

THE STATUS OF SOCIAL SCIENCE RESEARCH IN BORNEO

edited by

G. N. Appell and Leigh R. Wright



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INTRODUCTION†

G. N. Appell
Brandeis University

On April 2, 1974, the Borneo Research Council held a symposium on the *Status of Social Science Research in Borneo* at the 26th annual meeting of the Association for Asian Studies in Boston, Massachusetts. This volume presents the results of that symposium.

It was decided that the symposium should not only attempt to assess the status of social science research in Borneo, but that it should also focus on the contributions of such research to general knowledge. Where possible, the assessment of a specific discipline should include a summary of the present goals of research, the problems still to be solved, and directions research should take in the future. One of the major purposes of these assessments was also to consider how the results of research in one discipline might have relevance to research in other disciplines. Where possible, sources of funding for research and the development of adequate research personnel were also considered.

As the Council was not in a position to cover the travel costs of participants, presentations were limited to those made by individuals who were already planning to attend. As a result, many outstanding scholars of Borneo studies were not able to contribute, such as Derek Freeman, Rodney Needham, Benedict Sandin, Nicholas Tarling, J. Avé, H. S. Morris, P. M. Shariffudin, Peter Kedit, Lucas Chin, Clifford Sather, and James Ongkili.

Illness, sudden cutbacks in university funds because of the energy crisis, or unexpected opportunities to continue research prevented some persons who had planned to participate from coming; their papers were presented *in absentia* (W. Solheim, J. Osborn, C. Rubenstein, Tom Harrisson). After the conference, to complete our assessment, we asked Gale Dixon to prepare a report on geographic research. We also approached several linguists who have worked in Borneo for an assessment of the state of linguistic research, but we were unable to elicit any interest in this task, which in itself is a statement of the status of this discipline.

The symposium was unexpectedly well attended by scholars from all disciplines, and the discussion after the presentation of each paper was lively and productive. We will attempt to outline here some of the major themes that arose from the papers and summarize the results of the discussion. Before doing so, it should be noted that all of these papers were written prior to the untimely deaths of M. Jaspan, Tom Harrisson, and Bishop Galvin. University affiliations of the participants are those at the time of the conference.

†This Introduction was written in consultation with Leigh Wright.

The Present Status of Social Science Research

An underlying theme around which the symposium was planned was interdisciplinary contact. Indeed, as the discussion of papers progressed it became evident that the interdependence of scholars and the cross-fertilization of ideas across the several disciplines was central to successful research in all disciplines. One example of differing points of view concerned the value placed on oral tradition by historians, on the one hand, and by social historians and anthropologists, on the other. Although the discussion showed quite clearly that there are wide areas of fundamental concurrence regarding assessment and analysis of taped interviews, for example, disagreement remains over the reliability of conclusions based on oral accounts. And yet, as one historian pointed out, it is imperative that political historians of Borneo, and indeed of Southeast Asia in general, use the results of anthropologists' in-depth research on ethnic groups much more than they have.

This brings us to one of the unanticipated conclusions of the conference. Most of the papers and the discussions following pointed out the critical and generative role that social-anthropological research plays in the development of the other social sciences (cf. Appell's paper for a discussion of some of the possible reasons for this). Without anthropological data, theory, and perspective, most of the other disciplines, it was concluded, are handicapped in reaching sound scientific conclusions. This suggests the importance of continuing support for anthropological inquiry.

It was also concluded, however, that social science research in Borneo has hardly begun. All the social sciences are in their nascent stage. There are several reasons for this--attitudes towards social science research on the part of local governments, lack of funds, and lack of indigenous research personnel. These themes will be explored more fully below. Other themes that arose included the lack of input from social science research into development planning, the speed of social change now taking place in Borneo, and the resulting loss of knowledge not only for the peoples of Borneo but for all mankind.

Attitudes of Local Governments toward Social Science Research

A major concern expressed in both the papers and in the discussion was the attitude of local governments toward social science research, specifically research that involves fieldwork and contact with ethnic groups or segments of plural societies. Attitudes vary greatly, but in general, research is not encouraged. Many social scientists find this position difficult to understand. They believe that research not only contributes to basic knowledge, but that it also has very practical applications. However, they have a hard time engaging governmental representatives in discussions over these issues.

Certainly, where there are sensitive political issues, discouraging any research efforts may be justified. Yet some believe that such an approach could actually blind the government and make the situation even more sensitive, since the government must then act with limited knowledge. Thus, without understanding, the situation can progress to one of spiraling tension. There is no question that under such circumstances a government should be very wary of the type of researcher they accept. Some social science researchers just do not exhibit the degree of sensitivity

to local political tensions that they should. Nor is there any question that the methods used in research should be subjected to some scrutiny. For it has been the experience in every region of the world that those researchers who use survey instruments, schedules of questions applied to a population at random, instead of obtaining first-hand familiarity with a population through intensive fieldwork, can sometimes raise questions which are not only insulting but highly disturbing. In such sensitive situations it would seem more appropriate for the government, with the help of the social science community, to select mature researchers and put certain controls on their research.

Attitudes toward Foreign Scholars

There are many reasons for this growing governmental resistance to research by foreign scholars. In addition to those discussed above, there is a feeling that research opportunities should be given only to local scholars. This view overlooks the problem that, were the number of social scientists in Borneo increased twenty-fold in the next three years, there still would not be enough scholars to study all the problems immediately critical to the social sciences, not to mention those that can contribute to formulating development policies and achieving an understanding of the ongoing processes of social change. In terms of anthropological inquiry alone, many indigenous cultures are disappearing at such a rapid rate without note or interest being taken that it is impossible, given the present distribution of resources in the *international* anthropological community, much less the local social science community, to study more than a minuscule fraction of them. This problem is no better illustrated than by a footnoted example in Rubenstein's paper, where she observes that the recordings of Melanau religious literature collected by H. S. Morris in the early 1960s were not recognized by her own informants in the mid-1970s.

In addition, the view of restricting foreign social science in order to reserve research for local scientists overlooks the fact that the materials of social science research are a constantly renewable resource. For example, not even two, three, or more anthropologists, no matter how skilled, can completely cover all aspects of a sociocultural tradition in their fieldwork. They cannot gather data on all questions of interest to the social sciences or of ultimate value to the world at large. Even if they could do so at any one point in time, the answer to one question immediately poses other problems to be studied, so that social science research, in fact, is a continuing search. This is particularly true in those instances where indigenous peoples are in the process of sociocultural change, which means that each new field study begins from a different base than the preceding one.

And finally, as Rubenstein nicely phrases it, a social scientist who studies his own society is doomed to only limited insight. It takes a "foreigner" to perceive and delineate the most subtle of values, the most deeply penetrating ones that in fact suffuse all throughout the sociocultural system, that is, those premises that are so fundamental to the world view that they are accepted without question. Without an understanding of these, the social fabric of a village, a tribe, or a nation cannot adequately be described or understood.

Thus, it is hoped that productive interaction can take place between local social scientists and foreign social scientists, both of whom can enrich their own research and their own lives by such interchanges. To nationalize scientific research and limit the potentialities for growth of knowledge is a backward step.

Selection of Social Scientists for Local Research

One of the critical problems facing local Bornean governments is the screening of applicants from abroad for local research. Not all applicants are equally qualified or adequately trained for field research; not all applicants exhibit those personality traits necessary to pursue research in the Bornean context; not all applicants balance the pull of their own interests, the demands of social science, and the impact of local interests. Part of the difficulty lies in the structure of academic research. The bald truth of the matter is that much of the fault lies with foreign universities, since there are no real controls within any social science discipline. Instead, the disciplines are represented only by university departments, which frequently have little contact with each other or little interest in issues aside from helping their own students through their doctoral research. As a result, inadequately trained and unqualified researchers appear in the field without the supervision that they need from more experienced researchers. That more disastrous blunders than those which do occur are not made more often is sometimes a miracle. Nevertheless, when researchers do less than an adequate job, it is a waste both of research funds and of the time and effort put into such research projects by local government representatives. For example, Appell reports several incidents where anthropological doctoral students went into the field and did their research without any attempt to familiarize themselves with the literature on neighboring ethnic groups, either to inform their own inquiry or to familiarize themselves with the thrust of inquiry in the area.

There is, of course, no easy solution to this problem. However, local governments could request evaluations of proposed research projects by representatives of the international social science community or organizations such as the Borneo Research Council. This was one of the purposes for which the Council was originally organized. The procedure of requesting interviews of applicants and reviewing projects by individuals locally available is commonly used by funding organizations and universities in screening their own applications.

The Use of Social Science in Policy Formation and Practical Affairs

Another theme that arose in the papers and discussion was the use of social science research for the formation of policy. Osborn points out that no state or province has adequately incorporated social science research into its policy formation for development. Concern is felt here, in the social science community, because major changes have been set in motion in the social fabric of these countries, some by encouragement, others more or less imposed on local peoples; failure to use the insights of social science could produce social and ecological damage which might never be repaired, and which might result in unexpected and ultimately unacceptable costs to the larger society (cf. Appell 1975). For example, in some instances it is believed that the national society will be more stable and more productive if all its diverse ethnic and sociocultural segments were homogenized into one common cultural system. This ideal might be referred to as the "melting pot model" which has proved so disruptive for American society. It has been shown that American ethnic groups have not fully assimilated into the larger society. Where ethnic identity is suppressed by the majority, it only creates disharmonic reverberations, as witnessed in the urban riots of the 1960s in the United States. The causes of the riots are partially to be found in the inappropriate belief that America did in fact produce a melting pot of ethnic and racial backgrounds.

Bedlington's paper carefully analyzes this problem. He states that conventional theories of nation-building and modernization assume that ethnicity will wither away under the impact of these processes. He argues that it can be equally posited that the very processes of modernization, through the increased physical integration of ethnic groups, actually exacerbates ethnic tensions. He argues further that attempts at ethnic homogenization through coercion generally fail and that the best course for new nations, and all nations, is to recognize that "ethnicity is not an aberration but a legitimate human condition."

Thus, an alternative model of cultural plurality, in which ethnic communities are encouraged to maintain their identity (such as is found in the national policies of Canada), would seem to be a more appropriate model for Bornean governments to consider in their social planning.

But, as Doering points out, social science research, particularly social-anthropological research, also has immediate application. This is particularly in the study of indigenous agricultural systems, in order to assess possible contributions in agricultural development and in planning and assessing the introduction of new agricultural techniques. Here is an area of minimal political sensitivity where social science input can make major contributions to increasing agricultural development and productivity. Yet it remains an untapped resource.

The Funding of Social Science Research

The funding of social science research in Borneo has come largely from external sources, and it has been less than adequate. Part of the problem lies in the failure of the social science community to take a sufficiently strong stand regarding the urgency of research in Borneo so that new sources might be found or funding redirected. However, external funding alone will not solve the problem. Internal funding, both private and governmental, is badly needed. The lack of adequate funding for research results from the failure of the Bornean states and provinces to recognize the contribution social science research can make to their development. Several interrelated processes contribute to the problem: the lack of interest in such research by local governments; the failure to develop a local social science community of sufficient size to encourage the development of internal sources of funding; and the lack of articulation between multinational corporations and the local community.

Borneo is rapidly becoming a major area of interest to the extractive industries operated by multinational corporations. Their goal, of course, is to maximize their profits. Since they are not integrated into the national social life, what ecological or social ruin they leave behind is of little interest to their boards of directors. The costs that eventually result from their actions, as for example the ultimate cost of a dislocated population, are not a concern of theirs, but rather must be borne by the national governments. The fact that these multinational corporations are using ecological capital and social resources as an unaccountable resource, a nonrenewable resource to be utilized and consumed in their extraction activities, has not yet fully been understood by either the multinational corporations or the local governments. The result is that these corporations, unlike in their home countries, have generally not shown sufficient concern for scientific and philanthropic interests and have not funded or supported social science research in Borneo.

Development of a Bornean Social Science Community

The need for an active Bornean social science community was stressed in much of the discussion. How such a community might be developed is a complex question, but a number of social scientists indicated a willingness to help in the effort. In fact, many social scientists doing Bornean field research have been willing to devote time to training local personnel or helping wherever they might be asked, such as lecturing at local colleges. There is, of course, a need for more museums, as indicated in Solheim's paper. To date there are only two museums of any note, but these are exceptionally fine institutions. There is also a need to develop local scientific societies. Major contributions can be made by informed and committed amateurs. The importance of such work was brought out both by Lockard in his paper on the importance of oral historical materials and by Solheim in his paper on the status of archaeological research. Foreign social scientists are willing to help in the development of local museums and scientific societies.

One suggestion was made in the informal discussion following the session. It was suggested that summer seminars might be one solution to the problems of creating greater interest and knowledge in the results of social science research, in stimulating local scientific societies, and in training local personnel in the social sciences. It was thought that various states and provinces might occasionally sponsor such summer seminars and send government personnel, teachers, museum staff, and others to participate in them. The problems of organization and funding remain unresolved. Solheim's paper outlined how cooperation with foreign archeologists might be organized to train local archaeological personnel through participation in an archaeological dig.

The Urgency of Social Science Research

Most of the papers and the discussants remarked on the urgency of social science research, a point which has been repeatedly mentioned in this Introduction. As one social scientist put it, "the name of the game is development and modernization, and let the costs fall where they may." Some researchers, such as Appell (cf. 1975), believe that development and modernization can have totally unexpected and unanticipated costs when policy is not enlightened by social science insight, but that it can progress with a minimum of social dislocation and social harm when properly planned.

Conclusion

In the meantime, much of Bornean culture is disappearing unnoticed. The result is both an irreparable loss of knowledge and unneeded and avoidable expenses for the developing nations involved. The full costs have yet to be accounted for. It is of some urgency that local and foreign scientists, local governments, and funding institutions work closely together and cooperate to improve our research effort so that such potential contributions to knowledge will not be lost.

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BORNEO ARCHAEOLOGY, PAST AND FUTURE

Wilhelm G. Solheim II
University of Hawaii

History of Archaeological Research

Tom Harrisson and I have been corresponding acquaintances for twenty years and friends for fifteen years, but this is the first time we have exchanged roles. Harrisson must feel as out of place in being a discussant of someone else's paper on Borneo archaeology as I do in writing such a paper for which he will be the discussant. As far as I know, Harrisson has been controversial all of his life, and certainly his career as Curator of the Sarawak Museum and Government Ethnologist was no exception. But no one can deny his preeminence in the field of archaeology in Borneo during the past thirty years of his life. He conceived of and gave birth to organized archaeological programs in western Borneo and was the architect not only of an extremely healthy museum program in Sarawak, within which the foundation of Sarawak prehistory was framed, but presented the model for and helped to start similar museum programs in Sabah and Brunei. My paper today would be vastly different if there had been no Tom Harrisson. Virtually everything having to do with archaeology which is of current importance in western Borneo is the result of Harrisson's work or is directly associated with it. His influence on future research is paramount, not only because of the foundation he laid, but also because of his continuing contribution to the building of that foundation through his present research and writing. His active participation in the field continues in Brunei, and to a lesser extent in Sabah.

Before Harrisson, there was a virtual void in the Borneo area as far as prehistory is concerned. A few prehistoric artifacts had been collected, by I. H. N. Evans (1913) among others, and some of these were in the Sarawak Museum, but that was all. This is not the place to describe the history of archaeological research in Borneo. There is no article or book to which I can refer for an organized history of this subject, but a general reading of the post-World War II issues of the *Sarawak Museum Journal* gives a reasonable idea of what went on. Harrisson has done some research on the earliest archaeological work and interest in Sarawak in connection with its involvement with the "missing link" and with Darwin, Wallace, and Huxley (1958a, 1958b). For those with an interest in the subject but who lack the time to go through the back issues of the *Sarawak Museum Journal*, I here list authors and include in the Bibliography a few of their more relevant papers: Tom Harrisson (1954, 1955: xiii-xvi, 1958b, 1969a, 1970); Harrisson and O'Connor (1969a: 3-6); and Solheim (1961).

There has been virtually no work done in Kalimantan. Van Heekeren, in his two-volume summary of Indonesian prehistory, mentions only two publications with information on Kalimantan prehistory, neither of these involving excavation (Van Heekeren 1958: 63; 1972: 44-46).

Present Status of Archaeological Research

There is no need to summarize the findings of prehistoric research in any detail at this time, because there are two recent summaries for the three areas where such would be useful: Sarawak, Brunei, and Sabah. Harrisson's primary summary (1970), "The Prehistory of Borneo," was first written for presentation at a meeting in Philadelphia in 1965 but was extensively revised in the spring of 1970. It covers the whole of Borneo and is the only fairly detailed summary for Sarawak and Brunei. A much more detailed summary was done just for Sabah by Tom and Barbara Harrisson (1971). A brief summary of Sarawak archaeology by Chêng Tê-K'un (1969) is of interest primarily because of its view of Sarawak as a virtual Chinese colony in the first and second millennium A.D., and of Southeast Asia as a Chinese hinterland.

Sarawak

Sarawak is the most extensively surveyed area for its size of any region in Southeast Asia. Besides this general archaeological exploration under Tom Harrisson's direction, two areas have been intensively explored and tested, and several major excavations have been made in both localities. The Santubong program, in the Sarawak River Delta, was concerned with a group of closely related sites that were involved in iron production and export for about 500 years, from the ninth to the fourteenth century A.D. The Niah Caves program involved multidisciplinary research on a complex of caves in a limestone formation near Niah, not far south of Miri, in the Second Division. These sites extend from 40,000 B.C., and probably earlier, to about a thousand years ago.

There is no one publication which draws together in any detail the research that has been done in either the Santubong or Niah areas, but there has been much written on both subjects. The one site which has been thoroughly covered at Santubong is the small cemetery at Tanjong Kubor (T. and B. Harrisson 1957; Solheim 1966). Much more detail has been presented by T. Harrisson and O'Connor in two volumes on the iron industry (1969a, 1969b) and one on gold and megalithic activity (1970). The latter book is one of a long series of publications by T. Harrisson that concern megaliths in western Borneo, including ethnographic papers on a living "megalithic" culture, and relationships with other areas. The papers on the Niah sites and associated subjects are very widely scattered, but probably a majority have appeared in the *Sarawak Museum Journal* starting with Volume 8, No. 12 (N.S.) in 1958. Only a minority of the Niah reports are strictly archaeology. Reports identifying and going into considerable detail on the animal bone recovered in several of the sites, primarily by Lord Medway, are probably the most numerous, but zoological reports on present-day aquatic and terrestrial animals of the area, ethnohistory, and ethnography and folklore of the people long associated with the caves are also included.

Work has continued in Sarawak since T. Harrisson's retirement and B. Harrisson's final work at the Niah Caves. This work is salvage archaeology in open sites, all in the First Division. All sites have produced porcelains, with Tang to Sung pieces at Gedong on the Sadong River (Nyandoh and Chin 1969); Sung and Sawankhalok at Bukit Sandong, Serian District; Sung and Yueh at Kampong Bundong, along the Batang Sadong; Sung sherds at Sungai Ensika Sebanan, Sadong District, about ten miles upriver from Gedong; and Ming sherds at Tanjong Sangidam, an old village site about half

a mile by boat from Gedong.[†] The Tanjong Sangidam site is the first from which post-Yuan Chinese ceramics have been reported in southwestern Sarawak, and it presents a problem which needs solving. Harrisson postulated a "Ming Gap" for the heretofore lack of Ming pottery in the area, while much early Ming pottery was found at Kota Batu at Brunei (1958c). His explanation for the lack of Ming porcelains was that the center of contact with China shifted from the Santubong area to Brunei with the end of the iron industry in Santubong (ca. 1350 or a bit earlier), and the Brunei center grew in power and importance. While one site with Ming porcelain at Tanjong Sangidam destroys the "Ming Gap," it does not change Harrisson's interpretation of the change in the center of gravity of economic activity from south to north, unless it should develop, from further excavation, that the site at Tanjong Sangidam was a major center. It is logical that there would have been a few small communities in the south that would have had something of value to trade in exchange for the Ming porcelains.

It is now the policy of the Museum to permit no archaeological work in Sarawak by outsiders until fully trained personnel are present on the Museum staff. When the Museum reinstitutes an active archaeological program under their own staff, it may be possible for foreigners to work as a part of the Museum program.

Brunei

Only two sites have as yet been located in Brunei, and both are in a sense historic, as they are associated with the Sultanate of Brunei. The first, discovered by T. Harrisson, is Kota Batu, the actual site of the ancient center of the Brunei Sultanate. The site is now owned by the government, and the new Brunei Museum is built on a hill overlooking it (T. Harrisson 1973a; Shariffuddin 1973). Extensive and intensive excavations will eventually be made, but there is no rush, as it is safe from harm. The first excavation, made by T. and B. Harrisson (1956), turned up quantities of porcelain and stoneware from T'ang, Sung, and early Ming dynasties of China. The resulting dates, from around A.D. 600 to 1400, are amply supported by dated coins and C-14 dates. According to T. Harrisson (1972, 1973b), two dates of unusual age, one of about 95 B.C. and the other about 12,500 B.C., suggest the possibility of earlier human activity in this area.

The second site is on the Sungai Lumut, about 48 miles by road from Brunei and three miles south of the sea. Considerable quantities of stoneware and porcelain were recovered, dating at the earliest from the second half of the fourteenth century A.D. and the latest from the very beginning of the sixteenth century A.D. (B. Harrisson and Shariffuddin 1969: 31). Very little earthenware has been recovered from this apparent burial site so far, and none of that recovered appears to be made locally. The one almost-whole vessel (restored) was decorated with a carved paddle (B. Harrisson and Shariffuddin 1969: 48 and Plate XXIXb). It is typical "Bau-Malay" pottery, similar in form and general decoration to vessels I have reported from Bohol in the Philippines and from West Malaysia (Solheim 1967: Plate III d and f; Solheim and Green 1965: Figure 16 and Plate XIIIN). But it is no more similar to the "Bau-Malay" pottery recovered from Tanjong Kubor in Sarawak (B. Harrisson 1969: 47; Solheim 1966) which is geographically much closer. This kind of "Bau-Malay" pottery, and particularly this type of vessel, may well have been involved in trade, possibly as a container of some sort--with its low maximum diameter relative to its total

[†]I thank Lucas Chin, Curator of the Sarawak Museum, for this information on recent work.

height, it is a very stable vessel that would not easily tip over. The materials associated with the burials in this site indicate that the people living here were not Moslem. B. Harrison and Shariffuddin point out (1969: 35) that the existence of such a sizeable pagan population close to the Brunei capital during the fifteenth century is of particular interest. A third potential site, where the golden hoard of Limbang was found, was never located (T. Harrison 1969b).

Sabah

Very little needs to be said about the present status of archaeology in Sabah. This has recently been covered by T. and B. Harrison in their book, *The Prehistory of Sabah* (1971). I have reviewed it twice (1973a, 1973b) and there is no reason to review it again here. It is worth restating, however, that the prehistory of Sabah does not duplicate that of Sarawak. Relatively little is known of Sabah's prehistory, but the extensive surveys done by the Harrissons have revealed enough sites to show that Sabah has great archaeological potential and that it differs considerably from Sarawak. The megalithic activities apparent in both are possibly the major point of comparison. These are well covered in the book and discussed again in a recent article by T. Harrison (1973c). Another article reports a large incised pictograph with curvilinear designs on a large boulder in the Ulu Tomani in the Tenom Interior Residency (T. Harrison 1973d). For relationships beyond Borneo boundaries it is well to note Spoehr's recent work in Zamboanga and the Sulu Archipelago (1973) and the joint project of the Philippine National Museum and the University of Hawaii to explore along the coast of southeastern Mindanao. Nothing has been published on the latter work as yet, but the final report is in preparation. Both areas certainly have relationships with Sabah.

Kalimantan

The Indonesian portion of Borneo is notable primarily for its lack of archaeological exploration. A few questionable flaked stone tools have been reported from southeastern Borneo. Typologically, these would appear to be lower or middle paleolithic chopper/chopping tools; but they were found on the surface, and so their dating is not known (Van Heekeren 1972: 44-46). Megaliths have been reported in some variety in Apo Kajan, in several different locations (Van Heekeren 1958: 63).

Directions of Future Research

There are a number of general problems that need to be solved for Borneo prehistory, problems that occur in any area where archaeological research is relatively new. For most of Borneo, the time and space framework is still largely unknown. For the most part, we still do not know what happened where and when. While this framework is being developed, we can also be studying the internal relationships of the different archaeological cultures within Borneo and the external relationships with neighboring Island Southeast Asia and with the mainland. Finally, within the area of present-day archaeology's primary focus, we can be working on cultural reconstruction--both ecological and social factors--of individual sites or groups of sites. While research which brings together data developing frameworks and for working out relationships could continue in much the way it has been carried on during the last decade or so, cultural reconstruction requires new techniques of excavation and a new approach to

analysis. Luckily, Harrisson, with his broad approach, has been gathering much of the sorts of data needed, particularly at Niah. As each political area of Borneo now has a different store of prehistoric data, it is easiest to look at each entity separately.

Sarawak

The last twenty years of archaeological research in Sarawak have made it possible to develop a reasonably trustworthy time and space framework. Much of the requisite data has been published only in preliminary form. Harrisson has recently informed me that he will continue to work about six months each year on the Sarawak data (and on Borneo in general), and publish his results. He has most recently completed two major papers on the Paleolithic and is now working on the first full monograph on Niah.

However, the study of the relationships between Sarawak archaeological sites (and cultures) and those elsewhere in Borneo, neighboring Indonesia, and the Philippines, must await data from excavation in those other areas. Relationships between the peoples who utilize the Niah caves and those who used the Tabon caves in Palawan can be worked on after the data which is already available on them has been published.

With the broad range of data that Harrisson has for Niah, he can begin an ecological reconstruction whenever he likes. While it is likely that considerable social reconstruction will be possible, it is likely that further excavation using new techniques will be needed if results are to be maximized. As both the Santubong and Niah sites are well protected, there is no hurry. The wiser path is to wait until Harrisson has published his major reports on both areas and then work out the next stage of approaching the new problems his reports will present.

Brunei

The small area of Brunei and the two known sites suggests a two-pronged, long-term program. Of first and immediate importance would be a systematic survey of the whole country in conjunction with a public education program. Depending on the amount of funds which could be made available, it should be possible to plan a program which could cover the whole country in from two to five years. In the process, local peoples should be informed by those people making the survey of the importance of reporting accidental finds to the Brunei Museum. Probably relatively little would be found in the survey, but a reporting infrastructure could be set up through the schools and community headmen that could bring all accidental finds to the attention of the museum quickly.

The second program would be a long-term intensive excavation program to do exceedingly careful excavation in the known sites, for purposes of cultural reconstruction. Excavation could proceed in such a way that the excavation itself would be a living museum for explaining to the Brunei people what archaeologists are seeking and how it is important for them. At the same time, these ongoing excavations would be tourist attractions, and the on-site museums would help to explain the life and history of the Brunei people to the tourists and to the people themselves. Exchanges of artifacts between the Brunei Museum and the other Borneo museums would enable exhibits demonstrating relationships between Brunei prehistory and that of surrounding areas.

Sabah

The major program for Sabah is to develop the time and space framework of its prehistory. This must be a combined program of exploration and small test excavation like that begun by the Harrissons. A public education program and an infrastructure for reporting accidental finds to the museum should be included. Studying relationships with outside areas and cultural reconstruction will have to wait until at least a tentative time and space framework has been developed and a fair quantity of data accumulated. Except where a site is endangered by some form of development, intensive excavation in single sites should be delayed until the total area is moderately known, so that intelligent selection of significant sites for excavation can be made.

Kalimantan

The prehistory of Borneo as a whole cannot be worked out without knowledge of Kalimantan's prehistory. If it were possible to plan and develop an organized program for investigating the prehistory of all of Borneo, the first priority would go to exploration and testing of sites all over Kalimantan. This is a very large area, but it is not large relative to the size of Indonesia as a whole. This means that it will be a long time before any sort of organized program can be started with Indonesian government funds. Impetus for such a program might therefore best come from within Kalimantan itself. Possibly the best approach would be by opening a museum in one of the coastal towns and by starting a branch of the Archaeological Institute of Indonesia within the museum, using moderate local funds.

The exploration program should cover the coast first, and then work into the interior along the major rivers. There are systems of large limestone caves in Kapuas and Bahew like those at Niah and in Sabah. Such known cave complexes should be tested as soon as possible. The results of fieldwork in Kalimantan are not only of importance to the other areas of Borneo, but to the Philippines as well. Perhaps a cooperative international program involving Indonesia, Eastern Malaysia, Brunei, and the Philippines could be developed within three to five years, a program in which both funds and personnel (including a training program) could come from outside and be joined with Indonesian resources.

Implementation

It is all very well to talk about what needs to be done, but if plans are made without thought being given to practical implementation, little, if anything, will develop. All of the countries involved in Borneo have the same problems in slightly varying degrees. The two major problems are budget and personnel.

Budget and personnel would appear to be two separate problems, but they are so closely interrelated that it would be better to treat them as one. A trained staff cannot be developed without an adequate budget, and a budget is not of much use without a trained staff. The two must advance together in phase.

In the United States, much archaeology takes place within a university framework. With few exceptions, this is not the case in Southeast Asia, and it is certainly not the case in Borneo. Here archaeological work is

done in conjunction with government-financed museums. The strongest argument for convincing any institution to spend money is to show that its return will be equal to or greater than its outlay. There are two areas of return for museums and associated archaeological programs. The first, and somewhat easier to demonstrate, is in the area of tourist development. Good museums are good tourist attractions. While it is an unusually good museum that can be a primary tourist attraction, there are such museums. Without question, there are some people who visit Sarawak primarily to visit the Sarawak Museum. Archaeological excavations are interesting not only to those directing and doing the excavation but to spectators (tourists) as well. An alert museum staff can include tours of excavations in progress as a part of a living museum program if they plan well and in advance. Interesting sites that are not too difficult to reach should be developed into permanent portions of a museum program. Eventually, this sort of site preparation can pay for the excavation of that site and perhaps even provide some profit.

The second area of return on investment of a museum-archaeological program is more difficult to demonstrate, but it is probably more important to a country in the long run: educating the people of that country, and that area, that they have a common and related background. This helps to develop a positive self-image for a person as a citizen of a country whose people have a common background and tradition, and it facilitates working together for national and regional goals. Archaeological research can produce the data to demonstrate common backgrounds, and by publication and museum display such data can be presented, as a story, to the public.

This brings us to the question of training personnel. The best and only training which is adequate for an archaeologist is to work together on projects with good, professional archaeologists. The necessary background for this training is available at universities in Indonesia, Malaysia, Thailand, and the Philippines. If an international program were developed in Kalimantan, as I have suggested, it could be used as a primary training program for the area. A long-term program of training and archaeological research could receive moderate funding from the local governments involved. It should be possible to obtain some funding for such a program from one or more American and other foreign foundations. By inviting foreign universities to send advanced graduate students to take part in this program as part of their doctoral studies, these students could bring further funding with them to help support specific phases of the program. In working with these students, the local students would intensify their own learning situation.

I am sure that all of this sounds like a figment of my imagination. But it could be done. Something of this nature should be done in one or two areas in Southeast Asia, and there is no reason why Kalimantan could not be the place.

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BORNEO ARCHAEOLOGY IN THE FUTURE:
COMMENTS ON DR. SOLHEIM'S PAPER

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It is difficult for Britons (famous for shyness) to follow up the flattering remarks of American friends. But such is my appointed task, as discussant.

Bill Solheim has indeed been a friend, not only personally, but to archaeological work in Sarawak as in all Southeast Asia. In particular, he helped elaborate the Sarawak Museum's work in that field during the time (somewhat longer than a year) when he was at Kuching on a Fulbright Fellowship; he was there especially to analyze our rich earthenware material obtained by excavation. He did a great job, as usual, not only with his own work but in stimulating the rest of us to think and, if necessary, re-think.

The more thoughtful aspects of our work as a whole were not expressed to any degree in the material published before the writer officially retired in 1967. But with time since then having been free of administrative and other duties, plus the continuing opportunity to visit Borneo annually, there has been more time for writing general, in-depth papers and monographs. It is intended to expand and accelerate these in the near future, with special reference to the Niah Caves (a series of full reports is planned); to the open sites and their rich ceramics (B. Harrisson has a grant to devote most of 1975 to "exportwares" in Borneo and at Cornell); and the wider implications of earlier Bornean prehistory, with reference to other islands and the adjacent mainland--paperston this topic, resulting from the Palaeolithic conference at Montreal (August 1973) and Groningen, Holland (May 1974), are now in press. It is thus hoped that more than twenty years of active fieldwork in Sarawak and elsewhere will be rounded off by adequate factual and theoretical accounts on a comparable scale.

Now to turn to the future in the field. Clearly Solheim is right in putting emphasis on the training of Borneans to do their *own* fieldwork. But only in Brunei does it appear that this goal might adequately be achieved in the near future. In Sabah and Sarawak--as previously in Singapore and Malaya--good local people were trained and qualified to dig, but they then left, either for lack of opportunity to do meaningful work or for lack of recognition and any prospect of a career. These problems must be faced and resolved in advance. There is no point in training more Malays, Chinese, Dayaks, or Kadazans, if all the effort and money spent is for nought. The status of museum staff and the upgrading of research personnel--as has been done in Brunei, and is pending in Sarawak--is involved here.

In connection with Dr. Solheim's remarks on Brunei, there hardly seems to be need for the survey he suggests there. Much of the state's limited terrain is of *very* recent geological origin. The rest of it has already been quite extensively explored, in earlier years by Sarawak Museum personnel and more recently by the Brunei Museum's own capable staff (aided with

helicopters where necessary). A "reporting infrastructure" already exists. The museum is now a dominant feature in this small, progressive stage, where the inhabitants are exceptionally well oriented to reporting *anything* of interest to the Curator promptly.

For the rest, his suggestions are eminently sensible. One would only *emphasize* the urgency of an intensified program for Kalimantan. Virtually no archaeology has been done in this, the largest of the four Borneo territories--and the one with easily the largest archaeological potential, especially Palaeolithic (for geological and for demographic reasons). The caves of the Apo Kayan also offer a unique chance in Borneo for large-scale excavation far inland, where the hitherto apparent lack of any strong stone-age evidence remains tantalizing (cf. the much *more* inaccessible area of central New Guinea).

I therefore associate myself warmly not only with Billts lively overall approach, but with his last sentence, which is quite outside my own personal interests. The ex-British states can go on very well as they are for years to come. Kalimantan cannot. As communications improve, looting of open sites and caves has already begun. The issue is urgent, the reward possibly enormous, in Kalimantan.

On a wider aspectt in Southeast Asia, a great deal of time, enthusiasm, and hard cash has been expended looking for the right thing in the wrong place; or, worse still, finding the right place but not looking carefully and *deeply* enough. Many sites have been spoiled, as well as incorrectly assessed, by too hasty examination--usually by people with limited time (not necessarily limited funds)t A much more *solid* gradualist approach is overdue; from the beginning we tried to apply this approach in Sarawak, then Brunei, then Sabah. Therefore, although in Kalimantan the situation is serious, it must be tackled *seriously*, too--not rushed at with a sort of "treasure-hunting" mentality, however clothed by high academic title or elaborate short-term expeditionary facade. The greater distances and poorer communications in the Indonesian part of Borneo make cost-effectiveness all the more important. Relevant here, also, is the precise definition of goals. It is no good simply digging at any and every cave or gravel-bed or open site showing early signs. Otherwise, the effort will be diffuse, its cost-effectiveness low. Criteria must be drawn up and decisions fully based on the problems faced and the total resources available. (My own experiences in Sarawak may be useful, and I shall be delighted to help.)

One other interesting point raised by Solheim: the "Ming Gap" and recent finds at Tanjong Sangidam. No one suggested there were *no* Ming ceramics to be found in the ground in southwest Sarawak. The Sarawak Museum has excavated a good many of them, including some on a small open site up the Sarawak River above Kuching, and sporadically elsewhere. What has so far been conspicuously lacking is any quantity of post-Yuan until historic times. The great trading and early iron-working locations in the Sarawak River delta and elsewhere have all been pre-Ming. But there may well have been lesser outposts of later date; indeed, that would be logical. In fact, the absence, the "gap," is itself the anomaly. But before revising our wider views, surely we must await identification of the sherds from the new site, as reported by Mr. Lucas Chin. If his report refers only to blue and white, recent research has shown that this can be dated back to Yuan and even late Sung. All thus depends on the *exact* character of the material, and also on the size of the site and evidence there of a trade or other center. Cemeteries containing older Ming or Ming-like Ching jars and plates, which were often put there in quite recent times as burial furniture, exist all over and even continue into the present day. Is this

a Ming habitation site, or a more recent village or its cemetery? Mr. Chin's published report will be of special interest to this writer, who will be as delighted to stand corrected as to advise others on "correct" site-selection criteria (as above)! A C-14 date in support would seem desirable, if possible. It is a curious fact that since 1967 no C-14 dates have been added for Sarawak, though we have a long series since then for Brunei (as regularly reported in the *Brunei Museum Journal*; and see Harrison 1973). There is not a single C-14 for Kalimantan.

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THE PRESENT STATUS OF RESEARCH IN COLONIAL HISTORY OF BORNEO:
NOTES ON THE HISTORIOGRAPHY OF MALAY PIRACY AND THE
NATURE OF POLITICAL POWER IN BORNEO

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In the paper which follows I deliberately departed from the pattern of the other contributions, as suggested by the title of the symposium, and have described some problems entailed in my own research in order to demonstrate how the traditional colonial historian might make more use of the growing body of work of anthropologists and social historians. Political historians have tended too frequently to neglect the work of other disciplines in Bornean studies. Yet, in the interests of interdisciplinary cross-fertilization, anthropologists and historians need to build upon each other's work. Thus, I look to Kieffer, Sather, and Brown to help me with my research into piracy and the sources of political power.

The handful of colonial and political historians of Borneo are widely scattered. Graham Irwin (*Nineteenth Century Borneo*) and K. G. Tregonning (*Under Chartered Company Rule*) now work in other fields. Since their work in the 1950s, two contributions have appeared covering the development of the British hegemony in Borneo. My book, *The Origins of British Borneo*, takes British colonial policy in Borneo from its origins to the establishment of the protectorate in 1888. Colin Crisswell's Ph.D. thesis, "The Establishment of a Residency in Brunei" (Hong Kong), overlaps with mine and carries up to the next milestone in British Borneo political history, the acceptance of the residency system in Brunei. Both works cover the whole area of Sarawak, Brunei, and Sabah. Crisswell is in the process of revising his thesis for publication and is at work on a biography of Rajah Charles Brooke. In addition, Nicholas Tarling, of the University of Auckland, has recently published his hefty *Britain, the Brookes and Brunei* which proposes some unique theories about British intentions in Borneo, especially during the last quarter of the nineteenth century. I hope to have published soon an annotated collection of treaties and other documents covering the whole colonial period from 1761 to 1963. This collection is planned as a reference work and will serve to update the Borneo portion of W. G. Maxwell and W. S. Gibson, *Treaties and Engagements Affecting the Malay States and Borneo*, which was published some fifty years ago.

The historian of the colonial period also welcomes the appearance of reprint publications of some of the important early accounts of Borneo by Europeans. Oxford University Press has produced George Earl's *The Eastern Seas* and John Crawford's *A Descriptive Dictionary of the Indian Islands and Adjacent Countries*. Both contain observations on nineteenth-century Borneo which are still invaluable.

Several young historians have also taken up Bornean studies. Ian Black at Monash University is interested in Sabah under the British North Borneo Chartered Company rule; J. E. Ingleson, also of Australia, has done his M.A. work on Rajah James Brooke; Ann Reber, who worked at Cornell, and Keith Reynolds, of Wisconsin, have both done research for M.A.'s on Sulu-Borneo connections.

I

This essay will be confined to discussing sources and setting out some of the problems involved in research on the subject of Lanun piracy, and to the formulation of certain statements on the nature of political power in the area.

It would be an exaggeration to suggest that trade and political interaction between neighboring states did not exist along the coasts of northern Borneo in the period before the consolidation of Chartered Company rule in the 1880s. However, it is a truism that commerce and political intercourse suffered grievously from the disruption of marauding bands of Lanun pirates, both along those coasts and in waters far to the southward in the South China Sea and the Macassar Straits. Lanun may have indulged in their great marauding cruises around the whole island of Borneo as early as the middle of the fifteenth century. Paul Wheatley (1961) notes that there were "pirate lairs at the strategic meeting point of the China and Java seas" at Singapore and Bentan at that time. He cites Tomé Pirez, who tells of pirate activity almost a century later in the area of the Anambas Islands.¹ We have no proof at hand that such pirates were Lanun, but it is tempting to speculate that this area, which in the period from about 1750 to 1880 became the yearly vernal hunting grounds of the Lanun warriors, was already utilized at an earlier period.

In attempting a survey of Lanun piracy in Bornean waters and the political ramifications of this piracy during the heyday of Lanun activity (the century or so before the coming of the British North Borneo Chartered Company), one faces a complex set of factors involving the nature and origin of societies in northern Borneo and the Muslim Philippines. The historian dealing with northern Borneo and Sulu is indebted to the handful of anthropologists who have given recent attention to such people as the Tausug, the Bajau, the Idahan, and several mixed communities in the area. Without their insights, the historian's picture from written sources of evidence, would be most inadequate.

It is quite apparent that strong government was absent from the coasts of North Borneo during the period from the mid-eighteenth century to the 1880s. The empire of Brunei had declined, and the Sulu authority was, by the first half of the eighteenth century, already confined to the island seat of its Sultan. None of the indigenous *datu* of the coasts were able or energetic enough to influence affairs much further afield than their own river village or island settlement. Displaced Lanun from their homeland in Illana Bay in the sphere of the old Sultanate of Magindanao, found convenient homes on the coasts of Borneo and in the islands of the Sulu Seas. During the first half of the nineteenth century they were well ensconced in half a dozen riverine states on the northwest and northeast coasts.

II

Marauding was, of course, not a new occupation for inhabitants of Borneo, nor was it confined to Bornean waters. Observers of Southeast Asia from earliest times note the prevalence of piracy at certain periods. Some of these early travellers from whom we have records were Chinese Buddhists

¹Wheatley 1961: 328; A. Cortesão (1944). See also Hall (1968: 339) for a note on their activity in the area in the next century.

on their way to and from holy places in north India. Fa-Hien, as early as the fifth century A.D., wrote of the danger of travel in the Malacca Straits because of piratical tendencies on the part of the Malay inhabitants of the coasts of Sumatra and the Malay peninsula. "This sea is infested with pirates, to meet whom is death," he wrote in his narrative (Beal 1869: 167; Wheatley 1961: 38). L. P. Briggs (1951: 1) says that somewhat later the Malacca route was "practically closed" to east-west traffic, the merchants preferring a track through the Sunda Straits and along the west coast of Sumatra.² And he noted that the Indian trade settlements in Java and Sumatra declined. Wolters (1967: 152) notes the prevalence of pirates off the Indochina coast and their interference with Funanese trade with China.

Later, the kingdom of Srivijaya was able to dominate the waters of the Straits from the eighth to the twelfth centuries and keep piracy under some measure of control. In his recent account of Srivijaya, Wolters (1970: 15, 39) notes the lack of references to piracy in Arab sources for the golden period of Srivijaya, and he speculates that the rulers of Palembang retained the loyalty of their vassals in the islands off the coast and in the Riau Archipelago by a generous bounty. The island chiefs, in turn, found it in their interests to keep out marauders and maintain peace. "The efficacy of the royal bounty had absorbed potential pirates into the Maharaja's service, and this meant that less loyal subjects would not dare to molest the China trade" (Wolters 1970: 39)e

Nevertheless, the previous situation of widespread piracy must have continued well into the period of Srivijaya-Palembang, for another Chinese pilgrim, Chia-Tan, observed late in the eighth century that the inhabitants of the Straits of Malacca "are mostly pirates" (Wheatley 1961: 56-57)e

As the hold of the Srivijaya prince over his vassals weakened in the twelfth and thirteenth centuries, there was a recrudescence of piracy in the area.³ The decline of the authority of Srivijaya, writes Wolters, "gave rise to rulers who made a living by trade and piracy" (Wolters 1970: 79), and in 1340 the most famous of all Arab travelers of the day, Ibn Battutah, observed several junks "preparing to make piratical raids" in the northern regions of the Malacca Straits (Wheatley 1961: 226)e

In the period during the long decline of Srivijaya and before the consolidation of Malacca's place in the maritime dominance of Malay waters, trading and travel were once again extremely hazardous because of the sea robbers. Singapore harbor was "a hotbed of pirates" in 1350, and some 300 marauding *prahu* operated out of that place (cf. Chen Ho's voyage in Mills 1970; Wheatley 1961: 81)e If, as seems probable, this pirate lair was utilized by Parameswara and his "band of Bugis corsairs" (Tomé Pires in Cortesão 1944 II: 6; Wheatley 1961: 307) as a base of operations before he moved up the coast and founded Malacca, it would seem to support the theory of some writers that piracy was, at times, a sort of intertribal warfare, carried on as a means of political gain, and the natural way for a local chief with some ambition to attain power and the wealth to sustain that power (cf. Hill 1919; Wheatley 1961: 307ff.; Wolters 1970: 177-78; Tarling 1963: 2, 8; Meilink-Roelofs 1962: 28-29; Leur 1955: 105n92)e

That piracy was a symptom of the decline of central state authority, as in Palembang in the second half of the fourteenth century, seems evident. But piracy was also the precursor of the respectable and civilized state.

²But see Wolters (1967: 267n)e who thinks that the Sunda Straits were not used regularly in "ancient times."

³Wolters (1970: 45) cites Arab writers such as Mahammad Ibn Idrisi and Ibn Said.

"By the early fifteenth century," writes Wolters, "Palembang had degenerated into a pirate lair, but the pirates of Malacca had become respectable harbor princes" (Wolters 1967: 225).⁴ When Malacca then assumed maritime power in the Straits and pursued legitimate trade, it became necessary to suppress piracy and marauding on the peninsula coast of Malaya and in the Straits. Malacca's success in the role formerly played in those waters by the princes of Srivijaya must have been less than complete, for Singapore, or Tumasik as it was known, remained a pirate rendezvous when Cheng Ho touched there in 1413 (Wheatley 1961: 91). His countryman Fa Hsin offered a similar observation of the place in 1436.⁵ It is quite possible that, for most of the five hundred years between the decline of Srivijaya and Raffles's founding of the modern port city, Singapore was a pirate "state" pure and simple; the same could be said of the small kingdom of Aru on the Sumatran coast of the Straits during the period after the decline of Srivijaya and well into the eighteenth century (cf. Cortesão 1944: 146-48; Marsden 1966: 419-20).

What happened in the wake of the fall of Malacca to the Portuguese helped create the complex political situation in the Malay world that confronted Europeans, who appeared in ever-increasing numbers during the following centuries. While Johore and Aceh competed for the honor of the mantle of Malacca, other local states and harbor principalities continued on the same more-or-less independent course which they had followed even as nominal vassals or respecters of Malacca. The only differences were the absence of the vague and always lightly-held suzerainty of the Malaccan Sultan and the occasional visits of European merchants. Even some petty village *datu* now began calling themselves "sultan." Among the local states which emerged under influence of Muslim chiefs were tiny riverine polities and large maritime and coastal empires. The Brunei Sultanate, centered on the northwest coast of Borneo, was of the latter type. Brunei rose as a Muslim sultanate in the fifteenth century. By the early sixteenth century, it had colonized into the Philippines as far as Manila Bay and influenced and allied itself with the chiefs of the Sulu Archipelago and Mindanao.

With the dawn of the early modern age (from the viewpoint of Western periodization) and the European advance into the Southeast Asian archipelago, we begin to have more than just fragmentary accounts of Malay Muslim pirates. Tomé Pires was one of the first European observers to describe the Bugis from the Celebes as pirates. "These men . . . are greater thieves than any in the world, and they are powerful and have many paraos" (Cortesão 1944, I: 226-28, II: 250ff.). These "Celates," as Pires termed them, were displaced from their island homes by the Portuguese, and they plundered as far afield as Pegu in Burma, through the Moluccas, and all around Sumatra. They held an annual slave "fair" at Aru. During the sixteenth century, the Bugis, ensconced in the Riau Islands at Linga, were "obedient" to the Sultan of Malacca and often served him as rowers. At other times their obedience did not interfere with their raiding. British and Dutch accounts seem to confirm this role of the Bugis up to at least the end of the seventeenth century (see, e.g., Foster 1934; Moreland 1934;

⁴See also Meilink-Roelofs (1955: 8). Mrs. Meilink-Roelofs notes that Palembang had fallen into the hands of Chinese corsairs, who, in alliance with local Malays, "rendered the sea route so unsafe that it retained little attraction for foreign merchantse" Admiral Cheng Ho destroyed a pirate force under one Chen Tsui-i at Palembang on his first voyage in 1407.

⁵J. V. G. Mills (1970: 326-28). In 1618 the Chinese voyager Chang Hsieh noted the "multitude of robbers" in Lang-ya Strait (Lung-Ya-Hsi-Chiao is identified as Langkasuka, hence the Malay Straits [Wheatley 1961: Ch. 16] or more specifically, Singapore Straits [Wheatley 1961: 91]).

Foyle 1929), after which these consair chiefs apparently settled down and became legitimate traders and politicians in Malaya and Sumatra.

In 1521, Pigafetta encountered pirates in the southern Philippines and earlier noted in passing the "Island of Thieves" in the Marianas, to the east (I: 32-33). The lack of reference to Lanun pirates in Pigafetta's narration could lead one to assume that these people had not yet embarked upon their annual cruises but were for the moment contented fishermen and farmers under their sultan in Magindanao, which Pigafetta called "a great city" and "principal Malay settlement in the southern part of the island of Mindanao" (I: 1).

Spanish accounts of piracy are so intertwined with narratives of the Moro Wars, which began around the 1580s, that one is inclined to grant some credence to Van Leur, Tarling, and others who contend that much of what was termed piracy by Europeans was considered warfare by native princes. The conquest of the Muslim Philippines is chronicled in E. H. Blair and J. A. Robertsons lengthy work, in which they draw from earlier (mainly Spanish) accounts (1903-9). The pirate wars are given full coverage by Spanish observers and participants of especially the sixteenth through eighteenth centuries. Particularly enlightening are the histories of Vicente Barrantes (1878), Emilio Bernaldez (1857), and Jose Montero-Vidal (1894). The Moors used tactics in fighting against the Spanish forward movement which the Spaniards themselves later employed to good advantage--the rapid thrust by sea and land, slaying and enslaving, and laying waste civilian settlement and military installation alike. In this fashion, Mindanao chiefs ravaged the Visaya for slaves and loot, and the Spaniards gradually conquered Mindanao. After the Sultan of Magindanao made peace with Manila in 1794, the Spanish islands decreasingly served as marauding grounds for his Lanun subjects. They concentrated their efforts in Borneo waters and on the annual cruises which took them as far as Riau and the Anambas Islands. The first Lanun raid at Banka is recorded in 1790.

Only in the eighteenth century do English and Dutch voyagers and writers give any great detail of geographic, political, or social conditions in Borneo and the Philippines. Despite the Dutch and Spanish monopolistic tendencies in commerce in the East Indies, and helped by a favorable European political situation in the late eighteenth century, Britain edged into Southeast Asian trade. She was motivated, of course, by the growth in the China trade and the need not only to service the tea and silk investment at Canton by entrepot operations in the eastern part of Southeast Asia, but also by her desire to circumvent the *cohong* system by trading directly with the junk merchants from the east-central coastal ports of China.

The coasts of Borneo and the islands of Sulu were only very sketchily known to English navigators until the eighteenth century. Seventeenth century navigational maps leave the eastern and northern coasts of Borneo largely blank, if they are drawn at all. But with Alexander Dalrymple's numerous voyages to the area beginning in 1759 (Dalrymple 1774, 1782, 1778; cf. also Rennell, edited by Harrison 1966), first-hand accounts of voyagers and their adventures multiply. By the mid-1800s, there is a veritable deluge of books and articles on "further India." Almost every subject of Victoria who ventured into the islands as captain, passenger, or crew set pen to paper with a narrative of his voyage soon after returning home. Most of these accounts make interesting (if quaint by today's standards) reading, and contain "factual" information of varying degrees of creditability. Almost all of them are valuable for the general purport of their observations and display varying degrees of sensitivity towards the native scene in the Indies. Voyager's accounts from this period depend heavily

upon the pioneering of Dalrymple and Thomas Forrest, sea captains and servants of the Honorable Company. The subject of piracy looms large in these narratives.^t Dalrymple noted particularly the propensities of the "Moorish Rovers from Magindanao" to disrupt commerce and trade, and histtreaties with the Sultan of Sulu in 1761 and 1764 included articles covering the mutual suppression of piracy.^e

Dalrymple and Forrest knew Sulu and Magindanao people intimately from having lived among them for some months. Forrest spent the period from May 1775 to the following January in and around Mindanao. Like Stamford Raffles and James Brooke, he achieved an empathy with Malays, especially with sultans and *datua*. He was decorated by Sultan Ala'uddintMuhammad of Aceh. Both observers refrained from the moral judgments on the life styles of the Malay ruling classes so freely indulged in by later Victorian observers. The coastline of northern Borneo was now surveyed from Unsang Point in the northeast to Point Datu in Sarawak. James Horsburgh, whose *Directions for Sailing to and from the East Indies and China* became the navigational handbook for generations of British sea captains, drew heavily upon Dalrymple's and Forrest's explorations in the area.^f

Britaints disastrous experience with her attempt to consolidate a trading position in northern Borneo at Balambangan at the beginning of the nineteenth century put an end to British trading enterprise on a profitable scale for several decades. Few British vessels ventured to the ports of northern Borneo and the Sulu Seas until mid-century. Horsburgh advised traders to keep away from the coasts of Borneo: "The predatory and treacherous disposition of the inhabitants of the extensive coasts that encircle the great island of Borneo have now discouraged almost every European from venturing to trade there. . . . There is no inducement for a ship to touch there (Marudu Bay), or at any other of the bays on N.W. or N.E. coasts of Borneo, the natives being inhospitable and perfidious.^t" William Milburn was presently also to make use of the surveys of Dalrymple and Forrest, and himself drew upon Horsburgh, concurring with the latter's views about piracy in Borneo and Sulu.^e

Perhaps the clearest and most descriptive account of conditions in northern Borneo following Dalrymple and Forrest's early surveys was that of John Hunt,^e who went to the area on a mission for Stamford Raffles, then Lieutenant Governor of Java. He made an exhaustive study of the people and politics around the coasts of northern Borneo. It is remarkable for its detail of places, names, estimates of populations in coastal ports, and for observations on trade conditions and the natural products and main trading commodities. Hunt's picture of Borneo is also remarkable for its accuracy. Later writers who were to spend their careers in Borneo generally corroborated Hunt's observations on piracy in Borneo waters, if at times disagreeing with his less than sanguine view of the political decay and decline of government in Sulu and Brunei.

While Dalrymple and Forrest noted the prevalence of pirate activity on the coasts, Hunt gives extensive detail of pirate ports. He observes, for example, "four hundred pirate prows" in Tempasuk. He notes that the pirates

^eE.g., the Sulu-British treaties of 28 January 1761 and 28 September 1764a

^fHorsburgh's *Indian Directory* (1811), as it came to be called, went through at least eight editions and was in use until the end of the nineteenth centurya

^gOf Dalrymple he said, "To that gentleman the English are indebted for the principal part of the information respecting this Archipelago" (Milburn 1825)a

^hSee Hunt (1815, 1820) and Mundy (1848)a

of the rivers north of Brunei town "are commanded by Datus from Borneo Proper," the pirates of Marudu Bay divide their nominal allegiance between Sulu and Brunei. He leaves the reader no doubt but that Tempasuk, Abai, Pandasan and Marudu contain large marauding communities. What is not always clear from Hunt's narrative is the identity of these pirates. Local Malay chiefs and Brunei *pengeran* did command some of the coastal raiders along the northwest coast at this time. But Lanun were also ensconced there. George Earl (1837) described their presence during his visit in the early 1830s. And there is little evidence to indicate that they had not already taken possession of Tempasuk, Pandasan, and Marudu. Dalrymple observed a settlement of Lanun at Tempasuk in 1763.

These early explorers were sponsored, if sometimes reluctantly, by the Supreme Government of the Honorable Company at Fort William, Bengal. Official reports of their exploits are therefore contained in various factory records (Bencoolen, Penang, and Singapore) and in the collections of secret correspondence with the Court of Directors in London. Here the earliest British observations on Malay piracy are recorded by country traders, sea captains, and politicians such as Raffles. They are deposited in the archives of the India Office Library in London.

It was left to the British naval captains, however, to provide the most vivid descriptions of Lanun strongholds. The naval expeditions against Malay piracy along the Sarawak coast in 1843-44 extended to the Lanun communities at Tempasuk, Abai, Pandasan, and Marudu Bay in 1845 and 1846. Captain Edward Belcher in HMS *Samarang*¹⁰ visited the Lanun Sultan of Tempasuk in 1844, noted his fleet of 200 *prahu*, and then went on to Sulu, where he met and exchanged formalities and information with Admiral Cecile, who was commanding the French naval expedition in the east. His observations on Sulu and the Lanun and Balanini people are perhaps the most accurate since those made by Forrest, and he corroborated Forrest's views in a very general way. Later English writers have for the most part accepted Belcher's observation that the Balanini were "considered a branch of Illanos." Belcher, as others, drew upon Dalrymple for his information about the northeast coast, which he was able to observe only briefly. Tunku, on the Unsang Peninsula, was already a "considerable slave market" in 1846, and "so complete a pirate den that no traders ventured there." Perhaps after Tempasuk and Marudu, Tunku was the most active of the Lanun "states" in northern Borneo.

The Lanun community of Sherif Osmun at Marudu was destroyed by the anti-pirate campaign in the summer of 1845 that was carried out by Royal Navy ships under the command of Admiral Thomas Cockrane.¹¹ In the following year, Tempasuk and Pandasan were attacked; those Lanun who did not flee and who survived submitted to the paramountcy of the navy and the new crown colony of Labuan. Their descendants became peaceful subjects of the Chartered Company.

The navy relied heavily upon Rajah James Brooke for its intelligence on piracy. Brooke's journals up to 1846 were first published in Mundy's narrative which appeared in 1848, although extracts appeared earlier in the volume written by Captain Henry Keppel (1846). But the clearest descriptions of the Lanun settlements were those written by Captain Mundy himself, and his journals relate the destruction not only of the settlements but of all Lanun political influence on the coast between Brunei and Marudu Bay.

¹⁰Belcher was on a naval survey mission of the Borneo coasts. The French naval squadron he met had attempted to purchase the island of Basélan from the Sulu Sultane

¹¹Mundy (1848); three more volumes of Brooke's journals are in J.e.C. Templer (1853).

The Lanun withdrew from Marudu Bay to Tunku in the Unsang Peninsula, already one of their strongholds on the northeast coast. There they remained relatively undisturbed for more than a generation. Occasionally an attempt was made to temper their power, e.g., the naval demonstration under Admiral Austin in 1851. The admiral appeared at Tunkut with a fleet of three vessels, two of which, HMS *Semiramis* and HMS *Pluto*, were steamers (see Keppel 1853). However, by having swept the Lanun pirates from the northwest coast in the campaigns of 1845 and 1846, the British navy had won their war for the time being.

Beginning in the 1840s, another source of information becomes available in the form of official correspondence and memoranda on the Admiralty and the Colonial and Foreign Offices. Many of these documents were produced by naval officers already mentioned. But they also involved the more exhaustive reports of Rajah Brooke, as governor of Labuan and consul general to Brunei, as well as those of his successors in both posts.¹²

Spencer St. John was Brooke's secretary from 1849 and succeeded him as consul at Brunei. He was a most acute observer and a prolific writer on Borneo, interested in its geography, history, fauna and flora, and in the politics and culture of the native peoples. Over a period of twenty years, the Foreign Office series of documents on Borneo is full of his memoranda, in which he laboriously detailed even small events occurring along the coast during his tenure. His two-volume work on his career in Borneo is a valuable history of British colonial policy and practice in that area during the mid-century period (1863). He later wrote a biography of James Brooke in his capacity as friend and literary executor of the late Rajah (1879). His writing and observations were those of the English gentleman in the colonies. He was accurate and thorough, even if inclined to draw value judgments concerning some Malay customs and life styles. Nevertheless, his approach was that of a keen outsider fascinated with the local scene and enthusiastic for furthering British influence in Borneo, as befit his role as protégé of the great white rajah. St. John was one of the innumerable colonial officers who labored diligently in remote outposts of the empire, immersing themselves in knowledge and experience of local culture, and never prospering very much financially from their endeavors. One of St. John's successors in Brunei and Labuan, Hugh Low, carried on the tradition in Borneo, and in 1876 was called to Perak, where he developed the model from which the British residency system in Malaya was to emerge. Such unknown men were the backbone of Victoria's colonial empire.

Until the time of St. John's departure in 1862, his reports formed perhaps the most authoritative information on Bornean piracy for the whole British period. His writing, for the most part, was based on first-hand observation. He went on numerous voyages around the northern coasts and to Sulu, and he was with Admiral Austen at the showing of the flag at Tunku in 1851. (His kinsman Horace St. John was likewise no mean chronicler of piracy in Borneo, albeit from second-hand sources [H. St. John 1853].)

But the British did not establish sufficient political influence on the northwest coast to enforce peace. Constant warfare ensued between petty chiefs. The remnants of Sherif Osman's people in Marudu Bay lived in constant fear of pirate raids from the Sulu Sea and northeast coast. There was, indeed, a resurgence in Lanun activity in the 1850s, when London

¹²The Admiralty sources are primarily found in "The Protection of British Trade 1840-49," Admiralty Series 125, Vols 94, and "China Station Records, 1840-1870," Admiralty Series 125, Vols 95 and 96a. Foreign Office (FO) Series on Borneo (12) and the Colonial Office (CO) Series for Labuan (144) contain the general correspondence and reports of those ministries on Borneo for the period.

began to impose some restrictions on the navy's anti-piracy operations in Southeast Asia. Brooke himself was called before a Commission of Inquiry in Singapore which investigated his own anti-piracy measures against the Sarebas and Sekrang Malays and their Iban allies. The formal records of this commission form a fruitful source for research into Bornean piracy (Parliamentary Papers 1854, 1854-55).

The annexation of northern Borneo by a commercial syndicate in 1878 and the formation of the British North Borneo Company with a charter from the crown were to spell the end of Lanun political domination along the coast eastward of Marudu Bay. That Lanun influence was pervasive was obvious from the earliest reports of the company agents. Both William Pretyman (Resident at Tempasuk) and W. B. Pryer (at Sandakan) reported on the depredations of the Lanun and the awe and fear with which they were regarded by indigenous tribes (see Harrisson 1956; Pryer 1894). Most of the coastal villagers had long since migrated up the rivers to avoid the coastal marauding and "revenue" extortion of the Lanun. Pryer noted that the first villages on the Kinabatangan were sixty miles upstream. Tunku, the last of the Lanun pirate states, was destroyed by forces from the HMS *Kestrel* in a renewal of naval warfare against the Lanun in 1879.

In the years since then, piracy has occurred sporadically. The early history of North Borneo is partly the story of the efforts of the Company to eradicate pirate activity and consolidate their own security along the coasts.¹³

III

Recent commentaries upon Malay piracy generally have attempted to analyze the nature of marauding as an institution. Professor Tarling's study is perhaps the most exhaustive work written on the subject in English. He moves from Van Leur's theories about the impact of Europeans on Indonesian society and builds upon the theme that Western incursion, especially by the Dutch, disrupted native patterns of commerce and drove the Malay chiefs to marauding as a means of livelihood. The destruction of the dynamic of the traditional state system was thus disastrous to both Malay society and Western commerce. He wrote: ". . . the new [European] settlements promoted disintegration of the old states and so, it would seem, prepared the ground for piracy" (Tarling 1963: 12).

The thesis is not new, of course, for Thomas Forrest and Stamford Raffles, among earlier writers, noted the effect that European attempts at monopoly and control had on native traders. Faced with loss of revenue and income, Malay chiefs began marauding. But Wertheim (1950) in his study of Indonesian society noted that the old trade system was outside the "closed" agricultural economy which predominated in the Malay world. He concluded that the great bulk of Indonesians-Malaysians were little affected by the new European commercial systems. "The Dutch traders," writes Wertheim, "brought about no essential changes in the economic structure." The harbor princes who were affected, however, encouraged piracy against the Dutch.

While native trading systems were disrupted, it is curious that, over all, trading opportunities increased under European hegemony. This was especially true during the second half of the eighteenth century as the

¹³The British North Borneo Company records are collected and deposited in the Public Record Office, London, under the Colonial Office series 874e. The Foreign Office (Sulu) series 71 contains much detail on the subject of piracy from 1849 to 1888e.

China trade and the entrepot operations on which it depended grew in volume. Those Malay traders who could change over from the old system to the new, such as the Bugis, were welcomed in European ports, especially those ports presided over by British free-traders. Was, in fact, the disruptive factor of the European incursion any more severe or destructive than that which normally occurred at intervals in pre-Western times? The decline of Srivijaya-Palembang, for example, and the rise of Malacca, caused considerable dislocation in the commercial world of Southeast Asia at the time. That period was also accompanied by a sharp rise in the incidence of piracy and marauding.

There is wide agreement with the proposition that there was a propensity for marauding along the Malay coasts. And marauding was institutionalized in part within the framework of state relations, as interstate and intertribal warfare was a part of that system. That recent studies have tended to neglect the persistence of piracy in the straits and along the seaways of Southeast Asia during pre-Western periods seems obvious from the current concentration on Western incursion as the disruptive element in native society. It seems most probable that there has been a recrudescence of piracy on a large scale from time to time since the beginning of sea commerce between East and West in the waters of southern Asia. In the nineteenth century, as in the post-Srivijayan period, it coincided with political disorganization in conjunction with the decline or absence of state systems of authority, which hitherto had been able to control or at least temper piracy (see, for example, Graham 1967: 362 ff.). In the Malay world, the decline of the authority of a sultanate took the form of weakening the sultan's authority from the center. The process brought about the removal of the sultan, as in the case of Malacca, or a falling away of the river and coastal chiefs on whose allegiance and respect his power, at least as suzerain, rested. Such was the case of Brunei in the eighteenth century and of Sulu at almost the same time. The petty chiefs, some of whom would begin to style themselves "sultan," were too weak individually to exert much influence of their own beyond their immediate bailiwicks. There were at least half a dozen self-styled sultans on the northern coasts of Borneo in the mid-nineteenth century.

The result of this process was that governmental control was virtually absent over extensive zones of coastlines, rivers, and sea passages. As these zones were frequently filled with busy and prosperous commerce, the temptation to rob, pirate, and raid was strong and in proportion to the profits to be gained: "Piracy was 'big business' better organized and more profitable than the various sea fisheries; consequently almost every coastal dweller was a potential pilferer, ready to turn buccaneer given the arms and his raja's blessing" (Graham 1967: 364). At such times as these, as Van Leur suggests, "Piracy was just as much a normal activity in Asian waters as in the Mediterranean. . . ."

Local chiefs supported raiding expeditions, for part of the reason for decline of the sultanate was the loss of income and revenue from the sultan's trading contacts. The sultan was consequently decreasingly able to reward his vassals and retain their loyalty. The loss of income was thus to be made up by piracy and raiding. This process of decline, from whatever seminal causes, and the subsequent shrinking of Brunei's authority in northern Borneo, go a long way in explaining the extensive marauding carried on by the raiding parties of Malay river *pengeran* and their Iban allies, against which Rajah Brooke campaigned in the 1840s.

But these reasons are not sufficiently convincing to explain the marauding in the case of Lanun pirates in Bornean waters. It is probable that Sulu's authority had also weakened at the center by the second half

of the eighteenth century, and that most of the more distant *datu* had opted for an independent existence, with only an occasional demonstration of respect to the Muslim prince of Sulu. An absence of strong governmental authority characterized the Sulu Seas and northeastern Borneo. Indeed, it is quite unlikely that Sulu ever exerted much political influence in the northeast, for the Spanish incursion into the southern islands occurred at precisely the time when Sulu could have been expected to consolidate in those areas where the authority of Brunei had declined--if she were not herself in decline, with her own vassal chiefs falling away.

But the Lanun pirates were not vassals or subjects of the Sultan at Jolo. They were, in fact, displaced subjects of the old Sultanate of Magindanao. And not entirely displaced, either, for there seems to be evidence enough to indicate that they were a highly mobile people--those Lanun buccaneers--moving back and forth sometimes at intervals of years, between their Borneo coastal realm and their homeland at the head of Illana Bay. The point is, I think, that as Magindanao submitted to Spanish power and as that same Spanish power gradually drove the Moro raiders from the Visayas, the Lanun concentrated their marauding energies in the seas to the southward and, in the absence of both Brunei and Sulu authority, took over some strategic harbors and river ports in northern Borneo. As the authority of the Magindanao Sultan weakened at the hand of the Spanish, so, too, independent *rajah* and sultans proliferated throughout the disintegrating sultanate. Forrest (1779: 301) counted seventeen of the former and no less than sixteen of the latter during his visit in 1776.

It might be useful to point out a parallel with the Bugis, whose independence was threatened by the Portuguese and Dutch incursions into the Celebes and the Moluccas, and who began freebooting throughout the Indies in the seventeenth century, taking over several river principalities in Malaya. Like the Bugis, the Lanun were a particularly energetic, ambitious, and able people. They, too, moved rather successfully into the prosperous business of piracy, or warfare against the entrepot system, at a time of great political disorganization accompanied by increased commercial activity.

It will undoubtedly be fruitful to investigate the phenomenon of the decline of Southeast Asian states in connection with increasing lawlessness, for political disintegration cannot be wholly explained by the impact of the Western incursion. There is a "natural" rhythm of development-consolidation-prosperity-power-decline which has seemed to be a part of the traditional order in most oriental states--and not only oriental states.

The progress of the natural syndrome of rise, flourish and decline does not, of course, preclude a consideration of the effect of Western invasion on the process. The presence of powerful European interlopers must have terminated the process in many instances. In the case of the western Malayan Sultanates of Perak and Selangor, some scholars (notably Cowan 1961 and Parkinson 1960) have concluded that further disintegration of the traditional polity was aborted by British intervention in the last century; and in the peculiar circumstances of Malayan multiracial society, the Sultanates were rejuvenated. The British relationship with Brunei in the present century followed a similar pattern, with British Residents presiding over the resurrection of the Brunei Sultanate. The sultans of the southern Philippines, on the other hand, disappeared almost completely. With the presence of Spanish power, no new princes arose to conquer and embark upon the pathway to a new dynasty or a new state system. There was little in the way of traditional renewal.

The work to be done in the present study involves further investigation into the nature of Lanun piracy and marauding, and its relation to the

political connection between Lanun communities in Borneo and the Sulu Sultan. It will be fruitful to see just how far "regular" government was carried on by the Lanun states in the century or more before their power was destroyed by the combined efforts of Rajah Brooke, the Royal Navy, and the British North Borneo Chartered Company. How effective was their tributary collection system¹⁴ for example? To what extent was the political order of the northern coasts maintained on a more positive basis than the mere maintenance of harbor and riverports for supply, refreshment, and refitting?

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¹⁴Spencer Sta John was able to obtain some information on the extent of the districts paying tribute to Sharif Osman of Marudu, and Captain Mundy noted several ports on the northwest coast which recognized the ruler of Marudu.

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COMMENTS

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Leigh Wright's paper is an inquiry into the nature of piracy in northern Borneo, a reinterpretation of the commonly held thesis that piracy was a response to dislocations of indigenous trading networks by Europeans in the eighteenth and early nineteenth centuries. His paper questions this thesis in two ways.

First, the paper suggests that piracy is older and more important in Southeast Asian history than it is often thought to be, and that it was tied into an ongoing process of state-building and state deterioration. There were many parallels in the pre-Western past when piracy evidently preceded the rise of major states or accompanied the decline of central authority. Thus, it is misleading to see piracy as strictly an eighteenth and nineteenth century phenomenon. The paper also suggests that later pirates--such as Lanun--may have "plied their trade" in the pre-European period. One way to perceive piracy was as big business, a traditional and normal activity whenever central authority was weak. This argument seems reasonable to me and thus a contribution to Bornean (and Southeast Asian) history.

Second, the paper argues that the rise and fall of states throughout time may help explain some piracy in Borneo--including Malay/Iban marauding--but not that of the Lanun. In the eighteenth century, Lanun began moving into northern Borneo and established half a dozen small riverine states at a time when the Spanish were pushing their frontiers southward in Mindanao and closing off the traditional Lanun pirate haunts in the Visayas. Lanun moved into a political vacuum in Borneo because Sulu and Brunei--the major local powers--were weak and decaying. I hope I am not reading more into this than I ought to, but the implication of the argument seems to be that Magindanao, rather than suffering a European-imposed decline, was in fact extending its authority into northern Borneo. There were continuing ties and much coming and going between Lanun states in Borneo and the Lanun homeland on Mindanao. The paper suggests that if we knew more about northern Borneo's political structure at that time, particularly whether any sort of stable and cohesive overriding political authority among the Lanun states existed, we might be able to see this Lanun settlement and piracy in a different light, perhaps as a form of state-building. Central to this thesis is the notion that the Lanun were not subject to Sulu but remained vassals of Magindanao. To extend Wright's argument further than maybe intended, perhaps Magindanao was in process of setting up a satellite state in northern Borneo, or the Lanun were building an independent or dependent state of their own until their activities were halted by colonialism. Piracy or marauding merely provided the most viable economic basis for their state (or states) but was, in any case, traditional, since it had been important for Lanun society in Mindanao for centuries.

This argument is interesting, though I think more debatable than the first point. For one thing, it is based on the concept of weakness and decline in Sulu and Brunei. The paper suggests many sources, especially

British and Spanish ones, which could answer some of the questions. I am not sure these sources can be definitive. For example, Ann Reber and Keith Reynolds have used some of them and painted a rather different picture of an influential Sulu in the eighteenth and early nineteenth centuries, not a society in decline. They suggest that Lanun were, in fact, dependents or vassals of Sulu, with whom they traded slaves for arms, and that Sulu's influence on the Sabah coast was significant, especially on the east coast. Reynolds goes so far as to call the Lanun "Sulu's navy." As evidence, he notes that Lanun never attacked Sulu or Chinese shipping in Sulu waters.

This raises the first question I would ask: Is the "weak Sulu" idea central to Wright's theory, and does he read the sources differently? How can we solve this problem of interpretation? Possible oral sources for a study such as this exist. I wonder if present-day Lanun in Sabah and Mindanao have satires and legends which might tell us about their piratical past, their coming to Sabah, kinship ties to Mindanao, about why they, more than others, went so far afield in their wanderings. Oral tradition might be supplemented by anthropological studies. I would be curious to know what cultural basis there might be for their wanderlust tradition and how it compared with that of the Iban and the Minangkabau.

Is a comparative perspective on piracy and state-building possible? How did Lanun piracy compare with piracy elsewhere in Borneo or in the Malayo-Muslim world? Indeed, might not piracy better be seen in a much broader perspective? West Indian pirates were usually displaced lower-class whites, and piracy functioned as a way of adapting to a changing socioeconomic world.

On the topic of state-building, do Lanun perhaps fit the pattern of a traditional method of state-building in Borneo? James Brooke, the founder of Pontianak, etc., were outsiders setting up states in political voids. Lanun were perhaps much more vigorous. Immigrant-based states seem to have been a common feature of Bornean history and may be seen as part of an on-going process. Does Wright's material fit into this scheme at all? Might it not contribute to a broader picture of political history?

PERSPECTIVES ON BORNEO'S PAST: THE STATUS AND
FUTURE OF RESEARCH ON SOCIAL HISTORY†

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Introduction

The sociocultural configuration of Borneo over the past two or three centuries has been diverse: Muslims of varied ethnic origins living chiefly along the coast; predominantly non-Islamic swidden cultivators, wet-rice agriculturalists, hunters and gatherers dwelling mostly in the interior, and often collectively termed "Dayak" despite major linguistic and cultural differences; Chinese traders, miners, and farmers who adapted to the Bornean environment in a variety of ways; Arabs, Indians, Europeans, and other immigrants. The wide spectrum of peoples, and the pluralistic states and societies of which they have been a part, have received little attention from historians of insular Southeast Asia in comparison to more populous, politically significant regions such as Java, Luzon, and Malaya. In recent years, however, a small number of historians and other social scientists have undertaken studies not only of Borneo but also of other neglected archipelago areas such as Sumatra and Mindanao, and so a clearer and broader picture of Southeast Asian history is beginning to emerge. Some of the major gaps in our knowledge of Bornean, and Southeast Asian, social history are now being filled.

Before discussing the existing literature, the nature of social historical inquiry must be considered briefly. The boundaries between various branches of history are not rigid, and there is no need here for rigorous definitions. Nonetheless, it can be said that, while political historians are primarily concerned with political structures, inter- and intrastate relationships, and international influences on Bornean history, social historians concentrate their attention on such problems as social structure, sociocultural groups, inter- and intragroup relations, social movements, and the impact of political, economic, and social change on peoples and societies. It should be noted that, in the Bornean context, political and diplomatic historians must of necessity be concerned with politically important but numerically insignificant groups from outside the island, such as the Dutch colonizers of southern Borneo, while the social historian is interested in these groups only insofar as they play a social role in Bornean society. The principal focus for the social historian, as for the anthropologist, is on the Bornean peoples themselves, whether indigenous or immigrant, rather than on their foreign overlords.

Thus the assessment of the existing literature will include many works which are only partly social-historical in scope; some will overlap with

¹I would like to thank G. N. Appell and Leigh Wright for commenting on an earlier draft of this paper, and Appell, Ian Black, Donald Brown, Lucas Chin, Han Sin Fong, Omar O. Hidayat, Robert Nicholl, and J. R. Wortmann for providing me with information on various research projects.

other areas of history and other disciplines. The writer is most familiar with the literature on Malaysian Borneo, and the discussion of the Indonesian section of the island is not intended to be comprehensive. There may be some important Dutch, Chinese, and Indonesian-language sources that are omitted from this summary. Furthermore, although the literature will be examined on the basis of modern political divisions, modern and pre-modern political boundaries did not necessarily correlate with ethnic and social boundaries. For example, the northwestern territory stretching from Brunei to the Kapuas basin in many respects constituted a coherent sociopolitical world of Malayo-Muslim sultanates, Dayak dependents, and pioneer Chinese mining camps. A similar case could be made that the Javan-influenced southeastern coast, and the Sulu-influenced northeast, comprised well-defined sociopolitical zones in the eighteenth and nineteenth centuries. The possibilities for comparative historical analysis of the complex interrelationships between coastal sultanates, interior Dayak, and immigrant peoples are myriad.

The Status of Research

As might be expected, the existing literature is uneven. With few exceptions, there has been little attempt to relate research comparatively to other areas of Borneo or to the broader context of Southeast Asian history. Furthermore, some areas have been discussed extensively, while others have been virtually ignored. Perhaps because of the romantic story of the Brookes and the presence of some of the best known Dayak groups, Sarawak has received the most attention from scholars. It has been relatively easy to do research in Sarawak, thanks in large part to the liberality of the Sarawak Museum and the Sarawak government; in addition, oral traditions have been systematically collected there, and the archival source materials in Kuching are extensive and well organized. Consequently, our knowledge of Sarawak's social history, while far from complete, is broader and deeper than for any other Bornean state. Both Sabah and Brunei have received increased attention in recent years, although many important problems remain unstudied. The most serious lacunae are found in the literature on Indonesian Borneo; few scholars of any nationality have worked in the southern two-thirds of the island. There are scattered works of merit, but nothing approaching the most impressive monographic work on Sarawak or Sabah. Kalimantan deserves the most urgent research priority.

Sarawak

The two most important social themes in modern Sarawak history were Iban migrations and the spread of Chinese settlement, both of which have been subjects of scholarly inquiry. In addition, recent studies have contributed to our understanding of such significant topics as pre-Brooke Iban social structure, the development of urban society, social change under the Brookes, interethnic relations, the creation of the Sarawak Malays from diverse origins, and the development of pluralistic societies in several regions of the state.

The most complete research has been done on the Iban. The pioneer in this field was Benedict Sandin, himself an Iban from the Saribas, with only limited formal education. Now retired as Curator of the Sarawak Museum, Sandin spent many years collecting legends, stories, genealogies, chants, family histories, and other oral traditions from a variety of indigenous peoples, but most particularly from the Iban of the Second Division (see his many articles in the *Sarawak Museum Journal* and *Sarawak Gazette*)

Sandin's efforts suggest that the promotion of social-historical study does not necessarily depend on the rigorous education of historians; talented Sarawakians with an interest in their own traditions can also make useful contributions. To the social historian, the significance of Sandin's most important work is that, through the utilization of oral tradition, he has been able to present a coherent and largely convincing picture of sixteenth and seventeenth century Iban migrations from the Kapuas into the Second Division and the subsequent expansion and consolidation of these settlements in the pre-Brooke period (Sandin 1967). Included in the story are the amalgamation or expulsion of earlier non-Iban peoples, intra-Iban rivalries, and later relationships with the Malays of the coast. The last was a confluence that was sometimes hostile and violent, other times friendly and cooperative. Sandin is now at work on a study which will take the Iban story through the Brooke period and into the postwar years. His work has also been given some recent conceptual depth by Stephanie Morgan (1968).

Robert Pringle, a Cornell-trained historian who worked closely with Sandin, has built upon these foundations to analyze the social history of the Iban in the nineteenth and twentieth centuries (Pringle 1968, 1970). Using both oral tradition and an impressive variety of primary and secondary documentary materials, Pringle elucidates the interplay between the Brooke *rajah*, with their policy of stabilization, and the migration-prone Iban--a relationship with far-ranging implications for Sarawak social history. In addition to the European impact on Iban society and life, Pringle discusses the sociocultural basis for Iban migration and headhunting, the complex relationships with the Malays and Chinese in the Second and Third Divisions, Iban-Malay "piracy," the nature of outstation life, the socio-economic transformation of the Malays, and the Iban response to educational and economic development. It would be interesting to know whether the Iban in West Borneo developed along similar lines, or whether any other Dayak groups maintained similar cultural attributes.

Pringle's findings give a valuable historical supplement to the anthropological accounts of the Sarawak Iban by such scholars as Derek Freeman (1970) and Vinson Sutlive, Jr. (1972). Freeman studied an Iban frontier area in the Baleh; Pringle, with his broad knowledge of Iban history and society, was able to indicate several areas in which Freeman's pioneering analysis was less universal for Iban than was generally believed (Pringle 1970: 25-28).t Sutlive's study of rural and urban settlements in the Sibuh district tells the social historian much about sociocultural change, migration, and the influence of urbanization on a Dayak people.

The only other detailed study touching upon rural social history is Tom Harrisson's socioeconomic analysis of Malay life in the Sarawak River delta (Harrisson 1970). In view of the fact that the Malay are a major ethnic group and have played a crucial political, economic, and social role throughout Borneo's history, it is unfortunate that they have been so neglected by scholars of any discipline. Although restricted in geographic scope, Harrisson's account does discuss the settlement of the delta since the early nineteenth century, the "creation" of the delta Malays from diverse ethnic origins, the probable course of Islamization, intergroup relationships over the past century, and the impact of political and economic change. Benedict Sandin has supplemented Harrisson's research through his collection of oral traditions on Sarawak Malay origins (see, e.g., Sandin 1969). Harrisson's work invites comparative study on Malay communities elsewhere in Sarawak and Borneo to trace the evolution of Malay (or Malayo-Muslim) society.

The most extensive study of urban social history is the writer's analysis of Kuching, the largest town in Malaysian Borneo (Lockard 1973). This

account, based on both documentary sources and interviews, traces the development of Kuching society from a small pre-Brooke Malay village to a predominantly Chinese Malaysian city. In addition to such general topics as residential and occupational patterns, demography, the impact of nationalism and political change, and the development of urban social institutions, special emphasis is directed to certain problems which allow comparisons with other Southeast Asian cities. These include culture change and the formation of new groups, the role of indirect rule in the administration of an ethnically heterogeneous urban area, the structure of the Malay and Chinese communities, the role of Chinese speech groups, interethnic relations, and the extent to which Kuching could be considered a socially and culturally plural society. Attention is paid not only to the Chinese and Malays but to numerically smaller groups such as the Indians, Dayak, and Japanese. Furthermore, Kuching is placed in a broader perspective through the use of comparative material from other Southeast Asian and Bornean urban centers. The writer has also published some of his material on the Chinese and Malays of Kuching (Lockard 1971b, 1974), and, with Graham Saunders, a collection of old Sarawak photographs and drawings (Lockard and Saunders 1972). There is as yet no similar study of another Bornean town or city.

The remaining literature on social history covers a variety of topics. Chinese immigration and settlement before 1917 has been discussed most extensively by the writer in his M.A. thesis (Lockard 1967, 1971a), but the account is only a rough outline. Richard Fidler's recent anthropological dissertation on the Chinese in Kanowit contains some relevant material (1973). Stephen Morris's study of the Melanau touches on social history and the development of the trading system (Morris 1953). The impact of mission, government, and Chinese schools on the various peoples during the Brooke period has been analyzed by J. M. Seymour (1967). Peter Varney has studied the spread and role of the Anglican church under the Brookes (Varney 1968, 1969). A Chinese historian in Sibul, Liew Yung Tzu, has written several books in Chinese, including an account of the Japanese occupation (Liew 1956). Three American Peace Corps volunteers in the late 1960s wrote on the local history of Marudi, Lawas, and Kanowit respectively (Goldman 1968; Edwards 1969; Stevens 1970). Various aspects of European society in the Brooke period are discussed by Steven Runciman (1960) and Elizabeth Pollard (1972).

A final source of relevance to social history is a rather untraditional one, namely John Chin's fine but brief novel, *The Nyonya* (Chin 1962). Using a fictional form, Chin, a Kuching Chinese, tells the story of a Chinese immigrant woman and her adaptation to Sarawak (and Sarawak Chinese) life and culture. Encouraging literate and talented Borneans to write of their lives, families, and experiences in either fiction or nonfiction could be one of the most effective ways of recording social history. The Borneo Literature Bureau in Kuching has occasionally offered annual prizes for fiction by Sarawak writers, and if possible such encouragement should be continued. Fiction provides a forum for description and personality study that more formal scholarship usually neglects. Perhaps a talented Iban could write a fictional account of the coming of Brooke influence on an Iban community along the lines of Vickie Baum's *Tale of Baliq* or a Malay could analyze the social disruption of the Cession controversy in a Malay village.

Sabah

Major research areas in Sabah's social history have included the relationships between coastal Muslims and the interior Dusunic and Murutic peoples in the pre-British period, the impact of Chartered Company rule on

the indigenous inhabitants, the spread of Islamic and other religious influences, the development of Chinese society on the west coast, and socio-cultural change among the Bajau in the late nineteenth and early twentieth centuries. Although there is some limited information on social history in the colonial-oriented account of Chartered Company rule by K. G. Tregonning (1965), particularly on "slavery" and labor migration, it is to recent studies that one must look for sophisticated approaches to problems of interest to social and ethnohistorians.

Perhaps the most useful contribution to the monographic literature is Ian Black's analysis of the Chartered Company's Native Administration between 1878 and 1915 (Black 1968, 1969, 1970, 1971). Relying primarily on Company and other documentary records, Black traces the developing relationship between the British Company and the indigenous peoples, with particular attention to the non-Muslims of the interior. Among the topics discussed are interethnic relations, religious revitalization movements, the social basis for "rebellion" against Company rule, changes in leadership patterns, and the role of immigrant peoples such as the Iban, who were employed by the British as a police force. Black places some of the changes within the framework of traditional archipelago social and political activity. He is now at work bringing the story up to 1941 and hopes to publish the entire study.

James Warren's account of the British impact on the Bajau of Darvel Bay from 1878 to 1909 is another significant contribution to social history (Warren 1971). Using both documentary sources and some oral tradition collected while a Peace Corps volunteer, Warren discusses the varieties of sociopolitical organization found on the east coast in the nineteenth century, the structure of Bajau society, relationships with other ethnic groups, and the gradual transformation from seminomadic boat-dwelling to sedentary land-dwelling engendered by British policies. Warren notes that a systematic use of oral history would be necessary for developing the story more completely.

Appell (1968) discusses the problem of social process and ethnic identification for historical reconstruction. He concludes that the failure to recognize and identify the indigenous ethnic distinctions, as well as to control for the nature of the social processes at work as a result of culture contact, has vitiated much social-historical and anthropological research. He also concludes that these failures have impeded explanations for the problem of depopulation among the Murutic peoples, and he finds evidence that depopulation was also a feature of Dusunic societies because of the introduction of disease and stress-produced health impairments arising during the initial periods of European contact.

Some information on east coast society in the pre-Company period can be found in an M.A. thesis by Keith Reynolds, which concentrates on the relationship between Sulu and its Bornean dependencies from 1700 to 1878 (Reynolds 1970). The social implications are implicit, but there is some data on intergroup relations, migration, "slavery," and the often uneasy interplay between Sulu overlords and the local peoples. Perhaps the present research of James Warren, now at the Australian National University, which focuses on the socioeconomic patterns of the Sulu zone in the eighteenth and nineteenth centuries, will give a clearer picture of pre-Company Sabahan society.

There are several other works which have relevance for social history. Maxwell Hall's study of the Chinese-led guerrilla revolt against the Japanese in 1943 describes the impact of military occupation (Hall 1965); some of the material can be used in comparison with the discussions on Sarawak

in Liew (1956) and Lockard (1973). David Fortier's pioneering anthropological analysis of rural Chinese in the interior reveals much of value on sociocultural changes generated by the transition from subsistence farming to cash cropping over a forty-year period (Fortier 1964). Han Sin Fong, a Labuan-born Chinese and Michigan-trained cultural geographer, has made good use of interviewing and survey methodology in tracing changes in occupational patterns, social status, and interdialect group interaction among urban west coast Chinese over half a century (Han 1971). Han's material is valuable in providing a comparative perspective for the writer's analysis of social change among the Kuching Chinese. It should be noted that the research of Hall, a long-time British resident of Sabah, and that of Han suggests the important contributions which can be made by Sabahans, whether formally trained in research skills or not.

Brunei

Brunei has historically been one of the most important sultanates, and it is fortunate that valuable historical research has been done by an anthropologist, Donald Brown; his work is particularly important for the nineteenth century (Brown 1969, 1970, 1973a, 1973b). Brown's special interest is social stratification. Through the application of anthropological methodology to the documentary sources and oral tradition, he presents a detailed account of the political structure, administrative system, and highly complex development of the Brunei Malay elite. There is also information on ethnicity, inter- and intragroup relations, sociocultural life, and the social composition of Brunei town and Kampong Ayer. There is much that remains unclear--for example, the importance of Malayo-Muslim immigration in the "creation of the Malay." Furthermore, Brown makes no attempt to compare his findings with other Bornean and non-Bornean sultanates. But some of his material offers a useful contrast with Kuching. Brown's work is now being supplemented by the current research of Robert Nicholl, a longtime British resident of Sarawak and Brunei, who is studying Brunei in the fourteenth through sixteenth centuries. There is also some information of value in a recent book by Nicholas Tarling (1971), although it is largely political in focus.

Kalimantan

Despite accounting for some two-thirds of Borneo's land and population, the Indonesian section of the island has been virtually ignored by social historians. The existing literature is largely the work of anthropologists and geographers. One important monograph discusses the social, economic, and political development of the Chinese gold-mining settlements in West Borneo during the eighteenth and nineteenth centuries. This study, by the geographer James Jackson, places the Chinese "colonies" in an ecological perspective and devotes attention to such problems as inter-*kongsi* relations, sociocultural change, Chinese social structure, and the Chinese relationships with coastal Malay and interior Dayak (Jackson 1970). Jackson concentrates on the Chinese and only suggests some broad outlines of the West Bornean social world, but it is a good beginning for further work on the subject. His material on the gold-mining *kongsi* can be supplemented by the brief accounts of Barbara Ward (1954) and Lo Hsiang Lin (1955), and by the nineteenth century Dutch study by J. J. M. DeGroot (1885). There is still a need to broaden the Chinese material through a study which views West Borneo as a social unit.

The anthropologists Avé, Hudson, Miles, and Whittier provide the best modern accounts of the interrelations of Kalimantan ethnic groups (cf. Avé

1970; Hudson 1967a, 1967b, 1972; Miles 1964, 1965a, 1965b, 1966, 1970, 1971; Whittier 1973b). Hudson, using documentary oral sources and linguistic analysis, discusses the history of the Barito River Basin, development of the Banjarmasin Sultanate, Ma'anyan-Banjarese relations, Ma'anyan social structure, Banjarese expansion, Ma'anyan migration, and the sociocultural changes inaugurated among the Ma'anyan by warfare, migration, and Christian missions. The study spans four centuries and provides useful materials for comparison with Pringle's study of the Iban; migration, violence, and adaptation to modern influences play a role in both sagas. The analysis of the Banjarese chronicles by J. J. Ras also contains some data on Banjarese social history (Ras 1968).

Herbert Whittier paid considerable attention to social history and sociocultural change in his study of the Leppo Tau Kenyah of the Apo Kayan River (Whittier 1973b). Among the themes discussed are migration, leadership, social organization, relations with other peoples, and the impact of Christian missions. Most of the data was collected through participant observation and oral tradition. The Leppo Tau Kenyah, living for the most part in the deep interior and far removed from the traditional suzerain power of Kutai, present a very different picture from that of the Ma'anyan or Iban. Yet they, too, have been subjected to external as well as internal forces of social change over the past several centuries.

J. R. Wortmann, a Dutch scholar with long experience in East Borneo, is presently preparing an illustrated history and study of Kutai Sultanate that makes use of both oral and documentary materials. When completed, it may provide comparative data for the studies of Brown and Hudson. Wortmann has already published several brief outlines of his work which suggest the sociopolitical structure and the interaction of peoples in the eighteenth and nineteenth centuries (Wortmann 1971a, 1971b). Wortmann's study may be used in conjunction with a B.A. thesis by the Indonesian S. Abubakar Ba'boed on the history and the development of the Kutai kingdom (cited in Whittier 1973a: 42).

There are two other Kalimantan studies which focus on more limited subjects. H. Hassan Basry's book on the revolutionary period in Kalimantan is not explicitly social in emphasis but contains some material relevant to social changes during the revolution (Basry 1961). Another Indonesian scholar, Artum Artha, has written a short history of Banjarmasin city (Artha 1972).

Research Directions

The above summary suggests that a considerable literature on social history exists, some of it of very high quality. Clearly the future researcher will not be operating in a vacuum. Furthermore, there has been some use of oral tradition to supplement the available documentary sources. It is also important to point out that present knowledge is very much a product of interdisciplinary cooperation between historians, anthropologists, and geographers; nonhistorians like Brown have placed their social and cultural research in an historical framework, while social historians like Pringle and Warren have added historical depth to the social science literature. The potential for future cross-fertilization remains considerable.

The existing studies notwithstanding, many historical problems remain to be solved. In the suggested projects that follow, special attention is accorded those which depend heavily on the use of interviewing or oral

tradition. It seems likely that the increasing pace of political, economic, and social change in Borneo will gradually erode the traditions, stories, legends, and chants passed on from generation to generation in isolated communities or among the old royal families. These stories and traditions must be recorded soon, or they may disappear forever. Furthermore, the participants in the history of the early twentieth century, with their memories of a fast-receding past, are passing from the scene. Many research projects could be suggested, but the following (in no particular order of urgency) would seem to be the most significant for filling in lacunae or utilizing diminishing oral sources.

(1) Sociopolitical structure of sultanates. A study of the traditional Kalimantan sultanates could provide comparisons with the existing material on Brunei. Banjarmasin was the most important Kalimantan state and might merit research priority. There may have been many differences between the Javan-influenced, southward-looking Banjarmasin and the more isolated, northward-looking Brunei in such matters as elite structure, administration, ethnic configuration, relations with the hinterland, political stability, social organization of the capital, role of foreign traders, and the social impact of political decline. There should be a considerable Dutch and English documentation for such a study, but it could be augmented by oral traditions, particularly the interviewing of remaining members of the royal families.

(2) Social change under Dutch colonialism. The literature on Malaysian Borneo suggests that the expansion of European authority and influence generated resistance, rebellion, religious revitalization movements, Chinese immigration, labor migration, urbanization, changes in settlement patterns, religious conversion, suppression of "slavery" and headhunting, growth of cash crop economies, and transformation of Malays from traders to bureaucrats, among other phenomena. An extensive study of a Kalimantan district, region, or ethnic group could determine whether similar changes occurred under the Dutch.

(3) Urban history. The most interesting comparisons with Kuching might be found in Pontianak, an older and larger city with a more heavily Malayo-Muslim population but, like the Sarawak capital, founded by a foreign adventurer (an "Arab") and the home of an important Chinese community. Since the ethnic configuration, political structure, and economic base differed considerably from Kuching, the social organization, ethnic relations, cultural changes, municipal government, and social life of Pontianak may also have developed along other patterns. Ethnically heterogeneous but predominantly Islamic cities such as Banjarmasin and Samarinda, or heavily Chinese towns like Sandakan, might also merit study.

(4) Regional history. Rather than focusing on a particular settlement or ethnic group, a regional study could analyze social developments in a wider area, since historical changes often occurred on a region-wide basis. For example, it might be interesting to examine the interplay between different types of urban and rural settlements in West Borneo and Sarawak, where three distinctive patterns of urban settlement developed in the eighteenth and nineteenth centuries: the ethnically heterogeneous coastal trading ports and "Malay" political centers like Pontianak, Sambas, and Kuching; the Chinese mining towns like Montrado and Bau; and the upriver trading posts like Sintang, Simanggang, and Kanowit, with their close ties to the Dayak interior. All three were radically different in sociopolitical organization, ethnic configuration, economic function, historical development, and relationships with the interior peoples. It was through this type of regional social analysis that the writer was able to reevaluate the so-called "Chinese Rebellion" of 1857 in Sarawak (Lockard 1973: 83-105).

(5) Sociocultural change in the interior. During the nineteenth and twentieth centuries, the interior peoples (mostly Dayak) have been affected by various influences emanating from the coast. One way of measuring these changes would be by studying the interaction between an upriver trading post (bazaar) such as Belaga in Sarawak and neighboring peoples. These trading posts were, in their way, outposts of coastal "civilization," the most visible representatives of expanding coastal political power and culture. At the same time they were much influenced by the surrounding Dayak environment. The symbolism of the old Chinese longhouse bazaar at Belaga is very suggestive in this respect. The changing relationships between outstation and Dayak, and the changes affecting both parties, could be an interesting area of research. The use of oral sources would be absolutely crucial, supplemented by such documentary sources as district reports.

(6) The "making of the Malay." The Islamic peoples of Borneo are diverse culturally and derived from a wide variety of origins. However, with the notable exception of the Bugis and of many in the northeast whose origins are in the southern Philippines, most speak dialects of Malay, maintain a fairly similar Malay sociocultural pattern, and can be loosely classified under the term Malayo-Muslim or Malay. But the spread of a Malay identity in Borneo has been a continuous process since the introduction of Islam in the fifteenth or sixteenth century. Even today, the "Malays" of Sarawak are not yet a wholly cohesive ethnic group. Thus, the Sarawak Malays, Banjarese, and other Bornean Malays have been "created" over the centuries by the spread of Islam and the amalgamation of various Muslim immigrants with the Dayak. The process continues today on Sarawak's Melanau coast, in the upper Kapuas, and elsewhere, but it has received little historical attention. A study of "Malay making" would demand the use of oral traditions and linguistic analysis.

(7) Immigrant peoples of Kalimantan. The Chinese of southern and eastern Borneo have received virtually no attention from scholars, nor have the Bugis, who settled in significant numbers in both urban and rural areas. Future research might be directed to such problems as the economic and political role of immigrants, the immigration process, acculturation and adaptation to the local environment, social organization, intergroup relations, Bugis-Malay relationships, and the importance of Chinese secret societies.

(8) Social change since 1941. Only a few studies have dealt directly with the Javanese occupation and the postwar period, yet there have been tremendous social changes in Borneo, as elsewhere in Southeast Asia, since 1941. For example, a study of the impact of the Indonesian revolution on a particular settlement or district could provide a valuable complement to local studies on other parts of Indonesia. Or a study of the influence of prewar and postwar nationalism on a pluralistic community or region would provide useful comparative material for existing work on Sarawak.

The Future of Research

It is one thing to propose research projects or priorities and quite another to find the personnel and funding with which to carry them out. Non-Bornean scholars can still make a significant contribution, particularly in the study of the social history of ethnically plural societies. A Bornean researcher with local ties or ethnic sympathies may or may not have the necessary scholarly detachment. However, financial support and graduate admissions to Southeast Asian studies programs are being drastically reduced, and there may be fewer young Western scholars available in

the immediate future. Furthermore, in some instances Western researchers are becoming less welcome.

Fortunately, there might be some chance of involving Borneans more effectively and systematically in the research. The efforts of Benedict Sandin and others at the Sarawak Museum to collect oral and ethnographic materials confirm the contributions that Borneans without substantial academic training can make. Surely there must be other Sandins elsewhere in Borneo, with a keen awareness of their own traditions and a lively intelligence, who might be mobilized or trained for the purpose of interviewing and recording. The fruitful collaboration between Pringle and Sandin suggests the great advantage in the "marriage" of Bornean traditional knowledge and Western academic discipline.

There remains the problem of mobilizing this local talent. If outside funding could be obtained, the alternatives (not necessarily mutually exclusive) might include: the sponsorship of research posts with local museums or government departments, which would allow release from work commitments with government or other employers; the training (either degree or non-degree) in Southeast Asia studies and social science research techniques of selected Borneans from various states and ethnic groups at a major Western or Southeast Asian university with Bornean interests, with the understanding that they return home and engage in research at the end of their study; and the establishment of a research project and/or social science training course at one of the local centers of higher education, such as Batu Lintang Training College in Kuching. There are already young Borneans with useful skills who are underemployed and might be able to contribute to research projects if released from administrative commitments. The author knew several young and talented Sarawakian university graduates working off their educational bonds in bureaucratic positions below their abilities and training. Herbert Whittier has noted that the *skripsi* (theses) of some young APDN graduates in Kalimantan, now working as government officials, were of high quality (Whittier 1973a). These young Bornean intellectuals, together with the older Dayak, Malay, and Chinese with an interest in local history or oral tradition, might well do work that would go far toward solving some of the remaining historical questions.

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SOME COMMENTS ON CRAIG LOCKARD'S PAPER,
"PERSPECTIVES ON BORNEO'S PAST"

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Any discussion of social history concerning Borneo must begin by pointing up the dearth of published research on the subject. Lockard's paper does not disappoint in this respect. Little has been published on Kalimantan and Sabah, and both areas are ripe for investigation. Rather more has been heard about Sarawak and Brunei because of the efforts of young scholars in the field in the past decade or so. Lockard places the work of Pringle and Brown in its proper place, but I find no reference to Nicholas Tarling's recent work *Britain, the Brookes and Brunei*, which ought to brighten any listing of social histories for its intelligent observations of the Borneo scene in the latter part of the nineteenth century and particularly for its glimpses of the ruling "elite." Likewise Colin Crisswell's several seminal articles (1972a & 1972b) on social patterns in nineteenth century Brunei drawn from his study of indigenous land policy in terms of *tuéin*, *kerajaan* and *kuripan* rights are missing from Lockard's survey.

The section on oral tradition, the recorded observations of indigenous people in interviews, is particularly important. I generally agree with and applaud the efforts at interpretation of past events from remembered facts and from traditions that remain alive. The use to which such interviews and the data obtained are put must be very carefully circumscribed and controlled. My own experience is that the oral accounts are frequently found to be somewhat contorted and colored from actual fact to the point where corroboration is extremely difficult. The value of oral tradition is in the general feeling and color surrounding beliefs and traditional stories that have been passed on and related in interviews. The procedure is most valuable and worthwhile in developing background and "atmospherics." But oral tradition cannot replace written records, where available, as the main historical source.

It seems to me that the main thrust of Lockard's paper lies in the survey which he provides of work in the field of social history that ought to be carried out. He feels, as do I, that research into the history of urban development in Borneo would be especially fruitful. In this respect he sees three types of community ripe for investigation: the port city, such as Sandakan; the Chinese mining town; and the intervening bazaar town.

Research into Borneo-centric history is in its infancy; it requires the individual and corporate attention of scholars and is perhaps the logical vineyard in which young Indonesian and Malaysian scholars could labor with promise of fruitful harvests.

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POLITICAL SCIENCE RESEARCH IN BORNEO

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One should begin any survey of political science research in Borneo by remarking how little of it has been done, especially in Indonesian Kalimantan. Perhaps three main reasons can be suggested for this. Borneo seems almost automatically to be considered properly the territory of the anthropologist--a place into which people of other disciplines blunder at their peril. Secondly, the peoples of Borneo and their governments (in Jakarta and Kuala Lumpur as well as locally) are understandably wary of the pursuits of political scientists--they are always looking for potential or actual conflicts of interest, and tend to ask awkward questions--and have tended to discourage or restrict them. And thirdly, political scientists themselves have tended traditionally to focus their attention on nation-states and their internal and external behavior, and neither as a whole nor in its parts does Borneo neatly fit the category.

Having suggested why little has been done, let us turn our attention to what has. For Indonesian Kalimantan we are still very much dependent upon anthropologists for what political insights we have--particularly concerning relations between ethnic groups. For Malaysian Borneo we are (or are about to be) rather better served. Three books on East Malaysia will soon appear. They are (in order of submission to their respective publishers) K. J. Ratnam and R. S. Milne, *New States in a New Nation: Political Development of Sarawak and Sabah in Malaysia* (Frank Cass and Co., Ltd., 1974); Michael B. Leigh, *The Rising Moon: Political Change in Sarawak* (Sydney University Press, 1974); and Margaret Clark Roff, *The Politics of Belonging: Political Change in Sabah and Sarawak* (Oxford University Press, 1974).

As I have not seen either the Ratnam and Milne or the M. Leigh manuscripts, only comment of the most general kind is possible. It should be stressed, however, that both books (as does my own) come at the end of something like a decade of intensive interest in the areas dealt with, and they must therefore be presumed to represent considered views and a more than superficial knowledge of politics in Sarawak and Sabah. This is a welcome change from the "I visited for three weeks so know all about it" approach that is sometimes thought sufficient for the more obscure or less powerful parts of Asia. What these three books share, then, is the prolonged experience, interest and repeated fieldwork of their authors. It will be interesting to see, therefore, how profoundly different they will surely be. Hopefully, the different standpoints will add dimension and depth to the view, and certainly they must together add enormously to our detailed knowledge.

At this point, we might consider the tools of the political scientist and whether they are useful in the Bornean setting. Traditionally, political scientists have set themselves off from historians by emphasizing not just the use of documentary materials but the fruits of interviews as well. This is no longer a very real disciplinary differential, with more and more modern historians happily utilizing interviews with survivors of whatever

they happen to be studying and with the growing tendency in many places to tape the recollections of those who have been politically influential but who chose not to write their life stories. However, the interview does remain one of the most important (and pleasurable) of the political scientist's keys to reality. And in Borneo the interview is particularly valuable because of the very *newness* of political parties and political participation. Neither in the towns nor in the rural areas have people become blasé or bored, and they are still generally in that blessed (for the interviewer) state of being somewhat flattered to be singled out.

However, a second frequent tool of the political scientist--what is usually termed survey research--is not so happily situated. In the past few years there has been growing disquiet, not just in Sabah and Sarawak but throughout Southeast Asia, concerning the activities of foreign fieldworkers. Research proposals have come under increasingly careful scrutiny, and I can think of no surer recipe for being refused a visa than to state that one wished to administer vast numbers of questionnaires on some sensitive issue. So, for the time being, survey research is probably not practicable, and it may not be particularly appropriate to the Bornean condition.

It seems to me that this leaves the political scientist in Borneo with the tools of the historian (documentation) and the anthropologist (interviews), and I think political scientists ignore the writings of historians and anthropologists at their peril. Certain types of political behavior that might seem perverse, or merely inexplicable, fall into focus when the historical or anthropological dimension is added. Let me give just two examples. Historians of Sabah are somewhat given to talking about "the often uneasy interplay between Sulu overlords and coastal Muslim peoples." I would suggest, with that thought at the back of one's mind, that the internal party politics of the United Sabah National Organization during the 1960s make rather more sense than they otherwise might. Similarly, for any understanding of Sarawak's political process I would commend the thesis advanced by Pringle in *Rajahs and Rebels* that the Iban of the Second Division have historically felt more threatened by the Malay community because of their proximity to Kuching, while the Iban of the Third Division have seen the Chinese agriculturalists of the Sibiu area as more fearsome than the distant Kuching Malays.

Let me end by stating what I believe to be the most interesting, and indeed important development for political scientists, and all social scientists, to work on in Borneo for the future. It is the old historical and sociological problem of "becoming a Malay," but now given a new political impetus and dimension. In his postwar discussion of urgent social science research in the Malay peninsula, Raymond Firth singled out this process as particularly worthy of study, and it is true *a fortiori* for Malaysian Borneo. If one considers (as the Malaysian Constitution does) the twin poles of Malay identity to be religion and language, the political and social implications of mass conversion ceremonies and elimination of all but Malay and English language broadcasts by Radio Sabah become obvious. I have argued that becoming a Malay and becoming Malaysian are increasingly being seen as part and parcel of the same process by the politicians of Sabah and Sarawak and by those of their people who are politically ambitious, too. Neither of these processes can be very easily measured, but they are unquestionably real, probably accelerating, and almost certainly irreversible.

NOTES ON THE STATUS AND FUTURE OF POLITICAL
SCIENCE RESEARCH IN BORNEO

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Margaret Roff has rightly pointed out the dearth of published political science research in Borneo and has suggested a number of reasons to which this deficiency can be attributed. Although several important works have been published recently which do much to fill gaps in our knowledge concerning the development of "politics" (in modern definitional terms) in the turbulent and formative period starting in the early 1960s up to the present, the increasingly authoritarian attitudes that prevail in all parts of Borneo today mean that little objective and empirical research is likely to be permitted in the foreseeable future. It must be recognized that the academic aggressiveness of Western scholars, supported by large grants, conducting research (with published results) on topics often considered sensitive or even offensive by the host government, is no longer acceptable. Southeast Asian governments, Malaysia and Indonesia included, are today applying a sort of Einsteinian theory of political relativity to research applications: believing that to measure is perhaps to change, governmental authorities no longer grant permission for academic research on subjects either considered crucial to the stability of the political status quo or which might prove to be antithetical to pronounced, official ideology and policy. These strictures are equally applicable to indigenous scholars and students who wish to follow the dictates of their own research needs. Indeed, their careers, their futures, are so much more vulnerable to the vagaries of governmental disapproval and political expediency. Throughout Borneo, where modern political structures are of recent origin and where political attitudes towards the state are of recent formulation (and therefore tenuous), the governments concerned cannot afford or so they see it--to allow uncontrolled research into their efforts to forge their various Bornean components into an overarching, stable political entity with the central authority in Jakarta or Kuala Lumpur. Present and future constraints on conducting political science research in Borneo are, then, a fact of life with which researchers must come to terms.

It is indeed a tragedy to students of politics that such a state of affairs should rule, for it is in areas like Borneo that the fundamental processes of political change can be examined in the context of traditional societies and the abiding strengths of those societies' continuity. That is to say, communities of peoples who until only yesterday defined themselves in terms of their own parochial groups--as Murut, Rungus, Kelabit, Iban, or Melanau--are now required to transfer their identification outwards, towards a larger unit represented by the structures and symbols of the state. How is this done? How are local collectivities persuaded to participate or to acquiesce in a new political process that requires profound attitudinal and behavioral change? In Western societies these events

¹In addition to those cited by Margaret Roff, two books by a Sabah scholar, James Ongkili, are worthy of mention: (1) *The Borneo Response to Malaysia 1961-1963* (Singapore: D. Moore Press Ltd., 1967) and (2) *Modernization in East Malaysia 1960-1970* (Kuala Lumpur: Oxford University Press, 1972).

are now largely matters of history (although the Irish, the Basques, the "Yugoslavs,"^t and others, would argue differently), but in Borneo the political, social, and economic readjustments so necessary to state-building endeavors can be observed as ongoing, measurable processes, whose intricacies if analyzed from afar are but seen through a glass, darkly.

States like Malaysia and Indonesia are not homogeneous "nations" like Denmark. The former, for example, does not consist merely of one single social group focused upon a common set of institutions and customs, shared values, and an awareness of its own homogeneity. Rather, it is comprised of a variety of ethnic groups whose differences are accentuated by religion, language, and customary behavior. There is no Malaysian "man" or "woman" in the historical or cultural sense with whom to identify; appeals for state unity cannot be based upon deep-rooted affinities to kinship or territorial ties, to blood or land, but only on the less heady calls of common citizenship, economic improvement, and pride in the state as an efficacious, problem-solving institution.^t In other words, the new state needs to urge its component parts to invest sufficient loyalty (or acceptance) in the state to counterbalance the narrower and stronger pulls of what Clifford Geertz (1963: 109) has called "primordial attachments."^t

The large corpus of conventional political theory that emerged during the 1950s and 1960s to explain those twin phenomena of "national-building"^e (i.e., state-building) and political integration assumes that ethnicity will wither away and die under the assaults of a modernizing society. Integration and state-building theorists believe that the structures of the modern state, provided they perform their role effectively, will overcome the impediments to state unity caused by a nonuniversalistic, traditional parochialism and result in the emergence of a general consensus of values in the Parsonian sense. According to the theorists, this process of modernity has two faces: the one seeking to subdue the blandishments of familial and ethnic particularities, the other urging individuals to participate actively in plans to construct an economically viable, "modern" state. Thus the effects of modernization, brought about by such factors as an efficient communications network, increased educational opportunities, work experience in a modern setting, and increased urbanism, will supposedly lead to the evolution of an integrated citizen and a more cohesive state. The facility of these theories is but a flimsy facade for their ultimate poverty. The persistency of ethnic diversity and its consequences cannot be so lightly brushed aside, for it can equally be posited that modernization (and the greatly increased level of *physical* integration that results therefrom) exacerbates rather than resolves ethnic tensions. Ethnicity, I believe, will never be diminished through modernization alone. It is necessary to look at other means whereby the state attempts to overcome the centrifugal forces of ethnic and cultural pluralism.

One simple, and too often used, solution is that of coercion and repression. But the application of force, or the threat of it, is not a long-term answer. The coercive forces of the state can tamp down the embers of dissent and conflict only for so long, after which means must be found to make the various components cohere, as it were, of their own accord. Otherwise the state will tend to disintegrate or be replaced by a more effective governmental unit. Margaret Roff mentions the process of

²The term itself is misleading. Walker Connor, in a persuasive article, has demonstrated the delusory nature of the term "nation-building"; given the extent of a potentially divisive ethnicity in most newly-emerged states, the last thing any government needs to do is to build up feelings of "nationalism" in a state comprised of disparate ethnic groupings and driven by the fissiparity of diffused cultures. See Connor (1972)e

"becoming a Malay"--that is, conversion to Islam^e It is a factor indeed worthy of further study, for certainly in Sabah (and to a degree in Sarawak and Indonesian Borneo) the practice of converting to Islam goes further than simple religious exercise. To become a Moslem is also to become a Sabahan, and a Malaysian, in a real political sense, symbolizing acceptance not only of a new religion but also of a new political order. To this extent religious conversion in Sabah is tantamount to political conversion, and my own informants assure me that it is consciously used by the political leaders of the state to achieve this goal. Believing that political (and cultural) homogeneity can be accomplished by mass conversion to a single religion, Christians and animists, Kadazan, Chinese and Murut--all non-Moslems--are constantly exhorted by various means (some not so subtle) to swear allegiance to the Crescent of Islam. Indeed the haste, the numbers of persons involved, and the methods of persuasion exerted, have given pause to many devout Moslems, who believe that to become a Moslem is an intensely individual and spiritual conversion not to be lightly undertaken for the furtherance of mundane pursuits. But the process goes on, with results that cannot as yet be predicted.

Religious conversion is nothing new to the Borneo scene. Those people who so roundly criticize political leaders in Sabah and elsewhere for their proselytizing endeavors on behalf of Islam must recognize that conversion was carried on with equal fervor by Christian missionaries during the colonial era--if not with the collusion of colonial officials, then certainly with their acquiescence. Only the political factor is new--which is what ought to concern political scientists. One must also recognize that the tensions, and ultimately the divisions, between church and state--which continue to exercise students of Western, Christian societies--have never been quite so sharply defined in Islam until very recently. For Moslem theologians, the boundaries between what is religion and what is secular have always been blurred and often inconsequential. As one old Singapore Malay *haji* told me, "religion is politics, and politics is religion."^f

The political aspects of becoming a Moslem (and perhaps eventually a Malay in a cultural sense) should then receive the highest research priority from those scholars interested in attempts to weld new, solidary state structures out of a set of diverse ethnic and cultural ingredients. Tom Harrisson has written vividly of the significance of what it means, culturally and historically, to become a Malay (Harrisson 1970: 154-225). He demonstrates that the present Malay population of Southwestern Sarawak is derived from indigenous sources, and not from invasion or immigration. The emergence of a Malay population was not wrought by the arrival of large numbers of external peoples; rather it was, in Harrisson's words, "much more a matter of ideas, of words, and of a new and definite code impacting forcibly." Proceeding from this premise, the differences between, say, a coastal Moslem in Sabah and an inland Christian Kadazan may not be too deeply rooted; conversion of the latter to Islam may result in a more rapid acculturation to "Malay-hood" than one might otherwise have thought. The linkage between this eventuality, if it occurs, and the process of political integration is easy to observe. Harrisson stresses that the prime determinant in the formation of the Malays of Southwestern Sarawak as a separate community is that of religion, of Islam, representing almost the totality of the group's culture, and that although these Malays can be recognized today as a distinct ethnic group they "appeared as Malays only in comparative recent times, as the result of proselytization and (more

^eMost educated Malays today dislike the phrase "masok Melayu" (although it has been in common usage for many decades) inasmuch as it does not describe what they consider to be the only true significance of the act, conversion to Islam. The expression now favored is "masok Islam."

usually) drift from other groups. . . a ." Might not this process now be occurring elsewhere in East Malaysia, especially in Sabah, this time in political as well as in cultural terms? Of such questions is made the very stuff of comparative political research, with relevance for other states in the emerging Third and Fourth Worlds.

Obviously, something will have to be done in Sabah and Sarawak (political scientists speak with less authority on Kalimantan, about which, politically, little has been published) for political parties are solidifying along ethnic lines with potentially dire results for Malaysia's future. Michael Leigh has demonstrated in his excellent book on Sarawak that political parties in that State, after an initial period in which *intra*-communal conflict led to accommodation *between* the various ethnic communities, have not formed in a fashion similar to that obtaining in Western Malaysia, that is, "the politics of Sarawak are on the verge of conforming to the [West] Malaysian pattern, as each ethnic group becomes identified with a communal party, and the lines of cleavage deepen (Leigh 1974: 160).

The cause of this shift from an original tendency towards conflict resolution between ethnic groups to one in which ethnic tensions stand in danger of being ignited stems from a search for party unity and cohesion based upon ethnicity and religion. Unlike Western Malaysia, however, where interethnic conflict is partly resolved by accommodation and bargaining at the top between Malay, Chinese, and Indian elites within the ruling National Front (the old Alliance), there is little compromise in Sarawak between the Moslem-Malay elite and non-Moslem indigenous leaders, especially the Ibana. In Sabah, too, ethnic affiliations have provided the foundations for party formation to the exclusion of ideological considerations, as the USNO (the United Sabah Nationalist Organization, representing the Moslem-Malay sector of the population) struggled in the early days with UPKO (the United Pasokmomogun Kadazan Organization, representing non-Moslem indigenes) for political dominance--with the Chinese parties sitting quietly and pragmatically on the outside, offering support where expedient so to do (Ongkili 1972: 62-63). In both Sarawak and Sabah today the non-Moslem indigenous components of the population are excluded to a substantial degree from participation in those rewards which derive from political control over the state's resources. What does this mean for the future of state-integration and state-building in Malaysia? Can these two latter processes overcome the obstructions placed in their path by ethnic diversity, or must a new approach to this pernicious problem be sought? One alternative is to start from a recognition of the persistency of ethnic attractions as a baseline for new approaches. Once it is understood that ethnicity is not an aberration but a legitimate human condition, leaders of new and pluralistic states may be able to keep conflict within acceptable limits, that is, to domesticate it.

Having proffered a few desultory comments on the nature of the problems to be studied, one needs to turn to the dimensions of these problems and how they may be analyzed. This writer, at least, has serious reservations concerning the validity of quantitative social science research in non-Western societies, over and beyond the probability that the various Borneo governments will not allow it to be conducted. Within the limits of

⁴Equally important is the question of Chinese conversion to Islam. Will those Chinese who become Moslems also become Malay--or at least Malaysians? Or will they still remain discernible ethnic Chinese with Moslem names? Experiences in Indonesia suggest the latter, but in view of the long history of Chinese intermarriage with indigenous (but non-Moslem) natives in Sabah and Sarawak a greater level of integration may be achieved--although one doubts it.

my own experience, having lived and worked in Malaya, Borneo, and Singapore for almost seventeen years, and having administered a small random sample survey to Singapore Malays (1971)⁵ based on quantitative methodology, it is obvious that in terms of the Malay personality, for example, certain traits militate against an accurate and objective analysis of the issues concerned by means of survey research alone. First, most Malays generally avoid face-to-face confrontations and personal controversy unless there are compelling reasons for their involvement; they tend to shy away from entering into interpersonal transactions, especially with a stranger, in which an element of contention may surface. That is to say, questions in a random sample survey which tend to be of a controversial nature will probably elicit an evasive or noncommittal response. Second, Malays--to a far greater extent than Westerners--value politeness, or *budibahasa* in their relations with others. Strangers (unless of a suspicious character) who enter a Malay village, or a Malay household, are invariably treated with a high degree of formalism and courtesy, both of which lessen the chances of obtaining straightforward responses in a survey administered by persons unknown to the respondents, even if the interviewers are themselves Malay. Many Malays over the years have remarked to me, and have otherwise demonstrated by their behavior, that one aspect of Malay courtesy is a predilection to tell those outsiders who venture to question them what they think the questioners *want to know*, not what they really perceive to be the truth. Such behavior of course is not motivated by deceit but by a desire to be courteous. Even though attempts may be made in the survey to design questions which might counter these cultural idiosyncrasies, including use of open-ended and not precoded questions, it is impossible to be quite sanguine that a degree of bias has not been introduced. Some of these reservations apply also to other cultural and ethnic groups in Borneo.

How, then, is research to be conducted under these circumstances? The interview, as Margaret Ross has said, is an important item in political science research weaponry--the more so if the confidence is gained of the person interviewed. (If nothing else interviews are essential in any definition of the extent of the topic to be studied.) The anthropologist's method of participatory observation, whereby the researcher thoroughly immerses himself or herself within the host community over a period of time, is of equal significance.

The documents of the historian, too, are ignored at one's peril, for attitudinal studies alone, based on behavior at a specific point in time, do not convey any sense of history, nor of social structures helping to shape political outcomes, and analysis often marches in an unreal social and historical vacuum. Evidence collected by means of attitudinal research alone is seriously flawed by its inability to penetrate into more than one time period: it is not enough, for instance, to ascertain that a Sarawak Iban is at one particular moment (i.e., when he or she is approached by an interviewer) dissatisfied with the political process, for such an attitude is the end result of a complicated set of historical and social circumstances, many of which the person interviewed will be unable to articulate or even to identify. It seems to me that the ends of a research project in the field of political science are best served by a combination of the above methods, including survey research if desired as a complementary study, in mutual reinforcement.

In the final analysis, however, all these comments and notes are meaningless unless existing attitudes towards scholarly research among the present rulers of the various Borneo territories change in a direction more

⁵See S. S. Bedlington (1974: Appendix I)a

receptive to the demands of honest and objective scholarship, conducted by both indigenous⁶ and external researchers--and about this most observers, including myself, are pessimistic.

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⁶I understand that a branch of either the University of Malaysia or the Universiti Kebangsaan is shortly to be established in Sabah. Hopefully this will stimulate local research.

THE STATUS OF SOCIAL-ANTHROPOLOGICAL RESEARCH IN BORNEO

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Introduction

To assess the status of social-anthropological research in Borneo, I shall first summarize the results of research since World War II, indicating the contributions made to social-anthropological theory, and then consider the interrelationship between social-anthropological inquiry and the growth of knowledge in other social sciences involved in Bornean research. I shall also be concerned with social anthropology's potential contribution to planning and policy formation for social development in Borneo, because economic and educational development, as usually phrased, can destroy those very resources essential for the productive and orderly social development of Borneo. Therefore, I shall discuss the implications of development for the indigenous peoples and for the social, cultural, and ecological resources of Borneo. Then, as no assessment of the status of a discipline can afford to ignore the development of research personnel, particularly local research personnel, and the state of funding, I shall consider these. And finally, I shall attempt to delineate what future contributions to knowledge and social-anthropological theory further research in Borneo might have under these conditions.

However, I shall first present a brief synopsis of Borneo ethnography to provide a background against which we can assess what has been done, what still needs to be done, what impact change is likely to have, and what contributions social-anthropological research could make to the building of new Bornean societies.

Synopsis of Borneo Ethnography^t

Ethnographically, Borneo is a highly complex region, the nature of which we are just only beginning to understand, for the ethnographic map itself is far from complete. While this ethnographic complexity presents some fascinating problems both for the development of anthropological theory and for the culture history of the region, actual social-anthropological research has been sparse and scattered in comparison to other regions of the world. For example, I have listed below the number of social-anthropological investigations that have been undertaken in the various political units of Borneo since World War II (as of January 1974):

¹I will not attempt to include here a complete bibliography of publications resulting from the research described below. The reader is referred to the bibliography in LeBar (1972)e For a more detailed summary of work in progress or just recently completed and its contribution to social-anthropological theory, see Appell 1976a and 1976b, and specifically for Sarawak, see Appell 1977e

Sabah	6
Brunei	3
Sarawak	23
Kalimantan	5
Total	37

The number averages out to a rate of about 1.4 per year and can be compared to that in New Guinea in order to illustrate Borneo's relative neglect. New Guinea, while larger than Borneo, has a smaller population. Nevertheless, in one year, 1968, there were 64 social-anthropological investigations going on in the Trust Territory of Papua—New Guinea alone, which includes only the eastern half of New Guinea (cf. Bulmer 1969). In 1971, another 63 research projects were listed in *Man in New Guinea* (Vol. 3, No. 1, 1971), again for the Trust Territory of Papua—New Guinea alone.

The neglect of Borneo ethnography can also be illustrated by summarizing the various levels of sociocultural complexity that exist in the region and indicating what research has been directed towards these various levels. At the lowest level of sociocultural complexity there are nomadic hunting and gathering groups that can still be found in the interior jungles, although many have been encouraged to settle in permanent villages and engage in agriculture. These groups are generally referred to collectively as Punan. However, this in no way implies that these groups are in fact more closely related one to another, either ethnically or linguistically, than to their more settled neighbors; for there is evidence that many of these Punan groups have distinct and separate sociocultural roots. Yet this fast disappearing level of sociocultural integration has received almost no attention from anthropologists, and to date there is only the research of R. Needham among the Penan of Sarawak and the work of J. Nicolaisen among the same group. I can think of no anthropological research more urgent than the study of these peoples.²

Also at the same level of sociocultural integration are various groups of sea nomads living in boats along some of the coasts and engaging in maritime hunting and gathering. Again, we have had only one study of these groups, by C. A. Sather. In fact, we are not even sure of the distribution and cultural affinities of these various sea nomads. There are also populations of these sea nomads that have come ashore and taken up livestock raising or agriculture, and again, as with the nomadic interior hunters and gatherers, we have no studies of the processes and structural entailments of this critical change in cultural ecology, with the exception of that by Needham (1965) among the Penan.

At the next level of sociocultural integration, there are a variety of societies based on swidden agriculture that are found widely distributed throughout the interior of Borneo, such as those studied by J. D. Freeman (Iban of Sarawak), W. R. Geddes (Bidayuh Land Dayak of Sarawak), W. M. Schneider (Selako Dayak of Sarawak), J. Rousseau (Kayan of Sarawak), J. Deegan (Lun Bawang of Sarawak), P. Metcalf (Berawan of Sarawak), I. Nicolaisen (Punan Bah of Sarawak), H. and P. Whittier (Kenyah of Sarawak and Kalimantan), A. Hudson (Ma'anyan Dayak of Kalimantan), J. Avé (Ot Danum of Kalimantan), V. King (Maloh Dayak of Kalimantan), D. Miles (Ngaju of Kalimantan), and G. N. Appell (Rungus of Sabah).

Some of these swidden societies have essentially egalitarian social systems, while others have highly developed systems of social stratification, as defined by Morris (1967), involving at least three classes. Work

²Since this writing, B. Cerf of Columbia University plans research among the Punan and related groups in East Kalimantan.

on these types of societies has just begun with that of H. and P. Whittier among the Kenyah and J. Rousseau among the Kayan; because of the wide distribution of these societies, much more field research is needed.

Although this second level of sociocultural integration has received the most attention from anthropologists, there remain a large number of ethnic groups yet to be ethnographically described. In addition, the problem of the nature and function of a well-developed system of social stratification in swidden societies still needs to be resolved. We might find that highly stratified swidden societies should be classed at a higher level of sociocultural integration than the egalitarian ones, perhaps equivalent to or even higher than some of the irrigation societies which have been tentatively classed at the next level.

At the third level of complexity are those societies whose cultural ecology is based on wet-rice agriculture, such as the Kadayan, Kelabit, and many Dusunic-speaking groups in Sabah. This is a tentative conclusion based largely on the fact that these irrigation societies have a more advanced cultural ecology than the stratified swidden societies. Whether or not many of them are more complex socioculturally is open to question, for the degree of sociocultural elaboration of the irrigation societies is not clear, particularly those elaborations that one might expect to arise in response to the technology of irrigation. For example, the Kelabit, wet-rice cultivators of the interior highlands of Sarawak, appear to have a highly stratified society, but many of the other wet-rice societies, such as those in Sabah, do not; none matches the stratification found among the swidden-based Kenyah and Kayan. This situation, in marked contrast to that in highland Burma, presents some rather interesting problems on the genesis and maintenance of social stratification, and it is of particular interest since some theorists have recently maintained that social stratification cannot be supported on such a narrow economic base as swidden agriculture.³

This level might also be expanded to include those societies representing a transitional state between swidden and wet-rice agriculture or between a swidden and a cash-crop economy. Research in this type of society has been carried out among Kadayan by Allen R. Maxwell in Brunei, the Bisaya by Roger D. Peranio in Sarawak, the Lun Bawang by James L. Deegan in Sarawak, the Melanau of Sarawak by H. S. Morris, the Lun Dayeh by Jay B. Crain in Sabah, and the Ranau Dusun of Sabah by Robert Harrison.

At a higher level of indigenous sociocultural complexity we have the Islamic sultanates that arose primarily along the coast to control trade with the interior. The only social-anthropological research done on these to date has been that of D. Brown in Brunei.

Finally, at perhaps the highest level of sociocultural integration, are the various plural societies that arose as a result of the onset of colonialism. Although Morris (1967) has attempted to delineate some of the issues in the study of plural societies, little research has yet been done on the various sections of these societies, with the exception of the Chinese by D. Fortier (Sabah), R. Fidler (Sarawak), and Ju'K'ang Tien (Sarawak), and the Malays of Sarawak by T. Harrison and by Zainal Kling.

³It has also recently been claimed that swidden systems cannot support political institutions such as tribal chieftainship. Yet the Northern Murut peoples apparently had such chiefs, while neighboring peoples at the same level or at a higher level of sociocultural integration did not.

Theoretical Orientations: The Structure of Cognatic Societies

Borneo--The Land of Cognatic Societies

As far as we know, all the societies of Borneo are cognatic, lacking unilineal descent groups. And it is this wide distribution of cognatic societies, both ecologically and in terms of levels of sociocultural integration, that makes Borneo such a potentially fruitful region for developing our understanding of social processes. With the use of closely controlled comparisons of these societies, we can eventually isolate the basic organizational features of cognatic, and thereby all, social systems.

The second most salient feature of the anthropological landscape in Borneo is that kinship performs such a small organizational role in practically all the social systems found there. Perhaps this should not be unexpected, considering that Bornean societies are primarily cognatic.

However, there are two reasons why it is difficult at this point to assess the impact that the study of the cognatic societies of Borneo has had on social-anthropological theory. First, much of the research done in the past decade has not yet been fully published, or has only recently been published, so that its influence has yet to be felt. Second, it is my opinion that research on Bornean social systems has not had the recognition that it should because the wave of social structuralism--note I do not say structural-functionalism, for this is a red herring--because the wave of social structuralism broke around the apparent unresolvable problem of cognatic social organization (cf. Appell 1973b, n.d.a). As a result, and because it may at first appear more productive to harvest new fields, the attention of social anthropologists turned to other problems, such as those represented in symbolic structuralism, cognitive structuralism, various other actor-oriented approaches, and so forth. In sum, in my view, social-anthropological inquiry has failed to solve the anomaly presented by cognatic societies, and therefore it has failed to develop its theory in the domain of social organization fully. Instead, it has turned to other fields just at that moment when a solution of this anomaly would have produced a truly fruitful, powerful, and productive paradigm (cf. Appell 1973a).

But, be that as it may, what can we say has been the contribution to social-anthropological theory to date by research on the cognatic societies of Borneo? First, there is the well-known, pioneering work of Freeman among the Iban (1955, 1970). His analysis of a new form of cognatic social organization has had a singular and purging effect. Furthermore, his description of a new form of family organization, the Iban *bilek* family, and the analysis of its developmental cycle now serve as a paradigm of craftsmanship work for other Bornean research. And the discovery in a small-scale society of a corporate group, not based on kinship, again the *bilek* family, was a major discovery, the implications of which I do not think have yet been fully realized. He has also contributed to a rethinking of the concept of kindred (cf. Appell 1967), and of course his study of the cultural ecology of the Iban swidden system has had a singular influence on subsequent work.

Geddes's work (1954) among the Bidayuh Land Dayak has produced the description of a new type of land tenure system involving ambilineal categories.⁴ Morris has described a cognatic society with an elaborate system

⁴I have attempted to reanalyze the data of Geddes and Freeman in light of more recent research on land tenure in Borneo (cf. Appell 1971, n.d.a)

of social ranking, and as a result he has made a significant contribution to our understanding of the issues in the study of social stratification and the nature of plural societies.

Needham has used his ethnographic materials on the Penan in a number of interesting comparative articles dealing with specific theoretical issues. As a result he has made a major contribution to our understanding of the function of mourning-terms and death-names (1954, 1959, 1965). Specifically, in this latter article on death-names he uses the method of closely controlled comparison and concomitant variations to demonstrate that the use of Penan death-names contracts step by step with the various stages in the breakdown of Penan social solidarity (as measured by degree of acculturation to other sociocultural systems). Furthermore, he has broken important new ground in his study of the contradiction, caused by demographic factors, that arises between kinship category and age. Thus individuals of the same age may belong to different categories, and individuals of the same category may be of different ages. He concludes that in lineal systems this problem is resolved by ordering social relations primarily by categories, whereas in cognatic societies there is a lesser emphasis on categories in favor of social distinctions based most generally on relative age (1966).

In his work among the Bajau Laut (1971), Sather has demonstrated how a cognatic society without any corporate social groupings can maintain social order and continuity. He has also described a new type of cognatic, extended family structure, and a new form of kindred which ramifies only in ascending generations from the *propositus*.

Brown's research on the Brunei Sultanate has added materially to our understanding of the relation between perceived history and social structure (1970); the factors contributing to the persistence of an element of social structure (1973a); the nature of relations between ethnic groups organized within a single governmental hierarchy (1973b); the genesis of ethnic diversity as a function of the hereditary closure of ranking systems (1973c); and the development of social historiography as a function of open social systems (n.d.). He has also developed a cross-cultural approach to the analysis and scaling of corporate social groupings (1974).

My own work among the Rungus has been directed towards developing an abstract, analytical methodology for the analysis of the property domain of all societies, not just cognatic ones. I believe that our concepts have been much too particularistic, focusing as a result too much on kinship and descent. Instead, I believe that the breakthrough in our understanding of cognatic types of social organization, and thereby all types of social organization, will come from studying property relations (cf. Appell 1973a, 1974, n.d.a). I have attempted a new definition of corporate social groupings, which I hope will be a better analytical tool for discovering the discriminations occurring in all social systems. I have attempted to define the kindred more precisely by providing observational procedures for isolating this type of social unit (cf. Appell 1967).

At this point, before I move on to consider other theoretical orientations used in Bornean studies, I would like to correct one misrepresentation. I originally said that all societies in Borneo are essentially cognatic. This may not be so. There is some evidence from the survey work of D. A. Horr that there may be a series of societies in the Kinabatangan River basin of Sabah that have marriage sections, some with only two, others with multiple sections, and membership in them is apparently based on patrilineal descent. Unfortunately, this region may be still closed to research, but it certainly represents one of the most interesting areas in Borneo in terms of the development of social-anthropological theory.

Theoretical Orientations: Actor-Centered Approaches

The primary theoretical orientation of those who have done fieldwork in Borneo has been institutional analysis. There has been no research whose primary focus is that of symbolic structuralism, although Borneo offers some fascinating opportunities and there certainly should be such research as long as the dilemma of verification is properly dealt with. I do not see that this is a major hurdle. In terms of explicitly actor-centered approaches, with the exception of the work of Maxwell in Brunei among the Kedayan, there has been no research done specifically in the mode of cognitive structuralism. There are other approaches in social anthropology that have emphasized the individual as manipulator and creator of his own social world, such as situational analysis and network analysis, but, with the exception of a study of village political processes by H. Whittier, none of these methodologies has been attempted in Borneo.

Other Approaches to Social-Anthropological Inquiry

The study of the cultural ecology of Bornean peoples is an important topic, strangely neglected except for Freeman's pioneering study of the swidden ecology of the Iban and Christine Padoch's ongoing study of Iban with mixed dry- and wet-rice agriculture. We have as yet no full-scale, modern study of a Bornean religious system, although Freeman plans to publish his materials soon on the Iban and E. Jensen is in the process of publishing his studies on Iban world view. Economic anthropology has also been neglected in research on Borneo. And socialization as a field of inquiry has been the concern of only two field projects. Legal anthropology is also just barely beginning as a field of inquiry. Several years ago P. Goethals studied the *adat* law courts in Sabah, and M. Heppell is now working on the Iban *adat* law. Cultural change has largely been ignored except for the work now being done in Sarawak. There, H. S. Morris has been working among the Melanau peoples, studying the nature of change in their social organization and belief systems as a result of conversion to Islam or Christianity, and his project promises to be extremely productive in terms of its impact on anthropological theory. Morris and Sather have also been involved in a study of the social problems of resettlement in Sarawak, and these results are to be published soon. E. Jensen, V. Sutlive, and Peter Kedit have all made studies of culture change among the Iban. Sutlive's work in particular is not only a contribution to the study of culture change but also to the study of the processes of urbanization, the first such social-anthropological study done in Borneo. The interesting conclusion emerging here is that there is little evidence of disorganization among urbanizing Iban, and I think the crucial question arising from Sutlive's and others' study on Iban culture change is this: What is it in the nature of their social organization that makes the Iban so adaptive that they can move into all sorts of cultural-ecological niches without major cultural distortion and disorientation?

Research in other fields of the anthropological sciences, such as psychological anthropology and medical anthropology, has been almost nonexistent (cf. Appell 1973b and 1977 for a review of these). Yet it would seem that questions such as those involving socialization or indigenous medical practices and beliefs would have considerable value not only to science but to the future development of the area.

Social-Anthropological Inquiry and Its Contribution to Research in the Other Social Sciences

In Borneo, perhaps more than in other regions of the world, progress in one social science is dependent on progress in the others. For social-anthropological inquiry this interdependence is particularly true: the results of research in this discipline frequently constitute the basis for research in other disciplines. This fundamental and generative nature of the anthropological sciences, not only to the other social sciences but also to governmental planning and the development of policy, was recognized early on by the Rajahs Brooke (cf. Appell 1977). The result has been that social-anthropological research in Sarawak has not only made a significant and basic contribution to policy formation, but it has also contributed more to the development of social-anthropological theory there than research in any of the other Bornean states.

The reasons for this fundamental importance of anthropological research are many and complex. First, the press of ecological factors is stronger in Borneo than in many other regions, and the interlocking of these factors with the cultural economies of the various peoples is both more basic and more complex. Second, the diversity of peoples in Borneo poses many problems and questions, both scholarly and practical. Third, less is known anthropologically about the peoples of Borneo than about peoples in many other regions. But as change is rapidly occurring, anthropological knowledge has an important part to play in minimizing the cultural dislocations and stresses that might arise.

However, the relation of social anthropology with other social science disciplines is far from being one-way. Pringle (1970), for example, has shown that close consideration of both historical and anthropological evidence has produced insights on the nature of Brooke rule and its impact on the indigenous peoples that not only make a contribution to the social history of the country but also have important implications for the interpretation of Bornean anthropology. Furthermore, Black (1969, 1970), in his historical study of the administration of the British North Borneo Company, has produced evidence and drawn conclusions that explain much of the previously anomalous anthropological materials. His study also provides critical data on the status of ethnic groups in Sabah during that period and on the impact on these societies of the British North Borneo Company's use of Iban and other exogenous peoples in policing the country. No future anthropological inquiry can proceed without considering these results.

Social Anthropology's Contribution to Planning and Development

Social anthropology has played almost no part in the preparation of plans and policy for social and economic development. The only exception was the research that grew out of Leach's original survey in Sarawak in 1947 and the work by C. A. Sather and H. S. Morris, again in Sarawak, in gathering basic socioeconomic data for the Miri-Bintulu project.

Furthermore, social anthropologists have not been requested to make any evaluation of the results of development that could be fed back to planners to guide their future work. Therefore, it is difficult to estimate the contribution social-anthropological research and knowledge could be making to current development projects. However, there are indications that all is not well for the future, not only in the movement of peoples to

urban areas but also in the indices of social stress that are appearing. For example, in one instance we know that a people who had been moved from their usual cultural niche and provided with a new cultural economy experienced an increased rate of illness, for the planners did not provide them with substitute sources for the protein they were accustomed to. A degenerating diet is in fact a common product of development (cf. Appell 1975a), and with the effect that diet has on both physical and mental health, we can expect that much development, as now planned, may in fact create poverty and rural slums.

I would say that in addition to the lack of funds to support research, which I shall discuss shortly, and the lack of sufficient research personnel, the lack of appreciation for the results of social-anthropological inquiry in development planning is the most discouraging aspect of social-anthropological research in Borneo.

The Pernicious Effects of Development

"Development" is one of those curious words that produces a relatively good feeling on the part of the user and his audience while at the same time hiding its negative and pernicious effects. Development simply means destruction, and in the process of destruction and the creation of "opportunities," development also creates poverty. I have documented this elsewhere (cf. Appell 1975a, 1975b, 1975c, n.d.b) and consequently will only review three points here: the problem of social stress in development, the loss of knowledge and cultural inventions, and the loss of biological mechanisms. These last two consequences of development have, however, an impact not just on the peoples of Borneo but on the world population as a whole, as they affect its capacities to adapt to future challenges.

The Problem of Social Stress

Development involves social and cultural change and for each act of introduced change there must also be a commensurate act of adjustment; but in coping with change the means of adaptation may be functional or dysfunctional (cf. Appell n.d.b). Neither developers nor social scientists have really focused on this problem, so we do not have any full measure of the costs resulting from the stress of change (cf. Appell 1975a). Not having satisfactory knowledge of the full implications of such stress in a society, we are also in a difficult situation to introduce means to minimize it.

We do know, however, that health impairment is one usual concomitant of change. For example, in one development project in Africa ten percent of the population died within eighteen months. Health impairment comes from two sources. First, when a society moves from its former style of articulation with its ecosystem into a new cultural ecology, it does not have the sociocultural mechanisms to deal with the new ecosystem. As a result, it becomes vulnerable to new disease vectors and to the other health hazards endogenous to the new environment. Second, psychological and physiological health impairment arises from the stresses produced in trying to adapt to a new sociocultural system. Increased medical care during the period of transition is necessary.

These are not the only costs that are produced as a result of sociocultural stress. A drop in productive capacities and efficiencies usually follows psychological and physiological health impairment. Family relations

become more brittle, resulting in increased divorce and errors in socialization, which add to the cost of development, but affect the next generation. There is frequently a rise in antisocial acts of all sorts, not only as a result of the erosion of former methods of social control, but also as a reaction to the psychological stress that accompanies sociocultural change.

Thus, sociocultural change, and development in particular, puts stress on a society through an increased demand on its powers to cope. How well it copes will determine its future. Unfortunately, societies seldom adapt well, particularly without the benefit of planning and insight provided by social-anthropological research.

One final point in this regard. With change there is always loss. Members of societies undergoing change frequently suffer from what might be called "social separation anxieties." These may arise from many sources, but two appear to be particularly salient.⁵ First, there is the distress that arises from a loss of social identity as the old structure changes, before a new social identity can be constructed. Second, separation anxieties produce what might be called "the social bereavement syndrome" in social change. Change, even when it is desired, produces many conflicting emotions over the lost past, and unless these are resolved a population may be unable, may even refuse, to accept change, and as a result will regress. In order for a population to work through the emotional conflicts in change, as in an individual's bereavement, the past must be available for them to look back upon to see who they were and to provide direction for the future. The future cannot be achieved or fully integrated into a new social order unless those involved feel that there is a meaningful link with the past, that there is continuity between the past and the future, and that the past represented a valued state. Otherwise, negative social-psychological reactions may occur. These can range from apathy and fear of the future to widespread aggressive reactions against the agents of change.

It is here that the various Bornean museums have made and will continue to make an important contribution. In these museums, the accumulated traditions of the indigenous peoples have been and are being collected through the efforts of the staff ethnologists and the work of social anthropologists. However, this effort is hardly begun in many instances, and much more research is needed towards the day when the indigenous peoples want to look back on their cultural heritage to provide them with a sense of history, identity, and direction. It is here that social-anthropological research can be such a valuable contribution to the development of the local societies. One would wish that there were greater local support for this aspect of social-anthropological research, as it is well worth the investment.

The Loss of Indigenous Knowledge and Cultural Inventions

As man adapted to the various ecological niches of the world, he accumulated important knowledge about his ecosystem and devised critical cultural inventions to deal with his environment. However, as indigenous societies were overwhelmed with the spread of the technology of the Western world, much of their unique knowledge was lost, even though until the twentieth century vast amounts of this knowledge and the concomitant cultural inventions were incorporated into the developing worldwide industrial society (cf. Appell 1975a, 1975b, 1975c). Moody (1966), for example, has shown

⁵See Appell 1975a and n.d.b for a discussion of other sources of stress in change, such as role conflict and ambiguity and the aspiration-achievement gap. Also see Appell 1977 for an extended discussion of the theme in this section.

what has been incorporated into Western medicine from indigenous societies. The extent of these incorporations has still to be adequately assessed; but they were facilitated by the narrower gap in level of sociocultural integration between the Western societies and the indigenous ones than that which exists today and by the slow speed of destruction of the local societies. When a society undergoes change today, its destruction is more extensive and more precipitous. The problem of loss of indigenous knowledge is illustrated by the fact that I know of no research undertaken in Borneo to assess the value of indigenous pharmacopoeia and chemotherapeutic knowledge (cf. Appell 1975a, 1975b, 1975c, 1977 for examples of the value of such knowledge). There have also been only a few instances of research to collect the valuable agricultural knowledge of the indigenous peoples.

In other words, development is moving rapidly, with the result that much of the indigenous knowledge and the cultural inventions of the peoples of Borneo are being lost without any assessment being made of their potential value to mankind. Social-anthropological research has frequently functioned to preserve such knowledge, but with the lack of research support that now exists, little help in resolving this problem can be expected from this discipline. The problem is nowhere more apparent than in the loss of biological mechanisms that is occurring.

The Loss of Biological Mechanisms

Each ecosystem has a set of unique wild biological mechanisms consisting of both plants and animals. In the process of adapting to a particular ecosystem, man has domesticated and bred a variety of biological mechanisms, incorporating these into his cultural inventory for the purpose of surviving in his ecosystem. Thus, each of the societies of Borneo has unique strains of cultivars, unique strains of domestic animals, and unique uses to which they put the biological mechanisms that are endogenous to their ecosystem. These have a potential value for mankind's future adaptation to a limited biosphere. For example, in rice alone, I estimate that there is a potential to improve productivity of rice agriculture in the world to the extent of several billions of dollars. Yet in Borneo, to my knowledge, there have been only two limited attempts to gather strains for possible crossbreeding (cf. Appell 1970). At the same time, local varieties are rapidly being lost through indigenous peoples moving to urban areas and through the replacement of present rice varieties with more refined, but genetically more vulnerable, varieties developed by Western agricultural scientists.

To document the implications of the loss of indigenous knowledge and the loss of cultural inventions through rapid change resulting from development is not the purpose of this paper. For this, let me refer you to other papers of mine and the references they incorporate (cf. Appell 1975a, 1975b, 1975c, 1977). Let me assure that the loss is enormous; it is a needless waste, for without these biological mechanisms, without this indigenous knowledge, we limit our ability to adapt.

Bornean Students of Social Anthropology and Research Personnel

While a great deal of ethnological research is being done at the various civil service academies and universities of Kalimantan and at the various Bornean museums, such as the Sarawak and Brunei museums, this is generally descriptive work of various cultural traits and patterns of indigenous peoples, focusing frequently on culture history. The work is of high quality, and the contributions made are significant. However, this research does not fill the need for social-anthropological inquiry focusing

on questions of how social systems operate and the concomitant social processes. Without the close relationship between social-anthropological research and ethnological inquiry (cfe Appell 1973, 1977), both disciplines suffer; for theoretical questions enter ethnological inquiry from the social-anthropological side, while the data produced from ethnological inquiry challenge the social anthropologist to develop better theories. The lack of social-anthropological research as an integral part of local ethnological and ethnographic work is certainly one of the fundamental problems facing the development of the anthropological sciences in Borneo.

Another problem inhibiting the development of these sciences is the lack of local students. To my knowledge, there are no students from Borneo now engaged in training for the Ph.D. in social anthropology, nor has there been any such degree granted to a Bornean student. Part of the problem is that in the four political divisions of Borneo--Sarawak, Brunei, Kalimantan, and Sabah--there are no universities providing advanced training in social anthropology as there are, for example, in Papua-New Guinea. Universities providing such training are located in regions near the metropolitan centers of Malaysia and Indonesia. But the problem is also exacerbated by the students' attitudes toward anthropology. As one anthropologist wrote: "In general anthropology is not a popular subject . . . because of the association with 'primitives.' If anything, the feeling is one of positive apathy. . . . to a people obsessed with being [as] up-to-date and 'modern' as possible, anthropology in contrast to the other social sciences has nothing to say"

Obviously, part of the fault may be with how we present the anthropological sciences, but a large part of the problem lies with national governments. Until they take the initiative, recognize the potential worth of anthropological insight and knowledge, and provide the necessary economic opportunities for those who have studied anthropology, apathy towards anthropological education will continue; and until anthropologists themselves have a receptive audience for their potential contributions towards building the new societies in the region, we will not only fail to attract the best students, but anthropological research itself will also remain on a starvation diet.

The Funding of Social-Anthropological Research

The status of funding for social-anthropological research in Borneo is dismal and is reflected in the rate of research now going on. For instance, in comparison to New Guinea, which is roughly of equivalent size demographically and geographically, I estimate from the figures given at the beginning of this paper that over twenty times as much research is going on there at the present time as in Borneo. There is, unfortunately, no institution or foundation that has taken Borneo as its focus of concern or major interest. Funding for research in Southeast Asia in general is not much better (cf. Southeast Asia Regional Council 1974). Furthermore, several of the major foundations that are concerned with Asia and Southeast Asia have recently modified their policies on supporting research to focus primarily on projects that involve indigenous facilities and personnel. The irony is that there are no indigenous facilities or personnel dealing specifically with social-anthropological research in Borneo.

The point is that without local student interest and government support there can be no research projects that involve indigenous personnel; without funds there are neither students nor research, from internal or external institutions. What social-anthropological research does take

place, and that which will take place in Borneo, is the result of the unpredictable and sporadic interests of graduate students from Western universities doing research for their Ph.D.s in social anthropology and the availability of funds to support such doctoral research.⁶ Thus, fundamental to the question is whether sufficient funds can be generated for Bornean studies to do the necessary research before it is too late. The present method of causal, haphazard, and adventitious funding of research just will not do. Social-anthropological research in Borneo can only develop and will only develop over the next two decades through an increase in support from external sources, by the transfusion of external research personnel and funding, and by local government support and interest. If the world scholarly community evinced sufficient interest in these problems, internal interest would eventually develop.

The Future of Social-Anthropological Inquiry in Borneo

The Potential Contribution to Social-Anthropological Theory

The research that can be undertaken in Borneo in the future will not only contribute to filling in the ethnographic map, which is far from complete, but it will also continue to make significant contributions to the development of social-anthropological theory. It is the cognatic societies of Borneo that still offer the major opportunity for developing social-anthropological theory. Research should provide a healthy corrective to assumption and theory based on experience with unilineal societies. The cognatic societies of Borneo offer a significant puzzle. For if social-anthropological inquiry had really focused on the anomaly presented by cognatic societies, rather than turning to other problems, I believe that the theory of social structure would have been significantly revised by now; it would have become more productive, and it would have opened up new horizons, rather than leading us, as claimed by some, to an apparent dead end. The challenge still has not been fully accepted, and anthropologists are harvesting in the other fields of symbolic anthropology and social stratagems. However, when the cutting edge of their question-sets has become dulled, when the growth rate in their findings slows, when they gather more chaff than grain, then I suspect we will have a resurgence of interest in many of the problems of the nature of cognatic social organization. The point is that for too long we have permitted the question-set of kinship to produce answers that appeared more and more ridiculous, rather than facing the hard questions posed by cognatic social organization.⁷

Finally, it should be pointed out that the nature of Borneo ethnography is such that it presents a unique opportunity for the use of closely controlled comparisons, not only to frame and test problems in social-anthropological theory, but also to add to our knowledge and understanding of social processes. Unfortunately, we still do not have sufficient ethnographies to proceed at this level. But it is hoped that the time is not too far away when there will be enough data compiled to use this approach.

⁶Rubenstein in this volume draws attention to the point that Sarawak is limiting research by foreign students for a variety of reasons including bad experiences they have had. Please see the Introduction for comments on this problem.

⁷See Appell 1976a for an extended analysis of this problem.

The Problem of Urgent Research

As I have pointed out, the societies of Borneo are undergoing rapid change, with the result that their knowledge and cultural inventions are being lost forever to anthropological science, much to the detriment of mankind. There does not seem to be any interest on the part of funding institutions or the local states of Borneo to attempt to preserve these cultural heritages. The *Borneo Research Bulletin* occasionally has published lists of societies that are rapidly losing their cultural traditions, trying to encourage research before it is too late. Yet to date I know of no research that has been undertaken in response to these needs. Interior hunters and gatherers are rapidly being resettled, but there still is no study of these groups in Kalimantan. We have had only one study of the Bornean sultanates, even though this level of sociocultural integration has had such an important impact on the social history and development of Borneo. And we still need a full-scale study of the social elaborations that have occurred as a result of shifting to an irrigation-based cultural economy (cf. Hunt and Hunt 1973 for a review of the possibilities and the state of knowledge in this field). Thus, we do not know what social inventions have been made to deal with the control and distribution of water resources, although this kind of information would be extremely useful in the construction of the large-scale irrigation schemes that are being put into operation or which are being planned.

The Future

We have indicated that it does not appear that there will be much encouragement to social-anthropological research in Borneo from the field of development planning and policy formation. We have also indicated that there appears to be no interest in the product of social-anthropological research in terms of evaluating the results of development and change to provide feedback to planners. There also seems to be little interest in medical anthropology, even though medical-anthropological research has a great deal to contribute towards the development of healthier populations. The situation is similar in psychological anthropology, even though this branch of the discipline has much to contribute towards understanding socialization and its impact on development. There appears to be little internal interest in using the results of social-anthropological research for developing school curricula on the basis of local history, local knowledge, and local literature. It does seem a bit inappropriate to base school curricula on Western history, literature, and knowledge--an approach which clearly contributes to a loss of self-identity.

What then is the future of social-anthropological research in Borneo? It appears that such research will still lie in the hands of foreign institutions and foreign research personnel, providing that local governments continue to permit it. This may indeed be problematical, since they do not appear to be interested in the products of social-anthropological inquiry. However, as prospects for such foreign institutions to carry out Bornean research are also bleak, we can foresee that a major opportunity to contribute to anthropological knowledge and to contribute to the orderly development of the societies of Borneo will be irretrievably lost.

Conclusion

In summing up my assessment, I must conclude that the future looks dismal. Because of lack of funds, there is little external interest in the social anthropology of Borneo, even though the ethnographic situation there

offers unique opportunities for social-anthropological theory; nor is there much internal interest in social anthropology. This lack of internal interest not only has detrimental effects for the development of the discipline, but it would also seem to be short-sighted in the long run, since social-anthropological inquiry can help in preparing appropriate plans and policy for development, in evaluating the success of such plans, in developing a base for the cultural traditions of the region which could be used for teaching--and for providing the historical and cultural background that no society can be without and still build a healthy social identity, and in preserving the unique knowledge and cultural inventions and the unique biological mechanisms that the indigenous societies have developed, which could someday be of immeasurable importance to mankind. How one breaks this Gordian knot is difficult to say, but it seems that the development of adequate financial support to Bornean research would be the first step.

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COMMENTS

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1. Dr. Appell has argued cogently and eloquently for more basic anthropological and allied social science research in Borneo. Despite a growing roster of active or interested social scientists concerned with Borneo, the total published product remains disappointingly small.
2. Large areas of Borneo remain seriously--I would say dangerously, and deliberately so--underresearched. The areas most neglected are Sabah and Kalimantan. The danger lies in the fact that the sensitive and delicately balanced ecological equation between man and nature is being suddenly and traumatically dislocated as a result of the large-scale denudation of the forest resources that has occurred, largely within the last decade. Representation is a term almost never heard, and one wonders if it is one that those in the administrations would rather not have or hear mentioned. Appell has achieved a bull's-eye analysis of the real meaning of research and the spuriousness of what is called development but what he calls destruction.
3. Dr. Appell is a far too genuine and modest scientist to do justice to his own case. On the one hand he argues logically and convincingly for more and better quality basic ethnographic research--we must above all else have the facts--yet his deep respect for current anthropological scholarship has led him to be waylaid by the ongoing kaleidoscopic changes in fashion in social-anthropological theory. From structural functionalism he advances to cognitive structuralism, symbolic structuralism, transactional anthropology, i.e., from one fashionable mirage, one voguish and tempting cul-de-sac to another. As a result he is indirectly encouraging the production of more and more theses which are one-third fact and two-thirds transient and pretty useless (because rapidly obsolescent) theory. But the one-third or one-quarter factual material is *ipso facto* highly biased in its selection to meet the requirements of the superimposed theoretical constraints.
4. This is not an argument against theory. On the contrary I believe that a nomothetic framework is a basic desideratum for all sciences. But theoretical formulation ought to grow out of social reality, and not in reverse order, i.e., a scientist's construct of social reality should underpin his/her assembly of relevant ethnographic and sociographic facts.
5. As I understand it, the main burden of Dr. Appell's paper is a serious plea for a resumption of the intensive and high quality social-anthropological research initiated by Dr. Edmund Leach's 1947 Report and followed by the field research and publications of Geddes for the Bidayuh, Freeman for the Iban, and Morris for the Melanau. Nothing comparable has yet been done for Sabah or Indonesian Kalimantan, although in the work of Appell, the Whittiers, and King there is undoubtedly much promise. For Sarawak and Brunei, the tradition of the 1940s and 1950s has been continued in the work of Zainal Kling and Brown,

though for the Kayan, Kenyah, Kelabit, Kajangan, and other peoples we still have no reliable or authoritative studies. In Indonesian Borneo, urgent studies are required for the Ngaju, Ot Danum, Kayan, Kenyah, and other congeries, and for Sabah on the Bajau, Dusun, Murut, Kadazan, Kinabatangan, and Lundayaa peoples.

6. The Borneo Research Council, despite some ups and downs, has without doubt proven its value. But are its functions to remain static--in the face of Dr. Appell's telling reappraisal? I hope not. The time is ripe for the BRC to act as an initiator or catalyst for the sponsorship of new field research in those areas that he and I have indicated as most urgent. Cannot the Council make a serious effort to raise funds for the implementation of a program of graduate field research determined by and rigorously pursued by the Executive Committee, or by a specially selected Research Committee of the Council?
7. Finally I should like to thank the organizers of this meeting for giving me an opportunity for discussing Dr. Appell's paper and thereby of expressing some of my own views too.

* * * * *

A Note on the University of Hull's Involvement
in Borneo Studies

1. Dr. James C. Jackson--Geography of Sarawak and Historical Geography of West Kalimantan.
2. Mrs D. John--British North Borneo Company
3. Dr. David Bassett--Borneo History Seventeenth-Nineteenth Centuries.
4. Dr. Zainal Kling--The Malays of Saribas
5. Mrs V. T. King--The Maloh and Related Peoples of the Central-Upper Kapuas Basins
6. Dr. John Flenley--Kinabalu and High Altitude Lakes
7. Professor M.A. Jaspán--Malays, Iban, Murut.

GEOGRAPHICAL RESEARCH NEEDS IN BORNEO

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In at least two respects geographical research in Borneo has been disappointing. First, not enough has been done, and, secondly research accomplished thus far has not necessarily been of the most useful sort. In the light of the amount of useful research waiting to be accomplished, geographers have scarcely begun their task. But geographers are not alone in this respect. More importantly, the work done by geographers in the past has tended to be more descriptive than analytical. Thus, it has been of greater use as background information and textbook material than as a sound basis for policy. Geographers may not be alone in this respect either, but they, perhaps more than others, might be justly accused of hiding their lights under bushels.

Geography is a curious discipline. In a typical university department one may find people analyzing pollen samples from archaeological sites, modeling future migration patterns with the aid of a computer, and delving into the realm of psychology in an attempt to understand man's perception of his environment. All live quite happily and comfortably within a geography department. Even with so broad a range of interests and skills, they can and do talk with one another. The gravity which holds these seemingly divergent interests in orbit is a common concern with spatial distributions and space relations.

Geography shares with other branches of science the problem of understanding man and his natural environment. Thus, the raw stuff of geographical research may be indistinguishable from the data collected by other scientists. Indeed geographers often rely heavily upon the information collected by people trained in other disciplines. But geographers approach the data with a different dimension, the spatial dimension, in mind. Their conclusions are couched in terms of density, pattern, dispersion, and spatial interaction (mobility). Most questions of concern to science are answered more completely with the addition of spatial analysis (the geographical point of view), and a few can only be answered in this context. Consequently, the science of geography has much to contribute to our understanding of man and the natural environment--and the relations between them--in Borneo. Why, then, do we find relatively few geographers devoting their research time to Borneo? I believe we can identify some reasons.

Borneo must compete with more accessible areas for the attention of geographers. The research skills of the geographer are, in the main, applicable to any place on earth. The geographer who may have a potential interest in Borneo is likely to be attracted to other areas which are easier to travel to, more hospitable to foreign researchers, and which have been the focus of greater world attention and are, therefore, more likely to draw research funds.

Also, in the eyes of geographers, Borneo may appear to be a relatively difficult place to do research. In some respects this impression is correct. Applying for a research visa to Kalimantan or Sarawak is cumbersome

and slow; applications to Sabah are usually a waste of time. Nowhere in Borneo is there a research-oriented tertiary educational institution to which the foreign researcher could attach himself. Transportation is usually either slow and unreliable or relatively fast but very costly, and geographers in the field often need to travel a great deal. Approaches to government departments for maps, aerial photographs, and census data (the raw materials for much geographical research) are often treated with suspicion. All of these factors tend to discourage potential geographical research.

Another factor is the generally mistaken belief that the data required for many forms of geographical research is simply not available in Borneo. True, when compared with North America, Western Europe, or Australia and New Zealand, manufacturing and trade statistics, basic land use studies, agricultural production figures, and some forms of census data are not easily available. Also time-series data (comparable collections of data assembled periodically) are generally absent. But I have been consistently surprised at the amount of good information which is available so long as one has the credentials to gain access to it and the energy to dig it out. The difficulty of gathering primary data is balanced by the satisfaction of breaking new ground.

The language problem may also be discouraging to geographers contemplating research in Borneo. The incentives for geography graduate students to learn a foreign language are rapidly disappearing, and even those departments which still require foreign language competence to qualify for a graduate degree are usually unwilling to accept a language such as Indonesian or Malay in which there is no significant body of geographical literature. But geographers who do not speak Indonesian or Malay should not automatically disqualify themselves from working in Borneo. English is spoken widely enough among the educated to accomplish many types of studies, and it is possible to learn enough Bazaar Malay in a very short time to satisfy one's basic needs of travel, food, and accommodation.

Both the science of geography and the island of Borneo have developed sufficiently that the time is ripe for a major new thrust of geographical research in the area. Studies similar to those published in the past should continue to be produced. Studies of settlement, land use, demography, and general regional works (material along the lines of works on Sarawak published by James C. Jackson and Y. L. Lee) remain useful to geographers, scholars in other fields, and to the general public. Kalimantan and Sabah are scarcely touched by works of this sort.

But the new thrust of geographical research should adopt a policy-oriented stance and should concentrate on issues for which modern geographical analysis is particularly suited. Studies of spatial organization, circulation, central place systems, regionalization, environmental perception, and diffusion, which are process-directed and which utilize modern techniques of cartographic analysis, statistical analysis, and data collection (such as remote sensing), are needed in Borneo. Research along such lines would add a vital new dimension to our understanding of the area, suggest possible changes which might be induced to further economic and social development, and predict the potential harmful consequences of change that they might be minimized. There are several specific areas towards which new geographical research should be directed.

Transportation studies in Borneo have long been neglected. Comparative studies of the costs and benefits of various modes of land, water, and air transport may indicate that alternative policies are required for optimum development of transportation systems. Roads are very expensive to

build and maintain in humid tropical environments, and the greatest benefits from roads appear to accrue (at least initially) to those segments of society already in an economically advantageous position. Perhaps it would be preferable in both an economic and social sense to channel a greater proportion of transportation development funds into improving inland and coastal water transportation systems. But thorough studies in numerous areas are required before such an alternative policy can be considered rationally.

Regional planning is another untouched area in which geographers can play a useful role. Development now tends to proceed at a project-by-project level. Broader studies which inventory and analyze the development and employment of resources, both natural and human, over a larger area, are required to ensure that these resources are being utilized in the best possible ways. The functional interrelationships between the various regions of Borneo and the national centers (Jakarta and Kuala Lumpur), other regional centers (Ujung Pandang, Surabaya, Singapore), and overseas metropolitan centers require thorough analysis so that linkages might be modified for greater social benefits and economic efficiency.

As a number of important processing and manufacturing industries, especially those ancillary to mineral and timber extraction, appear almost certain to develop, studies in industrial location are urgently needed. Several such industries are already established or are in advanced stages of planning, yet studies of their location have rarely gone beyond the local level of simple site selection. Careful studies of the relationship of industrial sites to markets and labor supply, as well as to raw materials, would allow the formulation of policies to ensure that these industries are established where they will contribute most to economic and social development.

City and town planning is another neglected field in Borneo. Studies which commence with the basic question of the role of urban places and service centers in a developing, primary producing area would provide the background for sound government planning. There is no need for the cities and towns of Borneo to repeat the errors of other developing areas so long as analyses of how and why the cities are growing and ways of directing that growth are conducted. To my knowledge, no urban geographer has yet directed his attention to the urban places of Borneo.

Related to both city growth and industrial development is the problem of environmental pollution and here, too, geographers can contribute significantly. Developing countries have thus far, and perhaps with some justification, assigned a lesser priority to air and water pollution control than Europe or North America. Yet there is a need to know whether the fishing industry downstream from a pulp mill will be wiped out or whether a residential area will become less attractive due to air pollution. Studies of atmospheric circulation and stream flow characteristics, coupled with investigations into present requirements and desires for unpolluted air and water, are within the purview of geography.

These are only a few of the ways in which geographers can contribute to the orderly and successful course of the almost inevitable development process. Geographers already have the ability to conduct such studies; their skills have been sharpened and proved in numerous other parts of the developing world. But to get geographers in the field and start on this work, three conditions must be met.

First, governments, particularly provincial and state governments, must be willing to extend their cooperation. The removal or relaxation of

many official barriers to local travel and data collection would do much to encourage geographical research. That numerous geographers have already accomplished studies in Borneo amply attests that these barriers are not insuperable, but it must be recognized that geographical research has some special requirements. Geographers normally need to travel extensively in the area of their research, and they often need to investigate a variety of data sources. They may not have a time to cultivate the personal relationships too often necessary to gain access to places and information; therefore, they may require official credentials if they are to work efficiently.

Second, funds must be made available for research projects. It must be recognized that much of this research may be comparatively expensive, particularly if extensive local travel is required. Funding agencies will receive much more for their investment if their field worker can afford to charter the occasional Land Rover, speedboat, or light aircraft. Further, many geographical studies will require additional expenditure outside of Borneo; laboratory and computer analysis may be required. Return air tickets and a living allowance are not adequate if geographical research in Borneo is to take a new direction.

Finally, geographers themselves must recognize that there are interesting and urgently required research possibilities in Borneo. More basic, primary data is available than most people realize. Conditions for research can be improved in any field situation and perhaps there is more room for improvement in Borneo than in some other places. But the difficulties, which are even now little more than annoyances, are surmountable and the needs for, and the satisfactions to be derived from, geographical research in Borneo justify the efforts.

A GEOGRAPHER'S BIBLIOGRAPHY OF BORNEO

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The complete bibliography of the geography of Borneo would include virtually everything which has ever been written about the island. The nature of geography as an academic discipline (being defined by its point of view, not by its subject matter or methods) means that the political geographer consults a body of literature very different from the geomorphologist's. Indeed, I cannot imagine a single work on Borneo which would not be of interest to some geographer, some place, working on a particular aspect of Borneo geography.

Accordingly, this bibliography is selective in the extreme. It includes English language works written by geographers (professional, recognized people with academic training in the discipline) and works appearing in publications which geographers normally consult. In addition, a few works which do not meet these criteria are included because I deem them important to some aspect of Borneo geography. This bibliography intends to illustrate the range of geographical works on Borneo, and, hopefully, to uncover some works which might otherwise escape the attention of scholars with allied interests.

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AGRICULTURE AND THE STATUS OF SOCIAL RESEARCH IN BORNEO

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A number of us who participated in the Borneo Research Council Symposium and the Association of Asian Studies meetings were spawned by the multidisciplinary area programs and research centers which were created after the Second World War. This multi- or interdisciplinary tradition makes us quite accustomed to the fact that a historian may be as conversant with the economics or anthropology of an area as he is with its history. This style of research has not been without its critics and problems, but most have come to accept it as something normal in the social sciences. Many of us who were educated under this system believe that it has clear advantages over traditional notions of single discipline research. However, it may be time for us to pose some basic questions about the direction and composition of our multidisciplinary research that would influence our approach to research in Borneo and in other areas.

What is envisioned here is the possibility of some form of multidisciplinary research involving the agricultural and social sciences. The social scientist in Borneo works within an agricultural society which includes distinct units at various stages of agricultural modernization. Many social scientists have found that an understanding of agriculture in Borneo is absolutely essential to their work. I am of the opinion that the social sciences in Borneo need the knowledge of the agricultural sciences, and I know that the agricultural sciences need that of the social sciences. My personal experience has been that one gains a very different perspective of an area while working with farmers as an agriculturalist than one ever does as an urban-based political scientist armed with survey instruments. On the other hand, if an anthropologist is going to work with people whose livelihood is farming, he will have a substantial advantage if he has some knowledge of tropical crops and soils.

However, social science researchers interested in Borneo face a serious problem today. For any number of reasons, local authorities may consider social science research to be anything from irrelevant to disruptive, and researchers are not given permission to undertake such work in some instances. I suspect that certain kinds of research will remain in this unacceptable category for a number of years. However, there are any number of social science efforts which are highly relevant to the course of agricultural change and development that are of great interest to government officials in Borneo. This is increasingly apparent on the part of the Federal Government of Malaysia and underlies the recent stipulations that research results and the research process be useful to local institutions.

Yet agricultural research has generally tended to ignore the relevance of social science research for improving the indigenous agricultural system. Much of the work done by agricultural departments thus tends to be focused on traditional problems. For example, one reply to my enquiry on the status of research stated: "In view of limited funds and shortage of qualified research scientists our efforts have been concentrated mainly on problem-oriented research on oil palm, cocoa, rice, maize, sorghum, tapioca and

other short term crops. The programs embraced largely on the production of improved material through breeding (for cocoa and oil palm), introductions, and selections; fertilizer requirements and other agronomic requirements; and crop protection.t'

Thus, resources are just not available now for agricultural departments to initiate major crop breeding programs, much less social science research on agricultural problems. However, the work of taking new local varieties and selecting those strains best adapted to local conditions and exhibiting the most desirable characteristics is absolutely essential as the first step to any agricultural improvement.

Other research, such as the effect that deficiencies of trace elements in the soil (e.g., manganese) have on pepper yields, will partially determine future cultivation of pepper in Sarawak. Yet the concern that I have today is how such agricultural research actually relates to the welfare of the farmer. Various extension methods have been borrowed from Western countries to transmit technical information to agricultural producers. This line of communication has also served to bring some pressing technical problems to the attention of agricultural researchers. But, as in the West, we find that farmers closer to the researchers and farmers who are practicing an agriculture more familiar to the researchers' own experience receive greater attention and interest. This is not intended as a criticism; it is only natural that one feels most comfortable attempting to tackle the problems that one is most familiar with because of training or experience. It is in the areas where agricultural scientists feel insecure that research in the social sciences may be able to contribute to the alleviation of the problems of traditional agriculturalists.

Researchers concerned with tropical agriculture have only begun to ask why certain crops have received most of their attention. Some of these, of course, have been commercially valuable export crops, and colonial and even current national policy have dictated a substantial research investment. Rubber and the work of the Rubber Research Institute in Malaysia is a good example. However, we find that many other crops are major sources of food in the tropics, but little work has been done with many of these. Some of the most interesting work has been done by Professor Bradfield at the International Rice Research Institute. His focus has been on multiple cropping of secondary crops along with rice in a variety of rotations and cultivation systems. This is somewhat similar to the pattern of subsistence agriculture as we know it in Borneo. What this suggests is that the social scientist, having considerable experience and expertise in the study of traditional agricultural systems, may be able to make a considerable contribution towards the improvement of agricultural practices in Borneo by further research on precisely these traditional systems. Natural scientists also are now investigating traditional practices in order to find effective natural plant protection methods to relieve dependency on pesticides; as they have in the past, anthropologists may play an important role in discovering these techniques.

On the other hand, I find it disturbing that social scientists have shown little interest in problems dealing with the nutritional habits, choice of crops, and other cultivation practices that have relevance for the introduction of new agricultural techniques such as have grown both from their own research and that of the agricultural scientist. I happen to be associated with an institution that has provided the genetic breakthroughs in high lysine corn and sorghum. The sorghum breakthrough occurred only a few months ago. Some of the critical parent material came from Africa, and as a result our scientists are somewhat familiar with this area and its agriculture. This contact with African farmers has led to a

feeling of frustration among the plant breeders and geneticists responsible for this breakthrough. They observe the relatively low adoption of high lysine corn and are concerned about the future adoption of high lysine sorghum. There have to be reasonable explanations for these problems of variety adoption that relate more to human factors than to technical factors. In the past, these kinds of problems have been best analyzed by social scientists familiar with agriculture or by agriculturalists with a background in the social sciences.

It is equally disturbing to read projections by scientists who consider themselves experts in tropical "agroecosystems." One recent paper pictures Western agricultural expertise as hell bent upon converting the tropics into a cash crop garden for the developed countries.¹ Historians and political scientists should be able to tell us something about the complexities of national aspirations with respect to export and food policies and the mutual relationship that exists between these and the development of a commercial agricultural economy. Furthermore, most competent agriculturalists recognize that the primitive slash and burn cycle may have substantial advantages and be the most productive agricultural system in certain areas with poor resources. The Sarawak studies by anthropologists who recognized the agricultural reasons for this style of subsistence agriculture are still classics.

Contrary to the protestations of some natural scientists, there are agriculturalists who recognize the special characteristics of the tropical ecosystem, particularly with respect to disease and insect problems. This is now one of agriculture's major technical concerns. Yet, I would venture that long-term solutions to the pest and insect problems will also depend upon related work in the social sciences. Natural scientists sometimes do not recognize human society as part of the tropical ecosystems.

The common notion appears to be that agricultural scientists create high-yielding varieties with sufficient theoretical productivity to stimulate commerce but with no resistance to pest and diseases. This results in spreading around vast quantities of noxious chemicals to solve the disease problem, lest farmers lose their crops. The fact is that the same techniques that result in the high-yielding characteristics can be used to induce disease resistance such as that ascribed to traditional varieties. One also must question the notion that all traditional varieties are disease resistant. Some do have substantial resistance to certain diseases. However, this resistance may be no more permanent than the resistances that can be bred into high-yielding varieties, and in both cases our extensions of world communication allow pest and diseases to move into areas where natural selection processes have not had the opportunity to occur, and then even traditional varieties are devastated by unfamiliar enemies.

The variations in the ecosystem of pests and diseases which confound new or traditional varieties make necessary the extensive field tests carried out by the various departments of agriculture in Borneo. Plant stock suitable in Kedah may exhibit disease susceptibility in Semongok. Changes in cultivation practices to reduce pest or disease losses may require substantial changes in customs and habits whether one is dealing with new or traditional varieties, and this requires the skill and training of the social scientists. For example, it was thought to be relatively easy to get farmers to apply granular pesticides when they had already become

¹Daniel H. Janzen, "Tropical Agroecosystems," *Science* 182: 1212-19 (21 December), 1973. [It has been reported that the Weyerhaeuser Company plans to clear-cut its forest concessions in Kalimantan and plant California Pine.--G.N.A., ed.]

accustomed to applying fertilizer. We do know that the timing and regular application of pesticide is absolutely critical if it is to be a sufficient infestation preventive. But we were amazed to find that the amount of pest damage for a large number of farmers was directly correlated with the amount of pesticide that they applied. In other words, the more pesticide applied the more damage there was from pests.

Farmers knew that they had to apply their fertilizer to get the appropriate increase in yields, and they did so. Most apparently hoped that pests would not attack and held their pesticide back to apply only when the pests actually appeared. However, when signs of pest damage are visible, it is already too late to use pesticides effectively. As a result, the more damage that appeared, the more pesticide farmers applied ineffectually. If we have difficulty promulgating these relatively simple plant protection practices, how can we ever hope to utilize in the environment of traditional farming the more complex biological plant protection practices being considered today?

Some farmers in West Malaysia have recently bootlegged a new rice variety from a government experiment station, and it is now widespread over a major irrigation area under the nickname *Apolo limabelas*. It is a superior high-yielding variety, and luckily so far has proven disease resistant. Other varieties might have been taken which have proved highly susceptible. No responsible agricultural institution can afford to release a variety until it has some confidence in its disease resistance and other important characteristics under local conditions. Yet many agriculturalists and government officials forget that a monoculture based on high-yielding varieties represents a certain risk. Subsistence producers have long avoided this risk and usually choose to practice multiple cropping with traditional varieties. Much of the geography of Borneo is not suitable to consider wholesale adoption of the new rice varieties with their more stringent water and nutrient requirements. This explains the derision that greeted the well-meaning political statement that the coast of Sarawak would soon become a rice bowl for Malaysia. The options for agricultural improvement appear to be limited to systems closer to traditional subsistence multicrop practices. Yet, agriculturalists generally are less familiar with these systems and know very little about the human component of the tropical agroecosystem.

Neither agricultural scientists nor agricultural economics has been terribly successful in measuring quantitatively or qualitatively the labor input into traditional agriculture. Modern agricultural systems have replaced intensive labor systems with machinery and high energy inputs in the form of fuels, fertilizers, and agricultural chemicals. The best labor input data in traditional agricultural systems has often come from anthropologists. It is not possible for the agricultural scientist today accurately to assess the adaptability of new crops or cropping patterns by subsistence farmers because he knows so little about the organization of current systems and their exact labor requirements.

It would be immensely valuable if we could now make a more thorough examination of existing multicrop systems in Borneo. A group of social scientists, in conjunction with some agricultural scientists, might start with some of the following questions:

1. Can we clearly define the important agricultural and social characteristics of the major multiple cropping systems now practiced in Kalimantan, Sabah, and Sarawak? Can we pinpoint them geographically?

2. Are the present systems adequate for the self-perceived or externally defined nutritional, economic, and social needs of the communities involved?
3. How fragile are these systems in terms of their susceptibility to population pressure, economic pressure, crop failure, etc.? What would be likely to happen if the existing system failed?
4. What value judgments can social scientists make about the introduction of cash crops or other activities in areas that are now dependent on subsistence agriculture? The requirements here are for something more than the market-bound production function that often becomes the driving criterion for economists and governments.

Individual social scientists and agriculturalists have attempted at various times to answer some of these questions for individual communities. At no one time have we tried to consider these questions for adjoining communities. Unfortunately, because pieces of this work have been done in the past, it may not now be considered fertile ground for original research. Yet, we know that the populations and many of the cropping systems have changed since these original studies were made, and some of the base-line data is excellent.

These suggestions are not as specific as they should be, and I am sure that they can be added to and easily improved upon by others with more experience. The general notions discussed here relate to George Appell's comments on development which are subtitled under social stress, the loss of indigenous knowledge and cultural inventions, and the loss of biological mechanisms. I believe that a better case can be made for social science research with the governments involved if it includes investigations of problems related to agriculture.

THE STATUS OF RESEARCH ON DEVELOPMENT AND
DEVELOPMENT POLICY IN BORNEO

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Development

Development and development policy, as phenomena, are of central importance in the dynamics of change for most countries. The machinations of national and regional governments as they grapple with growing problems and attempt to modernize their societies can be fascinating and at the same time misunderstood, even when, as is rare, a wholistic view is sought. It would thus be natural that there should be a great deal of research and monitoring of development--as defined by governments in terms of goals and forms--and development policy--as set out in planned expenditures and administration, and manifested in projects. Yet in fact there is very little research on this subject generally, except by economists, and specifically as relating to subareas of countries. In particular there is an absence of research on the impact of development activity by governments on the physical and human systems of the developing societies, and at the microregional level.¹

The absence of such research is seen in Malaysia and Indonesia, where economic sectoral studies and large amounts of political behavior analysis are carried out centrally, and village and urban studies are carried out rather widely; yet the role of government--the largest single force for change in the modernization drama--in the evolution of the societies, economies, and ecosystems of the two countries is vastly ignored. When studies exist (see below), they usually are quite specific to an area and project, ignoring the largest developmental and governmental-institutional context. Having little commonality of methodology, there is, therefore, little comparability among such studies.

The planning documents of the central and regional governments themselves frequently couch descriptions of the situation, the impacts of policy, and the goals in the language of sectoral growth targets, even for regions. This obfuscates, one sometimes suspects deliberately, the question of real policy-derived change in the society and on the landscape. Partial exceptions to this generalization in Malaysia and Indonesia are the urban development policy enunciated in the *Second Malaysia Plan*, whose purpose is to raise Malays' incomes and opportunities in the modern economy by assisting them to be successful as city dwellers; and the *desa* development and Inpres programs in Indonesia, in which special funds are granted to villages and *kabupatèn* for improvements of the physical environment. These are directed and labored upon locally, with obvious locally approved and monitored impacts. In time, there should be a rather full literature on the successes of these special approaches.

¹Cf. G. N. Appell (1975).

The Case of Borneo

For students of the island of Borneo, there are three important and related questions with respect to development policy. First, what is the impact of all this policy on the landscape and societies of Borneo? How is it adding up and what difference does it make? Second, what is the relationship of the Borneo states or provinces to their countries? And third, what is the meaning of national development policies for the island of Borneo as a whole system?

The latter two questions may be seen as two sides of the same omelet, or cold fried egg: on the one hand, each country has severe national integration problems, compounded by being parts of an extensive archipelago. A central feature of national development policies, therefore, is the enhancement of national unity and exploitation, on behalf of the national economies, of all regions, each to its own capacity. Thus the Bornean states or provinces have particular development policy acts directed at them designed to bring them closer in mood and format of activity to their national centers and to earn foreign exchange and grow natural products for use in the national economies. On the other hand, the two halves of Borneo have great similarities in physical and human resources, even their organization. While the national policies of Indonesia and Malaysia may be seen to be pulling the two parts of the island further apart, for purposes of national integration and exploitation, the nature of possible and desirable development in the Bornean regions may still be producing actual convergence--of similarity of activity and organization. Borneo thus is schizophrenic, if not bipolar, because of the pull of the central governments and cultures of the two countries, as always.

At a deeper level, however, Borneo as a whole could begin to manifest a wide systematic modern similarity and even increase connections and circulation among the parts as the technologies of resources exploitation and the commonalities of problem solving, including that of communist insurgency, increase over time. The interesting thing is that the roles, opportunities, and problems of Borneo for both countries are so similar. The challenges for comparative and macrosystematic research are the more exciting because within this similarity, the national styles, level of development, and the relative importance of the Borneo regions to their central government are so clearly, if transiently, different.

Development and Development Policy in Borneo

The Bornean regions, especially of Indonesia, are most clearly seen by their national governments as potential producers of valuable exports to the center and/or abroad, while embodying potential centrifugal forces in the nation-states. Parts of each, moreover, are less richly endowed than others, and these seem to require development initiatives, if only to prevent or lessen separatist tendencies, while they do not serve the national enriching functions of the others. The apparent problems of handing the cultural minorities and communist harassment in some areas add to the national concern in both countries. The results have been a degree of autonomy and financial strength of the Bornean states or provinces generally absent in their counterparts in or near the national ecumenes (Western Malaya, Java). But this is combined with increasing penetration of the local governments and civil service, of late, by the representatives of the centers having a specific and foreign cultural identity, i.e., the Javanese and Malays. The Malaysian States (Sarawak and Sabah) have had statutory

independence to raise special revenues and control certain domestic functions. The Kalimantan Provinces have received extraordinary revenues (ADO) from the Indonesian Government as return from the export earning they produce.

Given these constraints, national development, and therefore development policy in both parts of Borneo, has five interrelated strands: (1) natural resources exploitation (forestry, minerals including oil, and fishing; (2) provision of infrastructure, especially to support the preceding, in the form of roads, ports, electricity, and air and telecommunications services; (3) agricultural development, both extensive for export on estates and more intensive for resettlement from more populous areas in the countries; (4) improvement of the settlements systems, especially physically in the form of basic services; and (5) development of the local governments' capacities to govern, and to promote and finance local development while adhering to national priorities.

These strands are in order of seeming importance in terms both of government and government-approved private investments. (We can consider Pertamina as part of the Indonesian government.) Improvements to settlements systems are directed primarily at capitals and cities like Balikpapan and Sandakan, through which export products are transshipped and from which the essential services required by the logging, petroleum, estate agriculture, and fishing industries are provided. All activities and their impacts could stand in-depth analysis, with research directed both to cases and overall patterns and the relationship of these to national needs and systems. As there will be no turning back of the drive to extract and sell resources from Borneo, it would be pointless to dwell on the destructive aspects of this kind of development policy without making the connection through project effectiveness research to enhancement of efficiency. That applies especially to the policies on resource exploitation and the provision of infrastructure.

At the same time, as a new equilibrium between human and natural systems in Borneo is being evolved, research into the most effective forms for increased agricultural settlement and production and the necessary services systems organized through settlements at various scales will be extremely valuable. The opportunity is to do the inevitable development better, with some thought, rather than plough ahead heedless of the complexity of the extant and imported systems as they clash.

The object of development policy which should be the most crucial to the blending of development objectives and the most receptive to the results of research of the kind suggested above is, of course, the government system itself. This suggests that, development policy being the artifact of government, research on decision-making and policy implementation at all administrative levels is essential. Research on governmental behavior, especially expenditures and local impacts, however, is not easy. It is perhaps only done by or through the governments themselves and clearly identified with important governmental needs of the moment. The fact that Indonesian and Malaysian provinces or states have their own development offices, operations rooms, budgets, and so forth can sometimes make the task feasible. Under the Repelita II's regional policies and with the larger Indonesian government revenues accruing with suddenly increased oil prices, the provinces are expected to receive vastly greater development grants from the central government from 1974-75 onwards. Thus, the problem of what the Kalimantan governments do will now be taking on added importance. With that should come increased concern about the development processes with which they are involved and, therefore, added research opportunities.

Extant Development Research and Sources on Borneo

Having suggested the obvious needs and research headings, it is sad but not surprising to note the paucity of development research in both parts of Borneo. I am personally less familiar with recent research in Malaysia than that in Indonesia but have noted in the past a general disinterest in East Malaysia in the universities and the central government. Indeed, works on the economy and geography of Malaysia frequently deal only with West Malaysia, apparently because of the discontinuity of data sources between the two parts of the country and the far higher development of research tradition and information on the Malay Peninsula.² Recent work on East Malaysia has, of course, included Lee Yong-Leng's continuing excellent population and settlement studies, some economic-industrial studies, and urban studies. There must be some theses on local and state governments, trade, and infrastructure development, but I can't put my finger on them where I am. Recent studies of the politics and political institutions of East Malaysia have generally ignored development policy aspects and institutions in favor of party and communal analysis. For example, I have seen no study touching on the excellence and impact of the Kuching Port Commission or the Kuching Water Board, or the newer Rajang Ports Authority, as activators and operators of development in their regions.

For Indonesia, I have been kindly permitted by Ms. Syamsiah Achmad, Chief of LIPI's Bureau of Foreign Relations, to peruse their lists of foreigner research approvals since 1968. These reveal a great indifference to Borneo and development policy there. Research on Java, Bali, Sumatra, and even Sulawesi follows fields of urban migration and development, rural development, national integration, regional development, stabilization, demography, foreign assistance, taxation, family planning, development planning, bureaucratic evolution, and the development of various industries -- a set of problems equally important to Indonesian researchers in the central and provincial universities and research centers. The Economic faculties of Universitas Indonesia and Gajah Mada and the Planning Department of ITB are especially important for their ongoing studies by faculty and students. Yet, as noted above, there is practically no recorded research that makes the connection between and follows the dynamics of government development policy and its impact on areas.

Certainly this is so for Kalimantan. The only evident foreign research operating there exclusively of late has been in the fields of anthropology, linguistics, social organization and orangutan studies. Some comparative studies including other islands have involved urbanization processes (and national development in my present research), and bargaining strategy in the timber industry.

Then there are other sets of enquiries and data which, while not necessarily or usually classifiable as scholarly research, can nevertheless be valuable to the scholar as source material, particularly on development issues. These are of three classes, all of which have bearing on Kalimantan. First, *training exercises* are produced in various institutions of the governments by officials taking special courses. The APDN *skripsi* noted by H. Whittier (1973) are a good example. In addition there are the cases prepared for instruction in the PPN courses at U.I. under Leon Mears, and these presumably are used at the Bogor Agricultural Faculty, by LAN, and in Pertamina.

²I would blushinglly point to my own work (Osborn 1974) as an exceptiona

Second, there are, naturally, a large number of *planning documents* floating around and through the government at all levels, including basic compilations of policy data; some can be made available to the scholar. One might particularly mention the regional plans and data collections, including a large number for cities, produced by the Direktorat Tata Kota dan Daerah of the Public Works Department. There are great lacunae, but these are useful for baseline information. Recently, too, regional studies have been supported by various foreign aid organizations, with vast, sometimes encyclopedic, sometimes skewed reports resulting, including ones of West Sumatra and southern Sumatra, with more contemplated, *inter alia* for South Sulawesi, North Sumatra, and the southeastern islands. Last, the Center for Regional and Urban Studies at ITB has done a master plan for Pontianak.

Growing out of the latter are more specific *development project support documents* of all kinds. These are often generated by the aid-giving institutions, but are organized directly by BAPPENAS and local governments as well. They range from East and Central Java power studies with load forecasts and the like, but which also contain basic economic data, to physical plans for upgrading airports in Balikpapan and similar cities. Emphasis in development assistance has been heavy on electricity, roads, irrigation, communications, and water supply in the recent past; so these are the subjects, for various areas including Kalimantan, on which a large amount of collecting and in some cases analyzing of specialized data has occurred, usually with more general background data prefacing these. Changes in assistance policy in the last couple of years will generate further data and analyses on the subjects of population, health, regional and area development and the like. Unquestionably, the private companies that operate development projects in Indonesia, as in mineral exploitation, and consumer products' marketing also have their own analyses and data to which, with the right use of friendship and guile, one can get access.

For Malaysia, all of the above applies. The local governments of Malaysian Borneo have far more elaborate, deep, and reliable information available on their affairs, going back much farther in time. Consulting engineering firms, like Valentine, Laurie and Davies, have worked on road and traffic problems in Sabah; while the World Bank has been much concerned with ports in Sarawak. The town and country planning departments of the state and city governments, together with their engineering and survey offices, generally have a wealth of material at hand on physical dispositions of human phenomena.

The research problems in both countries, however, remain those of verifying data and opinions at hand, gaining sufficient coverage, and obtaining time series. Access to officials can be a serious problem but should not be if handled correctly. The resulting development research would be of immense value to all concerned.

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THE COSTS OF DEVELOPMENT: COMMENTS ON OSBORN

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Any act of development inevitably involves an act of destruction, as I have pointed out (Appell 1975). It may destroy important species of flora and fauna and critical species of indigenous cultivars. It will certainly destroy the articulation of a society to its ecosystem with the consequent loss of rare knowledge, pharmacopoeia, and so forth. But it may also destroy the integration of a social order, promoting loss of self-esteem and social identity, increasing role conflict and uncontrolled aspirations. These in turn work themselves out in increased antisocial acts, increased incidences of physiological and psychological health impairment, higher rates of divorce, accidents, etc.

It should not be construed that my position is antidevelopment, however. On the contrary, my position is that we do not include the costs of such destruction in our development planning, so that we do not know the real cost-benefit of such plans. What is worse, we seldom take steps to minimize such destructive effects. That is, we do not screen the social and ecological structure under development for valuable items to preserve for future use. In destroying, as we develop, we in fact lessen man's ability to adapt to future challenges to his society and to his human ecosystem.

Modernization is likewise a delightfully deceptive term. It sounds good; it means well; but it inevitably includes dehumanization, since we do not look for its pernicious effects and counter them. We take the good with the bad--which is not necessary. Fletcher, the theologian, makes the point that such change produces anger, anomie, and alienation. These social costs may indeed outweigh the benefits thought to come from modernization, for modernization means increasing the costs to the government for social control and increased costs for medical services to deal with the health impairment that arises from such culture change.

The point I want to make here is simple. When are we going to stop treating "development" and "modernization" as ideological concepts, banners around which we rally? When are we going to analyze systematically both their costs and benefits? When are we going to realize that the goal of life is not solely material, but that the character of the social system in which one lives is just as important? When are we going to design into our development and modernization projects those particular social factors that will promote a better, more pleasant and creative social environment?

As Osborn rightly points out, much more research on the processes of development needs to be undertaken, but with a greater emphasis on the character of the social order that a society wants to promote. With this problem solved, I believe the economic aspect will fall into place and not need to be forced.

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NOTE ON THE STATE OF RESEARCH IN THE
ORAL LITERATURE OF BORNEO

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Sarawak Museum

The Work of Translation: Past and Present

Progress Report

In August 1971, I began work with the Sarawak Museum on a project to translate songs of indigenous peoples of Sarawak. In November 1971, the Ford Foundation provided a grant making it possible for this research to continue until March 1974. The results are to be published in two volumes by the Sarawak Museum: *Poems of Indigenous Peoples of Sarawak: Some of the Songs and Chants*. This work includes seven groups: Iban, Bidayuh, Melanau, Kenyah, Kayan, Kelabit, and Penan.

To gather this material, I worked with interpreters from the Museum at field locations or at the Museum. Variations of English, Malay, indigenous dialect, and song-language phraseology were used, in word-for-word translation. This was gradually expanded to phrase, sentence, and stanza. Very careful attention was given to locating and following the progress of the images that make up the poems, that is, their natural structuring. Being poems, they were essentially the same as poems elsewhere; and since my primary field is the creation and study of poetry, finding these picture-markers and directional cues for changes were not as difficult as might be imagined, considering the ostensible strangeness of the material and the differences in cultures.

The main problem I encountered was assuring accuracy in translation. Interpreters sometimes considered what to me were real differences in meaning as mere shades of differences, or too obvious, too unimportant, or simply inexplicable in terms of another culture. I had to learn a great deal very quickly about what is usually left unsaid in each of the seven groups. Especially bemusing to some I worked with was my great desire to learn the picture of how a thing was being done or said--for therein lay the building-blocks, in fact, often the very keystone, of the entire poem. Part of the difficulty was that some indigenous interpreters, although having had some exposure to wider education, often could not believe that I wished to "see" the poem as it was naturally seen and understood, without explanation, by indigenous listeners; that it was as crucial to me to visualize and understand it as it was for the singers to sing all of its correctly; or that such was possible (It is, more or less.)

Without question, it is often difficult for an outsider to query and for an insider to answer, for both to get together and communicate the picture and the meaning a poem is meant to convey in its original atmosphere and terminology, and especially for the meaning to be relayed, in essence, without destroying the picture through which the information is meant to come. This is not just a problem of a far-outsider approaching a closed society. It holds true, although for perhaps fewer instances and to a

lesser degree, for persons living no farther away than another village. The songs are often in special song-language known only to the initiated practitioners, and they must be interpreted even to those living in the same society, especially the young and those who have lived outside the society for any substantial time.

The immediate goal has been to record, transcribe, and translate songs in danger of being entirely lost. All of the tape recordings are retained and filed in the tape library of the Sarawak Museum. The singers who know the songs and the meanings of archaic song-language are generally old, and the previous tradition of training new singers in the essential songs has usually been abandoned, since the songs are usually no longer essential to the society. The results have been that some of the songs have been saved from total neglect, but many have disappeared because too few researchers are available, trained, or interested. For example, the entire set of ancient Melanau prayers to accompany the dead on their journey is a closed book to all living Melanau. The recordings done by Dr. H. Stephen Morris in the early 1960s meet no spark of recognition, the oldest of the singers having already themselves gone on the journey.

Current Status of Research

Much fine early work was done, largely incidentally, by such noteworthy anthropologists and students of Sarawak as H. Stephen Morris, Tom Harrisson, W.t.R. Geddes, E. Leach, and Derek Freeman, and much is still likely tucked away in the files of others and can hopefully be discovered. Much notable translation of the Kenyah oral traditions has been and is still being done by the Rt. Reverend Bishop Galvin of Miri, Sarawak. Benedict Sandin, former Curator of the Sarawak Museum, has done a great deal of translation of Iban oral literature in many of its forms. Presently the staff of the Sarawak Museum, with Curator Lucan Chin and a newly appointed Government Ethnologist, the Australian-educated Iban, Peter Kedit, are engaging in a program of research-study of the lesser-known groups--those whose languages and literatures are much in jeopardy owing to the smallness of the group and the speed of cultural transition.

Patricia Whittier is now developing a grammar of the Kenyah song-language, in contrast to ordinary speech, and Herbert Whittier is studying ritual traditions, efforts which will produce some useful oral literature. However, no new Ph.D. candidates are to be permitted to do research in Sarawak unless the proposed study is not being done locally or unless further study is requested.

Sabah has long since shut out researchers, and since I have little first-hand knowledge, the status of research on the oral literature of indigenous groups there is beyond the scope of this brief note. However, a Canadian musicologist, Ben Neufeld, a former CUSO Volunteer, and his wife recorded much indigenous music and song, and I understand he transcribed and translated a manuscript as well. The tape and the manuscript have been presented to the Chief Ministers Department, being held for possible use by Radio and Television Malaysia and the Department of Youth, Culture, and Sports. Copies of the manuscript have also apparently been sent to the National Archives in Kuala Lumpur, with ethnographic data to George Appell (Brandeis University, U.S.A.) and John Prentice (Australian National University). Mr. Neufeld, in the *CUSO Malaysian Newsletter*, notes that he would like to return to Sabah to help direct a program of popularizing these songs.

The Brunei Museum continues to publish articles, stories, and songs. A concerted effort also seems soon to be launched by Malaysian and Indone-

sian writers to locate and utilize local oral literature as a matter of Malaysian pride. A new journal is now put out by the Malaysian Historical Society, presently in whatever language a contributor happens to know but likely soon to be in Bahasa Malaysia, and the Society is much interested in collecting disappearing folk traditions, especially among the Malay. A meeting of writers is scheduled to be held in Kuching in Sarawak, and the government is interested in helping writers concern themselves with these matters.

The Relevance of Research on Oral Literature for Other Disciplines

The results of work in this discipline may indeed have relevance for research in other disciplines. The oral literature is a clear verbal emanation and living picture of the society. It is almost a film strip of both the everyday and the imaginary, the real and the ideal; thus the oral literature still at least partially provides the play between both aspects of human nature and as a result helps make the society function. The emotions and dreams are especially to be found in both public and private song.

Relevance to other disciplines can thus be found at every turn. The practical life of the times can be reconstructed (also materials and methods used or replaced, and the reasons for the changes). As a gauge of the changes within a society, oral literature is an excellent source, giving fresh views of the world taken for granted by so-called developed nations. For studies of variations in how cultures express, delay, or disguise psychological states; of the formality or informality of expression; or of how class and kinship are expressed in unsaid understood ways--that is, unsaid except by the manner of expression, depending on public or private nature of the circumstance, oral literature is likely to be interesting to anthropologists and psychologists. The songs carry the full range of the life cycle, daily responsibilities and possibilities, and the ideal which is kept as the model. Studies of cross-cultural influences can be made, tracing a particular expression or implement in a song, prayer, or ceremony.

For example, the image of the honey tree is clear in a Bidayuh prayer. The climbing of it, the motifs of upriver and downriver, the grandmother figure who is asked for blessing while one is nearing the danger of the bees (and who is, by double image, the bee itself)--these elements are similar to those in a brief and half-forgotten Melanau honey-tree prayer, and the relationships among the images were quite obscure until the link to the Bidayuh was seen in it. Then, certain linguistic problems were made clear; and the relationship between the songs and the possibility of a former greater cultural closeness, as supposed by other equally nebulous evidences, were noted. To specialists in tracing culture interaction, such revelations (or further confusions) are reason enough to cause scholars to want to give greater attention to the songs. My own purpose is to help bring forth the loveliness and clarity of the poem; but to others, and to a degree to myself as well, there is much value in seeing other possible implications of such work. These "secrets" cannot be forced--they come out naturally as one becomes closer and more involved with the songs, and many other possibilities for interdisciplinary study become obvious.

Problems to be Solved and the Direction of Future Research

The problems still to be solved remain the original problems: to locate the best sources of the oral literature, especially the songs, in my view,

since their language is often veiled and they are more crucial to ritual and the deeper sources of inspiration within the society than are the stories, which are often more casually passed on; to manage to travel to isolated areas with proper funding and assistance; to record clearly and if possible to film as well; to transcribe accurately (either to a preset standard of the dialect, if it exists, or to a known phonetic system); to translate each word and phrase, even if one knows or thinks one knows the dialect, in order to avoid a gloss of the meaning and to locate the picture-blocks that build the poem; to develop these sections into a viable poetry translation that tells (trans-lates) the poetry sense intended, as it is meant to be heard in the original, while maintaining a high standard of fidelity to the prosaic meaning. This I recommend to be in standard, flexible, expressive English (or whatever language), while at the same time as much as possible avoiding contemporary colloquial expressions, which may have little meaning to indigenous readers of the English and which also become obsolete and further obscure the meaning.

The direction that I think research needs to take in the future, and the special problems to be solved, are built into the changing societies which still possess these ancient and sometimes still-living traditions. The necessity is for teamwork at every phrase. The teamwork aspect is a major problem to be solved, for it involves rethinking the role of the researcher, the intermediary, and the original source or singer, the poem being presented, and the audience for it, as well as the purpose for translation.

Necessary Components for Productive Teamwork in the Collection and Translation of Oral Literature

There are four main components for developing productive teamwork. One is a researcher who has sufficient education and insight to recognize values not readily recognizable to those within the society and who wishes to comprehend the past and present via its communicative, expressive formst. Such a person may be someone from outside the society being studied (for example, a Westerner in an Eastern land) who learns the modes and is sensitive to them, or someone who is from the society but has been exposed to culture-paradox and can see, for example, Western-Eastern elements and relate to both. Whatever the source of education, it should be one which provides a broader and deeper viewpoint than is to be found in a closed society. First, learning indigenous modes requires an extremely sensitive appreciation to a way of life which requires that at least half of what is meant is what is unsaid. Second, learning the outside view requires an extremely sensitive appreciation of the necessity to explain what is meant by what is left unsaid.

The second component is a person who is skilled chiefly as an intermediary and who has had some exposure to both societies. He may not himself know all the old songs and their meanings, but with his perhaps partial knowledge and closeness to the society, he would know how to inquire of those who did. He may not himself know or fully appreciate the implications and possibilities of culture study, and he might be unaware of all that is implicit in the study of oral literature; but he recognizes his own worth and his own limitations in both areas, is truthful about what he knows and does not know (a problem in itself), and is pleased to move from one to another approach and to be part of the team that is working to preserve and vitalize the oral tradition. This is often a selfless role, for one often is not given due credit and lacks full prestige in either quarter. If the contribution is substantial, this person should be listed as second author, otherwise at the very least be given status as first informant.

The third component is the original singer^a priest^a priestess^a or spirit medium^a who knows the songs and the meanings (although often the practitioners learn by rote and lack real comprehension of what they sing or why, especially if the ritual associated with it is no longer performed), or who can perhaps ask another for the meaning of the songs that are forgotten. A pair of singers is often better than one, since each can refresh the other's memory and may feel less timid than if alone. These contributors are of course listed as informants, but if the songs have been composed by them (i.e.^a those songs in secular style)^a they might well be listed as third author.

The fourth component is the poem itself. This is an entity not to be toyed with and used for anyone's pet theory, but a thing as real as any building being envisaged from plans or foundation stones. If the work of oral literature is in songs, and sometimes in stories as well, particular attention should be given to the poem as a thing in itself as an artifact or work of art. The images and variations of images are themselves valuable beyond their their apparent use in a ceremony or as entertainment^a and no substitute can be had for the reconstruction, via information and imagination^a of the original^a in complete "natural" form.

Simply persuading researchers and members of local communities that oral literature has intrinsic value is a difficulty which is less readily recognizable than the more obvious necessity for realignment of researcher roles; but increased respect is necessary for the work as literature. Whether oral or written, it is increasingly less meaningful in terms of value judgment, especially in the present state of changing art patterns, the event-and-happening art^a the verbal and nonverbal modes combined^a wherein recordkeeping modes are open to conjecture.

Development of Research Personnel

The development of research personnel and training local people are interrelated problems. An international meeting, Seminar Tradisi Lisan (Seminar on the Oral Tradition) was held under the sponsorship of UNESCO at the University of Malaya^a Kuala Lumpur^a from March 29 to 31, 1973. Professor Mohammad Taib Osman, of the University of Malaya, was the Chairman. Then on November 12 to 16, 1973, a regional meeting on oral tradition was held in Kuching, Sarawak, which was attended, as in the former meeting, by persons from Malaysia, Indonesia^a Thailand^a the Philippines^a and Singapore^a.

In my view, the internationalization of scholarship is necessary, inevitable, and useful—but for some time to come a temporary reverse nationalism is likely to keep out outsiders and to rely on the ability of those within the community. There is always the danger of local and political issues taking precedence over or forcing certain scholarly concerns, but that is likely not very different from the experience elsewhere and in other disciplines. The issues differ^a the pressures are similar. Disinterested scholarship may or may not suffer; and the degree to which previous scholarship has been disinterested is of course variable.

¹For those interested in conducting research in South and Central Kalimantan, the person to contact is Dre Paul Lambut, of the University of Banjarmasin, who is himself a Ngaju.

Conclusion

The scholarship aspect, however, is not likely to be a major problem. Guidelines are fairly clear regarding academic techniques, although art aspects of the poetry content are generally neglected. In this possibly fresh start, both for indigenous and outside views of oral literatures, in the approaches to investigating and advancing the study of oral literature, the fact of the poetry itself is worth considering. For some this would be a vast understatement, but for many it is a rather odd consideration. I should like to suggest that much work needs to be done to convince researchers of the reality and extreme value--for themselves in their various disciplines, as well as for the poem and the artist's view--of the intangible aspects inherent in the work. The oral literature is, first of all, art. I suggest that all researchers in this field study poetry, so that they know it when they see it. They can thereafter make other uses of it, as they wish, but they will not necessarily destroy it in the process. An archaeological site of the first magnitude can be destroyed by faulty scaffolding or other carelessness, and a work of oral-literature art can be destroyed by faulty translation, since no one is likely to be passing that way again, and even if they do, no one will be there to know what the question is; much less the answer.

Persons deeply ingrained in the academic tradition, along with those new to it and trying to imitate its style; tend to be suspicious of either simple or expressive statement. In particular, researchers intending to work with oral literature (and even those who do not, since many fine pieces have been located and translated incidental to the main work of the researcher) might find it useful to study not just poetry in the academic sense of the dissection of it for critical purposes but the writing of it. The speaking of poetry made spontaneously, the rhythm of speech in their own writing, the structuring of images to a natural closure--these are necessary studies. Otherwise, researchers will be embarrassed or feel inadequate when dealing with poetry in all its seemingly strange forms, vaulting concepts, low comedy, exaggerated metaphor, simple loving detail. They will tend to use poetical archaisms in artificial English (or whatever language) when dealing with what appears to be poetry-language in the original. They will try at all costs to retain an academic view--not that the academic view is not necessary, especially for honest comparative study. But it is inappropriate in the writing and recreation of poetry; and an academic view can too often, in this context; be an attempt to hide the poetry from view, because one is simply unaccustomed to dealing with it.

Much simpler and better is to relearn the capacity to think in pictures that change, which seems to me to be the essence of oral literature, and to express the poem more completely. Footnotes, or a note prefacing the poem, can explain whatever is obscure and cannot appropriately fit into the poem. I do not like footnotes, since they tend to interrupt the flow, and instead prefer to explain everything necessary in a preface to the poem; but there is no need either to leave out explanation or to make of the whole an artifice that never drew breath. One loses a great deal of information --and for this alone researchers might well take an interest. Thus, both scholarship and artistry can be enhanced, and the transition from oral to written tradition need not mean the elimination of its very life: the poem itself.

