

Subject: Ecology and Evolution - Decision on Manuscript ID ECE-2019-06-00827 [email ref: DL-SW-2-a]

Date: Tuesday, August 13, 2019 at 5:10:57 AM Pacific Daylight Time

From: WOA Admin

To: Laura Cowen

CC: gjenkins@wiley.com

13-Aug-2019

Dear Dr. Cowen:

Many thanks for your Manuscript ID ECE-2019-06-00827 entitled "The effect of recycled individuals in the Jolly-Seber model with tag loss." which you submitted to Ecology and Evolution.

I will be happy to accept your paper pending minor revisions, detailed by the Associate Editor and referees - they are largely focused on clarifying several aspects of your work.

Once again, thank you for submitting your manuscript to Ecology and Evolution and we look forward to receiving these final revisions.

Sincerely,

Dr. Gareth Jenkins

Editor in Chief, Ecology and Evolution

gjenkins@wiley.com

-- Instructions --

Before submitting your revisions:

1. Prepare a response to the reviewer comments appended below in point-by-point fashion. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response, using line numbers to indicate where changes were made. If your manuscript does not currently include line numbers, please add them during revision..
2. Prepare a revised manuscript (word document), highlighting the changes you've made. Save this new document on your computer as you will be asked to upload it during the revision submission process. NOTE: Please be sure to keep in mind reviewer comments and incorporate your responses within the manuscript. There may well be areas where you disagree; for example, you may want to write, "A reviewer suggests that... However, I disagree because...". In any case, please try to address all of the concerns that are raised within the manuscript.
3. In addition to your revised manuscript with changes highlighted, please also save a "clean" copy where the changes are not marked.
4. If you have not yet uploaded an organism photograph, please do so and, if appropriate, designate this photograph as Figure 1. This may require you to include one or two explanatory sentences at the beginning of your manuscript. TIFF format files with a resolution of at least 300 dpi are preferred. Please direct any questions about this ecoevo@wiley.com.

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4. File Upload: Delete any files that you will be replacing (this includes your old manuscript). Upload your new revised manuscript file with changes highlighted, a "clean" copy of your revised manuscript file, any replacement figures/tables, or any new files. Once this is complete, the list of files in the "My Files" section should ONLY contain the final versions of everything. REMEMBER: figures/tables should be in jpeg, tiff, or eps format. If you haven't done so already, please consider uploading an image of the organism(s) studied in your paper to be considered for online cover and blog publication.

5. Review and submit: please be sure to double-check everything carefully so that your manuscript can be processed as quickly as possible.

Deadlines:

Because we are trying to facilitate timely publication of manuscripts submitted to Ecology and Evolution, your revised manuscript should be uploaded as soon as possible. If it is not possible for you to submit your revision in 2 months, we may have to consider your paper as a new submission. If you feel that you will be unable to submit your revision within the time allowed please contact me to discuss the possibility of extending the revision time.

Associate Editor Comments to Author:

Associate Editor

Comments to the Author:

I have received three reports on your manuscript, with all three referees agreeing it is suitable for publication pending minor revisions. All their suggestions should be addressed/commented upon, but they appear to be relatively straightforward.

I disagree with Reviewer 1 on the suitability of your manuscript for E&E, but do agree with Reviewer 2 that the code should be made available. Reviewer 3 mentions that the abstract is not suitable - I think this is subjective, and we are flexible with format here. I do think their suggestions on the title have merit - I would mention capture-recapture here, and possibly the study species as well. It would flag it more clearly as an ecological paper. However, this will be Open Access, so your chosen audience will be able to find it regardless, so it's entirely up to you.

Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

It's usually hard to get excited about a simulation paper, but this one was really a pleasure to review. Good job, all.

I like how clearly the purpose and message were communicated, and that the math, figures and text complimented

I like how clearly the purpose and message were communicated, and that the main figures and text complemented each other so well.

My one recommendation would be that the authors should add a paragraph to the discussion to discuss when the issue of bias is likely to become problematic; the example they used was instructive, but did not give much guidance in terms of when researchers should be using a model to account for recycled individuals

In addition, I have not done a line-by-line edit as I am not certain of the suitability of the paper for E&E. The paper is mostly methodological with not much of an ecological or evolutionary focus.

Reviewer: 2

Comments to the Author

This manuscript evaluates the effects of tag loss on parameter estimates in the Jolly-Seber capture-mark-recapture model. The authors use both simulated data to quantify how tag loss affects estimates of survival, recapture probability, tag retention rate, and population size under a variety of scenarios for those parameters. The authors also apply their tag-loss model to empirical capture-mark recapture data from Southern Elephant Seals that are both double-tagged and have a permanent brand that enables researchers to identify individuals that have lost both tags. Both the simulated and empirical data show that counting “recycled” individuals that have lost both tags as new captures leads to overestimates of population size, and this issue is exacerbated when tag retention rates are low and capture and survival rates are high. Including recycled individuals has little to no effect on estimates of survival, capture probability, and tag retention.

I find the manuscript to be clearly written (with one exception, described below), and useful as someone who conducts capture-mark-recapture studies where tag loss occurs. Although there is substantial literature on tag loss, I believe this issue and its effects on demographic parameter estimates are often ignored. I especially like that the authors demonstrate through simulation that the bias in abundance estimates compounds over time when recycled individuals are treated as new captures. This could be very concerning for long-term studies of long-lived species where estimating population trends is the goal. My main critique is that the language used to describe the models that recognize previously marked individuals which have lost tags (excluded or without recycled) individuals and models that treat such individuals as new captures (included or with recycled) is inconsistent and that led to confusion at times. I had to re-read statements multiple times to make sure I properly understood what a particular model represented. I recommend the authors pick one pair of terms for these two types of models (for example, included and excluded), explain them clearly, and stick with that terminology throughout the manuscript.

I also wonder if one aspect of the data simulation is unrealistic considering how researchers are likely to respond to loss of one tag in a double-tagged individual. I imagine most researchers would replace the lost tag for an individual that was recaptured retained one of two tags. I am curious how that would affect results from the analysis of simulated data and have provided more detailed comments about this issue below.

Major comments

-Under Data Accessibility, it states that this study’s data and scripts will be available on Dryad upon the paper’s acceptance. In the methods (line 107) it states that code can be obtained from the second author. I would encourage the authors to upload their data and R scripts, including the JSTL model to Dryad so that others can build upon their work. Requiring people to request code from an author adds an unnecessary barrier in my opinion.

-The terminology for recycled individuals included/excluded and with/without recycled individuals was not always clear and it took me a few readings to get it straight in my head. In part, I think this was because at times the terms were “included/excluded” and other times it was “with/without” recycled individuals. I recommend picking one set of terms to distinguish the two model types and sticking with it.

-For example, on lines 220-221 the text states “This bias is corrected in the analysis without the recycled individuals considered.” I did not like this phrasing because that model does “consider” the recycled individuals, it just correctly recognizes that they are not “new” individuals but had previously been marked. I have the same issue on line 353 (“excluding these recycled individuals from the analysis can improve accuracy of the abundance estimates”) because “excluding” makes me think these individuals are just thrown out of the data, rather than properly recognizing they were previously marked.

-Later under “Case Study: Elephant Seals” the two models are presented as 1) ignores the effects of recycled individuals and 2) recycled individuals were recognized. I would consider revising the description of #1 because saying that recycled individuals were ignored sounded to me as though they were not double counted. Perhaps explicitly label 1) and 2) with the included/excluded terminology to connect these two scenarios to earlier descriptions.

In the discussion, the authors state (lines 342–345) that future studies could simulate data for more levels of the relevant parameters (survival, capture probability, tag retention) to see how recycled individuals affect parameter estimates. I think the authors chose 3 reasonable values that pretty well cover the range of possible values for these parameters. However there really is not anything stopping the current study from simulating data with different parameter values and putting those results in the appendix. My point is that I would not expect a similar future paper to be published if it just repeated the same analysis but with slightly different values of p , ϕ , and λ , so if the authors think greater insight could be gained from simulating a greater range of values (or filling in the gaps between 0.2, 0.5, and 0.9), this study might be the place to present that information.

In the simulated data, some individuals are double-tagged and some have single tags. The status of individuals after their first capture is simulated sequentially to see if an individual 1) survives from time t to $t+1$, 2) loses any tags and 3) is recaptured if it has at least one tag. Presumably, if an individual is recaptured and has lost one tag, researchers could add a new second tag to replace the lost tag. If the goal of a study was to look at the effects of tag loss, one probably would not re-tag. But if the main goal is to estimate abundance and/or survival, one would likely give that individual another second tag to decrease the chances it is not recognized at time $t+2$ and later. Indeed, that is what I do in my own work, individuals shed PIT tags but can usually be recognized as recaptures because they are branded and brands last several years. What effect would replacing lost tags (but recognizing it is the same individual, and not treating it as new) have on parameter estimates? I expect it would reduce bias in parameter estimates because an individual would have to shed both tags between captures to be completely unrecognizable as a recapture.

Minor comments

Line 53 – Change “it’s” to “its”

Lines 69 – 71 – This sentence might be better in the “Case Study: Elephant Seals” section because it provides details about the study of one population of elephant seals. The previous sentence would be a stronger ending to the introduction.

Line 107 and Line 110 – R is cited as the software used for analysis twice. I think once is sufficient.

-Line 178 – I think the opening statement of the Simulation Results section (“The relative bias of the survival estimates are biased for some parameter combinations...”) is confusing and needs to be revised to something like “Survival estimates are biased for some parameter combinations...”

Lines 197 – 200 – This sentence does not contain much information and largely exists to tell readers to look at Table 1. I prefer statements that present a result (i.e., bias was greater for scenario X than scenario Y) and then point the reader to the table. The sentence on lines 221 – 224 is a good example of this style.

Figure 4 – The legend does not show that the three line types represent different values of λ , the caption just states that three different tag retention probabilities are shown in the figure. Also, the caption for Figure 4 states that three values of ϕ are shown, but this figure only presents relative bias for two values of ϕ (0.5 and 0.9).

Line 284 – Correct “analyzes” to “analyses.”

Table 2 – I would like to see the estimates of N_{super} for both scenarios included here.

Lines 345–347 – This is good, I agree that evaluating models with temporal or group-level variation would be an important follow-up study.

Line 348 – Remove “unsurprisingly”

Line 351 – The JSTL estimator of population size is only weakly affected by recycled individuals when tag retention rates are high (e.g., Figure 2, upper left panel). Saying that the estimate of population size is not affected feels like an overstatement.

Line 369 – I recommend revising “the JSTL model we looked at” to “the JSTL model we used” or something similar because this study did more than just look at the model.

Lines 372 – 374 – I find this statement to be too vague. It would be better to name some of these assumptions that are violated in the real world.

Lines 374 – 375 – The statement “... has the potential to answer and inform researchers and managers” is missing something. Has the potential to answer what? To answer researchers and managers’ questions?

Line 378 – I find “manage efficiently an ever increasing list of endangered species” to be awkward. It almost sounds like the goal is to manage the list (i.e., by adding or removing species) rather than to manage populations.

Line 450 – The author list for the Schwarz et al. (2012) reference appears incomplete. The middle authors are M.A. Hindell and C.R. McMahon.

Reviewer: 3

Comments to the Author

The authors present an interesting work based on simulations for studying double tagging experiments and assessing effect of recycled individuals on parameter estimates in the Jolly-Seber model with tag loss. The approach is also highlighted using long-term census data of elephant seals. They find a larger impact in estimating populations size when recycled individuals were ignored. The paper is mostly well written and provide a good readership. However, I have some concerns that described below.

1. I'm not sure current format of the abstract is what the journal is expecting. I would rather prefer a detailed abstract not in bullet form. The abstract need to be expanded highlighting main contributions. I also think that the title need to be rephrased to represent the work better.
2. It would be better to restructure the order of the paper to provide a better readership. As of now, many details are in web appendix and I find it difficult to follow. For example, complete data likelihood can be brought to the paper where the likelihood development is discussed.
3. Simulations settings and scenarios need to be discussed in terms of selections of parameter values: For example varying capture and survival probabilities.
4. In table 2, parameter estimates with and without recycled seem to be very close but population estimates show a considerable difference. Is there any reasoning for that?
5. In JSTL, how would you account for loss?

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