Biological Conservation xxx (2009) xxx–xxx

Contents lists available at ScienceDirect

Biological Conservation

journal homepage: www.elsevier.com/locate/biocon

Letter to the Editor

The logic of persistence

It seems unreasonable to think Räikkönen et al. argued that Yellowstone wolves (or any other wolves) are, or are not, genetically threatened. If we had, as Mech and Cronin suggest (paragraph 3), we would have committed what logicians refer to as the fallacy of overgeneralization – a mistaken use of inductive logic. There is a significant difference in the logic of our argument: we referred to others who had used Isle Royale wolves to support arguments that other small populations are viable (genetically or otherwise), and then explained how this is inappropriate.

Mech and Cronin criticize contending that "many... downplay the threats posed by genetic deterioration" and that "genetic deterioration is likely a problem in many populations." An extensive literature supports the former concern (see references in Räikkönen et al.), and the latter represent a fundamental premise of the subdiscipline conservation genetics.

Mech and Cronin write that we "claim that selection against deleterious alleles (purging) will not increase population fitness." In fact, we wrote "...purging is now understood to be unreliable for mitigating inbreeding depression...", and we provided supporting references. Again, there is a significant difference in logic between what we argued and what Mech and Cronin claim we argued.

Moreover, Mech and Cronin argue that the existence of many captive populations represents evidence of purging's efficacy. However, Speke's gazelle is the only captive population considered, and only by some, as evidence of successful purging. Moreover, the costs of inbreeding for captive populations are too well documented to review here, and evidence suggests that inbreeding costs tend to be even greater in wild populations. Similarly, the existence of inbred, domesticated lines is not evidence of inbreeding's unimportance. The relevant observation is that most inbred lineages end in extinction.

Mech and Cronin write "despite inbreeding, the [Isle Royale] population has survived, the ultimate test." Like others cited in Räikkönen et al., they seem to believe Isle Royale wolves represent evidence that being small and isolated does not preclude viability. This seems the essence of Mech and Cronin's criticism. That proposition represents an argument whose salience goes beyond inbreeding, and relates to the logic of population viability generally. That argument seems to be:

P1. Some population is small and inbred.
P2. That population has lived in isolation with no detectable, adverse signs of genetic impoverishment.
P3. That population has survived for the past 60 years, 15 generations, or some similarly short period of time.
C1. Therefore, small, inbred populations are viable, or at least genetically viable.

A related argument might conclude: "Negative consequences of inbreeding are a relatively unimportant concern for small populations." Although these (or related) arguments seem necessary for supporting Mech and Cronin's central criticism, they are inappropriate for several reasons.

First, the arguments are invalid for neglecting an untenable premise that would equate persisting for 60 years with viability.

Second, persisting up to the present moment is not generally evidence of future persistence. Virtually every species having gone extinct during the current extinction crisis had, prior to its extinction, persisted for a very long time. Past conditions are often a poor basis for inferring future conditions. Isle Royale itself is an apt illustration. For two decades, Isle Royale wolves seemed to exert a strong, top-down effect on moose. For the next two decades they seem not to have. Most specifically, if the negative consequences of genetic deterioration accumulate over time in small, isolated populations – and there are good reasons to think they do – then persistence to the present does not reliably indicate future performance.

For many years it had been presumed that Isle Royale wolves were insensitive to inbreeding, because there had been no evidence to the contrary. In fact, deleterious effects had all along been accumulating undetected. It took 51 years of observation to detect what had been developing for some time. Deleterious effects are often difficult to detect.

The third difficulty with the argument above is that Premise 2 presumes absence of evidence is evidence of absence. To be valid, conservation-related arguments usually need a premise specifying where the burden of proof should lie (e.g., Precautionary Principle).

Times to extinction and inbreeding depression are both known to be extremely variable processes (Mangel and Tier, 1994). Taking note of one, or several, small populations with long persistence times is not necessarily evidence that small populations are viable. The argument above is therefore also an example of the fallacy of overgeneralization – the same fallacy Mech and Cronin believe we committed.

Mech and Cronin write: "The Isle Royale population is informative but not directly applicable to other populations of wolves or other species." Rather than representing evidence of some error in our logic, the claim reminds one of a fundamental and often underappreciated challenge for every ecologist, i.e., judging the appropriate limits of inductive inference, which represents so much of our ecological knowledge. Insomuch as good conservation requires the "unflawed application of research," as Mech and Cronin suggest, then it requires wrestling with logic that can be genuinely challenging for understanding appropriate uses of induction, and deeply normative for involving principles concerning burden of proof for conclusions that are otherwise scientific. These difficulties are associated with the advancement of arguments that are sometimes poor and sometimes have the unin-

0006-3207/$ - see front matter © 2009 Published by Elsevier Ltd.
doi:10.1016/j.biocon.2009.10.018

tended consequence of working against conservation (see, e.g., Nelson and Vucetich, 2009).

This exchange reminds one of that which surrounded Hedrick et al. (1996). Is concern for conservation genetics as unresolved now as it was 14 years ago?

References

Hedrick, P.W., Lacy, R.C., Allendorf, F.W., Soulé, M.E., 1996. Directions in conservation biology: comments on Caughley. Conservation Biology 10, 1312–1320.

Mangel, M., Tier, C., 1994. Four facts every conservation biologists should know about persistence. Ecology 75, 607–614.

Nelson, M.P., Vucetich, J.A., 2009. On advocacy by environmental scientists: what, whether, why, & how. Conservation Biology 23, 1090–1101.

John A. Vucetich
Rolf O. Peterson
School of Forest Resources and Environmental Science,
Michigan Technological University,
Houghton, MI 49931,
USA
Tel.: +1 906 370 3282.
E-mail addresses: javuceti@mtu.edu (J.A. Vucetich),
ropeters@mtu.edu (R.O. Peterson)

Michael P. Nelson
Lyman Briggs College,
Department of Fisheries and Wildlife,
Michigan State University,
East Lansing, MI 48825,
USA
Department of Philosophy,
Michigan State University,
East Lansing, MI 48825,
USA
E-mail addresses: mpnelson@msu.edu

Jannikke Räikkönen
Swedish Museum of Natural History,
Department of Contaminant Research,
Frescativägen 44, P.O. Box 50007,
S-104 05 Stockholm,
Sweden

Available online xxxx