

A Program for Monitoring Biological Diversity in the Amazon: An Alternative Perspective to Threat-based Monitoring

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ABSTRACT

Ferraz *et al.* (2008) indicated the need for planning in biological monitoring in the Amazon, and reviewed what they considered recent progress. The problems and solutions they discuss are well known, and it is unlikely that many biologists would disagree with most of them. However, the authors do not indicate that most of these issues are already being addressed in practice in the Amazon, especially by the Programa de Pesquisas em Biodiversidade (PPBio) of the Brazilian Ministry of Science and Technology. The only major issue about which we do not agree with Ferraz *et al.* (2008) relates to the design of monitoring programs. The authors recommended tailoring experimental designs to specific threats, which will result in a multitude of idiosyncratic designs and incompatible data sets. We suggest that generic broad-scale designs will allow us to answer more questions with greater confidence, because the investment in each location provides information about regional and global questions. Although specific designs may be more effective in some cases, there are insufficient resources available to install different research infrastructure for every possible threat.

FERRAZ *ET AL.* (2008) INDICATED THE NEED FOR PLANNING IN BIOLOGICAL MONITORING IN THE AMAZON, and reviewed what they considered recent progress. However, in their essay, they ignored much recent progress, and suggest optimizing monitoring designs for a small number of threats. This ‘business as usual’ approach has provided very little returns for the vast amount of resources invested in the past. Our response offers a different perspective to Ferraz *et al.*'s (2008) broader thesis that monitoring should be focused on specific threats. Instead, we argue that a standardized and integrated design can maximize the return of ecological information from finite conservation funds.

THREAT-ORIENTED MONITORING—AN ALTERNATIVE PERSPECTIVE

We believe that the threat-based monitoring arguments of Ferraz *et al.* (2008) are fundamentally flawed, in that they underestimate the complexity of many threats and assume that we have sufficient understanding of the threats to prioritize them and formulate efficient sampling solutions. In contrast, we contend that well-designed

sampling programs of a generalized long-term nature provide a stronger basis for understanding of the patterns, processes and key drivers of diverse and poorly understood ecosystems. This information can then be used to monitor and manage these environments, and specific programs can then be designed to counter the major threats to them and their constituent species.

We are perplexed to see the views expressed by Ferraz *et al.* (2008) from Tom Lovejoy. He initiated one of the most successful long-term monitoring programs in the Amazon, now known as the Biological Dynamics of Forest Fragments Project (BDFFP). When this program was initiated, the major threat to the Amazon system was thought to be cattle ranching, and only direct uses of the forest, such as logging, exploitation of nontimber forest products, or preservation in parks were touted as alternatives. With time, the threats changed (*e.g.*, soy bean farming, climate change), and the alternatives increased (*e.g.*, carbon markets). The main objective of the project was initially to determine the minimum critical size of Amazonian ecosystems, but this objective, the name of the project, and the major funding agency changed. The principal researchers also changed, bringing with them new questions and potential experimental designs. Despite this apparent lack of focus and consistency, the project was an outstanding success, and provided much of the ecological understanding that underpins current ecological research, management and decision making in the Amazon. This

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includes the papers produced by the BDFFP that covered many taxa and topics, from birds to fish, and taxonomy to carbon sequestration. We argue that the experimental design was not optimal for any of the scientific questions posed, so why was the project so successful? We suggest that the critical features were that it was relatively broad-scale (tens to hundreds of km), long-term, invested in infrastructure for access to the forest, and was associated with intensive training of students, features recognized as important by most authors, including Ferraz *et al.* (2008), and not that it had a narrow, or even optimal, experimental design.

SCALE AND INFRASTRUCTURE

One aspect of scale that Ferraz *et al.* (2008), and many other researchers, do not appreciate is that scales are hierarchical. If you provide infrastructure that permits studies over large areas and long periods of time, that infrastructure is usually useful for researchers working over smaller areas, even though they may have to complement some of the infrastructure (*e.g.*, increase the density of plots, mark individual animals, measure physiology of individual leaves). The BDFFP is a good example of this. Therefore, multiscale designs are both possible and feasible, with the added advantage of providing comparable data between sites. In doing so, they maximize the return of ecological information from finite conservation funds. In contrast, infrastructure designed to maximize results for a study covering a small area is usually of little use for researchers working with organisms that use larger areas.

We emphasize the importance of infrastructure because we believe that many people do not realize how limited the infrastructure for research is in the Amazon, and many first-world researchers take the presence of infrastructure for granted when they develop their experimental designs. Ferraz *et al.* (2008) compared the results of Wilkins *et al.* (2006) with those of the North American Breeding Bird Survey. There are obvious problems with the breeding-bird survey. However, many of these problems could be corrected, or at least the magnitude of the problem evaluated, with relatively minor changes to the sampling protocols (J. Nichols, pers. comm.).

Both the breeding-bird survey and the study of Wilkins *et al.* (2006) benefited from the billions of dollars worth of infrastructure, in the form of roads, airports etc., available in the US that are not available in the Amazon. Besides this, the data used by Wilkins *et al.* (2006) were collected over a period of more than 50 yr by thousands of researchers maintained by the Fish and Wildlife Service, universities, and the dozens of state wildlife authorities, in both the US and Canada. This was possible because the ducks were the basis of a hunting industry that contributes billions of dollars to the US economy every year. The true efficiency of a system must take into account all of the associated costs. It might have cost the hunter hundreds of dollars for each duck that gets 'efficiently' summarized as a hunter return. Just the environmental cost of the tens of thousands of tons of lead shot in aquatic ecosystems that sustained the Wilkins *et al.* (2006) study casts doubt on the high benefit/cost ratio implied by Ferraz *et al.* (2008). There can be no doubt that

the overall investment was orders of magnitude greater than for the breeding-bird surveys.

Despite the assertion to the contrary by Ferraz *et al.* (2008), a generic design will always be more efficient at identifying new threats, because it was designed to be flexible and necessarily will install infrastructure over large areas. Additionally, generic infrastructure distributed across the basin will allow new threats (or opportunities) to be evaluated quickly when they appear. Until now, monitoring in the Amazon has followed Ferraz *et al.*'s (2008) recipe. Designs were problem specific, and every time a new threat or opportunity appeared, it was necessary to scrap all that had gone before and start again, with important exceptions, such as BDFFP. Because no data were collected at common scales and locations, data from such studies cannot even be integrated in a metaanalysis. Why do we worry so much about efficiency instead of maximizing returns for specific questions? It is unlikely that all the resources that have been invested in the Amazon to date for biological monitoring would add up to the amount spent on North American ducks in a single year. We will always need specific designs for some questions. However, answering the many questions that can be addressed using common infrastructure will free up huge amounts of research resources for studies when the need arises. Importantly, such infrastructure also provides the basis for considering hitherto unknown issues, interactions or processes, and the long-term effects of climate change.

LONG-TERM BIODIVERSITY MONITORING

Many long-term monitoring programs have been initiated (Craine *et al.* 2007). Among the most well known in Amazônia are the Brazilian PELD program (PELD is the Brazilian acronym for Long-term Ecological Research (LTER) sites), BDFFP, Tropical Ecology Assessment and Monitoring Initiative (TEAM), Center for Tropical Forest Science (CTFS), Large-scale Biosphere-Atmosphere Experiment in Amazonia (LBA), long-term studies by Carlos Peres (Peres 2000, Peres & Lake 2003), and the Amazon Forest Inventory Network (RAINFOR). Most of these focus on a limited range of taxa (TEAM, CTFS, RAINFOR, studies by Peres), or principally on abiotic processes (LBA). All these initiatives are important, because many environmental problems operate over broad scales, and solutions cannot be based only on local studies (Johnson *et al.* 2007).

In 2004, the Brazilian Ministry of Science and Technology instituted the Program for Biodiversity Studies (PPBio—<http://ppbio.inpa.gov.br>), which sought to incorporate the strengths, and avoid the weakness of the existing programs to develop a truly integrated system (Magnusson *et al.* 2005). Because data integration is simple, the long-term ecological sites have a positive feed back. As more fieldwork occurs, more information becomes available, the more interesting the site becomes to new students and researchers, and more robust information is available to land managers. Other programs are also evolving toward more integrated systems that will allow more general conclusions and less focus on a limited range of questions and taxa. For example, the CTFS plots are being incorporated in the Smithsonian Institution Global Earth Observatories

(SIGEO) (<http://www.stri.org>), and the LTER sites stimulated the creation of the NEON system for standardized ecosystem monitoring (<http://www.neoninc.org>).

Long-term ecological sites are too expensive to be installed in all areas necessary to answer management questions important on regional scales, and many organizations have adopted rapid assessment (RAP) surveys. However, partly because their designs were tailored to answer different questions, the data from one RAP site are generally not comparable to those from other RAP sites, or to data from LTER sites. RAP surveys are often justified as means to distinguish between sites that should have priority for conservation and those that do not. However, as they are not comparable among themselves, none of the RAP surveys has revealed a site that should not be considered priority for conservation. Without standardization, data cannot be used effectively to help stakeholders make hard decisions.

The survey system in PPBio-PELD sites is modular. That is, there are modules consisting of a reduced number of sample units that can be used in rapid surveys. These have the advantage that they are comparable among themselves, and with the same modules in RAP sites. Most funding for biodiversity studies comes from organizations that have to conduct rapid environmental impact assessments, or monitor the impacts of ongoing enterprises. These studies are often criticized for poor experimental design, but more importantly, even if they do answer questions relevant to that site at that moment, they contribute little or nothing to understanding the regional processes that affect biodiversity. If it were required that studies of impacts included at least an element of standardized monitoring, even if this design was not optimal for the particular question, the huge investment in impact-assessment research could be integrated with the network of LTER sites that it is viable to maintain with public financing.

Long-term ecological research sites are used for training local people, and serve as important sources of income for guides and parataxonomists. There is little incentive for local people to develop generic skills in relation to these careers unless there are long-term sites that can sustain them between the expeditions carried out by researchers with narrow research agendas, such as those recommended by Ferraz *et al.* (2008). An advantage of long-term programs, such

as PPBio, is that, if and when new threats arise, there is already an established rapport with local people and a skill base from which to quickly implement new programs. The North American Breeding Bird Survey put thousands of citizens into the field to evaluate how birds and the environment are being affected in their local areas. The effect of this on conservation should not be discounted, independent of the specific analyses undertaken by federal biologists. We need to think globally, but most of the actions will be undertaken by local people who do not read scientific papers.

Improved understanding of poorly understood ecological systems in a rapidly changing world demands that we maximize our returns from finite conservation dollars. In such circumstances, focusing on narrowly defined questions represents a poor investment and risks failing to identify and respond to less obvious but more important issues. It is our belief that gaining a broader and longer-term perspective is a better basis to understand the poorly understood Amazon system.

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