**Review of “Population Viability Analysis of Western Gulls on the Farallon Islands in relation to Potential Mortality due to Proposed House Mouse Eradication,” by Nadav Nur, Russell W. Bradley, Derek E. Lee, Pete M. Warzybok, and Jaime Jahncke.**

**Reviewed by: Brian Halstead**

**Review date: 24 April 2013**

General Comments

This was an interesting analysis of the potential for a pulse of mortality caused by non-target effects of rodenticide application to persist into the future for a Western Gull colony. The model was appropriate, and the data used were likely the best available. Importantly, these data were derived from the population of interest. Most modeling assumptions were made explicit in the narrative, but additional discussion of the consequences of some of these modeling decisions might benefit readers. The clarity of the report could be improved in a few places, but the writing was overall very good. This report does a fine job of examining the potential effects of a pulse of mortality on this Western Gull colony, quantifying the mortality that could be sustained without lasting impacts differentiable from stochastic projections, and interpreting the results in the context of other factors affecting the trajectory of this Western Gull colony. Nice work!

Specific Comments

Last sentence of first paragraph on p. 10: Clarify that over-estimation refers to process variance (if that is indeed what would be overestimated by using the between-year standard deviation from 1986 – 2009).

Beginning of last paragraph on p. 10: Using capture probability in this way assumes that capture (resighting in this case, I presume) probability is 1.00 when an individual is breeding, and that no non-breeding individuals are observed. Are these reasonable assumptions for this system?

End of last paragraph on p. 10: Could you use the recruitment parameter from a Jolly-Seber model to estimate the probability of a non-breeding individual being recruited into the breeding population using the CMR data used to estimate the other parameters? This might then be confounded with the use capture probability presented above (as opposed to its traditional sense of the probability of capture, given that the individual is a member of the population and exposed to sampling). The age-specific initial breeding probabilities used are likely more appropriate for constructing a Leslie matrix, but it isn’t stated whether a measure of annual variation in these parameters is available.

First paragraph on p. 11: What are the consequences of being conservative in variance estimates (here and elsewhere) for the conclusions of this report? I suspect that an increase in variance (uncertainty) would result in a greater estimate of *C*, the number of individuals below which an effect of a mortality pulse from rodent control could not be distinguished from environmental stochasticity. Thus, underestimating uncertainty results in a conservative estimate of the level of mortality that the population could sustain without noticeable effects in the long term.

Second paragraph on p. 11: This approach seems a reasonable way to model episodic reproductive failure.

First sentence of second paragraph on p. 12: Would “age-specific” mean the same thing as “age-by-age?”

Third paragraph on p. 13: For the purposes of this analysis, how much does the distinction between apparent survival and survival matter? If the gulls permanently emigrate, they no longer contribute to the island population.

Third paragraph on p. 15: Is the proportional removal assumption realistic for this population, or might dominant adults potentially accumulate rodenticides at a greater rate via intraspecific competition? In the absence of data, proportional removal would appear to be a reasonable assumption.

First paragraph on p. 16: The description appears to be for a one-tailed “test:” if the median is shifted by 5% (i.e., the 0.50 quantile becomes the 0.55 quantile), then under the stated assumptions, only 90% of the two distributions would overlap (i.e, the upper 5% of the first distribution and lower 5% of the second would not overlap). I think the one-tailed test (proportion of the first distribution overlapped by the second) is appropriate here, but it might need a bit of clarification so readers are not confused. As written, I found the lengthy discussion of this more confusing than clarifying.

First paragraph on p. 17: What other values of *C* were examined?

Third paragraph on p. 19: I think this is a particularly important conclusion: the potential one-time loss of individuals has a much smaller effect on Western Gull population growth rate than other factors over which managers have little control.

P. 19: It might be worth stating that if density dependence operates on the gull population, it would tend to reduce the effect of one-time losses through compensation in survival or recruitment rates (or both). Earlier in the report it is stated that no evidence of density dependence exists for the gulls, so feel free to ignore this comment.

Throughout: Be careful of extra spaces and punctuation marks.