nederland in de Oost verkondigd. Ter illustratie van deze ook door Henssen geconstateerde zienswijze van Gerretson wijs ik op de volgende passage in *Briefwisseling Gerretson-Geyl*, dl. IV, p. 128, waar Gerretson schrijft: "Het is opvallend hoe vele geschiedschrijvers uit het begin en midden v. d. vorige eeuw, of remonstrant, resp. doopsgezind zijn, of tot de anti-gereformeerden in de kerk behoorden; en het zou nuttig werk zijn eens na te gaan, hoe die gezindheid in de koloniale geschiedenis b.v. bij den remonstrant Tiele, het oordeel heeft beïnvloed".

Samenvattend moet het oordeel luiden: een leesbaar boek, dat ons Gerretson, zij het soms wat eenzijdig belicht, nader brengt, maar dat helaas ontsierd wordt door vele slordigheden en drukfouten, die in de 3½ blz. errata niet alle uitgewist worden.

H. D. Kubitschek, I. Wessel, *Geschichte Indonesiens. Vom Altertum bis zur Gegenwart*, Akademie Verlag, Berlin, 1981. xiii + 266 pp., DDR 11.80 M.

M. HEKKER

The object of the authors of this East German publication is to present a brief outline of the history of Indonesia. Under the restrictions of the

modest scope of the book they confine themselves to the main events and developments. Their special aim is to describe the making of the present Republic of Indonesia, while giving attention to the specific geographic and ethnographic features as well as referring to the wider context of world history. The book is for the greater part devoted to the period from the beginning of the 20th century onwards, so that this "Geschichte Indonesiens" is in fact a history of modern Indonesia.

In chapters I-VI Kubitschek deals with the period up to about 1900. He describes the centralized empires of Java and Sumatra, the coming of the Portuguese and the Spanish followed by the Dutch, and the establishment and development of the Dutch colonial empire. Wessel next, in chapters VII-XII, describes the rise of national resistance against Dutch supremacy, the situation under the Japanese occupation, the attainment of independence, and the Sukarno era. The book concludes with the coup of 1965.

As this summary of its contents already indicates, the core of the book consists of the chronological description of the arrival of the European powers, the building of the colonial empire, the Indonesian national resistance and the ultimate achievement of Indonesian independence. In short, the central theme is European expansion and the indigenous response to it. Moreover, somewhat more light tends to be thrown upon the European side of the question, while additionally the most attention has been paid to the political history, and to boot that of Java and Sumatra. To some extent all the events are viewed from the perspective of the eventual independent unitary state. In these respects the book does not differ fundamentally from similar general surveys of the history of Indonesia.

The factual description is supported by a historical-materialistic analysis. The successive stages of world history constitute the wider frame of reference for events and developments in Indonesia, while politics are considered as being determined to a high degree by social and economic factors. The nature of production relations and the related class structure in a society at a given point of time provide the model of explanation for historical phenomena. According to the authors, the imperialistic phase of Dutch colonialism entailed a forced introduction of capitalistic relations in a largely pre-capitalistic society. A balanced process of class differentiation failed, however, to proceed and the uncompleted class formation explains the fragmentary character of the Indonesian national resistance and, later after that, the internal political problems following independence.

Unfortunately, the reader finds himself left in the dark on this point. Although the authors seem at first glance to be employing a clear-cut and unambiguous class scheme, it is in fact not made wholly clear what is meant exactly by "class formation" in the specific Indonesian context. Although it is indicated that since the end of last century the simple opposition between aristocracy and peasants has become more differentiated, precisely which classes have come into being, by whom they are made up and how they are mutually related remains a mystery. Pointing at the "uncompleted" character of this class formation process of course offers no real solution to the problem.

Sometimes the authors' approach leads to interpretations which are not altogether beyond criticism, and generates differences in emphasis in comparison with the existing literature in this field. An example is the case of the so-called Java War of 1825-1830. This uprising, led by Prince Diponegoro, is explicitly depicted by Kubitschek and Wessel as a popular insurrection of peasant masses against the exploitation of the Dutch colonial system. However, this revolt is viewed by most authors as the final flare-up of resistance among the Javanese aristocracy against Dutch rule.

Notwithstanding these divergent interpretations and notwithstanding the one-sidedness and limitations which – necessarily – characterize general surveys, the authors have succeeded in presenting a clear, sound outline of the history of modern Indonesia. They not only describe the main events and development in a systematic and readable way, but also manage to integrate these into a coherent whole.

W. R. Smyser, *The Independent Vietnamese: Vietnamese Communism Between Russia and China*, Athens, Ohio: Ohio University Center for International Studies, Southeast Asia Program, 1980, 143 pages, bibliography. No price indicated.

DR. HUỖNH KIM KHANH

During his lifetime Ho Chi Minh often reminded his younger comrades: "Our relationship with our friends is a thousand times more difficult than the one we have with our enemies". This book, written by an American foreign service officer, amply illustrates the late Vietnamese leader's caution. The book examines the complex Vietnamese relations with the Soviet Union and China during a difficult period of Communist international relations — from Khrushchev's "secret speech" in 1956, which signaled the beginning of the deterioration in Sino-Soviet relations, until the outbreak of the Sino-Soviet armed border conflict in 1969, a year which also witnessed Ho Chi Minh's death. This, too, was a difficult period for internal Vietnamese development — one in which the country needed all the external help it could obtain for consolidating the social and industrial base in the North while satisfying the drive to complete the reunification of the country; the latter demand led to a confrontation with the U.S.-sponsored regime in the South, and ultimately the United States itself.

In spite of minor errors in historical interpretation, Smyser's book is a useful addition to our knowledge of Vietnam's foreign relations. Applying "content analysis" in examining published Communist sources, the author leads readers along the principal turning-points in Vietnam's "straight zig-zags", as Bernard Fall would have called them, in the Sino-Soviet dispute. He correctly grasps the essential problem which Vietnam faced in its relationship with the two Communist giants in those years, viz: how to maintain Vietnam's freedom of action and protect its consistent national interests in an inconsistent

and troublesome international environment. Dismissing the simplistic analysis of many Western experts on contemporary Vietnam, which posits "riftism", a belief in the division of the Vietnamese leadership into "pro-Chinese" and "pro-Soviet" factions, Smyser insists that Vietnam never took a "neutral" stance in the Sino-Soviet conflict. Rather, it had assumed an "independent" position which protected the Vietnamese Party's standing in the Communist world as definer of Marxist-Leninist orthodoxy, arbiter in the dispute between the Soviet Union and China, and at the same time defender of Vietnam's national interests — national independence and unity. Smyser has rendered a valuable service to students of Vietnamese and Communist international relations.

Sandew Hira, *Van Priary tot en met De Kom. De geschiedenis van het verzet in Suriname, 1630-1940*, Rotterdam: Uitgeverij Futile, 1982

SASKIA KELLER

Sandew Hira is a young, zealous and very prolific writer. An economist by origin, he has widened his horizons far beyond the traditional boundaries of his trade. *Van Priary tot en met De Kom* is his first major work and faithfully reflects the precedence that political activity takes over scholarship in his mind. The book sets out to present a history of resistance by the oppressed "masses" against capitalist exploitation in Surinam over a period spanning more than three centuries. The drama begins in 1630 with the first extended colonial enterprise in Surinam, the settlement of Captain Marshall and sixty Englishmen in the interior, and ends on the eve of the Second World War, which announced the definitive demise of plantation society. Resistance is an intriguing subject, which has enjoyed ample attention in Caribbean historiography over the past decades, but Hira is not bothered by comparative inclinations. Only by the bare fact that he employs a Marxist frame of analysis does he tie in with recent tenors in this field.

The main concerns of the book are basically political. Hira states that there exists a profound need for a history and analysis of the unending struggle against repression and exploitation in Surinam, which need his work aims to meet. Secondly, he strives to pass on the lessons of the past to today's young revolutionaries. And lastly, he hopes to stimulate the development of a Marxist tradition in Surinam and the formation of a new vanguard for revolutionary action. Hira claims that on the historian rests "the ungrateful task to construct the network of cause and consequence in the drift of events in such a way as to fit every deed, every success or every failure, and every mistake necessarily in with the logic of the historical process which he is analysing" (p. VIII). *Van Priary tot en met De Kom* presents in addition a critique of the "bourgeois" interpretation of the history of Surinam, especially of the kind elaborated by the so-called pluralists, exemplified by R. A. J. van Lier. In Hira's opinion, the theory of

pluralism cannot explain the crucial developments in Surinam history. It fails most conspicuously when trying to illuminate the heroic fight of the masses against capitalism. Hira is convinced that his brand of revolutionary Marxism can supply a more consistent and revealing analysis of this struggle. He therefore applies these insights with unflinching partisanship to the whole range of insurgent actions in Surinam.

The book opens with a critical evaluation of the famous study by R. A. J. van Lier, *Samenleving in een grensgebied* (1949). Although Hira considers this work still valuable — a bright lantern in an obscure labyrinth — after 33 years, he has little regard for its theoretical foundations. In his view, they consist of a “hotchpotch of contradictory concepts”, the most sophisticated of which represent little more than an idealistic misconception: the perception of Surinam as a pluralistic society, a state based on the consolidation of ethnic differences. He believes that the theory of pluralism links socio-cultural developments with shifts in the motivations and aspirations of the respective races. He concludes that this theory (a) does not adequately explain social differentiation, and (b) lacks internal consistency. He is particularly critical of the — admittedly unfortunate — fondness of Van Lier for bestowing facile psychological labels upon historical personages. Van Lier, Hira claims, can interpret the resistance of the masses only as sudden outbreaks of hostility on the part of groups of people collectively exceeding their “frustration threshold”. Why then, he asks, do the masses revolt “regularly”? Obviously, the history of resistance in Surinam was in dire need of revision.

The bulk of the book is taken up by detailed descriptions of the great battles against the “monster of capitalism”: the Indian war, the (convict) soldiers’ mutiny; the guerilla of the Maroons, especially the Boni war; slave revolts; the ‘Koeli’ strikes; the Killinger conspiracy; and the struggle for union rights. The chapters on these subjects are preceded by a theoretical analysis of the development of Surinam colonial society in relation to the world economy. Hira distinguishes three principal phases in the evolution of production relations (and, parallel to these, three phases in the development of the centralized state): (1) the foundation of the colony (the period up to 1688); (2) the rise and fall of slavery (1688-1863); and (3) the disintegration of the plantation society (1863-1940). The first period was shaped by the contacts between trade capitalism and the pre-capitalistic, classless Indian communities. The second period saw incorporation of the colony into the world economy through the production of commodities for the international market, while the profits from this were accumulated in the Mother Country. The agrarian labour was performed by black slaves, which defined the nature of the class struggle in this era. No form of organization of the workers could be tolerated, since this implied a threat to the proprietary rights of the masters and an undermining of the essence of the slavery system. The racist ideology functioned as a justification for exploitation and at the same time, by imbuing the masses with an awareness of their inferiority, discouraged

resistance. Surinam, in this period, constituted a "segmented state", characterized by a weak and often impotent central government and a politically vocal planter class. During the third period, differentiation of production relations followed as slavery gave way to contract and wage labour, independent peasant farming and prospecting. Only after the extirpation of slavery was it possible for a genuine centralized state to develop. As ethnic and class boundaries coincided, the class struggle was shaped by the clashes between the various ethnic groups. However, the expression of class conflicts along racial lines can be seen as a "necessary part" of the general struggle, and Hira fervently hopes that the masses of Surinam will eventually come to see the light and unite against the common foe.

Each stage in the historical process was accompanied by certain forms of resistance of the oppressed. The ruling elite was endangered by the revolts of the Indians and soldiers in the first period, by the passive and active opposition of the slaves and the guerrilla warfare of the Maroons in the second, and by the fight for labour rights and the Killinger plot to overthrow white supremacy in the period after emancipation. Hira staunchly proclaims the revolutionary fervour of the masses, whom he considers as ever ready for action when the circumstances permit. The prospects for revolution are determined by various factors, among them the severity of exploitation, internal divisions in the master class, and the existence of an economic basis for a sustained struggle. These factors explain why certain groups in Surinam society rose in rebellion at certain times. Hira does not apply these rational considerations when referring to people who chose not to enlist for the holy war, however; they are dismissed as "scabs and traitors", or at the very least as mean cowards.

In many respects, the book adds to the existing knowledge of the history of Surinam. The descriptive chapters in particular brim with interesting information, much of it hitherto undisclosed. But there are several serious flaws which greatly detract from its value. To begin with, Hira repeatedly barks up the wrong tree in his criticism of the work of Van Lier. He errs in assuming that for Van Lier the theory of pluralism "explains" the historical development of Surinam society. Van Lier indeed does not employ a consistent theoretical framework, but neither does he use a hotchpotch of contradictory concepts. He views Surinam from different angles: it can be regarded not only as a *plural* society, but also as a *colonial* society and a *frontier* society. These perspectives are little more than "devices for heightening perception". While in his analysis of contemporary society, Van Lier leans heavily on the concept of pluralism, in his interpretation of the development of the slavery system, he uses the notion of a *frontier* society, with all its implications for the mentality of its inhabitants, to much greater advantage. Van Lier's analysis of slave resistance is largely couched in psychological terms, but he incorporates references to economic, geographic and demographic factors. Consequently, his historiography is less blatantly "mentalistic" than Hira would have us believe. In fact, it often displays more dialectical subtlety than Hira's mechanistic way of reasoning.

Hira has not conducted a thorough investigation in the archives. He depends largely on a number of well-known books and some easily available documents. He therefore overlooks primary sources which are vital for his theory. He based his description of the soldiers' mutiny on the work of Pistorius (1763), for example, instead of using the eye-witness accounts contained in the archives of the Society of Surinam. His interpretation of the data displays a tendency towards inconsistency, since he strives to show the inevitable logic inherent in the historical process. His argumentation is further colored by a rigidly orthodox Marxist view. Hira's basic framework of economic phases has dictated the scheme of the book and the way the material is analysed. His expositions on the roots of the revolutionary struggle have a somewhat archaic flavour: they remind one more of C. R. L. James than of Eugene Genovese. The book would have benefited if Hira had taken better notice of the more sophisticated studies on this subject which have appeared recently, such as Aya (1979), Genovese (1979), and Skocpol (1976). Hira is most convincing when he restricts himself to analysing the economic developments proper; the precise relations between these developments and the expression of resistance never become clear.

The book gives the distinct impression of being unpolished: a "second draft" published with undue haste. This has resulted in often superfluous repetition and weak composition. There are several annoying mistakes which make one suspect that Hira is more intent on proving his point than on gaining an insight into the history of Surinam. His haste shows in the misspelling of names, the lack of uniformity in the spelling and the frequent use of inane metaphors (the book features a touching scene in which the "struggle bug" mates with the "resistance virus", with momentous results). The cover, depicting in garish colours a hideous capitalist (complete with monocle) gnawing at Surinam does justice to the often Caucasophobic content, but will undoubtedly scare away many serious readers. Furthermore, the book falls apart at the slightest provocation.

In the final analysis, *Van Priary tot en met De Kom* amounts to little more than a catalogue of heroes, skilfully excavated from among the debris of history. However, Hira does deliver what he promised in the introduction, and this perhaps should be the ultimate yardstick by which to judge an author whose preoccupations one does not share. So budding revolutionaries looking for inspiration are well advised to hurry to the bookshop, but scholars interested in the history of Surinam had better turn elsewhere.

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R. Z. Leirissa, Z. J. Manusama, A. B. Lopian, Paramita, R. Abdurachman (eds), *Maluku Tengah di Masa Lampau. Gambaran Sekilas Lewat Arsip Abad Sembilan Belas*, Penerbitan sumber sumber sejarah no. 13, Arsip Nasional Republik Indonesia, Jakarta 1982. XIV + 218 pp.

GERRIT J. KNAAP

The editors of this source publication may justifiably be considered to be Indonesia's leading scholars in the field of the history of the Moluccas. The present volume is a new testimony of their fruitful cooperation. It presents a selection, one could even say an "anthology", of historical documents for the period 1811-1868, deposited in the Amboina section of the Arsip Nasional Republik Indonesia, Jalan Gajah Mada 111, Jakarta. The documents have been arranged in ten chapters according to subject, covering amongst others village government, military affairs, education, and so on. Moreover, the volume contains a general introduction, a short bibliography and a glossary. Unfortunately, however, there is no index.

Every separate chapter is opened by the editors with an introduction on the subject-matter of that chapter. In it the most important historical processes and events are described to the readers in a very careful and instructive way. Much attention is given to continuities and/or discontinuities with the preceding forms of colonial government, those of the English interregnum and the V.O.C. Subsequently, every document is presented with a few introductory lines. The transcription of the texts, most of them written in Dutch, others in Moluccan Malay, has been done very carefully.

This selection of sources indicates the rich potential of this particular section of the Arsip as a source of historical information and as such provokes the reader's curiosity. With the exception of the years around 1817, the period of the war between Thomas Matulesia alias Patimura and the Dutch, the nineteenth century is still a virgin era as far as historical description is concerned. In this respect the volume could function as a guide for future research. However, the reader is left in the dark on one point – is this selection a representative one or is it a presentation of the most interesting documents? In other words, the

editors would have done well if they had more explicitly indicated the principles guiding them in their selection.

Nevertheless, the documents include several which are of outstanding interest, for instance, the colourful description of the meeting of the *Saniri Tiga Air* in West Ceram in 1842. As far as I know, such a description has never been published before. Other interesting documents, to mention only a few, are the report on the actions of Ulupaha, the Hituese ally of Patimura in 1817, the fragment of the *Landbouwverslag* of 1863, and records of the conference of 1859 between the colonial government and the chiefs of the villages of the Lease islands concerning the intended abolition of the government's monopoly on the cultivation of cloves.

With the publication of this selection of sources the editors' aim has been to illustrate the importance of archival material from the period of the Dutch Indies administration for writing the history of several Indonesian *daerah*. In my opinion, they have succeeded very well in this purpose.

Stammeskunst, Vol. I. *Australien, Ozeanien, Afrika*, ed. by Tibor Bodrogi, 306 pp., 396 photographs (16 coloured), 13 sketch maps. Vol. II. *Amerika, Asien*, ed. by Tibor Bodrogi and Lajos Boglár, 274 pp., 238 photographs (16 coloured), 8 sketch maps. German translation of *Tözsi Művészet*, Budapest, Corvina Kiadó, 1982.

S. KOOLJMAN

The art illustrated and discussed in this book is designated as tribal art. Other names have been, and still are, used as general indications for this wide variety of non-European art forms, such as exotic art, folk art and primitive art. The latter term still serves to denote the famous collection in the Metropolitan Museum of Art in New York brought together by Nelson A. Rockefeller, which was originally housed in his private museum, The Museum of Primitive Art. In the introduction (pp. 15-56), dealing with a number of general subjects, such as the history of research, art and society, the role of the artist, and the concept of style, Bodrogi attempts to justify the choice of the term *Stammeskunst* – thus agreeing with W. Fagg who could not find a more appropriate designation, and disagreeing with A. A. Gerbrands' view that 'tribal art' is a pejorative term. He further discusses a few general aspects of this art, in particular its function in religion and social life.

Bodrogi has also made a significant contribution to the factual contents of the book by the presentation and description of a number of art provinces and style areas, as well as through the final chapter dealing with modern developments, particularly in African art. He has written the section on Australia and most of the section on Oceania, i.e., the introduction, the chapters on Polynesia and Micronesia, and the chapter on Melanesia, with the exception of a few contributions by Frank Tiesler, curator of the Staatliche Museum für Völkerkunde in Dresden,

dealing with the art of the Sepik river area, the Gulf of Papua, the Admiralty Islands, and the group of islands west of the Admiralties, i.e., Hermit, Kaniet, Ninigo, Aua and Wuvulu. Bodrogi is also the author of about half the section on African art, i.e., the introduction, and of the contributions on the art of the West Sudan, the area of the west coast from the Niger to the mouth of the Congo river, and Madagascar. The other contributors to the part on African art are Csaba Ecsedy, writing on the rock paintings and engravings in the Sahara and the West African *Hochkulturen* on the north coast of the Gulf of Guinea, and Mihály Sárkány, dealing with East Africa. In the section on Asia the short introduction and the chapter on Indonesia have also been written by Bodrogi. The remaining 'tribal areas' of Asia are covered by two other authors, i.e., János Kodolányi and Lydia Schwalbe, dealing with the arts of Siberia and South Asia respectively.

The vital part played by the editor in the preparation of the sections on Oceania, Africa and Asia is nothing to be surprised at, since he is well acquainted with the arts of Oceania, Africa and Indonesia. He has published on the art of the Pacific in his *Oceanian Art*, Budapest 1959 (*Die Kunst Ozeaniens*, Budapest 1960), and has further written general surveys of African and Indonesian art, i.e., *Afrikanische Kunst*, Budapest/Wien/München/Leipzig 1967, and *Die Kunst Indonesiens*, Budapest/Wien/München/Leipzig 1971 (*Kunst van Indonesië*, Den Haag/Budapest 1971). One of his special fields of interest is the material culture and art of the groups living on the north-eastern coast of New Guinea and the adjoining islands, including Astrolabe Bay, the Huon Peninsula, the Umboi and Siassi Islands, and the Vitu or French Islands west of New Britain. The results of his researches in this area have been published in a book – *Art in North-East New Guinea*, Budapest 1961 – and three articles.

In the section on American art Lajos Boglár, the co-editor of Vol. II, has written a short introduction, a chapter on Eskimo art, and one on tribal art in South America. The North American Indians are discussed by László Nagy, except for the Prairie Indians, whose art has been dealt with by József Lorencz. The art of the *Hochkulturen* of Central and South America is treated by György Domanovsky.

With the exception of Tibor Bodrogi and Frank Tiesler, none of these authors appears in the list of bibliographical references. Nor are they mentioned in, for instance, the general introduction. Some of them may belong to the staff of the Ethnographical Museum in Budapest. Their professional identity is by no means clear, however, which is regrettable since it obstructs a discussion with colleagues in other countries on the materials and ideas presented in the chapters written by them.

In any book on visual art the illustrations are of vital importance. These consist of 438 drawings and 649 photographs, 32 of which are in colour. The drawings which have been inserted in the text were made by Zsolt Csalog. They are of an excellent quality, illustrating objects, ornamental patterns, motifs, etc., with a realistic exactness equalling photographic reproduction – as a check by the reviewer through an analysis of drawings of those museum materials he is familiar with testifies. The quality of the colour photographs is very good. Quite a few

black-and-white photos have been produced less satisfactorily, however, so that in a number of cases the objects portrayed are too dark to allow observation and study of the details. This becomes particularly obvious when the photographs of art objects from Oceania are compared with the clear reproductions of the same items in Bodrogi's *Oceanian Art*.

The photographs are arranged at the end of each volume. The vast majority depict art objects. More than half of these are from the collections of three museums in Central Europe, i.e., the Ethnographical Museum in Budapest, the Staatliche Museum für Völkerkunde in Dresden and the Museum für Völkerkunde in Leipzig, as well as from private collections in Budapest; 38% of the total number of illustrated objects are in the possession of the Hungarian Museum. In this connection mention is made of art objects in the Museum in Budapest from Astrolabe Bay and the Huon Peninsula of New Guinea, New Britain and New Ireland. The Museum in Dresden has contributed illustrations of, among other things, Benin bronzes and items from north-western America. The presentation of these materials, which are not easily accessible to people living in Western Europe and the USA, is one of the chief merits of the book under review.

A few critical remarks may be added to the ones already made above. They concern primarily the section on Pacific art, with which the reviewer is particularly familiar. In the list of bibliographical references no mention is made of the descriptive catalogue, *The Art of the Pacific Islands*, compiled by P. Gathercole, A. L. Kaeppler and D. Newton on the occasion of the exhibition at the National Gallery of Art in Washington in 1979. Kooijman's *Tapa on Moce Island, Fiji* has been listed under Tonga. The area of origin of the bark cloth in Fig. 212 is Fiji, and not Celebes as stated in the caption. It is further regrettable that the information on the objects depicted in the illustrations is rather scant. The captions indicate the size but not the colour of each object. Nor are detailed data provided on the basic materials used for its manufacture. A source of growing importance for our knowledge of non-European cultures is provided by ethnographic films, a number of which are focused on art. A mention of these, together with the written sources, would have added to the information given by the bibliographical references.

These flaws will hardly detract from the value of the book for those with a non-scholarly interest in 'tribal art' who want to extend their knowledge of this art beyond the objects and collections presented by 'Western' museums. A fairly large number of people outside German-speaking countries, however, will not be able to read the text or the captions, and one wonders whether a publication in English is being considered.

For scholarly readers, i.e., anthropologists specialized in material culture and art, and particularly for curators of museum collections, the book provides a useful summary of the tribal arts. Those living in 'Western' countries will enjoy the stimulating presentation of materials that are less familiar to them and not treated as fully in the existing general art books.

Raja Ali Haji Ibn Ahmad, *The Precious Gift (Tuhfat al-Nafis)*. An annotated translation by Virginia Matheson & Barbara Watson Andaya. Kuala Lumpur: Oxford University Press, 1982, XIV + 476 pp., maps, ill.

H. M. J. MAIER and R. G. TOL

It would be hard to determine to what extent Malay readers have regarded the *Tuhfat al-Nafis* as the precious gift which its authors, Raja Ahmad and Raja Ali Haji, intended it to be. That its translation, *The Precious Gift*, now published by Oxford University Press, is a precious gift for Western readers is beyond any doubt.

The *Tuhfat al-Nafis* is a work presenting a picture of events in the Malay world, in particular in the Riau-Lingga area, during the period between 1700 and 1860. So far, students of Malay literature and historians of South-East Asia have had to rely on an edition in Jawi script published by R. O. Winstedt in the *Journal of the Royal Asiatic Society, Malayan Branch* in 1932 and a very defective transliteration published in Singapore in 1965. So there was a great need for a more easily accessible Malay text, not to speak of an English translation. All of a sudden, both wishes have been fulfilled at the same time. First, Virginia Matheson had the so-called 'short version' of the *Tuhfat* published in 1982, while here we are presented with an English translation of the 'long version', a joint effort of Barbara Watson Andaya and Virginia Matheson, who hint at the prospect of the publication of the Malay text of this 'long version' in the near future.

The Precious Gift, the book under review here, consists, apart from the translation, of a short historical introduction, some maps for the orientation of the reader, genealogies offering the necessary clarification of the complicated relationships between the heroes of the narrative, and an extremely useful index of names and places – like the three glossaries, they are precious stones indeed. The real jewels, however, are the annotations to the text: containing a wealth of information, they are proof of impressive scholarship.

In the promised edition of the Malay text of the 'long version' we look forward to finding a more detailed account of the method that has been used in establishing this text, as the brief section on 'The Text' (p. 9) in this volume raises a number of questions that are left unanswered.

The translation is said to be based on a critical edition of four manuscripts of the *Tuhfat al-Nafis*, MSS A, B, C and D. 'MS A is a short or base version . . . MSS B, C, and D represent a longer or revised text of the *Tuhfat*'. In other words, there are two recensions of the *Tuhfat* distinguishable, one of which is the base version. 'Base version' seems an unfortunate term here, for on page 5 we are told that Raja Ahmad drew up the first drafts of the *Tuhfat* and that Raja Ali Haji brought his father's work to fruition – should we conclude that those first drafts are the same as this base version, or is this a dangerous confusion of literary and chronological considerations? Moreover, and more importantly, if a distinction should be drawn between two recensions, why then are they both used for a critical edition, and why make such a distinction at all?

Apparently the aim of the edition has been the restoration of the

original *Tuhfat al-Nafis*. But what exactly is this original *Tuhfat*? Is it a text somewhere in between the first drafts of Raja Ahmad and the revised version of Raja Haji Ali? And how is the base version related to this original? Perhaps it would be wiser to refrain from speaking about *the Tuhfat al-Nafis*: which manuscripts can be identified as *Tuhfat* manuscripts when we know that this work has gone through various revisions and elaborations? The sentence at the end of the translation ('If in the future any of my descendants wish to add anything to this chronicle, they may.' (p. 308)) raises the same problem: would the text following this still be identifiable as part of the *Tuhfat al-Nafis*? The relations between the origin, genesis and subsequent changes of a work are usually very complicated in the Malay literary tradition, and so far this has been insufficiently accounted for in this volume.

The presentation of the translation is made rather confusing by the addition of the folio pages of MS B, a MS that is described as a revised and longer version – what is the use of this? In this connection it may be pointed out also that the idea that the homilies which are restricted to MS B are not part of the original *Tuhfat* is a dangerous one. Textual critics certainly would not agree with this.

Watson Andaya and Matheson present the *Tuhfat* as a piece of historiography, and do so emphatically, their translation being preceded by a short summary of what actually happened in the Malay world between 1700 and 1860. Moreover, to make sure that the text is read in the appropriate manner, a hundred pages of footnotes are added to the 300-page text, in which details of the narrative are clarified in the light of data that have been collected by 'research in published sources, primary material from the archives of Britain and the Netherlands, and little used or hitherto uncited Malay manuscripts' (p. 1). The translator's claim that the *Tuhfat* is a 'treasury of information for historians' (p. 7) seems fully justified: *The Precious Gift* tells us a lot about Malay history.

The standard of the translation is hard to judge as long as the Malay text which was used remains unpublished. The English is very pleasant to read, and the narrative has been conveniently divided into paragraphs. Whether it is a faithful rendering of the original, however, is another question.

Translators have to work from certain options. On the basis of a careful interpretation of the original narrative certain features are distinguished as dominating its formal organization, and these are the features which are preserved in the translation as adequately as possible, necessarily at the cost of features that are believed to be less important. Therefore, a translation is always a formal distortion of the original, and as interpretations are based on formal correspondences within the text, such distortions inevitably result in changes in the interpretive potential of the narrative.

The translators' remark that the 'Malay style of the *Tuhfat* is prolix and repetitive' comes as no surprise to readers of Malay literature. Partial and total repetitions of forms are a dominant feature of almost any Malay text, and precisely because of this some of us will be disappointed at being told that these repetitions are neglected in the translation: 'wordiness and gentle repetition were literary qualities of worth

to contemporary audiences, but they find little favour in modern English, so we have tried to condense where necessary' (p. 10). Thus, the possibility of connecting fragments of the text together on the basis of their formal correspondences seems restricted, and the interpretive potential undermined.

Literary theory has taught us that the act of reading involves a choice between two attitudes towards a text: are we reading it in its referential mode or in its figurative mode? In other words, are we primarily interested in the connections between elements in the narrative on the one hand and reality on the other, or do we prefer to focus on the narrative itself and try to understand how it has been constructed and how the devices and forms used steer us in the direction of certain interpretations?

Watson Andaya and Matheson have already made that choice for us. By their introduction, their footnotes, their maps – by their entire presentation, in short – they make clear that the *Tuhfat* should be read primarily as a text dealing with historical reality. Their frank admission that the narrative in the process of translation has been adjusted to the taste of English readers implies that they have done no justice to the form of the Malay original: this translation is intended more for readers who are interested in the *Tuhfat*'s historical claims than for those interested in its literary form. This gives rise to disappointment at the restrictions on opportunities of establishing formal connections and exploring the narrative, but also to suspicions about the translation of sentences emphasizing its referential qualities. In such sentences as: 'What I want to do is to set out the pattern of events which concerned both Malay and Bugis kings' (p. 12), '*In brief*, we cannot deny every phenomenon which we are unable to verify; since the evidence exists, we must accept it' (p. 26), 'I do not know which of these accounts is the more factual' (p. 43), 'I have found no records of these talks, but with divine assistance it will be explained' (p. 181), and 'It is not possible to be exact because it is not specified in the chronicle' (p. 185), what is meant by 'pattern of events', by 'verify' and 'evidence', by 'factual' and 'explained'? What are the words used in the Malay original? Did the authors of the *Tuhfat* have any idea of what we refer to as a 'fact' or an 'event'? In other words, we wonder whether such translations are justified in view of what is known of the character of the Malay historical imagination. Reading a narrative for its figurative qualities may require the availability of the original, after all.

The straightforwardness of the translators should, however, be respected: they have made a choice in favour of a referential reading and tell the reader so. Yet, this may leave some of us with some sort of an uneasy feeling: why are we not allowed to make that choice ourselves? A more precise translation doing greater justice to these dominating repetitions would have been more desirable, even though this would inevitably have led to other kinds of distortions and to a decrease in the readability of the text for modern readers. A more balanced introduction, in which not only events in the Malay world as covered by the *Tuhfat* and the literary life in Riau at the time of Raja Ali Haji would have been described, but also some of the most striking features of the

Tuhfat's form would have been given attention, could have been another possibility. At least some comments would have been called for on the obsession with succinctness testified by such sentences as: 'The words and style are succinct in order that anyone wishing to understand them will be able to memorize and comprehend them easily' (p. 13), and 'To make it easier for those who wish to memorize it, the style will be brief and concise' (p. 41), on the division of the narrative into two parts, on the distinction between the 'I's' of the authors and the 'he's' of Raja Ahmad and Raja Ali Haji (whose eyes?), and on the sentence that seems to summarize the main theme of the narrative, viz. 'When a fire has gone unchecked it is difficult to extinguish and eventually it becomes a conflagration of enmity and anger, consuming all reason. Sparks and flames escape in the form of words and actions, seeking out anything which can wreak destruction and tribulation on an enemy' (p. 95). Sentences of this kind require at least some clarification of the conceptions of historiography they betray and their effects on the interpretation of the text. This way the reader's attention would have been drawn to the importance of the form of the narrative and the problems confronting the reader wishing to use the *Tuhfat* as a source for historical studies.

However, if we adjust our perspective to the one opted for by the editors and read *The Precious Gift* in its referential mode, we can draw only one conclusion: this admirable book is a very important contribution to the historical study of the Malay world. It is to be hoped that Volume Two of this outstanding publication will appear soon.

J. A. de Moor, *Indisch Militair Tijdschrift (1870-1942): A Selective and Annotated Bibliography*. The Hague: Sectie Militaire Geschiedenis van de Landmachtstaf; Leiden: Centre for the History of European Expansion. 1983. XIV + 237 pp. Price fl. 24.00.

GERARD A. NAGELKERKE

In January 1870 the first issue of *Militair Tijdschrift*, a monthly periodical under the editorship of E. B. Kielstra, at the time a 1st lieutenant and later a captain of the Netherlands Indies Engineering Corps, appeared with Bruining en Wijt publishers in Batavia. The journal continued under this name until 1877, in the second half of which year it changed its name to *Indisch Militair Tijdschrift (IMT)*. It became the official, leading periodical of the Indisch Officiers Korps for over 70 years.

Aside from a biannual list of contents and a "systematically arranged list of contents of the 33 volumes of the *Indisch Militair Tijdschrift* which have appeared from 1870-1903" compiled by the then editor, no access has ever been provided to the *IMT*. This gap has now been filled by J. A. de Moor's select, annotated bibliography.

The bibliography lists around 50% of the articles published in *IMT*, that is, 2075 titles in all. The selection criterion applied by the compiler

was to consider only articles with a colonial character, which seemed at all relevant for the study of colonial military history, for inclusion. The titles listed are systematically arranged in 65 chapters and provided with an English-language abstract or annotation. In addition each entry has been assigned a UDC (Universal Decimal Classification) number, as well as a series of catchwords. The work concludes with an alphabetical author index and a rotating catchword index.

De Moor has produced an impressive piece of work here. The detailed catchword index alone covers 27 pages. The catchwords themselves have been taken mainly from UDC numbers 355-359 (National defence, Military force, Army and navy, and Various military arms). The abstracts and annotations give a clear idea of the contents of the articles, often providing as added information details of the rank and/or function of the authors. The Introduction contains a brief outline of the history of the *IMT*.

This bibliography is a valuable new contribution. It will no doubt prove most useful to researchers studying colonial history, in particular the colonial military history of the former Netherlands East Indies.

Albert Helman, *De foltering van Eldorado. Een ecologische geschiedenis van de vijf Guyana's*. 's-Gravenhage, Nijgh & Van Ditmar, 1983. 495 pp.

GERT J. OOSTINDIE

Tot de verschijning van Albert Helmans *De foltering van Eldorado* (1983) bestond er geen uitgebreide geschiedschrijving van de Guyana's. Sinds Wolbers' *Geschiedenis van Suriname* (in 1861!) is evenmin een volledige historische studie over deze voormalige Nederlandse kolonie gepubliceerd. In Van Liers *Samenleving in een grensgebied. Een sociaal-historische studie van Suriname* (1949; 1977³) wordt bewust geen chronologisch overzicht gegeven, terwijl de studie zich beperkt tot de Nederlandse periode, en met name de achttiende en negentiende eeuw. Alleen al om die reden is de publikatie van Helmans *De foltering van Eldorado* van groot belang. Niet minder belangrijk is het feit, dat Helman, pseudoniem van Lou Lichtveld, zich zeer uitdrukkelijk ten doel heeft gesteld een geschiedschrijving "vanuit het gezichtpunt van de inboorling" (p. 7) te schrijven, als tegenwicht tegen de zijns inziens misleidende en pijnlijke, "volgens hun Europese oriëntatie altijd eenzijdig gedocumenteerde koloniale geschiedenissen" (p. 8). In dit streven ging Anton de Kom hem voor (*Wij slaven van Suriname*, 1934). Pas in de laatste jaren werkt een nieuwe generatie Surinaamse historici met hetzelfde oogmerk; hun publikaties zijn echter nog beperkt tot specifieke thema's.

Helman heeft zich tot doel gesteld de geschiedenis van de Guyana's vanaf hun vroegste tijden te schrijven. Zijn verhaal begint dan ook in de proto-historie van dit gebied, dat wordt begrensd door de Atlantische oceaan, de Orinoco, de Rio Negro en de Amazone. In zijn beschrijving van de geschiedenis tot aan de Europese kolonisatie benadrukt de auteur voortdurend de ecologische eenheid van dit Groot-Guyana; de

Indiaanse bevolking kende geen andere grenzen dan de steeds wisselende afbakening van de territoria der verschillende stammen. De Europese kolonisatie leidde tot een volstrekt willekeurige opdeling van het gebied in aanvankelijk zeven, later vijf Guyana's, van west naar oost gerekend: Spaans, later Venezolaans Guyana; Guyana, daarvoor British Guyana, daarvoor de drie Nederlandse kolonies Essequibo, Demerara en Berbice; Suriname, het voormalige Nederlands-Guyana; Cayenne, nog steeds Overzees Departement van de Franse Republiek; en Braziliaans-Guyana. De grenzen tussen deze gebieden zijn willekeurig getrokken en worden tot op vandaag betwist, volgens Helman een bewijs temeer van de onmogelijkheid van deze verbroekeling.

Na een uitvoerige beschrijving van de pre-historie en de ontwikkeling van de verschillende Indiaanse bevolkingsgroepen schetst Helman, in uiterst kritische bewoordingen, de Iberische *conquista* van Zuid- en Midden-Amerika en de latere, moeizaam verlopende kolonisatie van de Wilde Kust, waar verschillende Europese mogendheden elkaar het land betwistten. Hun belangrijkste motief was volgens Helman de hoop op grote hoeveelheden goud; pas later werd de kust van de Guyana's tot een plantage-economie gemaakt, al bleef de stille hoop op gemakkelijker rijkdommen leiden tot voortdurende, vergeefse expedities naar het binnenland.

De ontwikkeling van plantage-economieën kostte de Indiaanse bevolking een groot deel van haar grond en zelfs haar bestaan; Helman schrijft hierover met meeslepende verontwaardiging. Het leidde tevens tot de aanvoer van grote aantallen Afrikaanse slaven, later ook Aziatische contractarbeiders. De vestiging van blanke kolonisators was in de centrale Guyana's, in tegenstelling tot de Iberische, numeriek van weinig betekenis. Van deze plantage-maatschappijen, met alle excessen van onvrije arbeid, koloniale uitbuiting én verwaarlozing schetst Helman een zwart beeld.

Ook de dubbelzinnigheid die spreekt uit de titel van zijn boek wordt hier duidelijk. Niet alleen *Eldorado*, de mythische Gouden Koning van de indianen en als *pars pro toto* voor deze bevolking aangeduid, wordt gefolterd. Ook de nieuwkomers, of zij nu vrijwillig of gedwongen naar de Wilde Kust komen, worden gepijnigd door voortdurend mislukkingen en teleurstellingen. Groot-Guyana geeft zijn schatten niet gemakkelijk prijs.

De desillusies en het altijd maar tegenvallen van de Westindische winsten veroorzaken dat de kolonisators steeds minder geïnteresseerd raken in de Guyana's. Bitter stelt Helman het kolonialisme verantwoordelijk voor versnippering en onderontwikkeling van deze gebieden. Wanneer uiteindelijk het voormalige Brits- en Nederlands-Guyana onafhankelijk worden is er nauwelijks sprake van een levensvatbare economie, maar vooral niet van een interne eenheid, laat staan van nationalisme. Wat hij ziet als het gevolg van de koloniale verdeel en heerspolitiek vreet tot op de dag van vandaag door, met gevolgen die in het geval van Guyana en Suriname pijnlijk zichtbaar zijn.

In Helmans visie geldt dit niet alleen de economische en politieke problematiek in de landen zelf, maar ook de massale exodus van Surinamers naar Nederland.

Zonder overigens oplossingen te prediken, suggereert Helman dat het gehele gebied (dat “inderdaad één land is; niet alleen van oorsprong, maar ook vandaag nog: een grondgebied bewoond door mensen wier individueel lot onherroepelijk verbonden is met het collectieve lot van de overigen die daar de samenleving vormen”, p. 462) tot een (of misschien twee) naties zal moeten worden: “minder een voorspelling dan wel een veronderstelling op goede gronden” (p. 466). De nu bestaande deel-Guyana's zijn voor hem niet-levensvatbare ficties.

In feite werkt Helman in *De foltering van Eldorado* consequent naar deze interpretatie toe. Zijn grote persoonlijke engagement staat daarbij borg voor een meeslepend betoog, maar ook voor een (bewuste) neiging tot provocatie. Het is de vraag of daarop kritiek past. Enkele andere kritische kanttekeningen zijn echter wel op hun plaats. Allereerst het feit, dat de integrale geschiedschrijving van alle Guyana's wordt beëindigd wanneer Helman toekomt aan de Europese kolonisatie. Zoals het gebied dan verdeeld raakt, wordt ook zijn geschiedschrijving dan verbrokkeld. Juist omdat de economische, sociale en culturele achtergrond in vergelijking met de politieke gebeurtenissen steeds beperkter aandacht krijgt wordt daardoor het verhaal bij tijden wel erg opsommend.

Verder boet het boek aan wetenschappelijke waarde in door een aantal storende omissies, zoals het ontbreken van een notenapparaat, dat slechts gedeeltelijk wordt gecompenseerd door een uitgebreide, zij het niet uitputtende bibliografie.

In tegenstelling tot de populaire en tot Suriname beperkte voorpublicatie, *Avonturen aan de Wilde Kust* (1982), is *De foltering* niet geïllustreerd. Uiteraard is dit geen probleem, wél is het onbegrijpelijk dat in het (dure) boek slechts één, onbruikbaar landkaartje is afgedrukt. Verder heeft Helman, naar eigen zeggen om te objectiveren (p. 8), het gebruik van persoonsnamen zoveel mogelijk vermeden. Dit is echter in de beschrijving van de na-oorlogse situatie in Suriname weinig consequent toegepast, terwijl een volledige naamgeving in feite beter zou zijn geweest. (Overigens noemt Helman zichzelf éénmaal expliciet (p. 409), verschillende malen anoniem (p. 356, 363)). Tenslotte ontbreekt cijfermateriaal over de economische, sociale en demografische ontwikkeling van de Guyana's.

Een methodische verantwoording wordt niet gegeven, afgezien van een motto dat tot stilzwijgen lijkt te dwingen: “La méthode, c'est le chemin qu'on a parcouru” (Grousset). Of, in Helmans eigen verantwoording: “Hem (de auteur – GJO) heeft het een heel mensenleven van studie, nadenken en meemaken gekost, voordat de meeste schellen hem van de ogen vielen . . . om de historie van zijn geboortestreek en van zijn voorouders duidelijk genoeg . . . te doorzien, zodat hij er ook iets zinnigs over zou durven zeggen” (p. 8). Op zo'n verklaring lijkt alleen stilzwijgen gepast. Toch moet gezegd worden dat met name in het slothoofdstuk véél gesuggereerd wordt, een zekere tweeslachtigheid ten opzichte van wenselijkheid dan wel onvermijdelijkheid van de toekomstige eenheid van Groot-Guyana onopgelost blijft en zelfs een enkele haast mystieke opmerking daarbij niet wordt vermeden. Het argument wint daarmee niet aan kracht. Het blijft onduidelijk hoe Helman zich de integratie van de Guyana's met het Latijns-Amerikaanse continent voorstelt. Het is in

dit verband opvallend dat hij specifiek Caraïbische invloeden, variërend van de Haïtiaanse slavenrevolutie tot de twintigste eeuwse traditie van migratie in en vanuit het Caraïbisch gebied in zijn boek niet met zoveel woorden noemt. Vindt hij de Caraïben op zich ook een fictie?

Uiteindelijk kan echter niet anders worden gezegd dan dat Helman, die *De foltering van Eldorado* als zijn testament bestempelt, hiermee een erfstuk achterlaat, dat net als zijn levensloop getuigt van grote eruditie en een eigenzinnige visie. Het zal niemand verwonderen dat de dichter en romancier Albert Helman zijn boek in een voor historici benijdenswaardige stijl heeft geschreven.

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- G. Adrian Horridge: 1) *The Prahu, traditional sailing boat of Indonesia*, Oxford, O.U.P., 1981. xv + 106 pp.
 2) *The lashed-lug boat of the Eastern Archipelagoes*, Maritime Monographs and Reports no. 54, National Maritime Museum, London, 1982. 75 pp.

GERARD PERSOON

The Prahu has been written primarily as a guide to the various types of Indonesian sailing boat, though in the preface the reader is also promised passages about the way of life of sailors and boat-builders, comments on the use of specific words, information on aspects of construction, snippets of history, hints for tourists, and an account of the impact of western technology on local boat-building techniques. The book contains sixteen beautiful colour plates and twenty-four black and white photographs of boats under construction or sailing at sea. In addition there are many very fine drawings by Chris Snoek illustrating facets of boat technology and complete models.

It is hardly surprising that a guide of a mere 100 pages on such a subject and containing so many bits and pieces should be superficial in some ways. None of the subjects are dealt with in great detail. Most of the very brief chapters each deal with a certain type of sailing vessel. The uses and performances, history, and construction of these as well as the places where they are to be found are mentioned. Besides there are chapters on models, museums and boat-building yards. There is a heavy emphasis on Buginese and Madurese sailing ships.

On finishing the book, the reader is still left with a lot of questions. In the first place he wonders what exactly is meant by the subtitle 'The traditional sailing boat of Indonesia'. Though the book opens with representations of ships on the carved panels of the Borobodur and an account of certain boat types which are non-existent nowadays, other vanished types of sailing ship within the Indonesian Archipelago are not mentioned (e.g. those around Sumatra).

Though reference is sometimes made to the installation of engines, it is not stated to what changes this has led in the construction of the ships. Once an engine is installed in a Buginese *pinisi*, only one mast instead of two remains and the back part of the hull is changed. These modernized

ships also assume a different appearance because of their bigger deck-houses.

One would also like more information about the durability of the ships, and about maintenance problems and such social aspects as crew composition, ownership and wages. Especially at a time when the Indonesian government is working on a Maritime Development Plan, one wonders what the future of traditional sailing vessels in Indonesia will be. It is to be regretted that Horridge, as an expert on this subject, does not raise this question in his book.

In spite of these remarks, however, *The Prahu* is a finely presented book and a very welcome contribution to the rather scant or outdated available knowledge on this subject.

The second book is a more technical kind of monograph on one particular constructional type: the lashed-lug planked boat. In this book, his fourth in a series of publications of the National Maritime Museum in London, the author adduces different kinds of evidence to produce a clear picture of this type of boat, which was used in the Eastern Archipelago, including the Philippines, already in pre-European times.

Beginning with information from a Spanish manuscript (dated 1668) by Fr. Alcisco Alcina, the author puts forward data from other accounts as well as evidence from archaeological finds, aspects of surviving boat technology in isolated places and elements of two old ship models (one from the museum in Berlin and one from Breda). He argues that these techniques of boat construction have not received due attention in the accounts of the movement of peoples in the area. He also blames anthropologists and ecologists, rightly or wrongly, for having overlooked the significance of the structure of boats (the *pledang*) at Lamale-rap, on the island of Lomblen, east of Flores.

It would be interesting to combine historical, ethnographic and linguistic evidence with the results of his research on this plank boat construction. Very few examples of this type have survived down to the present day.

Considering his monographs and his guide on the Indonesian prahu together (and there is more to come), it is obvious that Horridge's work is of great value. It is a challenge to anthropologists to join him in his interest in this aspect of material culture.

H. Coerts. *De A.R.P. en Nieuw-Guinea, Historische analyse van een partijcrisis*. Franeker: Wever, 1983, 104 blz.

HARRY A. POEZE

De Antirevolutionaire Partij (A.R.P.), een rechtzinnige gereformeerde partij, heeft in de Nederlandse politiek vaak een invloed uitgeoefend die het betrekkelijk geringe aantal parlamentszetels overtrof. Na 1945 leek hieraan een einde te komen. Op principieel-godsdienstige gronden werd geopponeerd tegen overleg en een akkoord met de Indonesische nationalisten. Daardoor kon geen directe invloed op het regeringsbeleid, als

partner in de coalitie, worden uitgeoefend. De resultaten van de Ronde-Tafel-Conferentie en de soevereiniteitsoverdracht kregen in 1949 dan ook geen parlementaire steun van de A.R.P. Deze lijn werd doorgetrokken ten aanzien van Nieuw-Guinea, dat voorshands onder Nederlands gezag was gelaten. De A.R.P. wilde niet weten van overleg met Indonesië, te minder nog toen de Indonesische binnenlandse ontwikkelingen in een richting gingen die werd beschouwd als een schending van de R.T.C.-afspraken. Heel lang, ook als regeringspartij (sinds 1952), bleef deze koers gehandhaafd, met slechts enkele nieuwe argumenten ter rechtvaardiging van de handhaving van het Nederlandse gezag. De omslag was abrupt en sensationeel; in 1961 spraken de A.R.P.-leiders Bruins Slot en Berghuis zich uit voor onderhandelingen met Indonesië die tot een overdracht zouden kunnen leiden. De politieke schok was groot; in regering, politiek en partij. Internationale druk en de bijna-oorlogssituatie met Indonesië bewerkstelligden in 1962 het einde van de Nederlandse aanwezigheid in Azië.

Coerts heeft over de Nieuw-Guineapolitiek van de A.R.P. een doctoraalscriptie geschreven, die in bewerkte vorm is uitgegeven. Hij heeft uitvoerig archiefonderzoek verricht, alle A.R.-periodieken nagelezen en een aantal betrokkenen geïnterviewd. Gelet hierop is het resultaat helaas mager: een klein boekje, dat een beknopt feitelijk overzicht verschaft. Aan de hand van relevante citaten wordt een chronologisch verslag gegeven, dat zich beperkt tot de A.R.P., waarbij de ontwikkelingen in de rest van de Nederlandse politiek in te geringe mate aan de orde komen. Met het verzamelde materiaal zou, lijkt het mij, een uitgebreider boek moeten zijn te schrijven. Dan zou ook veel meer achtergrond kunnen worden gegeven over de ontwikkeling van standpunten en argumenten, een betere kenschets kunnen worden gegeven van mensen als Schouten, Bruins Slot en Berghuis en de rol van zending en afwijkende opvattingen kunnen worden uitgediept. Ook de verklaring voor "het omgaan" zou uitgebreider kunnen. Nu staan slechts de grote lijnen op papier. Meer details en een persoonlijke toets van betrokkenen (uit interviews en geschriften) zouden het verhaal niet alleen boeiender maken, maar ook tot meer begrip leiden. Wellicht kan Coerts zijn scriptie in deze zin nog eens omwerken tot een dissertatie. Dan zou hij ook de moeite moeten nemen een register te vervaardigen, dat nu node wordt gemist.

Dit boekje geeft nu niet veel meer dan het voortreffelijke overzichtswerk van Arend Lijphart, *The Trauma of Decolonization, the Dutch and West New Guinea*, New Haven/London: Yale University Press, 1966, dat in Nederland ten onrechte nauwelijks is opgemerkt.

John U. Wolff and Soepomo Poedjosoedarmo, *Communicative Codes in Central Java*, Data Paper Number 116, Southeast Asia Program, Department of Asian Studies, Cornell University, Ithaca N.Y., June 1982. X + 188 pp. Price \$7.50.

S. O. ROBSON

As stated in the Foreword, this study is a report of fieldwork done over a period of sixteen months in 1973-74 in the area of Yogyakarta. "Our aim is to provide a grammar of code choice in the speech community of Central Java, basing ourselves upon an empirical examination of texts collected with a cassette recorder and transcribed exactly" (p. v).

Numerous assistants (also called "employees") worked on the project, making the recordings, transcribing and consulting; the main ones are listed on p. v: the majority were students. These collected the materials "usually within their circle of acquaintances" (p. 11), including a detailed commentary on "other factors, linguistic and extra-linguistic". (An odd "I" in a footnote on p. 72 is, however, probably Wolff speaking.)

The monograph claims to be a study of the vernacular of Central Java, but is in fact confined to "Yogyakarta and the surrounding towns" (p. 11). It is stated that the Javanese speech community is split between the Peranakan Chinese on the one hand and the Pribumi on the other; also "The Pribumi community itself could probably be further broken down from the point of view of different repertoires into groups that speak a clearly marked regional dialect, and groups that use only a portion of the repertoire available. We have done no more than make a few general comments about usage by various classes" (p. 3). This seems to mean that the speech of the countryside was of no interest, and that the social background of speakers was regarded as of little consequence. Terms such as "a white collar occupation" lack precision. The prominent place attributed to the Peranakan Chinese is reflected in the fact that one whole chapter (Chapter 5), i.e. pp. 91-122, is devoted to their speech. The present reviewer cannot avoid the conclusion that these biases have something to do with the use of student assistants and the choice of informant that they made.

Chapter 1 is a general introduction on background, aims and methods. Here the rather original suggestion is made that "... comparisons with speech levels and caste dialects in south India may possibly help explain the original features of the speech levels, as it may turn out that Javanese speech levels are historically related to those of India" (p. 8)! Surely if one wishes to look for historical relations it would be better to do so within the Austronesian family – what about Samoan?

Chapter 2 on "The Function of the Speech Levels" contains a number of valid insights, e.g. on the use of Madya. Chapter 3 on "The Function of Indonesian" is even more interesting, as it formulates what were hitherto intuitive feelings about the role of Indonesian among Javanese speakers: "... Indonesian is a code which confers a certain sort of prestige. For example, Indonesian is associated with education and modernization" (p. 50). It is shown how Indonesian and Javanese can be

mixed or can alternate for particular purposes. Chapter 4 is on "Shifts in the Speech Level".

A large proportion of the book is taken up with nine sample texts (pp. 129-188). Here, and throughout, a "phonemic" spelling is used, e.g. *emboh* (= *embuh*); *tandor* (= *tandur*); *wés* (= *wis*); *séng* (= *sing*); *Léq* (= *Lik*); *kenô* (= *kena*); *jôgô* (= *jaga*); *yhô* (= *iya*) etc. This spelling has a distracting appearance to anyone already familiar with the standard spelling of Javanese, and apart from this it has the disadvantage of obscuring the phenomena of allophonic variation. The authors would do well to abandon it forthwith. Each conversation is preceded by a few lines sketching the situation and mentioning who the speakers are, although not enough is said to determine whether dialectical factors may be present or to give more insight into the personal relationships which, after all, may well have an influence on language-use. The conversations are translated into idiomatic American, which is useful as a guide to interpreting the texts, containing as they do a great deal of colloquial Javanese not to be found in other sources.

The merit of this study is that it presents materials taken from actual recordings and draws attention to the phenomena of the spoken language. There is no doubt that this deserves further study; but then more attention should be given to methodological considerations, in particular where these touch on sociological and cultural factors.

An index would have been a useful addition, and more care could have been taken with the typing.

Harmut K. Hildebrand. *Die Wildbeutergruppen Borneos*. Müncher Ethnologische Abhandlungen, no. 2. München: Minerva Publikation, 1982. xxvii + 374 pp., 11 maps, 2 tables, bibliography, English summary

JEROME ROUSSEAU

The nomadic hunters-gatherers of central Borneo have always held a fascination for visitors to the island, but, unlike the Kayan, Kenyah, Bahau and Kajang agriculturalists among whom they live, they have not been the object of any extensive fieldwork. What we know of them is based mostly on short-term contacts or hear-say. There is a large body of scattered information on them, and it is the purpose of this book to collate and integrate that information. After a brief discussion of the sources, Hildebrand approaches the problem of ethnic classification. The nomadic hunters-gatherers have been referred to in the literature under a number of names: Ot, Bukat, Bukitan, Ukit, Basap, Punan, Penan, etc. The author shows that there is no simple correspondence between ethnic label and actual ethnic differentiation; rather, the use of given ethnic labels seems to be determined by the *settled* groups: "The name *Punan* is mostly found in the Kayan and Kenyah areas, *Ot* in the Ngaju area, *Bukitan* in Land Dayak and Iban areas, and *Basap* in the area of the east coast inhabited by the Malays". The situation is more

complex, but this is indeed part of the truth. "Punan" has been used throughout Borneo as a generic term for "nomadic hunter-gatherer". Hildebrand reviews the controversy between Harrison, Needham and others about the relative significance of "Penan" and "Punan", and he shows that it failed to come to any solid conclusion because it excluded a consideration of nomadic groups in Kalimantan.

The next section considers the – scant – linguistic evidence. Hildebrand establishes some tentative subgroupings: with the exception of the Punan Busang, the nomadic "Punan" or "Penan" of the 4th and 7th Divisions of Sarawak speak the same language; on the other hand, he sees a relationship between the languages of the Ukit, Bukitan, Punan Busang, Punan Batu, Punan Sadjau (or Sadjau Basap), and Punan Ba. The similarity between Punan Sadjau and Punan Ba had already been pointed out by Cense and Uhlenbeck in *Languages of Borneo*, and this brings an additional element to another controversy in the classification of Borneo groups. The Punan Ba are swidden agriculturalists in the 4th and 7th Divisions of Sarawak. Because they are called "Punan", early authors included them among the nomadic groups. Leach and Needham showed this attribution to be unjustified, as they are part of the Kayan-Kenyah-Kajang socio-cultural group. However, there are enough specific similarities in the wordlists of the nomadic Punan Sadjau of Kalimantan Timur and the Punan Ba to suspect that, after all, and despite the fact that they deny it, the latter might indeed have a nomadic origin (which does not change the fact that they are now part of the Kajang).

In Hildebrand's account, the "conversion" of nomadic Punan Sadjau into settled Punan Ba appears as an isolated event. He should have emphasized that this was part of an ongoing process. Not only has there been in recent years a strong pressure on nomads to take up agriculture, but such shifts have been happening for a long time. We know that the Penihing, Sebop, Seputan and Bukat used to be nomadic, and some of the Penan seem to have been settled already in the 19th century. It has even been hypothesized that the Kenyah, who are the largest group of agriculturalists in the centre of Borneo, are nomads who took up agriculture under the influence of the Kayan. There is also some evidence that the shift sometimes took place in the other direction, i.e. that some agriculturalists became hunters-gatherers. Taken in this wider context, Hildebrand's hypothesis that the Punan Ba might have been nomadic in the past takes on a different significance. The nomads and the agriculturalists constitute a single system with two sectors, and movements take place from one to the other, according to circumstances.

The following section will be of great use to researchers. It is an alphabetical catalogue of all nomadic groups and their distribution. The author manages to establish some order in what has been a very confusing body of data. The next chapter deals with the nomads' involvement in trade. This is the best documented section, because this is the subject on which the literature has focused: much of the interest in hunters-gatherers lay in the fact that they were the collectors of valuable jungle produce. The monograph ends with a brief discussion of settlement patterns and a detailed consideration of means of subsistence. There is an important gap in Hildebrand's work: very little is said about

political organization, kinship and marriage, childrearing and religion. This is a consequence of a bias of the literature, which focuses on nomad-agricultural interaction. However, this was by no means *terra incognita*, because of various articles by Needham, Nicolaisen and others.

This is an important book for the the student of Borneo, as it collates much scattered information; it is an essential reference for anyone doing research on central Borneo. However, it could have done with more careful editing; there are a number of errors in quotes, and the names of many authors are misspelled; some quotes are out of context; the English summary is not completely accurate.

Some conclusions are not compelling. For instance, from the facts that (a) iron implements are necessary for making wooden blowpipes, (b) the nomads obtain such implements from agriculturalists, and (c) the blowpipe is a central feature of hunting-gathering technology, the author concludes that hunters-gatherers were dependent on "foreign goods" for their existence. These factors indeed establish an important link between nomads and agriculturalists, but not a relation of dependence, because the fact that the nomads usually obtain their iron implements from agriculturalists is not evidence that they are unable to make them themselves – because many nomads *are* skilled blacksmiths; rather, it is usually more convenient for them to obtain iron through trade than to make it themselves. In the same way, agriculturalists have stopped smelting their own iron because iron bars can be bought inexpensively and readily. But it would be wrong to conclude that they used to be dependent on trade for their iron implements.

In his discussion of hunter-gatherer population numbers, Hildebrand assumes that the shift from hunting-gathering to agriculture necessarily reduced the nomadic population (p. 34); this is of course possible, but it might also be that this shift was a consequence of population pressure.

Given the symbiotic relationship between nomads and agriculturalists, it is not possible to deal with the former as a separate society; no analysis can be complete without a consideration of central Borneo as a whole. This is why a number of issues cannot be resolved here. This is however *not* a criticism of the book; so far, the information on Borneo hunters-gatherers has been so scattered that it was difficult to give due attention to this symbiotic relationship. The present book thus answers a need and makes further studies possible. It is a significant contribution to our knowledge of Borneo and will remain an important reference tool.

Jan Bank, *Katholieken en de Indonesische Revolutie*, Baarn: Ambo, 1983, 576 pp.

T. B. SIMATUPANG

What happened in Indonesia and in The Netherlands during the period of conflict from the Proclamation of Indonesia's Independence on August 17, 1945, to the Dutch recognition of Indonesia's sovereignty on

December 27, 1949, will have an abiding impact on the relationship between the two countries. A deeper insight into the thoughts, feelings and hopes on both sides during that decisive period of history will be beneficial for a deeper mutual understanding between Indonesia and The Netherlands. For this reason, Jan Bank's book, *Katholieken en de Indonesische Revolutie*, focusing on the role of Roman Catholics both in The Netherlands and, to some extent, in Indonesia during those years, should be welcomed as an important contribution to the respectable body of Dutch-language literature dealing with the Indonesian revolution.

The book is an example of good Dutch scholarship: it is solid, thorough, exhaustive, and also rather bulky. Apparently the author did not consult literature in the Indonesian language. The book has some "Schönheitsfehler" which do not, however, detract from its merit or quality. For example, the author refers to "the Indonesian Red Crescent" (*de Indonesische Rode Halve Maan*) instead of the Indonesian Red Cross (page 408), to "luitenant" A. H. Nasution instead of "cadet vaandrig" A. H. Nasution (page 118), to Sjarifoeddin Prawiranegara instead of Sjafoeddin Prawiranegara (pages 403 and 456).

The present reviewer, who was involved in the military and diplomatic aspects of the Indonesian revolution, read Jan Bank's book with one main question on his mind. This question is how far the book provides an insight into the origin and nature of what we in Indonesia considered to be the "unholy alliance" between the Roman Catholic leadership (the trio Romme, Sassen and Beel) and General Spoor in the shaping and execution of the unrealistic and therefore fatal Dutch policy aimed at the destruction of the Republic of Indonesia and the liquidation of the Indonesian National Army in the search for a viable independent Indonesia linked with The Netherlands by ties of friendship and mutual respect. It is noteworthy that P. A. Kerstens used the same words, "kwade trouw" (bad faith), which at that time were used in Indonesia, when referring to the "aankleding" (the dressing up) of the Linggarjati Agreement (Jan Bank consistently speaks of the "Linggajati Agreement"), which in Indonesia was primarily associated with Romme and the KVP (Katholieke Volkspartij).

On the one hand, Jan Bank's book gives a good illustration of the tragic inability of the Dutch political leadership and the Dutch people to grasp, and the tragic inability of the Indonesian political leadership to convince them, that, despite their serious shortcomings and troublesome excesses, the Republic of Indonesia and the Indonesian National Army constituted the genuine and authentic expressions of the Indonesian revolution. On the other hand, it provides a good description of the development of the role of the Roman Catholics as a minority which was out to prove its patriotism until it reached the moment when at long last it had the opportunity to fulfil its wishes with regard to Indonesia in alliance with General Spoor (page 375).

The inability of the Roman Catholic leadership in The Netherlands to give a serious hearing to the views of the Roman Catholic leadership in Indonesia in interpreting the Indonesian revolution is an indication that the ultimate criterion for the Dutch Roman Catholic leadership was not

the future of the Indonesian people, including the future of the Roman Catholics in Indonesia, but the dictates of internal Dutch politics. In the interesting description of the role of Roman Catholics in Indonesia one misses the notion that in a country like Indonesia the role of neither Roman Catholic Christians nor Protestant Christians can be understood unless they are seen as parts of a common Christian presence, in a dominantly non-Christian environment.

Reading Jan Bank's book led this reviewer to wonder if not perhaps the time has come for a joint Indonesian-Dutch project to study the events between 1945 and 1949 and present a more balanced and a more complete picture of that unique period in the history of both countries. Such a joint study may make for a deeper mutual understanding and appreciation, not only with regard to the past but also for the years ahead, when Indonesia expects to meet with understanding and co-operation from The Netherlands in coping with the ambiguities of national development in a post-revolutionary period.

Richard McGinn, *Outline of Rejang syntax*, NUSA Linguistic Studies of Indonesian and other languages in Indonesia 14, Jakarta 1982, xvi + 76 pp.

H. STEINHAEUER

Compared with the huge majority of languages in Indonesia, Rejang has been studied quite extensively. Yet our knowledge (i.e. the knowledge of the interested community) of it has never become more than just potential. The reason is that until the publication under review most studies dealing specifically with Rejang were never printed. Jaspán's dictionary of the Musi dialect is only available in manuscript form. A vocabulary compiled by Kähler has not been published, either. Voorhoeve's material for a dictionary of the Lebong dialect became lost in the second world war. Aichele wrote an article on Rejang sound changes which never appeared in print. The only materials that have been published are a glossary, a wordlist and some texts – all dating from the 19th century and linguistically less reliable – and a one-page comment on the older sources on Rejang by Voorhoeve (Voorhoeve 1955:20-21). Recently four Indonesian study reports dealing with (aspects of) Rejang grammar (Proyek Penelitian, 1979, and Atika S. M., 1980a, 1980b, 1981) have appeared, but there are only a few stencilled copies of these available; moreover, these studies are of a (very) preliminary character. Given this unsatisfactory state of affairs, the book under review, "a slightly revised version of" the author's "1979 University of Hawaii dissertation" (p. xiv), is a welcome contribution to our knowledge of Rejang.

The author did fieldwork in the four major Rejang dialects (Musi, Lebong, Kabanagung and Pasisir) from 1973 to 1976. The present study is focused on the Musi dialect. McGinn duly acknowledges the valuable

contribution of his main informant, Mr. Zainubi Arbi, who not only by himself collected over 70,000 words of text in the four dialects, but who also carried out the initial analysis of these data (comprising a phonemic transcription and the provision of phonetic and cultural notes).

Yet, in spite of this wealth of material, the actual number of Rejang constructions in the grammar is extremely limited. I counted about 120 different (short) sentences/phrases, and besides fewer than 400 different words/morphemes.

It is significant that the author not only acknowledges that “no attempt has been made to achieve a total description of the language”, but also stresses the fact that the “focus of the dissertation has not been a description for its own sake, but a description that supports the special theory of a language type” (p. 70). This special theory – which is repeated many times throughout the book – is the (hypo)thesis that in Rejang grammar no transformational rules are necessary for the movement of noun phrases; a (hypo)thesis which “is plausible only within the theory of transformational grammar” (p. 8), and more specifically within the so-called Revised Extended Standard Theory of Noam Chomsky c.s. as it had been developed up to 1977-78. The author admits in his Preface (p. xiv) that “in 1982 it would certainly have been possible to revise the dissertation and bring it up to date in terms of developments in linguistic theory since 1977-78, of which there have been many, especially in Chomsky’s camp”. The importance of the theoretical stands proposed in the study is deflated not only because of these later developments in “the” theory, but also because of the (correct) picture, evoked by the term “camp”, of mutually opposing, belligerent sects within “linguistic” – i.e. especially transformational generative – theory.

It is a well known fact that the transformational generative grammar has been evolved and tested mainly by reference to English, if not to unobservable “underlying” English. Its application to an “exotic” language such as Rejang should therefore in principle be welcomed. Typically, however, the author was unable to free himself from this English bias, for example where he states on p. 7 that “the *problem* [italics mine, H.S.] for any theory of Austronesian languages is to explain why the analogues [whatever these might be] of the following English sentences are ungrammatical: (i) *What* did John see?, (ii) I know *what* John saw, (iii) I know the man *whom* John saw, (iv) *Whose* nose is long?, (v) *Who* did John give the book to?, (vi) *The book* is easy to read, (vii) *What* did John claim that Mary bought?, (viii) *Who* is Mary taller than?” Characteristic for a transformational generative approach also is the number of ungrammatical constructions that are discussed throughout the grammar: 45, besides the 120 grammatical ones. In most cases they are word-to-word translations of *English* syntactic constructions; in a number of cases they are illustrations of lexical syntactic constraints; and only in a few cases are they functional, i.e. where they refute generalizations which are likely on the basis of *Rejang* constructions which *are* grammatical.

It may be a matter of taste, but for someone working with a non-transformational generative paradigm – as it used to be called – these constructions are the most important and interesting. However, the

exigencies of the theory-centric approach make for their presentation being rather unstructured and incomplete.

The structure of most simple sentence types that are possible in Rejang seems to be discussed in one place or another; compound sentences, however, are hardly presented at all. Neither have I found any discussion with examples of "unusual word orders", which would have to be accounted for by so-called rules of "scrambling", which are said to be different from transformations and thus excluded from the core grammar (cf. p. 4). By what criteria these orders would be unusual (by those of "universal" typological considerations or of language specific statistics) is not explained.

Furthermore, the grammar does not contain any discussion or inventory of function words, while the only information on Rejang intonation is limited to the *prima facie*, not very likely, observation that the intonational features found for Indonesian by Amran Halim (cf. Halim 1974) are precisely the same as those operative in Rejang (p. 5).

The discussion of the apparent counterexample to the "Noun Phrase Movement Prohibition Hypothesis", namely constructions such as *pilem ?o coa si temoton* "movie that not he see" (= "that movie he didn't see"), is unsatisfactory. The solution to the problem these offer is highly *ad hoc*, as the author proposes derivation by means of a deletion rule from the "underlying" construction ***pilem ?o coa si temoton ne*, which would be in accordance with the generative rules of the grammar because of the grammaticality of *Jon, si bi bélé?* "John, he has gone-home". Incidentally, according to McGinn's own grammar, it is impossible for *ne* to be the form of the 3d person singular pronoun after an active verb form such as *temoton*; one would have expected *si*. But more important is that the proposed solution makes the ungrammaticality of **pilem ?o si temoton* inexplicable. On the whole the discussion of this counterexample (p. 23-24) is too short. It is merely suggested that *coa* in the above grammatical construction is replaceable by another "verbal element", such as *buléa?* "may", *bi* "PAST", *mulay* "begin" and others (McGinn's list is not complete, either). One would like to know whether combinations of these forms may replace *coa* and whether *si temoton* may be replaced by its passive counterpart, *tenoton ne* "seen by-him". If the answer to the latter question is negative, probably the following semantic proportion would apply:

si temoton pilem ?o : pilem ?o tenoton ne =
si coa temoton pilem ?o : pilem ?o coa si temoton.

If, on the other hand, the answer were to be affirmative, the additional problem of the semantic difference between *pilem ?o coa si temoton* and *pilem ?o coa tenoton ne* would have to be solved.

For me, the most unsatisfactory part of the book is the description of Rejang morphology. This "reasonably complete discussion of Rejang morphology" (p. 37) is unfortunately confined to an inventory of morphological processes with hardly any concrete examples and only traces of a semantic qualification. Not the slightest indication is given of semantic relations between different morphological extensions of the same root, or of the classes of roots to which the morphological processes can be applied. The function of the roots themselves, when they occur as

words, is apparently not considered part of morphology and is nowhere discussed. Neither is the matter of the (im)productivity of morphological processes; what seem to be fossilized and what are highly productive processes are both dealt with as completely similar cases.

The curious formal parallelism between passive and causative constructions (cf. *si kemléa? Jon/ Jon kemléa? si* "he saw John/ John saw him", *Jon kenléa? ne/ si kenléa? Jon* "John was seen by him/ he was seen by John", and *si kemlaley* "he looked around", *Jon kenlaley ne* "John made him look around") would certainly have deserved a morphological discussion in terms of (im)productivity and predictability.

The morphophonemic side of the morphological processes is discussed only cursorily (p. 56-58) and leaves a lot of questions unanswered. According to the rules on infixation (p. 57), for instance, roots beginning with //p// behave differently from roots beginning with one of the other voiceless stops; how they behave is not explained, however, nor is any example given.

Many similar inconsistencies as well as contradictions can be pointed out. I will not list them here. They are partly the result of printing errors, of which there are many. The circumstance that cross-references often refer the reader to wrong or non-existing paragraphs is probably also a result of these.

The critical remarks of this review are directed not so much against the author of *Outline of Rejang syntax*, as against a school in linguistics in which attention to theory for its own sake and to *theoretical* facts is more important than attention to observable facts about specific languages. A striking example is the following quotation (p. 62): "A great many doublets are exemplified by the following pairs: *tedung* ~ ?*dung* 'snake', *tejé* ~ ?*jé* 'stand', **bebet* ~ ?*bet* 'belt'", in which the asterisk is *not* a printing error!

The conclusion must be that *Outline of Rejang syntax*, in spite of its importance as a printed source of information on modern Rejang, still leaves many questions unanswered. Let us hope that the "several projected works" on Rejang, such as the dictionary announced on page 2, will offer us a more satisfying picture of the language.

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Yeo Kim Wah, *The Politics of Decentralization. Colonial Controversy in Malaya 1920-1929*; Oxford University Press/Institute of Southeast Asian Studies, Kuala Lumpur, 1982, xiv, 395 pp., map and photographs.

HEATHER SUTHERLAND

This detailed study of ten years of bureaucratic politics is by no means a fashionable book. Concentrating as it does on the apparently dry theme of administrative decentralization, Yeo's work lies within the classic tradition of colonial Malayan history, emphasizing the role of British officials within formal government structures. But it would be a mistake to underestimate the value of *The Politics of Decentralization*, just as it would be an unwise self-limitation to ignore the contributions of that historiographical tradition. For while current enthusiasms may focus upon peasant communities or local history, it was often the decisions of civil servants which decisively influenced the wider frameworks which shaped the lives of ordinary people. The organisation of village administration, of schooling, health services and law courts, the routing of railways or steam shipping, all changed the infrastructures which generated change, created opportunities and undermined old ways. These structural changes were initiated by small groups of men, independent of local community control, but deeply affected by European ideologies, perceptions and interests. So while it is true that the time for uncritical recording of "pacification and development" histories is past, we still need solid, critical studies of the political strife which underlay much colonial decision-making.

By the outbreak of the first World War, British Malaya had assumed the complex form which was to remain dominant until after the second World War. There were five Unfederated Malay States (Perlis, Kedah, Kelantan, Trengganu and Johor), the UMS, four Federated Malay States (Perak, Pahang, Selangor and Negri Sembilan), the FMS, and the Straits Settlements of Penang with Province Wellesly, the Dindings and Singapore. These divisions roughly paralleled the social, economic and demographic contrasts between the predominantly agrarian, Malay UMS, the booming, tin and rubber oriented multi-ethnic FMS and the more urban Chinese Straits Settlements. The highest power was of course in London, in the Colonial Office, but the overall local authority was the Singapore based High Commissioner, who not only led the Straits Settlements and had general supervisory rights over the UMS, but was also, in his capacity as Governor, officially head of the FMS administration. In practice, however, his theoretical subordinate in Kuala Lumpur, the Chief Secretary, had considerable power over the Federal Departments and, through the Residents, within the Federated States. According to the Residential system, the top British officials of the MCS (Malayan Civil Service) were dominant within each state, though the range of their activities was gradually reduced as the central Federal bureaucracy expanded, reducing the Resident's task to increasingly insignificant paper work. This trend was also evident in the lower ranks of the MCS, where the once all powerful local District Officers were more and more desk bound and alienated from rural

society. These tendencies were much weaker in the UMS, where the indirect rule of the British was reflected in the Advisory role of their officials and the continuance of genuine Malay power.

Such a diverse system was bound to have its tensions, and in the 1920s these came to a head in the bitter conflict between the Governor-High Commissioner (1919-27) Sir Laurence Guillemard and the headstrong Chief Secretary (1920-6) Sir George Maxwell. Both favoured some form of decentralization, but Guillemard wanted to abolish the Chief Secretaryship and concentrate power in Singapore, thus providing an overarching general structure within which the FMS would be more free of bureaucratic meddling, while Maxwell wanted a strong Chief Secretaryship but with more power for the FMS Residents, who would also have a partially Advisory role. The long term aim of London was to loosen the ties on the FMS so that they would be more like the UMS, and hence the way could be opened for a Malayan wide administration which was ruled out by the opposition of the UMS as long as the FMS were shackled to Kuala Lumpur and Singapore. Decentralization meant different things to different people, and the faction fighting that the Maxwell and Guillemard proposals produced, as carefully described by Yeo, reveals much about the balance of power and conflicting perceptions within colonial Malaya. In Yeo's judgement, it was the MCS, the career officials, who were decisive in defeating decentralisation plans in the 1920s, though the struggles cleared the way for the Clementi initiatives of the early 1930s.

The Politics of Decentralization is a solid and readable account of a bitter personal struggle about bureaucratic planning. But it is also more than that. The long term British goal of an efficient, integrated Malayan wide administration was revived in the mid-thirties, but came to nothing, and then, in April 1946, was presented again, in the blueprint for a Malayan Union, which could serve as the framework for an independent multi-racial state (excluding Singapore). The plans for the Union galvanized Malay political feelings, led to the rise of UMNO, and the forging of a successful opposition front with the Old Malayan lobby in England. The defeat of the Malayan Union reaffirmed the special political rights of the Malays, particularly the ruling families, and determined the broad outlines of modern Malaysian politics. It is our awareness of the crucial dimensions of decentralisation and integration within the multi-ethnic Malayan polity which gives Yeo's book an extra poignancy.

Any scholar interested in Malaysian political history would be well advised to consult this work. Yeo is thoughtful in his evaluations of motive and personality, aware of the wider significance of the issues described, and thorough in his archival research. It could be argued that there are too many details on the bureaucratic in-fighting, and not enough indication of the long term meaning of events or of the deeper ideological drives of those involved. *The Politics of Decentralization* is a limited book, but within those limits it is a good book, and a welcome addition to our knowledge of Malayan administrative politics.

Jerome A. Offner, *Law and Politics in Aztec Texcoco*, Cambridge Latin American Studies nr. 44, Cambridge University Press, Cambridge, London, New York, New Rochelle, Melbourne, Sydney, 1983.

RUDOLF VAN ZANTWIIJK

De politieke en de sociale geschiedenis van de op één na belangrijkste van de drie centrale deelstaten van het Azteekse Rijk en de analyse van het daar opgebouwde stelsel van wetgeving en rechtspraak vormen het onderwerp van deze studie. Offner opent daarbij nieuwe wegen, komt met oorspronkelijke denkbeelden en maakt soms uitdagende gevolgtrekkingen.

Het beschrijvende deel van dit werk is over het algemeen van een hoog gehalte. De schrijver toont een grote belangstelling voor de kleine bijzonderheden, zonder nochtans ooit de hoofdzaken uit het oog te verliezen. De hoeveelheid verschaft inlichtingen is erg groot en Offner is zuinig met tekstruimte. Het opnemen van alle informatie vergt dus concentratie van de lezer.

Hier en daar zijn enkele hinderlijke vergissingen gemaakt. Zo wordt op blz. 26 gesproken van "Quahuacan, to the east of the valley of Mexico", terwijl deze plaats ten westen van het genoemde dal ligt. Offner's kennis van het Nahuatl, de Azteekse taal, blijkt soms enkele tekortkomingen te vertonen. Zo spelt hij op blz. 25 Mazahuacán als 'Macahuacan'. Verder meent hij op de Mapa Quinatzin de woorden "ayacmo tlacotli" te lezen en stelt dan dat "this is best transcribed as ayamo tlacotli, which may be translated as 'no longer slave'" (blz. 141). Dit laatste is nu juist niet zo, want 'ayamo' betekent 'nog niet', maar 'ayocmo' = 'niet meer' en deze laatste vorm had dus gelezen moeten worden. Op blz. 246 meent de schrijver, dat 'tlacacoyan' (plaats waar gehoord wordt) hetzelfde betekent als 'tlacacoayan', doch de tweede term beduidt: plaats waar mensen vergaderen (of zich verenigen).

In het algemeen gesproken, valt Offner niet in de vele valkuilen die de Tetzcoaanse (en liever niet Texcocaanse!) chauvinistische geschiedschrijvers in het historische materiaal hebben aangebracht. Aan enkele daarvan is de auteur echter niet ontsnapt.

Zo ziet hij, in navolging van Ixtlilxochitl, Tetzco-Acolhuacán toch nog overwegend als een staat met vrijwel volledige soevereine macht, welke aanvankelijk slechts door 'verdragen' aan de andere Azteekse centrale deelstaten gebonden was en eerst tegen het einde van de regeringstijd van Nezahualpilli (1472-1516) onder de opperheerschappij van Mexico-Tenochtitlan geraakte. Naar mijn mening heeft Offner te weinig oog voor de taakverdeling, die van het begin af aan in de z.g. 'Driebond', liever de 'Drie-vuldige Troon', ingebouwd geweest is: Mexico – militaire opperheerschappij, Tetzco – rechterlijke en hydraulische voortrekkersfuncties, Tlacopan – laatste stem in diplomatie tegenover buitenwereld en aldus de taak om de oorlogsverklaringen van het Azteekse Rijk af te geven. Tlacopan, als restmacht van het voormalige Têcpaneekse Rijk, dat door de twee andere deelgenoten in het Azteekse Rijk was onderworpen, had hiermede de onaangenaamste

plicht in de diplomatie – de brengers van oorlogsverklaringen brachten het er vaak niet levend af.

Wel ben ik het met Offner eens, dat in de Azteekse periode, de politieke macht van Tetzcoaco verder is afgenomen. Wij verschillen slechts van mening over de aard van de Tetzcoacaanse staatsmacht, zowel vóór de Têcpaneeks-Mexicaanse onderwerping in het tweede decennium van de 15de eeuw, als daarna bij het gedeeltelijk herstel ervan onder Nezahualcoyotl. Het is opmerkelijk, dat Offner ten aanzien van de Tetzcoacaanse staat in de 14de eeuw een zeer kritische mening heeft en de in de Tetzcoacaanse bronnen zo geroemde vorst Techoatlala niet hoog aanslaat, terwijl hij dezelfde kritische houding tegenover diezelfde bronnen goeddeels laat varen als het Nezahualcoyotl betreft. In verband hiermede is het tekenend, dat Offner, wanneer hij op blz. 157 de verhouding van de Tetzcoacaanse vorsten tot de kooplieden bespreekt, met geen woord rept over de vaak geciteerde redevowering van Nezahualcoyotl, waarin deze zijn onderdanen op het hart bindt, zich met grote zorgvuldigheid tegenover de Mexicaanse(!) kooplieden te gedragen, die binnen het Tetzcoacaanse rechtsgebied steeds geholpen en beschermd dienen te worden.

Zeer belangwekkend is Offners stelling, dat het Tetzcoacaanse rechtstelsel een van de weinige “legalistische” was, die buiten de invloed van de Romeinse beschaving tot stand kwam. Een andere plaats en tijd, waarin dat gebeurde zijn volgens hem te vinden in China, tijdens de Ch'in Dynastie (221-206 v. Chr.) en mogelijkерwijze in het Inca-Rijk (1438-1533).

Bij de beschrijving van de ingewikkelde Azteekse landrechten, verschaft Offner tal van nuttige nieuwe inzichten, maar in enkele gevallen lijkt hij in vage omschrijvingen te verbergen, dat hij er niet helemaal uitgekomen is (voorwaar geen schande overigens!). Zo heeft hij het op blz. 135 over het feit dat “Only nobles could hold individually land that was not also held by a corporation”. In het laatste geval denken wij dan vooral aan calpulli's als de eigendomhebbende corporaties. Maar een paar zinnen verder doet Offner vrij abrupt een voor een Mexicanist opmerkelijke uitspraak, waar hij zegt: “The calpulli members (at least) probably possessed more than simple usufruct rights in these lands (also held by the corporation!) and probably bought, sold, and passed on land in many areas of the empire with minimal or no government interference”.

Na deze uitspraak blijft de aard en het wezen van het corporatieve landeigendom voor de lezer een volkomen raadsel, dunkt mij.

Een ernstige misvatting van Offner over de calpulli's toont zich op blz. 169, waar hij de mobilisatie-eenheden voor publieke werken, geleid door de z.g. 20- en 100-hoofden, als onderdelen van de calpulli-organisatie blijkt te beschouwen, terwijl uit de onderzoeken van Broda, Carrasco, Prem en Dyckerhoff reeds eerder duidelijk geworden is, dat die mobilisatie-eenheden ook de niet-calpullileden van een lokale gemeenschap omvatten.

Offner's volledige afwijzing van “bilineal or unilineal descent” als een concept, dat ten grondslag zou liggen aan de corporatieve eenheden, lijkt te ver te gaan waar hij dit eveneens afwijst “even on their highest

level". Zijn uitspraak op blz. 129, dat "here is not one scintilla of evidence" voor Carrasco's opvattingen in deze, is zonder meer als roekeloos te kenschetsen (vergelijk ook blz. 284).

Eveneens aanvechtbaar zijn schrijvers opvattingen over de *telpochcalli*, de halfinternaten voor jongens van 7 tot 20 jaar. Dat de calpulli-hoofden "full authority" over deze scholen zouden hebben, lijkt *hoogst onwaarschijnlijk* en niet "*mogelijk*", zoals Offner op blz. 224 (op grond waarvan eigenlijk?) beweert. Op blz. 269 zegt de schrijver, dat telpochcalli-leerlingen altijd "nonnobles" waren, hetgeen door verschillende bronnen tegengesproken wordt. De edelen hadden het *recht*, maar waren zeker *niet gedwongen*, hun zonen naar het andere schooltype, de *calmecac*, te sturen. En de *calmecac* op z'n beurt was ook geen school uitsluitend voor edelen, want zonen van kooplieden en veelbelovende zonen van gewone calpulli-leden werden er ook toegelaten.

Een Mexicanist die het rechtsstelsel tot z'n voornaamste onderwerp van onderzoek heeft verkozen, kan men het zwaar aanrekenen, dat hij de meest authentieke bron over het beroemde proces tegen de overspelige Mexicaanse gemalin van vorst Nezahualpilli van Tetzco over het hoofd heeft gezien. Offner komt weer met de bekende opvatting dat de genoemde overspelige vorstin, Chalchiuhnenetzin, dochter van de grootgebieder Axayacatl, in Tetzco terechtgesteld zou zijn. De Códice en Cruz laat echter zien, dat alleen haar drie minnaars terechtgesteld werden, maar dat zij gespaard bleef.

Evenzeer verwonderlijk is het, dat waar Offner op blz. 245 tot de gevolgtrekking komt, dat "the law of Texcoco seems not to have been an instrument for ensuring fair and equal treatment for all Texcocans involved in various types of disputes", hij niet herinnert aan de veroordeling door de overheid van een Tetzcoaanse rechter, die tijdens een proces een edelman bevoordeelde boven een gewone onderdaan, hetgeen toch op z'n minst een streven naar "equal treatment" verraaft.

Ten aanzien van het vraagstuk van de omvang van de vóór-Spaanse bevolking toont Offner zich een tegenstander van de opvattingen van Cook en Borah. Daarin staat hij niet alleen, maar zijn standpunt is nu toch langzamerhand een ander uiterste geworden. Slicher van Bath, die ook kritisch over Cook en Borah heeft geschreven (zie *Boletín de Estudios Latinoamericanos y del Caribe*, no. 24, juni 1978), komt toch veel dichter bij de omstreden onderzoekers uit Berkely dan bij Offner uit.

Een belangrijke gevolgtrekking van Offner ten aanzien van de Azteekse economie verdient de aandacht en kan een aanmoediging zijn voor nieuw onderzoek: "the private sector of the Texcocan economy was rather large, whereas the economic importance of the imperial tribute organisation was more limited than others have claimed" (blz. 281).

Deze en andere, soms gedurfde, uitspraken maken het boek belangwekkend en dikwijls ook aanvechtbaar. Hoewel in deze beoordeling vele kritische kanttekeningen gemaakt zijn, is mijn eindoordeel over dit werk toch alleszins gunstig. Het is zonder twijfel een belangrijk boek en een waardevolle bijdrage aan het inzicht in de sociaal-economische en politiek-juridische ontwikkelingsprocessen in dit deel van het Azteekse Rijk.