

The Epistemology of Southeast Asia's Anthropogenic Grasslands : Issues of Myth, Science and Development

Michael R. DOVE*

"When elephants dance, the grass suffers." [W. S. Merwin]

One of the characteristic aspects of the human-ecology of Southeast Asia is its fire-climax grasslands. The dynamics of these grasslands are one of several great mystified topics of natural resource use in the tropics. This mystification is the subject of my analysis here. I will begin with an accounting of the major myths regarding the ecology and economy of these grasslands, and then discuss the lack of study of these myths. I will then place the failure to problematize these myths in the context of contemporary development, suggesting that development planners are better served by misrepresenting than representing the grasslands. I will next consider the role of science in this misrepresentation, examining the way that the facts of grassland dynamics are avoided, ignored if they cannot be avoided, misused if they cannot be ignored, and then "forgotten" in any case. I will conclude with suggestions for the future directions of research on the region's grasslands and the global environment.

I Grassland Myths

The modern discourse about grasslands has been dominated by a tenacious complex of state resource myths regarding fire, shifting cultivation, and grazing. This complex of beliefs was the subject of the monumental, three-volume bibliography that Harley H. Bartlett published in 1955–1961, in an attempt to set the record straight on these matters : it was titled "*Fire in Relation to Primitive Agriculture and Grazing in the Tropics*." The tenacity of this complex of beliefs is reflected in the fact that even when Bartlett's bibliography was published, at a time when there was far less information available on these issues than there is today, Bartlett was still able to fill 1,657 pages with abstracts of works containing useful information on fire, primitive agriculture, and grazing. And yet despite this substantial written record, the mythical views of these phenomena flourished then, and they flourish still today.

1. *Economic Aspects*

At the base of all of the grassland myths is the belief that grasslands are unproductive and even destructive in character. For example, *Imperata cylindrica* has been long and widely interpreted as a

* Program on Environment, East-West Center, 1601 East-West Road, Honolulu, Hawaii 96848–1601, through 11/97, School of Forestry and Environmental Studies, Yale University, 205 Prospect Road, New Haven, Connecticut 06511, as of 12/97.

sign — or even as an agent — of poor soils. A frequently cited reference in this regard is the statement in Nye and Greenland's [1960 : 9] classic work on soils under shifting cultivation, that soils under *Imperata* are "useless for cultivation." Based on his observation of grassland tillage in New Guinea, including *Imperata* grasslands, Clarke [1966 : 356] long ago cast doubt on Nye and Greenland's conclusion. He suggests that grasses vary in their impact on soil fertility (cf. [Burbridge *et al.* 1981 : 241 ; Soepardi 1980]) and also that young forest (at least) may not be much better than grassland in this regard [Clarke 1966]. Sherman [1980 : 124–132] devotes an extended analytical discussion to disputing the Nye and Greenland statement : he argues that grassland fertility has been misunderstood because it is usually interpreted in terms of a very different, forest fertility model. Sherman [*ibid.* : 132] argues that whereas forest fertility is based on a build-up of nutrients in the forest biomass, which must be burned in order to release them, grassland fertility is based on a build-up of nutrients in the sod-matrix (formed of grass rhizomes), which releases its nutrients through decay.¹⁾ The fact that a different model prevails in grasslands is reflected in the fact that grassland-using peasants, like the Banjarese whom I have studied in Southeast Kalimantan [Dove 1981 : 191], view *Imperata* as an indicator of soil arability not non-arability.

The myth of soil infertility under grassland is associated with a myth about the "unproductivity" of grassland-based management systems.²⁾ According to the most extreme version of this myth, the fertility of grasslands is too low to support agriculture at all ; according to less extreme versions, grasslands can be cultivated but productivity is low. (This leads Peters and Neuenschwander [1988 : 52] to erroneously assert : "Large-scale use of savanna grassland for agriculture by traditional methods is seldom seen.") In his publication on this subject Clarke [1966 : 356–357] acknowledges that there is an apparent decline in the productivity of the land as the result of grassland succession, but he argues that the adoption of more intensive techniques of cultivation as a result of this succession may in effect raise the productivity of the ecosystem (although he also acknowledges that productivity per unit of labor expended may decline). I have elsewhere [Dove 1981 : 195–197] acknowledged that rice yields from grassland are less than those from forest (viz., 2,500 li/ha versus 3,500 li/ha), but he argues that because grassland can be cultivated much more often than forest, the average annual yield is much higher in grassland (viz., 1,750 li/ha/yr versus 350 li/ha/yr). Sherman argues that the productivity of the best grassland rice fields may exceed that of either forest fields or irrigated fields [Sherman 1980 : 129–131] ; and on this basis he critiques Geertz's [1971 : 25] famous — and oft-quoted — characterization of *Imperata* grasslands as Southeast Asia's "green desert."³⁾

1) Sherman's conclusions contradict orthodox thinking, such as that of Peters and Neuenschwander [1988 : 54], who write "many observers in temperate zones . . . once believed that grass fallow improved soil structure. Apparently this idea does not hold up in the tropics, where trees, not grasses, are the soil builders."

2) A generation ago, the opposite and equally invalid myth prevailed, namely that tropical grasslands were potentially as productive as the best temperate zone grasslands. This myth prompted Whyte's [1962] article entitled "The Myth of Tropical Grasslands." Both myths, that of over- and under-productivity, stem from an imposition of external preconceptions on local bio-cultural realities.

3) Geertz [1971 : 25] writes, "Given less than ideal conditions, it [swidden agriculture] is highly susceptible to breakdown into an irreversible process of ecological deterioration ; that is, a pattern of change leading ↗

Agricultural use is just one of a number of reasons why this "green desert" label is incorrect. A second major reason and a second productive use of *Imperata* grasslands involves fodder.⁴⁾ The myth is that cattle cannot eat *Imperata*, that it is inedible. I have elsewhere reported on official beliefs to the effect that not only can cattle not eat *Imperata*, if they somehow chanced to do so their stomachs would swell up and kill them [Dove 1986 : 176]! In fact, the only relevant dimension here is age : young *Imperata* is tender and nutritious, old *Imperata* is not (but neither is it poisonous) (cf. [Soewardi *et al.* 1974]). Most of those who have actually studied *Imperata* in the field report its consumption by cattle [Conklin 1959 ; Dove 1981 ; Sherman 1980].⁵⁾ (I have even reported on a system in montaine Central Java in which *Imperata* is hand-cut, carried, and fed to stall-bound cattle [Dove 1986].) Gibson [1983 : 381] observed that fenced cattle even prefer *Imperata* over sown pasture legumes (thereby explaining why so many attempts to introduce "improved" grasses into upland Southeast Asia have failed).

2. *Ecological Aspects*

Because the economic character of grasslands is not understood (viz., because their productivity is denied), their ecological character also is completely misunderstood. Most critically, because the economic interest of people in the persistence of grasslands is not appreciated, this persistence is attributed not to active human management but to the grasslands' purported physical "indestructibility."⁶⁾ To this day a basic premise on the part of national and international agencies that have to deal with grasslands is that grasslands are a stable and tenacious climax community, which will not disappear unless dramatic steps are taken to make it disappear. This premise is diametrically opposed to the view of grasslands obtained through empirical study, namely that it is a dynamic and unstable community that must be actively maintained in the face of all sorts of forces — both natural and man-made — that jeopardize it. Gibson [*ibid.*], for example, writes that simple grazing will overcome *Imperata* grasslands in northern Thailand (and this has been widely observed throughout Southeast Asia) ; and Wharton [1968] similarly notes that in the Naga hills *Imperata* grassland becomes dominant only when grazing is stopped.⁷⁾ Conklin [1959 : 61–62] echoes the importance of grazing (but also notes the importance of no burning) and quotes a Hanuno'o informant as saying, "Only cattle can conquer cogon [*Imperata* spp.]." (Conklin [*ibid.*] adds that grassland

↘ not to repeated forest recuperation but to a replacement of tree cover altogether by the notorious *Imperata* savanna grass which has turned so much of Southeast Asia into a green desert." Cf. Hutterer's [1983 : 179] comment that the rainforest itself has been described as a "green desert."

4) Another major economic used of *Imperata* is for thatch : Yanes and Zeegers [1992] report on market-oriented gathering of *Imperata* for this purpose in the Cagayan valley in Luzon, the Philippines. Minor but still significant economic used of *Imperata* include medicinal use of the rhizomes [Tjitrosoedirdjo 1993 : 44].

5) Cf. Burbidge, Dixon, and Soewardi [1981 : 241] and Seavoy [1975 : 49] on the burning of *Imperata* in Indonesia for grazing and game.

6) Bartlett notes [1956 : 702] that *Imperata* is easily-enough suppressed, by mowing, whenever Europeans wanted to establish golf courses in the tropics [cf. Bartlett 1955 : 401].

7) Cf. Singh *et al.* [1985 : 49], writing on South and Southeast Asian grasslands : "Almost invariably a release from grazing initiates accumulation of organic matter and moves the community to woodland situations."

succession may be affected by other factors as well, including repeated cropping, no inter-cropping, topography, exposure to wind and rain, and proximity to sites of shifting cultivation as opposed to forest.)⁸⁾ Finally, most scholars who have conducted empirical studies of grassland have also noted that one of the greatest threats to grassland is simply the passage of time : in almost all of the grasslands that have been discussed thus far, the passage of time allows natural processes of vegetative succession to take place, which gradually replace the pioneering, quick-growing and sun-loving grasses with slower-growing, more shade-tolerant vegetation. It is, in short, basic principles of ecological succession that make grassland communities not indestructible but fundamentally unstable.

The human intervention that most commonly interrupts this process of succession, and thus preserves the grassland, is anthropogenic burning. This is perhaps the most misunderstood aspect of all in the human ecology of grassland management. Burning is still widely condemned by external authorities as something destructive and “primitive.” However, such beliefs ignore the actual role of fire in grassland ecology, natural ecosystems, and human evolution. Komarek [1967 : 154] suggests that early man was an evolutionary product of “fire environments” and was, indeed, “fire selected” in evolutionary terms. Stephen J. Pyne, one of the leading contemporary researchers on fire and human society, similarly argues that the “domestication of fire” was integral to the development and spread of human society [1993 : 246] :

Everywhere that humans went — and they went everywhere — they carried fire. The hominid flame propagated across the continents like an expanding ring of fire, remaking everything it touched. Within that ring lived humans ; outside it, the wild still reigned . . . Much as humans killed wolves and propagated dogs, so they drove back the domain of wildfire and substituted a regime based on anthropogenic burning.

Facetious remarks to the effect that some “primitive” groups *lived* off fire [Hodgkinson *et al.* 1984 : 141] were not as far from the truth as their speakers may have thought. Because human beings do not just use fire environments but also reproduce these environments (through the use of fire), they are, in ecological terms, “pyrophytes.” (A pyrophyte is a species whose traits include those that make fire more likely.) Indeed, Pyne [1982 : 69] argues that humans are foremost among pyrophytes, in that they can “project fire rather than endure it.” Among plants, one of the major pyrophytes is grasses : D’Antonio and Vitousek [1992 : 73] write in a recent review that grassland and fire can be considered to be an “identity” and that we can speak of a “pyrophytic grass life form.” In the fire-climax grasslands of Southeast Asia, therefore, two major pyrophytes, people and grasses, come together in a mutually supportive relationship. People and grass both benefit from fire — and from one another — and in turn both promote fire.

A final misunderstood aspect of grassland ecology involves their role in water and soil retention versus run-off. It is widely believed that grassland does not properly conserve either soil or

8) Cf. Peters and Neuenschwander [1988 : 50–51]: “Swidden succession to grassland rather than secondary forest depends partly on topography (particularly a long dry season), and partly on clearing practices, fire, and grazing. In areas with substantial relief, grasslands are more common on hilltops and ridges.”

water. Again, this belief is disproved by all of the empirical studies of grassland. Sherman [1980 : 129] draws on published, secondary data to dispute Nye and Greenland's [1960 : 135] conclusion that erosion is promoted by grassland "on all but the most gentle slopes." Gibson [1983 : 379–381] draws on both Thai and pan-tropical data to argue that when a watershed is covered with grassland as opposed to forests, soil erosion is less and water run-off or yield is greater. Evidence of the positive impact of *Imperata* on erosion-control is reflected in the bizarre and ironic fact, reported by Sherman [1980 : 127, n. 41] that *Imperata* was blamed for the erosion that occurred in colonial rubber plantations *following its removal*.

3. *Studying the Myths*

Little if any study has been made of the origin of these myths of grassland ecology and economy. Only recently have scholars attempted to understand why disjunctions between the published evidence and the public belief persist ; only recently have scholars begun to ask why such myths are *necessary*. Current examples include the histories of forest policy carried out by Richard Grove [1995], and the studies of attitudes toward fire carried out by Pyne [1982 ; 1993 ; 1995].⁹⁾ For example, Pyne [1993] suggests that many of the current attitudes toward the use of fire in tropical resource management were borne in and of the context of the colonial state. He points out that these states has little reason to love fire [*ibid.* : 255] :

Fire threatened fixed property and often the social relationships of rigidly ordered societies. Broadcast fire encouraged varieties of nomadism : the seasonal cycling of pastoralists, the long-fallow hegriras of swidden farmers, population mobility that made political control and taxation difficult.

Pyne [*ibid.* : 256] argues, therefore, that colonial state antipathy toward fire was not based on emotion or cultural differences but on real and basic differences in material self-interest, and states acted accordingly :

As soon as it was politically and technically feasible, [colonial] foresters instigated fire control measures. As often as not, fire suppression was one of the most powerful means of controlling indigenes.

It is notable how little remarked this means of "controlling indigenes" has been, considering all of the resources that have been devoted to critiques of colonial governance. One explanation for this is because fire policy has not been sufficiently problematized to be seen *as* policy, as a tool in the furthering of state interests. The fact that there *was* a subjective policy is seen in the marked ease with which it was discarded when, under particular circumstances, it no longer served these self-interests.

The historical development of the famous Deli tobacco cultivation system in Sumatra presents an excellent example of this. During the early decades of the industry, the planters did not care if *Imperata* consumed their fields (after harvest), so *Imperata* spread like wildfire ; but then the planters' attitude changed, and the *Imperata* disappeared. Pelzer [1978 : 29–30, cf.42, 43] summarizes

9) Cf. Lewis' [1989] comparative study of attitudes toward fire among park rangers and aborigines in Australia.

this history as follows :

During the first three or four decades that the tobacco industry operated, land cleared for the raising of wrapper tobacco was quickly taken over by *Imperata* species and other tropical grasses, so that vast expanses of man-made savannas replaced the rainforest because frequent grass fires prevented natural reforestation. So long as the planters believed that their land could produce only *one* tobacco crop, they did nothing to combat the spread of grasses. Once they realized, however, that they had been far too pessimistic and that tobacco could be planted repeatedly provided the land lay fallow under the second-growth forest, or *blukar*, for not less than seven or eight years, they took measures to prevent the burning of the grasses and the concomitant killing of young trees . . . These actions greatly altered the physiognomy of the tobacco plantations, as second-growth forest smothered the grasses and spread steadily at the expense of the savannas.

Thus, in the mid-nineteenth century, in colonial Indonesia, the knowledge of how to suppress *Imperata* was both possessed and used by the plantation sector *when it wished to do so*.

Further light is shed on these myths by the fact that they have also occurred in the relatively recent past of the now-developed West. It was not so long ago that a debate raged in the U.S. over the use of fire in local community management of grasslands and forests, much as it does today in many developing countries. For example, in 1939 the U.S. Forest Service commissioned a staff psychologist to find out why the residents of the forested south burned the forests. This study led to publication a year later of the (in)famous article, "Our Pappies Burned the Woods," in which John P. Shea (the psychologist in question) attributes burning to "emotional satisfaction." He writes "The sight and sound and odor of burning woods provide excitement for a people who dwell in an environment of low stimulation and who quite naturally crave excitement" [Shea 1940 : 162]. Shea [*ibid.* : 160] ascribes the persistence of fire burning in the face of government proscription to the strength of tradition, saying that "Their strongest law is the custom of their forefathers." He quotes one of his informants as follows : "Woods burnin' is right. We allus done it. Our pappies burned th' woods an' their pappies afore 'em. It war right fer them an' it's right fer us" [*ibid.* : 159].

Shea is quite happy to accept this invocation of tradition ; he is less willing to accept more ecologically-oriented explanations from the forest-burners. Thus, Shea [*loc. cit.*] subsequently quotes the same informant as saying, "Fires do a heap of good, kill the' boll weevil, snakes, ticks, an' bean beetles. Greens up the grass. Keeps us healthy by killin' fever germs." Of this analysis Shea [*ibid.* : 162] later writes, "Their explanations that woods fires kill off snakes, boll weevil and serve other economic ends are something more than mere ignorance. They are the defensive beliefs of a disadvantaged culture group." Rejection of local interpretations of resource-use and mythologizing of this use is thus not just a characteristic of the contemporary Third World, therefore ; rather, it is a characteristic of state authority, regardless of time and place, that does not wish to acknowledge the validity of local, non-state resource management.

II The Development Context

The impact of implicit institutional agendas on official perceptions of grassland ecology and economy

is particularly problematic in development contexts. Beginning in colonial times, throughout Southeast Asia, grasslands (especially of *Imperata*) have been viewed within development policy as an unproductive and undesirable land-use that should ideally be replaced with something more productive and more desirable (e.g., a plantation or permanent agricultural fields). A generation of research before and another following World War II was devoted to active interventions designed to do just that. These efforts were characterized by an emphasis on technological innovations with little or no consideration for economic (or social) costs [cf. Whyte 1962 : 8]. The more astute of the participants in such efforts now recognize that grassland "reclamation" is economically unfeasible [Vandenbeldt 1993 : 5],¹⁰⁾ but the perception that they *need* to be reclaimed persists [e.g., Grist and Menz 1995].

1. *The Development "Niche"*

At the end of his study of the Batak grasslands of Sumatra, Sherman [1980 : 143] points out that the complex, composite system of perennial crops, food crops, and forage grasses that contemporary academic "experts" have proposed for the "abandoned" *Imperata* lands of Sumatra is in fact *already* being practiced there on an indigenous basis. If the end-state that the development experts are pursuing is *already attained*, then what is the purpose of the development process? Another way to ask this question is, What are the implications of portraying skilled, indigenous resource managers, like the Batak grassland farmers, as needy victims, as people confronted with resource degradation that they supposedly cannot cope with? That is, what are the implications of outsiders asking, "How can we 'help' get rid of the grasslands?" The public construction of a situation as one in which help is needed is typically both empowering of the potential helper and dis-empowering of the potential helpee [Dove 1993 ; Edelman 1974]. It is vitally important for any agency involved in development to be able to publicly portray a potential development subject as being needy, as needing in particular the resources that agency has to offer.¹¹⁾ It is important for any such agency, in short, to create a sort of conceptual welcoming niche for itself [Ferguson 1990].

The creation of this niche can involve great misrepresentation. Thus, Leach and Fairhead [1994] recently showed how a process of forest incursion into grassland zones in Guinea is misrepresented as a process of grassland incursion into forest zones, in order to construct the picture of "environmental crisis" needed to obtain donor funding. In my study of the Banjarese, I identify a similar reversal of the reality of grassland ecology [Dove 1981]. Whereas the government sees the *Imperata* grasslands as a problem, the Banjarese see them as an important solution (to many of life's problems) ; and whereas the imperative in government planning is to eliminate (to get rid of) the grasslands, the imperative in the peasant system of grassland management is to maintain (to keep) them.

10) Thus, the emphasis has shifted from trying to completely reforest grasslands to trying instead to accelerate natural afforestation [e.g., Drilling 1989].

11) Note the echo of this in the previously-cited reference by the forest service psychologist Shea [1940] to the "defensive beliefs of a disadvantaged culture group."

2. *The Importance of Grassland Abundance*

The importance of constructing a social reality of grasslands that dominant institutions can live with needs to be interpreted in the context of the *scope* of these grasslands. Recent estimates in Indonesia, for example, suggested that grasslands cover 10–12 million hectares of the country's land area [Tjitrosoedirdjo 1993 : 33 ; Vandenbeldt 1993 : 3]. This is an area so large as to have profound economic, social, and political implications — especially in light of the fact that the “problematic” status of the grasslands means that alternate resource regimes can be considered for this entire area.¹²⁾ There is much at stake over lands so vast, and when this is the case, reality tends to be mystified. The mythology that is spun about the grasslands helps, in turn, to explain the paradoxical fact that their extent is often even exaggerated (e.g., the World Bank estimated in 1988 that Indonesia's grasslands covered 30 million hectares, which is 2 1/2 times the estimates given above). This paradox is easily explained : given the typical attempt by outside bureaucracies to portray these grasslands as problematic and thus in need of the bureaucracy's attention, the greater the extent of grasslands that can be claimed to exist, the greater the scope for bureaucratic intervention.

3. *Implications for Development Analyses*

This emphasis on self-privileging in representations of grasslands has important implications for how development analyses are carried out, especially with regard to development failures. The development community commonly blames development failures on recalcitrant development subjects, sometimes on poor implementation, and occasionally on poor policy. But even the explanation that hits closest to home, poor policy, is naive, because it treats this and other factors as isolated phenomena as opposed to phenomena that are deeply “embedded” in wider social and historical processes [Hecht and Cockburn 1989 : 99]. Analyses based on these false assumptions result in an inability to correct, and thus a tendency to perpetuate, development failures [Esteva 1987 : 136].

Since these false assumptions are themselves socially determined — Hecht and Cockburn imply that the attempt to seclude policy from political economy is determined by that same political economy — this raises the further question. To what extent are “unintended” development failures in fact “intended” (in some structural sense)? Returning to the subject of this study, this

12) There is considerable historical precedent for indigenous regimes of resource use and tenure being denied by outside authorities on the basis, in part, of their magnitude. Compare the citations by Cronon [1983 : 57–58] and Bryant [1994 : 235] from colonial authorities in seventeenth century North America and nineteenth century Burma, respectively :

We did not conceive [wrote the New England minister John Cotton] . . . that it is just Title to so vast a Continent, to make no other improvement of million of Acres in it, but only to burn it up for pastime.

If anything of the kind [Karen tenure to lands used for swidden agriculture] were recognized [wrote the colonial forester Dietrich Brandis in 1876], there would hardly be a square mile of forest in these Yoma hills, which could not be claimed by some Karen family or other . . . the theory of any occupancy rights being acquired by these erratic and temporary clearings of the forest is quite untenable.

raises the question whether the contested grassland landscapes that dominate much of upland Southeast Asia are in fact what the wider society is *bound* to achieve.¹³⁾

III The Scientific Context

Science is implicated in the attainment of the ecological landscapes that we actually get, as opposed to the ones for which we are purportedly striving.

1. *Information Flows*

We may start by looking at information flows. Information, or the lack of it, is an important part of the social construction of the reality of the developing areas of the world. What Hecht and Cockburn [1989 : 1] write regarding the Amazon applies world-wide :

The mystery that is part of the Amazon's allure is not merely a function of the region's immensity and of the infinitude of species it contains. It is also the consequence of centuries of censorship, of embargoes placed on knowledge and travel in the region by the Spanish and Portuguese crowns, of the polite silences of the religious orders during the Amazon's colonial history.

This mystery or secrecy can be viewed as part of a wider framework of asymmetrical relations. Thus, Dove and Kammen [forthcoming] have recently argued that the flow of information between global centers and global peripheries is characteristically asymmetrical : that is, information generally flows *from* centers and *to* peripheries as opposed to the reverse. Chambers [1983 : 76] similarly writes :

From rich-country professionals and urban-based professionals in third world countries right down to the lowliest extension workers it is a common assumption that . . . knowledge flows in one direction only — downwards — from those who are strong, educated and enlightened, towards those who are weak, ignorant and in darkness.

The absence of information from and on peripheries is clearly an important element in the kinds of development policies that are formulated for peripheral regions. For example, in a recent analysis Ascher [1993] argues that a "rent transfer" strategy is responsible for the rapid degradation of Indonesia's tropical forests, and that the persistence of this strategy is dependent upon "embarrassment minimization." Ascher [*ibid.* : 17] writes :

The rent transfer strategy is both a potential embarrassment and the object of concerted opposition (especially from international donors). Therefore, the Forestry Ministry and other agencies have an incentive to suppress, restrict, or simply neglect to gather relevant information.

13) Ascher [1993 : 15] asks this same questions regarding forests, as follows :

What are the institutional interests of the Forestry Ministry? If this question can be answered, we may understand — and suggest ways to counteract — the seemingly paradoxical behavior of a forestry agency that has been aiding in the liquidation of the forests.

The result is powerful institutional support for uncertainty, which must be seen in this context as a *sociological* phenomenon. Thompson, Warburton, and Hatley [1986 : 23] write :

Uncertainty, we begin to realise, is not just the absence of certainty but, rather, a positive thing in its own right — something that can be socially generated and socially imposed in order to protect the legitimacy of established institutions and to prevent that legitimacy from being eroded by a creeping tide of certainty.

2. *Poor Research*

When research is done on sensitive topics like grasslands, it tends not to be properly focused or properly conducted. Regarding research focus, I already have mentioned the fact that the aspects of grassland ecology that are central to the “myths” about tropical grasslands are never studied empirically. Regarding the proper conduct of research, examples of misunderstanding from the developmental and biological sciences abound. This includes examples from the work of even the most astute scholars of tropical ecology. Thus, Sherman [1980 : 118, 128–130] points out repeated inconsistencies in the analysis of grassland ecology even in the classic work on tropical soils and shifting cultivation by Nye and Greenland [1960] and in the more general text on tropical soils by Sanchez [1976].

Even more surprising to me (as a social scientist) are examples of misunderstanding from social scientific studies. In his careful review of the relevant literature, Sherman finds faults in practically all of the pioneering studies of society and tropical environment by social scientists, including those by Pelzer [1945], Geertz [1971], Hanks [1972], and Leach [1954]. Sherman’s critiques are convincing, in part, because they are based on textual analyses of what these scholars wrote themselves. Based on these analyses, Sherman demonstrates that the authors presented out-dated views of grassland ecology that were not even supported by their own data. Sherman [1980 : 139–140] argues that the wider interpretation of grassland ecology in these accounts was so flawed as to throw into question the picture that they presented of society and environment in Southeast Asia.

3. *Poor Use of Research*

There have clearly been major obstacles in the way of understanding systems of grassland ecology and management, but lack of information has not been one of them. Sherman’s textual critiques show in case after case that scholars had in hand the information that they needed to properly interpret the grasslands, but they suppressed or otherwise misused it (much as Shea [1940] reported and then deprecated the reasons given by his informants in the U.S. South for burning the woods).¹⁴⁾

14) In some cases, the myth and the reality are simply reported together, with little if any apparent sense of cognitive dissonance. For example, the characterization by researchers of *Imperata* lands in the Cagayan Valley in the Philippines as “idle grasslands” [Maus and Schieferli 1989] is repeated by later researchers in the same project, even though the latter’s research concerns the gathering of *Imperata* for the market, for thatch, as an important — and in some cases the most important — source of income for the inhabitants of the region [Yanes and Zeegers 1992]. The later researchers add the prefix “so-called” to the label “idle grasslands” and also the caveat that “these areas are still suitable for agricultural and other purpose,” but they still do not directly contest — much less problematize the basis for the validity of the evaluation “idle lands.”

Accurate interpretations of grassland ecology also existed in the literature but, again, were either ignored or misused. The disregard of Bartlett's [1955–1961] encyclopedic review of written accounts of traditional grassland (and forest) management has already been cited. Another example pertains to the Batak system of grassland agriculture that was described by Sherman in 1980, in contravention of the literature and expert opinion : it turns out that this system had already been described by the Dutchman Junghuhn in a published account [1847] over one century earlier. In addition, some of the studies cited here — notably those by Conklin [1959] and Bartlett [1956] — represented early and explicit corrections to the prevailing myths about grassland ecology. And there have been a series of insightful studies in the years since, including those of Sajise [1972] in the Philippines, and Soewardi *et al.* [1974] and Suryanata and McIntosh [1980] in Indonesia.

The fact that such studies existed but had little or no impact on beliefs about grassland ecology is *sociologically* meaningful. As Holling, Taylor and Thompson [1991 : 21] write about mistakes : “Surprises — the mistakes we go on and on making — are profound truths, even though (indeed, precisely because) they cannot tell us what is true.” As Thompson has said in another publication, repeated developmental mistakes are, in effect, development “signposts,” which point us toward the most important development truths [Thompson, Warburton, and Hatley 1986 : 147].¹⁵⁾ In this case, we suggest that the repeated failure to properly interpret the available evidence on grassland ecology says something important about linkages between science, society, and environment.

4. *Research Paradigms*

Whereas the popular image is that science proceeds in a social vacuum, we are periodically reminded that scientific thinking is in fact constrained by social institutions. Douglas [1986 : 70, 71, 74] illustrates this well in an essay that looks at the curious phenomenon of scientific “forgetfulness.” Her analysis is based on the work of the sociologist Merton [1961 : 1963], who found that scientific “discoverers” routinely deny the existence of the prior discoveries that contributed to their work. As a result, the same scientific question may remain “in a static condition, as though it were permanently condemned to repetition without extension” (Merton cited in Douglas [1986 : 74]). As to why scientists forget previous solutions, Merton concludes that such forgetting is integral to science. The fact that understandings of grassland ecology are periodically obtained, and published, but ignored, is an example of this phenomenon of scientific forgetfulness.¹⁶⁾ Merton's thesis would lead us to suggest that this forgetfulness is not an “accident,” therefore, but that it is integral to the science of development — and to the institutions that sponsor as well as carry out this science.

One of the best-known analyses of why some findings are accepted in science and some are not, is Kuhn's [1962] thesis of paradigm change. According to this thesis, scientists spend most of their time doing “normal science” within an (often unconscious) paradigm, which is a sort of pre-

15) Thompson, Warburton and Hatley [1986 : 147] write : “Natural and institutional obstacles, if we are prepared to learn from them, become development signposts.”

16) Thus, Gerlach [1938] published one-half century ago an accurate account of the same system of Banjarese grassland agriculture described in Dove [1981], but to little avail.

theoretical ordering of reality. Because of differences in world view and conceptual language, there is no communication between followers of different paradigms : evidence gathered and interpreted within other paradigms is simply ignored. Kuhn's analysis of the operation of these paradigmatic "blindness" in science may help to explain what appears to be the non-empirical character of much of the discussion of grassland ecology.

Most of the work that is done on grasslands — at least the work that is policy-related — is characterized by a studied *avoidance* of empirical investigation. Thus, Sherman in his study [1980 : 126, n. 39] notes with regard to the myth of barren grassland soils :

It should come as no surprise that in all the time it was assumed that forest-covered soil regenerated its former fertility while grassland caused erosion and leaching, no tests were done on the possibility of increased fertility levels under grassland conditions.

The same lack of empirical documentation holds true for the myths pertaining to grassland's purported economic inutility, predisposition to erosion, and indestructibility. (What Leach [1954 : 22] said of swidden agriculture four decades ago could still be said to hold true for grassland : "It has been the subject of much learned abuse but not much careful observation.") It appears that what is really indestructible is not grassland but these beliefs about grassland, and this — following Kuhn [1962] — can be attributed to the fact that they belong to a distinct scientific paradigm, which is shielded from conflicting evidence.¹⁷⁾

This paradigm, which forestalls research on the critical aspects of grassland ecology, privileges the science, policy, and resource-use regimes of the center as opposed to the local knowledge and resource-use systems of the periphery. For example, my study [Dove 1981] of the grasslands in Southeastern Kalimantan demonstrates how prevailing beliefs about fire-climax grasslands support government plans for hydro-electric development and tree plantations, at the same time as they undermine — by denying the existence of — local use of grasslands for rice cultivation, pasture, thatch, and hunting [cf. Dove 1983]. The government resource regimes are supported, and the local ones are undermined, primarily through simple denial of the existence of rational local management regimes for grasslands. This denial has critical implications for analysis of the success and failure of development, in particular the apportionment of blame. According to the prevailing paradigm, local resistance to government development plans is based not on a conflict of interest between government and local communities, but on a developmental conflict between rational central planning and irrational local resistance, which is associated with lack of education, antipathy toward change, and so on. This interpretation places the blame for development failure squarely and solely on local communities. An increasing number of observers of development are critiquing this paradigm, however, in particular the way that it explains development failure, arguing that less attention should be paid to the subjects of development, or even to particular development programs, and more attention should be paid to the institutions of development itself [Thompson

17) Cf. Ascher's [1993 : 2] comment that, "The incompleteness of official statistics allows the Forestry Ministry to claim, without fear of definitive contradiction, that commercial logging direct accounts for only ten per cent of Indonesia's deforestation."

et al. 1986 ; Ascher 1993 ; Blaikie 1985 ; Dove 1994 ; Ferguson 1990].

IV Conclusions

What do the findings of this study mean for the study of Southeast Asia's grasslands? Above all, I suggest they mean that the developmental challenge of the grasslands is not just to understand and adapt to their human-ecology, but also to understand and *demythologize* the way that this human-ecology has been comprehended.

1. *Past and Future Evolution of Critical Thinking on Grasslands*

Critical scholarship on the anthropogenic grasslands of Southeast Asia has moved through a number of different stages. The first stage consisted of efforts to examine and then report on particular systems of grassland management and ecology without prejudice from prevailing views on the subject. Key contributions in this stage were those of Terra [1952–1953], Bartlett [1956], and Wharton [1968]. The next stage consisted of self-conscious efforts to analyze, and then critique (on the basis of field data) the prevailing views of grassland. Studies by Conklin [1959], Dove [1981], Sherman [1980], Clarke [1966], and Gibson [1983] all represent important contributions to this critique. These developments have given us the basis for the next stage of analysis, in which we attempt to examine the prevailing beliefs about anthropogenic grasslands *as* beliefs, which are constituted by and for particular social institutions ; and in which we attempt to assess the implications of our findings for wider theories about society and environment.

Important tools for this next stage of analysis have been provided by recent work on “interpretation” in the social sciences and humanities [e.g., Clifford and Marcus 1986 ; Marcus and Fischer 1986]. This work has heightened our awareness of the way that language is used to implicitly privilege the speaker or writer, not (e.g.) in the way that questions are answered or problems are solved, but in the way questions are initially posed and problems are initially framed. In a similar fashion implicit, and un-problematized systems of classification and categorization can be self-privileging. The common government classification of *Imperata* grasslands as “wastelands” (and perhaps also the biological classification of them as “dysclimaxes,” and the anthropological classification of them as “green deserts”) is obviously privileging to the classifier with an interest in alternate land-uses. The vision of an unchanging and degraded ecosystem implies the absence of active management of the resource. And this, plus the implication in the term “wasteland” of a lack of value, is prejudicial to any local claim to the resource.

Characteristic to all of these examples is a complete lack of “reflexivity” on the part of government officials and development planners. The planners characteristically problematize the “other” — meaning the grassland, and its inhabitants, and their relationship with it. They characteristically ask what is wrong with the local situation, what is lacking, and what is needed. But what is really needed is a reversal of the problematic. What is needed is to ask, “Who benefits from the myth of grassland wastes?” And, “How is power served by seeing grasslands the way they are seen?” We must not forget to turn the spotlight on ourselves too. Thus, we need to ask,

“How does persistent forgetting of understanding of grassland serve the scientific community?” And, “How does scientific emphasis on research to *change* grasslands versus *understand* grasslands serve the scientific community? These questions, and this approach, will hopefully characterize the next generation of study of anthropogenic grasslands.

2. *No Problem?*

Grasslands are one part of a complex of resource development problems — including shifting cultivation, use of anthropogenic fire, etc. — that appear to be insoluble. But the reversal of problematic that we are promoting here should lead us to ask if this seeming insolubility is an obstacle or in fact a key to the puzzle. Thompson, Warburton, and Hatley [1986 : 36] write that, “If there is no solution, then there is no problem. It is important not to lose sight of this possibility.” I suggest that in the current case there is indeed no problem in this sense : grassland management is often not problematic. In another sense, of course, there is a tremendous problem, involving uneasy relations between state versus peasants wherever grasslands exist. It is the treatment of grassland management within the peasant-state relationship that is problematic.

I suggest that this is not the least but the *most* important aspect of grassland management. The same holds true for a number of other areas of natural resource management, including soils, forests, water, even the air we breathe. I suggest that the ecological challenge of the next millennium will *not* be the popularly imagined one of scientifically-driven balancing of the elements of our global ecology. Instead, it will be the politically-driven challenge of negotiating ecological meaning among the equally self-interested parties to the social construction of our global ecology. Although this negotiation is a social process, its outcome will not only be social, it also will have concrete environmental consequences. The outcome of this negotiation is, indeed, likely to be a major determinant of the sustainability versus non-sustainability of the coming global ecology.

Acknowledgments

This analysis draws on the ethnographic literature of Southeast Asia as well as my own field studies in Indonesia. I first carried out research in Indonesia (Kalimantan) from 1974 to 1976 with support from the National Science Foundation (Grant #GS-42605) and with sponsorship from the Indonesian Academy of Science (LIPI). I gathered additional data during six years of subsequent work in Java between 1979 and 1985, making periodic field trips to the outer islands, with support from the Rockefeller and Ford Foundations and the East-West Center and with sponsorship from Gadjah Mada University. A recent series of field trips to Kalimantan, beginning in 1992, has been supported by the Ford Foundation, the United Nations Development Programme, and the John D. and Catherine T. MacArthur Foundation, with sponsorship from BAPPENAS and Padjadjaran University. An earlier version of this paper was presented at the “Dry Areas of Southeast Asia” workshop, held at the Center for Southeast Asian Studies, at Kyoto University, on 20–23 October 1996. I am very grateful to Dr. Hayao Fukui for the opportunity to attend this workshop. I also am grateful to three anonymous reviewers of *Southeast Asian Studies* for constructive comments on an earlier draft of this article. I alone, however, am responsible for the analysis presented here.

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