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# Managing Forests and Improving the Livelihoods of Forest-Dependent People

Reflections on CIFOR's Social Science Research  
in Relation to its Mandate for Generalisable Strategic Research

*Andrew P. Vayda*

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CIFOR was established under the CGIAR system in response to global concerns about the social, environmental and economic consequences of loss and degradation of forests. It operates through a series of highly decentralised partnerships with key institutions and/or individuals throughout the developing and industrialised worlds. The nature and duration of these partnerships are determined by the specific research problems being addressed. This research agenda is under constant review and is subject to change as the partners recognise new opportunities and problems.

# Managing Forests and Improving the Livelihoods of Forest-Dependent People

## Reflections on CIFOR's social science research in relation to its mandate for generalisable strategic research

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### Summary

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### Introduction

The following report is intended neither as a comprehensive review of all that CIFOR has been doing in social science nor as a comprehensive set of recommendations for what it should be doing in relation to its mandate for generalisable, strategic research on managing tropical forests and improving the livelihoods of forest-dependent people. Written in Bontang in East Kalimantan and without access to much (possibly pertinent) literature, what follows is simply an attempt to draw some lessons relevant to CIFOR's mandate from both my recent excursions into the philosophy of explanation (Vayda 1995a, 1995b, 1996) and my experience or contacts with several CIFOR projects during the time that I spent as a Distinguished Visiting Scholar at CIFOR in the first part of 1996. My criticisms of the projects or of the ways in which CIFOR's mandate has been interpreted are intended to be constructive. Among the questions I consider in this paper are the following:

1 How, from a pragmatic standpoint, should CIFOR's commitment to "generalisable, strategic research" be construed? More specifically, should it be defined to require the formulation and testing of

globally applicable models and research hypotheses? Alternatively, should it suffice to seek local answers to research questions as long as they are sought with experientially derived recognition of the potential broader significance or generalisability of what is locally observable and researchable? Clearly, between these logical extremes lie various possibilities.

2 What, if any, problems are there about the present division of labour between, on the one hand, those of CIFOR research activities which were established to study forest management by local communities and the case for greater devolution of decision-making to such communities and, on the other hand, those which are concerned primarily with the collection, trade, use, users and sustainability of non-timber forest products? Both deal with the interface between forests and their users-managers at the very local level. If there are problems, what are their sources? And are the problems such as to require a new division of labour?

3 Does East Kalimantan's Kayan Mentarang region, consisting of an extensive nature reserve, within which the World Wildlife Fund for Nature (WWF) is

engaged in research and management planning, and an adjacent 300,000 ha of forest made available by the Government of Indonesia to CIFOR for research, provide appropriate locations for CIFOR's research in social science? If not, why not? If undue priority has been given to Kayan Mentarang as a locale for CIFOR's social science research, what factors influenced this decision and how might future site selection be made more appropriate?<sup>1</sup>

## On Generalisability and Other Research Desiderata

A refrain in CIFOR's early publications and planning documents is that CIFOR research must be generalisable and that, perhaps in reaction to much forestry research in the past, research intended simply or mainly to answer questions about particular sites or locations – e.g., about how they should be used – is to be left to others. A characteristic statement is Jeffrey Sayer's (1994a, 1994b) in his forewords to CIFOR Occasional Papers Nos. 2 and 3 (Dewees 1994 and Lele *et al.* 1994):

*CIFOR undertakes generalisable, international, strategic research; CIFOR expressly does not undertake adaptive research to solve specific, localised forestry problems – there are National research institutions and development agencies which can do this better than CIFOR.*

Even in quite recent documents, such as *CIFOR's Strategy for Collaborative Forestry Research*, there are references to leaving “detailed, component research” or “locality-specific research” to others and exploiting CIFOR's comparative advantage for conducting research with “broadly generalisable” results and for formulating, testing, and analysing “generalisable research hypotheses”.

That CIFOR's *raison d'être* must lie in its ability to develop and conduct research in which others are not already engaged in substantial measure is unarguable. Moreover, CIFOR's limited budget would be ill directed to dealing with local problems of little or no extra-local significance. Nevertheless, it may be questioned whether the alternatives to CIFOR's dealing

with such problems have been clearly enough conceptualised and whether the statements about them that have been made provide useful enough guidelines to CIFOR researchers. Indeed, it seems that in practice the distinctions so far made have resulted, at least sometimes, in overvaluing generalisability and sacrificing other research desiderata, such as increasing our understanding of causal connections, structures, and processes – a desideratum emphasised in my own recent publications (Vayda 1995a, 1995b, 1996).

For the purpose of illustration, here, from one of those publications (Vayda 1995b: 362), is a cited example of the overvaluing of generalisability: the high commendation that some Darwinian ecological anthropologists accord to the prey choice model of optimal foraging theorists on the grounds that the model applies to some foraging behaviour of not only seed-gathering Paiute Indians and seal-hunting Inuit Eskimos but also other species, notwithstanding that “the cognitive processes or behavioural tactics underlying prey choice in bees, birds, and humans... doubtless are quite diverse” (Smith and Winterhalder 1992: 50-51). In other words, finding regularities so general as to pertain not only to diverse groups of humans but also to other species is, *ipso facto*, a satisfactory research outcome for these anthropologists, regardless of how little the regularities concern either the causal mechanisms triggering the behaviour of interest or the causal chains (possibly several or many converging ones) producing the ability or disposition to engage in that behaviour (Vayda 1995b: 362).

Along the same lines, another Darwinian ecological anthropologist has set himself the “ultimate goal” of developing a single model applicable to the foraging patterns of different groups of *Homo sapiens* across time and space (Hill 1988: 194, cited in Vayda 1995b: 364). The problem with such grand theoretical aspirations, as has been suggested, *inter alia*, by Hawthorn (1991: 160-161) in his discussion of historians' and social scientists' “retreat” from the Enlightenment's generalisation-oriented analytic programme, is that the more general that statements and models concerning social change and human behaviour become, the less likely are we to find them to be informative and to address our interests in causal explanation.

This is not intended in any way to suggest that generalisations should be eschewed or scorned. My point is rather that considerations of generality should not be

<sup>1</sup> This is not a trivial question, as CIFOR is currently considering the extent and nature of research to be undertaken in the Iwokrama Research Forest in Guyana – another remote, barely modified area of natural forests with an extremely low population density of traditional subsistence indigenous communities.

given priority over causal considerations in choosing research problems and developing research projects. A corollary of this point is that generalisations should not be regarded as the goal of research but rather as means to the goal, which, in a view that I share with many others (although with perhaps more in philosophy than in social science), is causal explanation. How generalisations may serve as such means has been described as follows in my previously cited article (Vayda 1995b: 363):

On the one hand, they [generalizations] are important in that our being able to draw on a stock of them give us head starts in developing causal explanations of whatever cases are at hand. On the other hand, it is also important that finding generalizations that can be applied in those cases enhances our confidence in our explanations: the generalizations help to assure us that we have not made *ad hoc* ascriptions of illusory causal connections and that, on the contrary, the causal connections which we have identified conform to real-world causal connections that have been discerned elsewhere (cf. Vayda 1988: 4 on this and also Cartwright 1989: 2 on regarding regularities as one kind of “evidence...that certain kinds of singular causal fact have happened”).

It must be made clear that the generalisations to which I am referring here are generalisations about causal connections, chains or structures. In other words, they are causal stories that recur in different times and places (Vayda 1995b: 363). In order to illustrate this, I hope I may be forgiven for again quoting myself (Vayda 1995b: 363):

Examples may range from such psychological ones affecting behavior as what Elster (1983: 110-111) calls “adaptive preference formation” (the “sour grapes” phenomenon) and “counteradaptive preference formation” (the “grass is always greener on the other side” phenomenon) to natural selection itself as a causal story, i.e., a sequence of changes whereby a trait spreads within a population by virtue of the greater reproductive success it confers on individuals who possess it...These are not generalizations necessarily applicable to all or even most of what are, in some sense or other, the same kinds of cases. Thus, in cases of

recognition by individuals that they have no efficacious practical means of achieving some goal, we may find a turning to magical means rather than adaptive preference formation. And, as Darwin himself recognized (see the citations in Gould and Lewontin 1979: 589-590, Kitcher 1985: 141-142, 179), other mechanisms than natural selection may be responsible for the spread of traits in some cases.

But even if not applicable in all cases, Darwin’s mechanism of natural selection is of course very widely applicable and knowing it has of course made it immeasurably easier for countless scientists to explain cases at hand. However, the fact that it applies in many cases does not give it any greater explanatory import – or make it any more true – for whatever particular cases we are considering. By accepting that generalisations known to us will apply in some cases and not in others, we can use them to expedite causal explanation without our having to discover, develop or employ generalisations of wider applicability.

What I have been saying here about using generalisations for explanation applies also to using models if they encapsulate causal stories recurring in different times and places (rather than being simply or primarily correlational models). To illustrate, I once more quote from my earlier article (Vayda 1995b: 364):

Consider, as an example, the commons model. It has not only been applied to many, by-now commonplace cases of the self-interested, ultimately destructive use of such common-property resources as fish and game, but it has also given researchers a head start in knowing what to look for in causal explanations of some not so familiar and superficially quite different cases, such as the breakdown of a traditional Javanese communal rice-harvesting system (Sturgess and Wijaya 1983). It is, however, increasingly being recognized that there also are many cases of well regulated use of common-property resources and that to these cases, contrary to some expectations when the commons model first became widely known in the late 1960s and early 1970s, the commons model does not apply (Feeny *et al.* 1990: 10-11).

Actually CIFOR projects are still too new to be able to say with certainty that there has been an overvaluing

of generality at the expense of causality in them. Nevertheless, generalisation-oriented theoretical ambitions paralleling those of Darwinian ecological anthropologists and optimum foraging theorists may be detected in some CIFOR researchers' proposals, reports and oral statements for their projects. Examples include J. Vanclay's goal of developing a single, general model of the people/tropical forest interface; M. Ruiz Pérez's assignment of high priority to (1) developing a single broad conceptual framework for research on non-timber forest products, (2) formulating research hypotheses at a "very general level", and (3) carrying out research to support general theories and models (Ruiz Pérez 1995); and, if a not primarily social science example may be included, A. Gillison's primary objective of the development of correlational models of plant, animal, and human attributes at the landscape level.<sup>2</sup>

My preferred alternative to the kind of research priorities and goals indicated in the above examples has been set forth in a CIFOR/WWF Special Publication (Vayda 1996). The argument there is that "we need to devote our efforts not so much towards developing or testing general theories, or even some broad propositions about behaviour in particular regions or societies or types of societies, as towards empirically answering questions about why things have occurred" (Vayda 1996: 1).

Rather than repeating that argument in detail, I want to emphasise here its compatibility with choosing *why*-questions for research on the basis of the potential broader significance or generalisability of possible answers to them. The selection of such questions is a crucial step in research. For example, in the course of the East Kalimantan research referred to in the CIFOR/WWF publication, when we decided to investigate why Dayak shifting cultivators who had migrated from the montane interior to the lowlands were making clearings many times larger than those of their homelands (Vayda 1996: 20), it was with the idea that the answers we would obtain might apply elsewhere as well and might bear on general questions being asked about the respective roles of "tradition" and changes in technology and economy as determinants of land use by indigenous people (cf. Vayda *et al.* 1980). Interestingly similar to this is Sayer's (1994a) apparent justification for devoting "one of CIFOR's first

*Occasional Papers*" to a review and analysis of localised research on southern Africa's miombo woodlands (Dewees 1994), i.e., he suggests that what is found there may provide guidance for the analysis of apparently similar processes in dry tropical woodlands elsewhere (e.g., India and Eastern Indonesia).

Two other points about Dewees's paper on miombo woodlands are worth noting in the present context. One is that Dewees (1994: 10) explicitly calls for the analysis of causal factors and decries that "most studies about tree and woodland use in southern Africa...consist of extensive tables of descriptive statistics which emphasise only that rural people use and manage trees" – so that we may, for example, learn that a certain percentage of households depend on woodland resources but not which households, when, and under what conditions!

The second point is that the potential broader significance of answers to some of the *why*-questions posed by Dewees may lie not so much in their generalisability as in their disproving broad generalisations that have been too readily accepted. Thus when he recommends longer term historical and archival studies of why woodland cover in southern Africa has been modified or cleared, it is partly to counter certain widely accepted, broad generalisations, namely, that population growth places "increasing and inexorable pressures on natural environments, and [that] there are few approaches which can be taken to mitigate the impacts of these pressures". As Dewees (1994: 17) notes, longer term studies indicate a variety of changes in land use and can help to identify both positive, effective measures in response to pressures from population growth and also the policy initiatives – like the technical exercises, dating from the late 1920s, to "rationalise" Zimbabwe land use – to be avoided in the future. My own recent study (Vayda and Sahur 1996) of why Bugis migrants in East Kalimantan, some of whom are (or have been) employed also in the booming industrial sector of the town of Bontang, have cleared forest in Kutai National Park and are growing cocoa and oranges on the cleared land may likewise be less significant for coming up with widely generalisable answers than for countering broad generalisations about poverty as the cause of encroachments in forest reserves and national parks. Similarly, Oates's (1995) study in Nigeria's Okomu Forest Reserve is significant for countering the

<sup>2</sup> While not denying that they place high value on generality, some CIFOR staff, after hearing my arguments, have contended that they have not confused "generality" with "universality" – they have, for example, claimed neither that "common-property management inevitably collapses everywhere" nor that "it always works for the common good". Rather they have regarded it useful, for the purpose of both explanation and prediction, to try to generalise about the conditions under which one or the other result obtains.

generalisation, widely accepted by conservation and development organisations and funding agencies, that economic development projects for forest residents are conducive to forest conservation (see Wells 1994-95 on this, as cited in the next section).<sup>3</sup>

It may be all the more important for CIFOR social scientists to engage in such generalisation-countering investigations because of the tendency in such fields as rural development to embrace, without sufficient evidence, certain causal sequences as if they were generally true and therefore usable by policy makers as blueprints for how change should proceed in particular locations without further investigation of the situations at hand there. This tendency has been perceptively discussed by Roe (1991), who uses as one of his examples the “tragedy of the commons” notion that privatising rangeland will avert its degradation (see also Pelkey 1995 on the tendency). Examples more relevant to CIFOR social science are the generalisations countered by Oates’s study and mine, i.e., that poverty causes forest encroachments and destruction and that, accordingly, improving the livelihoods of people in or near forests – even if by means of projects in agricultural development rather than, for example, in sustained-yield forestry – will be conducive to forest conservation. Another relevant example is the generalised notion that “returning” control over forest resources to the communities most dependent on them will necessarily result in more sustainable use of the resources because earlier community controls existed, were effective, and will continue. Doubts about this notion have been voiced by various observers (e.g., Dewees 1994: 3 in southern Africa and, as cited in the final section, Dove and Nugroho 1994: 23-24 in the Kayan Mentarang area that is CIFOR’s priority research location in East Kalimantan), and some evidence has been presented that community regulation for sustainable use fails sometimes to continue effectively (e.g., Rudel 1995: 504, citing Coello Hinojosa 1992 on the Ecuadorian Amazon; Anon 1993 and Conklin and Graham 1995: 703 on the Brazilian Amazon). More systematic studies are, however, in order and could be appropriate for CIFOR social scientists’ attention.

Moreover, although I am arguing that the potential for countering generalisations should suffice as a justification for certain studies, it is still noteworthy that the studies may lead to new generalisations – for example, as will be discussed more in the next section, about

what makes community regulation fail and about what makes it work – which could be applied internationally to the analysis of cases by CIFOR researchers and their partners. Such generalisations would be widely useful insofar as they would facilitate the analysis of many cases, even if, as I have argued earlier, this would not mean that the validity of the analysis thus facilitated in a particular case depends on the number of other cases to which the same generalisations apply.

## **Community Forest Management, Income from Non-Timber Forest Products, and the Division of Labour between CIFOR Research Projects**

During the approximately twelve weeks that I spent at CIFOR during the first five months of 1996, I heard repeatedly that there was concern about overlaps between two of CIFOR’s long-term, inter-disciplinary projects. I was initially puzzled about what the issue was. This is because what first caught my eye in statements about the two projects – which, incidentally, is what is still emphasised in some very recent statements about them – seemed to me not only to give good justification for each of the projects but also to make sense of their separation.

Project 7’s theme of “community-based management and devolution” seemed well justified in view of the fact that, as stated in *CIFOR’s Strategy for Collaborative Forestry Research*, policy experiments have occurred in many countries during the last twenty years to shift, in varying degrees, both forest management decision making and the allocation of forest resources and their benefits from central bureaucracies to local communities. (This devolution has been occurring in some countries at least partly because of insufficient funds for strictly state management.) Research on the devolution theme could, in my mind, address a variety of questions, including those about the various forms that the process has taken and why it has taken them and also, if cases could be found where devolution occurred long enough ago, questions about the effects it has had on the conservation of forest resources and the distribution of benefits from their exploitation.

Such questions, it seemed to me, could be well pursued mostly apart from Project 8’s important and

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<sup>3</sup> Thus research which sets or re-shapes the agenda, or which debunks myths and incorrect generalisations, is a legitimate and valuable role for CIFOR’s research, in my view.

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well justified questions about changes occurring in the collection, trading, uses and sustainability of the non-timber forest products on which, according to CIFOR's *Strategy*, millions of people depend for their livelihoods and employment. Granted that there could be overlaps insofar as devolution could effect some kinds of changes of interest to this Project. There would, however, still be enough separate and significant questions for each project.

In practice, however, there has been a focus by the Community Forestry-Devolution Project on income generation among forest villagers, and this has led to overlaps in the activities and priorities of the two projects. This focus may have resulted, *inter alia*, from the decision to conduct research in Kayan Mentarang, which, as will be discussed more in the next section, is not very suitable at present for studies of the devolution of forest management. Concentrating more on the income that Dayak villagers obtain from forest products could be regarded as collecting data to be used eventually to show interrelations between the devolution of forest management and changes either in forest product collection or in the incomes from it.

There is also a currently popular belief in conservation circles that economic development is a prerequisite for biodiversity conservation and that "people will be more inclined to conserve biodiversity if their living standards improve" (Wells 1994-95: 7). This lends some support to deciding to concentrate the Kayan Mentarang research on Dayak villagers' income generation from forest product collection and to use as one justification for the decision the possibility of using the findings for testing conditions under which such income generation "creates incentives to conserve or exploit the forest" (Wollenberg 1995: 2). To date, as in other projects on the same theme, the links are "not yet clear" (Nawir and Wollenberg 1995). As I will discuss in the next section, there are good reasons for doubting that the desired clarification can, in the foreseeable future, be found in Kayan Mentarang.

Changing the emphasis in Project 7 to "devolution to community management" would allow it to address a number of broadly significant questions. I have already suggested that these fall into two broad categories: (1) questions about the various forms that devolution has taken and about why it has taken them; and (2) questions about effects of devolution on the conservation of forest resources and the distribution of benefits from their exploitation.

Questions in the first category may be expected to relate to CIFOR's mandate insofar as the forms that devolution takes are likely to influence its effectiveness for improved forest management and better liveli-

hoods for forest-dependent people. Thus, it has been noted that hurrying up devolution – perhaps, as Little (1994: 353) has suggested, in response to pressures from donor agencies – can lead to the domination of management by local elites (see King *et al.* 1990 for a Nepalese example of this in contrast to another Nepalese case in which devolution proceeded more slowly) or to the failure to include in management planning some particular interest groups whose participation is needed for successful long-term management. An example of such an excluded group in the case of Cameroon's Oku Mountain Forest Project were the mainly female goat producers, who, instead of following the project's local management strategies, are increasingly encroaching with their herds on forest lands (Little 1994: 357, citing Brown and Wyckoff-Baird 1992). Such examples underscore Dewees's (1994: 3) recommendations about the importance of conducting research to identify mechanisms for equitably devolving natural resource management.

Further, noting the swell of NGO involvement in forest-related development projects, Neumann (1995) refers to evidence that many so-called local NGOs have no history of community interaction and have been created, sometimes by former government officials and politicians, to take advantage of current donor infatuation with funding NGO-sponsored rather than state-sponsored projects. Because NGOs "appear just as susceptible to corruption as government bureaucracies" and just as likely to find "opportunities for personal enrichment" in projects, Neumann argues that "there is a great need for research into the origins and roles of NGOs and the consequences of their activities..." CIFOR researchers, as members of a USAID-funded international network of social scientists studying devolution of forest management, should be able to identify likely locations either for such research as Dewees and Neumann are recommending or for other "devolution" research that is likewise potentially generalisable or, at least, broadly significant by the kind of criteria that I have set forth in the preceding section.

The sources, nature and likely duration of the current donor infatuation may themselves merit investigation. It could be asked, for example, whether it is the case that donors, disenchanted by the corruption, inefficiency and questions about political legitimacy that pertain to government projects, are now supporting local communities and so-called local NGOs out of a genuine conviction that these generally are democratic institutions with the will and know-how to manage forests sustainably. If so, will the donors quickly turn elsewhere if and when they are confronted with



enough cases shaking that conviction? (For an account of Brazilian Amazon events bearing on such questions, see Conklin and Graham 1995.) Or, on the contrary, do some donors and international environmental NGOs like WWF recognise differences between their agendas and those of local leaders or managers and nevertheless accord support to them? If so, what is the basis of the support? Is it, for example, simply the idea of achieving the short-term objective of fending off commercial logging, mining or plantation companies capable of large-scale forest damage or destruction? Or is there also an idea of achieving the longer-term objective of effecting local accommodation in the direction of the donors' or international NGOs' goals of forest management and conservation? Becoming alert to such possibilities, CIFOR researchers may be able to develop useful research projects in which donors as well as recipients are appropriate subjects.

The possibility of lessons to be learned from historical or archival research on devolution should also be explored. Noteworthy in this connection is that during the colonial period in many parts of Africa, as Neumann (1995) has remarked, government authorities tried different models for local control, including "native forest reserves" whereby local communities managed and received the benefits from certain village forests. In Malawi, legislation giving recognition to Village Forest Areas (VFAs) came into force in 1926 and, although eventually most VFAs fell into disuse, there were some 2,900 of them, covering over 64,000 ha, established by 1934 (Dewees 1994: 17). If the needed documentation can be obtained, factors in the rise and fall of such systems may well merit CIFOR's research attention.

When we turn to the second broad category of questions and ask about the effects of devolution, research relating to longer time spans may be more than just potentially useful. Especially with respect to positive effects on forest conservation, such research may be crucial for reaching any significant and reliable conclusions. This is so because of a biological fact which should be obvious but is sometimes ignored amidst the enthusiasm for popular solutions to forestry problems, namely, the fact that it takes considerable time for most trees to grow. This means that many forestry activities (in contrast, for example, to certain wildlife conservation activities) do indeed, as Otto and Elbow (1994: 234) have put it in a passage cited below, "take years to show their impact".

Certain negative effects or failures of community management to bring about conservation may of course be more quickly and more readily apparent, as, for example, when the aforementioned goat producers

in Cameroon violate local management strategies by encroaching on forest lands. It is also the case that some claims for positive effects of recent community management for conservation have already been made – for example, in some of the miombo woodland reports cited by Dewees (1994: 6-7). Regrettably, I have not yet had the opportunity to examine these reports, but I suspect that they concern short-run successes in restricting some obviously damaging uses of the woodlands, such as livestock pasture, timber extraction for commercial purposes and forest clearing for farming or settlement (cf. Wily 1995: 18 on "banned uses" under the village management system established in 1995 for the miombo woodlands adjacent to eight Tanzanian villages). However, in the case of some written and oral reports on research with which I am more familiar, there seems to be little basis yet for conclusions about conservation.

Consider, for example, the kind of statements made by WWF-affiliated and CIFOR researchers about the steps taken recently by some villages in Kayan Mentarang to restrict outsiders' collection of a particular valuable forest product, aloes wood or *gaharu*, from village lands. These steps, starting with demands for user fees and harvest shares and escalating to attempts to exclude all outsiders and to confiscate their collections and equipment, have been described as "local responses to over-exploitation" (Mombert *et al.* 1995) and as being "compatible with conservation". As I will discuss further in the next section, there is, in fact, no good evidence that the *Aquilaria* species that are the sources of the resinous, fragrant heartwood called *gaharu* are being "over-exploited" in the sense of being in danger of local extinction (notwithstanding that it may be increasingly difficult for collectors to find the fungus-infected trees in which *gaharu* has already formed). Nor is there evidence that the villagers' actions, however apparently compatible with conservation, are not motivated mainly by other concerns, such as wanting all the income from *gaharu* exploitation for themselves rather than for outsiders.

The desire to gain for themselves the benefits of temporary increases in the value of forest products seems to have been the motivation behind some past attempts by other Dayak villagers to restrict access to their territory (e.g., the Bulusu' villagers described in Appell 1985: 193), just as similar motivations, rather than conservation, have been behind some attempts to establish the marine reserves – for example, in Papua New Guinea (Polunin 1984: 273) – that have been prematurely hailed as Pacific Islanders' "traditional conservation measures" (see, for example, Johannes 1978 and 1981: Chap. 5). Indeed, when, perhaps

because of past experience of booms and busts in forest products, villagers recognise that the increased value of a product like *gaharu* may be temporary, their restricting access to their lands may be followed not by conservation-oriented management of the resource but their own intensified exploitation of it (cf. Browder 1992: 176 for generalisations about such behaviour as a factor in the intrinsic instability of commercially oriented extractive economies). In short, despite WWF and CIFOR researchers' professions, the kind of developments reported by them from Kayan Mentarang provides little basis for drawing any significant conclusions at the present time about the positive effects of community management on the conservation of forest resources.

Some other researchers have, however, acknowledged that longer runs of time are needed for clear and useful conclusions to be drawn about effects of devolved management. Thus, Otto and Elbow (1994: 234-235), in their report on the involvement of local resource users in natural forest management (NFM) in Niger, state the following:

Community-based management of forest lands, particularly in the politically repressed environment of Niger, will require many years of evolution before meaningful patterns emerge and data can be extracted to predict long-term sustainability. When fieldwork for this report was carried out in 1991-92, at one site the new type of local institution called the forest cooperative was only months old. Likewise, experimental conservation and coppiced cutting, forestry activities that take years to show their impact, had been underway for just a few seasons at two other sites. Crucial changes in government policy, necessary to legitimate these NFM regimes, have yet to be enacted; in fact, the very form of national government is under a protracted period of negotiation. Given these conditions, a decade may be a reasonable period after which to review Nigerian experiments in radical reform of resource-management regimes.

Asking about the positive effects of devolved management on tropical forest conservation is certainly in line with CIFOR's mandate. But if CIFOR researchers want to get beyond simple conclusions about the "compatibility" of some behaviour with conservation (even if the behaviour has no demonstrated or expectably demonstrable conservation effects), they

will need to find pertinent data covering long enough runs of time (cf. Rudel's [1995] use of data over a 40-year period to test the effectiveness of the Tennessee Valley Authority's grass-roots reforestation programmes in the southern United States). Possibly there are long-run data to be obtained concerning the effects of the aforementioned local forest management in Africa's colonial past. Certainly the history of indigenous and/or traditional community management of forests in Nepal has been documented for a period of more than 150 years (see Gilmour and Fisher 1991; Malla 1992). However, if enough pertinent, long-run data cannot be found either about effects in these places or elsewhere (in India, for example), then researchers' attention may be best directed mostly to other questions about devolved management.

## Kayan Mentarang and CIFOR's Social Science Research

In the preceding section, I referred a number of times to problems with giving priority to Kayan Mentarang as a locale for CIFOR's social science research. In this final section, I will review and discuss the reasons why I think such priority should not be given.

I must make clear, first of all, that I am very much in favour of more research in Kayan Mentarang. Indeed, as a consultant to WWF in 1992, I made recommendations for such research, and they are reproduced here as an appendix. But I regard those recommendations as having little congruence with CIFOR's mandate for generalisable, strategic research on managing tropical forests and improving the livelihoods of forest-dependent people. Accordingly, in this final section, I will consider both what makes Kayan Mentarang little suited to carrying out this mandate and what procedures might be followed for finding better suited locations.

First, let me refer again to CIFOR's actual social science research in Kayan Mentarang. This is focused on Dayak villagers' income generation from forest product collection and is concerned also with identifying conditions under which such income generation creates incentives to either conserve or exploit the forest and its products. Three reasons why this should not be priority CIFOR research may be noted:

1. Others are already engaged in the research and probably can do it better than can CIFOR investigators, whose research at any particular location is limited to periods of no more than a few weeks at a time because of their responsibilities at CIFOR headquarters and their extensive travels to develop and co-ordinate international research activities. By

contrast, consider the time that the three authors of a recent manuscript on the “*gaharu* rush” in Kayan Mentarang (Momberg *et al.* 1995) have already spent on people’s use of the forests in Kayan Mentarang: Momberg, two years; Puri, two years; Jessup, more than four years. Furthermore, some of all that time was spent, particularly by Puri, in participant observation of the villagers’ collecting expeditions. Recourse to the time-honoured but time-consuming anthropological method of such observation has enabled Puri and his colleagues to know and contextualise Kayan Mentarang forest-product collection to a degree not attainable by means of the interview methods to which CIFOR researchers are limited because of time constraints (and possibly also because of the difficulty, known to me from first-hand experience, of keeping up with indigenous hunters or foragers on expeditions in tropical forests). For example, they have already found (and reported) not only how collecting is adjusted to the demands of “labor- and time-intensive agricultural activities” but also how the collecting itself is often casually conducted – “with collectors spending as much time [for] hunting for pigs, fruit and other forest products, as for locating and extracting *gaharu*” (Momberg *et al.* 1995). And while their long and intimate experience with Kayan Mentarang people has not kept Momberg, Puri and Jessup from joining CIFOR researchers in prematurely or unduly attributing conservation import to the restrictions that some Kayan Mentarang villages have placed on outsiders’ access to village lands for the purpose of collecting *gaharu* (as noted in the preceding section), Momberg *et al.* have nevertheless made and presented observations relevant to assessing these conservation claims – for example, observations about continuing to find *gaharu*-bearing *Aquilaria* trees in previously searched areas, which could well be areas previously said to have been exhausted of *gaharu* and therefore to have been prematurely cited as evidence of the need for conservation.

2. A second reason why CIFOR’s current Kayan Mentarang studies should not be CIFOR research is that income-generating forest product collection does not seem to be critical to the livelihoods of the Kayan Mentarang villagers who are the subjects of CIFOR’s research. This limits the likelihood of being able, in accord with CIFOR’s mandate, to make findings from the research relevant to analysing and improving the situations of the millions of people elsewhere in the tropics (e.g., in

India, China and African countries) who are said to depend on forest products for meeting such basic needs as that for food. In the case of Kayan Mentarang villagers, their food needs are, as a rule, adequately met by their shifting cultivation of rice and other crops and their hunting and fishing and gathering of wild plants. Their longer *gaharu*-collecting expeditions, sometimes lasting for a month or more, are mounted, according to Momberg *et al.* (1995), “to pay off debts incurred by purchasing motor boats, engines, radios, watches and whole wardrobes from traders and part-time expedition sponsors”. The proceeds from shorter trips, usually of less than a week’s duration, are used to pay for school supplies and church expenses and traditional trade goods such as tobacco and cloth.

Momberg *et al.* (1995) mention also the use of proceeds to purchase rice in times of shortage, but I believe this mostly applies not to the primarily agricultural Kenyah Dayaks who are the subjects of CIFOR’s research but rather to Punan and Penan hunter-gatherers whose practice of agriculture remains somewhat casual or half-hearted. In any event, there is little to suggest that, for the purpose of developing insights, hypotheses or generalisations which will be useful in studies of apparently widespread and exigent uses of forest products, the dependence of this group on such products is sufficiently comparable to the dependence on them to be found among the many poor people living in or near forests in other parts of the world. While many Kayan-Mentarang men exercise the option of relatively well-paid employment in logging camps and other industries in Sabah and Sarawak, few forest dwellers in Africa, for example, have such options.

3. The third reason why CIFOR’s current Kayan Mentarang studies should not be CIFOR research is that it is not known now, and will be difficult to establish in the foreseeable future, whether outsiders’ exploitation of *gaharu*, the villagers’ main income-generating forest product, threatens *Aquilaria* populations with local extinction. It follows from this that it cannot be known whether villagers’ institutional innovations to restrict access to their lands by outsiders, even if not followed by increased *gaharu* exploitation by the villagers themselves (see the discussion in the preceding section), can be regarded as a clear and instructive example of conservation behaviour.

The few available recent studies of *Aquilaria* trees by botanists or forestry researchers point, in fact, to the

conclusion that local extinction of *Aquilaria* is unlikely to occur as a result of *gaharu* exploitation by outsiders. The studies indicate that the trees are distributed fairly evenly and at low densities over tropical forest landscapes: LaFrankie (1994: 304) found a density of 2.5 trees per ha (>1 cm dbh) in West Malaysia's lowland Pasoh Forest Reserve (with "no indication of...strong spatial patterning"), while Paoli *et al.* (1994), including only trees  $\geq 20$  cm dbh in their sample (because collectors told them that trees <20 cm dbh "rarely contain adequate *gaharu* wood to make harvesting worthwhile"), found pre-harvest densities ranging from only 0.16 trees per ha in alluvial bench and lower montane forest formations to 0.32 trees per ha in lowland sandstone and lowland granite formations of West Kalimantan's Gunung Palung National Park.

The low density and even distribution of *Aquilaria* trees make it costly, in time and energy, not only for collectors to search for *gaharu* but also for researchers to monitor its exploitation and the success of purported control measures over longer runs of time. Nevertheless, from their studies over a three-year period in Gunung Palung, Paoli *et al.* (1994) did succeed in estimating that collectors found and felled 18 out of 24 (or 75%) of the larger trees ( $\geq 20$  cm dbh) in a 125 ha sample area of random plots. The conclusion from their studies was, however, that *Aquilaria* was "unlikely to become locally extinct" in Gunung Palung. Among their bases for this "optimistic assessment" were two points made also by others (e.g., Yamada 1995: 186) about the behaviour of professional collectors (the "outsiders" from the standpoint of villagers), namely:

1. the progressive decline in residual *Aquilaria* densities and the corresponding progressive increase in the costs of finding *gaharu* in old centres of collection, like those in Kalimantan, eventually induce professional collectors to shift to new areas, like some in Irian Jaya which have more favourable *Aquilaria* densities; and
2. the shifts occur when there are probably still enough reproducing adult trees as well as a good density of juveniles to ensure *Aquilaria* regeneration in the old centres.

The second point is supported by various observations and findings noted by Paoli *et al.* (1994). Thus, since *Aquilaria* are monoecious or bisexual, the very low density of adult trees either not found or else found but not felled by collectors in Gunung Palung did not preclude continuing reproduction by these adults, which, in fact, included some relatively large trees likely to produce seeds abundantly. Paoli *et al.* noted

also a "potential for an increase in the adult population and expansion of the regenerative capacity" insofar as the density of *Aquilaria* juveniles was substantially greater than that of pre-harvest adults – almost four times as great in the case of Gunung Palung's lowland sandstone forest. I cite these recent *Aquilaria* studies not because I regard them as definitive. Indeed those who have conducted the studies do not regard them as definitive either and accordingly use such terms as "likely" and "unlikely" in their conclusions. The studies do, however, suffice to invalidate an important, implicit justification for CIFOR's current Kayan Mentarang studies: the aforementioned notion that villagers' institutional innovations can be regarded as a clear and instructive example of conservation behaviour. Momberg *et al.* (1995) may have realised that such notions are problematic, for they cite the recent *Aquilaria* studies and recommend more of such research in Kayan Mentarang.

This recommendation merits endorsement by CIFOR insofar as further research should make for better assessments of currently tentative conclusions about professional collectors' abandonment of areas before the *Aquilaria* populations in those areas lose their regenerative capability. However, under the inter-project division of labour that I am recommending in the present document, such assessments might be of greater interest to the NTFP Project, with its concern for sustainable use of non-timber forest products, than to the Community Forestry-Devolution Project. Moreover, the already mentioned expense of monitoring *gaharu* exploitation over longer runs of time makes it unlikely that CIFOR researchers, faced with severe budget and time constraints, would themselves want to undertake the recommended research rather than simply being interested in the results to be obtained by other investigators, who might include local Dayak research assistants trained by regular WWF project staff (see WWF 1996b). A consultative role for CIFOR in developing such research, monitoring its progress, and analysing results from it should, however, be considered.

In any event, such research would be more biological than social, and my concern in this section is with Kayan Mentarang as a locale for CIFOR's research not in biology but rather in social science. In line with this concern, there are some further questions to be asked. For example, notwithstanding what I have said above about why CIFOR's current Kayan Mentarang studies should not be CIFOR research, might Kayan Mentarang be especially suitable as a locale for other social science research pertinent to the themes of CIFOR projects?

Social science with such pertinence may certainly be pursued in Kayan Mentarang. Cases in point, relevant to the theme of devolution of forest management, would be the *tanah ulin* (or *ulen*) studies recommended by Dove and Nugroho (1994) as reviewers of WWF's "Culture and Conservation" project in Kayan Mentarang. After noting disagreement between project co-directors Jessup and Sellato on whether *tanah ulin* means "forest reserve" (as Jessup says) or just "my reserve" (as Sellato says), the reviewers make the following observations and recommendations:

...the revival or "re-invention" of local institutions for different, often anti-conservation reasons should be another important area of study in its own right. B. Sellato noted to the Review Team that some Kenyah aristocrats are asking for *tanah ulin* "forest reserves"...to be "returned to the people", whereas in fact they were the former owners and they want them back for their own exclusive use. Whereas the project might prefer these tribesmen to vouchsafe more selfless and conservation-minded motives, it is not surprising if the greatest interest in conservation within a traditionally hierarchical society (like that of the Kenyah) comes from those who have the most to conserve (under the old order). Nor is this necessarily in conflict with the project's conservation goals...Indeed, this [whether it is in conflict or not] is precisely the sort of question that needs to be studied. In some cases, however, it is clear that cultural revival does not serve the interests of conservation, whether directly or indirectly. This is the case, for example, in the current attempts by Dayak in government positions on the coast of East Kalimantan to re-interpret traditional institutions like *tanah ulin* to obtain timber concessions (typically destructive of the environment) for themselves. This capitulation to the state's pattern of resource exploitation is not unexpected in patterns of development (like that in Indonesia), which result in increasing involvement of local elites in the broader monetary and consumption-oriented economy...Charting the impact of these forces on traditional tribal identity and pride, studying their fluctuation, and in particular studying the associated impact on resource conservation, would appear to be an appropriate research topic for Culture and Conservation.

Among my reasons for giving this lengthy citation from Dove and Nugroho is that, by and large, the questions they are raising and the research they are recommending seem to me to be both important and clearly pertinent to the theme of devolution of forest management. Note, however, that their questions are much like the questions I have set forth in a more general context in the preceding section's discussion of broadly significant possibilities for CIFOR's "devolution" research. By raising such questions again and by following Dove and Nugroho in raising them in the context of Kayan Mentarang studies, we are led to ask whether Kayan Mentarang is especially suitable for getting the questions answered. I want to suggest here that it is not and that CIFOR should not commit itself to pursuing the questions in Kayan Mentarang without considering first whether there are other, more suitable locations.

One noteworthy limitation on Kayan Mentarang's suitability is the apparent unavailability of any substantial historical documentation of what *tanah ulin* was like in the past, how it worked, and what changes it has undergone. Such documentation should be at least comparable to – and preferably better than – what Zerner (1994) was able to find as a basis for his arguing that, in the Central Maluku islands, the so-called *sasi* restrictions on entry into resource areas or on harvests from them, far from being the indigenous conservation institutions that they have been recently called, have been continually re-interpreted and used for different purposes not only by local elites and others in Maluku communities but also first by Dutch colonial officials, then by Indonesian government officials, and, most recently, by environmental NGOs as well (cf. Neumann 1995 on the dynamic and negotiable character of so-called customary systems in Africa). While it is true that recent ethnographic inquiries have enabled Momberg *et al.* (1995) to make a few general statements about past aristocratic control of *tanah ulin* in Kayan Mentarang, the fact remains that better data are available from elsewhere concerning the changing historical uses of institutions now being revived supposedly for conservation. This means that there is greater possibility elsewhere to draw lessons from the past in trying to answer the broadly significant questions that Dove and Nugroho and others have raised about such matters as the possibility of achieving conservation goals through the revival of institutions previously used for control of resources by local elites.

Other limitations on the suitability of Kayan Mentarang are those that have already been noted as impediments to discerning conservation effects of the recent institutional changes that have occurred there.

These other limitations include not having any relatively easy means of inventorying the most exploited forest product and monitoring its exploitation and not having a long enough run of time to be able to assess either how effective the instituted practices are for conservation or how likely they are to persist under varying conditions.

Some of these limitations may be more readily overcome by CIFOR's selection of research locations outside Indonesia. This may be especially so with respect to needing longer runs of time to assess the conservation effects of the devolution of management, since such devolution in Indonesia has been limited in comparison with other countries, including the African ones referred to in the preceding section. That it has been limited even in comparison with other South-east Asian countries is suggested by Lynch and Talbott (1995: 52), who, noting the immense profits that may still be gained from the extensive forests of Indonesia, refer to the greater persistence and pervasiveness of a "colonial mentality" there – "favoring centralized retention of legal power and authority over local resources and management practices".

Nevertheless, some Indonesian locations may be worth considering for CIFOR's "devolution" research. There are, for example, possibilities in West Timor, despite the fact that obtaining government acceptance of community forest management remains "the major issue" for WWF in its work in and around West Timor's Gunung Mutis Forest Reserve (WWF 1996a). However, the wealth of available anthropological materials concerning the interior of West Timor (see Sherlock 1980 and Rowland 1992 for bibliography) and the likelihood of a similar wealth of Dutch and possibly Portuguese archival materials especially in connection with the centuries-old sandalwood trade make it a good bet that Timorese research can provide useful answers to some questions that are appropriate and important for "devolution" researchers. These could, for example, be questions about how and why certain political and land-use institutions, now regarded as indigenous by casual observers, have developed and changed through interactions of local elites and external colonial and commercial agents in past contests for the control of forest resources (cf. Zerner 1994 on Maluku) and about problems and possibilities in adapting such institutions, as WWF is trying to do in its Mutis project (Setyawati 1996), for community-based forest management and conservation.

In case CIFOR would consider developing a longer-term project in Timor or one of its neighbouring islands if and when community-based management is implemented there, it should be noted also that obtain-

ing measures of the success or failure of such management by means of inventorying forest resources and monitoring their exploitation would be much easier in the dry woodlands of Eastern Indonesia – comparable, as noted above (p. 4, citing Sayer 1994a), to southern Africa's miombo woodlands – than in Kayan Mentarang's much more diverse and ecologically complex rainforest.

If we are looking to the future, we should recognise also that it may be one in which there are possibilities for appropriate CIFOR research in Kayan Mentarang as well. Thus, as noted in a recent WWF proposal (1996b) for Kayan Mentarang, one possibility is that "long-term planned and unplanned economic development over the next 5 to 25 years will bring new pressures to bear in the form of roads and an accompanying influx of migrant farmers and plantation schemes". If this should happen, CIFOR may well want, years from now, to study the effects that the changes have on Kayan Mentarang's forests and people. It may be especially interesting to CIFOR to see whether the institutions now being revived or developed with the help of WWF and other organisations can stand up to the new pressures.

In the meantime, important research on people and forests, even if mostly not research within CIFOR's purview, remains to be done in Kayan Mentarang. As mentioned at the beginning of this section, the recommendations that I myself have made for such research are reproduced here as an appendix. Along the same lines as some of my recommendations but with greater refinement, Puri has been developing a project to study the successions of tree species in sites abandoned by Kayan Mentarang's people – including, as described in Puri (n.d.), the abandoned forest campsites which semi-sedentary Penan hunter-gatherers have littered with the seeds of many tree species after having gorged themselves with fruit there during the large mast fruiting events that occur every two to three years. This project is part of Puri's overall long-term programme, paralleling research efforts in other parts of the world (e.g., Roosevelt *et al.* 1996 in the Amazon) to show human impacts on tropical forests formerly thought to be virgin or little affected by human activities. The programme is, in my mind, important for both forest ecology and human ecology. Furthermore, if programmatic statements such as those in CIFOR's *Strategy* (1996) about "studying forests and the people who influence them as a single integrated system" are to be trusted, it should be of at least tangential interest to CIFOR's Project on "Forest Ecosystem Management", which has so far had only very limited social science input. It should, however, be noted that the actual

research called for in the programme is mostly the biological study of plant successions and biodiversity at selected sites, notwithstanding that some anthropological research will be needed to choose the sites on the basis of their being known to have been used by human beings in certain ways before being abandoned. Further, it should be noted that the programme has no appreciable relevance to CIFOR's fieldwork-oriented projects in which the role of social science has so far been more substantial.

If the research for which Kayan Mentarang is especially well suited is either not social science or else is not relevant to CIFOR's chosen and/or mandated social science foci and if there are more suitable locations than Kayan Mentarang for doing the kind of social science in which CIFOR is interested, why should it have given priority to Kayan Mentarang as a locale for social science? The answers relate mostly to pragmatic reasons for concentrating research in and around the area that the Government of Indonesia has officially made available to CIFOR for research purposes. CIFOR also believes that there are some synergistic benefits to be gained by having social and biological scientists working in the same area. To date, however, CIFOR's biologists have not yet done any substantive Kayan Mentarang research.

Despite this, I found that several of CIFOR's senior biologists believed that CIFOR's current Kayan Mentarang project on possible links between income generation and conservation is likely to be inconclusive. I found, moreover, that these same biologists felt they had insufficient opportunities to voice their misgivings. This points to CIFOR's need for a clear, well defined, and rigorous in-house and/or external process for reviewing projects so that more will be done in the future to eliminate those with little chance of generating key findings and to identify appropriate locations for those that are more likely to succeed.

As suggested in the first section, having a rigorous review process should not mean requiring conformity to the notions of scientific method nurtured by one's specific disciplinary training and experience. For example, experimental methods or quantitative methods or projects designed and conducted solely and expressly to test particular hypotheses or causal claims may often be desirable but need not be categorically required.<sup>4</sup> However, what should always be looked for are, first of all, clear and potentially broadly significant research questions relevant to CIFOR's mandate. Much of what

I mean by "potentially broadly significant research questions" has, I believe, already been sufficiently indicated in the first section. Since, however, the emphasis there was on why-questions, some brief discussion of the appropriateness of what-questions for CIFOR's research agenda and for inclusion in the project proposals to be subjected to the review process that I am recommending is in order here.

Certainly there are what-questions that can be important for CIFOR insofar as they concern possibly widely occurring events that are likely to significantly affect forest resources and the people dependent on them. Thus, before the research on the why-questions referred to in my second section about devolution of forest management could be meaningfully proposed for CIFOR, it had to be established that what was happening in many different countries was such devolution. Something similar may be said about Neumann's more specific why-questions concerning the rising involvement of local NGOs in forest-related development projects or the related why-questions that I have raised about donor infatuation with such involvement. Before proceeding to these questions, we need better documentation of both the rising involvement and the donor infatuation. That is to say, we need to know more clearly what has been happening before we commit ourselves to going very deeply into the question of why.

Similarly, the what-questions set forth by Ruiz Pérez (1995) about non-timber forest products, especially the questions about such possible trends as the increasing collection of the products from "more anthropogenic types of forests" and the increasing explicit attention to the products in national policies, need to be answered so that CIFOR researchers may know whether some changes or trends have been widely occurring and whether, accordingly, research projects to answer why-questions about them should be developed. Presumably Ruiz Pérez himself looks forward to developing such projects and has them in mind when he alludes to eventually having "field-test hypotheses" in addition to the so-called research hypotheses that he has formulated at a "very general level" (Ruiz Pérez 1995: 6).

But, however important it may be to CIFOR projects to have answers to what-questions such as those noted in the preceding paragraphs, I still maintain that, as argued in my earlier CIFOR paper, showing causal connections among events and answering why-questions about them should be "basic goals" in our research

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<sup>4</sup> For a report on a social science project in which many of the hypotheses that were considered did, in fact, emerge only in the course of fieldwork, see Vayda and Sahur 1996.

projects (Vayda 1996: 1-2). In the present context, I maintain also that, in the review process being recommended, it is mainly with respect to why-questions that proposals should be judged. The why-questions posed in the proposals should of course be broadly significant by the kind of criteria indicated in the first section. In addition to this, here are a few non-disciplinary considerations – including some rather banal ones – on which I regard it appropriate to base judgements of proposals concerning why-questions:

whether the events about which either the why-questions are asked or the claims of possible causal connection are made are described concretely enough so that the concrete evidence needed for providing answers and for deciding among alternative answers can also be clearly indicated;

whether the literature relating to the questions has been adequately reviewed so that the proposed research would build on rather than repeat previously successful research, would avoid rather than repeat previously unsuccessful research, and would take into account the causal connections plausibly proposed but not yet adequately tested by others;

whether enough information, derived from the available literature and/or from the previous experience and observations of either the proposal-writer or others with whom she or he has been communicating, is provided to indicate that the methods and locations chosen for the research are appropriate for seeking the needed evidence.

The most substantive of these considerations may be the first one, which echoes similar considerations put forward by me in my CIFOR paper (Vayda 1996) and by some philosophers of social science elsewhere (e.g., Kincaid 1996, especially chap. 8). This consideration is worth underscoring because it is often ignored. An example of not describing concretely enough the events about which claims of possible causal connection are made are the previously mentioned claims currently

in vogue in conservation circles about the connection between sustainable economic development and biodiversity conservation. Since, according to Wells (1994-95: 7), there has been no adequate reflection on what these claims mean, his statement that adequate research on them has also been lacking is no surprise.

The other considerations concerning literature review and research methods and locations are worth noting here insofar as they could have helped to minimise the problems encountered by CIFOR's current research in Kayan Mentarang. Thus, a literature review should have led to modification of Kayan Mentarang research plans and objectives on the basis of such studies as I have cited here, including Polunin's (1984), showing restriction of access to traditional resource areas for reasons other than conservation; Browder's (1992), suggesting that a factor in the intrinsic instability of commercially oriented extractive economies is increased exploitation rather than conservation as particular forest products become more scarce and fetch higher prices; Wells's (1994-95), deploring the fact that projects on interrelations of biodiversity conservation and sustainable economic development have proceeded without adequately defining their subject matter or being able to measure changes in it in anything but the crudest way, if at all; Zerner's (1994), indicating the need for historical documentation to identify impacts of past external agents on ostensibly indigenous conservation institutions; Rudel's (1995), indicating the need for – and uses of – data on longer runs of time to show forest conservation effects; and the recent *Aquilaria* studies, indicating the difficulty of monitoring either *gaharu* exploitation or the success of measures intended to control it and also suggesting that *Aquilaria* is unlikely to become extinct in village forests as a result of *gaharu*-collecting by non-villagers.

I believe that the adoption of more rigorous procedures for proposing, evaluating and accepting projects will make CIFOR more successful in carrying out its mandate for strategic research on managing tropical forests and improving the livelihoods of forest-dependent people.



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## APPENDIX

**Report on Kayan Mentarang Consultancy  
July-August 1992  
for  
The World Wide Fund for Nature (WWF) –  
Indonesia Programme**

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## **Background<sup>1</sup>**

Indonesian Borneo's Kayan Mentarang Nature Reserve, with a gazetted area of 1.6 million ha in the far interior of the province of East Kalimantan near the Malaysian border, is the largest protected block of rainforest in Southeast Asia. It is vitally important as a refuge for numerous species, particularly rare and endemic ones. The reserve is in a region of great geological, altitudinal and biological diversity. About half the reserve lies below 1,000 m and contains areas of species-rich dipterocarp forest, while the mountain forests, ranging up to more than 2,500 m, are believed to contain a high proportion of endemic species. As of mid-1992, there has been little biological or anthropological research in the region.

About 10,000 Dayak people live in or near the reserve in East Kalimantan. They practice agriculture and use forest resources in a wide variety of ways. Traditional rights of access to farmland and forest products are complex and variable. Some formerly inhabited areas within and near the reserve have been depopulated by emigration, leaving numerous patches of young and old secondary forest. There are also a few scattered grasslands, where wild cattle (*banteng*) occur.

The far interior of Borneo has been inhabited for several centuries at least. Extensive archaeological remains, including stone burial chambers believed to date from between 100 and 500 years ago, occur in the reserve. Other evidence, including secondary forests and grasslands in areas now uninhabited and Dayak migration histories, also suggests that human activities have had significant effects on forests in the region for centuries.

## **Recommendations for Research in Human Ecology and Anthropology in Relation to Conservation Management**

The biodiversity of Kayan Mentarang Reserve, indigenous people's knowledge of that biodiversity and their practices which have effects on it have begun to be documented by the short surveys already carried out regarding birds, fish, tree flora, rattan, medicinal plants and rice varieties, as well as by Rajindra Puri's longer-term ethno-zoological studies. Clearly, however, many more surveys are needed and much more research is to be done if a comprehensive conservation program for Kayan Mentarang in which the indigenous people have key roles is to be developed. In line with some current thinking in conservation biology and human ecology, at least some of this research should be directed towards ascertaining not simply what diversity and what knowledge of diversity are present but rather how that diversity and knowledge of it are generated and maintained (see my review of D.B. Botkin's *Discordant Harmonies in Human Ecology*, vol. 19 (1991), and the articles by B. Walker in *Conservation for the Twenty-first Century*, Oxford University Press, 1989, and S. Pickett *et al.* in *Conservation Biology*, Chapman and Hall, 1992). Thus, Dolvina Damus's valuable surveys in the Upper Bahau, showing 27 rice varieties planted in one village and 31 varieties in another village, need to be followed up by research, similar to that con-

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<sup>1</sup> This section, prepared by Timothy C. Jessup of WWF (Indonesia Programme) in August 1992, was inserted in the text in October 1995 and revised very slightly in May 1996.

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ducted by Paul Richards on rice-farming in Sierra Leone (see his *Coping with Hunger*, Allen & Unwin, 1986), on how knowledge of the varieties is distributed within villages and what the sources of that knowledge are. In Richards's project, a complete inventory of rice varieties planted in a village was first made and then a random sample of farmers was asked about their current varieties, i.e., when they were first grown, where and how the seeds had been obtained, whether current varieties had taken the place of others which the respondent was no longer growing, and why the changes had been made. These questions are appropriate for the further research in the Upper Bahau too, but there would need to be special attention as well to varieties abandoned by most farmers but still grown by a few (such as one Kenyah woman I heard about during the visit to Kayan Mentarang). Among the questions to be asked are why the individuals in question are maintaining the varieties and whether and under what circumstances their knowledge (and seeds) of the varieties might be shared with others. During the visit to Kayan Mentarang, Indah Setyawati became interested in this project, and she should be considered for undertaking it after completion of her 1992-93 academic-year studies at Rutgers.<sup>2</sup>

Questions similar to those concerning rice varieties need to be addressed concerning other biological resources known and used by Kayan Mentarang people. For example, the valuable research by Herwasono Soedjito and others, which has already resulted in the collection, cataloging, and chemical analysis of some 200 medicinal plants found to be used in Kayan Mentarang (*AsiaWeek*, May 1, 1992, p. 55), needs to be followed up with systematic research on who knows about the plants and how that knowledge was acquired and will be, or can be, transmitted to others. Especially with respect to biological resources used relatively infrequently (e.g., wild plants eaten only in the case of crop failures or animals encountered only rarely by hunters), research needs to be directed at how and by whom is knowledge stored and transmitted (see discussions of these matters in E. Colson's article in the 1979 *Journal of Anthropological Research* and S. Mithen's book, *Thoughtful Foragers*, Cambridge University Press, 1991, in which questions are raised, inter alia, about the roles of rituals and story-telling in maintaining knowledge and about whether the knowledge retrieved by individuals and transmitted to others is knowledge in the context of particular past events involving a biological resource or else knowledge abstracted from that context). In all of this research, particular indigenous knowledge on which it will be important to focus is knowledge of the habitat and propagation requirements of animal and plant species (e.g., forest trees bearing edible fruits) insofar as that knowledge is used by the people themselves or may be used (by themselves or others) for maintaining both the species and their habitats (cf. pp. 142-145 of J.P. Brosius's article in *Human Ecology* 1991 on Penan knowledge and practice for the "stewardship" of sago palms).

Research on indigenous ignorance as well as on indigenous knowledge can be important for conservation planning and management. Examples of possible mismanagement of biological resources as a result of indigenous ignorance are provided by anthropological research conducted in conjunction with training programs in integrated pest management in Honduras and Java; in both places, it was found that some farmers relied excessively on chemical control of insect pests and were held back from accepting biological control because of their notions about spontaneous generation of insects and their having little idea of some insects as predators or natural enemies of others (see J. Bentley's articles on the Honduran case in *Agriculture and Human Values* 1989, *Human Organization* 1991, and *Culture and Agriculture* 1992; the research in Java was conducted in 1990 by University of Indonesia students supervised by Iwan Tjitradjaja, Anto Achadijat, and me).<sup>3</sup> Although over-reliance on chemical insecticides may not be a problem anywhere in Kayan Mentarang at present, these examples should still alert researchers to the need to identify misconceptions which may affect local people's management of biological resources. The fact that policymakers and scientific advisors in developing countries so long ignored indigenous people's knowledge as an invaluable resource for environmental management should not be a reason for going to the other extreme and not recognizing that such knowledge does not pertain to every component of the indigenous people's environment but depends rather on the component's observability, utility, and other characteristics (which may themselves merit investigation). In the aforementioned Honduran and Javanese cases, likely factors in indigenous ignorance included the difficulty of making certain kinds of observations about the insect pests (their reproductive behavior, their metamorphoses, their being preyed upon by other insects) and the fact that there had been little incentive for making such observations until recently when the recourse to broad-spectrum chemical insecticides killed off the pests'

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<sup>2</sup> This work has been completed. See I. Setyawati, 1995, *Indigenous People and the Conservation of Crop Genetic Diversity: Knowing, Selecting, and Using Rice Varieties in East Kalimantan*, Master's thesis, Rutgers University.

<sup>3</sup> On the Javanese case, see A.P. Vayda and I. Setyawati, 1995, "Questions About Culture-Related Considerations in Research on Cognition and Agro-Ecological Change: Illustrations from Studies of Agricultural Pest Management in Java," in: A. de Ruijter and L. van Vucht Tijssen, eds., *Cultural Dynamics in Development Processes*, Paris: UNESCO, pp. 259-268.

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natural enemies which had previously kept them in check. Similar factors no doubt account for instances of indigenous ignorance already encountered in Kayan Mentarang, e.g., with respect to soil fauna (as discovered by a researcher in the "Culture and Conservation" project) and with respect to the feeding habits of dragonflies in rice-fields (as discovered through interviews by Timothy Jessup, Indah Setyawati, and me in the Kayan Mentarang village of Long Alango). Sometimes, however, the factors which may underlie ignorance are not readily identifiable, and it may then be fruitful to undertake research to determine its causes. This is illustrated by Paul Sillitoe's study (to be published in *Environmentalism*, Routledge, 1993) of why, unlike other shifting cultivators, the Wola people whom he knew to be highly skillful agriculturalists in the Papua New Guinea highlands disclaimed any specific knowledge of the soils of the sites which they were choosing for their garden plots or swiddens; his research disclosed that the young soils of the Wola region (in contrast to the old soils of areas of shifting cultivation in Kayan Mentarang and elsewhere) are characterized, on the one hand, by overall homogeneity and, on the other hand, by the unpredictability, even to Western soil experts, of changes in their properties under cultivation, i.e., as they are subjected to varying degrees of erosion, intermixture of topsoil and subsoil, etc. Sillitoe suggests that the Wola themselves had some awareness of these characteristics; if they did, that would help explain why, unlike some Dayaks that we observed in areas of old and heterogeneous soils, Wola farmers were not testing soils in their prospective swidden sites. In any event, Sillitoe's study shows that what may seem at first to be a simple case in which better resource management could be achieved by the expedient of getting indigenous people to pay more attention to their soils or to some other particular components of their environment may turn out to be not so simple after all and may require further research to determine whether indigenous practices, with whatever knowledge (or ignorance) is behind them, diverge significantly from resource management as effective as may be currently feasible.

If we recognize the complexity and variability of human behavior and of nature in general, we cannot hope to formulate comprehensive and exhaustive agenda for research on human influences on biodiversity in Kayan Mentarang. Some kinds of research, in addition to those already noted, may, however, be mentioned as deserving high priority. Since patches in forests constitute more or less favorable habitats for different forest species, it clearly is important to have systematic research on patches resulting from shifting cultivation and from people's tree-felling in other contexts. The data to be obtained include, on the one hand, data on the frequency with which patches are made, their size and spatial distribution, and the responses of forest species to them (or in them), and, on the other hand, data on the various factors affecting or determining the timing, frequency, location, and magnitude of the human patch-making activities. Guidelines for such research, as well as illustrations, may be found in articles by K. Kartawinata *et al.* (in *Tropical Rain Forest Ecosystems, Part B*, Elsevier, 1989) and C. Mackie *et al.* (in *Proceedings of the Regional Workshop on Impact of Man's Activities on Tropical Upland Forest Ecosystems*, Universiti Pertanian Malaysia, 1987). As a means of obtaining a temporally and spatially more extensive record of human patch-making and human-induced variations in biodiversity in Kayan Mentarang, using archaeology, ethnohistory, paleo-ecology, and remote sensing technology should be considered. George Morren's ongoing project on temporal and spatial variations in Papua New Guinea forests is a possible model for combining remote sensing with on-the-ground ecological research in inherently dynamic tropical forest landscapes in which humans are important actors (see Morren's article in *Applications of Space-Age Technology in Anthropology*, NASA, 1991).

Like their role as patch-makers, the role of humans as predators should be accorded high priority in research. Rajindra Puri's work on this topic is an exemplary beginning and has already yielded invaluable data on prey animals and on the methods, times, and contexts of their capture by Penan and Kenyah people along the Lurah River<sup>4</sup>; thorough consideration should be given to the detailed recommendations which he himself has made, in his June 1992 "Preliminary Report" (pp. 42-45), for building on and extending his work. As he notes, important topics for other investigators to deal with include the long-term effects or historical impacts which local people as predators have had on wildlife and also the effects currently being produced by an influx of hunters and collectors from downriver areas. Additional data on some topics with which Puri has been dealing would also be welcome. An example is the topic of indigenous people's recognition of so-called ecological indicators. Concerning this, Puri's report (p. 38) notes that pig movements and the locations of such forest resources as aloes (*gaharu*) wood and of fruiting trees favored by prey species are inferred by Penan hunters and collectors from the calls or observed presence of certain birds, cicadas, and honey bees. It would, however, be good to know also whether and how (and how reliably) estimates are made of the abundance, density, or frequency (or changes therein) of animals and whether such estimates lead to an increase or a reduction in efforts to capture particular prey species (either the species to

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<sup>4</sup> Puri's Ph.D. dissertation in anthropology, based on his Kayan Mentarang research, was completed at the University of Hawaii in early 1996.

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which the estimates refer or else other species thought to be associated with the “indicator” species). Data obtained on this could lead to some specific recommendations for involvement of indigenous people in conservation management in Kayan Mentarang. Incidentally the data might also be useful to cognitive scientists in their current debates concerning human beings in general as “intuitive statisticians” (see the articles by G. Gigerenzer in *European Review of Social Psychology* 1991 and L. Cosmides and J. Tooby in the 1991/92 Preprint Series of the Center for Interdisciplinary Research, University of Bielefeld).

Certain other matters must be mentioned here because of the importance of making research relevant to conservation management. In planning for Kayan Mentarang people’s participation in such management, fixed or stable patterns or practices in their relations with their environment might seem good foundations to use. Therefore it might be thought that discovering or identifying such patterns or practices should be a main objective of research. Arguing against this, however, is, first of all, the possibility that investigators will find more variation than fixity or stability in various domains. I have, in a general way, discussed this elsewhere (e.g., in my article in *Canberra Anthropology* 1990).<sup>5</sup> The possibility is, moreover, strongly indicated by preliminary observations made in Upper Bahau villages by Jessup and me in August 1992 and by Bernard Sellato earlier. For example, in the domain of agriculture, the following forms of cultivation were found, many in the same village with individual farmers or households either combining forms or switching from one year to another between them: tuber gardens; semi-managed stands of fruit trees and sago palms; mixed-crop swiddens; cash crop gardens of coffee, cinnamon, and black pepper; and irrigated rice fields. Comparably, with respect to hunting, fishing, and collecting activities, very considerable diversity is described in Puri’s report, which refers as well to frequent, unpremeditated activity-switching in the “daily jumble” of subsistence-related activities (p. 35). Even with respect to land allocation and tenure, which will be important in relation to conservation management, we may not find an established set of principles, generally known and usually and predictably observed by members of a particular ethnic group and thus identifiable as their land tenure system (e.g., the “Kenyah land tenure system” or “Kayan land tenure system”, which George Appell posits in various articles, including the paper which he presented at the Borneo Research Council Conference in Kota Kinabalu, July 1992). What we may find instead, as Jessup has already suggested from his studies among Kenyah villagers in the Apo Kayan (outside the present boundaries of Kayan Mentarang), is land allocation by means of flexible recourse to various principles, precedents, and options, known and articulated to varying degrees by different individuals and involving consideration of not only previous use of particular sites and consequent rights to them but also such matters as present differences between households in their size and composition (see Jessup’s article in *The Heritage of Traditional Agriculture among the Western Austronesian*, Department of Anthropology, Australian National University, 1992, and, for a more general discussion of the variability of land tenure in interior Kalimantan, J. Rousseau’s *Central Borneo*, Clarendon Press, 1990, pp. 138-144). If detailed research is conducted (as it should be) on land allocation and tenure in Upper Bahau villages or elsewhere in Kayan Mentarang, it will need to be research focused on how decisions about land use and land tenure are actually made and acted upon in different contexts and in relation to different contingencies. By showing the contingencies and the contextual factors that must be taken into account, such research can be valuable even if it does not disclose stable, culture-specific patterns. John Schneider, a Rutgers graduate student in anthropology, is interested in undertaking such research in Kayan Mentarang villages and should be encouraged in his efforts to develop and fund his project.<sup>6</sup>

And even if apparently stable or long persistent indigenous patterns conducive to (or at least compatible with) conservation management should be found in certain domains, there can be no guarantee, in the absence of controversial policies of “enforced primitivism” (see P.C. West and S.R. Brechin, eds., *Resident Peoples and National Parks*, University of Arizona Press, 1991, especially chaps. 1, 25, and 29), that the patterns will continue as new technological possibilities and economic opportunities present themselves to the people. A conservation management plan to be carried out with the people’s cooperation and participation must allow them to change. In conjunction with any plan which does that, research in human ecology and anthropology can and should have a continuing, important role in tracking the changes that occur in people’s conservation-related practices and in analyzing the causes and effects of the changes.

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<sup>5</sup> A slightly revised version of the *Canberra Anthropology* article, “Actions, Variations, and Change: The Emerging Anti-Essentialist View in Anthropology,” was published in 1994 in R. Borofsky, ed., *Assessing Cultural Anthropology*, New York: McGraw-Hill, pp. 320-330.

<sup>6</sup> Schneider’s research on causes and effects of variations in land use/tenure and land boundaries in Kayan Mentarang began in 1994 and, after an interruption in mid-1995, is being resumed in the latter part of 1996.

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