A Response to Brian Walker

In his essay (1992), Brian H. Walker criticizes the rhetoric that surrounds the current biodiversity debate. He states that "sections of the scientific community" choose to reject this problem as a "serious scientific topic" due to the nature of the debate. He proposes that the debate is "confusing and superficial and could divert policies and activities into directions where desired goals won't be achieved." It is possible that some of his proposals may have similar outcomes.

Walker states that ethical questions related to species loss are "an important topic, but for a different essay." Separating "practical" and ethical considerations avoids the necessity to deal with the latter and removes one of the most powerful arguments for conservation measures. His proposals do not return to this point and do not suggest mechanisms to address these issues. He states that commodity arguments "seem to relate to future possible benefits from as yet unknown specific pharmacological properties, etc." The drug Tamoxifen, which provides hope for treatment of breast cancer (New Scientist 1992), is example of an unforeseen benefit from a previously undervalued species. The scramble now underway to preserve or exploit the yew trees from which the drug is extracted, illustrates the perils associated with ignoring the diversity/commodity argument. Walker states that the present lack of information makes difficult the attaching of levels of probability to the usefulness of species. Surely this must inspire caution. Ecologists must advocate a system where it is the responsibility of those whose actions potentially reduce species diversity to demonstrate that their actions do not constitute a reduction in utility instead of trivializing this argument.

Walker states that there are no "apparent ecological distinctions" among the newly differentiated members of the Jack-jumper (*Myrmecia pilosula*) species group. How can we be sure these distinctions do not exist? Despite these species being large and drawing attention to themselves by a painful bite, we have only recently discovered the internal diversity of the group and are largely ignorant of their ecology. Even if they don't exist at present, the diversity in the species group may act as a buffer in different conditions. These conditions may relegate some species to refugia, leaving the remainder of the changed landscape without representatives of this group if those adapted to the new conditions have previously become extinct. Walker addresses this possibility but does not explain how it is to be overcome. Failure to do so imposes serious limits on the applicability of a redundancy-based approach. How are these time related interactions to be assessed in an unpredictable world? A genetically diverse assemblage must possess a greater ability to resist variations in environmental parameters and other factors such as disease, than a simplified group. What unknown, temporally moderated interactions associated with less obvious organisms might we be tempted to dismiss as redundancy?

He argues that to adopt his proposed approach we must understand the functioning of communities. This quest has absorbed ecologists since the establishment of the science and with a lack of success as demonstrated by healthy disagreements among theorists. Walker highlights one of these major debates, i.e. the equilibrium versus non-equilibrium argument. The equilibrium view of ecological theory states that each species is packed into its niche tightly by competition from its neighbors. If this is accepted then redundancy in most systems is nonexistent as it is the coevolved integrity of the guilds themselves that resists functional changes, for example, the invasion of outside organisms, i.e., equilibrium systems generate narrow specialists with little niche overlap. Walker's argument draws heavily on equilibrium theory for its terminology and theoretical basis but ignores the related conclusion that equilibrium systems possess no redundancy due to the highly coevolved nature of their members. The non-equilibrium approach dismisses guilds as having no ecological reality.

Walker proposes a detailed plan for increasing the level of resolution of ecological knowledge until all interactions are understood. Surely if this were achieved we would have unravelled the entire biology of each species and could treat the species individually, thus negating the need for specialist studies of interactions.

The advocates of economic development would be unlikely to start business negotiations by stating the lowest acceptable price of any goods they wish to sell. Similarly, those of us who wish to conserve as much of the diversity of life on this planet as possible should not simply accept a situation where species loss is not only inevitable but acceptable and provide arguments to trivialize the attempts of those who would resist these trends. The political pro-

cess that Walker criticises may be confusing and illogical but it provides a forum that other systems lack. Perhaps we should simply get on with the task of cataloging diversity and attacking the causes of its decline.

Greg Blake
Department of Geography and Environmental Studies
University of Tasmania
GPO Box 252 C
Hobart 7001
Tasmania

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Backgarden Biodiversity

While I sympathize with the stated aims of the editorial in the June issue of *Conservation Biology* (Brussard et al. 1992) I fear the authors are making an important error by omission.

In seeking to equate "biodiversity" with "wildlife" when communicating with the general public, they run the risk of further encouraging the massive extinctions that are already afflicting our food supply.

Reliable data are hard to come by, but it is unmistakeably true that around the world the diversity of crops is diminishing. In North America, approximately 97% of the vegetable varieties on old USDA lists are now extinct (Chiosso 1983), largely as a result of simple market economics. In Europe, legislation establishing national lists of permitted varieties resulted in the instant loss of 1,547 varieties in all countries of the European Community (HDRA 1981), a loss that continues today. Throughout the developing world, the policies of governments and developed world donor agencies have combined with inappropriate agricultural models to eliminate untold numbers of farmers' landacres (Cooper et al. 1992).

I do not need to remind the readers of *Conservation Biology* of the dangers inherent in a narrow genetic base. When the risks of pest and disease epidemics are added to the social, environmental, and financial costs of controlling those epidemics and applied to something as basic as our food supply, we have good reason to be concerned.

By all means let us stress the value of wildlife. But let us not forget that backgarden biodiversity is every bit as important, and often easier for individuals to do something about themselves.

Jeremy Cherfas
Henry Doubleday Research Association
National Centre for Organic Gardening
Ryton-on-Dunsmore
Coventry CV8 3LG
United Kingdom

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Using and Conserving the Rainforest

In the June issue of *Conservation Biology* (1992), Francis Putz points out the current rifts occurring in the "use it or lose it" forest conservation movement, between those who advocate solely sustainable nontimber forest product use, and those who consider sustainable logging as the only long-term way to make a living from tropical forests. The controversy is indeed unnecessary, not least because it overlooks the present reality that much commercial and subsistence extraction from tropical forests—for fruits, medicines, latex, fiber, game, and timber—is nonsustainable.

Harvesters maximize their short-term returns (to capital, land, or labor) by overharvesting, because it is rarely worth their while to plan for the long term. As many ethnobiologists have emphasized (for example, chapters by J. B. Alcorn, J. Clay, G. P. Nabhan et al., and V. M. Toledo in Oldfield & Alcorn 1991), strengthening traditional rights of land tenure and access by rural families and communities is a prerequisite to sustainable use, largely because it gives peasants the chance to adopt lower-yielding sustainable systems instead of higher-yielding nonsustainable systems. That the converse notion ("sustainable harvesting of tropical forests produces greater returns than deforestation") has so much credence among many biologists and rainforest activists, can be traced to problems with the paper by Peters et al. (1989*a*), and to the media's and conservationists' misinterpretation of it.

First, the impressive net present value (NPV) of $6,330 that Peters et al. calculate for fruit and latex harvesting in one hectare of species-rich tropical forest near Iquitos, Peru, is hypothetical, and depends on several unrealistic assumptions. The estimated NPV would be greatly reduced if they had employed a much higher, more realistic, dis-

count rate than the 5% that they used. In practice, because land tenure and access rights for many rural people are poorly defined and enforced, there are strong incentives to maximize short-term yields from forest resources by destructive harvesting and forest conversion, regardless of potential future benefits of maintaining the forest. Additional difficulties exist with the special case of fruit extraction. Fruits must be collected from up to 25 m high in the canopy, and transported to town before they rot. The relatively shallow markets mentioned by Putz also restrict the potential contribution of wild fruits to the local economy. In reality, market-oriented wild fruit collection in the Iquitos area is less important than agriculture or logging, is done mostly by the destructive practice of felling fruiting trees, and is based mostly on species-poor palm forests, rather than the diverse terra firma forests that prevail in Amazonia (Padoch 1988; Peters et al. 1989*b*).

Second, I believe that most forests actually produce less biomass of edible fruits than the authors estimated for their plot. In the first study to measure directly the total biomass of edible fruit produced per hectare of species-rich tropical forests (*Biodiversity and Conservation* in press), I found that total-per-hectare fruit production in three forest types in south-east Peru averaged one order of magnitude less than the theoretical values suggested for the Iquitos plot. Furthermore, I calculated that the biomass of locally valued fruit easily available to ground-based collectors, per hectare per year, was only 29 kg for sandy soil terra firma forest, 53 kg for clay soil terra firma, and 139 kg for alluvial soil forest. These values are about two orders of magnitude less than reported short-term yields from shifting cultivation of manioc or multiproduct intercropping!

Perhaps the main reason that the notion of the "rainforest cornucopia" has taken such a hold on the urban world's collective psyche, is because it holds out the hope of a painless solution to the problems of tropical deforestation. Hence, the enthusiastic response to the *Nature* paper by the media and some conservationists, who mostly ignored the authors' caveats that their calculations were hypothetical, and that different values of forest productivity and distance to market could affect their conclusion that "the financial benefits generated by sustainable forest use tend to exceed those that result from forest conversion."

I contend that the opposite is generally true. Wherever peasants are forced to act in their short-term interest, slowing deforestation will be very difficult. More sustainable and complementary systems, including limited hunting, collecting of fruits and medicinals, managed successions from field to forest, *and* appropriate silviculture, can indeed prevail, but only if rural people are accorded the secure land tenure and access needed to make a sustainable living from the forest environment. Without this prerequisite, well-meaning attempts to prove the attractiveness of forest conservation by calculating theoretical long-term monetary benefits of sustained use can have little effect on the ground.

Oliver Phillips
Missouri Botanical Garden and Washington University
Biology Department
One Brooking's Drive
St. Louis, MO 63130 U.S.A.

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Valuing Tropical Rainforests

The concept of valuing tropical rain forests (Balick & Mendelsohn 1992) seems conceptually sound. As pointed out in the article, the method probably results in an underestimate of value since it considers only traditional medicinal uses and not coproduction of modern medicinals, timber, or food. The purpose of the analysis is to compare the value of rotational harvest of "natural" forests to silvicultural or agricultural uses. While the article is conceptually satisfying, it is not however rigorous in its economic analysis. I list several points below that could be improved upon relatively easily in future analyses.

The authors state that unprocessed medicines are purchased from farmers at the rate of $2.80/kg. This might be a valid overall assumption or average but is unlikely to be accurate for both plots, as is implied. The medicinal species vary between plots due to location and successional stage, resulting in different harvests by both species and proportion. Since buyers probably do not purchase all material at the same value (the market implications are obvious), this means that the value of harvested materials will vary between plots.

Both gross and "net" revenues are presented for each plot. The "net" revenues are not accurate in that they are net of labor only; they do not account for transportation costs to market or other marginal costs of harvest, such as gasoline for chain saws (if used).

The discussion states that at least

some areas of forest could be used as extractive reserves for medicinal plants and other products. While this is the case on a world market, it might not be the case on a local level. Belize has a very small population and, therefore, a limited market for traditional medicines. It is not possible to judge the size or elasticity of this local market without further information, but it is probable that only a few acres would be necessary to supply it before having a significant price effect. Of course, to the extent that these same products are used as traditional medicines in other areas and are price-competitive, the revenues would not change.

The authors compare the revenue stream of extracting medicinal products to agriculture and silviculture in similar climates. It is not clear whether or not the cost and revenue accounting for all of the studies referenced were conducted in the same manner. The values are not comparative, however, since they are not adjusted for inflation in the decade between studies. More important, they are not adjusted for relative costs of living in the different countries or for the proportion of harvest used in subsistence lifestyles. Both of these factors greatly influence the relative revenues realized by harvesters. Likewise, land use decisions would be based on opportunity cost of land, including differential tax rates between land uses.

Perhaps all of these questions and others are answered in a larger study from which the article was taken. If so, I would like to know how to receive a copy. If not, I can only suggest that the authors take greater care in highlighting their assumptions and stressing the inherent uncertainties in the conclusions. Economics has a great deal to offer in the realm of conservation of natural resources; in many ways it will be what changes political opinion. For this to be effective, economic analyses must be conducted fully, accurately, and in such a manner that results and conclusions can be validated.

Richard Tremaine
LGL Alaska Research Associates
4175 Tudor Centre Drive, Suite 101
Anchorage, AK 99508, U.S.A.

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Advocacy and Responsibility

The 1992 Society for Conservation Biology meeting in Blacksburg included a well-attended panel discussion on advocacy. Panel members and many in the audience commented on when, how, and why (or why not) professionals in the field of conservation biology should adopt positions of advocacy. I suggest that we should be calling for professional responsibility rather than debating advocacy. Let me illustrate that assertion with an example.

Seattle's position between Puget Sound and Lake Washington requires bridges to provide the only practical transportation corridor for thousands of daily commuters using private vehicles and public transportation. Engineers design and construct those bridges to precise technical specifications. If an engineer could demonstrate that a bridge was structurally flawed, should he ponder the merits of advocating redesign or is it his responsibility to call attention to the design flaw? The answer is obvious!

By the same logic, I would be irresponsible if, as a professional ecologist, I was silent when my scientific knowledge lead me to conclude that the life support "bridges" upon which society depends are being destroyed by our actions. My decision too is obvious! It is my responsibility to be articulate in conveying the consequences, including the scientific evidence that leads me to expect those consequences, of continued destruction of existing "bridges." To do otherwise is to violate the trust of society, a society that paid for my education through subsidy of state universities and through the agency grants that supported my research for the past two decades.

Too often, we are timid because we cannot precisely predict when the failure will occur. Which extinction, rate of soil erosion, or level of pollution will cause the "bridge" to collapse? I am reminded of another track record of prediction. The National Bureau of Economics, using the most powerful econometric models and a massive data base, failed in 80% of its predictions of even the sign of change in the gross national product (Business Week 2698:11, July 27, 1982). Surely, existing knowledge allows conservation biologists to attain that level of accuracy.

Most politicians and policy makers do not understand the consequences of environmental decisions, just as they are not knowledgeable about bridge design. How can they make informed decisions in either situation if the trained professionals in those disciplines do not exercise their responsibility to inform decision makers and the public at large? In short, until we consider use of our scientific knowledge as a responsibility, society will view us as advocates.

James R. Karr
Institute for Environmental Studies
University of Washington
Engineering Annex, FM-12
Seattle, WA 98195, U.S.A.

Taking Conservation Biology to the Field

In your June 1992 issue, Stanley Temple, President of the Society,

asked, Is the Society for Conservation Biology Having an Impact? He responded enthusiastically in the affirmative, and I certainly agree. The Society for Conservation Biology has provided a strong, credible body of information on the effects of anthropogenic influences on our landscapes and the potential for the mitigation and reversal of those effects. This body of information has provided a long-overdue resource for policy makers, land and resource managers, and grass-roots environmental activists. I write this letter as a representative of the latter.

As programs coordinator for Preserve Appalachian Wilderness Network (PAW NET), I and my fellow PAW activists have found the work of the Society of Conservation Biology to be an indispensable tool in our efforts to promote the preservation and restoration of native biological diversity in the eastern regions of the North American continent. It is the goal of Preserve Appalachian Wilderness, as a member of what has come to be known as the "New Conservation Movement," to link environmental activists with the ecological sciences. It has too long been the case that ecological and biological academicians have been holed up in "ivory towers" with no input into "real issues," while environmental activists run amok in the streets and in the media with no idea of what they are talking about. Preserve Appalachian Wilderness, the New Conservation Movement, and the Society for Conservation Biology are trying to do away with these stereotypes. Not by changing our media images, but by undertaking what Gary Snyder has called "the real work."

It doesn't matter how many scientists write important academic papers on the effects of current extractive practices if no one is advocating change, and it won't matter how many people are out on the streets advocating change if they don't have any idea why, or what changes to make. These are the crucial issues that our organizations are addressing.

Unfortunately, the grass-roots New Conservation Movement faces a new problem that needs addressing. We divide our time between attending conferences on land management, held by organizations such as yours (such as the sixth annual meeting of the Society for Conservation Biology) as well as by land management agencies (such as "Biodiversity in Managed Landscapes: Theory and Practice," held earlier this month in Sacramento, California, by the U.S. Forest Service) and dealing at home with on-the-ground, in-the-field policy implementations of agencies such as the U.S. Forest Service.

At both aforementioned conferences, land managers and research scientists spoke of issues such as reducing the deleterious effects of artificially-induced edge. Meanwhile, while we New Conservation Grass Roots activists were attending these conferences, our mailboxes were filling up with environmental assessments and Findings of No Significant Impact by the U.S. Forest Service stating that creating artificial openings in the midst of the few remaining parcels of contiguous interior forest tracts through clearcutting would "Increase biodiversity through the creation of a diversity of tree species and age classes." Yes, this sounds absurd given the proceedings of these conferences and the current body of scientific knowledge, but, be assured, it is still happening.

What to do?

If the Society for Conservation Biology and groups like it wish to have a real effect, they need to divide their time between "top-down" policy conferences and participation in on-the-ground resource conflicts. Those who are dealing with the day-to-day issues of habitat conservation desperatelly need your help.

In the U.S., state legal bars and organizations require lawyers to give a certain amount of in the pro bono time in the field in order to retain their licenses. I recommend that members of the Society for Conservation Biology be asked, in some way, to work towards implementing current scientific wisdom in the field, as well as at conferences, by donating some portion of their time to working with conservation organizations seeking well-informed change.

Only when Conservation Biology leaves the conference rooms of universities and enters the field will it really have an impact in this country.

Mathew Jacobson
Programs Coordinator
Preserve Appalachian Wilderness Network
117 Main Street
Brattleboro, VT 05301, U.S.A.