Review of PeerJ ms #86853 *Development of a new control rule for managing anthropogenic removals of protected, endangered or threatened species in marine ecosystems.*

Summary

The subject matter is relevant for the journal, and the overall approach is a valid one. However, there are substantive issues with the work such that the results are probably misleading. One concern is that the annual removals are assumed to be proportional to population size, without even any noise in the relationship. This assumption is not made in mainstream fisheries assessments, because the relationship between catch and fishing effort tends to be complex and subject to high variance. The assumption implies that the removals data provide the control rule with unrealistically precise information about the population. It also contradicts what the ms itself says about the quality of removal data. It matters because it is probably the main factor driving the results.

As noted below, the ms makes some further important (and questionable) assumptions without discussing or even stating them. There are also some errors of fact that should be corrected.

I consider that the authors should be strongly encouraged to address the points listed below, but obviously that is ultimately a matter for the editors and authors to settle.

Major points

1) The symbol *K* is used for two different purposes in the MS:
   (i) in the section headed **Legislative Framework** it is defined as the carrying capacity in the context of the ASCOBANS conservation objective to restore populations to 0.8*K* or above.
   (ii) in the section headed **Development of a stochastic SPM,** the model parameter labelled *K* seems to correspond to the population level at which expected value of the net recruitment rate is zero; if so, this should be stated explicitly.

   It would be better to use a different symbol for the two concepts. It is problematic to equate the two notions of *K* because under definition (ii) the population can drop below 0.8*K* even without any removals. The idea of the ASCOBANS 0.8*K* target is that removals should cause only modest population reductions below the "unfished" level. If a definition of *K* is used under which the population can drop below 0.8*K* even without any removals, then the ASCOBANS objective hardly makes sense for that definition of *K*.

2) In the section **Development of a stochastic SPM.** R_t is the (true) removal from the modelled population at time *t*, but in the section ***Likelihoods***, the R_t also denote the "observed" removals. It appears that the time series of R_t gives a perfect (zero-noise) index of relative abundance over time (where the scaling factor depends on the product of ρ and the bias factor in the removals data). This assumption amounts to treating the removals series as a zero-variance CPUE (Catch Per Unit Effort) series where effort is assumed to be constant. It stands in contradiction to what is written in the first para under **Control rules for managing removals** that such data are not collected systematically and are usually of low quality.

   Even if perfect (zero-error) removal data were collected, there would, even under constant fishing effort, in reality be considerable process variance in the relationship between removals and population abundance, because marine mammals are typically highly mobile and opportunistic; the factors that bring marine mammals into contact with fisheries can vary greatly from year to year because it depends on what prey the mammals are chasing and where they find it. Furthermore, fishing effort typically varies greatly: large fisheries come and go over the space of a few years.

As any cursory examination of ICES fisheries assessments will reveal, CPUE series have a notoriously high variance, and furthermore require a complex process of standardization to eliminate the effects of covariates other than abundance (vessel type, gear type, month, nationality, target species etc etc).

A key feature of Management Strategy Evaluation (as outlined in the papers on MSE that the authors cite) is that in simulations the management procedure should not be granted privileged information about the true population, beyond that contained in the data which are collected and used for management. The simulations in the paper appear to assume 50 years of removals data prior to the start of management. Coupled with the assumption that the removals data are assumed to provide a zero-noise index of relative abundance, this is a huge (and unrealistic) amount of information. It seems likely that the results that use the stochastic SPM are driven primarily by these "data", rendering the results of questionable relevance to the actual bycatch issue.

3) The ms states that both the PBR and RLA "hinge on an estimate of *r* (or r_{max} as input). This is not correct as stated. The RLA uses a prior for *r*, not an estimate or an input value. Arguably, the choice of prior is indeed influential. However, even more influential are the values of *r* in the simulated populations used to tune the procedures. This applies to all control rules, regardless of whether or not they contain a parameter that represents *r*.

4) Although the paper is about Management Strategy Evaluation, there is no explicit mention of the actual management measures that would be triggered by the control rules. The unstated assumption seems to be that a removal limit is set, and that each year a magic wand is waved to force the bycatches to exactly match the limit. This approach seems to be to have been copied over from that used to develop an RLA for whaling, where catches were reported on a daily basis and the season closed when the limit was reached. That is obviously not feasible for bycatch. Management measures adopted to influence bycatch levels tend to be of a different nature, such as mandating the use of acoustic warning devices, or seasonal closures of areas where bycatch levels have been especially high, or banning certain gear types. The relationship between such measures and bycatch levels is typically subject to considerable uncertainty and it can take some time for their effects to become apparent.

   A problem is that the ms doesn't explain exactly what question it is addressing. Is the aim to test actual, implementable management control rules, or is it just to obtain a general idea of what levels of bycatch might be consistent with conservation objectives? Either way, there needs to be some discussion of the how the outputs of the control rules would actually be used to influence removals.

5) In the simulations of this paper, the different control rules are not provided with the same data, hence it is not possible to discern to what extent the results reflect the different properties of the rules themselves or merely the different amounts of data they are given. As noted above, the control rules that use the stochastic SPM were provided with an annual index of relative abundance (in the form of the removals time series). As stated in its specifications, the RLA can use such data (called CPUE in the whaling and fisheries literature), even though it was not used in the tuning of the RLA because the fishing effort was considered hard to quantify (IWC 2012). In the simulations of this paper, the RLA was provided with the bycatch series for use as removals, but was not

directed to use the bycatches also as a CPUE series. Hence the comparison with the SPM-based control rules is not on a like-for-like basis.

6) **Appendix 2.** In the context of MSE, the identifiability of parameters is not directly relevant. However, the surprisingly high precision shown in Fig. 6 for the estimates of the parameters confirms the suspicion voiced above that the results are driven primarily by the assumption that the removals time series provides an exact index of relative abundance, every year from the start of the simulations.

   r, ρ and σ would obviously not be identifiable from just one abundance estimate (the posteriors would be the same as the priors). Even with three abundance estimates, the posteriors would not be much narrowed relative to the priors. The only way to explain Fig. 6 is that most of the information is coming from the removals, which are assumed (see comment above) to provide a perfect (zero-noise) relative index of annual abundance from the start of simulation (50+ years!). This is an unrealistic amount of information. The choice of the number of abundance estimates for the x-axis in Fig. 6 is misleading, because these are not the main source of information driving the results.

Other points

7) The structure of the paper is confusing. First, the SPM is described. Then two control rules (the PBR and RLA), which do not use the SPM, are (wrongly) described. Then two candidate control rules that do use the SPM are defined. The description of the SPM should be grouped with the control rules that use it.

8) **Development of a stochastic SPM.** The statement that "[the model] encapsulates all important knowledge about the phenomenon, in this case population dynamics of PETS" is not true for this model (think: migration, age structure etc) but nor does it need to be. Under the MSE approach, models are evaluated on management performance, not on their ability to encapsulate knowledge.

9) Equation (1). The text states that r^* is the "intrinsic population growth rate", which is normally defined as the population growth rate at low population size. However, because of the factor $(z+1)/z$ in equation (1), r^* in this equation actually the growth rate at N = MNPL (this growth rate is called MSYR in the marine mammal literature).

   The unnumbered equation below Eq (1) gives a continuous time version of the equation, but without the $(z+1)/z$ factor, so r^* in the two equations is not comparable. Since the continuous version is not used anywhere in the analysis, the mention of it could be omitted.

10) The reference to a "σ-algebra generated by the successive removals, abundances and environmental noises" should be explained or omitted. A σ-algebra is a subset, satisfying certain closure properties, of the power set of an underlying set. What is the underlying set in this case, and which of its subsets are included in the σ-algebra? Explain or omit.

11) Why are the mean and variance of ε_t defined conditionally on $\mathcal{F}_{t-1}$? In this model, the distribution of ε_t is independent of all that precedes t and has mean 1 and variance σ^2 unconditionally. Why not just say so?

12) **Reparameterization.** The relevance of this section is unclear. If the two parameterizations are indeed equivalent, then for the results it doesn't matter which one is used. If the two parameterizations are not equivalent, then we are talking about a different model, not merely a reparameterization.

13) **Removals Limit Algorithm.** This section erroneously describes the Catch Limit Algorithm (CLA). Under the CLA/RLA, no point estimates of D_t or r are computed, nor is any "best available abundance estimate" selected or used. At time t, the nominal removal limit RL_{nom} is a function of N_t, D_t and r (not of point estimates of these parameters):

    $RL_{nom}(t) = r \times N_t \times \max(0, D_t - IPL)$ (note : no « hats » over N_t, r or D_t)

    Because the fitted model is deterministic, N_t is itself determined by r and D_t and the removals history. The distribution of RL_{nom} is determined by the posterior distributions of D_t and r. The actual removal limit, RL, is fixed percentile of the distribution of RL_{nom}. Point values are not selected for any other quantity. To use a physical analogy, you could say that the "wave function" of the parameter vector is only collapsed in the final step in the calculation, to yield a point value for RL.

    The RLA removal limit is an absolute value, not a fraction of a "best available abundance estimate". The algorithm does not select or use any such "best" estimate. See IWC (*J. Cetacean Res. Manage.* 13(Suppl.):485-494 (2012)) for a specification of the algorithm.

    Looking at the supplementary info, it appears that the authors used a correct version of the RLA in the simulations (as far as one can tell), but the description in the text is wrong.

14) **Appendix 1.** With regard to the model actually used for the analysis, Appendix 1 does not seem to provide any additional information beyond what is in the main text. It can be omitted.