

Response to the recommender

We would like to thank you again for evaluating our work and for the quality of the reviews. We tried to address all the remaining concerns of the reviewers about the structure and content of the article. Please, find below our answers, and find at the following link the new version of the manuscript:

<https://www.biorxiv.org/content/10.1101/2020.04.14.038893v3>
<<https://www.biorxiv.org/content/10.1101/2020.04.14.038893v3>>

We also provided a pdf with the tracked modifications.

We first want to thank the reviewer again for this review of our paper. We tried to take into account all remaining comments.

>There is one major point that still requires some attention: One of the reviewers is still concerned with the number of eusocial species included in your analysis and the conclusions that you draw based on that limited number of species. While I understand the rationale for sticking to the dataset of Peters (which you have explained very clearly in your rebuttal) I agree with the reviewer that a thorough test would require a larger number of species. I think you should explicitly mention that in your manuscript, make the proposition on how to test this based on the reviewer's suggestion, and put less emphasis on this finding throughout your manuscript (e.g., the largest part of your discussion still focuses mainly on eusociality).

We agree that our results regarding the effect of eusociality lack support due to the small number of eusocial species in the dataset, and that our discussion still focused too much on these results. We modified our abstract, the last two paragraphs of the introduction, as well as a good portion of our discussion to put less emphasis on eusociality. The first section of the discussion, which was entirely about eusociality, has been compressed to one paragraph. We also added in this section one small paragraph discussing body size and parasitism, as well as one final paragraph expressing the limitations of the dataset.

>As you mention in your rebuttal, you consider the high dN/dS ratio in social and non-social pollinating bees, as well as the *Antophila*, as your most novel findings. I think it will be worthwhile to spend more time on this in your discussion and to provide a perspective for future studies.

This is indeed our main results and we have now put more emphasis on it. The last two paragraphs of the introduction have been modified, notably around 1.57, where we put emphasis on pollen collecting and introduce at more length the hypothesis regarding the effect of parental care on N_e . The second section of the discussion, focusing on bees, is now longer, and contains a new paragraph, dedicated to discussing the links between pollen collecting and the distribution of species (as the reviewer suggested in the first review, which turned out to be a stimulating proposition, leading to constructive reflection on our side).

Also, as requested in your carefully commented document, we added a new supplementary figure (figure S3) that represents the phylogenetic position of parasites on the hymenopteran tree. We also took into account all the useful corrections, many thanks for having improved the quality of the manuscript.

Response to reviewer 1.

We first want to thank the reviewer again for this review of our paper. We took into account all suggestions comments.

>The revised manuscript “Relaxation of purifying selection suggests low effective population size in eusocial Hymenoptera and pollinating bees“ by Weyna & Romiguier uploaded to bioarxiv and under review for PCI Evolutionary Biology is a bit difficult to review as no responses to reviewers were delivered.

The response to reviewers appears on our PCI interface along with the tracked changes file, maybe it was missing or not clearly presented in the PCI interface of the reviewer? Here is the link:

https://evolbiol.peercommunityin.org/download/t_recommendations.reply_pdf.997807a0d5eab13b.726573706f6e73655f746f5f7265766965775f726f756e64312e706466.pdf

>However, it seems that the author addressed all major conceptual issues I raised during the first review. There are just a few minor issues listed below that I could point out now, as line numbers were used in the revised version of the manuscript.

>L29, L47 L61, L74, L79, L240, L251, L252, L253, L262, L277 Ne in italics.

>L75 ...expected to rely....

>L89, L101 hymenopteran species or species of Hymenoptera

>L154 literature

>L166 R2, btw, what is the justification of using that many digits? Can you justify that type of precision and accuracy by the methods used to calculate, I think it implies higher precision than you actually have. I think you can stick with 2-3 significant figures.

>L216 relaxation of purifying selection

>L270 too many brackets for the citation

>In some place you might have used two subsequent spaces. Check and replace with single space.

All done, many thanks for all these corrections.

Response to reviewer 2.

>The authors have done a decent job of addressing most of my previous comments.

Thanks again to the reviewer for all the useful comments.

>However, I still remain concerned that they have such few eusocial species in this dataset and are trying to write a paper about eusociality. The genomes you need to do this exist and while I realize it would take a lot of work to redo all of your analyses, I still worry that your results do not reflect what is really going on. Perhaps someone will use your paper as a hypothesis to test this idea across a much larger dataset.

We agree that putting so much emphasis on eusociality was somewhat inadequate given the limited number of eusocial species in the dataset. We have now re-written our abstract, the last two paragraphs of the introduction, as well as a good portion of our discussion to put less emphasis on eusociality. We have added a paragraph at the end of the first section of the discussion that explicitly mentions the limits of the dataset regarding eusociality and ponders our results in that regard. The main focus of our article is clearly the surprising high dN/dS featured by solitary bees , and we hope that the new version of this manuscript reflects it well.