Has the term "conservation biology" had its day?

The "conservation biology" literature has increased tremendously in volume during the past two decades. Furthermore, the field has expanded in breadth to include various disciplines, reaching far beyond biological or ecological science. Consequently, is it time for a name change?

Although the importance of social science disciplines within conservation biology has already been acknowledged in Soulé's landmark paper (Soulé 1985), non-biological disciplines therein were largely neglected during the 1980s and 1990s. For example, the topics covered at the Society for Conservation Biology's (SCB's) first annual meeting, held in 1987, were almost entirely restricted to biology and land management (Ginsberg 1987). Since then, there is no doubt that conservation biology has – in practice – developed into a truly interdisciplinary subject (see eg Meine *et al.* [2006] for a historical account). Thus, the list of topic areas for SCB's 2009 annual meeting included – in addition to more "traditional" conservation topics – environmental economics, politics and policy, anthropology, sociology, and psychology.

A question that is increasingly being asked is whether the term "conservation biology" is appropriate in light of that conceptual expansion. We do not think so. In "conservation biology", the word "conservation" is used as a noun adjunct modifying "biology". Linguistically, this makes conservation biology a subdiscipline of biology, just like cell biology, freshwater biology, or invasion biology.

Are there any alternative naming conventions that would capture the full contemporary span of this discipline? The term "conservation research", which is already being used to some extent (eg Bhagabati 2007), would better acknowledge the field's breadth. In cases where the ecological context may not be obvious, it may also be necessary to add qualifiers to distinguish the field from other disciplines that involve conserving things (eg architecture, art, or digital information). Here, terms such as "biodiversity conservation research" could be used.

The term "conservation biology" is certainly appropriate as a name for the discipline addressing biological aspects of conservation. However, when referring to the wider interdisciplinary field that has developed over time, we argue that adopting a new terminology is warranted. This is an important step toward embracing the broad range of actors – from many disciplines – who are needed to save the world's biodiversity.

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Dose-response versus ANOVA

Recently, I reviewed manuscripts that reinforce a pattern I previously observed in published papers, whereby ecologists are improperly using analysis of variance (ANOVA) designs when they should be using the dose-response model. This pattern is present in such diverse areas as *Bacillus thuringiensis* (Bt)-corn non-target toxicity, biotic effects of ultraviolet light, and invasion biology. My advocacy builds on Cottingham *et al.* (2005), who argued for the use of replicated regression over ANOVA, and other, similar reasoning. The dose-response model also generates a curve (in this case non-linear and typically analyzed with logistic regression) to evaluate the response (typically, but not exclusively, mortality) of test subjects to varying doses of a stressor. Curve generation provides insight into test-subject vulnerability, and allows lethal or effect doses to be calculated and compared with other stressors. Lethal doses (LD) are typically expressed as doses needed to kill 50%, 95%, or 99% of test subjects (LD_{50}, LD_{95}, LD_{99}, respectively) over specified time periods, typically 24 hours, although there is no reason why duration cannot be tailored to the system in question. It is a standard toxicological method of analysis.

Unfortunately, many ecologists apparently choose not to use the dose-response model or are unaware of it. Instead, they employ an ANOVA-based analysis of survival between treatment groups exposed to different doses or exposure times. Yet, when evaluating toxicity, choosing what doses and durations to compare with ANOVA is completely arbitrary. The dose-response model eliminates such subjectivity. Investigators are omitting treatment levels, especially at low doses. Test-subject responses to low doses provide valuable information, especially when evaluating the toxicity of agents that might be improperly applied in the field as part of a management protocol (eg through unintended dilution of a biocide). In addition to the "good" reasons to use the dose-response model, there are few, if any, reasons not to use it when evaluating toxicity. Dose-response reveals patterns that ANOVA cannot. Use of ANOVA reduces comparability between studies and stressors, obscuring evaluations of relative toxicity.

Use of dose-response in place of an ANOVA-based approach requires thinking a bit differently about experimental design and analysis. Such studies often begin with a range-finding experiment, in order to efficiently bracket test-subject response between doses that produce low response rates and doses that produce responses in all test subjects. Whereas differences in survival are typically tested with ANOVA, it is mortality that is analyzed in dose-response.

Ecologists could use a dose-response model at scales not typically seen in toxicology. The response of whole communities or ecosystems to varying doses of large-scale, non-traditional stressors could be examined – evaluating the presence of tipping points and providing hypotheses for temporal responses to increasing levels of stress. Such applications would not be without caveats, of course. In the case of large-scale ecological application of dose-response designs, there may be practical limits to maximum doses that can be produced in the field, resulting in truncated response curves.

As a "classically" trained ecologist, I was biased toward ANOVA until I conducted my own toxicological studies as a postdoc. I suspect that underappreciation or ignorance of the dose-response design is a product of canalization between training in basic and applied fields of study. Yet basic and applied ecology increasingly overlap, as the importance of the science underlying environmental issues increases. Thus, as ecologists – not otherwise trained as toxicologists – lead studies that blur the distinctions between ecology and toxicology, appropriate quantitative methodology should follow.

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No Agency positions are established in this expression of the author's views.

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A disappointing end

In the Dispatches section of the September issue of *Frontiers* (*Front Ecol Environ* 2009; **7[7]**: 349), Virginia Gewin's article, "FACE facing the end", discussed the termination of the Free Air Carbon Dioxide Enrichment (FACE) experiments and the unique insights into the influences of increased levels of atmospheric CO_2 on forest carbon dynamics gained as a result of that research. Although there is undoubtedly much to be learned by ending the experiments, harvesting the trees, and excavating soils and roots – the results of those measurements will certainly be enlightening – it seems as though the results of the FACE experiments themselves to date have demonstrated that its continuation is scientifically valuable and justified. Indeed, a number of results have shown unexpected temporal shifts in ecological responses, thus raising questions concerning the continued evolution of these ecosystems over the longer term, under elevated atmospheric CO_2 conditions. That being said, what is the "long-term" value of this type of ecological systems research, and should experiments such as FACE be extended?

To answer the first question, we need look no further than the Long Term Ecological Research (LTER) network to understand the benefits and scientific value in creating and maintaining a permanent network of research sites and experiments. While the scope and mission of the FACE experiments are fundamentally different from those of the LTER network, they are nevertheless a distinctive and critical set of experiments that should be maintained indefinitely, lest a landmark project on climate-change research be shuttered. The temporal scale and ingenuity of the FACE experiments have provided, if nothing else, a unique opportunity for scientific inquiry.

An answer, or rather an opinion, on the second question is that FACE and similar long-term investigations should indeed be continued. Long-term research is an invaluable asset in natural systems science and "in the absence of the temporal context provided by long-term research, serious misjudgments can occur…in our attempts to understand and predict change in the world around us" (Magnuson 1990). Without the continuation of the FACE experiments, even on a limited scale, it seems a valuable temporal perspective has been sacrificed, thereby limiting our long-term understanding of terrestrial ecosystems in a changing climate.

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Funding needed for assessments of weed biological control

Invasive non-native plants are a serious economic and ecological problem worldwide, and major efforts are therefore devoted to reducing weed abundance in agricultural and natural settings. Effective options for reducing invasive abundance and spread are few, although one common approach is biological control – the introduction of specialist herbivores or pathogens from a weed's native range to suppress weed abundance in the introduced range. Biocontrol is a crucial tool in invasive species management because, once biocontrol agents establish, they are often self-sustaining and can greatly reduce invasive populations.

Yet, as with all weed control efforts, biocontrol has its costs. Establishing a new biocontrol program is expensive, costing well over US$1 000 000 for the discovery, testing, rearing, and release of specialist enemies against a single target weed (Page and Lacey 2006). Weed biocontrol has also been