Decision

by Thomas Bataillon, 2019-02-25 10:05
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Revisions needed for your MS "The joint evolution of lifespan and self-fertilisation"

Dear Thomas & Sylvain First apologies for the very lengthy review process on that round!

Please find enclosed a last set of comments by the two anonymous reviewers.

Basically one of the reviewer is "content" but the second reviewer made a thorough reading of your revised version and flags a number of issues where possibly minor but important clarifications are still needed. He also make some suggestions for minor text reorganisation that are not compulsory per se but well worth considering

I am trusting that these last outstanding issues can be rapidly resolved within a week or two and I will be happy to recommend this paper provided that the revisions and clarifications suggested are implemented (no further review round will be needed).

While you prepare this last needed revisions, I will meanwhile draft my recommendation text.

Thanks again for submitting this interesting piece of theory for recommendation to PCI Evol Biology I look forward to your last and final revised version and to recommend your work within PCI Evol Biology

Kind regards
Thomas

Additional comments of the managing board:
We'll soon send you a message with specific requirements. So please do not upload the new version of your preprint before we send you this message.

Dear Thomas,

We replied to reviewers' comments and revised the manuscript accordingly. We hope you will be satisfied with these modifications. Thank you for taking the time to consider and ultimately recommend our work,

Best regards,

Thomas Lesaffre and Sylvain Billiard

Reviews

This manuscript is much improved. I appreciate all and is as close to being the best version of the paper that could be written, given the major assumptions and caveats that the authors are well aware of. It is still unclear to me if this result is

(A) a simple artifact of the lack of an apple-to-apples comparison between life history, as lifetime inbreeding depression is not held constant or (B) An important contribution pointing out that lifetime inbreeding depression will vary by lifespan and this has important consequences.

While an apples to apples comparison would strengthen the paper, it's unclear if that is realistic anyways.

Ultimately the answer to my concern will be settled by the paper's reception,

To perform an "apple-to-apples" comparison between life-histories, that is keeping lifetime inbreeding depression levels constant, one would need to artificially rescale the adult inbreeding depression parameter (δ_a), so that it diminishes as lifespan increases and ultimately leads to the same lifetime consequences. We believe this is rather unrealistic.

It is clear that if one was to apply such rescaling, no difference between life-histories would occur anymore, as these differences are mediated by the repeated effect of inbreeding depression on adult survival, as we state at the beginning of the discussion.

In this revision, Lesaffre and Billiard have updated their manuscript by adding new figures to clarify their results; elucidating the mathematical derivations; and include new results on the effect of inbreeding depression if it affects fecundity. The manuscript has been improved as a result but still needs further revising before it can be fully recommended.

(1) Outline of the methods. The introduction of the 'Model Outline' section needs some editing, as it combines mathematical results with simulation methods in an inconsistent manner, making it hard to follow. I propose streamlining it, so it first gives an overview of the basic model; then a description of the simulation/numerical methods, before presenting mathematical results from section 2.1 onwards.

We modified our manuscript to take this comment into account. Specifically, we split the description of methods and the results into two separate sections (2 and 3). We also updated the '2. Methods' section to make it more structured and clear.

Some specific points:

 • The methods and results that include inbreeding depression based on fecundity (e.g. P6 L103–104, section 2.3.2) seem oddly placed since they are not included in the basic model. I propose moving these results, along with a short description on how the simulation was changed, to a separate appendix.

We moved results regarding inbreeding depression affecting fecundity to a new appendix, Appendix VI.

• The paragraph on the effects of inbreeding depression (P6 L105-112), and the results on the proportion of selfed individuals (P6–7 L120–127), should be moved to section 2.1 so all the mathematical results, and their interpretation, are placed together.

We updated our manuscript. All mathematical results are now presented in the "Results" section. This result is presented at the very beginning of section '3. Results', as it is used in all the later sections.

(2) Figures. I'd like to thank the authors for including the new Figures 2–4, along with the simulation results. Together they greatly improve the manuscript. That said, they could still do with some refinement:

• Figure 2: Each panel is quite hard to read since both the arrows and the simulation points are very small. In addition, the text on P12 L241–246 that describe the figures themselves should be added to the caption for Figure 2. Finally, it seems that the phase trajectories deviate from the theoretical expectations for high selfing values in the first two panels; why is that?

We increased simulation points' and arrows' size in Figure 2, and removed the figure description from the text. We also checked that captions were precise enough for the reader to understand the figure without this small text.

The deviations noticed by Reviewer #2 for high selfing rates in the first two panels likely refer to the fact that the selfing rate and reproductive effort appear to stabilize close but not <u>at</u> one exactly, in contradiction with analytical predictions. This can be attributed to the fact that modifiers are allowed to mutate freely on the [0,1] interval. Indeed, as the trait value approaches a bound, mutations will tend to occur in the opposite direction as all mutations going over this bound will be cut back to the bound value. Since mutations are occurring constantly, modifiers will never become fixed for a single allele, and some variability will therefore always be observed.

We added a small paragraph to explain this phenomenon in section 3.3 (P13 L258-267).

• Figure 3: Here too the simulation points could be made bigger, and the text on P13 L260–264 also seems like it should be added to the caption. There does not also seem to be a discussion on the meaning of these results.

We made simulation points bigger in Figure 3. This figure is simply a summary of the fit between simulated and analytical results, and does not bring new biologically relevant information. It is only intended to bring confidence to the reader in our analytical approximations. We stated this in the small paragraph mentioned above (P13 L258-267).

• Figure 4: The caption for each subfigure should be changed, as it appears to suggest that the *ratio* of j/f is the parameter that is being changed.

Agreed. We updated Figure 4 accordingly.

• Finally, please consider adding the parameter name to the axes for all figures, and not just the symbol.

We believe the space necessary to make these larger axes titles readable would be to large and figures would lose in clarity. We checked our figures captions, to make sure it explicitly stated what each axis depicted, and stuck with symbols in axes labels.

(3) Mathematical derivations. I'd also like to thank the authors for updating Appendix II, however I have to admit that I still cannot follow some of their derivations. In particular, while $D_{m,m}$ is now defined for juveniles, it is unclear how A18 follows from A17. It seems that if there's a m,m term, then X_{jm} is somehow related to X_m but it is not made clear. Could the author clarify this point, and double–check the rest of the appendix to improve the clarity of the derivations where necessary?

We added more details about how Equation A18 relates to Equation A17 in Appendix II, and checked the rest of the appendix.

Some small suggestions for improving the main text: • P3 L57: Write "on *the* one hand". • P3 L60: Replace "was only" with "has only been". Similarly, for P4 L62, replace "was never tacked" with "has never been tackled". • P9 L191: Replace 'get' with 'are'. • P9 L194: Should this be 'differentiating' instead of 'deriving'? • P10 L224: Write 'there are' instead of 'there is'. • P15 L273: Write 'in both case*s*' • P15–16 L275–276: "thereby diminishing the proportion of selfed individuals in the population" repeats what was previously written, so should be deleted.

We incorporated all these suggestions to the main text, thank you for pointing them out.