



The Role of Experiments in Ecology

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# LETTERS



## One world?

Ecologists discuss a News article in our special section “Frontiers in biology: Ecology” (21 July, pp. 313–360) and subsequent letters (1 Sept., p. 1201). While 24 letter writers describe one dispute as a “minor squabble,” the views expressed in other letters belie this description.

## The Role of Experiments in Ecology

We thank *Science* for giving ecology coverage in the “Frontiers in Biology: Ecology” special section (21 July, pp. 313–360). It was unfortunate that the lead News article by Wade Roush, “When rigor meets reality,” highlights a minor squabble that stemmed from the remarks of one postdoctoral researcher. We encourage the editors and reporters of *Science* to continue coming to ecological meetings so that they can broaden their knowledge and expand their coverage of the substantive issues. Ecology is a true frontier, being perhaps the most complex system that science has ever tried to understand. Increasingly, ecologists are combining experiments, observations, and theory to expand the temporal and spatial scale of our inferences. We are strongly motivated by the pressing need for answers to major questions of direct relevance to the long-term sustainability and habitability of Earth.

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Roush’s article portrays the American Society of Zoologists’ symposium “The State of Experimental Ecology” as an “organizational rally of sorts” for the “new experimentalists” and as part of a “revisionist movement” advocating a return to more “muddy-boots biology.” As co-organizer of the symposium, I strongly disagree with this portrayal. Although the coverage given to this symposium is appreciated, the article confers a negative tone on the proceedings and does not convey the scope and goals of the symposium. I also disagree with the article’s presentation of the important issues in experimental ecology.

The symposium brought together experimental ecologists representing the broad array of experimental approaches used in ecology, from laboratory microcosms to manipulation of entire ecosystems, in order to illustrate the myriad ways in which experiments are applied to ecological questions. The symposium specifically emphasized the value of a plurality of experimental approaches; it was definitely *not* about attacking other ecologists or “challeng[ing] . . . colleagues’ methods” (nor were my own discussions with Roush). It was experimental ecologists critiquing themselves to move experimental ecology forward on all fronts, from better designs, to better links between experiments and theory, to more realism in experiments designed to explore specific natural systems. It was also a forum in which to discuss the limitations and obstacles to applying experiments to specific ecological systems and questions. Our only agenda was to reinforce the importance of experiments and experimental rigor in un-

derstanding ecological processes and to stress the need to continually improve our application of experimental methodology and achieve better integration between experiments, theory, and natural history. Our goal was to ensure that the rate of progress in the application of experimental methods to complex ecological problems continues to accelerate. It is unfortunate that the article did not capture the energy and positive tone of the symposium, and missed the real story of experimental ecology: the tremendous progress in ecological understanding achieved through experimentation.

Similarly, the article depicts my personal views in ways that I would not and so vaguely ascribes opinions that I have subsequently been criticized, in print and elsewhere, for statements I did not make and opinions I do not hold. I presumably criticized "experiments [that] often reduce nature to oversimplified caricatures that have little to do with the real world." That certainly does not reflect my view, as much of my work makes use of mesocosms (1), and I firmly believe that such simplified systems instruct us about the real world. Subsequent letters (1 Sept., pp. 1201–1203) criticize me for attacking Andrew Blaustein. I was not quoted regarding his work, as I had, in fact, refused to discuss it.

The article's negative tone was amplified by exclusion of positive statements or by their paraphrasing into negative, critical statements. I have been critical (2) of Dolph Schluter's recent experiment (3) and agreed to discuss it because the paper was published and criticisms rendered in *Science*. However, my repeated caveat that criticisms were limited to the specific experiment and that Schluter's other work on character displacement is compelling was not included. Even a positive prescription for experimental ecology penned (with Joseph Bernardo) at the request of *Science* was paraphrased into a series of negative statements on what experimental ecologists "fail" to do, and then linked with another quote that neither should have been made nor printed.

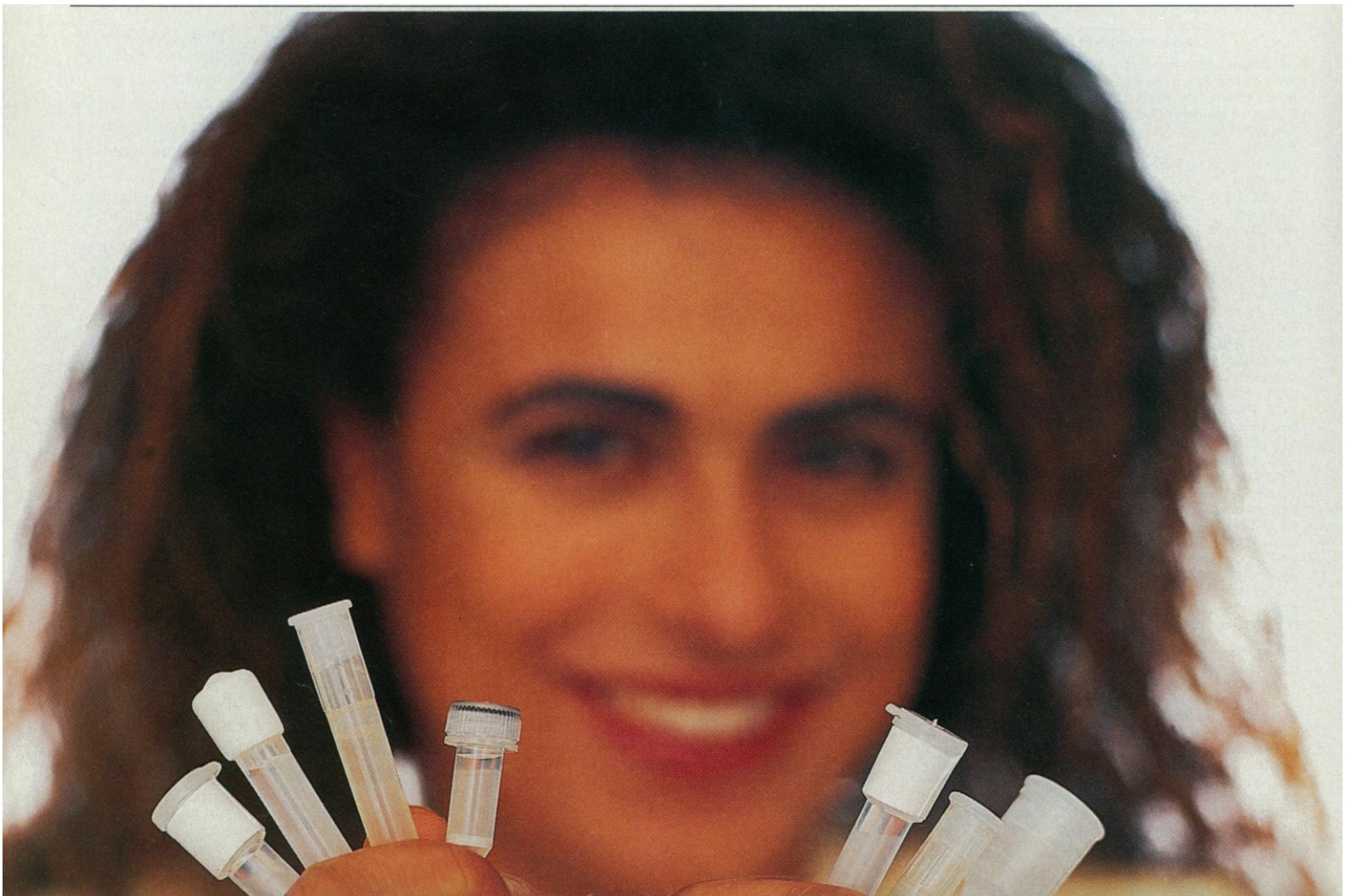
There was an interesting article to be written about the tremendous strides made in ecology through experimentation and the many directions experimental ecology is taking under several generations of experimental ecologists. Indeed, many of the important figures in the evolution of experimental ecology were interviewed, many more than were represented in the article. Why, then, were these strides and directions not made the focus of the article? The rationale given by *Science's* News editors was that these topics were simply "not engaging." I disagree.

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#### References

1. W. J. Resetarits Jr., *Ecology* **72**, 1782 (1991); *Oikos* **73**, 188 (1995).
2. J. Bernardo, W. J. Resetarits Jr., A. E. Dunham, *Science* **268**, 1065 (1995).
3. D. Schluter, *ibid.* **266**, 798 (1994).

My purpose in criticizing high-profile ecological experiments (1) is to stimulate reasonable debate about the fair extent of inferences that scientists make from their experimental results. This general aim is reflected in my efforts to co-organize a symposium whose goal was to offer constructive insights to improve the future practice of experimentation in ecological and evolutionary research. It is also reflected in my efforts to ensure the accuracy of *Science's* article, which I understood was to be about the role of experiments in contemporary ecological research, the focus of the symposium. To this end, I gave Roush our symposium proposal that detailed its diverse goals and a list of names and addresses of all of the symposium participants (many of whom he interviewed). I also spent more than 6 hours in three



interviews over several weeks expanding on these themes. Roush's article inaccurately represented the symposium and the spirit of our conversations. My criticisms span a variety of issues in the use of experimentation in ecological inference, ranging from problems of confounded designs and unnatural experimental conditions (1), to difficulties with the choice of experimental variables and treatment levels that affect interpretation, and overgeneralization (comments I made in Roush's article). I concur with Reznick (Letters, 1 Sept., p. 1202) that such issues are complex and deserving of careful discussion.

Neither my criticisms, nor our symposium, had much to do with young naturalists leading a rebellion against experimentation, or a call for a return to "natural history." Thus, I took exception to a draft of Roush's article that told a story of young naturalists revolting against the approaches of their older, experimentalist mentors. The draft included quotes from esteemed experimentalists—some of whom I had cited as instrumental to the development of experimental ecology—which were clearly at odds with my supposed views. I called Roush to respond to his draft. I told that it inaccurately represented the sym-

posium and our views, and that there was, in fact, no generational controversy about the role of experiments in ecology. I asked that he revise the piece to reflect the issues we had discussed and that he remove an introductory vignette that highlighted a nonexperimentalist's views that were extreme and, hence, did not fairly represent the symposium. Barring this, I insisted that references to the symposium and our quotes be removed from the piece, because the story that he said he was authoring was about broader issues surrounding experimentation in ecological research, not about resurging interest in natural history, a bias retained in the published article.

Further, it is disturbing that Roush ignored many constructive remarks I made in multiple interviews and that he chose to highlight—in a highly contrived, negative paragraph that distorted other statements we had made in an explicitly constructive way—part of a statement I made in an off-the-record conversation (not in one of the three interviews). My comment came at the end of a frustrating, 72-minute conversation (initiated by me) in which I tried to convince an unwavering Roush of the inaccuracy of his draft. I made an unfortunate, blunt statement emphasizing that there are

both older, seminal experimentalists who rooted their studies in natural history and many young ecologists who do experiments without the benefit of same, that is, that controversy between young naturalists and old experimentalists was imagined. I then contacted Roush's editor.

After I conveyed these concerns to the editor, the introductory vignette was deleted, and additional emphasis was to have been placed on other issues (experimental design, multiple causality, and so forth) discussed in the symposium. I suggested that a historical synopsis of ecology as a discipline would be a logical replacement introduction, but the editor dismissed this as "not engaging." *Science's* interest in provoking controversy rather than in telling a factual story about experimental ecologists of all ages and career stages taking a hard look at experimentation in our discipline—while ignoring indications from me and other ecologists that the story was inaccurate—is at best, regrettable. Curiously, the editor refused my repeated requests to review the final version of the article. This is particularly disconcerting in light of assurances to me by Roush and his editor that *Science's* motivation was to publish an accurate piece and their repeated thanks for my efforts to ensure this goal. Such an article would have

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been informative and easy to write, given the diversity of ecologists with whom Roush spoke and our symposium proposal that provided the necessary background. It is unfortunate that the article took such a narrow view both in topic and in highlighting my comments, particularly since it was the lead article in a special issue devoted to ecology.

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#### References

1. J. Bernardo, W. J. Resetarits Jr., A. E. Dunham, *Science* **268**, 1065 (1995).

**Response:** We invited Bernardo and other knowledgeable ecologists to comment on our article and we made changes based on their comments. As Bernardo points out, we even removed a vignette about a researcher with whom Bernardo disagreed. It was not appropriate, however, to shape the entire article to reflect Bernardo's views, which his letter makes clear was his intent.

Bernardo and Resetarits say that we ignored their efforts to focus the article on experimental design. Yet the article high-

lights their own comments and those of other scientists on some of the very issues—such as multiple causality and inference—they raise in their letters. And although they object to our portrayal of the roots of the debate, it was supported by other researchers, some of whom were quoted by name in the article. No one told Resetarits that the strides made in ecological experimentation were “not engaging”; indeed, the article included a long section describing those strides.

It is unfortunate that Bernardo now seeks to distance himself from one of his many “blunt statements” by saying it was made off the record. At no point in our discussions, including the interview he initiated, did Bernardo request that we not quote him.

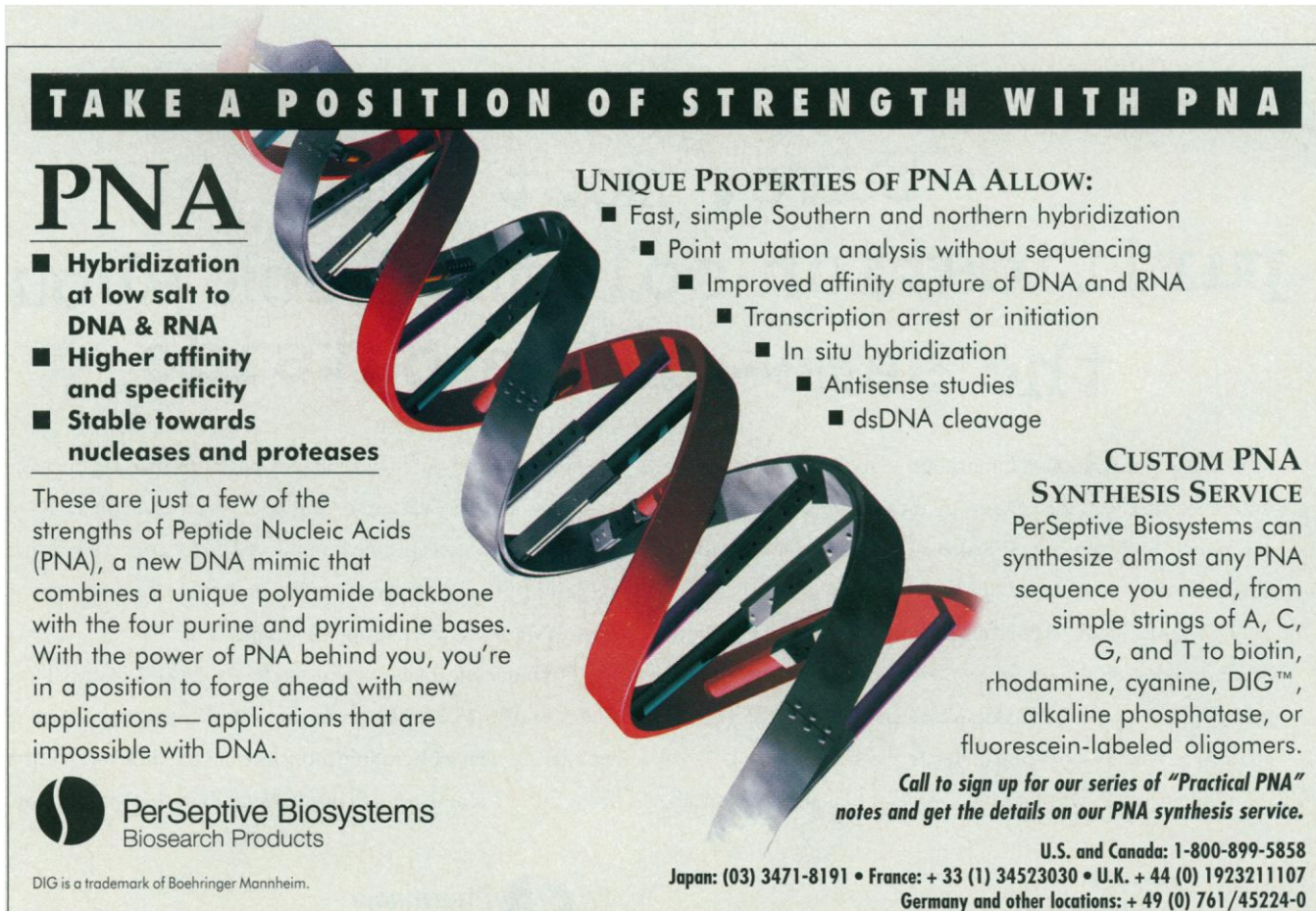
We regret that the idea of researchers seeking value in myriad experimental approaches did not come across more clearly in the article. We agree with Power *et al.* that ecology is a rich and important field and intend to continue our coverage of it. Our intent in this article was certainly not to provoke controversy, as Bernardo asserts. As these letters, and letters we published on 1 September, indicate, ample controversy already exists.

—**Joshua Fischman, Deputy News Editor**

## AIDS Intervention in Uganda

Rachel Nowak, in her News article “Testing AIDS interventions: When is the price too high?” (8 Sept., p. 1334), suggests that our study in Rakai District, Uganda, which uses intensive control of sexually transmitted diseases (STDs) through mass treatment as a means of preventing HIV (human immunodeficiency virus) transmission, “runs counter to internationally accepted guidelines.” The basis for this statement is that the international guidelines recommend that should the therapy prove efficacious, it should “be made reasonably available to the inhabitants of the host community or country,” and Nowak writes that “If the intervention works, most Africans may not be able to afford the drugs.”

Drug costs are a relevant issue, but many of those used in the Rakai study are cheap, readily available in Uganda, and appropriate to the Ugandan context. Two drugs, Azithromycin and Ciprofloxacin were selected for their high rates of effectiveness against key STDs and their ease of administration, and their prices have been falling in the United States. Azithromycin now costs approximately \$9.50 per course of treatment, which is comparable to other recommended prescription regi-



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