Scientism as a Barrier to Our Progress

After 12 years of faithfully reading and enjoying *Conservation Biology*, I was never inspired to write a letter to the editor, but after the "Conservation in Context" articles appeared in the December 2002 issue, my fingers eagerly sought the keyboard. The issues concerning the relationship between sustainability and spirituality raised by Orr and elaborated upon by McDaniel, Porritt, and Christie are, I believe, critical to our progress as a society, both at large and within our respective disciplines of conservation biology. I use the word *progress* without presuming that material progress, such as the accumulation of knowledge or wealth, is our primary goal. Rather, let me pose another idea of progress: increased harmony between human society and the remainder of the natural world.

Will science alone move us forward on this path? Many good scientists would unambiguously answer yes, but others are beginning to align themselves with Orr, who requires "a higher level of spiritual awareness." As described in the McDaniel article, however, there remains a strong belief in the god of scientism, which was well summarized by a famous evolutionary ecologist who recently told me that "science is my religion." It is as though scientists are expected to excise from their minds all other ways of knowing, like a misdirected ascetic seeking enlightenment by gouging out his eyes and chopping off his tongue. Society, in turn, has done the same by developing a strong faith that science will prevail and that, in partnership with monetary capital, it can reduce all problems and solutions to these currencies.

Such arrogance, which is often perpetuated by the university and by scientists, not only limits our ability to effectively engage with nonscientists, but also weakens our resolve to critically reflect on our own scientific epistemology. We must courageously ask whether the products of scientific inquiry are the only guideposts to aid our progress or whether higher human qualities such as compassion and love might be important to cultivate within our educational institutions and within ourselves. Are we incapable of integrating multiple ways of knowing, which, when fully scrutinized, all arise from the same place?

I argue that as conservation biologists we must strive to promote an ongoing dialogue about the limits of science. There are three levels at which I propose we work.

(1) As individuals, we need to become more aware of our own biases against nonscientific knowledge and welcome discussions to reveal the roots of our discomfort. This may require spending less time on scientific work and more on exploring knowledge beyond the borders of our discipline, in the arts, philosophy, and within ourselves.
(2) As teachers and professionals, we need to promote the idea of critiquing all types of knowledge, especially scientific ideas. Students learn from their teachers how to approach a scientific problem rationally, so why can't we teach them to use this technique to probe the foundations of their own knowledge?
(3) As a scientific society, we should formalize our commitment to moving beyond scientism and into a dialogue with spirituality—the metaphysical roots that give rise to compassion, love, and an awareness of our true nature beyond mere intellectual musings.

Perhaps there is much to learn from the simple Buddhist monk and poet Thich Nhat Hanh, who, when asked by an American scholar why he didn't spend more time writing poetry rather than working in his garden, respectfully replied, "My dear friend, if I did not grow lettuce, I could not write the poems I write." Maybe the time has come for scientists to spend more time in their gardens.

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Delisting of Species under the ESA

The 2001 paper by Doremus and Pagel in *Conservation Biology* offers some useful insights into the process of delisting under the Endangered Species Act (ESA). Although I appreciate their opinion that future protection and management actions should be taken into account at the time of delisting, I do not agree with their conclusion that the limited ability to delist species under the ESA is a strength of the act. If species added to the list of endangered and threatened wildlife are rarely deemed "recovered," how can the ESA be effective at achieving its goal of recovering listed species? Furthermore, there will be little political support for the act without successful recovery stories.

First, one note of clarification. Doremus and Pagel cited Gerber et al. (1999) as reporting that population size and growth rate were sufficient to determine whether species should be delisted. The approach used by Gerber et al. (1999) was originally presented by Gerber and DeMaster (1999). Post-delisting protection is included as a criterion in this classification scheme.

Social science and law have an important role in endangered species recovery (and conservation biology),

but this does not contradict the importance of quantitative recovery criteria. It is critically important that regulatory protections be in place and deemed adequate before delisting occurs. The ESA requires consideration of five factors when a species is delisted. One of these factors is the adequacy of existing regulatory mechanisms; others include habitat loss, overutilization, disease or predation, and "other influences." Although the precise influence of each of the five factors on the continued persistence of a particular species may be unknown, the collective consideration of these factors was intended to assure protection from threats *after* delisting. Furthermore, if clear goals and measurable criteria are not specified, a great deal of discretion is left to administrators to make delisting and downlisting decisions. For example, the proposal unveiled by Bruce Babbitt in 1998 to delist 29 species was remarkable in light of the inconsistency or absence of delisting criteria for each of these 29 species. Without clear criteria, these decisions are more likely to be arbitrary, susceptible to political goals, and potentially detrimental to the conservation of listed species.

Doremus and Pagel assume that delisted species will not receive the same level of protection they did when listed. In fact, the 1988 amendments to the ESA specify that monitoring plans be developed and implemented for any vertebrate population that is removed from the list. The ESA further requires an initial period of 5 years of monitoring and assessment after delisting. Admittedly, 5 years is a short time frame for monitoring, especially for fluctuating or long-lived populations. However, Congress' intention in amending the act to require that longer-term monitoring plans be developed following delisting was that a precautionary approach be taken in delisting species to provide some assurance that the initial decision was not in error. If information collected during this period suggests the decision was in error, or if a species' status has changed substantially, the act requires agencies to reclassify species.

I appreciate the perspective that the goal of the ESA should be to prevent extinction rather than to delist species. But how do we measure the prevention of extinction? Conserving listed species is often complex and multifaceted, so it is difficult to attribute "preventing extinction" directly to conservation actions mandated by the ESA. Second (this may be a semantic issue), I believe that the goal of the ESA is not only to prevent extinction but also to promote recovery. Then the question becomes, what is recovery? According to the ESA, a recovered species is one that has recovered to the point that it should be delisted, based on objective, measurable criteria. In addition to implementing protective mechanisms after delisting, quantitative recovery criteria should include explicit metrics of post-delisting habitat requirements. The goal of delisting (i.e., recovery) adds an essential layer of precaution to the ESA's goal of preventing extinction. Further, "preventing extinction" could be achieved in zoos, whereas "recovery" necessitates protection of the habitat of endangered species.

It is our job as conservation biologists to help determine when species should be listed as endangered or threatened and when species should be heralded as recovered. Toward this end, modern conservation biology has yielded new tools for evaluating species viability, determining appropriate strategies for protecting threatened species, designing nature reserves, and initiating captive-breeding programs. These practical approaches may help fulfill the ESA's requirement that recovery plans include "objective, measurable criteria which, when met, would result in a determination that the species be removed from the list." Without a better ability to remove species from the list when they are deemed recovered based on quantitative recovery criteria, how will the ESA accomplish its mission?

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Gerber's thoughtful response to our paper (Doremus & Pagel 2001) provides an opportunity to clarify our position on delisting under the U.S. Endangered Species Act (ESA). Contrary to her suggestion, we did not claim that the limited ability to delist species "is a strength of the act." One long-term goal of the ESA is progress toward recovery and delisting. The limited number of delistings is plainly not a strength.

Our point was rather that it is unfair to blame the ESA or its implementation for the slow pace of delisting. The responsibility lies elsewhere. We agree with Gerber that "[i]t is critically important that regulatory protections are in place" prior to delisting. We sought to demonstrate that no such protections are available for the vast majority of species threatened by habitat destruction. In our view, the limited number of delistings so far is both entirely predictable and directly attributable to the unique importance of the ESA, which provides much stronger protection against habitat destruction than any other law. Unless and until other state and federal laws are strengthened, or other mechanisms are developed to assure habitat pro-

tection, it is unrealistic to expect the pace of delisting to increase.

The ESA's 5-year monitoring requirement after delisting is no substitute for long-term, enforceable protective measures. Experience suggests that monitoring plans are not necessarily in place or subject to public scrutiny prior to delisting. When it delisted the American Peregrine Falcon (*Falco peregrinus anatum*), for example, the U.S. Fish and Wildlife Service (USFWS) simply stated that it was revising its monitoring plan, which would be available for public review "in the near future" (USFWS 1999). A proposed monitoring plan was issued almost 2 years later (USFWS 2001), and portions of that plan are still being debated within the USFWS. Additionally, activities that threaten species could be delayed until the end of the monitoring period. Effective control of threats to the species is necessary to assure long-term persistence. Short-term population stability may or may not indicate that threats have been effectively countered for the long term.

We agree with several of Gerber's points and tried to make our views on those points clear in our paper. Like her, we are anxious to see species persist, and if possible thrive, in the wild. We have no quarrel with the idea that recovery to the point of delisting is desirable and believe we clearly stated that species should be removed from the list when they no longer require the act's protection. Our difference of opinion seems limited to how often delisting might be expected to happen and how to identify species that qualify for delisting. We believe that species threatened by habitat destruction will always require some form of protection, and we do not currently see any effective source for that protection other than the ESA. Therefore, it strikes us as unrealistic to expect many of those species to reach the point of delisting. The record to date confirms our intuition, as critics of the ESA have noted. We also believe that evaluation of legal and social protections outside the ESA is essential to determining whether species are eligible for delisting. The narrow focus of many conservation biologists on quantitative recovery criteria stands in the way of including that evaluation. Both elsewhere (Gerber & DeMaster 1999) and in her letter, Gerber has noted the importance of post-delisting protection as a recovery criterion. But quantitative data about the effectiveness of legal or social protective measures is extraordinarily rare. Expert qualitative opinion is often the only tool available for that purpose. Conservation biologists need not provide that opinion, which they may feel goes beyond their expertise. But they should be ready to build collaborations with the legal specialists and social scientists who can provide it, and they should concede that not all recovery criteria can or should be quantitatively expressed.

Gerber, like other conservation biologists with whom we have discussed our views, worries that our frank discussion of the practical limits of delisting may undermine political support for the ESA. We have a different set of worries. We worry about encouraging the public to view the ESA as a short-term conservation fix. Because delistings are unlikely to accelerate, it will inevitably become clear that conservation is a long-term process, with no obvious end-point, imposing costs into the indefinite future. We also worry about the potential for extinction of species delisted solely on the basis of population numbers or trends, without guarantees that effective protections survive delisting. Unfortunately, by emphasizing precise, quantitative recovery criteria and focusing on the political benefits of delisting, well-meaning conservation biologists may be unwittingly encouraging that result.

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Adaptive Management and SCB's Evaluation of Species Recovery Plans

In addition to my day job as an academic conservation biologist, I am a conservation practitioner. My main efforts are planning and implementing a community-based sanctuary on the Ghana–Burkina Faso border, with hippopotamus as the focal species (since 1999), and restoring connectivity to the southern California landscape (since 1990). In both efforts, my colleagues and I make adjustments and make progress, but we often feel that we are groping in the dark, wishing for a way to learn from the experiences of others and to share our lessons with others. Thus, I was intrigued by two essays in the December 2002 issue of *Conservation Biology*.

As a good skeptic, I reacted to the grandiose title of the essay by Salafsky et al. (2002) with skepticism, expecting a statistical approach that missed the boat, a bucket of trendy buzzwords, or yet another editorial

that preaches to the choir. But by the introduction I was excited, and by the end of the paper I had written a page of notes to share at my next board meetings. The elements of their approach—clearly identifying threats, considering the full spectrum of conservation strategies, developing clear measures of success that are sensitive to the phenomenon being tracked, recognizing that threat reduction is usually a better measure of conservation success than population size, developing a full set of critical skills in the conservation organization, and constructing a learning portfolio—are precisely what need to become the "next big thing" that helps conservation biology move from an academic discipline to a profession. Papers on these topics should occupy a big chunk of our journal pages and every meeting of the Society for Conservation Biology (SCB). I am writing to encourage other readers to take a serious look at the paper by Salafsky et al. and previous papers in that vein (cited by Salafsky et al. 2002). Assign these papers in your undergraduate and graduate classes. Use them to help yourself get out of the classroom and into the boardroom and the community of conservation practitioners.

The same issue provided a paper that took a more traditional statistical approach to adaptive management. Clark et al. (2002) summarized the key findings of a recent SCB-sponsored review of recovery plans under the U.S. Endangered Species Act. Superficially, both papers were about adaptive management and a learning portfolio, but they were otherwise starkly divergent. Indeed, they did not have a single citation in common!

The SCB study (detailed in 13 papers in the June 2002 issue of Ecological Applications) provided masses of descriptive and inferential statistics but had some major limitations. First, the only dependent variable (measure of success of a recovery plan) was the estimate of population trend—"stable, increasing, decreasing, unknown"—that the U.S. Fish & Wildlife Service generated for each species on the endangered species list in 1996. The authors of the various papers from the SCB effort acknowledged that the trend estimates were outdated, not rigorous (many were based on no data), and a poor measure of how well the listed entity was faring, but they used trend estimate as the main dependent variable because "it's all we've got." In addition, the values of the independent variables were generated by 325 graduate students in 19 university seminar classes who filled out a 2600-question survey on each recovery plan based on only two documents (the listing decision and the recovery plan). Although students were 75% consistent in scoring the single recovery plan used to calibrate the reviewers, this consistency remedies neither the lack of grounding in other literature and on-the-ground experience nor the fact that the average time spent to score a variable must have been only a few minutes. These constraints precluded having an independent variable that measured whether any aspect of the recovery plan was good or not and instead limited the independent variables to easily scored variables such as date written, number of authors, and number of pages in the plan. The massive statistical analyses are probably impeccable. However, with a weak dataset of independent variables and a primary dependent variable that may have been meaningless, the review revealed a few mildly interesting trends over time and produced some rather predictable recommendations.

I was most concerned by Clark et al.'s (2002) conclusion that "this project could serve as a model for additional studies." My conclusion would have been: "Our statistical analyses were not very helpful given the paucity of meaningful dependent and independent variables. Our most important recommendation is to develop meaningful measures of plan success, including a threat-reduction index for each challenge facing the species. Without such measures, further statistical analysis would be premature and superficial. In the infancy of adaptive management, we need instead to develop chains of causally linked factors and to think clearly about threats and measures of success. Future attempts to build and profit from a learning portfolio should use statistics to support the analysis, but statistics should never *be* the analysis."

I admire the many excellent conservation biologists who contributed to the SCB review. Their subject matter is of the utmost importance: as conservation biologists, we need to critically examine all our tools, such as recovery plans. My point is not to discredit the SCB effort, but to encourage practitioners of conservation biology to (1) realize that we have only begun to improve the *practice* of conservation science and (2) consider what mix of these two approaches will best improve practice during the infancy of our profession. In the tension between conceptual clarity and statistical precision, I am unabashedly biased toward the former. I think it is more important to seek approximate answers to the critical questions than precise answers to surrogate questions. Over time, as we develop better measures of conservation success and other variables, we can make effective use of the power that more sophisticated statistical analyses provide.

I encourage instructors in graduate seminars to have students spend the semester designing an approach for measuring the success of conservation efforts, such as recovery plans, community-based conservation efforts in developing nations, or ecoregional conservation efforts of The Nature Conservancy or The Wildlands Project. The literature cited sections of these two papers would form the basis of a great class reading list. Conservation practitioners and the boards of directors of nongovernmental organizations in both developed and developing countries should read the same papers to help them think strategically

about their plans for the next few years. In the long term, a marriage of the two approaches will help us take action in the face of uncertainty and work to reduce that uncertainty. The approach taken by Salafsky et al. (2002) of focusing on conceptual clarity will be most productive in the short term, when meaningful data are scarce.

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Contrary to Beier's assertion, our paper is not about adaptive management; it is about improving the recovery plans of the U.S. Endangered Species Act (ESA). We find it peculiar that Beier believes our paper (Clark et al. 2002) and that of Salafsky et al. (2002) warrant critical comparison. Not only are the papers on entirely different topics, they represent fundamentally different types of commentary. That our paper has no citations in common with Salafsky et al. should be no surprise. Salafsky et al. use principles of adaptive management to provide a general conceptual framework for improving conservation practice. We summarize and discuss the findings of a data-driven study narrowly focused on a single conservation issue.

Setting aside the red herring of the paper by Salafsky et al., we will respond to the three major criticisms of the SCB recovery plan project and our paper that Beier offers: (1) use of U.S. Fish and Wildlife Service (USFWS) trend data as a dependent variable; (2) use of a large questionnaire to collect data; and (3) our suggestion that the project could serve as a model for other studies.

Beier's skepticism regarding the reliability of USFWS trend data reflects our own. In fact, this concern motivated one of our paper's major recommendations (and subheading): "Ensure that species status-trend data are current, quantitative, and documented" (p. 1514). But attentive readers will recognize that Beier's assertion that these trend data were our only response variable is wrong. Of the 13 analytic papers referenced in our review, only 3 used trend data as one of their dependent variables.

Beier also criticizes the project's use of a large questionnaire to collect data. The fact that a response to any single question may have taken only a few minutes during a 40- to 50-hour review of each sampled recovery plan is hardly damning. The questionnaire was designed to structure and discipline how researchers reviewed each plan and to capture detailed information about plan contents and characteristics so that researchers could analyze and compare these diverse documents.

Finally, Beier misunderstands us when we suggest that the project could be a model. We do not intend the project to be a model for adaptive management or even for its science. Rather, we offer this project as a model for how academic scientists and government personnel can collaborate to address complex questions of science and policy in conservation biology. Academics wonder why government agencies lack high-quality biology, implement conservation actions slowly, and rarely fund basic research. Agency scientists grumble that academics are too quick to criticize, are ignorant of legal, political, and fiscal constraints, and rarely conduct research that applies to real-world concerns (Boersma & DeWeerdt 2001). The project was a cooperative effort to start bridging this gap.

During the project, a dedicated team of graduate and faculty researchers applied their analytical skills to conducting an inexpensive programmatic review for the USFWS, which provided practical recommendations for improving ESA recovery plans. The USFWS has already begun implementing many of the changes recommended in our paper. We reaffirm our belief that the SCB recovery plan project could serve as a model for future studies.

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Salafsky, N., R. Margoluis, K. H. Redford, and J. G. Robinson. 2002. Improving the practice of conservation: a conceptual framework and research agenda for conservation science. Conservation Biology **16:**1469–1479.

The U.S. Fish and Wildlife Service (USFWS) welcomes the findings and recommendations of the study by the Society for Conservation Biology (SCB) of endangered and threatened species recovery plans (Clark et al. 2002). The USFWS was a full collaborator on the SCB recovery-plan study. In addition to the technical findings and recommendations outlined by Clark et al., a major benefit of the study (as noted by Crouse et al. 2002) was the expansion of interactions between the USFWS and the academic community, in

particular the SCB. We anticipate the results of this expanded relationship to benefit endangered species in the future. Revision of the USFWS recovery planning guidelines is already underway, and a number of the recommendations from the SCB study are being incorporated.

As noted in its final report to the USFWS, the SCB study was based on an analysis of correlations between attributes among recovery plans. Therefore, the USFWS views the findings of the SCB study as identifying issues of endangered species recovery planning that bear closer evaluation, rather than a direct measure of cause and effect.* The final actions to be taken in each of these issue areas must be based on both the recommendations of the SCB study and the findings of that closer evaluation.

For example, the SCB study found that multispecies plans were less likely to include species-specific biological information or adaptive-management provisions, or to be revised (Clark & Harvey 2002, as cited by Clark et al. 2002). It recommended reevaluation of the use of multispecies plans to ensure that species recovery is not compromised for reasons of administrative expediency. It is not uncommon, however, to have less-than-perfect knowledge about the biology and threats to many endangered and threatened species at the time of listing and recovery-plan development. The USFWS often opts to fold these species into multispecies plans with other species that are either related or occur in the same geographic area or habitat type. Rather than suggesting that multispecies plans are inherently inadequate, these findings may be an artifact resulting from multispecies plans frequently including species about which less is known.

The decision to include lesser-known species in multispecies plans is based on the assumption that related species or species occurring in the same area or habitat type are likely to be subject to similar threats or to respond in a somewhat similar manner. Under these circumstances, we believe it is better to include the species in a less-detailed recovery plan, while seeking additional information about the species and their threats, than to delay recommendations for recovery until more is known about the species. Given the potential ambiguity in interpretation of the results of the SCB study on this issue, the USFWS's revised recovery-planning guidance will include reminders against knowingly shortchanging species-specific information in multispecies recovery plans and to revise these plans as new information becomes available. However, we do not feel compelled at this point to curb development of multispecies plans.

We look forward to increased collaboration with the SCB as we work to improve recovery planning for endangered and threatened species and promote its implementation.

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***Editor's Note**: The original manuscript submitted by Clark et al. (2002) contained a paragraph discussing the correlative nature of their findings as opposed to a cause-and-effect relationship. Due to the length of the original manuscript, this and other material was deleted from the published version in the editorial process.*