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Environmental services and land use change in Southeast Asia: from recognition to regulation or reward?

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Abstract

Awareness of environmental services and land use change in Southeast Asia is high among scientists, policymakers, and society. In the case of transboundary smoke, the level of awareness and concern in the region is high, but subsides in between periods of 'crisis'. Although there is a rising level of awareness of habitat loss and associated loss of genetic diversity, the basic cause–effect relationships underlying the ecological roles of biodiversity are still debated. Degradation of watershed functions is the most mature of our three meso-scale environmental topics; indeed it shows signs of being 'fossilized' by vested interests in the present consensus. Land use planning and other regulatory approaches have had little success. Policy instruments for achieving meso-level environmental policy objectives through changing incentives such as payment schemes for environmental services, have not been tested widely in Southeast Asia (or anywhere else). Further research and experimentation needs to incorporate strategic consideration of processes and spatial scales of environmental impacts and resource governance.

Keywords: Land use change; Environmental services; Smoke; Biodiversity functions; Watershed functions; Environmental issue cycle; Southeast Asia

1. Environmental services: natural capital or human effort?

Once they were taken for granted or, if perceived at all, were viewed as free 'gifts of nature'. But by the beginning of the 21st Century, various forces—global and local; social, political and economic; climatic and ecological—have produced heightened awareness of degradation of environmental services in contemporary Southeast Asia. Landslides, flooding and smoke now figure regularly and prominently in the news media. Global climate change and loss of biodiversity remain rather abstract concepts, but loss of natural habitat and its consequence for 'flagship' species is evident to many. So, in the environmental 'issue cycle' (Tomich et al., this volume), the three specific topics of this collection (smoke, biodiversity, and watershed functions) would seem to be firmly established on the policy agenda and also in public awareness, poised for concrete actions toward mitigation of the associated problems.

The term 'environmental services' often is used as a generic concept. Yet, for any effective relationship between outside beneficiaries of these 'services' and the upland land use systems and communities that generate the services, it is necessary to be explicit in defining what the functions are, and how they can be measured

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and monitored. So, we need to decompose the broad concept of 'environmental services' into constituent components in order to be clear on the cause–effect chains underlying the provision of services. To create appropriate incentives in order to effectively maintain or enhance these services, rewards must be properly directed toward those providing services. Conversely, to establish and maintain a willingness to pay on the side of the beneficiaries, a broadly shared and accurate understanding of the cause–effect relations is required.

Tomich et al. (this volume) introduced this collection by formulating a series of questions to help unpack perceptions and clarify whether environmental issues actually correspond to real 'causes' and important 'effects'—in other words, whether they constitute significant 'environmental services'. In all cases, measures to sustain or enhance environmental services require appropriate quantitative methods and data analysis. But, as encapsulated in the environmental 'issue cycle', the emphasis on evidence will shift from understanding causal pathways ('processes'), recognizing spatial extent and distribution ('patterns'), developing 'proxies' or 'indicators' for easy recognition and monitoring, and simplified yet accurate and validated measures that will facilitate negotiations among various groups, often with conflicting interests.

In the case of transboundary smoke, the level of awareness and concern in the region is high, but the private economic benefits of converting swamp forest to other land uses through slash and burn clearing methods and from the logging that often initiates a forest degradation cycle (which enhances fire susceptibility) are too great as yet to be effectively controlled in many countries. For biodiversity, the global existence values and option values of maintaining genetic diversity may be clear, but basic cause–effect relationships underlying the ecological roles of biodiversity are still debated. So, from a policymaker's perspective, it may not yet be clear what we should care about. Degradation of watershed functions is the most mature of our three environmental topics. It may even be 'over mature', as it shows signs of being 'fossilized' by vested interest in the present consensus, while challenges to the popularly held 'cause–effect' model of forests and watershed functions are resisted.

As we discuss in the concluding sections, regulation has been the conventional approach to mitigation of environmental problems, but some of the same forces that have produced heightened environmental awareness also have spawned an interest in positive alternatives in the form of incentive schemes to reward positive actions that maintain or increase the provision of environmental services. Pilot schemes, still in their early stages of testing, to reward upland farmers and communities for specific types of land use and land use change to sustain or enhance environmental services are a case in point. But in order to be effective and sustainable, these mechanisms need to fit within the changing governance landscape of Southeast Asia, and to be based on stronger and more spatially explicit biophysical research that can effectively assess and predict human impacts on environmental services at multiple spatial and temporal scales.

2. Managing smoke

As Byron (this volume) and others have shown, forest, land, and coal seam fires associated with drought and human activity are not new in Southeast Asia. But smoke problems are perceived to be worse than ever before, and that may well be true. From their historical review, Brookfield et al. (1995) concluded that '... the impact of drought and fire over the past 10 years has been much more devastating than at any time in at least the previous 100 years, and probably much longer'. And that assessment preceded the 1997/1998 crisis, which drew sustained international attention. (For comparative perceptions of the events of 1997/1998 by leading scientists working in Indonesian Borneo and the Brazilian Amazon, see interviews in Wuethrich (2000).)

Follow up investigations of the underlying causes of the 1997/1998 fires on Sumatra and Borneo has confirmed and extended the preliminary diagnoses put forward by Tomich et al. (1998) and Stolle and Tomich (1999). Remote sensing and social science investigations using participatory mapping conducted by a team of researchers from the Center for International Forestry Research (CIFOR), the World Agroforestry Centre (ICRAF) and the United States Forest Service (USFS), showed that both smallholders and large-scale plantations used fire as a tool, primarily for land clearing but also in specific contexts in extractive activities (Applegate et al., 2001; Dennis et al., 2004; Suyanto et al., 2002). For the first time, these social science investigations also have documented specific patterns in the use of fire as a weapon; arson arising from land disputes was shown to be an important albeit secondary factor. Finally, these detailed studies also confirm Vayda's (1998) observation that the incidence of accidental fires (fires that are set by smallholders for a purpose but which spread accidentally) may have been higher than is conventionally believed, particularly in Borneo. The importance of these accidents and the social context in which they occur in Northern Thailand is addressed in Hoare (this volume). Moreover, it has become clear that fires in the swamp forest zone produce a disproportionately large amount of smoke/haze per hectare burnt, and this ecological zone should thus receive specific attention (Murdiyarso et al., 2002).

It is encouraging that the CIFOR–ICRAF–USFS research on underlying causes has begun to discern meaningful patterns in the shape and extent of remotely sensed burn scars and to relate those burn scars to the underlying causes and broader environmental and social context documented in the social science studies (Dennis et al., 2004). This holds promise as a cheap and replicable tool for future fire forensic investigations, but it is not yet possible to attribute shares of the smoke problem between smallholders and large operators or arising from purposive burning (for land clearing, resource extraction, or arson) and accidents on the scale of large areas, such as Sumatra and Borneo.

Hoare's analysis (this volume) of the relative success of a provincial level fire and smoke management project demonstrates that it is not necessary to have a comprehensive national or regional analysis as a prerequisite to effective action. Hoare's documentation of significant incidence of accidental fires (and similar results for Indonesia) indicate a role for training programs in fire awareness and management. But the evidence from the CIFOR–ICRAF–USFS studies in Sumatra and Borneo, indicating that most fires are set deliberately, means that training alone will not be sufficient and that more fundamental attention to underlying driving forces, particularly the effects of insecure land tenure and property disputes (Dennis et al., 2004; Suyanto et al., 2002) will be necessary to manage the smoke problem. The private economic gains related to the causes of fire so far have not been effectively off-set by negative incentives that reflect the public costs.

Table 1
Estimates of fire and haze related damage (millions of US dollars)

Type of loss	Lost to Indonesia	Lost to other countries	Total
Timber	493.7	–	493.7
Agriculture	470.4	–	470.0
Direct forest	705.0	–	705.0
Indirect forest benefits	1077.1	–	1077.1
Capturable biodiversity	30.0	–	30.0
Fire fighting costs	11.7	13.4	25.1
Carbon release	–	272.1	272.1
Short-term health	924.0	16.8	940.8
Tourism	70.4	135.8	256.2
Other	17.6	181.5	199.1
Total fire and haze	3799.9	669.6	4469.5

Source: EEPSEA (1993–1998, p. 13).

The widely cited study by EEPSEA and WWF (1998) (Economy and Environment Program for Southeast Asia and World Wide Fund for Nature), estimated over $ 4.4 billion in damage from Indonesian fires and smoke in 1997 (Glover and Jessup, 1998, 1999). The EEPSEA/WWF estimates have been refined since by others, but that study was among the first and arguably was most influential in forming regional and global awareness of the smoke problem. A *global* environmental disservice, estimated as imputed value of carbon release of this event, was by far the biggest cost external to Indonesia considered in that study. After this global cost, those authors estimated that almost $ 3.8 billion (85%) was borne by Indonesia itself (Table 1). Although the situation in Singapore and Kuala Lumpur received most of the media attention, the EEPSEA/WWF estimates indicate that Indonesian citizens suffered the most short-term health effects by far ($ 924 million out of a total estimate of just over $ 940 million; about 98%). If this is the case, is this really primarily a regional problem? Balancing private gains of causing smoke and the public costs of the impacts should not require transboundary mechanisms, as the within-country net benefits would be sufficient, if effective institutional mechanisms could be found.

The impact of the EEPSEA/WWF study highlights the great time value of information. Above all else, efforts to publicize who is burning probably have the highest impact during a smoke emergency. Fortunately, remote sensing and the worldwide web provide powerful tools for doing that (see interview with Nabil Makarim in Tomich et al. (1999), also Tomich and Lewis, 2002).

Would further research to refine the influential EEPSEA/WWF cost estimates make a big difference to policymakers' perceptions or efforts to mitigate the problem? In other words, if more and better regional data on alternatives, causes, impacts and costs were available, would they likely prompt more effective national or regional action? The report on just such a science-policy activity by Murdiyarso et al. (this volume) does not provide much basis for optimism about effective *regional* action any time soon.

Fortunately, a grand regional strategy for Southeast Asia is not the only option to manage smoke—nor does this approach even seem to be the most obvious one given the insight that a big share of human costs are concentrated within the areas where the smoke problem originates. Hoare's study (this volume) of community and provincial level efforts in Northern Thailand moves longstanding interest in local institutional measures for management of fire and smoke from the anecdotal to the practical assessment of actual experience based on smoke mitigation interventions at this level. Even more fundamental is evidence from Indonesia (Applegate et al., 2001; Suyanto et al., 2002; Dennis et al., 2004) showing direct links between smoke and land tenure problems, thereby also establishing much more clearly how resolution of conflicts over property rights and resource access underpins a comprehensive approach to the problem.

Rhetoric, research and policy initiatives all would seem to have over emphasized comprehensive regional solutions to the detriment of efforts to identify, develop and replicate local approaches to the smoke problem. As Byron (this volume) stresses in his synthesis, a comprehensive solution rests fundamentally with local political accountability and local incentives to better manage fire and smoke. In other words, a comprehensive solution will be feasible when local government capacity and legitimacy exists concerning sanctions on those who reap the benefits of burning and when local government is accountable to those bearing the bulk of the costs. In the meantime, instead of ASEAN wide pronouncements, Hoare's study suggests that more could be accomplished (albeit incrementally) through efforts that begin by understanding local conditions, interests, and institutions. Not all fires are equal, and as Byron emphasizes, it makes sense to target big, smoky fires especially when climatic conditions already have adverse effects on air quality (also see Tomich and Lewis, 2002). Successful regulation of timing of burning to reduce smoke pollution in the Brazilian Amazon indicates that regulatory means of smoke management can minimize costs to land users while improving public health outcomes (Reinhardt et al., 2001), but that result also depends on even enforcement among rich and poor farmers, which in turn derives from some measure of broad-based local political accountability.

3. Distinguishing the two faces of biodiversity: existence and resilience

The concept of biodiversity has at least two distinct aspects or 'faces'. The global 'face' is charismatic, an international superstar, exemplified by the 'flagship taxa' of animals and plants that stimulate campaigns to prevent their extinction. Conservation of habitat to preserve this global biological heritage for future generations (of people) is a legitimate goal that inspires considerable public support within Southeast Asia and worldwide. The support originates primarily among middle and upper class urban populations, and when habitats are maintained in someone else's backyard, especially where animals such as tigers and elephants are concerned.

When it is time to be counted, charisma and size clearly offer real advantages. Four papers in this collection make contributions toward more cost-effective and rapid assessment of plants, animals, and ecosystems (forests and coral reefs) that people seem to care about most and to assessment of the costs of maintaining them. Beukema and van Noordwijk (this volume) successfully demonstrate the use of Pteridophytes as a recognizable plant taxon indicator to address a question that is highly relevant to densely populated Southeast Asia: to what extent do ecologically disturbed (but from a farmers' perspective enriched) forest systems retain some of the ecological character and function of the original natural forest habitats? Their answer is that the land use system matters a great deal regarding the potential to combine conservation and development. While this approach can effectively demonstrate how complex production systems and landscape mosaics may contribute to maintenance of forest-based biodiversity, no taxonomic group can be expected to be a good indicator for biodiversity as a whole, as taxa differ in their response to human-induced ecological change (Lawton et al., 1998). As an alternative to the taxonomic approach, Gillison and Liswanti (this volume) use plant functional types as a strategy to cope with the questions of vegetation structure and diversity in life forms. They also link their functional indicators approach to abiotic factors to inform sampling and produce a much more efficient assessment of a wider range of variation. Loreau et al. (2001) suggest that such abiotic factors "tend to be the main drivers of variations in ecosystem processes across environmental gradients".

Among the three themes of this collection, the functional role of biodiversity at the landscape level is by far the most difficult conceptually and empirically. The analysis of conflict between wildlife and people by Nyhus and Tilson (this volume) tackles this issue for two big animals (tigers and elephants) that each are icons of high global values even as they may impose costs on local human populations, who suffer property damage, personal injury, and death. Evidence presented by Nyhus and Tilson that risks to people from wildlife (and vice versa) peak in moderately disturbed systems (like agroforestry) highlights a troubling dimension of hopes for the integration of conservation and development objectives in the segregate–integrate analysis of multi-use landscapes (Van Noordwijk et al., 1997), especially since more integrated landscapes can also increase local values of other (less charismatic) types of biodiversity that can be (and often are) harvested sustainably for a range of products for local use.

All five of the papers on biodiversity in this collection attest that pragmatic approaches can produce valid conclusions that have policy relevance without attempting to measure everything, despite mind-boggling biological richness and ecological complexity. The Participatory Rapid Economic Valuation (PREV) methodology illustrated by a case study by Cannon and Surjadi (this volume) of valuation of ecotourism derived from biodiversity conservation is a particularly elegant practical demonstration that careful framing of questions to reflect specific objectives can produce useful results that economize greatly on information requirements without compromising either validity or legitimacy. Indeed, the participatory approach they used is fundamental to the usefulness, validity, and legitimacy of the results. Although the approach of comparing extreme ranges of estimates of high and low values produced a high level of confidence that ecotourism dominates other alternatives in this case—the confidence derives from use of conservative figures for the former and generous figures for the latter—this strategy will not always produce useful results without additional data and refinement. However, a general feature of this participatory approach is that it economizes on information by focusing valuation efforts on the components of greatest concern to policymakers and stakeholders. Moreover, such partial, incremental comparisons not only economize on data but also frame specific practical questions in ways that are more meaningful for policy analysis than calculation of the total value of ecosystem services (Daily et al., 2000). Estimates of total value can, however, change the public mindset (Costanza et al., 1997), even if the details do not really matter for specific decisions to be taken. Results from research by Fergus Sinclair and Laxman Joshi (see Tomich et al., 1999, p. 63, for an abstract) are encouraging regarding some scope for extrapolation of results beyond particular settings. They find evidence that suggests that farmers facing similar agroecological conditions, but in widely different locations, appear to have similar knowledge of functional aspects of biodiversity and that their apparent understanding of general patterns may be transferable across similar agroecologies. These pragmatic, participatory approaches that begin at the landscape level are not without pitfalls (what if some important dimension or threshold is overlooked?), but they are at least a way forward since it remains costly and difficult to scale-up assessments to the landscape level. And even the most cost-effective methods developed for assessment of species richness (i.e. existence) may be of limited use in assessing functional values at the landscape scale and the skills required will be agronomic, ecological, and economic rather than taxonomic.

Although the synthesis by Swift et al. (this volume; also see Loreau et al., 2001) is primarily conceptual, it too provides practical insights from better understanding of the functional roles of biodiversity that help set priorities for measurement among the bewildering range of organisms involved at the landscape level. But it is at this level where the remaining gaps in our knowledge are particularly large. Despite the efforts of Swift et al. (this volume), no functional typology at the landscape scale exists. Compared with global existence values, much less attention has been given to these functional values of biodiversity within landscapes where local people seek their livelihoods day-to-day, season-to-season, year-to-year. A dynamic view of resilience, patch dynamics and the ability to recolonize areas after an ecological disturbance is needed, but these key properties cannot be assessed easily in a survey methodology.

Anthropocentric as it may be, few would argue with the idea that being killed by a tiger or elephant would be a disservice. But beyond that, as noted by Tomich et al. in their introduction to this volume, there had been no clear consensus about the basic functions and dysfunctions of biodiversity at this scale. To remedy that deficiency, Swift et al. (this volume) developed a functional typology of the groups that support productivity, sustainability, and resilience in landscapes including agricultural uses. In addition to plants and their pollinators, the list of keystone groups by Swift et al. features parasites, micro-symbionts, decomposers, ecosystem engineers, and elemental transformers. These latter classes are the homely local 'face' of biodiversity—the millions of microbes next door and mycorrhizal fungi under our feet—and are not at all charismatic (to most people). By any measure, most of these organisms live belowground or otherwise out of sight. They are tiny and tedious to classify and count.

If one species within one of the (presumably keystone) functional groups supporting agricultural production were to become extinct—which surely must be a nearly continuous process—would that matter and how would we ever know? Swift et al. describe the basic functional biodiversity rule, why any landscape needs at least one organism in each group in order to function sustainably. But is there redundancy in having many species in a functional group? Perrings (1998) has argued that '. . . the main external cost of biodiversity loss lies in the reduced resilience of agroecosystems in the face of environmental and market shocks'. The main evidence on these functions comes from studies of North American grasslands, which showed biodiversity plays a role in recovery of total biomass production after drought (Tilman and Downing, 1994; Gowdy, 1997; Vandermeer et al., 1998). Swift et al. (this volume) discussed the more recent interpretations of these experiments and the need to distinguish co-evolved communities from random assemblages of species, as used in the experiments. But what does this mean in practice in the tropics? Over what spatial and temporal scale should we be concerned and, if dangerous thresholds exist, how can they be detected in time to avoid catastrophe? Loreau et al. (2001) suggest that 'the relative effects of individual species and species richness may be expected to be greatest at small-to-intermediate spatial scales . . . ' but more work is needed to confirm this conclusion. If the probability of catastrophe is small, but not trivial—as may be the case for biodiversity functions at the landscape scale—then Perrings et al. (1997) point to an additional methodological challenge: conventional decision models do not work well for this class of problems.

Overall, we have very few answers for the practical questions regarding biodiversity function at the landscape scale that were put forward in the introduction to this collection by Tomich et al. From a national perspective, and putting aside existence values, potential use of unique genetic resources and ecotourism potential, there is little basis for national policymakers to place the same level of concern in degradation of biodiversity in agriculture as in, say degradation of watershed functions discussed in the next section. This lack of information on functional aspects of biodiversity in Southeast Asia—particularly information that policymakers can use—is paralleled in sub-Saharan Africa (Frank Place, pers. comm.). We simply do not know what risks there are to stabilizing functions of biodiversity compared to other pressing national concerns in developing countries in the tropics nor do we have any idea of the magnitudes of potential losses if a threshold is crossed.

In particular, the central question of the value of redundancy within functional groups remains one of the 'grand challenges of environmental science' (National Research Council, 2001, pp. 20–27; Loreau et al., 2001). And, at the landscape scale, we still do not know specific threshold effects of biodiversity loss on stability of production such that land use change that could be sustainable for a limited number of actors on a limited area would be an ecological catastrophe if everyone did the same thing; nor do we have the indicators needed to assess or predict ecosystem function at this level (Hobbs and Morton, 1999; United

Nations Development Programme et al., 2000; Harvey, 2001). For example, suppose for a moment that a perennial monoculture plantation provides watershed services that are indistinguishable from natural forest. What, if anything, would be lost (or gained) on-site from conversion of natural forest to monoculture plantation in terms of stability of the production system? Perhaps an even more important question is what effect (if any) would conversion from natural forest to a monoculture plantation have on the level and stability of production off-site on land adjacent to the monoculture plantation? Would neighbors face fewer production options because of loss of wild seed sources? ... new difficulties in managing fallows or soil nutrients? ... would they suffer more (or fewer) outbreaks of pests and diseases of crops and livestock? ... or would familiar pests and diseases be replaced by exotics? In short, should the neighbors worry? In this vein, it also is worth noting that a direct use role of 'non-charismatic' elements of the local flora may be specifically important for livelihood resilience as 'famine crops' or wild species harvested for use or sale in times of hardship due to economic or climatic fluctuations.

One obvious priority for further work is whether the risk of pest and diseases increases as biodiversity richness declines within these changing landscapes (Naylor and Ehrlich, 1997). Although not often mentioned prominently by national and regional policymakers, farmers in the humid tropics typically rank crop pests and diseases (including weeds) as their paramount resource management concern. With rare exceptions (collective action for pig hunting in Sumatra, locust control, synchrony in rice planting to reduce opportunities for rats), interventions beyond the plot/household scale seem rare.

As a preliminary set of working hypotheses on these agroecological functions at the landscape level, we offer the following nested hypothesis as a basis for further applied research:

Null hypothesis. Landscape-interactions that regulate or promote problems of pests and diseases either (a) don't matter or, if they do matter, (b) are difficult to perceive or, if perceived, (c) it is difficult to effectively organize collective action to address these problems because of the usual reasons ('free riders'; monitoring, and enforcement problems).

Weeds and other crop pests and diseases, admittedly, are not very charismatic, but this may be where resilience resides in greater biodiversity at the landscape level. There has been practical demonstration of this in Asia for significant areas, albeit for the very simple case of disease control through greater *genetic* (i.e., within species) diversity in irrigated rice achieved by planting more than one rice variety within each field and coordinating this effort through collective action at the landscape level (Zhu et al., 2000); Weitzman (2000) provided a general ecological economic framework for this phenomenon. But we have very little evidence on the effects of reduction of biodiversity at the landscape level in the much more complex upland systems, in part because the measurement problems are much greater. (However, see Vandermeer et al., 1998, for general discussion and Kiss et al., 1997 for an example from temperate agriculture.)

There is, of course, the possibility that international resources and workable means will be found to protect habitat for the charismatic elements of biodiversity (or for other global public goods, such as carbon storage), and that there is enough overlap in assemblages such that richness of the very different groups of species underpinning agroecosystem resilience may be conserved as a byproduct (Daily et al., 2000). However, despite some progress in this direction, that day seems far off. In the meantime, there would seem to be an urgent need to move beyond the pioneering stage to identify whether these environmental services merit greater recognition by policymakers and, if so, to develop and validate clear, compelling examples (possibly drawn from 'natural experiments') to demonstrate why they should care.

4. Broadening and focusing questions on watershed functions

Whenever there is a flood or drought, there is a peak in public interest in 'deforestation', 'watersheds', and 'reforestation'. In contrast to biodiversity, watershed issues have had a remarkable amount of sustained attention from policymakers, not to mention billions of dollars in public funds. But which among (a) *on-site* effects of soil erosion on productivity, (b) *off-site* effects of soil transfer on agricultural productivity and other effects, such as sedimentation of reservoirs, (c) *flooding*, (d) *seasonal water shortages*, and (e) *water pollution* from land use are of greatest concern at various scales (communities, provinces, nations)? Kramer et al. (1998, p. 2) observed that 'most analyses of watershed services have focused on soil erosion effects. Studies of other watershed services, such as streamflow stabilization, water quality and quantity effects (particularly in the case of tropical settings) have seldom been done'. Despite decades of research, it appears that science has produced surprisingly little useful information for policy questions about different watershed functions.

Policy analysis seems to be incomplete even for the topic that has received most emphasis by researchers, the plot-scale effects of erosion on agricultural productivity. Lal (1998), one of the best known researchers in the field, concluded that 'agronomic effects of erosion on crop yield have not been adequately assessed. . . . A major cause of controversy and confusion about the agronomic impact of erosion is due to weak, incomplete and unreliable data on soil erosion and its impact on productivity'. Based on careful econometric analysis of data from soil samples taken intermittently since the early part of the 20th century in Indonesia (and also since the late 1930s in China), Lindert (1998) concluded that the analysis of erosion 'failed to show that it was a key source, or an accelerating source, of soil degradation in Indonesia over this half century . . . Perhaps research on soil degradation should concentrate less on erosion and more on other human-induced processes, such as fertilizer, water control, and nutrient depletion'.

Policy-relevant results are even thinner regarding sedimentation and other downstream effects of soil transfer. Erosion from steep slopes and deposition in the lowlands could increase or decrease aggregate agricultural production at the watershed scale. But, even if erosion were to halt completely in Southeast Asia, it is impossible to know the likely effect on agricultural productivity. Again, Lal (1998) suggested that much remains to be done:

". . . the magnitude of soil erosion for principal soils and ecoregions is also not known. The available information on the magnitude or severity of soil erosion, voluminous and often replete with rhetoric is confusing, qualitative, incomplete, and unreliable. . . . The information on soil erosion is also erratic because of lack of scaling procedures. It is difficult to aggregate the data from point or field scale to landscape, watershed, ecoregional and global scales".

The model developed by Shively and Coxhead (this volume) may well represent the state-of-the-art of policy analysis of the economic effects of erosion at the landscape scale. Their stylized model (i.e. highly simplified to reveal certain key relationships more clearly) uses conventionally available data to examine erosion outcomes on upland crop productivity. Although based on an extremely narrow view of farm household decision making, the results for on-farm effects are particularly revealing. Their stylized upland farms 'choose' higher profitability (but eroding) activities, even for discount rates of only 5% on future productivity decreases on-site. In other words, the optimal rate of erosion from a private perspective (without considering the off-site effects) is almost certainly greater than zero. This, of course, is anathema to soil conservation programs seeking to 'eliminate erosion' by preaching to farmers about losses of on-site productivity. But, despite the (vastly) simplified assumptions in the model, this result does not seem unrealistic, and is consistent with the apparent need for substantial government subsidies (as usually associated with soil conservation programs worldwide), or as they demonstrate, disincentives to production of erosion-prone crops. For poor upland farmers facing difficult choices and much higher interest rates in the real world, gradually declining productivity may well appear to be a fair trade-off for more money right now.

As Shively and Coxhead recognize, the limitations in the data typically collected by soil scientists and agronomists severely restrict the *off-site* effects that can be modeled. Unfortunately, these off-site effects are the ones of greatest potential interest for environmental policy and only the simplest sort of lateral flow can be captured in the Shively–Coxhead model compared to the range of policy-relevant possibilities identified by Van Noordwijk et al (this volume). It is noteworthy that Shively and Coxhead are able to incorporate one major economic externality, the accumulation of sediment at downstream locations. However, filter effects in the uplands, which could moderate sedimentation in downstream irrigation systems, and effects on lowland agricultural productivity—which could be positive or negative—could not be modeled because of the sorts of data problems mentioned in Lal's critique quoted above. It is important to emphasize that this is not a problem of lack of measurement or an isolated botched case; rather, these problems appear to derive from application of standard soil measurement practices.

Similarly, the economic valuation of the effects of land use change on seasonal water shortages (known as base flows, low flows, or minimum flows) by Pattanayak (this volume) is constrained by availability of hydrological data. In this case, the problem is not one of technique. Instead, as Mungai et al. (this volume) argue, long-term studies (lasting decades, not seasons) may well be necessary to produce credible evidence on longer-term phenomena, such as the effects of deforestation or reforestation on base flows, a topic that will be taken up again below. Moreover, Bruijnzeel (this volume) urges research that supplements the well-established paired catchment approach with process measurements and physically based model applications to improve understanding of effects on low flows of filter elements and specific vegetation types within upper watershed landscapes.

Indeed, while many challenges also remain in economic theory and quantitative spatial methods, it appears that real progress on economic valuation of watershed functions depends on reorientation by soil scientists, hydrologists or physical geographers to measurement of the various lateral flows involved in an explicitly spatial framework. As Van Noordwijk et al. (this volume) have pointed out, spatially explicit biophysical modeling of lateral flows can be an especially enlightening way to 'read the landscape', and thereby focus measurement activity where it will count. Measurements of high within-plot soil movement but low sediment transfers to streams were discussed by Rodenburg et al. (2003). At least, for the quick processes (erosion, sedimentation, flooding, possibly water pollution) there are prospects of obtaining useful new data on effects of land use with a few seasons of well focused, directed and located observations.

The work by Ziegler et al. (this volume) epitomizes what can be accomplished through savvy scientific efforts to measure lateral flows. They provide evidence that unpaved roads produce as much sediment as agricultural land in an upper catchment in Northern Thailand, despite the fact that these roads occupy less than one-tenth of the area occupied by agriculture. Bruijnzeel (this volume) presents additional evidence of disproportionate erosion rates on (incompletely) compacted surfaces such as roads, paths, tracks, and human settlements. Further stages of compaction may lead to runoff without much soil loss, but surface flows may pick up soil as soon as they pass over soil with a higher propensity to entrainment elsewhere. Although conversion of forests to agriculture invariably is accompanied by tracks, roads, and settlements, the focus of most researchers on the former with almost complete neglect of the latter suggests an inadvertent 'misreading' of landscape processes, at least in the case of soil transport and sedimentation.

Just as with the critique of conventional wisdom and measurement practices regarding soil erosion and sedimentation, fundamental questions have been raised in the past few years about the hydrological functions of forests compared to alternative land uses. Over the past decade, numerous reviews of available evidence have concluded that deforestation has little impact on flooding (Chomitz and Kumari, 1996; Calder, 1998) and that forests (whether natural or plantation) 'use more water than most agricultural crops or grassland' (Bruijnzeel, 1990). Chomitz and Kumari (1996) summed up the emerging revisionist mood: '... the levels of the [hydrological] benefits are poorly understood, likely to be context-specific, and may often be smaller than popularly supposed'.

Based on the most recent results, Bruijnzeel (this volume) revisits these uncertainties about basic relationships between rainfall, watershed functions, deforestation, reforestation and other aspects of land use change in the humid tropics. We focus here on two elements of his comprehensive review: flooding risk and, conversely, low flow (risk and severity of water shortages). Bruijnzeel finds convincing evidence linking deforestation to increased local risks of flooding (i.e., within small catchments). But, while the possibility cannot be ruled out, he finds no comparable body of evidence linking deforestation to flooding in larger areas. Similarly, a summary of opinions expressed in an 'electronic workshop' organized by the FAO indicated no case of measurable land use impacts on peak flow (or base flow) in basins over 100 km^2 (Kiersch and Tognetti, 2002). Thus, if such impacts exist, they have yet to be clarified and measured through research. What is clear, however, is that 'truly devastating' major floods are, in Bruijnzeel's words, generally the result of a 'large and persistent field of extreme rainfall

. . . particularly when it occurs at the end of the rainy season' when soils already are saturated. This helps explain why land cover may matter least during the extreme events that produce large-scale floods.

Since extreme rainfall is the dominant factor in the worst floods—including localized flash floods (and deep landslides), as well as larger general floods—this suggests that greater attention be paid to risk assessment of settlement locations (especially expansion of settlements in floodplains) than to the often futile efforts to influence land use in upper watersheds discussed in the next section. In addition, monitoring of rainfall across catchments to provide early warning to lowland areas when rainfall exceeds dangerous thresholds could reduce risk of human tragedies that so often have made headlines in Southeast Asia. Of course, some idea of the threshold level is necessary to implement this approach.

Pioneering efforts in Thailand are involving local people in monitoring rainfall and making assessments of related risks. After recent flash floods and landslides associated with extreme rainfall patterns in some highland areas, trials of early warning functions are being incorporated into pilot local watershed service monitoring networks in sub-watershed areas of Mae Chaem, Northern Thailand, which primarily utilize data collected, analyzed, and used by local communities themselves. Since there are tensions between some upper watershed villages and their lowland counterparts that center on lowland criticism related to the impact of highland agricultural land uses on downstream watershed services, upstream villagers' efforts in support of this early warning system may help improve channels of communication and relationships with downstream communities. Villagers are also beginning to move some recent settlements in high-risk floodplain areas. Moreover, a major focus of these monitoring networks is on attention to other related issues, such as water quality (using biological indicators), seasonal stream flow (with particular interest in low flows), and soil movement in different types of agricultural fields. In this way, the broader monitoring system established under the later stages of the 'environmental issue cycle' (Tomich et al., this volume) may speed recognition of emerging problems as conditions change, possibly leading to reduced impacts and/or lower mitigation costs in a new 'issue cycle'.

Determining effects on low flows of filter elements and specific vegetation types within watershed landscapes is identified by Bruijnzeel (this volume) and Bruijnzeel et al. (2004) as the single most urgent watershed research need, because we still are unable to make real predictions for any particular area, and especially under mosaic landscape conditions common in Southeast Asia. In order to accomplish this complex task, he recommends that the traditional paired catchment approach be supplemented with spatially explicit distributed hydrological process models capable of representing complex feedback mechanisms between climate, vegetation and soils at multiple spatial scales. Refinement of the latter will also require carefully targeted systematic measurement of hydraulic characteristics under post-forest land cover types. Although it is clear that reforestation and soil conservation can reduce enhanced peak flows and stormflows associated with soil degradation, there is no well-documented case where this has produced an increase in low flows. If it turns out that land surface conditions and soil characteristics are indeed more important than tree cover per se in determining land use impacts on base flow, this would have major implications for watershed policies and programs.

5. Quest for policy levers that can influence land use: rewards or regulations?

Better understanding of the various drivers of land use and cover change may well be more important to certain regional and national policymakers than the changes in landscape structure and environmental services that result. What policies and institutional options *really* can influence the rate and pattern of land use change? Policy options of particular interest cluster under two broad categories: (1) regulations, which are the more traditional administrative approach, and (2) rewards, used as shorthand here to refer to various new ideas for environmental service incentives, which are usually positive (e.g., payments, subsidies investment in services or infrastructure), but in principle could also include 'negative rewards' (e.g., taxes, penalties, and other sanctions).

Some might guess that market-based rewards always would beat regulations in terms of efficiency of implementation and effectiveness of outcomes.

Weitzman (1974), however, showed how the choice between rewards and regulations depends on specific technical, institutional and informational circumstances and, above all, on uncertainty. On the technical side, regulation may be the better option when there are important threshold levels for damage or benefits and the system is incompletely reversible. Basically, a 'don't cross this line' rule saves on monitoring costs since enforcement effort focuses on those close to (or beyond) the threshold. Smoke pollution is one example where thresholds matter; some countries in Southeast Asia already have air quality standards that apply to smoke and particulates. Similarly, water quality (and even quantity) standards could be developed based on thresholds of damage. In principle, the threshold concept suits biodiversity functions the best. The practical problem, discussed above, is that there is not yet much empirical understanding of the stabilizing functions that really matter, so we have no idea what the thresholds are for maintaining biodiversity (or even the monitoring unit). On the institutional and informational side, uncertainty about control costs favors regulations over rewards, but uncertainty about damage has no effect on the choice of instrument from an economic perspective (Helfand, 1999). Although not insurmountable in theory (say through auctions), finding an optimal reward could be difficult in practice, particularly when there are many potential polluters or providers of environmental services, each with very different cost structures. So economic theory would tend to point toward regulation rather than reward for the environmental services discussed in this collection and also for upland farmers who are potential polluters/providers of these services in Southeast Asia.

In reality, however, regulations aimed at forest protection have had mixed success, at best, and land use planning has even less impact on the ground in Southeast Asia. When government regulations have attempted to impose '*protected*' status on areas of land or water, historical, cultural, or de facto established rights of local people (who are often ethnic minorities in mountain areas where most terrestrial protected areas have been declared) typically were not adequately respected or even recognized, nor were these people compensated for foregone resource use and loss of development opportunities that protection would entail. As a result, the regulatory approach to environmental policy often has been ineffective because of high transactions costs resulting from incentive incompatibility and limited administrative capacity, or even counterproductive, because it perverts and destroys local resource management incentives. Worse yet, government regulations aimed at resource protection have mandated expulsion of people from 'protected areas', depriving them of their land and livelihoods and forcing some into poverty and further resource degradation elsewhere.

Yet, there is cause for some hope. Conservation can produce local benefits as well. In substantial parts of the humid tropics, local forms of land use have emerged that allow people to make a living while protecting environmental resources. The resulting levels of environmental services are below those of (perceived) 'pristine' nature, but are superior to many other agricultural development options from an environmental perspective. The 'agroforests' of Southeast Asia (with counterparts across the humid tropics) are a prime example of how 'domesticated forests' can provide food, timber and income, while harboring a substantial share of the original forest biodiversity, which often lacks adequate protection elsewhere. Depending on commodity prices, investment opportunities and government policies, however, the small-scale managers of the agroforests can be (and are) induced to replace these systems by monocultural plots of oil palm, rubber or other crops. Who can blame them for doing so, if the outside world has not found ways to express their appreciation for the environmental qualities of the agroforests in a way that is meaningful for the farmers? Similar issues relate to community-based management of mosaic agroforestry landscapes in mountainous areas of mainland Southeast Asia whose centuries-old long-rotation forest fallow land use systems have only relatively recently come into question, and are now under heavy pressure to convert to intensive permanent commercial field crop production on sloping lands.

Workable means for effective recognition and rewards for environmental services are receiving increasing attention (Fig. 1; also see Johnson et al., 2001; Pagiola et al., 2002; Powell et al., 2002; Scherr et al., 2002). Measures to create appropriate rewards could emerge as an important part of the answer to the practical and ethical dilemma of how to reconcile broad environmental objectives and local livelihood

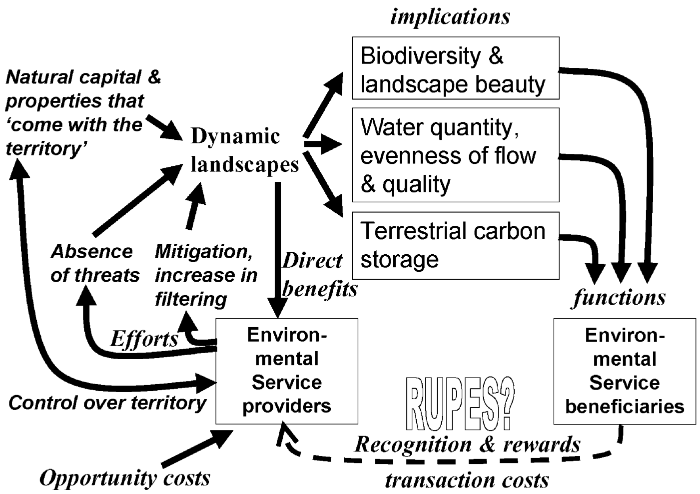

Fig. 1. Examples of the landscape-level implications that are perceived as environmental service functions and the type of efforts and activities that can qualify actors as 'environmental service providers': absence of threats and mitigation or increase in filter functions.

imperatives—and may even play a complementary role with the regulatory approach to environmental protection.

Putting such reward systems into practice often raises a host of cultural issues and institutional questions. Consider the case of the Mae Taeng watershed in Northern Thailand, where water yields had declined for two decades. Although it was not possible to definitively identify the complex causal factors underlying this trend, it was clear that competition for water between upstream, agricultural uses and downstream, urban uses was increasing (Vincent et al., 1995). The research team observed that 'it may be cheaper ... to allow farmers from the Irrigation Project area to sell their water to users in the city' but 'buying water from farmers would require an institutional revolution in Thailand ... ' (Vincent et al., 1995, pp. vi–vii). To date, such a market-based approach has not been tried, and it remains to be seen whether it would be possible to create and manage mechanisms for compensating people for foregone livelihood opportunities in favor of environmental services to other groups. Where there is only limited potential for influencing the total amount of water available from rainfall minus evapotranspiration by natural vegetation, changes in the use of water (for indirectly supporting dry season evapotranspiration in irrigation schemes) provide scope for increasing domestic or industrial water use. Selling water that is not used for irrigation almost invariably raises questions of ownership and rights to sell, however, that are deeply rooted in society and are not easy to answer.

In the cause–effect relations that underlie the generation (or degradation) of environmental services, we can generally distinguish three key elements (Fig. 2): natural capital (including rainfall and inherent richness of flora and fauna), a 'guardianship' role of preventing destruction of the natural capital that itself largely depends on social capital, and an active management or stewardship role that is part of the human

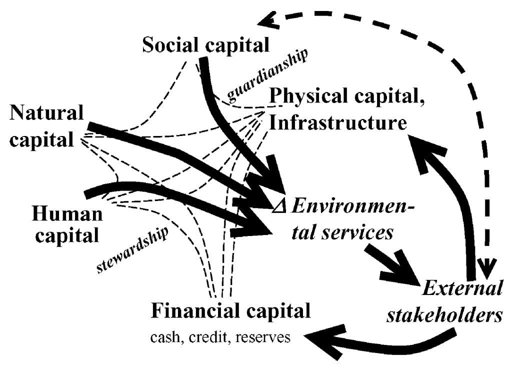

Fig. 2. Relationship between the five types of capital (Carney, 1998), changes in environmental services, and the possible rewards provided by external stakeholders.

capital, but which also draws on social capital for adaptation and replication across generations. Reward mechanisms can take the form of direct payments (financial capital), support for infrastructure (physical capital), rights to use natural capital (for consumption or investment in other forms of capital) that typically are socially mediated and controlled (hence based on social capital), indirectly through say investments in social or human capital (support for clinics or schools conditioned on supply of environmental services) or some mix of these options. The distinctions among these five types of capital matter not only because of the distinct functions above but also because of their different characteristics (e.g., regarding time frame for reversibility of investment decisions) and because of imperfect scope for substitution between them (e.g., money cannot buy trust). So, for example, a simple market-based approach relying on financial payments and competitive price formation may prove ineffective or unsustainable if complementary investments to build trust across spatial and institutional scales are ignored. Moreover, since the costs of different options for implementation rest on existing endowments of these five types of capital, the optimal mix of reward mechanisms (in the sense of least transaction costs to sustain a particular level of environmental service) likely will vary greatly across countries and even between neighboring communities.

A comparative action research approach based on pilot projects, such as the Rewarding the Upland Poor for Environmental Services (RUPES) project in Southeast Asia (Fig. 1; http://www.worldagroforestrycentre.org/sea/Networks/RUPES/), would seem to be a timely and essential step toward filling gaps in our knowledge of effective implementation of reward-based environmental policy levers.

6. Conclusion: toward nested levels of understanding, governance and equity

Tomich et al. introduced this volume with a set of questions about problems at different spatial scales and different sequential stages of an environmental issue cycle. Issues of problem recognition, perception, and requirements for measurement and scaling vary through that cycle and cut across this collection. Indeed, identifying the scale of analysis—and of intervention—depends on the specifics of a problem and often varies through the cycle.This collection has focused on three environmental issues as the ‘meso’-scale, that is they have important lateral flows (Van Noordwijk et al., this volume), but they are not global. Perhaps because fires and smoke are readily detectable with remote sensors, the smoke management issue is the most fully developed regarding empirical scaling. For watersheds, too, there has been progress on identifying the scale of specific dimensions of those functions (Bruijnzeel, this volume; Kiersch and Tognetti, 2002). The scale concept is least developed in the case of landscape-level biodiversity functions and indeed for ecology more broadly (Schneider, 2001). This certainly is related to our limited understanding of those functions. However, even for biodiversity functions, there are early suggestions of the scale of the services. As mentioned above, Loreau et al. (2001) suggest that these effects may be greatest at ‘small-to-intermediate spatial scales’. Similarly too for watersheds, at least for the cases of erosion, sedimentation, and flooding (but perhaps not for base flow or water quality?), the primary effects of land cover change occur within smaller catchments. And even in the case of the ‘regional smoke problem’, both the costs and the most promising interventions were shown to be essentially local. So for many (but not all) of our themes, impacts and actions at intermediate-to-local ‘meso-scales’ appear likely to play a critical role in efforts to effectively address these environmental issues, which are linked to land use and land use change.

For global–local conflicts regarding the environment—for example, the case of human wildlife conflict presented by Nyhus and Tilson (this volume)—the human ‘stakeholders’ with conflicting interests typically never meet. However, for landscape-level environmental issues, political and social activity and overt conflict focused on land use and cover change may be an important indicator of the existence of significant environmental issues at the landscape level, as well as a reflection of needs to strengthen resource management capacities at intermediate-to-local levels of governance.

In this sense, trends toward more decentralized and democratic governance systems in a number of Southeast Asian countries may bode well for improved capacity to manage these environmental services. However, a recent synthesis of environmental governance case study findings in mainland Southeast Asia by Dupar and Badenoch (2002) indicates there are still many questions about the appropriate scope of powers located at different levels of governance hierarchies, as well as about incentives, accountability processes and fiscal arrangements of intermediate-to-local level institutions appropriate for managing natural resources. Further efforts to improve our understanding of biophysical processes and functions at different nested spatial scales of analysis could help in formulating appropriate mandates for different jurisdictional levels, in identifying spatial domains that provide and benefit from various types of environmental services, and in valuing and monitoring environmental service flows. This could help strengthen foundations for further refinement and testing of environmental service reward mechanisms.

But this multi-level approach implies that in addition to the gap between researchers and national policymakers, there also is a new urgency toward reaching across the gaps that often separate analysts, policymakers, specialized government agencies, 'civil society', business interests, and other major stakeholder groups at key spatial scales of resource governance. Because of the likelihood of conflicting interests, it is naïve to expect that research alone will be sufficient to produce and implement better public policy. Social and political mechanisms will be needed to support negotiations to address these conflicts (Van Noordwijk et al., 2001; Wollenberg et al., 2001) within and among appropriate units of governance. In the likely case there are winners and losers, the challenge is to strengthen or create mechanisms for negotiation support and conflict management—between neighboring communities; upstream and downstream populations; local, national, or perhaps even global concerns–that promote social justice as well as environmental benefits. Thus, environmental issues and opportunities may be even more complex than commonly perceived. But recognition of the scope of this complexity based on more robust understanding of underlying biophysical processes and human behavior also helps expand the range of policy options for supporting the sustainable provision of environmental services and provides a rich set of conditions for more systematic strategic testing of key concepts and mechanisms within the context of continually emerging and evolving environmental issue cycles.

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