

is mistakenly attributed to David Reznick, apparently because Reznick paraphrased one of my papers (5) on amphibian declines to Roush (6). It is unfortunate that Bernardo and Resetarits appear not to have read our papers carefully and have criticized us for what some of the popular press has said about our work.

Instead of being poorly grounded in long-term field data, as Bernardo alleges, we believe that our work demonstrates how long-term observations point the direction toward relevant, realistic experiments.

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3. A. R. Blaustein, *Herpetologica* **50**, 85 (1994).
4. ———, D. B. Wake, W. P. Sousa, *Conserv. Biol.* **8**, 60 (1994).
5. A. R. Blaustein and D. B. Wake, *Sci. Am.* **272**, 52 (April 1995).
6. D. Reznick, personal communication.

I wish to express my concern over the quote attributed to me in the article by Roush. The quote (which gives the incorrect im-

pression that I am critical of Blaustein's work) was actually derived from Blaustein's own writings (1). Blaustein is at the forefront of the worldwide investigations into all the potential causes of amphibian decline, including UV radiation. In view of his clear statement of likely multiple causes of the amphibian decline, I interpreted Blaustein's experiment as a test of the plausibility of UV radiation as one of those possible causes. The fact that the experiment was performed without the benefit of prior long-term data indicating an increase in UV radiation should not be a concern because, in a rapidly changing world, it is impossible to foresee what the important changes might be. Rather than criticize the work for not being motivated by such data, I instead view it as contributing to the motivation for collecting such data in the future.

More generally, it is ironic that Roush featured criticism of two such fine papers. Both Dolph Schluter (2) and Blaustein were working on systems for which there are abundant ecological data. Both took these prior observations into account when designing and executing their experiments. Both studies represent novel approaches to a problem and produced interesting results that should be of interest to a general, critical audience such as *Science's* reader-

ship. Both studies incorporated complexities that merit some open debate, so it is not unreasonable that one of them has been discussed in *Science's* Technical Comments section (3); however, the tone of Roush's news article in no way represents the subtleties of this kind of work or the costs and benefits of alternative experimental approaches to a problem, such as the role of density or the use of hybrids in Schluter's work. In my opinion, Schluter made the right decisions. For all of these reasons, I feel that Roush's article presents an inaccurate, destructive view of the scientific process.

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References

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3. P. A. Murtaugh, *ibid.* **268**, 1065 (1995); J. Bernardo, W. J. Resetarits Jr., A. E. Dunham, *ibid.*; D. Schluter, *ibid.*, p. 1066.

I am appalled and dismayed by the views attributed to Bernardo and Resetarits in the article by Roush. Experiments in ecology, as in all branches of biology, must be well grounded in an understanding of the natu-

This is my system for affinity
separations of polyclonal
and monoclonal antibodies,
enzymes and fusion proteins.

