

THE SUPERIORITY OF INDIGENOUS SCHOLARS?

Some facts and fallacies with special reference to Malay anthropologists and sociologists in fieldwork

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INTRODUCTION

In this very brief essay, I will discuss an issue which I consider to be one of the major problems facing Malay anthropologists and sociologists¹ who strive to understand their own society. The problem concerns their perceptions of their own relationships with the Malay peasantry in particular, and with Malay society as a whole, because ultimately this perception will affect the way in which they conduct their research on their own society and hence may limit the depth of their findings.

It is not uncommon for some of my colleagues, themselves Malay anthropologists and sociologists, to assume, almost with an air of subtle or even blatant arrogance, that being Malays, they have *a priori* knowledge of all aspects of Malay society and culture. With this assumption dominating their approach to research, many of them adopt the attitude that they do not need a period of familiarisation with the community they study, because they assume that acceptance into the community or the establishment of rapport will not be problematic.

As a result, they are content with a short period of field research, during which they depend heavily on survey-questionnaire methods instead of opting for the more intensive and time-consuming participant observation. Alternatively, they prefer to depend heavily on research assistants (mainly their own students) who are instructed to study the various aspects of the community being examined.² These teachers, or 'scholars', then play the role of "coordinator" and editor of the data", and finally produce a thick thesis or monograph of their own.

I am aware of many extraneous factors which force Malay scholars to adopt such strategies. But, I also wish to stress that the opportunity for them to conduct their own intensive field research is always there for the taking.³ However reasons, or excuses, such as job security (which basically means their own economic survival) or bureaucratic problems (which demonstrate more their lack of initiative or ignorance of regulations than the eternal problem of red-tape) have been advanced as obstacles in their endeavours to carry out intensive and comprehensive research.⁴

In this paper, I wish to argue that the common practice of Malay social scientists of taking refuge under the notion that they have "superior knowledge" or that they are "experts and authorities" on Malay society and hence it is unnecessary for them to conduct prolonged field work when studying a Malay community, has become a convenient strategy which obscures more deep-seated reasons. I would argue that this strategy has become a sort of "ideology", which is academically unhealthy and which pervades the thinking of the Malay social science community and the students it produces.

However, in this essay I do not wish to present a comprehensive survey of the causes of this problem among Malay scholars. Rather, I will focus on its effects, by examining the work which they have produced in published form. Most of my comments on this issue are based upon my own reflections after the experience of conducting a long period of anthropological field work. However, I will begin the discussion with a brief overview of the origins of the "superiority" notion.

SOME ROOT CAUSES OF THE PROBLEM

Prominent sociologists, like Galtung (1966), have argued that it is not uncommon for indigenous scholars, especially in Third World nations, to adopt a very "defensive" strategy, such as, claiming they have "superior" knowledge about their own society as a result of specific historical consequences which occurred in the colonial era. For example, Galtung argues that there has been a continual suppression by colonial regimes of indigenous intellectuals' views of their own society. As a result, the intellectuals readily accepted the colonial or colonial-sponsored scholars' definitions of the colonized society. To a great extent, this happened in Malaysia with regard to ideological matters such as education, Malay language and culture, etc.⁵ But this situation was not accepted passively by the Malay scholars and intellectuals of the time. Many of them, in fact, became very antagonistic towards the so-called "experts" or "scholars" of the colonial government who claimed to know best about the Malays.⁶

This antagonism became stronger and was expressed more openly after Merdeka with the advent of the "new scholarly hegemony" (Kessler, 1978 : 17) which originated in the countries like the United States during the 1950's and 1960's, during which the "free world" nations, such as Malaysia, were flooded with hordes of foreign scholars, mostly sponsored either by the advanced nations or their multinationals. The funding agencies were basically motivated by strong political and economic interests (Chomsky, 1969). This phenomenon formed the "second wave" of ideological domination over Third World nations by the imperialist powers after the first wave of colonialism.⁷

Some foreign scholars came as "expert advisers" to the local oppressive regimes to "advise" them on how to avoid "political decay, disintegration, disorder and instability" from becoming obstacles in their efforts to "modernise and develop" their societies.⁸ There were also some who were more blatant; academic mercenaries garbed as consultants who tried to do "patching-up and repair jobs" especially in the implementation of the oppressive regimes' so-called development plans (Mortimer, 1973). These "scholars" produced voluminous reports, thick theses and a plethora of publications for their sponsors. Ironically, the opportunity to do research and to publish helped them to secure jobs in the universities of their home countries or gain "prestigious appointments" within government or quasi-government institutions in the advanced nations (Melman, 1970). But what is crucial here is the fact that the interpretations they offered about the underdeveloped societies they studied were often simplistic and misleading. Although this was partly a result of the theoretical and methodological paradigms they adopted in their research, which have been heavily criticised (e.g. by McVey, 1981), this did not mollify indigenous scholars who disliked their work intensely.

In Malaysia, many local scholars have criticised these studies openly for their superficial, simplistic and sometimes misleading nature.⁹ And as a result, local scholars' antagonism towards their foreign counterparts has heightened, even though the latter may include a few genuine and committed scholars who sympathize with the plight of the former.

Hence an outcome of this situation has been the adoption of the attitude that "local scholars know best about their own society." Among Malay scholars (i.e. anthropologists and sociologists), for example, this attitude has been reinforced by their own set of immediate problems (as mentioned above). It is regrettable because this attitude has a negative impact on the way they relate to their respondents in the field and the way in which they conduct the fieldwork in general. The overall effect is to "blur" their view of their own society. Of course, their own methodological and theoretical persuasions also play a crucial part in this "blurring" process.

For example, there is a tendency for Malay scholars to forget that Malay society is not homogeneous. It is not only divided by class but also by specific subethnic-cum-ideological factors, such as language or dialect, specific cultural forms and practices. The scholars often forget that, even within Malay society, they belong to a particular social class. They also ignore the fact that, as individuals, they have become urbanised to a high degree although their original background may be rural. This urban factor has the potential to influence their perceptions of their very own community. Here, I am referring especially to those scholars who go back to their own *kampungs*, intermittently, to do research.¹⁰

I do not deny that a Malay scholar does have a head start compared to a newly-arrived foreign scholar when both of them study a rural Malay community, for example. But what finally determines the depth of the findings of both scholars is not the "ethnic" factor but rather factors such as the length of time spent in the field, the choice of research method and the theoretical orientation which guides the whole research.¹¹ I will attempt to explain this further, based upon my own research experience, both before and during the recent fieldwork I conducted.

REFLECTIONS FROM PERSONAL EXPERIENCE

Recently, I raised the issue of S. Husin Ali's methodological shortcomings in conducting his research on politics in rural Malay society (Shamsul Amri, 1982a : 3). S. Husin Ali is the pioneer figure in Malaysian social anthropology and has been its most prominent representative for more than a decade.¹² However, even as a Malay studying his own society he was unable to capture the crucial intimate details and minutiae of the political processes within the communities he studied. This, I argue, is mainly a result of his not-so-intensive fieldwork.¹³ As a result, in his monograph (S. Husin Ali, 1975) he is able to provide us with many insights into the crucial aspects of Malay village politics but no explanations. This point is also mentioned by Kessler (1976) and Turton (1979) when they reviewed the monograph.

S. Husin Ali admitted that he did not spend sufficiently long time in the various communities he studied to enable him "... to observe the full cycle of activities of the villagers within a calendar year ..." (1975 : vii). This was probably due to underlying bureaucratic and, probably, financial factors. His admission and the practical conditions under which his research was conducted are sufficient to vindicate him from the methodological shortcomings of his research, and hence from most of the generalisations found in his monograph.

But I would argue that this "vindication" has been negated by his next statement: "... being ethnically of the same stock as the majority of the people in the communities studied, acceptance was not a problem and a period of familiarisation was therefore not necessary ..." (1975 : vii). In short, he adopted the problematic explanation of "superiority" which he thought could compensate for his earlier methodological shortcomings. Although initially this seems to be a declaration of his advantaged position vis-a-vis the Malays, upon close scrutiny we find that it had become a "disadvantage" for him, in the sense that, this strategy has somewhat constrained his ability to probe deeper into the communities he studied. Hence, some of his statements about Malay rural politics are unclear. Elsewhere, I have discussed the theoretical origins of his methodological shortcomings (Shamsul Amri, 1980a : 87-90).

However, I do not disagree with S. Husin Ali's views on other crucial issues, e.g. that of foreign scholars: firstly, his strong criticism regarding the slipshod way in which some of the so-called prominent scholars, especially from the United States, have conducted their research in Malaysia; secondly, his remarkable efforts to demystify the stereotypical view once held by uncritical foreign scholars of the idyllic Malay village, as a place of plenty, equality and harmony (S. Husin Ali, 1972; 1974); thirdly, his opposition to foreign advisors playing the *dalang* role behind the scenes in Malaysia's development planning, which I have discussed elsewhere also (Shamsul Amri, 1980b). In fact I consider S. Husin's alternative explanation of "development" in Malaysia (S. Husin Ali, 1976 : 1-16) as constituting a turning point in the recent proliferation of critical literature on "development" in Malaysia.¹⁴

But despite my admiration for his pioneering contribution to Malaysian social science, I choose to disagree with his "superiority notion" strategy in conducting field work in Malay communities. In fact, this disagreement had a positive impact on the way in which I approached my recent field research. Hence I adopted the attitude that, although I am a Malay and, supposedly, very knowledgeable about the Malays, I am still somewhat "alien" or a "foreigner" to the particular Malay community I studied, for various important reasons.

Firstly, I do not belong to the same social class as most of the villagers I studied. Secondly, long years of urban living make me different from members of the rural community, in many ways. Thirdly, the factor of language.¹⁵ Although I could communicate with them in standard

Malay, the lingua franca of villagers is Javanese because most of them are of Javanese-Malay origin. Therefore, an extended period of participant observation was necessary to learn the language, as many local meetings were conducted in Javanese as were all the daily conversations. Finally, I was born and brought up in a matrilineally structured community in Negeri Sembilan which differentiates me from the more male-focussed communities in the rest of Malaysia.

In short, I wish to stress that there are more differences than similarities between myself and the people I studied, although we all belong to the same "ethnic stock". Hence, it would have been unwise for me to assume, before commencing my field research, that because the villagers are Malays and I am a Malay also, therefore establishing rapport is not a problem, and hence a familiarisation period is unnecessary.

In this context, I argue that all Malay researchers would undergo, with different intensities, experiences similar to their foreign counterparts when conducting field work in Malay communities (Freilich, 1970).¹⁶ However, the amous term "marginal natives" is probably not applicable to Malay anthropologists or sociologists because Freilich was referring basically to foreign anthropologists who have to adopt, as part of their methodology, the culture of the "natives" they study during their field research. Malay researchers are natives themselves but probably the term "displaced natives" is more appropriate to describe the position of Malay scholars vis-a-vis the majority of the Malays, because they no longer belong to the rural society from where they originated, although they may still maintain social ties with immediate family members there. They are just "weekend ruralites" who go back to their respective *kampungs* during weekends or festive seasons, interacting mostly with their immediate extended family, or that of their spouse.¹⁷

Moreover, in my case, as an outsider to the community I studied, I had to spend about four months to familiarise myself with the research location and various aspects of the community. The first and the easiest task was to familiarise myself with physical layout of the village and the important landmarks within the area. A more difficult task was to learn how to converse in Javanese, i.e. *bahasa kasar*. It took me about six months of daily practice to acquire a certain level of proficiency in speaking Javanese. But what was more crucial to develop was a higher level of ability to understand conversations rather than to speak. In the field research context, the former is more valuable than the latter.

The initial problem I encountered was not merely getting to know people, i.e. "the right ones" and selecting my key informants; rather, it was finding a place to stay. This became an issue because the village I chose to study was divided along political party lines, viz. UMNO and PAS. When I was looking for a place to stay I received three offers, two from UMNO officials and one from PAS. I only realised this after being told by the headmaster of the local school, but I still did not foresee the intensity of the differences and antagonisms between the two party camps. So, to avoid making the "wrong choice" which could prejudice my long-term research aims, I decided to stay in a "neutral place", i.e. in the school compound, along with a group of bachelor school teachers. This decision was based on the advice of the headmaster who offered to help. But a few months later I learned that being in the school and associating myself with the headmaster was not neutral at all. In the village there was not only "enmity" between PAS and UMNO, but within UMNO itself there were tense internal factions, and the headmaster was categorised as a sympathiser of the "rebel group" within the village UMNO branch. In a way, being an outsider saved me from being caught in this local political cross-fire. Both faction leaders gave me their fullest cooperation in my research, such as allowing me to go through files and documents, including those classified as *sulit* (classified). However, it took me about ten months before I could make inroads into the PAS camp, which, I learned later, had categorised me initially as an UMNO-man. Ironically, this impression was given to them by an UMNO official who had secretly set up a *syarikat* (company) with the PAS officials.

However, the most surprising thing that happened was the way in which the PAS people obtained concrete information regarding my personal background from my own *kampung* in Negeri Sembilan. One of the daughter-in-laws of a PAS member came from a Negeri Sembilan village about ten miles away from my own. She goes back to her village of origin once every

three months. So she was requested to ask around among villagers from my *kampung* about my family. Thus, they found out not only my personal background but also my father's long history of active participation in UMNO. And, hence, the label stuck that I was an UMNO-man.

Class differences between myself and the villagers I studied, generated a host of different research problems initially. For example, they did not know how to address me. My status as an educated fieldworker seemed not to exist in their social terminology of deference. Some called me *tuan* (sir), some *encik* (mister), and many simply avoided addressing me directly. A few months after my research began, I realised that they had decided to call me *cikgu* (teacher), because I told one of the village elders whom I had interviewed to call me so, in view of the fact that I teach in a university. What made the whole situation more difficult for the villagers, who are basically peasants, was the fact that I did not appear to be a *cikgu* in the way I dressed and in the way I conducted myself during village meetings, like the high ranking government officials that they knew of. I did not wear a bush or safari jacket or closed shoes. I did not have a short haircut; nor did I sit in front with the officials, facing the audience, during the meetings or weddings I attended. I did not come in a van or car like the officials of the Malaysian Statistics Department who were conducting the 1980 Census at the time of my fieldwork. All my idiosyncracies were revealed to me in the latter part of my field work during the 1981 Hari Raya visits I made to various homes of the villagers who, by then, were my very close friends. In other words, I did not fit their general stereotype of the Malay bureaucrat and political class with whom they were more familiar. In addition, I was the first person to have done anthropological research in the village and hence to have stayed with them for so long a period. They were more used to researchers who only come during the day over a period of two weeks or so.

Another challenging task was the reconstruction of a detailed village history. As I recognised the importance of detailed historical and structural factors in understanding the contemporary village social formation, I spent a few months before and during the field research itself going over Selangor history during the pre-colonial, colonial and post-colonial period. Besides going through the standard texts, and numerous publications and theses on Selangor and Malaysia in general, I did some work at the National Archives. My main intention was to learn whatever I could from the British records about activities in and around the research area from the last quarter of 1800's up until the early 1950's. From this overall understanding, I was able to ask relevant questions about the village history in general and when probing into some crucial events that occurred in the *mukim*. In this way, the task of putting together fragments of the village history was made much easier, but was still not without its problems. For example, I had to travel to various *kampungs* within the district I studied and Selangor state to meet the few surviving members of the pioneer group who opened up the village around 1916, now mostly in their late 70's. On those trips, I also met former villagers of the community I studied, who had migrated during the turbulent 1936–1939 period in the village history.¹⁸ Cross-checking the material I collected through the in-depth interviews was a time-consuming task in itself, but it helped me to formulate further probing questions. In fact, for the whole period of my research, I never stopped asking questions about the village history even while collecting data about the contemporary village situation. As a result of this persistence, I stumbled on the complete local school records, dating back from the very first day the school was opened, and photographs of the school, the teachers, the pupils and their activities in the 1930's and 1940's.

Another exciting episode during my field work was when I discovered that one of my key informants was a prolific poet who had devoted his life to writing poems about the miseries of peasant life. After a long period of coaxing, I managed to see his albums of the news cuttings of his published poems which totalled over 200, and also his short-stories, novels and children's books. This was one of the high points in my research. In fact, I have analysed his life and literary works which is now published in book form.¹⁹ From his literary contributions, I now have the empirical views of a poet on the research area and the sufferings of its inhabitants. With this approach, I believe that I am incorporating a new dimension into the anthropological study of a Malay peasant community.

Personal bereavement also disrupted my field work, viz. the deaths of two of my immediate family members. The villagers knew about this when my foster family in the *kampung* organised a *tahlil* or prayer session at the local *surau* (small prayer house). It was indeed helpful to know that so many of the villagers were in deep sympathy with my personal sufferings. Their warm reaction not only boosted my already low morale but also brought me closer to those among them who had suffered similar tragedies a few months earlier.

After eight months field work, I left the village for three months for the 'break' deemed necessary within the anthropological research tradition. Knowing that I would come back again was important to the villagers. And when I did return to the village I was received with a warm welcome, which demonstrated the closeness of the ties I had developed during the first part of my stay in the village.

In fact, it was during the second part of my research that I was able to obtain more intimate details regarding the facts behind the village politics, economy, and history, etc. Deep village secrets ranging from personal scandals to who were members of the *kampung* peeping-tom group were revealed to me. After that, it was a matter of how much I could consume and collect; sifting out what I wanted for my research. This period of protracted field research was an invaluable eye-opener to me not only as a researcher but also as a once-rural-boy who has now become an urbanised member of the well-off, well-educated Malay class.²⁰ It was an eye-opener in the sense that, like many other concerned Malaysian academics, my interest lies in studying the problems of the masses; the oppression they have suffered from the various classes within the community. But we academics cannot know exactly what happens amongst the people that we are supposed to be so concerned about, until and unless we spend a long period of time with them. Otherwise, we will continue to represent, or misrepresent, the issues pertinent to the villagers' survival, in a very broad and theoretical sense only. Convincing as this theoretical presentation may be at the macro level, we must still ask the question, what difference is there between us and the pro-establishment scholars who are also concerned with macro issues? The difference is probably the "radical cloak", and nothing else.

SOME CONCLUDING REMARKS

I have argued that Malay anthropologists and sociologists do not necessarily possess the so-called "superior knowledge" about Malay communities they study. This belief is an illusion and a fallacy which hinders their ability to penetrate beyond the present level of research on Malay society. Already they can be accused of being a-theoretical, a-historical, a-structural, or of having narrow fanaticisms of various kinds, caught in their intense self-centred interests as the new local class of academic mercenaries, or simply locked in a state of unconcern about the rest of the world.

This means that this particular group of Malay scholars must now reassess their roles critically in the light of the problems mentioned above. Because, although we have doubts about the nature of their professional practice as anthropologists and sociologists, the fact remains that they are, and will continue to be, teachers of these disciplines. And as such, they are responsible for training more teachers and students in the two disciplines.

What I have discussed above forms only a part of the wide ranging set of problems confronted by Malay scholars (and their non-Malay counterparts too), not only in anthropology and sociology, but in other social science disciplines. It is always easy, to the extent it has become an intellectual habit, for Malaysian scholars (Malays and non-Malays), to criticise others, be it on the basis of their personality, theory, method, or position in the establishment. But it is no easy task for them to criticise themselves with the positive aim of redressing the problems that beset them. Or, will the situation "*kuman di seberang laut nampak, gajah di tepi mata tak nampak*" continue?

FOOTNOTES

¹ In this essay, I have chosen to focus on Malay anthropologists and sociologists for three main reasons: (i) they make up about 90% of the community of Malaysian anthropologists and sociologists, (ii) their works, published and unpublished, constitute the major proportion of the literature available to date; (iii) historically, the development and the teaching of both disciplines began as the "cultural stream" within the Malay Studies Department, Universiti Malaya. It is only since 1970 that separate departments of anthropology and sociology came to be established in the local universities. Nearly 50% of the staff of these departments, at Universiti Kebangsaan Malaysia and Universiti Malaya, for example were graduates of the Malay Studies Department, Universiti Malaya.

The emergence of anthropology and sociology in the West was not unrelated to the economic, political and ideological situation about two centuries ago, vis-a-vis imperialism and colonialism (Leach, 1982 : 16). And hence a sort of "division of labour" between the anthropologist and sociologist took place; the former studied societies outside the West, predominantly in the "colonies" of their own nation; and the latter studied Western society per se. In Malaysia's context, a former British colony, anthropology was introduced first, then sociology. These disciplines were taught together under one department, and still are even to this day. Owing to the specific historical experience of Malaysia as a colony and indeed of other colonised nations, these two disciplines, their teachers and their students have rarely been separated. This situation is reinforced by the fact that the scholars of both disciplines have little choice but to conduct research among the same community and using similar research techniques. Since anthropology with its focus on pre-industrial society, grew up within the power dimension of the "coloniser and the colonised", where were the indigenous "colonised" anthropologists to study, except in their own community? Likewise indigenous sociologists study their own community, as in the West. Hence the subject matter of the two disciplines overlaps and is difficult to distinguish.

² Most of the sample I am discussing here are lecturers or tutors in Malaysian universities, who are simultaneously postgraduate students, pursuing either a doctorate or masters degree. This involves Malays and non-Malays academic staff too.

³ However, this is not a problem for anthropologists such as Hood Salleh (1978) or Bahron Azhar Raffei (1973) who studied the *orang asli*. They spent very long periods in the field. So, as demonstrated by Hood and Bahron, it is not impossible for Malay anthropologists to conduct a long period of fieldwork, which is the single most important professional requirement of anthropology, if they have the initial intention to do so.

⁴ This is particularly true for the lecturers who go overseas to pursue their postgraduate studies sponsored by their universities. Many of the university administrators and government representative who sit on the committees which make final decisions regarding research funds and study leave directly affecting these lecturers, are either ignorant or have very superficial knowledge about the need for anthropologists to conduct research over a long period of time. More often than not, the lecturers have been forced to reduce their research time as a result of this — a very real but regrettable situation faced by most lecturers on study-leave.

⁵ cf. Stevenson (1975), Wheeler (1928), Chelliah (1960), Loh (1970).

⁶ cf. Syed Hussein Alatas (1977) and Syed Naguib Alatas (1979).

⁷ We are all well aware of direct and indirect attempts made by government and non-government agencies of the advanced nations, e.g. the CIA of the United States or ASIO of Australia, to solicit information from anthropologists about peoples in the politically and militarily strategic parts of the world, like Southeast Asia, Central Latin America, etc. Some anthropologists, or individuals parading as anthropologists, have succumbed to this pressure, others remained ambivalent about it, and a few have rejected and even challenged, on ethical grounds, the efforts made by the said agencies to use them and other social scientists as political and ideological instruments. See, Stephenson (1978), Berreman (1978), Wolfe & Jorgenson (1970, 1971).

⁸ Kessler (1978 : 17–19) gives an excellent account on how, first, the economists and political scientists and then, the anthropologists were involved in this context. Also, see Coburn (1971), as cited by Kessler.

⁹ cf. S. Husin Ali (1972); also see the debate between Parkinson (1967, 1968) and Wilder (1968).

¹⁰ It is not uncommon for Malay anthropologists to return to their *kampungs* of origin to conduct research, because of financial reasons and more importantly, because they believe they will have an “advantage” research-wise.

¹¹ cf. Rogers (1977) whose long fieldwork did not prove beneficial to him because he chose a problematic theoretical and methodological paradigm (Shamsul Amri, 1982a : 3–6).

¹² Because of this specific reason, I feel that it is necessary for us to highlight the research problems he encountered, because his work is very widely read.

¹³ See S. Husin Ali (1975 : vii, 1–3).

¹⁴ I must stress here the fact that what I have experienced in my recent fieldwork could be classified as an extreme case, because my community of origin is in fact a minority within Malay society in the cultural sense, i.e. an *Adat Perpatih* community. And the community I studied is not only outside Negeri Sembilan but also a Javanese-Malay community which has its own specific subethnic characteristics, e.g. language. Except for this, what I went through did not differ very much from other Malay researchers’ experience, especially in terms of the research problems that one would face in studying the mainstream Malay community. For example, I faced the problem of class difference between myself and the people I studied. Also, my urban background created the need for mutual psychological and social adjustments in the initial period of the research. These are problems which are commonly confronted by Malay scholars in studying their own society.

¹⁵ This problem does not apply in my case only, nor to Malay anthropologists who have studied other language or dialect groups within the Malay community but outside their own. It also applies to non-Malay Malaysian scholars who study Malay communities. For example, a non-Malay scholar, like a foreign scholar, must cope not only with standard Malay but also with the Javanese language, for example, if he studies a Javanese-Malay community. It is in this context that he or she, as a university lecturer, could take advantage of seeking help from the Malay students as research assistants. Or, if the students are enrolled in the honours degree year, the lecturer could direct them to do research and write a dissertation on a topic directly relevant to his or her PhD proposal or programme. In this context, I argue that, the local non-Malay lecturers who are either anthropologists or sociologists probably face more problems than even their foreign counterparts when studying Malay communities; and confront almost similar problems as their Malay counterparts when studying their own society.

¹⁶ Elsewhere I have raised this issue (Shamsul Amri, 1980c : 30–40).

¹⁷ This includes Malay public servants, too, who occupy crucial positions in the various government bodies that make decisions on “rural development”. From my own fieldwork, I have learnt that in high-level decision-making government committee meetings, these officers, often allude to the fact that they are of *kampung* origin, and hence know best the problems that beset the *kampung* folks. But, in actual fact, being “weekend ruralites” they usually interact closely with their closest relatives only, whenever they *balik kampung*. These relatives are probably the elite of the *kampung*. In this context, their claim that they know and represent the *kampung* folks’ interests could be untrue. In other words, the possibility of them misrepresenting the interests of the majority of the villagers from their own place of origin and elsewhere is very high indeed. I would argue that this complacency creates further problems in the process of development planning, implementation and re-evaluation of development projects at all levels within the Malaysian state.

¹⁸ See, Shamsul Amri (1982a : 12–15, 1982b : 7–8).

¹⁹ See, Shamsul Amri (1982c).

²⁰ It can also be an emotionally draining experience, where it involves long separations for married couples. In this context, I owe the most to my wife, Wendy, for her patience during our long separation while I was in the field, and for the endless inspiration and moral support she provided. Of course, to the villagers into whose lives I intruded, I remain indebted forever.

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