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15 Whither Tropical Ecology?

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Preamble

There are few things more presumptuous than a U.S. scientist holding forth on the future of tropical ecology. The easy defense is that AIBS asked for it. The much less palatable answer, but one that must be faced squarely in the future of tropical ecology, is that I am here by default. The fantastic diversity of resources and cultures in the tropics is singularly lacking in indigenous scholarly exposition on the ecology of the tropics. I must be quick to add that a description of site resources is in itself not an ecological study. This apology aside, my goal is to generate a dialogue on tropical ecology, not offer a definitive treatise on the future of tropical ecology. The tropics are far too diverse for one person to absorb and relate to as one can to the Rocky Mountains, the Mediterranean coast, or the Russian steppes. The literature must itself be the clearing house and collating device.

Introduction

The only justification for a non-medical terrestrial tropical biology is in the area of ecology, whether it be applied (agriculture) or esoteric,*

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* As used in this essay, "esoteric" refers to scientific investigation conducted purely in the pursuit of knowledge for its own sake, with no practical justification intended (i.e., "basic" in pre-1970 terminology).

and I will restrict myself to that area. The major unsolved ecological problem in the tropics is the gross failure of the decision-making processes in tropical countries to incorporate what the aggregate scientific and folklore community already knows about tropical ecology and its application. This problem will not be much alleviated by further research of the type classically conducted by tropical scientists, and therefore, the content of classical tropical research will not be the subject of this essay.

The tropics are many things to many people, and tropical ecology is headed in many directions. This heterogeneity is based on four phenomena: 1) There is far more basic diversity of habitats and resource bases between the tropics than outside of them. 2) These habitats and their indigenous cultures have been invaded by a wide variety of cultures and economic practices evolved to deal with the seasonal pulsed energy systems of extra-tropical regions. 3) There are two conflicting goals in planning for resource use: Is the tropical country to be treated as a primarily self-contained ecosystem, or is it to be treated as a collection of components of a global ecosystem and, therefore, available for competitive exploitation? 4) Tropical environmental predictability (and often a lack of extremes) at any particular site allows an array of solutions to resource use, each of which is approximately as profitable as the other, and thereby, allows greater variance through personal idiosyncracies.

And what are the tropics? To adopt a definition approximately congruent with current usage, they are those areas with moderately predictable major weather events and year-round strong solar radiation. This definition underlines the precaution often voiced by researchers in the tropics—some temperate habitats are as predictable as the majority of tropical habitats, and some tropical habitats are extraordinarily unpredictable. Unfortunately, this definition also bolsters a grossly-erroneous impression held by most temperate zone scientists and policy makers; their decisions reflect a strong bias toward thinking of the tropics as a unit rather than as a collection of problems and resource bases easily as diverse as occur in temperate zones. The ecology of droughts, subsistence agriculture/trans-Amazonian highways is as dissimilar from that of rubber/paddy rice/herbicides as the ecology of grapes/tourists/Mediterranean winter rains is from that of Douglas fir/SST/rainforest.

We may also offer two pragmatic descriptions of the tropics. When in a relatively undisturbed state, the tropics is a fantastic natural laboratory for the generation of ideas in the esoteric pursuit of understanding the

interaction of organisms with their biotic and physical environments in all parts of the world. On the other hand, the tropics is a set of habitats already converted in great part to a living machine that maximizes (with varying success) the sustained production of human desiderata from fossil fuel, sunlight, water, and inorganic ions. Obviously for any given piece of terrain, these two descriptions are only very marginally compatible, if at all. I shall discuss them separately in this essay and adopt the general philosophy that the only real decision left is how much of the tropics' habitat shall be allotted to land fitting either description.

And what is tropical ecology? It is ecology as practiced in the tropics. Here we encounter a second major area of heterogeneity that is strongly interwoven with the four points stressed earlier. The high diversity of tropical organisms and interaction systems maximizes the opportunity for the development of polarization between those researchers whose primary goal is to understand nature (esoteric ecology), and those who wish to manipulate it. (Let us not quibble over the obvious fact that manipulation requires partial understanding, and understanding derives only from partial manipulation). Between these two camps lies a host of opportunities for splinter groups to form and retain their identity. Heterogeneity in tropical ecology is also maximized by the fact that peer judgement, that bastion of quality control in science, is a nearly useless tool owing to the paucity of peers. It is rather easy to be an unquestioned world "authority" on the ecology of Central American acacia-ants, the silviculture of mahoganies, or the foraging behavior of weaver finches when you are the only scientist in the country with first-hand experience with the subject.

And where is tropical ecology going in the next 25 years? Some say "back to the temperate zones"—they clearly have the goal of understanding nature uppermost in their priorities. The National Science Foundation, the Institute of Ecology, and many other planning agencies, both temperate and tropical, want to see temperate zone dollar resources pumped into tropical research—they appear to have the goal of manipulating nature uppermost in their priorities. Why they wish to manipulate it had better be left for another essay.

Tropical Ecology as a Manipulatory Science

Let us for the moment be quite cynical and assume that nearly all of the esoteric research in tropical ecology (as, for example, reported in the

pages of *Ecology, American Naturalist, Evolution, Journal of Ecology, Journal of Animal Ecology, Journal of the Zoological Society of London, Oikos, Oecologica,* etc.) will have little or no direct impact on the rates, directions, and quality of the conversion of tropical natural habitats to living machines for the production of society's desiderata. We may then safely assume that the primary input to habitat manipulation will come from the application of resource management technologies, ranging from age-old farming traditions to the integration of genetic and biological control programs, to technological innovations such as converting wood from rainforests into cattle feed. So what's new? Sounds just like the temperate zones.

This leads me to realize that the primary problem area is not whether the ecological rules for manipulating complex natural systems have a very large potential future use in the tropics; this may be safely taken for granted. The hitch comes in identifying a mechanism to insure the incorporation of these ecological rules into the socio-economic decision-making processes and in deciding which of several possible technologies should be used to generate the particular ecological recipes for manipulating any particular habitat.

INCORPORATION OF ECOLOGICAL RULES

Peoples of temperate zone countries generally feel that it is out of ignorance that tropical countries apparently disregard ecological principles in habitat exploitation. This condescending elitist view results directly in the "developed" country being very susceptible to the argument that it should fund research to produce the information that the "uninformed" tropical government "must be lacking." (Let us leave for later the question of what part of this information is already known elsewhere in the world but not immediately available to the tropical person making a decision.) What is clearly missing is the proof that lack of ecological planning is not simply due to an imbalanced exploitation program which is to the immediate advantage of the person with the power to control it. For example, the administration may be much more concerned with meeting the press of immediate produce needs than with producing longer range sustained (and generally lower) yields and higher standards of living. We then have the consequence of Nobel prizes being given for "green revolution" strains of grain. A second example, often discussed

by Latin American politicians, is the excellent game and timber harvest laws that are on the books of many neotropical countries. These are rarely if ever enforced, and then generally only when to the advantage of the economic power directly concerned.

I think that we can safely assert that much of the disregard for ecological principles in tropical habitat manipulation is due to asking tropical habitats to produce yields incompatible with sustained yield levels or quality. This applies at all levels, from the farmer who wants cash for a new radio up to the government that wants to convert its forest reserves to cash for guns to protect its borders from a country that some years ago converted *its* forests to cotton fields.

So, what can be done to interest tropical decision-makers in the incorporation of sustained yield systems into their agroecosystem* development plans? (Please note that at this point I am not arguing for anything even remotely resembling conservation of natural systems.) As a predictor of the future, I cannot help but be cynical if I look around me in the tropics; planners will become aware as the agroecosystem in each tropical country degenerates until the lack of ecological planning is felt strongly and identified. At that point, the economic-social system will begin to incorporate its own checks and balances. The carrying capacity (*n* people at *x* standard of living) will be at some low level, depending on the intensity of destruction of natural capital up to this time. It is most likely that no semblance of natural habitats will remain even as national reserves. The critical question, then, becomes how much of the natural capital has been permanently destroyed, requiring a permanently lower human population density for a given standard of living. But need I paint such a gloomy scenario? Probably yes, but that should not obscure the obvious fact that there are positive ways to locate the carrying capacity of a habitat without such a dramatic country-wide or global experiment. As will be a recurring theme throughout this essay, I would like to emphasize that these positive ways are where the first priority should be in funding.

Even our crude level of understanding of tropical ecology is quite adequate to recognize major areas where money invested would yield very heavy returns:

1) Tropical decision makers are usually very provincial in their first-

* "Agroecosystem" as used here refers to all types of land or water use where organisms are manipulated to produce products desired by humans.

hand experience with agroecosystems. When they have the opportunity to study abroad, it is generally in temperate zone countries. They should have the financial means to view and work with those few functioning sustained-yield harvest systems that are scattered about the tropics.

2) Tropical decision makers are likewise unfamiliar with the ways that tropical agroecosystems outside their own country have repeatedly gone awry. They are badly in need of an educational system that communicates and produces scenarios based on real examples drawn from around the tropics. Farvan and Milton's "The Careless Technology" (1972) is a long overdue start in this direction. For example, must each country have its own failures at official land colonization by settlers drawn from urban slums? Must each country rediscover the interaction between large-scale cotton growing and insecticide resistance in tropical insects?

3) When tropical biologists travel abroad for advanced training, they return home to a governmental hierarchy badly in need of Ministers of Agriculture, Department Chairmen, Bank Vice-presidents, etc. who have technical expertise coupled with a broad education. They are quickly led or forced out of applied research itself, and into administrative roles, substantially weakening the educational-research structure at its base. Funding is badly needed to bolster job security and other rewards for the first-class researcher or teacher when doing the job he was trained for. Suggestions are also in order, but beyond the scope of this essay, for ways to minimize the administrative loads of tropical countries.

4) The directors and assistant directors of tropical agricultural field stations, potentially the most important roles in the tropics, generally regard the job as a hardship post. It often becomes a nine to five job or less and is viewed as a necessary evil in the steps to becoming the Minister of Agriculture or other top government official. We can hardly expect such a person to take a truly active interest in the integration of the results from his station's research into the agricultural ecosystem that surrounds him. Conversely, the director can hardly be expected to search out actively the needs of his surrounding agricultural ecosystem and pursue them with research. Funds are badly needed to make this position an honorable and pleasant one which holds the best people for long periods.

5) Schools are devices for transmitting the rules that run our socie-

ties. We teach math, biology, chemistry, languages, etc. because these abilities will presumably be needed in the years ahead as the child grows. The teaching of the socio-economic rules of a steady-state, non-expanding economy, attuned to the concept of living within the carrying capacity of the country's or region's resources, is an equally important subject from the first year in school.

TECHNOLOGY OF GENERATING RULES FOR ENVIRONMENTAL MANIPULATION

We must recognize the existence of an extremely important dichotomy in the philosophy of tropical applied ecology. The arguments are essentially as follows:

The Ecosystem Model Hypothesis (EMH).

We should attempt first to devise a general model of tropical ecosystems. Such a model would contain subsystems of such detail that current knowledge is inadequate to detail them. Once generated, this model would be used to predict the effects of various environmental perturbations on a local basis and, therefore, would be of great use in planning and carrying out the development of sound tropical ecosystem management/manipulation.

The Regional Development Hypothesis (RDH).

We should establish agricultural experiment stations for each of the major ecosystems on a regional basis, working toward a pragmatic set of sustained-yield solutions for each major region. The basic ecological background that is currently held in aggregate by the global scientific and folklore community is considered an adequate knowledge base to initiate such a program. Detailed information would be obtained as needed through direct large-scale experimentation at the station and on sub-leased neighboring land. Under this approach, each tropical agroecosystem is treated as relatively unique. Scientific linkage among the agroecosystems would be provided through publications and a greatly-increased exchange of personnel among field stations rather than through a formal model.

The philosophies underlying the above dichotomy deserve consider-

able discussion. As will become obvious, I feel the first hypothesis to be unnecessary and feel that the future in tropical applied ecology lies in the second hypothesis.

The EMH assumes that there is a thread of generality among the planning and development programs for a wide variety of tropical sites, a thread of generality that is not already known to the scientific community and that is not functional without formal incorporation into a model. I cannot disagree with the first assumption but have yet to see any evidence for the latter pair of assumptions. RDH, on the other hand, assumes that each site has very unique aspects; to make an *a priori* overall model detailed enough to deal simultaneously with all sites would require an inordinate amount of time to produce and maintain in the face of changing needs and technologies. In view of the extreme heterogeneity of tropical systems, such a model may be impossible even with the worldwide time and money resources available. In short, I feel that global heterogeneity within the tropics is so extreme that the pragmatic returns will be far greater through ecologically-sound regional development than development of overall ecosystem models.

By the fact of its attempted generality, the EMH appears to require extensive study of natural and quasi-natural ecosystems. A good deal of attention is given to documentation of community-wide energy flow, productivity, maintenance metabolism, etc. It is highly questionable that any of the general statements (e.g., "tropical lowland rainforest has the highest productivity of any habitat on earth"—which may well not be true, incidentally) produced by such a study, even when well documented by numbers, are relevant in any useful way to the development of a specific sustained-yield agricultural ecosystem. As a Southeast Asian biologist recently said: "but how important is productivity? The productivity of a crop plant is only of importance in so far as it affects the yield of the economic part of the crop. The padi (rice) planter is interested only in the harvest of rice grain, the rubber planter in the yield and quality of latex and the oil-palm grower in the yield of palm oil. Whether these plants come from plants (and I may add, communities) with high or low general productivity is immaterial." (Bullock, 1969.)

There is no evidence that an optimization of productivity of man's desiderata at various item- and system-specific levels will be a solution that even approximates the ecosystem structure and parameter values being examined in the development of ecosystem-wide models of natural

communities in the tropics. If there is anything at all in common between natural and quasi-natural systems, and agroecosystems, it is the individual control mechanisms and their principles; these are the very aspects of ecosystem models that receive the least attention. To use a temperate zone example, the highly successful agricultural ecosystems of temperate zones were developed with virtually no understanding of natural systems. To maximize stable corn production in Iowa, a knowledge of undisturbed prairie productivity is probably unnecessary.

Even a complete Tropical Ecosystem Model in hand could be a very obstructive device. Its structure would necessarily be based on statements of central tendency, as its predictions would be. Tropical man's needs have a habit of lying many standard deviations from means determined by averages taken over all the energy flows or units in the ecosystem; this, of course, is not to say that the optimal human system cannot be a sustained-yield system. I merely wish to stress that natural solutions, or models based on natural solutions (systems), are very likely not to be optimal for man, and adherence to such models is likely to greatly delay the development of innovations (such as converting forest to cattle food directly). This adherence is especially likely in tropical economic-social systems where "elegant science" with all its trappings of prestige (computers, foreign aid programs, big name scientists, elegantly-illustrated publications, new equipment with sophisticated technology, etc.) is often treated with greater reverence than merited by its information content or pragmatic value. Further, the presence of such an official "solution" causes researchers to try to use it (much as there is the temptation to use a faulty tool rather than to construct a new one) and, thereby, become distracted by the inability to apply a technique rather than focusing on the true problem at hand. The highly formal educational systems of many tropical countries also generate a predilection for this error in tropically-trained technicians.

Viewed overall, the tropics has a tremendous heterogeneity of optimal solutions for any given site; this heterogeneity of optimization is aided by the diverse needs of a tropical country and especially by the world market in aggregate. A rainforest in Nigeria may be best maintained on a 50-year shelterwood cutting cycle for furniture, construction, and veneer stock; a rainforest in Costa Rica may be best converted to secondary vegetation to be processed directly to cattle feed; a rainforest in Sarawak may be best converted to paddy rice. Or a given rainforest may

be best converted to all three or more. I maintain that the choices can only be identified through an active regional experiment station seated in the local agroecosystem that contains the rainforest. The station will have to assess the kinds of use to which its forest can be put (through experiments and through observation of other current forest uses, experimental or otherwise) and the world and local needs that have an impact on the local ecosystem. There is no evidence that the scientific and agricultural community at large does not already possess the needed technological information to conduct such an evaluation. And where the information is lacking, this information is of sufficient specificity that it will have to be generated by specific and directed experiments set up for local conditions, species, labor forces, etc.

It should be obvious that we are discussing a system where the conclusions of a regional experiment station can neither be taken lightly nor lightly made. Further, for it to develop a viable sustained-yield program over a large area, it or its agents will often have to have the power to overrule private interests; for example, such would be the case in an attempt to control insecticide resistance in cotton insects through sequential cotton-free years on a regional basis. It is also obvious that such a station must be managed by dedicated persons commanding the respect of scientists, local land owners, and the political powers of the country itself. Such a state of affairs would be at the extreme opposite end of the spectrum from the current status of most tropical experiment stations.

The system being proposed here is very susceptible to biological imperialism from well-meaning, but nevertheless destructive, temperate zone countries. Major sets of policy decisions must be made as to whether the regional experiment station is to aid and abet a maximally self-contained agroecosystem, with most of the produce of the region going to raise the standard of living of that region or its immediate political superior, or whether it is to produce an ecosystem highly integrated with the world ecosystem. A clear example is whether to generate a coffee-based economy, and use the cash to provide goods and services, or to go to a much more diverse agriculture and land-use program that will have little output to temperate-zone countries. When providing economic or educational assistance, it is extremely difficult for temperate-zone countries to avoid the temptation of structuring their aid in a manner that will destroy self-sufficiency on the part of tropical ecosystems.

Tropical Ecology as Esoteric Research

The esoteric research yet to be done in tropical ecology, as in temperate ecology, will have little or no direct impact on the development of sustained-yield tropical agroecosystems. This annoying statement requires elaboration. Under the rubric "esoteric research," I include those studies that are usually justified (if at all) by a general statement such as "tropical ecosystems are the most complex on earth, and man surely has something yet to learn by understanding how they function." ("Tropical ecosystem," as used here, is almost always an erudite synonym for "undisturbed lowland tropical rainforest.") Or another view states that "diversity enhances stability, and since we are after stability, we must study diverse ecosystems." Such studies are usually done on habitats nearly or totally undisturbed by man. Even when organisms or systems of economic importance are studied, the structure of the study is usually the same as if it were in an undisturbed habitat. As mentioned earlier, the results are generally reported in a set of journals regarded as prestigious. The results, wishfully, tend to increase our general understanding of ecological processes and the patterns they generate and, thereby, raise the quality of education of future biologists/ecologists whether they be esoteric or applied researchers.

But here is where my pessimism appears. I have seen no evidence to suggest that increasing the amount of ecological understanding of the tropics through more esoteric research would increase the speed with which high-quality sustained-yield agroecosystems will replace the exploitative-expansionist systems currently operating in most of the tropics. There are vast amounts of basic ecological knowledge stashed away in the world's journals and in scientists' heads that is never brought to bear on the decision-making processes or, once there, is rejected by those making the decisions. The same may be said of the agricultural folklore that is rapidly becoming extinct as subsistence agriculture is replaced by cash crops and land "development." On the other hand, once one has focused on a particular tropical agroecosystem, one can very quickly identify a large number of applied research projects which would be clearly of use in improving that particular agroecosystem. Further, one can often identify the bottle-neck in the absorption process where money should be applied to get the information taken

up by the administration, rather than using that money to generate more information to pile up behind the obstruction.

It is quite obvious that esoteric research in tropical ecology will occasionally come up with a fact or process that is of use to the person putting together a sustained-yield agroecosystem. Aside from the problem that such information is generally inaccessible until it has filtered through the educational digestive system, there is a very grave danger in its use and interpretation as science is currently practiced. As mentioned earlier, one of the criteria of good esoteric research in ecology is the general applicability of the study being reported. For example, we may have a study that shows that in tropical deciduous forest, a large proportion of the insect community passes the dry season as active adults concentrated in the humid shade of riverside (riparian) forests. When the rainy season starts, they move back into upland pastures, fields, and forests. Two agricultural predictions follow. First, with an insect community of this type, dry season irrigation may greatly increase the pest problem by providing a finely-reticulate network of dry season refugia. Second, we should destroy the riparian forest through clearing or stream diversion. The careful agroecosystem planner cannot afford to be satisfied with this kind of average information. Each of these conclusions may be invalid for any given agroecosystem, depending on the exact behavior of the precise pest species *vis à vis* dry season refugia.

Only a minuscule fraction of a natural tropical insect community falls in the pest category, and it is unreasonable to expect those pest species to always behave as the average insect does. If the planner is alert and has good ecological training, he will know that insects migrate, diapause, are generally host-specific, require moisture, and vary at the species level in their behavior. This knowledge should be adequate input to cause him to have a brief study conducted for his own particular ecosystem and its pests and, thereby, decide the fate of the forests that constitute dry season refugia for his insects.

I think that we must recognize that esoteric tropic ecology is essentially an art form. Like all such complex intellectual human endeavors, it occasionally generates a useful overview and, more rarely, some specific bit of information that is of use to applied ecology. Its most important potential is the dual role of its practitioners who simultaneously serve as teachers of general ecological principles to up-and-coming applied persons. A second important task is in stimulating more experi-

enced people through presentation of a fresh view or an intellectually-titillating solution to an esoterically-complex problem. It is my opinion that funding of esoteric tropical ecology should be restricted to the best artists and to those who are also willing to put in the time and emotion to share their studies extensively with those who choose to work with construction and maintenance of tropical agroecosystems.

A substantial part of current "ecological" tropical research does not fit in either category. Several examples might be illustrative. During the past 10 years, there have been at least 25 studies in major journals describing this or that piece of undisturbed to lightly-disturbed tropical forest; these studies were not aimed at bolstering our conceptual framework of tropical ecology, and they do not achieve that goal by accident. They will be of little or no use to a carefully-managed regional experiment station developing an agroecosystem. This is true even for the forestry portion of that program. The station director will have to gather on the spot his own information in the format that he needs. As a second example, productivity studies are clearly on the rise, with one focus on mangrove swamps. We hear that the goal is to determine the impact on marine productivity of the removal of mangrove vegetation. Unfortunately, some ecologists have decided that the way to get at this is to measure photosynthetic productivity of the crowns of mangrove plants. When we consider the incredible network of control loops and compensation systems between the crowns of mangrove trees and the actual amount of useful fish caught by the fisherman, it seems obvious that the only productive experiments may be those similar to the ones already kindly provided free of charge by Uncle Sam (though unfortunately lacking controls).

Our goal should be to understand tropical nature, not to describe it. This understanding can have intellectual justification and/or enhance habitat manipulation. With infinite time, resources, and interest, we could simply let the description roll on, and eventually understanding would come out of it, though one could also envision the description becoming so mammoth that we would have to do research on it to rediscover it—in short, the tropical world as it functions is the epitome of a pragmatic description. Our goal is to reduce it to workable size.

This seems the best point to bring up conservation of tropical habitats. At present, most of the acres of most tropical habitats have been mildly to severely altered by man's recent activities. Many of the animals and

plants still remain (though often not as breeding populations), but the interaction systems that produced and maintained them through natural selection are often gone; much of the tropics has been converted to a haphazard zoo and botanical garden. There seems to be an alternative. Throughout this essay, I have been arguing for development of carefully-planned and tropical-regional agroecosystems. If intellectually-stimulating or aesthetic experience is not regarded as useful produce from such an agroecosystem, then there is very unlikely to be justification for inclusion of natural habitats. There is a substantial part of any tropical flora and fauna that man can outcompete and do without. If this is carried out well, as I predict it will be, then the conservationist should prefer the currently mismanaged tropical lands, as dismal as they may appear. Brushy cattle pastures (the haphazard zoo mentioned earlier) and weedy fields support a very large flora and fauna that will definitely be missing from carefully managed pastures and fields.

It seems that the best the esoteric ecologist can ask for is small bits of strategically placed and carefully chosen undisturbed natural vegetation on which to practice his art. The careful agroecosystem planner will probably give him vegetation types that are representative of those from which his fields, pastures, and forests are derived. It is obvious that once the society changes to where undisturbed habitats are regarded as having aesthetic value in their own right (rather than being the environment against which man competes as is currently the view in most of the tropics), the amount of land under this type of use may be legitimately increased. This is, of course, provided that there has been a large enough piece retained as a reservoir of participants.

Most of the classical non-aesthetic arguments for the preservation of unmanipulated tropical forests appear to be specious. As stressed earlier, there is no *a priori* basis for assuming that stable natural communities are ideal for high output sustained yields. Further, to be stable, a cleverly manipulated community need be neither natural nor diverse to the extent found in nature. There is no obvious reason why man should not competitively displace a large part of the natural tropical community and yet keep it in balance. Nature is probably not a realistic gene pool for future agricultural marvels; when a new strain of corn, beans, cacao, or ginger is needed, the plant breeder turns to the variability expressed in domestic plantings rather than putting on his pith helmet and heading for the jungle. To be sure, the forest is a seed source for lumber and

drug species, but it hardly needs to be undisturbed to produce seeds. To a minor extent, the forest may contain phenotype pools in the form of undomesticated plants and animals, but the cogent evidence is very weak that contemporary tropical man has a shortage of useful phenotypes or is interested in filtering such phenotypes out of natural vegetation; it would have to be a rather highly specialized phenotype for man to have missed it already. Such plants are usually of value only for their secondary compounds. Biosynthesis being what it is today, it seems rather unlikely that there is a huge store of valuable compounds sitting there to be discovered. For such obvious things as erosion control and timber, undisturbed forests can be replaced with diverse ones of much greater direct value and still provide game cover and erosion control.

Recommendations

This essay is not intended as definitive. Rather, I wish to make explicit an unpalatable view that floats in the backs of many ecologists' minds but is carefully suppressed owing to its obvious threat to both our material livelihood and our intellectual egos. However, it seems better to discuss it explicitly among ourselves than to have our credibility gap progressively widen. I have been rather negative to this point. Can I make some positive suggestions?

I have referred constantly to the concept of regional experiment stations for major agroecosystems. It should be emphasized that these are not just stations for breeding better strains of corn, cotton, etc. They would be expected to have the personnel and resources to investigate and develop all aspects of the agroecosystem, in many ways filling the combined roles of the United States land grant colleges of agriculture, fisheries, forestry, the associated state experiment stations, the U.S.D.A., and some private companies. I suggest that temperate-zone funding in the tropics could be far better spent in aiding and abetting such field stations than through the current vogue of funding large conferences where the same tired questions (e.g., "are tropical soils fragile?"; "is subsistence agriculture consistent with modern society structure?") are pontificated on by various people with a few years' experience in the tropics. Any competent director of a tropical experiment station could sit down for an afternoon and draft the same list of questions while looking out his office window. What he needs is the funding,

equipment, and innovative scientific manpower to get to work on these questions for his own specific region. I suggest that we stop planning the solutions of the tropics' problems from our temperate zone offices and start returning some of the resources we have ripped off the tropics over the past 400 years in the form of hard cash and technical expertise directly to the development of regional sustained-yield agroecosystems. This should not require planning conferences at all except perhaps among the tropical countries on the general philosophical underpinning to be discussed below. The details of the research should be the subject of discussions by the field station directors, staff, and whatever imported personnel are interested in direct involvement. If it is felt that block funding cannot be trusted to a particular experiment station, then, some sort of review process could be generated whereby the reviewers at other experiment stations would participate, with occasional input by esoteric and applied ecologists the world over.

But this would not be enough. The governments of tropical countries obviously have to come to grips with several policy questions before a steady-state agroecosystem can develop. First, a decision has to be made about the average standard of living and the frequency distributions of the individual standards of living. Further, the currency units in which it is to be expressed (income, suicide rate, education level, immigration rate, etc.) must be identified. Second, once this is done, planning councils can decide relatively easily how many persons the region can support and determine the needed intensity of birth control.

A second major change is also needed. I think we should undermine the current concept that, to be really elegant, applied research must also deal with fundamental or theoretical ecological ideas. Vice versa, we need to undermine the idea that there is such a thing as an overall, general-purpose ecological study which will maximize practical results and contribute to sophisticated ecological theory as well. Very often the gathering of "basic" ecological data during applied studies dilutes the applied study almost beyond usefulness.

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