SOME COMMENTS ON THE CALCULATION OF EXTINCTION RISK FOR HAWAIIAN INSULAR FALSE KILLER WHALES IN NOAA TECHNICAL MEMORANDUM NMFS-PIFSC-22

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Inferences of an overall downward trend over the last two decades, and associated estimates of extinction risk for a postulated Hawaiian insular false killer whale population in Memorandum NMFS-PIFSC-22 (NMFS Memorandum) arise primarily from two sources of information:

a) an estimate of minimum abundance from a 1989 survey (Reeves *et al*., 2009) which is much greater than later estimates of abundance from aerial surveys and photo-id based mark-recapture estimates; and
b) the declining trend in sighting rates from a series of aerial surveys reported by Mobley *et al*. (2000) and Mobley (2004).

This information is converted into extinction risk estimates using the methodology set out in Appendix B of that Memorandum. However, I consider that further analyses are needed before such extinction risk estimates could be accepted, particularly as the sources of concern raised below suggest possibly appreciable sources of positive bias in these estimates.

1) Reeves *et al*. (2009, pg 258) acknowledge the possibility that their minimum estimates include offshore animals. This is acknowledged in the NMFS memorandum (pgs 104 and 115) as well. However in calculating extinction risk using a Bayesian approach, no consideration is given to this possibility. It is not included in any way in the “prior” options listed on pgs A-11 to A-13. Sensitivity 3 (pg B-13) with a broader distribution for the 1989 abundance prior might appear to account for this, but the results for that test are heavily influenced by the Mobley survey sighting rate time series, concerning which some questions arise below. A more appropriate sensitivity would use a much lower range.

2) The relative weights given to different realisations from the priors constructed depend on the likelihood evaluated for the abundance-related information. Here a number of queries arise.

   a) The formula given at the top of pg B-11 for the contribution from the photo-id based mark-recapture estimates is wrong. The *cv* should be squared and there is a multiplicative factor of 0.5 missing. It is unclear whether these are simply typos or whether this calculation has been performed incorrectly.

   b) Information detailing how the Baird *et al*. (2009) (unpublished) photo-id based mark-recapture estimates listed on pg B-6 were computed does not seem to be publically available, but the text there seems to suggest common factors for the estimates for the two different periods (2000-2004 and 2006-2009), in which case a (likely positive) covariance should be computed and incorporated in a modified formula to that at the top of pg. B-11.

c) While the change to a Poisson distribution for the likelihood component from the Mobley time series of sighting rate estimates (middle of pg. B-11) is appropriate, no attempt seems to have been made to take account of what might be substantial overdispersion in these distributions, leading to over-weighting of this information.

3) Put another way, point c) above might be re-expressed as a concern about the compatibility of Baird's (probably somewhat negatively biased due to neglect of heterogeneity effects) abundance estimate for the 2000-2004 period, and the absence of sightings by Mobley in the 2000 and 2003 surveys. The three earlier Mobley surveys in the 1990s provide an estimate of the survey "efficiency" parameter q. This allows a calculation to be made of whether the subsequent absence of sightings on these surveys is consistent with the Poisson distributional assumptions made, given the known abundance at the time. If there is not such consistency, the comparability of these surveys over time becomes questionable, which would be contrary to the current assumptions underlying the likelihood formulation.

4) Questions also arise about the CVs of the more recent Baird *et al*. (2009) estimates from mark-recapture used in the likelihood (typically about 20%), given that these are much less than the CV of 0.72 reported in Baird *et al*. (2005) for an estimate for the earlier period. Why such an enormous difference (and one with consequences for the weight accorded to these data in the extinction risk analysis)? The extent of compatibility of the estimates is unclear, given absence of detail on the methodology used for the later estimates. The earlier estimate comprised a likelihood-weighted average over a number of alternative models for including interactions in the computation of the abundance estimates. The two most heavily weighted of the eight alternative models considered in this process have non-overlapping 95% credibility intervals, which does raise questions about the defensibility of the averaging approach used. How has the issue of interactions been dealt with in the later Baird *et al*. abundance estimates?

5) A quite elaborate population model incorporating a number of different effects has been used for the extinction risk estimation within a Bayesian framework. However a Bayesian approach makes the underlying assumption of compatibility of the model assumed and the data input. That assumption becomes open to question in this case given, for example, the issue raised in 3) above. A particular concern is that a Bayesian approach can give an answer even if mutually inconsistent data are input, when that answer would clearly be wrong. Rather models and data inputs must be consistent, with if necessary different model-data combinations (each mutually consistent within themselves) computed, followed by consideration of relative plausibility.

I recommend that before the extinction risk estimates of NMFS Memorandum are considered, diagnostic checks be carried out on simpler models fit on the basis of maximum likelihood, in particular to check mutual compatibility or otherwise of the data used and the model and statistical distribution assumptions made. This exercise should also seek to include further reality checks. For example, the NMFS Memorandum suggests that fishing effects may have caused the population of the postulated insular stock to have declined since 1989. This would then imply that the fishing before 1989 also caused some decline – but if one back-projects on that basis, taking account of changing levels of fishing effort for which data would presumably be available, is the result compatible with the estimates quoted in the NMFS Memorandum for the likely maximum "pre-disturbance" population in the region? Only once firm conclusions about model, assumption and data choices had been drawn from such an exercise, would it seem to me to be appropriate to move further to the Bayesian framework to attempt to quantify extinction risk.

While my comments above do not pretend to be a comprehensive critique of the approaches taken in the NMFS Memorandum, I consider that until the concerns which I have addressed are addressed, the conclusions drawn in the Memorandum about extinction risk should be considered premature.

References

Baird, R.W. *et al*. 2009 (Unpublished, presentation to SRG in 2009.)

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