

Review on “Influence of sex-limited mimicry on extinction risk in *Aculeata*: a theoretical approach” for PCI Ecology

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.

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.
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. 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.
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.
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.

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.
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.

Missprints and typos

- 163: “mentioned, we considered” is in a different font
- 201, 202: It should read β instead on β_j
- 236: “ h_1 ” should be “ h_1 ”
- 350: “Two” instead of “tow”.

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.