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DOING AND KNOWING: QUESTIONS ABOUT STUDIES OF LOCAL KNOWLEDGE

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ABSTRACT

Anthropological claims about indigenous or local knowledge often exaggerate the cultural mystique of such knowledge "systems" and the difficulties associated with rendering local knowledge accessible to outsiders and with ascertaining its utility for initiatives in economic development and environmental conservation. We argue that part of this confusion reflects a common tendency for researchers to preoccupy themselves with understanding the knowledge per se rather than with understanding what knowledge actually influences human behavior in different situations or contexts and how and why it does so. We propose, as an alternative, an approach which begins by focusing on specific events or actions, rather than knowledge systems, as our objects of study and explanation and then goes on to consider what actors know mainly insofar as that can help us to make sense of the events or actions of interest. Such an approach de-mystifies the notion of knowledge systems and makes productive investigations possible without always requiring laborious investments in ethnographic fieldwork. Illustrations of this approach and of the arguments presented in its support are drawn mainly from our research in Indonesia and the Philippines.

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INTRODUCTION

Like others who are advocates of greater involvement of anthropologists in studies of local knowledge relevant to new initiatives in economic development and environmental conservation (see especially Sillitoe 1998a, 1998c), we are interested in the actions that people take in using and managing their environments or environmental resources, in the knowledge that their taking those actions and not taking certain others is based on, and in the causes of changes in the actions and their knowledge bases. Presumably also shared with these advocates is our belief that studies of these matters can be important for the success of the initiatives in economic development and environmental conservation. Our participation in the studies to be described below was motivated, *inter alia*, by this belief.

However, we part company with at least some of the advocates on certain methodological issues related to practically relevant studies of doing and knowing. These are the issues that will be addressed here, and we will cite our experience in research projects in Indonesia (Vayda and Setyawati) and the Philippines (Walters) to illustrate how our methods for dealing with the questions differ in practically advantageous and consequential ways from methods that have been employed or advocated in studies of indigenous or local knowledge.[1]

In brief, our view is that anthropologists can deal more effectively and expeditiously with the matters specified in the first paragraph if they do not commit themselves to so-called holistic studies of necessarily shared or socially or culturally embedded local knowledge. In support of this view, we will present methodological arguments echoing those we have previously made elsewhere (especially in Vayda and Walters 1999; see also Vayda 1996, 1997, 1998, Vayda and Sahur 1996, and Vayda and Setyawati 1998). On the one hand, these are arguments in favor of being guided in our research by questions about the causes of outcomes of interest. On the other hand, they are arguments against limiting those questions to *how*-questions, concerned with how

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[1]Sillitoe (1998a:223, note 2), conceding that it is "difficult to draw lines between indigenous knowledge, local knowledge, popular knowledge, folk knowledge, and so on," opts for the term "indigenous knowledge" because it has wider currency in contemporary development discourse than do the other terms (cf. Purcell 1998:259). All of the aforementioned terms are, however, problematic in one way or another. In this article, we opt for "local knowledge" because it seems to us a somewhat less incongruous label than "indigenous knowledge" for some of the recently acquired knowledge of farmers and fisherfolk which is considered in our case studies. As will be apparent, not all of the knowledge to be discussed by us has what some anthropologists engaged in studies of indigenous or local knowledge include among its defining characteristics, i.e., its being clearly local, culturally embedded, time-tested, and intergenerationally transmitted (Berkes 1999:5-7; Ellen 1998: 238; Hunn 1993).

factors privileged in advance by the investigator influence such outcomes as environmental changes and the actions that people take in using and managing their environmental resources. These arguments were made in some of our previous publications (especially in Vayda and Walters, 1999) against privileging certain kinds of political factors in advance, but here we will make them against so privileging cultural knowledge and so-called indigenous knowledge systems. And we will further argue here that eschewing such privileging is important for making our studies bear more effectively and expeditiously on development and conservation initiatives.

Our arguments for studying local practice and knowledge without necessarily engaging in long-term, holistic ethnographic research should, however, not be construed as opposition to all in-depth, ethnographic fieldwork in relation to development and conservation initiatives. In fact, as the case studies in the following two sections should make clear, the amount of time and effort invested in field work will need to vary, depending on a variety of pragmatic considerations. Clearly there will be cases where in-depth ethnographic studies are needed. But the fact that the results from such studies *sometimes* justify the time, effort, and expense put into them cannot be used to argue that the studies must *always* be undertaken. By the same kind of illogic, the fact that the studies are sometimes unproductive could just as well be used to argue that they should *never* be undertaken. There have indeed been unproductive studies, i.e., research leading to the discovery of local knowledge and practice that turn out on analysis to be "useless, harmful, or otherwise scientifically indefensible" (Lees 2000, citing Hess 1997). But, as Hess (1997: 79) has noted, such cases are, by and large, omitted from the literature on local or indigenous knowledge systems. In her report on her own research on local knowledge and practice related to sheep management in an Andean community in Ecuador, Hess does ask for granting the same validity to our own and indigenous views of reality, including the different views of the etiology of disease in sheep. But, despite this appeal, she brings forth no local knowledge and practice on which development initiatives might build. On the contrary, her account indicates that local knowledge and practice have resulted in sick and unproductive sheep and impoverished people.

As has already been stated, it would be illogical to use such cases to argue against *ever* undertaking in-depth, holistic studies of local knowledge. The cases do, however, support our view about the need for making local knowledge studies bear more effectively and expeditiously on development concerns and initiatives. In light of the possibility that there will be no significant payoff from some such studies regardless of how much time, effort, and resources are invested in them, it becomes all the more important to find and adopt ways of zeroing in on the local knowledge and practice which do have significant and positive practical relevance to development and conservation initiatives.

Our arguments here are in accord with certain points that others have made in favor of "rapid rural appraisal" and similar shortcut, rapid research methods in development studies. In particular, we agree with points made by Chambers (1991: 522) to the effect that it is important to know what is not worth knowing and to abstain from trying to find it out. Regrettably, however, such points are left virtually as slogans by Chambers and like-minded advocates in the development-studies field. Unlike us, they are not concerned with explicating procedures in research guided by questions about the causes of outcomes of interest. Accordingly, no clear

advice is to be found in their writings about how, in the course of such research, decisions are to be made about what is worth knowing and what is not. Thus, notwithstanding all their discussion of surveys, check-lists, flow charts, diagrams, and other methods for obtaining and recording locally available information (see, for example, Chambers 1991, Mikkelsen 1994), what is obtained and recorded by their methods is, both in our view and that of some other critics (e.g., Pelkey 1995), too often only background information for the more sharply focused inquiries needed to produce usable evidence for or against particular causal possibilities in the kind of research that we are advocating (cf. Walters et al. 1999:209-210).

Our own research procedures will be explicated mainly in the context of the case studies presented in the next two sections. The following key features of our approach are, however, worth noting here:

(1) Identifying at the outset certain environment-related or resource-related actions as objects of study on the basis of their relevance to development and/or conservation concerns or programs rather than on the basis of their meeting conventional criteria for anthropological subject matter.

(2) *Not* undertaking studies of knowledge per se and *not* singling out shared or so-called cultural knowledge for investigation but, instead, trying only to ascertain any local knowledge likely to be useful to us for deciding about focusing on and explaining particular actions because of their relevance to development and/or conservation concerns.[2]

(3) *Not* assuming that the practices and knowledge behind them which are of interest to us are embedded in a "whole system" (Sillitoe 1998a:247) or "encompassing cultural matrix" (Ellen 1998:238) which must be elucidated or comprehended in toto if we are to understand the practices and knowledge well enough to use them effectively to meet development and/or conservation goals. Instead, subscribing on these matters to the views of such philosophers as Lewis (1986) and Rorty (e.g., 1989: chap. 1) and such social scientists as Hawthorn (1991:173-174, 180, and *passim*), we assume the following:

   (a) That understanding or explanation of anything that people do or know can be based on seeing or showing its connections to any number of other things or events, whether within an encompassing cultural matrix or not.

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[2]An illustration from the Javanese pest-management studies described in the next section are the actions of impaling crabs on sticks and then planting the sticks at the edge of rice-fields. Learning from farmers that they based these actions on their knowledge that the crabs would attract rice seed bugs which could then be set fire to, we looked more closely at the actions and used the farmers' knowledge to help explain them.

(b) That partial explanations, indicating only some connections and missing others, are useful and, practically speaking, necessary.[3]

(c) That our decisions about which connections to pay more or earlier attention to may be made on pragmatic grounds, such as those discussed in the next two sections and in our earlier publications (e.g., Vayda 1983 and 1996:9-16, 22-24), rather than on the basis of theories or discipline-rooted biases about the kinds of systems within which connections with explanatory import must be sought.

Before proceeding to the case studies, we must make clear that we have been referring to *cultural embeddedness* only because of the importance assigned to it by others presenting anthropological views of indigenous or local knowledge (e.g., Sillitoe 1998a; Ellen 1998). Because we share the view of some critics (e.g., Sunley 1996:345-346; Portes and Sensenbrenner 1993) who argue that concepts of cultural or social embeddedness generally suffer from vagueness and that their significance or utility remains uncertain, we do not employ any concept of embeddedness when we report on our own research.

There is, however, something to be said about a special, relatively clear sense in which a concept of embeddedness is sometimes used. In this sense, embedded knowledge refers to knowledge which local people have gained from their forbears about ways of doing things but without knowledge of why those ways work. What is known is simply that those ways must be used if the crops are to grow well or if the prey is to be captured (see, for example, the discussion in Alcorn 1989:65-66). Although not encountered in the course of the Indonesian and Philippine research from which our main illustrations have been drawn for this article, such knowledge might still seem to some anthropologists to need to be considered here. This is because it might seem to them that, on the one hand, our less opaque examples of knowledge from Indonesia and the Philippines have led us to overstate our arguments against committing ourselves to holistic studies of cultural systems and that, on the other hand, such holistic studies are still needed to elucidate local knowledge which is embedded in the special sense indicated in this paragraph. Accordingly, an example of such knowledge from the New Guinea highlands will be briefly considered after the Indonesian and Philippine case studies presented in the next two sections.

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[3]A classic statement of the complexity of event causation, clearly implying the need for partial explanations, is the following from Carlyle's 1830 essay entitled "On History": "...actual events are nowise so simply related to each other as parent and offspring are; every single event is the offspring not of one, but of all other events...and will in its turn combine with all others to give birth to new..." (Carlyle 1899:89). Referring again to the pest-management studies, we may reasonably claim that useful explanations of such actions as the spraying of a particular Javanese rice-field with a particular insecticide at a particular time are possible without our considering all causal antecedents of the actions in chains of events extending back at least as far as the Big Bang.

STUDIES OF KNOWLEDGE AND PRACTICE RELATED TO INSECT PEST MANAGEMENT IN INDONESIA

As part of a nationwide Indonesian program intended to train 2.5 million rice-growers in integrated pest management (IPM), an invitation was extended at the beginning of 1990 to Vayda and a number of University of Indonesia faculty members and students, including Setyawati, to engage in research on variation and change in agricultural pest management as practiced by Central Javanese rice farmers. The questions which entomologists and other scientists in the program put to us as anthropologists concerned not only the farmers' actual practices and the ideas on which these were based, but also the likelihood of change in the practices with the acquisition of new knowledge such as the IPM training was intended to help develop. Examples of this included knowledge about interactions between insect pests and their predators, about damage caused by different pests at different stages in the rice plants' growth, and about the dangers of resurgence of pests following indiscriminate use of broad-spectrum pesticides which do a better job of eliminating the pests' natural enemies than eliminating the pests themselves. The hope was that farmers who had presumably been overusing pesticides would become ecologically informed decision-makers and actors in their rice-fields. This, suggested one of the entomologists, would mean a "paradigm shift" among the farmers.

Coming to the project as ecological anthropologists but regarding as its essential subject matter the interrelations of knowledge and action or of cognitive and agro-ecological change, we sought to make up for our own lack of training in cognitive anthropology by reviewing potentially relevant literature in that area. The discussion in this section, incorporating much of a previously published article (Vayda and Setyawati 1998), will be concerned with why that review was disappointing to us and how it led us to see that anthropologists' assumptions about their proper subject matter were keeping them from making their studies more effectively and expeditiously relevant to programs of economic development or environmental management. There will also be further explication of the research procedures that we favor for achieving such relevance.

That much of cognitive anthropology has been little concerned with practical activities or behavior is well known. It has been noted both by critics of the field (e.g., Harris 1968: chap. 20 and others cited in Johnson 1974:199) and by some of its practitioners (e.g., Gatewood 1985; Johnson 1974; Lave 1988; Quinn and Holland 1987; and, more recently, Hutchins 1995:xi-xii and *passim*; Keller and Keller 1993) and was known to us before we began our literature review. Even so, we hoped to find more than we actually did concerning possible knowledge bases of pest management practices. Thinking, for example, that such bases might include specific knowledge of insect reproduction and behavior, including predator-prey interactions, we hoped to find some good descriptions of people's knowledge, as well as identification of their ignorance, concerning such matters. Instead we experienced disappointment similar to that reported by Bentley, another anthropologist involved in IPM-related research. What he found was that too many accounts ostensibly treating indigenous knowledge of the natural world emphasized linguistic distinctions with little practical relevance. He also found that even as detailed and ethnographically far-ranging a work as Hunn's on zoological classifications by Tzeltal-speaking Indians in Chiapas (1977) was deficient in describing aspects of the people's knowledge and ignorance which could be important for our understanding their actual or potential pest management practices (Bentley and Andrews 1991:117; on the "reluctance in ethnography to record what people do not know," see Last 1981).

Our disappointment did not, however, end with accounts in which behavior was, at best, a secondary concern. It extended to studies and methodological prescriptions by some in cognitive anthropology who were vocal in decrying inattention to behavior. The problem for us here was somewhat different from the already mentioned problems with studying knowledge prior to ascertaining that it lies behind some actions or behavior of actual or potential practical significance. The problem here lay in the restrictive nature of these cognitive anthropologists' definition of both their behavioral and cognitive subject matter. In line with Goodenough's original mandate (1957) to cognitive anthropology to study whatever one must know to behave in a culturally appropriate or societally acceptable manner, the knowledge featured in their studies and programmatic statements was shared, so-called cultural knowledge, while the featured behavior was behavior that is (or could be presumed to be) culturally appropriate or societally acceptable or, in some important way, influenced by culture-specific models of the world (see, for example, Quinn and Holland 1987; Keller and Keller 1993; Goodenough 1994). Even when "ecological effectiveness" was explicitly a concern of the analyst, studying how such effectiveness is achieved through the pursuit of culturally defined goals was the program recommended (Hunn 1989:145).

We realized in due course that such programs were at odds with our desire, supported by our entomological colleagues, to be expeditious in making our studies relevant to Indonesia's IPM program. In light of this, our bases for deciding what actions and knowledge to pay attention to and investigate had to be different from the culture-related considerations used by cognitive anthropologists whose work we had reviewed. Our criterion had to be whether the behavior and its knowledge bases were, or might be, significant for (or as) actual or potential pest management, regardless of what we could see or hope to see about its being shared, culturally appropriate, culturally influenced, or societally acceptable. This criterion was geared to our project's practical goal of change in pest management by Indonesian farmers. However, the criterion also was compatible with more academic goals which we wished to pursue in the project, for example, the goal of further developing research procedures and explanatory models concerning interrelations of actions, the actors' reasons for them, and the contexts in which they occur. As indicated elsewhere (e.g., Vayda 1983, 1994, 1996, 1998; Vayda and Walters 1999) and as, in effect, is illustrated in the present article, the actions to which these procedures and models apply are whatever actions occur rather than just the kinds of standardized actions on which some cognitive anthropologists have been wont to focus.

Having said all this, we have to make clear also that, in the IPM project, we were simply not privileging rather than shunning more traditionally anthropological lines of inquiry (cf. Barth 1994:358 on focusing on acts and events rather than privileging "culture"). Thus, we certainly were not uninterested in the influence of shared or cultural knowledge and beliefs on the behavior we were studying, and we were aware that some novel or idiosyncratic pest management practices and ideas which we were finding might constitute culture in the making. But the fact remains that behavior did not have to pass some kind of cultural test in order to become a focus of our investigations and analyses.

What then was some of the behavior on which we concentrated our attention? What knowledge and ideas as bases for the behavior did we find among the farmers? What relevance to the IPM program did our findings have? And are there more widely applicable lessons here for conducting practically relevant studies of interrelations of knowledge and action and for making distinctively anthropological contributions to such studies? These are some of the questions to which the remainder of this section will be devoted.

Our examples of behavior are drawn mainly from five villages in which Setyawati and four of her fellow students from the University of Indonesia conducted field research for seven months during 1990.[4] The villages are located in the four regencies of the Special Region of Yogyakarta. Weekly morning-long IPM training sessions, including both field and classroom exercises, were held for approximately 25 farmers in each of the villages for ten weeks. Each farmer received 1,000 rupiahs (about US$0.55 in 1990) and a snack for each session attended. Research involved observation of these sessions (the "field schools"), as well as other participant observation and interviewing in the villages.

Behavior learned in the field schools was certainly interesting to us, and, for making assessments of needs and prospects for IPM, we were *especially* interested in seeing whether or how or to what extent such behavior was continued outside the schools (cf. Lave 1988: chap.2 and *passim* on learning-transfer research). An example concerns making observations of the numbers of insect pests and predators in the fields in order to provide a basis for ecologically informed decisions about pest management. The specific procedures taught in the schools for making such observations involved counting insect pests and their predators in sample rice plants along a transect in a rice-field in order to decide whether to spray the field with a chemical pesticide. Of interest to us were not only these procedures but also any alternatives to them developed by the farmers themselves.

We found that many of the farmers succeeded in learning the prescribed sampling and counting procedures and in putting them into practice in the fields rented for use in the school sessions. However, although the farmers had been told in the schools to follow the same procedures in the fields in which they were growing their own crops, we did not find any farmers doing this. Going with the farmers to their fields, we sought therefore to identify what they did instead in order to decide whether or not to spray. We found some walking through their fields, usually near the edges, and looking out for the more visible pests such as grasshoppers and rice seed bugs (*Leptocorisa*). Sometimes, as they walked, they also stretched their arms out sideways and passed them back and forth over the tops of the rice plants in order to see how many or how dense were the stemborers (*Scirpophaga*), rice seed bugs, and possibly other insect pests flying forth. Mostly the farmers did not walk into the fields at all but instead inspected them for pests from the bunds. These inspections, whether by walking through the fields or only along the bunds, were for the pests alone and not for their predators.

The observed differences between the farmers' practice in the schools and outside them led to interviews which indicated to us that evidence of whether the paradigm shift hoped for in the IPM program was being achieved had to consist of more than seeing whether the procedures of Western scientists were

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[4]The four other students were Rama Chandra, Indrati, Dian Rosdiana, and Bambang Setiawan. We are grateful to them for data which they made available to us. The students were supervised in the research by Vayda and by Iwan Tjitradjaja and Anto Achadiyat of the University of Indonesia. The program in which our project was included was called "Training and Development of Integrated Pest Management in Rice-Based Cropping Systems in Indonesia" and was funded mainly by the Food and Agriculture Organization of the United Nations (FAO).

being copied. As already noted, there indeed were farmers who could and did learn rules of sampling and counting procedures; however, they regarded these simply as rules to be followed in the schools and they saw no good reason for applying them to their own farming activities. Neither the logic behind sampling, nor predator-prey dynamics among insects (rather than among larger creatures), were understood well enough by the farmers in 1990 for them to devote -- or probably to even consider devoting -- the extra time and effort which would have been required to monitor the fields as prescribed at the time by the IPM program. That they had in fact decreased their use of pesticides was explained by some farmers by saying that pesticides had become too expensive. The rise in pesticide prices had occurred with the gradual elimination of subsidies after IPM had become the national pest management strategy by presidential decree in 1986.

We were able to return to the study area for only a brief stay in 1992, i.e., just long enough for single-day visits to two villages where field schools had been in operation for two years. These visits sufficed, however, to impress upon us that some farmers attending the schools had become quite knowledgeable about predator-prey interactions affecting insect pests. They told us that their knowledge of these insect interactions had been gained in the schools, although some referred as well to their pre-school knowledge of *non-insect* predator-prey relations, including knowledge of predation by snakes on rodent pests in rice-fields (cf. van de Fliert 1993:97).[5] The newly knowledgeable farmers told us too that they had cut their use of pesticides because they were making their decisions about pest management on the basis of their observation of numbers of both insect pests and predators in their fields. This impressed us as being in accord with the paradigm shift hoped for by our entomological colleagues. However, the farmers were not following the sampling and counting procedures prescribed in the field schools at the time of our original research. In fact, after we had made our observations of differences between what farmers did in the schools and out of them in 1990, IPM training had been modified so that it was, in 1992, regarded sufficient for farmers, as distinct from IPM researchers, to make reasonably accurate estimates rather than exact counts (Gallagher n.d.). One farmer that we talked to had, on his own, hit upon the technique of monitoring stemborers by means of seeing what densities of stemborer moths were attracted to lights that he flashed in his field at night. It was by our criterion of practical relevance that such methods of estimating pests and predators clearly merited our attention, but, as will be discussed shortly, the methods are anthropologically

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[5]Why these farmers previously lacked knowledge of insect predator/prey relations may be related not only to the difficulty of making observations of insect predation with the unaided eye but also to the probable absence of strong incentives for paying special attention to insect pests prior to the first major outbreaks of such pests as brown planthoppers in irrigated rice-fields in the mid-1970s and mid-1980s. These outbreaks were, in large part, a result of inadvertent elimination of the pests' natural enemies by means of sprayed broad-spectrum insecticides, which had only recently been added to Javanese rice farmers' tool-kits as part of the Green Revolution (cf. Kenmore 1991; Gallagher, Kenmore and Sogawa 1994; Settle *et al.* 1996; van de Fliert 1993:94).

interesting as well because of the questions they raise about alternatives to their being interpreted or explained as culture-specific practices or developments.

During our original research, the other behavior to which we paid attention included, at least initially, whatever farmers did or had done with pest management as their goal. Our thinking on this was that, once any such practices had been identified, we would look at them more closely if it seemed to us and to our entomological colleagues that the practices could actually achieve, or be adapted to achieve, the pest management intended by the farmers. One example of such a practice is using crabs impaled on sticks, which were planted at the edge of the rice-fields, to attract rice seed bugs so that they could be set fire to. Among other examples are practices that are the same as those recommended by Western scientists, e.g., flooding rice-fields to drown mole crickets (*Gryllotalpa*) and draining the fields to rid them of nematodes (see the recommendations in Grist 1986:345-347). As already noted, it did not matter to us whether these and other practices that we were finding, and whatever knowledge we were finding them to be based on, were already widely shared or evidently culturally embedded or whether, on the contrary, they were novel or idiosyncratic. If we were to find the latter to be the case with respect to something actually or potentially effective for pest management, we hoped we could proceed, in follow-up research, first to discover factors, including cognitive ones, contributing to its being practiced by some and not by others and then to assess the possibilities for its being more widely adopted. As for old and disused practices that were still remembered, we were interested in pursuing inquiries about them too but, in line with our criterion of practical relevance, not if they clearly held no promise of being revived and being efficacious for pest management in the future. An example of such an unpromising practice about which a few informants told us consists of catching a certain uncommon species of grasshopper in the bush or forest and then carrying it around the rice-fields and chanting to the rice seed bugs, "This is your father [or, according to one informant, your husband]; go with him; your home is not here."

Of interest to us too were certain instances of deliberately either *not* taking action or ceasing to take action against pests. When not doing something is deliberate, it may call for explanation just as much as doing something does.[6] In our particular project, it was relevant for pragmatic reasons to ascertain whether inaction might have causes that could keep farmers from being or becoming ecologically informed pest managers. The cases in question concerned certain armyworm (*Spodoptera*) and brown planthopper (*Nilapavarta lugens*) attacks that farmers told us about. They said that, in the early stages of rice-field infestation, they acted on the basis of practical ideas about chemical control of the pests. However, as infestation became more severe and the measures taken proved unavailing, they decided they could do nothing about it. Some said that they resigned themselves to loss of their crop and sought temporary employment as pedicab-drivers or construction workers in the city of Yogyakarta. Reasons for inaction were found by some farmers in traditional knowledge of the supernatural. To these farmers, the failure of insecticides in these cases was proof of two things: (1) that the pests in question had been magically brought

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[6]This "passive counterpart" of action is called "forbearance" by von Wright (1970:170-171), but others, like Rheingold (1988: 173-174), have felt that there is a need in English for another word for "conscious nonaction." Rheingold refers to the term *wei-wu-wei* as meeting the need in Chinese.

forth from her realm by Nyai Loro Kidul, the goddess of the Southern Ocean; and (2) that the pests would not leave before sating themselves on the crop. Indeed, to some farmers, any major pest outbreaks were attributable to Nyai Loro Kidul.

With respect, however, to the brown planthopper outbreaks, other farmers drew on other knowledge to contest this view. Specifically, they argued against it by noting the following: the absence of such outbreaks in former times when the pests that the goddess sent forth were rats (cf. Becht 1939); the fact that the planthoppers did succumb sometimes to pesticides; and the fact that visitations by them, unlike those by armyworms, did not occur suddenly and as if by magic but rather were preceded by such warning signs as strong winds or heavy rain. In the view of some farmers, planthoppers were actually brought from the north by wind and rain. Our conclusion from these cases, as well as our overall impression from observations made in the course of the project, was that mystical knowledge or magical beliefs did not keep farmers from putting practical measures for pest control into effect. Such knowledge or beliefs were invoked by some farmers to justify inaction when the practical measures that they had taken did not work, while there were other farmers who found reasons not to use the same knowledge or beliefs even in these circumstances. As one of us has suggested elsewhere (Vayda 1996: 6), recourse to such ideas at some times and not others by the same farmers dealing with the same pests shows that we need to pay attention to context-dependent variability in whether or not particular knowledge or ideas or cognitive attitudes are efficacious in guiding actions (cf. Bratman 1992; Thomason 1987). In other words, there is an argument here not only for focusing first on actions rather than studying knowledge *per se* but also for focusing, as we had, on the contexts in which the actions occur in order to come closer to pinpointing the knowledge on which the actions are based.

A more general argument may be set forth here as well in order to close this section. It is worth noting that once that the criterion of actual or potential relevance to pest management had been used to make certain actions our objects of study in the IPM project, we could proceed in what Barth (1987:24) has described as "the tradition of the wondering naturalist." That is to say, we could look closely at particular situations in the search for whatever factors may have been operating to produce those actions, without *a priori* theory-based or discipline-rooted constraints on the factors we could consider or accord priority to.[7] Thus, when we focused on cultural factors, whether such Javanese mystical knowledge or magical beliefs as have been mentioned or more practical cultural knowledge, it was not because such factors are what anthropologists *must* focus on. Rather it was because it seemed to us, at particular times in the course of our research, that particular cultural factors were likely to have affected behavior that we needed to explain. For example, when it was found that some farmers had gained impressive knowledge of insect predator-prey relations and were applying it by making field observations of insect pests and predators to provide a basis for pest management

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[7]In line with this are the appeals in Vayda 1998:578 for "clear-eyed eclecticism in pursuit of answers to questions about the causes of...concrete actions" and in Vayda and Walters 1999:171 for not confining our consideration of situation-specific causal possibilities to "those prescribed by any single or simple agenda or theory."

decisions, our attention was drawn to the fact of widely shared pre-existing knowledge concerning *non-insect* predator-prey relations, including the knowledge that many farmers had of predation by snakes on rodent pests in rice-fields. We proceeded then to ask whether farmers were drawing on this knowledge in making their decisions to commit themselves to field observations of insect pests and predators.

However, at other times in the course of the research, we were just as ready to focus on factors other than culture-specific ones to explain behavior. A case in point is the farmers' making only visual estimates of insect densities and usually doing so by inspecting fields from the edges rather than following prescribed sampling or counting procedures. We recognized this to be like behavior observed by others in similar situations outside Java (e.g., in Honduras as described by Bentley and Andrews 1991:118 and in the Philippines as described by Kenmore *et al*. 1987:106). Accordingly we sought to understand and explain the behavior less as the product of culture-specific influences than as the product of such factors as time constraints, lack of practical experience of sampling, and habits of reliance on frequency estimates and statistical judgments like those which, according to some psychologists (e.g., Gigerenzer 1991, 1992; Cosmides and Tooby 1996), are intuitively made by people everywhere and are often adequate for the objectives at hand. Anthropologists intent on making their work as practically relevant as possible will need sometimes to pay close attention to such factors instead of feeling that they must always concentrate on factors to which they are directed by anthropological theories or by norms about anthropological subject matter.

STUDIES OF KNOWLEDGE AND PRACTICE RELATED TO MANGROVE TREE PLANTING IN THE PHILIPPINES

Illustrations from the Philippines are drawn from field research by Walters in 1997 on the causes and consequences of mangrove tree planting in Bais Bay, Negros Oriental (Walters 1998, 2000a, 2000b). As in the IPM research, investigations of mangrove planting led to the finding that practically relevant knowledge was often not widely shared in the community. Specific to this case, however, was the added discovery that knowledge factors were, in general, not always important for our understanding or explaining actions or behaviors of interest.

Mangrove forests provide important ecological services and are commonly exploited by coastal communities for firewood, construction materials, and various fish, crustaceans, and shellfish (Dewalt, Vergne and Hardin 1996; Diop 1993; Hamilton, Dixon and Miller 1989; Kunstadter, Bird and Sabhasri 1986; Lacerda 1993; Macnae 1968; FAO 1994). Seventy percent of mangroves in the Philippines have been cleared since 1940 to make way for brackish water aquaculture, residential settlement, and various public and commercial infrastructure developments (Baconguis, Cabahug and Alonzo-Pasicolon 1990; Primavera 1995). Moreover, most remaining forests are degraded as a result of past and continued wood harvesting. This state

of affairs has prompted a variety of policies and programs to protect and restore existing mangrove sites (Calumpong 1994; DENR 1990; Walters 1995). In particular, the deliberate planting of mangroves is now enthusiastically promoted by governments, NGOs, and aid agencies in the Philippines and elsewhere as a means to restore degraded ecosystems while enhancing livelihood options for poverty-stricken, coastal communities (e.g., Kaly and Jones 1998; Lewis 1990; Pomeroy *et al*. 1996; Primavera and Agbayani 1996; Thorhaug 1990; Sukardjo and Yamada 1992; Saenger and Siddiqi 1993; Van Speybroeck 1992).

This broader context made significant the discovery of cases where local fisherfolk and fishpond owners have been planting and managing mangrove forests for decades under their own initiatives (e.g., Yao and Nanagas 1984; Cabahug *et al*. 1986; Walters 1995, 1997).[8] The reasons for planting are varied and include the desire to provide a ready supply of construction materials and firewood, to increase tenure security, and to protect seaside homes and fishpond dikes from storm damage (Walters 1997, 2000a, 2000b). The extent to which these existing management systems might serve as a model or offer lessons to wider programs in mangrove restoration was an important consideration for Walters in undertaking the study. Accordingly, he sought specifically to understand the origins and spread of this "indigenous" planting. The possible importance of knowledge factors in its spread was recognized from the outset, and such factors were given due attention in the course of the research whenever they were seen to be relevant to explaining mangrove planting.

Investigations led Walters to discover, first, that planters are many and diverse in the few villages where mangrove planting does occur. It is typically a private, household-based activity, but participation does not otherwise strongly correlate with education, income, or other socio-economic characteristics. The ubiquity of mangrove planting in these sites led Walters initially to speculate that there must be a well-developed, widespread and systematic knowledge base for planting in communities where it is common. However, investigations in villages in Bais Bay -- where most of the field work was done -- revealed much variation in knowledge between planters and little evidence of widely held, systematic knowledge about mangroves and mangrove planting. In fact, many planters in Bais did not give much thought to their planting activities. Often, when made impatient or annoyed by questions intended to elicit their knowledge of the subject, they insisted that planting is "easy -- you just get the mangrove and stick it in the mud and it grows!"

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[8]Local mangrove planting has similarly been documented in Indonesia (Weinstock 1994). Extensive private mangrove plantations in Manila Bay also were described early this century by Brown and Fischer (1918). As far as we know, all of these have since been cleared and the areas developed into fish ponds or reclaimed for residential housing and urban-industrial infrastructure.

That these persons were able to plant at all reflects the fact that the mangrove planting is indeed not a basically complex task and requires little technical knowledge to accomplish. More specifically, it may be noted that *Rhizophora* species are the preferred and most widely planted mangrove trees (Walters, 2000b). *Rhizophora* are viviparous, meaning that seeds begin to grow and elongate into stems while still attached to the parent plant (Tomlinson 1986). To plant, one simply collects the elongated seedlings, called "tawin," from the parent tree when ripe -- a condition easy to assess -- and places them 1/4 - 1/3 their length deep in mud. If environmental conditions are suitable and the young plants are not damaged by disturbance (see below), they will sprout leaves and grow.

These very basic planting techniques are straightforward and widely practiced in Bais. However, planters were found to vary considerably in terms of the deployment of other, more specific practices. Such variation was found, in cases, to reflect genuine differences in knowledge between planters and, either demonstrably or else possibly, to have important consequences for planting success. The remaining discussion will describe three such practices: spacing, a method of controlling shell infestation, and test planting.

Something shown to have important consequences for subsequent survival and growth of plantation trees is the initial spacing distance applied between planted seedlings (Shepherd 1986; Wadsworth 1997). In Bais, 47% of the planters in Walters's sample (n=91) used 30-60cm spacing, and 41% used spacing of only 10-20cm. The more experienced and knowledgeable planters almost always employed wider spacings (30-60cm or more) with a clear understanding of their benefits, i.e., compensation for a degree of post-planting mortality and facilitation of the rapid growth of straight stems, most of which are subsequently harvested for fish-corral construction.[9] For example, experienced planters tend to use closer spacing (30-40cm) in areas

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[9]Foresters typically recommend initial spacing of 1.5 - 3.0m for most upland tree species (Shepherd 1986; Wadsworth 1997) and 1.0m or more for mangroves (e.g., Khoon and Eong 1995; Siddiqi and Khan 1990). Researchers who first documented indigenous mangrove plantations in the Philippines were, in fact, initially perplexed by the widespread use of close spacing (e.g., Cabahug *et al*. 1986; Emma Melana and Calixto Yao, personal communication). They responded by conducting formal field trials comparing tree growth and survival of planted mangroves at different spacings (0.5x0.5m, 1x1m, 2x2m and 3x3m). In general, these studies show that close spacing of 0.5m does not appreciably reduce growth in the early years and is practical, in particular, if one's goal is to grow relatively small, straight stems for use in construction or as fuelwood or if one is planting in areas where waves and other forms of environmental disturbance exact a significant toll on survival (Yao 1996; Pedro Balagas and Emma Melana, personal communication). The dominant and most highly valued use of planted mangroves in Bais is for posts used in the construction of traditional fish corrals, called *bunsod*. For this purpose,

where they anticipate, based on prior planting experience, relatively high post-planting mortality as a result of wave damage or other disturbance (see below). In sites where post-planting mortality is anticipated to be minimal, wider spacing (40-60cm) is typically used.

Other factors were sometimes found to be taken into consideration by planters when deciding on the appropriate spacing to use. For example, knowledge of local fishing, shell collecting, or boat passage practices led some planters to increase their spacing distances so as to enable those engaging in the practices to pass through planted areas without damaging young seedlings. Some planters were also found to initially use wider spacings (around 1.0m) so that they might quickly establish claims over planting sites. In such cases, these same planters might later return and inter-plant to establish more desirable, higher densities.

What of the 41% of planters in Bais who use spacing of only 10-20cm? Spacing this close is costly in terms of added planting expense (halving the spacing distance quadruples the number of seedlings required). It also creates extremely high stocking densities which, in the absence of very high seedling mortality, almost certainly leads to crowding, reduced stem growth and subsequently high tree mortality (Shepherd 1986; Wadsworth 1997). Interviews of planters who used very close spacing like this confirmed Walters's suspicion that such practices were often ill-advised. Planters who used very close spacing were, with few exceptions, less experienced and often had small plantations.[10] When asked to talk about the very close spacing they used, they often displayed a lack of awareness of the relationship between spacing and growth and/or they cited reasons for using close spacing that were suspect. For example, a number of planters explained that they had to compensate for having such a small area by planting more trees on that area! In short, many had planted without giving much thought at all as to the consequences of specific spacing practices, a fact less surprising given that most of these planters fish for a living and so lack appreciable horticultural experience.

A second practice of interest -- the application of used engine oil to control shell infestations -- came to Walters's attention during investigation of a particular site in Bais where only one person had managed to plant mangroves successfully. Upon questioning residents in the area, Walters was surprised to learn that many in this particular area had also planted mangroves, but these had all been killed by shell infestations.[11] Further inquiry revealed that the lone,

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tree stems must be straight and are typically harvested when small, i.e., between 2.5 and 5.0cm dbh (Walters 2000a). If spacing is too close, trees will become crowded early and grow slowly. If spacing is too wide, trees will manifest bushy growth and are more likely to have crooked stems.

[10]In fact, there is a highly significant positive correlation between plantation size and spacing used ($R=0.478$, $F=26.02$, $p<0.001$, $df=89$).

[11]Newly planted mangroves can be killed by barnacles and oyster spat which attach and grow on stems, often in such densities that

successful planter had repeatedly brushed used engine oil onto the stems of the young plants in order to prevent such infestations. The planter said he had learned this technique during a casual conversation with a man whose house he had been hired to build some years previously and who, by coincidence, was a teacher at the nearby fisheries college and a former project manager for an aid-financed mangrove reforestation project. In his view, the technique was effective by virtue of making stem surfaces slippery and thus keeping young barnacles and oyster shells from attaching. However, the oil may also have some toxic effect whereby young shells and barnacles are killed directly and/or barnacle larvae are repelled from the stem surfaces. We are not aware of any rigorous tests of the efficacy of the technique.

It is nevertheless interesting that interviews with the successful planter's neighbors revealed that none were aware of his method for addressing the problem of shell infestation. Even though they had wondered about their neighbor's success in planting, they had not actually asked him how he had achieved it while they had failed. Instead, they simply gave up, citing explanations of varying degrees of plausibility for their failures. The successful planter likewise had not been motivated to offer to share his trade secret with his neighbors; he cited *eja-ejas* ("each to his own") as the prevailing ethos in such affairs. More will be said about this later.

The third practice of interest was the frequent use of test-planting, in which persons plant prospective sites with small numbers of seedlings in order to evaluate the suitability of those sites for more extensive, subsequent planting. Test-planting was found to be commonly employed by the more ambitious, entrepreneurial, and typically prolific planters. These persons know that mangroves will grow only within a relatively narrow range of environmental conditions, i.e., between the mean- and high-tide levels (Macnae 1968). But, based on their experience as fishermen and planters, they also know that intertidal lands are changing remarkably quickly in some parts of Bais Bay as a result of fishpond development, sediment deposition from rivers, and natural colonization and planting of mangroves (Walters 2000a).[12] Such processes, sometimes

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young stems are either physically toppled by the weight of the growing shells or are asphyxiated by having photosynthetic surfaces on their stems blocked by the growing layer of shells (Cabahug *et al*. 1986; DENR 1994). Vulnerability to damage from shell infestations decreases rapidly as the stem strengthens with age and grows leaves. Planted trees are typically most vulnerable in their first six months.

[12]Sediment deposition near the mouths of rivers in Bais is often substantial and results from upland soil erosion (Calumpong and Luchavez 1997). Near the mouth of the Tamugong River in Bais Bay, for example, sedimentation during the past four decades has raised the topographic level of offshore lands sufficiently to expand the habitat suitable for mangroves by nearly 200 ha (Walters 2000a).

creating and sometimes eliminating habitats suitable for mangroves, change local opportunities for planting.

As well, planters in Bais face an onslaught of environmental events -- including storms, shell infestations, entanglement by floating seaweeds, and anthropogenic disturbance[13] -- that can quickly destroy a young plantation (Vayda and Walters 1999; Walters 2000a). Experienced planters recognize that success depends to a large degree on chance events (e.g., storms and certain anthropogenic disturbances) and factors that vary greatly over space and change quickly in time (e.g., shell infestation, sedimentation) and so are difficult to predict. They thus treat each planting as a distinct, trial and error experiment with the objective of evaluating site suitability. They similarly will observe the plantings of others as tests, even though they rarely actively share what they learn with one another, presumably because good planting sites are rare and an underlying objective of much test-planting is to find these sites before others do. Unlike many of the less experienced or less ambitious planters, these entrepreneurs are not easily discouraged by failure. In some cases, thinking another test is called for, they will replant at a later date; in other cases, efforts at particular sites are chalked up to experience and the planters try elsewhere. As noted above, these planters may vary the spacing or the depth of sowing used, either just to see what happens or because they believe that closer spacing and deeper placement may improve seedling survival in the face of certain forms of disturbance (e.g., waves, floating seaweed, rambunctious dogs). Assessing environmental conditions remains, however, at the forefront of most test-planting efforts.

In summary then, Walters's research led to findings about local knowledge that may be of value for mangrove restoration efforts elsewhere. Thus he learned that basic planting practices are straightforward and easy to learn and apply and so should be easy to introduce into novel settings. However, that these practices are being applied in a given community does not mean that there also is a well-developed, widespread and systematic knowledge base there for planting. In fact, it was found that many planters gave little thought to what they were doing and, as a result, demonstrably or possibly important techniques, such as optimal spacing and using waste oil to control shell infestation, were not widely practiced. Walters also learned that environmental events of various kinds often preclude successful planting, regardless of the level of technical knowledge or expertise of the planters. Considerable spatial and temporal variability in the occurrence of such events make it difficult to predict where and when they will constrain planting.

In addition to leading to technical insights about planting, the research also led to valuable discoveries about contextual factors and social processes that influence the spread of

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[13]Persons fishing, gleaning, or passing their boats along the shore often destroy young trees. In most cases, such damage is unintended, but some deliberate destruction of planted seedlings in protest to having common areas planted was also found to have occurred (Walters 1998).

knowledge. It was found, for example, that active sharing of knowledge was uncommon among planters in Bais and that, while this partly reflected the often large and amorphous character of villages there,[14] it was also fostered by the competitive nature of planting in a context where suitable planting sites are scarce. In Bais, knowledge of planting is learned by observing one's kin and neighbors, and through direct planting experience (Walters 2000a). The more knowledgeable planters are typically opportunistic and curious in their approach to planting: most of what they know has been learned from ongoing trial and error and from observing others from a distance. However, with so little active knowledge-sharing and so few opportunities to learn directly from experience (because good planting sites are scarce and because, being fisherfolk, most planters have little prior experience of growing other kinds of trees), it is not surprising that many do their planting without drawing on a substantial knowledge base for doing it. Instead they simply observe and copy the basic planting techniques of kin or neighbors. In line with this, one finds little evidence of a complex and widely shared knowledge system to guide the planting of mangroves in Bais. This is consonant also with the further fact that successful planting depends, to a large degree, on environmental events largely out of the control of individual planters.

In fact, the growing realization that knowledge factors were of only limited relevance to explaining the origins and spread of planting led Walters during the course of his research to enlarge the investigation to consider the influence of other factors already alluded to, including the role of environmental constraints and tenure (Walters 1998, 2000; Vayda and Walters 1999). Only about one-quarter of the nine consecutive months spent in the field was ultimately devoted to investigations of knowledge-related matters. The decision to thus extend investigations beyond knowledge as a subject reflected an appraisal of the relative importance of various factors and also the pragmatic consideration that the factors besides knowledge found to be important for explaining planting success and failure in Bais would need to be taken into account in mangrove planting projects elsewhere.

A STUDY OF LOCAL KNOWLEDGE IN THE NEW GUINEA HIGHLANDS

Is there more of an *a priori* case to be made for holistic studies of cultural systems when knowledge, unlike that in our less opaque Indonesian and Philippine examples, is "embedded" in the special sense referred to at the end of our introduction, i.e., when it is knowledge which local people have gained from their forbears about ways of doing things but without knowledge of why those ways work? Being confronted with such knowledge may well be a challenge to the researcher, but what we wish to emphasize here is that it is a challenge not necessarily to be met by holistic studies of cultural systems.

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[14] In addition to expanding populations of fisherfolk, landless families from surrounding areas now live along the shoreline around Bais Bay. Former village boundaries have been blurred in many areas as a result of increase in the density of houses.

A good illustration, cited by one of us elsewhere (Vayda 1995:227-228) and worth briefly repeating here, is provided by Fringe Enga shifting cultivators in New Guinea's central highlands. Conveyed by them to the geographer Waddell (1972, 1973, 1975) was their knowledge that their sweet potatoes would not grow well unless planted in carefully constructed, large plano-convex mulch mounds, more than a half meter in height and at least three meters in diameter. Why this was a requirement is something which Waddell's Enga informants were "not, strictly speaking, able to say" (Waddell 1972:136). Observations and tests by Waddell (1972:159-161) and others (e.g., Sillitoe 1998b:130-132) have indicated that the mounds, by virtue of the mulch in them, contribute to soil fertility (see also the sources cited on this in Sillitoe 1996:382-383 and 1998b:130). However, at the time of his field research, it was left to Waddell to surmise that the specific construction and design of mounds at the highest altitudes for growing sweet potatoes could be adaptations to protect the crop from frost. He then proceeded to make observations and tests to confirm this possibility. These further investigations by Waddell, far from being holistic studies of cultural systems, involved, *inter alia*, taking temperature readings from unmounded ground and from the upper parts of mounds and making measurements which showed that the height of mounds and the minimum height above ground at which sweet potatoes were planted increased with altitude, which generally correlated with the intensity and frequency of the frost hazard (Waddell 1973:36; 1975:255).

As remarked in Vayda's 1995 article, the thrust of the data obtained by Waddell was to show that the construction and use of mounds were so efficient and precise solutions to the frost problem -- so "well designed," in other words, to deal with it -- as to make it unlikely that mounding by the Fringe Enga afforded protection from frost just by coincidence rather than as a result of such causes as long-ago trial-and-error learning by some, followed by imitation by others. However, no data were (or could be) obtained on the actual cause-and-effect sequences whereby knowledge of mound construction and of its importance for agricultural success became established or "embedded" in Fringe Enga culture (Vayda 1995:228).[15]

There are two points to be made then with an illustration such as Waddell's case. One is that practices supported by opaque professions of knowledge, like those of the Fringe Enga about mounding, may well be important for development, resource management, or conservation and may need to be taken into account in new programs in these areas. The other point is that, with respect to these practices as with respect to some of those considered in the IPM and mangrove-planting cases, studying and explaining the practices so that their practical significance can be recognized and built upon for development or conservation initiatives call for something other

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[15]See Vayda 1995:225-229 for further discussion of how design analysis and comparative studies may be used, in the absence of direct historical evidence, to distinguish those beneficial consequences that are by-products or the effects of chance from those that occur because particular behavioral traits have been fashioned by some selective process, such as trial-and-error, to produce them.

than poorly focussed inquiries thought to be justified by the mantra of holistic study of cultural systems. In our view, the need is for knowing about and testing situation-specific causal possibilities, such as those related to the New Guinea highland frost hazard as well as those referred to earlier.

CONCLUDING REMARKS

The foregoing illustrations should suffice as supports for our arguments against using culture-related considerations, including assumptions of embeddedness in cultural matrices or systems, for the purpose, on the one hand, of delimiting the local knowledge and practice to be deemed appropriate for us to study and, on the other hand, of arguing for long-term, in-depth, holistic socio-cultural studies as a prerequisite for recognizing, explaining, and applying local knowledge. Eschewing such considerations for designating what we must study, not only can we still contribute both to practical action programs such as IPM or mangrove restoration and to theoretically meaningful research on the interrelations of cognition and action but also we can, in our view, contribute better and often more expeditiously. We can do this by virtue of being guided in our research more by open questions about why and with what knowledge do people do what they do than by restrictive questions about how actions and knowledge are affected by factors privileged in advance by us because they are cultural and/or assumed by us to be part of (or embedded in) cultural systems that must be elucidated.[16] We can, in other words, do better work and often faster work by not tying our hands in order to make the work distinctively anthropological.

However, as our case studies should have made clear, our saying this does not constitute an endorsement of "rapid rural appraisal" and similar shortcut, rapid research methods mentioned in our introduction. As stated there, what is obtained and recorded by the methods is too often only background information for the more sharply focused inquiries needed to produce usable evidence for or against particular, situation-specific causal possibilities in the kind of research that we are advocating on the causes of practically relevant actions. We believe that those who have turned to rapid appraisal methods, as much as those still committed to holistic ethnography, can make their research more useful by letting it be guided more, in the ways discussed and illustrated in this article, by clear questions about the causes of concrete actions or events relevant to development and/or conservation concerns.

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[16]It may be noted incidentally that our arguments here are in opposition also to Sahlins's decades-long insistence that the cultural must be the object of the "anthropological project" (see, for example, Sahlins 1976, 1977, 1995).

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