

First, we thank the recommender and the two reviewers for their constructive comments and corrections which improved the manuscript.

In addition to these modifications, we also tried to solve analytically the system for two species, as suggested by the recommender and reviewers. However, due to the offspring production function considered, and the different competition and predation effects on males and females, we did not manage to find an explicit expression for the male proportion at equilibrium. We added the details of the mathematical analysis for the two species system in supplementary information, and used it to justify the numerical approach of this study.

Review 1

General Comments

This work studies the influence of sex-ratio and female noxiousness on the population dynamics and the risk of extinction of one or several species in coexistence of Aculeata. The study of Aculeata populations drives the motivation of the paper, but the model studied can be used to study any species in which females are the only defended individuals and competition between males can be neglected. Results are numerical and are obtained by performing simulations on a mathematical model. First, they study the influence of sex-ratio and female noxiousness on local extinction risk in a single population. Then they carry a similar analysis for two populations in interaction. They do so comparing the presence and absence of mimicry between species. Lastly, they study the case of dual sex-limited mimicry, in particular, when males of one species mimic another species which is monomorphic. The title however, only reflects this last point. I suggest the title to be modified in order to reflect the full scope of the paper.

We thank the reviewer for this suggestion and we slightly modified the title by removing ‘sex-limited’ in order to more clearly reflect the main topic of the paper.

The work contextualizes extensively its novel contributions by providing a rich bibliography, both in the introduction and the discussion of the results.

Major issues

I do not find any major issues that prevent the publication of this paper.

Minor issues

1. Equation (7) should not have a $\times F_i$ at the end. M_i should not be multiplied by λ_i in the numerator of the right term.

We thank the reviewer for rising these misprints and we modified the text accordingly (Line 198).

2. In system (11), F_2 should not appear in the denominators of the first, second and fourth equations, since females of species 2 do not belong to the same mimicry ring. Indeed, when explaining the meaning of each term, it is written properly. These mistakes, I suppose, are typing mistakes. If not, they become a major issue of the model and simulations must be rerun.

We thank the reviewer for careful reading of the system (11). Indeed, it was typing mistakes and we checked that all our simulations were actually ran with the correct system (Line 284).

I miss a small explanation of how the “slightly different model” for the dual sex-limited mimicry is obtained. This can be done easily by explaining that, in this case, a different value of s_{ij} must be assigned for males and females.

We thank the reviewer for pointing out this lack of explanation and we modified the text following his suggestion (Lines 282-283).

3. In the beginning of section 2, if the amount of females (F_i) and males (M_i) is chosen, then its proportion (ρ_i) is fixed too. The three cannot be chosen at random.

Thank you for pointing this out, we modified the text accordingly (Line 214-215).

4. The values and intervals for the parameters are “chosen based on previous exploratory simulations”. Is there any reason to expect these values to occur in the wild? The plausibility of the values of the parameters is not discussed. It is only a recommendation since this might be out of the scope of the authors, but, despite being a numerical exploration of a model, it could benefit from some notes on this matter.

We now provide detailed information on the explored parameter values and their potential significance: *“Very few ecological data are available in the literature to accurately estimate the values of most parameters, and some parameters might be difficult to directly measure in the wild (e.g., λ , α and β). Therefore, the intervals explored and the fixed values were chosen based on previous exploratory simulations: we focused on parameters values enabling a large range of possible outcomes (i.e., values below or above these ranges force the maintenance or extinction of populations) to explore a diversity of ecological scenarios. Note that the absolute values considered might depend on the relationship between the parameters and the number of species studied.” (Lines 216-222)*

5. Figure 1. presents only few clearly distinct regions despite using a continuous color scale. Do values of the proportion of males at equilibria vary drastically across the dotted lines? If so it is worth commenting it. If these equilibria vary continuously, as I would expect, judging by the sampling done in the parameter space, the color gradient of Figure 1 should look more like the one in Figures 3 and 4.

Thank you for pointing this. As expected by the reviewer, the equilibria vary continuously. We modified figure 1 to clarify the color gradient (Line 316).

6. Last sentence of the first point of the discussion is too ambiguous in my opinion: “In the context of massive population decline caused by anthropic activities, the extinction risk in Aculeata might depend on the variations of their sex-ratio through time in the different species, but also on their resemblance with other defended species living in sympatry“. If we weren’t in a context of massive population decline caused by anthropic activities the extinction risk would depend on other factors? And if the massive population decline was not caused by anthropic activities? Without a massive population decline at all these factors would also influence the extinction risk? Results are based on a model which does not take context into account. This kind of sentence is repeated at the end of the point 2 “Since wasps and bees are important pollinators, [...], and since we observe a significant decline in pollinator populations, mimicry could be an important factor to consider to better understand the decline dynamics of these insects”. Wouldn’t it be an important factor if they weren’t important pollinators? I understand the authors try to convey the importance of the study they carry in the present context, but premise and consequence seem disconnected in these sentences. I encourage to modify them in the spirit of the ending sentence in point 3 of the discussion, where a similar idea on the importance of the results in the current context is conveyed in a much less ambiguous manner.

We thank the reviewer for highlighting these ambiguities in the discussion. We simplified the last sentence of the first point (Lines 482-485), and we rewrote the end of the second point to connect our results with the current context: *“Mimicry between wasps and bees is a relevant factor to better understand the population dynamics and co-existence of Aculeata species. More broadly, since Aculeata are important pollinators, as are some of their Batesian mimics such as hoverflies (Syrphidae; Doyle et al., 2020), the positive effect of mimicry on co-existence could be even more important to consider given the current decline in pollinator populations (Biesmeijer et al., 2006; Hallman et al., 2017).” (Lines 522-526).*

Missprints and typos

- 163: “mentioned, we considered” is in a different font
- 201, 202: It should read β instead on β_j
- 236: “h1” should be “h₁”
- 350: “Two” instead of “tow”.

We thank the reviewer for rising these misprints and we modified the text accordingly.

In conclusion, my advice overall is positive and I encourage the acceptance of this manuscript for publication at PCI Ecology once the issues I detailed have been addressed.

Thank you for the attention given to our paper and for these constructive comments.

Review 2

Dear Editor,

the authors present an ordinary differential equation based model for Aculeata male and female population sharing a common predator community and exhibiting competition for resources. More precisely, they explore:

- the effect of sex-ratio and female noxiousness on local extinction risk (one-species model);
- the effect of mimicry on species persistence and co- existence, with particular focus on the dynamics motivated by changes in the sex ratio (males/females) and the females' noxiousness.
- And finally, the interaction between mimicry and sex-ratio in the case of a coexistence equilibrium when dual sex-limited mimicry occurs between sympatric species.

It is, therefore, a really interesting problem to tackle.

The model itself is explicitly defined in the Material & Methods section. Therein, the authors assume mathematical representations, supported on previously published papers, for (a) offspring production, (b) competition intra and interspecies, and (c) adult mortality. This results into a quite accurate model, too complex for an analytic analysis, which necessarily is approached numerically. However some of the parameters are, approximately, obtained from the literature, some others (like birth/death rates, initial values of any sex population, predations rates, etc) have been chosen randomly. Instead of a possible dynamical systems' analysis the approach presented in the manuscript is based on a suitable number of realisations (with the above-mentioned randomly chosen parameters), mainly 500, and a subsequent study of their averaged consequences. The results and accurate biological conclusions are presented in the final section. The paper is well written and correctly structured: an introduction with many references to previous literature, a good justification of the different pieces forming the model, and an interesting final biological interpretation of the numerical results and their discussion. Because all of this, I believe it deserves being accepted for publication.

We thank the reviewer for his interest in the paper.