LETTER

REPLY TO SOLOW:

Sense and nonsense in the choice of extinction priors

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Solow (1) raises a priority dispute over an equation, suggests that my Bayesian method of inferring extinction probabilities (2) is too liberal, and draws attention to an entirely different way to handle the prior specification. None of these points are helpful, and all require discussion.

Solow describes his equation 1 as a formal expression for my verbally stated equation (2) and cites his paper (3) to suggest that he derived it first. The two are similar because they fix the conditionals by incorporating an equation first published by Goodman (4). However, they are not the same: Solow sums the per-interval prior over the range $T + 1$ to infinity to obtain an overall survival prior, but I used a fixed value of 0.5. With respect to priority, I first saw a version of his equation after my paper (2) was submitted. His and mine were developed independently.

Solow's second point is that if sighting probabilities decline before extinction, then my equation (and many others) might yield extinction probabilities that are too high. I have shown that my method is highly robust to such declines (5). Regardless, I have already said that it is “impossible to say whether some of the inferred frog extinctions do represent steep declines” instead of extinction (2), and the abstract of my paper only stated that “Extinctions or severe population crashes have accumulated steadily” (2). Thus, there is nothing new or illuminating about Solow’s comment on this matter.

Solow’s third and most important point is an assertion that the per-interval prior extinction probability $\pi_j$ should be varied from 0 to 1. Following this protocol would be nonsense because the resulting per-interval probabilities would be scale-dependent and therefore completely subjective. For example, if intervals are years, then Solow’s cumulative prior probability of going extinct within a century is 0.9901. If intervals are centuries, then it is 0.5 (the same as what I assume).

In addition to the fact that my method is not scale-dependent, a crucial difference is that Solow’s distribution is extremely steep—much steeper than an exponential curve. When a finely split time scale is used, the sum of the probabilities starting with the interval following the last sighting and running up until now is therefore very low. This fact explains why Solow obtains a lower posterior in the Gastrotheca chrysosticta case. Solow’s reasons for adhering to a steep nonsense prior are unclear because his prior’s lack of realism has already been discussed (6).

As I outlined, a more reasonable approach would be to vary not the per-interval prior but the overall prior between 0 and 1. Doing so would render my method more properly Bayesian on Solow’s account. However, as I said, doing so “would have no effect at all” (2) because the result would be exactly the same for mathematical reasons.


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