
Elasticity Analysis for Conservation Decision Making: Reply to Ehrlén et al.

L. SCOTT MILLS,* DANIEL F. DOAK,† AND MICHAEL J. WISDOM‡

*Wildlife Biology Program, School of Forestry, University of Montana, Missoula, MT 59812, U.S.A., email smills@forestry.umt.edu

†Department of Biology, University of California, Santa Cruz, CA 95064, U.S.A.

‡U.S. Forest Service, Forestry and Range Sciences Laboratory, 1401 Gekeler Lane, La Grande, OR 97850, U.S.A.

We are pleased that our recent article has spurred further discussion on the uses of elasticity analysis in conservation, and we appreciate the thoughtful feedback of Ehrlén et al. We are in complete agreement with them that results of elasticity analyses must be applied to conservation issues with considerable caution. The only real disagreement appears to be over the degree of the mismatch that we believe exists between the theory and practical application of elasticity analysis.

Ehrlén et al. state that “rather than focusing on arbitrarily chosen cases, we need to consider the overall performance of elasticities.” We concur, and this is exactly what we did in our paper, using three case studies from the published literature that we chose based on the completeness of their data sets rather than the results they gave. The cases that Ehrlén et al. cite (de Kroon et al. 2000) are no more or less “arbitrary.” Certainly the overall performance of elasticities should continue to be evaluated for many taxa and data sets.

Ehrlén et al. take issue with our use of qualitative assessments and argue for a more statistical evaluation of how often elasticities lead to inaccurate or misleading predictions. Although statistical analyses have an important role, they do not necessarily portray the overall utility of an analytical tool for conservation decision making. We used qualitative terms because statistical metrics such as correlation coefficients or p values say nothing about the consequences for management of elasticity performance.

As Ehrlén et al. state, our analyses show that large but equal changes in demographic vital rates (age-specific birth and death rates) seldom caused a change in elasticity rankings. We still believe (p. 826), however, that “Even if such crossovers in elasticities are not common,

the potential problem is worth careful consideration, given the consequences of incorrect management recommendations.” Ehrlén et al. argue that rankings of elasticities should be avoided and that instead conservation biologists should be “interested in the actual change in population growth rate (λ) that can be achieved through different measures.” We have shown (Wisdom et al. 2000) that realistic variability in mean vital rates can cause dramatic shifts not only in elasticity rankings and values of elasticities but also in quantitative predictions of changes in actual population growth. Furthermore, such analyses, incorporating the effects of variation and covariation in vital rates, are possible only if one has reliable estimates of these parameters for a given population. Because such data are almost never available, especially for species of conservation concern, most published papers applying elasticity analysis qualitatively rank the “importance” of mean or median vital rates based on elasticities. Our analysis of changes both in elasticity rankings and λ with variation in vital rates does not condone the use of elasticity rankings based on one set of mean vital rates, but rather recognizes that this is the way elasticities are typically used.

Although we did find that when changes in vital rates were large or simultaneous, elasticities were “good qualitative indicators of growth-rate changes,” the problem remains that elasticities say nothing about how much different rates change. This is why elasticities alone cannot predict the “importance” of different vital rates: “Where matrices are complex or vital rates change by different amounts. . . as will often be the case, elasticity values can give misleading predictions of growth-rate changes when vital rates change” (p. 826). Ehrlén et al. agree with this but express concern that we implied that unequal variation in vital rates is “a problem of elasticities.” We appreciate, as Ehrlén et al. point out (and as did we), that “underlying relationships” in a matrix are nonlinear and “elasticities are local estimates of this function.” In fact,

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we did not fault the mathematics of elasticity (see the introduction and discussion of Mills et al. 1999) but rather the use of elasticities in conservation. As we endeavored to point out, the problem lies in applying a model that makes certain assumptions (e.g., instantaneous, one-at-a-time changes of vital rates) to real-world data and decisions that will violate those assumptions. By analogy, many others have focused on the conservation consequences of applying deterministic logistic growth models to the harvest of naturally fluctuating populations where model predictions will often lead to disastrous overharvest. The problem lies not with the mathematics of the logistic growth model but rather with making predictions when a mismatch exists between the assumptions of the model and more complicated real-world dynamics. Likewise, elasticities alone do not tell us all we need to predict changes in population growth in a variable world, so relying on elasticities alone can lead to misleading management recommendations.

Unlike Ehrlen et al., we do not believe that the distinction between “prospective” and “retrospective” methods (Horvitz et al. 1997; Caswell 1997, 2000) is helpful in guiding the use or interpretation of demographic analyses for conservation or otherwise. This classification dichotomizes life-history analyses that use observed variability in vital rates (retrospective) from those that use only mean rates (prospective). The label “retrospective” is used to highlight the fact that past variability may not reflect possible future changes (Caswell 2000), an important point. But the separation of such analyses from prospective ones, which use only estimates of past mean rates, is at best artificial and at worst highly misleading. Estimates of mean vital rates are every bit as idiosyncratic, error-prone, and likely to be unreflective of the future as are variance and covariance estimates. All analyses should recognize that *all* estimates of demographic variables must be used with more than a grain of salt. Any projection into the future is based both on what we know about the past and what we expect in the future. From a decision-making perspective, such projections should use all available information about the past, coupled with the changes that are thought to be biologically, politically, and logistically possible under future management. A constructive way forward would be to make an explicit statement about whether or not variation is included in the sensitivity analysis, where the estimates of variation (and mean rates) come from, and the rationale for potential future changes in vital rates (e.g., Wisdom et al. 2000). The retrospective versus prospective dichotomy only obscures the care needed in inferring management actions from inevitably imperfect data.

Finally, we do not agree that the way we used the ranges of vital rates is “a problem.” As we pointed out, and as Ehrlen et al. reaffirm, the range of variation does not tell us how much vital rates vary on average, or whether the variance arises from sampling error or from

process variation (temporal and spatial) (Mills & Lindberg 2002). Obviously, it should be the goal of biologists to obtain not only information on variance but also probability distributions of any given vital rate. The first step is to recognize that variance is important for population projection models, and the next is to understand the form of variance, the degree of covariance, and the likelihood of any particular vital rate (Gaillard et al. 1998; Grant & Benton 2000). There is no doubt that the critical next step is to collect the data needed to better incorporate these other complexities into population models. As an aside, we would go beyond Ehrlen et al.’s suggestion that “If the observed variation in transition rates is relevant and manageable, then a better option is to include it by rescaling the sensitivities to give the effects of a change in standard deviation units.” Rather, we would emphasize that, in real-world applications, observed variation is always “relevant” and is only occasionally “manageable.” Furthermore, simulation-based alternatives often will better incorporate both variation and the effect of individual vital rates on population growth for conservation decision making (Wisdom & Mills 1997; Crooks et al. 1998; Citta & Mills 1999; Grant & Benton 2000; Wisdom et al. 2000).

We believe that the last few sentences of our paper underscore our agreement with the constructive points made by Ehrlen et al.: “Elasticity values are surprisingly robust under some circumstances and can be a useful first step in establishing qualitative guides to population dynamics. Nevertheless, they can also give quite misleading results if interpreted without a clear understanding of their assumptions and limitations.”

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