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# Methods and Explanations in the Study of Human Actions and their Environmental Effects

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## FOREWORD

This paper deals with a subject that is of central importance to nature conservation. Sound conservation management requires understanding how people behave under differing sets of incentives and attempts at regulation. This is a particularly pertinent issue in the context of the numerous attempts throughout the tropical developing world to conserve nature through so-called “Integrated Conservation and Development Projects” (ICDPs).

ICDPs have been attempted in several of Indonesia’s more important conservation areas. CIFOR and WWF are collaborating at two of these locations, the Kerinci Seblat National Park in Sumatra and the Kayan Mentarang Nature Reserve in East Kalimantan. Our two organisations are also involved in projects of this sort in other parts of the tropical world.

Andrew P. Vayda examines the philosophical basis of research to understand the behaviour of the natural resource dependent people who are intended beneficiaries of these projects. He analyses the difficult issues of determining causality in attributing conservation gains or losses to different management interventions. He addresses the fundamental issues of whether or not human responses can be predicted in such situations and whether general models can be usefully applied.

CIFOR and WWF Indonesia Programme are pleased to make this paper available as a joint special publication.

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# METHODS AND EXPLANATIONS IN THE STUDY OF HUMAN ACTIONS AND THEIR ENVIRONMENTAL EFFECTS\*

*Andrew P. Vayda*

As someone long concerned with relations between people and their environments, I have often been asked to join conferences organised by ecologists studying plants, animals and ecosystems and recognising a need to deal, at least to some degree, with how these are affected by the activities of human beings. The present article has grown out of my contribution to one of these conferences, the 1991 Cary Conference, intended by its organisers to provide a forum on the question of how human influences can be incorporated better into ecological studies. My answers to this question (e.g., as given in Vayda 1993 in the volume derived from the 1991 conference) are elaborated here with a focus on methodological and explanatory considerations that should be of concern to human ecologists or other social or biological scientists studying human actions and their environmental consequences.

There are both dos and don'ts in my main answers to the question. Thus, I argue 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. I further argue that this calls for (1) making concrete human behaviour and its concrete environmental effects our *primary* objects of study, and (2) tracing (with whatever guidance we can get from existing theories and models) the threads of causal influence upon these outward in space and backward in time.<sup>1</sup> These answers derive from my view that it is appropriate to regard showing causal connec-

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tions and answering why-questions as basic goals in collecting and analysing data, and that knowing how to proceed in these activities is greatly abetted by having such explanatory goals well in mind. In the latter part of this article, some studies, including Indonesian research in which I have been involved, will be drawn on for illustrations of procedures and findings in the kind of approach which I am advocating. First, however, I want to refer to certain other approaches which, unlike mine, accord no priority to proceeding empirically to determine which chains of causally connected events have led to specific actions and their effects. Although I regard these approaches as extremely problematic, they are closer to what many persons, including probably the majority of the biologists that I have met at the aforesaid conferences, expect from someone focusing on human influences on the biological ecologists' objects of study. It should therefore be useful to indicate (and illustrate) in some detail what is problematic about the approaches. Indicating this will involve, *inter alia*, considering how the approaches are deficient for answering why-questions or for dealing with causes.

## **Problems with Emphasising Concepts and Values about the Environment**

A common assumption is that human behaviour affecting the environment is governed by basic conceptualisations or values concerning nature or the environment. An expectation following from this assumption is that social scientists concerned with human influences on the environment will accord priority in their research to identifying these basic conceptualisations or values and showing how behaviour conforms to them. Both the expectation and the assumption behind it are fuelled by arguments which have been presented by some scholars. A well-known example, first put forward in 1967, is Lynn White's view that western cultures, in contrast to eastern ones without roots in Judeo-Christian religion, have an ideal of the conquest and exploitation of nature for human ends and that this is what has produced "our ecologic crisis" (L. White 1967).

More recently, other scholars have been distinguishing what they regard as basic types of interpretation of nature - or "myths," as the ecologist Holling (1978, 1986) has called them - and have been using these to explain such concrete, environment-related actions as a multi-

national household products corporation's introduction of a new toilet-bowl deodoriser or lavatory rim-block in West Germany and the subsequent protest of that introduction by members of the German Green Party (Schwarz and Thompson 1990: 2-10). Social scientists, Schwarz and Thompson, refer to four myths of nature: views of nature as *benign*, always returning to equilibrium no matter what knocks it receives; nature as *ephemeral*, subject to collapse from the least jolt; nature as *perverse/tolerant*, unperturbed by most events but vulnerable in face of certain unusual occurrences; and, finally, nature as *capricious*, operating randomly, thwarting attempts at rational environmental policy making and management, and allowing us only to cope with erratically occurring events (cf. Thompson 1988: 62-68; Thompson *et al.* 1990: Chap. 1). The fact that the multinational corporation failed to anticipate the reaction of the Greens and went ahead with its deodoriser introduction is explained by Schwarz and Thompson (1990: 2-5) by referring to the corporation's adherence to the myth of nature as *perverse/tolerant* while the Greens held to the view of nature as *ephemeral*. According to the latter view, paradichlorobenzene, a waxy material which cements the deodoriser's active ingredients (perfumes and detergents) into a solid block which gradually dissolves as the lavatory is flushed, is dangerous simply because its not having been previously introduced to the water cycle makes for uncertainty about its toxicity and biodegradability. According, however, to the corporation's *nature-as-perverse/tolerant* view, its original product (which, incidentally, was eventually replaced in response to the Green attacks) is innocuous and the Greens' failure to recognise that nature could handle it is irrational.

The foregoing is presented by Schwarz and Thompson not as an illustration of actions which they sought to understand and then determined empirically to be underlain by particular views of nature. Rather they *started* with just the sort of assumptions referred to earlier, namely, that there are basic conceptualisations of nature which are few in number and essentially timeless and that, once identified, they can be used over and over again to make sense, as in the toilet-bowl deodoriser case, of the strategies adopted and actions taken (Schwarz and Thompson 1990: 85).

What's wrong with all this? A few social scientists and perhaps a greater number of physical and biological scientists still subscribe to the old positivist notions that the same, so-called materialist models of explanation used in classical physics and chemistry must be appropri-

ate to all science, including social science, and that, accordingly, ideas cannot be used at all to explain behaviour. In response to this, let me say right off and in intendedly peremptory fashion, that I see approaches of the conceptualisations-of-nature type as problematic not because they involve using ideas to explain behaviour but rather because of *which* ideas they use and *how* they use them (cf. Vayda 1994: 326-327). With respect to the old positivist notions, I forbear here from flogging what seems to me an almost dead horse and instead simply affirm my agreement with the philosopher, John Searle, that it is possible to regard the world as consisting of material particles and of entities composed of material particles and still to recognise that changes in subjective mental states, which are properties of brains, function causally in producing human and animal behaviour (Searle 1984: 25-27 and 1991; cf. Jacob 1995). Other arguments for downplaying ideas and privileging such factors as nutritional, economic and fitness costs and benefits in explanations of environment-related behaviour have been critically examined by me elsewhere (Vayda 1987a and 1987b on Marvin Harris's "cultural materialism" and Vayda 1995a and 1995b on Darwinian ecological anthropology).

About some objections to emphasising basic conceptualisations concerning nature, we cannot be so quickly dismissive as about objections of the positivist-materialist type. A cogent anthropological objection is that it is ethnocentric to assume that conceptualisations of nature in general or of nature as a whole, known to us from our own culture, are to be found among every people. Anthropological accounts indicate that they are to be found among *some* people but possibly not among others. Consider just two examples. On the one hand, Koyukon Indians of north-western interior Alaska are said to conceive of nature as conscious and consisting of entities with various degrees of spiritual power; all these entities, in the Koyukon view, must be shown respect (Nelson 1983: Chap. 12 and *passim*). By contrast, Ponam Islanders, a maritime people of Manus Province in Papua New Guinea, are said not to have any unitary conception of nature; instead they see different species in their environment as existing more or less independently and, in the case of fish and shellfish, being simply "things to be caught, eaten, or traded" (Carrier 1982 and 1987: 150-156).

Problems of causation must also be considered. Even when basic conceptualisations or values concerning nature are shown to exist among a people, their efficacy in causing actions with significant environmental consequences remains a question. That they seldom affect

"more than a fraction of the total range of environmental behaviour" was stated by the geographer Yi-Fu Tuan (1968: 177) some years ago in commenting on Lynn White's thesis about the Judeo-Christian tradition as a source of environmental spoliation.<sup>2</sup> The main consideration on which Tuan's conclusion is based is that our actions are often guided not by our ideals or very broad conceptualisations but rather by ideas about achieving more specific or more immediate objectives. Thus Tuan notes that ideas of quiescence towards nature did not keep Chinese from deforesting vast areas as trees were burned to deprive dangerous animals of their hiding places and were felled for such purposes as making charcoal and obtaining the timber which was the predominant building material of old Chinese cities (Tuan 1968: 187-188 and 1970: 248).

Reference may be made here also to some conclusions emerging from studies by me and others concerning migration and land use by Indonesia's Bugis people (Lineton 1975a, 1975b; Vayda 1980; Vayda and Sahur 1985). Acknowledged as widely held and long persisting among Bugis - and conducive to their depleting the resources of a place and then moving on - are adventurousness and mobility as values or ideals, along with a belief in opportunities beyond one's village of residence. Indeed an ideal type of person has been described by some Bugis as one who stays and works in a place long enough to harvest the good fortune offered by the place and then goes elsewhere (in contrast both to itinerants or perpetual wanderers and to rooted or permanently settled persons) (Acciaioli 1989: 282-284). These Bugis ideas are more about relating to particular locations than to nature as a whole, but, with respect to environmental spoliation, the effect of acting on the ideas could be like acting on the alleged Judeo-Christian ideal of the conquest and exploitation of nature for human ends. The important point to be made in the present context, however, is that while some Bugis were roving the seas and setting up or joining impermanent trading and cash-crop farming colonies throughout the Indonesian archipelago, most Bugis led predominantly settled, peasant lives devoted to using their environment for rice-growing and fishing. However widespread and persisting the ideals and beliefs related to mobility may be, they have little efficacy - and, accordingly, little analytic or explanatory significance - with respect to a great deal of environment-related Bugis behaviour (cf. Vayda 1994: 328, note 4).

Another point to be made is that different ideas may be associated under different circumstances or at different times with repending to

the same or similar environmental events. For example, in a research project in which I took part in 1990, it was found that some Central Javanese farmers, in the early stages of rice-field infestation by insect pests, acted on the basis of practical ideas about chemical control and then, as infestation became more severe and the measures taken proved unavailing, switched to a course of inaction, recommended to them by resuscitated, old Javanese ideas that the pests in outbreaks were brought forth from her realm by Nyai Loro Kidul, the goddess of the Southern Ocean, and that one could do nothing but wait for the pests to leave after satiating themselves on the crop (Vayda and Setyawati 1995: 263-264). White, like other essentialist thinkers, may be committing the fallacy of assuming that ideas or ideals which are widespread and/or persistent are *ipso facto* efficacious. They may of course be efficacious at times, but it must be regarded an empirical question whether they, rather than some more idiosyncratic or transient ideas, are the basis on which some particular actions are taken at particular times or in particular contexts (Vayda 1994: 326; for further discussion of context-dependent variability in whether or not particular ideas or cognitive attitudes are efficacious in causing or guiding actions, see Bratman 1992 and Thomason 1987).<sup>3</sup>

"Beside the more glaring contradictions of professed ideal and actual practice, there exist also the unsuspected ironies," says Tuan in his 1968 article (p. 188), and his remark serves to introduce a remaining important point to be made against emphasising ideals or values about nature or the environment. The point is that many - perhaps most - of the environmental consequences of human actions are unintended. Possible examples are countless. For purposes of illustration, let me refer to just a few.

My first set of examples is drawn from an article by Richard Mack in the March 1990 *Natural History* on weed dispersal through the agency of the 19th century mail-order seed business in the United States. Mack notes that the typical customers at whom seed catalogues were directed in the early 1800s were self-reliant farmers, growing plants for food (for themselves and their livestock) as well as for medications and for fibre for home-made clothes. However, in the boom decades after the Civil War, seed catalogues increasingly offered ornamental plants as more and more Americans no longer grew their own food and had more leisure time and discretionary income for lawns and ornamental gardens and for such hobbies as collecting dried and pressed botanical specimens. Using elaborately illustrated, multim-

coloured catalogues, hundreds of pages thick, seed merchants view with one another to offer the most diverse and exotic plants. These included what have become some of the most persistent weeds in many parts of the United States, for example, baby's breath, Japanese honeysuckle, bachelor's button and the water hyacinth.

Consider just a few details about the last-named, which is indigenous to the Amazon Basin and attracted considerable interest at the 1884 International Cotton Exhibition in New Orleans. By the early 1890s, nursery owners, lavishly praising the water hyacinth's easy care and its beauty in ponds, were selling it through their mail-order catalogues for as much as a dollar a plant, equivalent to about eleven dollars in today's prices. Neither the merchants nor the customers knew at this time about the plant's ability to run wild outside its home range in South America, where natural enemies kept it in check. Its floatability meant that currents could carry it miles downstream. And when, for example, caught in the paddle wheels of a river steamer, it could travel long distances upstream too. Since each plant can replicate itself hundreds of times each year, the spread of water hyacinth was spectacular. In just a few years, many navigable rivers were reduced to streams by "impenetrable tangles of living and dead plants" (Mack 1990: 49), which, moreover, became breeding grounds in some regions for the insect vectors of malaria and encephalitis. The plant is still a serious problem today in parts of the south-eastern United States. In seeking satisfactory explanations of why all this happened, we must look first not to the basic values or ideals which seed-sellers and seed-buyers held about nature but rather to the contexts in which they acted, the intentions and knowledge (or lack of knowledge) with which they did so, and the reproductive and other characteristics of the water hyacinth itself.

At this point, I want to give just one other example of unintended environmental disruption. The example is the greater decline of warblers, wrens, nightingales and other farmland birds in Britain than in France in recent decades. The factors which research has pointed to as causes include differences in national agricultural policies whereby there were incentives in Britain and not in France for the consolidation of farms and the attendant destruction of the hedgerows which are favoured songbird habitats (Mills 1983).

In addition to unintended effects that seem to be environment-disrupting, there often are what seem to be environment-*maintaining* or conservational ones. Thus French farmers are reported to tend their

hedgerows not for the sake of bird conservation but for having sources of firewood, boundaries around their land, and windbreaks to prevent erosion (Mills 1983; cf. Grosrichard 1995). Such unintended conservation effects are to be found in the pre-modern as well as the modern world. For example, Bulmer (1982), in an excellent review of alleged "traditional conservation practices" in Papua New Guinea, found no evidence of conservation ideologies behind the practices. Rather he found that the traditional practices which had conservation effects were performed in pursuit of short-term yields. Thus it may be good conservational practice for shifting cultivators to let the land lie fallow for 15 or 25 or 40 years. But, notes Bulmer, the object for Papua New Guinea shifting cultivators was not conservation. It was getting that "next good crop for a reasonable minimum of effort." The cultivator knew from experience the approximate fallow period required both for an undiminished crop and for less effort than would be entailed if older secondary growth were to be hacked down. Similarly Bulmer (1982: 63) notes that: "If intensive hunting, trapping or fishing for certain species was concentrated in certain brief periods of the year, that probably was good for the survival of the birds and mammals and the fishes, but the reason for these practices was surely that it was a convenient and efficient way to get good catches."

From Bulmer's review, as well as from other research which has resulted in similar findings (e.g., Hames 1987; Alvard 1993, 1994), I draw support then for assigning priority in research not to basic values or ideals about nature but rather to concrete human actions, the intentions behind them, and their concrete environmental effects. However, unlike some of those who have done this research (e.g., Hames 1987; Alvard 1994) as well as some others (e.g., Low and Heinen 1993; Ridley and Low 1993), I do not accept the ostensibly Darwinian view that the evolved fitness-maximising nature of human beings predisposes them to act, as a rule, for short-term gain rather than long-term conservation from which they may gain no benefits. Why I do not accept views like this has been discussed at length elsewhere (Vayda 1995a, 1995b). Suffice it to say here that, as again I have said elsewhere (Vayda 1995a: 223) and as others (e.g., Elster 1989b) have documented, people may switch back and forth between actions based largely on calculation of advantages for themselves and actions based more on compliance with norms or ideals at odds with self-interest. Indeed the very capacity for switching may be reasonably regarded as a component of our evolved nature, while the fact of switching, by

countering both the ostensibly Darwinian view and essentialist, idealist views like Lynn White's, argues in favour of focusing first on actions and their effects and the proceeding empirically to determine the chains of causally connected events (including mental events) that have led to them.

## **Problems with Studying Humans as Components of Pre-defined Systems**

In the second problematic approach that I want to discuss, the study of relations or interactions between people and their environments is made a part of the study of pre-defined units or systems within which the relations or interactions are seen, or assumed, to be occurring. These units or systems are "pre-defined" insofar as their status as entities is already established or assumed and/or their boundaries are already demarcated before the research actually begins. In this and the next sections, I incorporate material not only from the 1993 article derived from the Cary Conference but also from earlier articles of mine (Vayda 1983, 1986, 1988).

First I want to refer to studies in which ecosystems are demarcated and the behaviour of human beings within them is then investigated with a view to specifying how it contributes to the self-regulation of the ecosystems and thereby to their stability or persistence. Without repeating in detail the criticisms that I have made elsewhere (Vayda 1986) concerning the ontological assumptions underlying such studies, let me simply note that by stipulating the ecosystem as an enduring entity whose components behave so as to maintain it, these studies set themselves apart from some other, less tendentious and more empirically oriented studies in which no pre-judgements are made about ecosystemic regulation or persistence. In these latter studies, humans are regarded as ecosystem components only or primarily in the sense of being organisms whose locale-specific interactions with organisms of other species and with things in the abiotic environment are included among the objects of study by ecosystem investigators. Later in this section, more will be said about some such studies and the rationale for them.

The study that I will discuss first here has become a classic of ecological anthropology: Roy Rappaport's *Pigs for the Ancestors* (1968). The boundaries of the ecosystems with which Rappaport is concerned

are taken by him to correspond to the territories of individual local groups of Maring-speaking people living on the fringe of Papua New Guinea's highlands - for example, the 8.3 square kilometers of territory of the approximately 200 persons comprising the Tsembaga local group with whom Rappaport resided during his research in 1962-63. His concern with ecosystemic self-regulations, i.e., with respect to maintaining within certain ranges such variables as the size and composition of both human and pig populations in the ecosystem, is made explicit at the beginning of his book (p.4). And I know from having been in the field with him that he assumed from the start that the ritual actions he was observing had roles in ecosystemic self-regulation. Indeed he became interested in the ritual actions precisely because of this assumption. Examples of the kind of claims made in his book are (1) that Maring local groups initiate certain ceremonies when their relation to their pigs has changed from one of co-operation or commensalism to competitive relation, and (2) that the initiation of these ceremonies and their culmination in massive slaughters of pigs, to be offered as thanks and rewards to ancestor spirits and allies for their help in earlier warfare, are to be regarded as systemic (or ecosystemic self-regulation). Although I was at one time sympathetic to Rappaport's interpretations and indeed wrote the foreword to the original edition of his book in 1968, I made a number of critical points in my review (Vayda 1986) of the book's enlarged 1984 edition. Two of these are appropriate to note here.

The first point is that Rappaport (1984: 351, 374, 401), while referring to the responses that units at "virtually all levels" make to perturbations, gives us no adequate criteria for what constitutes behaviour or responses not simply of human beings and other organisms but also of higher-level units in which human beings and other organisms are included, that is, such units as ecosystems, populations and societies. What are actually observed by us as ecological anthropologists or human ecologists are indeed the behaviour of human beings and their interactions with other organisms or other specific components of their environment - in the Maring case, such behaviour as shooting pigs invading and damaging gardens; fighting with the owners of such pigs; increasingly harvesting sweet potatoes especially for pigs instead of feeding them with garbage and with sub-standard tubers found in the harvests for humans; and agitating for the inception of the ceremonies culminating in pig-slaughters (Rappaport 1968: 156-165). If some of the observed behaviour is to be regarded as, for example, ecosystemic

functioning or ecosystemic response, we need to have defensible rules or criteria for so regarding it.

Consider the behaviour given as examples above, along with the behaviour of actually initiating the ceremonies and of ending them with the pig-slaughters. Can all this behaviour be described and explained not simply as human behaviour but rather as constituting mechanisms whereby the ecosystem regulated itself to keep pig numbers within its carrying capacity? Rappaport originally suggested that it can, notwithstanding that, in the 1984 edition of his book, he acknowledged his failure to have actually shown that the triggering of the ceremonies depended on how close the pig population was to carrying capacity limits (Rappaport 1984: 406). Originally, however, he assumed that the behaviour in question, by virtue of its recurrent role in reducing the size of the pig population when (according to Rappaport) it was threatening to become too big, contributed to something definable as the persistence of the ecosystem. And he made a second assumption: that this indicated ecosystemic response or self-regulation - that, if the ecosystem persists *because* of what happens, then it must be making it happen. Even if we ignore the failure eventually acknowledged by Rappaport and grant him the first of his assumptions, a point that must still be made about the second one is that any contribution made by the behaviour in question to ecosystemic persistence could have happened fortuitously rather than as a result of teleological control by the ecosystem. The mere fact of the contribution is thus no warrant for regarding the ecosystem as the agent of the behaviour or being capable of agency and of causing or orienting behaviour so as to regulate and thereby benefit itself (see Williams 1966: Chap. I and Richerson 1977 for fuller discussion of similar points; the discussion of "naive functionalism" in Elster 1986, Schwartz 1993, Vayda 1995a and my next section here is also relevant).<sup>4</sup>

Are there some other bases for regarding the behaviour as ecosystemic function or response? This question leads to my second (and related) point: the difference between being, by virtue of location, included in a unit and being regulated or controlled by it with respect to such matters as growth, reproduction and behavioural interactions with other species. In the "Epilogue" of the 1984 edition of *pigs for the Ancestors*, Rappaport concedes that organisms of different species included in ecosystems may have come together accidentally and may have initially not been subject to strong systemic controls. However,

he assumes that the coexistence of the species (including human beings) proceeds under stable conditions and that the constitutions of their ecosystems are accordingly "likely to become increasingly elaborate and coercive through time" (p. 375). By such unjustified assumptions, simply being included in ecosystems implies, to a greater or lesser degree, being controlled by them.<sup>5</sup> Ignored in this view is the generality of disquilibrating processes whereby inter-species articulations are currently sundered (Pickett 1980; Wiens 1984). Ignored too are the special problems posed by the mobility of many species. Elton, writing more than sixty-five years ago in reaction to the notion that animal communities operate like clockwork, expressed the problems figuratively by saying that the clockwork idea could be sustained only if were to accord each cogwheel "the right to arise and migrate and settle down in another clock" (Elton 1930: 17). A more recent statement is the following by Wiens (1984: 453-454) about difficulties resulting from mobility characteristics of birds:

These characteristics produce a substantial flux of individuals in local populations and spread the sources of biological limitation of populations, and the consequent determination of community structuring, over a large and undefined area. Because most local assemblages of birds contain mixtures of species that differ in migratory tendencies and pathways and in longevity and fecundity, the dynamics of any given local assemblage are likely to be driven by an amorphous complex of factors, the effects of which are likely to be different for almost every local assemblage. This uncertainty decreases the probability that any "patterns" that seems apparent are in fact real.

The disarticulating processes and mobility characteristics referred to in current debates among biologists raise questions about the utility of higher-level units simply as analytic concepts, regardless of whether or not they are regarded as entities with teleological properties. If human beings, with their mobility and wide-ranging and flexible use of resources are included in the units, the questions are magnified for us.<sup>6</sup> In accord with these questions, the ontological claims still made by some for the units (for example, besides Rappaport, by Golley 1993, especially pp. 191 ff.) have, it should be noted, been given up by most biological ecologists (e.g., Colinvaux 1976: 264-265; Simberloff 1980) and also by many ecological anthropologists (e.g., Orlove 1980:

248-249; Moran 1984: 21). This means that "the existence, importance, looseness, transience, and contingency of interactions" among species can now be key questions even for those calling themselves community ecologists (Abele *et.al* 1984: vii). Rappaport is impervious to these developments in ecology and rejects as "rather muddled" and "mistaken" (Rappaport 1984: 373, 1990: 43) the arguments which McCay and I previously set forward for questioning the status of the ecosystem as an objectively real entity and for emphasising instead its status as a concept put to use by ecologists in designating and analysing interactions of organisms of different species living together in certain, restricted spaces (Vayda and McCay 1975: 299-300).

Yes even if Rappaport is now in small minority among ecological anthropologists in making his ontological claims on behalf of ecosystems and similar "higher-level" units, there may be greater number of ecological anthropologists who, while disavowing notions of ecosystemic response or ecosystemic agency as well as other reifications to be found in Rappaport's work, still retain a methodological bias in favour of studying bounded systems or having definite, stable units of study. No doubt there are good historical reasons for such unit-mindedness. It may, for example, be something persisting from a world that could be regarded as more stable, perhaps the world of 19th century science in which the specificity, integrity and separateness of phenomena constituted, as Thomas (1975: 126) has suggested, a fundamental myth. But whatever the historical reasons for the unit-mindedness, the point is that it may, at the present time, hold back rather than help getting answers to research questions about why things have occurred and how they are connected. This point, together with the wide persistence of unit-mindedness, is a good reason for highlighting the study of pre-defined systems as a problematic approach for gaining understanding of people's interactions with components of their environments.

A good illustration is provided by Roy Ellen (1982), who, while rejecting *a priori* assumptions about the closure and stability of systems, still adheres to the view that the way to understand and explain relationships or interactions between people and components of their environment lies in thorough studies of some larger systems which has its boundaries demarcated before the start of the research and which presumably contains the people and/or environmental components of interest. As will be further discussed shortly, an alternative to this is proceeding empirically to situate people-environment interactions

within complexes of causes and effects. In other words, this alternative consists of placing the interactions into context without antecedent stipulation or demarcation of that context. It is an alternative which Ellen does not explicitly consider. He does acknowledge that describing an ecosystem holistically, which calls for measuring and describing how energy and materials flow throughout the whole system, is a formidable task (1982: 89, 93, 113-115) and "can make extraordinary demands on the time, care, patience and skill of the investigator and his back-up facilities" (1982: 113). However, except for noting that there are practical limits to the flows that ecological anthropologists can deal with (1982: 106-107), he expresses no qualms about the undertaking and devoted many pages of his 1982 book to methods and procedures in ecosystem research. By contrast, there is very little in his book about any answers which such research has provided to significant questions in anthropology or human ecology. And when Ellen makes suggestions about questions *which* might be answered (for example, in his chapter 5 [1982: 121-122], questions about the formation, size and maintenance of work groups), it is by no means clear that answers could not be provided more expeditiously and completely by inquiries much more focused - and, at the same time, much less restricted in the pursuit of causes and effects, through space and time - than the holistic study of ecosystems. In truth, no convincing reasons for such holistic study are given.

Further illustrations of problematic unit-mindedness may be drawn from the first decade in the history of the Man and the Biosphere (MAB) programme, which UNESCO launched in 1971 as an international research and training programme for providing information and methods for better resource management and land use. The programme had its roots in the previous decade's International Biological Program studies, in which ecosystems were the units of research. Early in the history of MAB, however, it was realised that the continuing use of these units might impede rather than facilitate research contributing to improved management of natural resources and better or more sustainable relationships between people and their environments. The main perceived difficulty, as expressed by di Castri (1976: 245), was that "human uses of the environment are not confined within ecosystems." But researchers and research planners were not - although they should have been - led by this realisation to rid themselves of the kind of methodological straitjacket which Ellen accepts. Instead, they concluded that if ecosystems were not to be their research

units, then other identifiable units had to be found. A 1974 MAB task force put forward the concepts of the "human use system" (UNESCO 1974), which was seized upon as the unit needed to meet the supposed difficulty. Agreement could not, however, be reached about whether these purported systems "through and by which resources are managed" (di Castri 1976: 245) are such units as households, tribes, nation-states and multinational corporations (UNESCO 1974: 10) or such spatial units as villages, watersheds, islands and archipelagos (di Castri, 1976: 245; di Castri et.al. 1981:56; Brookfield 1984:145, 157). Although I argued at MAB meetings in the 1980s for resolving the research unit questions by cutting the Gordian knot and doing without definite, stable units of research ( see, for example, UNESCO 1983:33-36), this is an alternative that does not appear to have been considered by most MAB researchers and research planners during MAB's first two decades.

Like Ellen and those in MAB during the 1970s, Rappaport does not explicitly consider the alternative of proceeding empirically to put people-environmental interactions into context without requiring antecedent stipulation or demarcation of what that context is. Instead, the spectre that is raised for him should there be failure to hold fast to his view of the ecosystem is that we will that confined to "the study of decontextualised interactions between humans...and one or a very few nonhuman elements in their surroundings" (Rappaport 1984:380 1990:51). Because of this spectre, it is appropriate to reiterate at this point that the problematic approach which I am warning against in this section entails making the study of relations or interactions between people and their environments a part of the study of units or systems whose boundaries are defined or demarcated in *advance* of the research. Eschewing this approach hardly confines us to studying decontextualised interactions. On the contrary, it leaves us free to start with some particular interactions and then, by showing the connections of these to other phenomena, to end up studying quite extensive complexes of causally connected phenomena. In order words, what we study can grow through being studied and can do so without ever having a unit of study corresponding to a culture or society or ecosystem or similar pre-defined whole (Vayda *et al.* 1990, cited in Vayda 1992: 303).<sup>7</sup> It may even be warranted to use the label of "system" for these complexes of connected phenomena, although my own preference is, as a rule, to avoid the term because of the various tendentious meanings with

which it has been saddled (cf. Taylor and Garcia-Barrios 1995: 7 on "loose" and "strong" senses of "system").

Rappaport's spectre makes it appropriate to mention also that there are studies which focus on particular environment-related interactions or activities and then not only show linkages of these but also, in showing the linkages, lead to the empirical discovery that some observed human behaviour does in fact contribute to the persistence of an extensive complex of interconnected human behavioural, non human biological, and physical phenomena. In other words, the discovery is that the behaviour has something like the regulatory functions attributed by Rappaport to Maring rituals but with respect to a complex of phenomena whose interconnections are empirically established rather than, as in Rappaport's study, being assumed *a priori* to correspond to a pre-defined ecosystem. Examples of such regulatory behaviour were found by Lansing in Bali in his studies of interconnections of certain features of agricultural production, the flow of water for irrigation and the control of agricultural pests. The behaviour which Lansing found to contribute to connecting and maintaining these phenomena over wide areas includes practices of water-allocation by the priests of so-called water temples and the co-ordination of the planting schedules of separate *subaks* (local-level irrigation associations, each comprised of about 100 farmers working 100 hectares on average) by means of their participation in annual meetings at which multi-*subak* water-temple congregations decide what and when to plant (Lansing 1987, 1991 and Lansing and Karemer 1993; cf. Gronhaug 1987: 89-94 on inter-linked water-allocation activities along a centuries-old 40-km irrigation canal in western Afghanistan).

## **Actions and Consequences as Object of Study**

Turning now to my preferred approach, namely, starting with concrete human actions and/or their concrete environmental consequences as our object of study and then tracing the threads of causal influence upon these outward in space and backward in time, I want first to give examples from research project that I directed from 1979 to 1984 in the Indonesian province of East Kalimantan on the Island of Borneo.

A general goal in these projects was to gain understanding of the forces contributing to deforestation in the province, and an early move that we made to meet the goal was based on what has been called a

*conceptual* version of methodological individualism (Dray 1980: 62-65). Some consideration of this version is appropriate here for the purpose of elucidating the relation between general research goals and primary objects of study. The consideration will, however, be kept brief in order to avoid getting entangled in the various ontological, methodological, ethical, ideological and epistemological issues that the so-called holism/individualism controversy has generated in social science and philosophy (cf. Lukes 1973; O'Neil 1973; Birnbaum and Leca 1990).

In unqualified, doctrinaire form, the claim of the conceptual version is that *all* concept or terms referring to groups or aggregates can be translated and thus, in principle, be replaced by concepts or terms referring to individuals. My preference has been to avoid disputation about the sweeping nature of this and instead, on pragmatic grounds, to make it a rule to try translating any aggregate concepts or terms in which a research goal has been formulated into "individual" concepts or terms and to ask whether such translation facilitates data collection and analysis in line with the goal.<sup>8</sup> A rationale for following this rule relates to the Humean principle of *contiguous causation*, meaning that causes can act only on what is contiguous to them in space and time and that so-called remote causes of events are impossible unless linked to the events by intermediate and contiguous causes and effects (Hume 1978: 75). An analytic or explanatory ideal that follows from this is that of identifying continuous chains of causes and effects. As Elster (1983: 28-29; 1985: 5; 1989b: 3-7) has suggested, moving from more to less aggregate levels of phenomena in our analyses and explanations can get us closer to this ideal - even if we do not, in practice, achieve it - and away from spurious explanations, such as those produced by mistaking mere correlation for causation (cf. Beauchamp and Rosenberg 1981: 172-189; Levine *et al.* 1987: 79-83). Thus, if we were indeed to find more resource depletion and environmental destruction in the aggregate by those who are heirs to the Judeo-Christian tradition than by others, it might be thought by some that such a correlation confirms Lynn White's view that the explanation of the environment-related actions of those heirs lies in beliefs that are part of their tradition. However, a causal inquiry starting with the actions themselves and heeding the Humean principle might well show that the ideas usually giving rise to the actions in particular situations are, as suggested earlier, considerations of possible economic advantage or other matters without a clear or exclusive connection to the

beliefs which White regards as coming from the Judeo-Christian tradition and as continuing to have general causal efficacy.<sup>9</sup>

A good illustration of progress towards attaining a research goal as a result of adopting my methodological rule is provided by Sen's well-known famine studies (1981), even though Sen does not explicitly refer to his following a rule applicable not only in famine studies but perhaps much more widely. His general goal was to explain famines, and he found that he could better analyse and explain famines by, first of all, not conceptualising them as severe food shortages for a society as a whole (notwithstanding the some correlation between such shortages and the occurrence of famine could be found), but rather conceptualising them as the starvation of individuals and by then asking who the starving persons were and what changes had occurred, either uniformly or variously, in their situations to make them starve while others in the same society still had enough to eat. One of the famines analysed by Sen, the Great Bengal Famine of 1943 in which about three million people died, was found in fact to have occurred during a period characterised by no clear overall food shortage but rather by a wartime economic boom in which the wages and incomes of landless rural workers, artisans, fishermen and others who became famine victims did not keep pace with the inflated prices of the staple grains.

Just as Sen found that he could better analyse and explain famines by conceptualising them as the starvation of individuals, we found in our East Kalimantan research that we could better analyse and explain deforestation by conceptualising it, at least in part if not entirely, as the felling of trees by individuals.<sup>10</sup> Guided by this conceptualisation, we made our initial objects of study such actions of individuals as cutting down trees and such effects as the actions as gaps and clearings in the forest. These effects were of special interest to us because, partly as a result of collaborations with Steward Pickett and other ecologists (cf. Pickett and White 1985), we were aware that gap or clearing size could be very important with respect to the ability of plants of particular woody species to re-establish themselves.<sup>11</sup> This could be so because the chances for seeds from trees around the sites of clearings to be carried by winds to the site centres would become fewer as the distance between the centres and peripheries became greater, and also because the fires set by shifting cultivators to prepare sites for agricultural use would be more intense in larger clearings and therefore more effective in killing coppices and seeds in the soil (see Kartawinata *et al.* 1984: 91). Finding considerable variation in gap or clearing size resulting

from human actions, we then looked more closely at the actions and their determinants in order to try to account for variations in the effects. In our research among Dayak shifting or slash-and-burn cultivators in the interior of Kalimantan, this involved us in looking at not only such contextual factors as topographic, technological and manpower constraints on the people's ability to make the clearings larger (Kartawinata et al. 1984: 91), but also such intentions as making the farm plots or swiddens of one's own household adjacent to those of neighbours; such purposes behind the intentions as having neighbours to lighten the burdens of trail maintenance and swidden fencing; and such beliefs or knowledge behind the intentions as that having one's own swidden as part of a cluster constituting a larger clearing will facilitate the burning of felled trees and slash and the protection of crops from monkeys and other pests from the forest (Mackie et al. 1987: 482-485; on distinctions among beliefs, ideas, purposes and intentions in explanations of actions, see A White 1967: Chap. 6 and Davidson 1980: Chap. 5).

It is of course conceivable that somewhere some shifting cultivators will be shown by research to deliberately keep their clearing small because of benefits to be gained by hastening the reversion of fields to forests, which are valued by the cultivators not only as future swidden sites but also as sources of game, firewood, building materials, and wild medicinal and food plants and as foraging areas for such domesticated animals as pigs. A point to be emphasised here is that we must indeed try to trace the threads of causal influence and that it would be unacceptable to explain the behaviour of any shifting cultivators by referring simply or directly to the generalisations relating forest recovery and gap size and to our own knowledge of benefits to be gained from forest recovery. Such explanations are the explanations of what has been called "naive functionalism" (e.g., in Elster 1986, Schwartz 1993 and Vayda 1995a), whereby specifying some beneficial consequences is tacitly regarded as sufficient for an explanation, with no allowance having to be made for the possibility that the benefits in question, like those cited earlier from Bulmer's discussion of conservationally sound traditional practises in New Guinea (above, p 8.), are the by-products of behaviour not caused, either now or in the past, by the benefits themselves (cf. Vayda 1987a: 489-507 and 1987b: 520). Tracing the threads of causal influence can enable us either to provide support for the by-product possibility or to dispose of it. In the hypothetical case at hand, we might dispose of it by somehow obtaining

evidence to show, for example, the following: (1) that hastening forest recovery is actually the reason held and given by the cultivators themselves for keeping their clearing small even in the absence of topographic, technological and manpower constraints on clearing size; and (2) that this reason was based upon their awareness or expectation of benefits to be gained from forest recovery as well as upon the knowledge that they had empirically gained over the years about the relation between such recovery and clearing size.

In our East Kalimantan research, not only did we not find such intentional limitation of the size of clearings but, on the contrary, we found that clearing size substantially increased when topographic and technological constraints were reduced and when incentives for surplus production appeared in the form of commercial markets for rice. Thus, Dayak farmers who had migrated from the steep and brokenlands of the interior to a lowland area of plains and low hills were using chain-saws to make clearings as large as 300 hectares - many times larger than the clearings of their homelands - to grow rice not only for their own consumption but also for sale to riverboat traders and local merchants. In their remote homelands, commercial rice-buyers had been absent and chain-saws had been prohibitively expensive to use, mainly because of the cost of obtaining fuel by means of air transport. We also found in the lowlands that the large clearings were indeed not reverting quickly to forest. Some years after being farmed, the sites of the clearings had mainly grasses, climbers and ferns (which have light, readily wind-borne seeds) and not many tree species (which generally have heavier seeds and consequently shorter ranges of dispersal) (Kartawinata and Vayda 1984: 108-109; Kartawinata *et al.* 1984: 91).

In the foregoing illustrations, research proceeded from such environmental effects as various-sized clearings in the forests to finding out more about the actions leading to them and about the ideas behind the actions. It is, however, worth noting another possibility: that of having findings about behaviour as guides in deciding which studies concerning consequences should be pursued. Thus, findings in East Kalimantan about the preference of shifting cultivators for making swiddens in previously farmed sites in secondary forest rather than in primary forest sites which are more difficult to clear have been cues for research on the botanical effects of recurrent felling and burning. The research so far, involving comparisons of plant successions on sites varying in how far back in time histories of being re-cultivated

extend, suggests that such cyclical disturbance as are associated with so-called traditional shifting cultivation favour certain types of trees, namely, vigorous re-sprouters and pioneer, light-demanding trees which produce seeds abundantly (Mackie 1986; Mackie *et al.* 1987). Plans are being made now for complementary research on human actions that are interesting to us precisely because, while affecting forest species, they occur in less regular or cyclical fashion. Examples of such actions include the collections of certain non-timber forest products, like rattan and *gaharu* (aloes) wood, in response not to local needs but rather to commercial demand, which varies unpredictably and, when high, induces collectors to forage widely and erratically through the forests in search of the products (cf. Jessup and Peluso 1986).

In tracing the threads of causal influence, we sought to obtain data also on the origins, determinants or reinforcers of some of the ideas that we found to lie behind Dayak shifting cultivators' actions - for example, such previously mentioned ideas as that having one's own swiddens as part of cluster constituting a larger clearing will facilitate the burning of felled trees and slash and the protection of crops from monkeys and other pests from the forest (cf. Ruyle 1973 on causes of the "differential replication of ideas" and Sperber 1985 on the "epidemiology" of mental representation). Historical data on how and why such ideas originated and spread were hard to come by. However, in the case of our research among Bugis migrants who had cleared East Kalimantan forests in order to establish pepper plantations, historical accounts did enable us to say more about the sources of certain ideas affecting Bugis actions. For example, we were able to suggest not only that some widely held and long persisting Bugis values and beliefs about mobility and migration were factors in how the migrants used their environments but also that those values and beliefs, referred to earlier, had been promoted or reinforced by geographically extensive social networks along which information about opportunities in other lands could flow back to whatever potential migrants there were, at particular times and for particular reasons, in Bugis settlements in the South Sulawesi homelands or elsewhere. Further, we were able to identify some of historical events whereby the networks had been established, maintained or changed. Tracking these events took us far back in time: as far back, for example, as the late seventeenth century when a bugis diaspora occurred after Dutch annexation of Makassar (Ujung Pandang) in South Sulawesi - and

even further back to the early sixteenth century when Makasar first became the entrepot for species from the Moluccas and for sandalwood from the islands of Timor and Solor and when, concomitantly, Bugis *perahus* first found a place in the extensive commerce of the eastern seas (Vayda 1980: 80, citing various historical sources).

The fact that tracing the threads of causal influence took us as far back as the early sixteenth century in the case of our Bugis research points to another questions sometimes raised with respect to the kind of approach that I am advocating. This is the question of how far one should go in tracing the threads. For historians whose explanations consist of citing chains of causally connected events, the question is a familiar one. In fact, as Dray (1986: 25) has noted, argumentation by historians about how far back in time should causal explanations be pushed has been considerable and has featured lively debates over such issues as "whether the causes of the English Revolution or Civil War were long or short term" (cf. M. White 1965: 142ff.)

Similar arguments have concerned pushing outwards in space for causes of local events. An illustration of pushing outwards is the already cited case of finding local hedgerow destruction and consequent local disappearances of songbirds in Britain to be causally linked with national agricultural policy making. In our East Kalimantan studies, we sometimes pushed far outwards to explain local events and sometimes did not. Thus, in dealing with booms in the collection of such forest product as *gaharu* and rattan in the interior of Borneo, we went beyond not only the boundaries of the island but also the boundaries of Indonesia and Southeast Asia as we found demand from Hong Kong, the Middle East, Japan, North America and Western Europe to be factors (cf. Yamada 1995). However, we stayed within the boundaries of a single Dayak village when our objects of explanation were the villagers' ongoing collection of forest products for local use in building, cooking and medicines (Vayda 1993:70).

Among some social scientists, the arguments about pushing outwards have particularly concerned the issue of whether explanations must include the causal influence of certain types of factors, privileged as explanantia or explanatory factors by scholars of particular theoretical persuasions, including Marxist and world-systems theorists. Examples of the types of factors that have been privileged are economic ones involving exploitation of local lands and peoples for distant markets and for the profit of extra-local capitalists, and political factors, often involving policy-setting and other actions by the state or

by either public or private actors supported by state power (see the statements on "political ecology" in Hjort 1982; Schmink and Wood 1987; Bassett 1988; Bryant 1992; Peluso 1992 and Hershkovitz 1993). An illustrative debate here is that between Sen (1981) and some who have criticised him for focusing too narrowly on changes in the means or entitlements whereby those who became famine victims had formerly obtained food. According to the critics, Sen has failed to extend his causal explanations outwards to certain political factors which, having led to the changes in entitlements, are the "real" causes of famines (Kula 1988; see also Rangasami 1985 and Reyna 1991).

In my own answer to the question posed, I emphasise certain practical considerations. These considerations are, however, supported by a view of events not simply as the products of causal chains but as (at least sometimes and maybe always) the products of several or many of them converging or intersecting with one another. It follows from this view that many different unfalsifiable explanations of the same event may be given, depending, *inter alia*, both on which (and how many) causal chains we follow and on how far we follow them.<sup>12</sup> If it should be argued that a true and complete explanation of the event requires us to follow all of the causal chains to their beginnings, we can fairly counter that this is, practically speaking, impossible. How then are we to choose among explanations which consist of citing causal chains? Certainly there are some explanations that can be rejected because of evidence against them. Otherwise, either accepting an explanation of an event or insisting on pushing further back and/or further out along causal chains leading to it will depend on such factors as what it takes to satisfy a particular explanation-seeker's intellectual curiosity and, as suggested in the preceding paragraph, what theories (or biases) he has to predispose him to look for one or another type of cause.<sup>13</sup> Philosophers are wont to refer to such considerations as the "pragmatics of explanation" (e.g., van Fraassen 1980: Chap. 5; Sintonen 1984). However, as I suggested some years ago (Vayda 1983: 272), constraints on the resources (time as well as tools and money) available for the research whereby we might find causes further back in time or further out in space are additional practical considerations that may affect our decisions about how far to go - and when to stop - in extending our causal chains.

It should be made explicit also that explanation-givers must often consider not only what satisfies their own intellectual curiosity but also what will satisfy intended explanation-receivers, such as colleagues,

Ph.D. committees and, in the case of project in applied social science or human ecology, the firms or governmental or non-governmental organisations that are funding the projects and are looking for answers to specific questions. Thus, knowing that there are colleagues like Harris, never satisfied with explaining people's actions by reference to their "inexplicable" beliefs or ideas (Harris 1985: 14,234) and always ready to attack others for "quitting early" and not looking for determinants of the ideas behind actions (Harris 1987: 111-115), may be one of the incentives for us to push on, at least sometimes (as we did in the Bugis case cited above), until we find sources of the ideas affecting people's actions vis-a-vis their environments. On the other hand, if the intended explanations-receivers are government agencies concerned with plans and policies for road-building and forest protection in provinces like REast Kalimantan, we must consider that they may be uninterested in the sources of Bugis values and beliefs about mobility and migration and may be satisfied simply with knowing whether those values and beliefs are conducive to the movement of forest-clearing Bugis farmers to forested areas made accessible (and exploitable) by new roads.

Lest all this should seem an unduly permissive or indulgent view of explanation, it should be emphasised that asking for evidence of causal connections or causal chains is an exacting requirement. It is a requirement that can be a basis for rejecting many explanations, including, from those cited earlier, some explanations of specific environment-related actions by reference to widespread and persistent ideas about nature or the environment. As previously noted, these ideas may be efficacious at times but, in the absence of evidence, it cannot simply be assumed that they rather than some more idiosyncratic or transient ideas are the basis on which particular actions are taken at particular times or in particular contexts.

The same evidential requirement is a basis for rejecting some other explanations which posit what Berry (1994: x) has critically referred to as the same "hegemonic influence of particular agents, institutions, or ideas."<sup>14</sup> Cases in point are some revisionist political-economic explanations of land use by the Kalahari Desert's San-speaking people, once regarded quintessential hunter-gatherers whose way of life told us about our own evolutionary past (see Lewin 1988 for a concise account of new and old views of the San). Lee and Guenther (1993: 226) properly complain that some of the revisionists, in the preoccupation with transformations allegedly wrought throughout the Kalahari by mer-

cantile activity, fail to distinguish between the Kalahari areas receiving one trading expedition per month and those receiving one per decade. In other words, hegemonic influence is being attributed, without sufficient evidence of causes and effects in particular places at particular times, to merchant capitalists everywhere in the Kalahari.<sup>15</sup>

A final point that must be made is a disclaimer about the novelty of what I have been advocating. Although it was common a few decades ago to value generality over causality in explanations, the idea of making them consist of showing causes is far from being new. Although only recently has it become, as Kitcher (1993: 128-129) has noted, once more the majority views among philosophers of science, it extends back, as Salmon (1990: 106) has noted, at least to Aristotle.<sup>16</sup> Connecting causes and effects so that they form chains which are then cited as explanata is nothing new either, as I have already indicated in this article by referring to discussions of the subject by such philosophers as Hume from the eighteenth century and Cournot from the nineteenth century (above, p.17, and note 12). For the sake of facility and coherence of exposition, I have been using illustrations mostly from our East Kalimantan research, but I could certainly have found illustrations in any number of other studies, including some, like ours, concerned with explaining deforestation (e.g., Rudel's 1993 Ecuadorian study or such historical studies as Thirgood's [1986] on Barbary forests). In fact, arguments have been made elsewhere to the effect that what may be called *narrative explanations*, consisting of fixing events along a temporal dimension so that some events can be seen to have given rise to others, are both appropriate and ubiquitous not only in the fields of study that I have considering here but in all such sciences as geology, palaeontology, astrophysics and evolutionary biology, which are concerned with changes through time (Vayda 1995b: 370, citing Richards 1992; cf. Gould 1988).

It has also been suggested that there are cyclical shifts in the extent to which such explanations find favour, with periods of their greater recognition and acceptance resulting from the disenchantment ending periods of greater faith in general models, prescriptions or formulae (Kingsland 1985: 209-210).<sup>17</sup> At the present time, this faith in the general is still being firmly expressed by some who study human actions and their environmental effects (e.g., the cultural materialists like Harris, as discussed in Vayda 1987a, and the Darwinian ecological anthropologists, as discussed in Vayda 1995a and 1995b). However, there is increasing expression of counter views (e.g., Harms 1987,

Shrader-Frechette and McCoy 1993, 1994; Taylor and Garcia-Barrios (1995). A possible interpretation is that this is a time of transition to paying more attention to historical changes and causal relations among particular events. If this is so, it may be hoped that those who are making the transition will find some useful guidelines in the arguments, methods, analyses and illustrations that I have presented here.

## NOTES

1. My views of models as having a sometimes useful rather than an always necessary role in research and explanation is elaborated in Vayda 1995b. I argue there against deciding to try to apply a model in a particular case and committing ourselves to research and analysis to test predictions from it unless, as Schelling (1987: 89) has suggested, we first ask ourselves "whether we need the model - whether the model give us a head start in recognising phenomena and the mechanisms that generate them and in knowing what to look for in the explanation of interesting phenomena."

2. On other kinds of challenges to White's thesis (from biblical scholars and theologians, for example), see Ayers 1986.

3. Someone else who may be too readily attributing context-independent efficacy to established ideas about nature or the environment, despite his having written eloquently and discerningly about the intricate and dynamic reticulations of nature, is the biological ecologist Botkin (1990). In analysing ecological successions in forest, he considers tree interactions changing in complex ways, involving both the operations of chance and such matters as not only how the growth of trees of particular species, size, and age changes with the amount of light received but also how this amount is affected by changes in the size and numbers of competing trees nearby. However, when he turns to interactions involving human beings, he is satisfied with making enduring images or conceptualisations of nature - for example, ideas of nature in balance and of nature as a machine - his main explanation of people's mismanagement of biological resources. In other words, he has, as I have suggested in a review of his 1990 book (Vayda 1991), a simpler view cause and effect with respect to environment-related human actions than with respect to such matters as tree growth.

4. Although my first point here is compatible with methodological individualism as formulated in certain versions that I regard sympathetically (see, for example, Boudon 1981: 36-37), I am making the point without espousing the extreme versions that attribute agency and/or intentionally exclusively to individuals and categorically deny them not only to classes, cultures, ecosystems, societies, and similar problematic "wholes" but also to such entities as football teams,

orchestras, labour unions, business firms, and governments (c.f. Schwartz 1993, Searle 1990, and Coleman 1986 and 1990 on collective agency and/or collective intentionality). At the beginning of the next section may be found another example of making methodological moves compatible with methodological individualism but making them without embracing methodological individualism in its extreme or sweeping formulations.

5. Borrowing Knorr-Cetina's (1984: 41) terminology, we might say that the assumptions are conducive to a "fallacy of unwarranted subsumption." It may be noted here also that Rappaport's *a priori* notions about human behaviour as ecosystems function or response may have influenced his selection of particular behaviour and events for emphasis in his research and his book. Thus, in claiming that the massive ritual pig slaughters are a way of dealing with a pig population threatening to increase beyond the limits of carrying capacity, Rappaport emphasises natural increase in pig numbers and neglects deliberate efforts made by maring at some times and not others to increase the size of pig herds trading for pigs (Lowman 1980: 88-92) and by perhaps resorting to other means that have been reported from elsewhere in the New Guinea highlands, e.g., allowing more piglets to survive in certain years than in others in anticipation of pig-slaughtering ceremonies and of the social and political benefits to be gained from having large pigs to kill and pork to give when the ceremonies are held (Salisbury 1975: 130-131; cf. Hide 1981).

6. Related to these considerations is an argument that I have made elsewhere (Vayda 1993: 71) to the effect that the many movements of people, resources, and ideas across whatever boundaries ecosystems, societies, and cultures may be thought to have render it specious for Rappaport and others (e.g., Geertz 1984) to make lofty appeals for attention to context and then to attach those appeals to *a priori* judgments to the effect that the context is necessarily an ecosystem or a culture or some other pre-defined whole.

7. The approach I am advocating corresponds in some respects to the "bottom-up" approach which Botkin (1993), as a forest ecologist, has adopted in preference to a "top-down" approach which "begins with characteristics at the largest spatial scale and attempts to explain phenomena at each lower scale from the understanding achieved at the

higher level" (Botkin 1993: 17). Botkin does, however, seem to start with a notion of a pre-defined whole, like a forest ecosystem, whose dynamics will ultimately be explicated by focusing first of all on what happens to individual trees of different species, sizes, ages, and vigour (Botkin 1993: 18). While Botkin and I agree about starting with small-scale observations with an objective of being able eventually to illuminate some wider-scale patterns or order, I do not see this as requiring us to specify *ab initio* the so-called systems to which those patterns or order should pertain (cf. Barth 1987: 24 on taking cues from Darwin about extracting "generalizable mechanisms and processes" from closely observed particulars).

8. Cf. Dore (1961) who, as a methodological individualist, argues that causal analysis is impossible without translation of aggregate concepts or terms into individual ones, and Mandelbaum (1973: 229), who, although critical of individualism, concedes that translation is sometimes necessary for verification of statements about group or aggregate properties.

9. Kincaid's (1993: 241) *reductio ad absurdum* of Elster's suggestions is that Elster's explanatory ideal ultimately requires citing events at the level of fundamental particles and that this, as a requirement, would render either unconfirmed or non-explanatory almost all of science's present causal claims. Kincaid (1993: 243-244), however, concedes that the move to less aggregate levels of phenomena is sometimes advantageous for the purpose of explanation. His insistence that whether or not it will be advantageous cannot be decided on a priori, conceptual or broad philosophical grounds but will instead depend on context and "substantive empirical information" is congruent with my own pragmatic justification for translating aggregate concepts or terms into individual ones (cf. Kincaid 1996: 179-182).

10. *Ex post facto* support for decision to move from more aggregate levels in our research is provided by recent macro-economic studies of deforestation. Thus, Shafik's conclusion from a review of such studies is that "the preferability of microeconomic and case study work": is indicated by "the absence of any conclusive results on the causes of deforestation at the microeconomic level" (Shafik's 1994: 95).

11. Our being guided in our research by the ecologists' generalisations relating forest recovery and gap size illustrates what I have discussed elsewhere (Vayda 1995b: 363) as an appropriate use of generalisations for getting a "head start" in empirical research to show causal connections.

12. A reason for saying "*inter alia*" here is that different explanations of apparently the same event will result also when different explainers or explanation-seekers are using counterfactual reasoning to explain the event and have in mind different alternatives to it - or different "contrast cases," as they have been called (e.g., in Hilton 1995: 496). Such occurrence of different explanations may be said to relate to the "contrast-case problem," in making causal judgements rather than to the "connections problem," which, in my usage, concerns the connections we make among events so as to constitute causal chains (cf. Hesslow 1988: 13, who makes a somewhat similar distinction). What may become the classic illustration of the explanatory consequences of using different contrast cases to explain the events is the imprisoned Willie Sutton's answer when a reform-minded priest asked him why he robbed banks. "Because that's where the money is" makes sense as an explanation of why Sutton robbed banks instead of news-stands and candy stores but not as an answer to the priest's intended question of why Sutton robbed at all instead of working honestly for a living. This illustration has been cited in various places (e.g., Garfinkel 1981: 21-22; Hawthorn 1991: 171, note; Vayda 1989: 171 and 1994: 328, note 7). For further discussion as well as anthropological/human ecological illustration of the explanatory consequences of using different contrast cases, see Vayda 1989. As for the "connection" problem, a lucid discussion of seeing events as having multiple causes and as being the products of *inter-dependent* and/or *independent* causal chains is given in a few paragraphs by the 19th century French philosopher and mathematical economist, Cournot (1956: 39-43). For more recent discussion along similar lines, see Lewis 1986: 214-217.

13. For discussions of how acceptance of an explanation depends on what the explanation-seeker already knows and what he still wants to know, see Scriven 1975; Gardenfors 1980, 1990; Lewis 1986: 226-230; and Hilton 1995.

14. Cf. Vincent (1990: 396): "...as political anthropologists began to place local political structures in the context of the modern world-system, they tended to forget the rich-grained political ethnography of the past. Megaconcepts like exploitation, domination, and imposition tended to take the place of both causal analyses and explanation."

15. While my preference is for direct evidence of the sequences of events regarded as constituting causal chains, I recognise that comparative studies and/or "design" analyses may sometimes be used for making certain inferences about causal chains when direct evidence of pertinent sequences of events is lacking. This is discussed in Vayda 1995a: 227-228.

16. As recently as 1993, the sociologist Mayhew was stating vehemently that "causal arguments" are both old wrong! How research on human behaviour and its environmental effects may be influenced by alternative conceptions of scientific explanation has previously been explored by me elsewhere (Vayda 1995a: 223-225 and 1995b: 360-364). At this point, it should be noted that those in the majority now in agreeing on connecting explanation and causation may still, as Lewis (1986: 217, note) has remarked, disagree substantially with one another - as Lewis does with Salmon - in their accounts of causation. Any detailed consideration here of such disagreements among philosophers would take us too far afield from issues of methods and explanations in studies of human actions and their environmental effects. It may, however, be suggested here that those who engage in such studies may, like the experienced historians and mechanics described by Scriven (1966: 251), recognise causal relation in their subject matter without having undertaken, needing, or even being aware of anything like Salmon's painstaking analyses (1984, 1994) of the means by which causal influence is transmitted and the means by which so-called causal structure is generated and modified (cf. Salmon 1990: 114-115).

17. Cf. Hawthorn 1991: 160-161 on historians' and social scientists' "retreat" from the Enlightenment's generalisation-oriented analytic programme. Among the reasons that he cites for the retreat is that the "causal connections or runnings-on that we have been able to detect...have turned out either to have to be phrased at a level that is so general as to be insufficiently informative and not address our interests in explanation; or to be conditional as not be general; or, when they have generated testable predictions, to be false."



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Andrew P. Vayda, drawing on his long experience in studying the relations between people and their environments, addresses here the question of how human influences can be better incorporated into ecological studies. Citing relevant sources from the literature of philosophy as well as social and biological science, he argues that we should be guided in our research by the goal of giving causal explanations of concrete human behaviour and its concrete environmental effects. Existing theories and models may have a role in guiding such efforts, but developing or testing the theories and models need not be made the main objective.

Before illustrating his advocated approach with various studies (including his own research in Indonesian forests), Professor Vayda considers other approaches and indicates problems with them because of their failure to give due consideration to the chains of causally connected events leading to specific actions and their effects. Among the problematic approaches are those which assume that human behaviour affecting the environment is governed by basic conceptualisations or values concerning nature or the environment. Certain systems approaches to the study of environment-related human behaviour are also critically examined.

Professor Vayda is currently a Distinguished Visiting Scholar at CIFOR. He has previously been a consultant to WWF - Indonesia on research, training and conservation management in East Kalimantan's Kayan Mentarang Nature Reserve. He has published more than ninety articles and several books and holds the title of Professor of Anthropology and Ecology at Rutgers University in New Brunswick, New Jersey.

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