Review of "Competitive ability of a tetraploid selfing species (*Capsella bursa pastoris*) across its expansion range and comparison with its sister species" by Yang et al.

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This paper aims at testing the hypothesis that selfing species should show reduced competitivity relative to outcrossing species. This prediction is based on two possible mechanisms: either a trade-off between colonization ability and competitivity, or an increase of the mutation load in selfing species or populations. This prediction is tested using an experiment in controlled conditions involving four sister-species of the *Capsella* genus: three selfers (two diploid and one tetraploid) and one diploid outcrosser. The idea of the experiment is to compare the fitness of individuals of each species in the presence or absence of individuals of another species. The fitness is estimated through the measure of one vegetative trait (rosette surface at two successive time steps) and two reproductive traits (the number of flowers and the probability of flowering). The authors predict that the tetraploid selfing species should show a lower decrease in fitness than the selfing diploid species, because of partial masking of the effect of deleterious mutations. The authors claim that the main results are in agreement with the predictions.
We think that the addressed question is interesting and is worth investigating, that the experiments are well-suited with the aim of the study, that the statistical analysis are well-conducted and the results are convincing. However, we think that the interpretation of the results needs much improvements, tuning and clarifications. Indeed, the authors make strong statements based on limited evidence, and alternative explanations for the results could be further discussed. In particular, only one of three measured traits showed significant differences in the response to competition among species. One can argue that the authors are biased in their intepretations in order to find evidence that go in the same direction as their predictions. Indeed, one could interpret the same results as evidence against the actual predictions: since two of three fitness traits do not show significant differences, selfing has no effect on competitive ability. We thus suggest that the authors be more moderate in their interpretations and conclusions.

1 Major comments

- Our major concern is about the conclusions of the authors regarding the agreement between their results and theoretical predictions. Throughout the discussion, the authors write sentences such as (*e.g.* p.14) "Our main findings are in agreement with theoretical predictions." Using the same results, one can conclude the opposite: most measured fitness traits are not in agreement with theoretical predictions. It seems to us that the authors draw strong conclusions that are only weakly supported. The paper would benefit from more balanced statements, interpretations and conclusions.

- Another important concern is about the absence of control for phylogenetic relatedness between species or within species/between area. This is important to take into account because of possible pseudo-replication in the statistical analysis. This further weakens the interpretation of the results by the authors since the significant differences that are observed on only one trait could solely be due to phylogenetic relatedness. This is true both for between and within species comparisons.

- A related point is whether the three selfing species evolved self-fertilization from a single or multiple independent events. This is important because if all selfing species are derived from a single ancestral selfing species, it would not be surprising that they share common features without giving good insights about the questions addressed in the paper.

- Another important point is that the correlation between mating systems and competitive ability could be due to confounding factors. We would recommend to i) describe more precisely the differences and common features of the different studied species regarding life-history traits and strategies such as lifespan, dispersal, size of flowers and fruits, size of the vegetative parts, the stability and other characteristics of their natural environment; ii) discuss possible alternative explanations for their results: for instance, their results could be explained by a trade-off between perenniality and competitive ability. More globally, the authors show an interpretation bias that is widespread in the literature on mating systems evolution, that is that the mating system is the most important driver for life-history traits evolution.
- We think that the way the authors investigate the effect of the rosette surface on the number of flowers is not entirely satisfactory. In our opinion, a better way would be to incorporate the rosette surface $\times$ treatment interaction into the statistical model. We can expect that this interaction will be not significant and in this case the authors would be able to more easily conclude that rosette surface does not explain the response to competition as measured by the number of flowers. As a consequence, Figure 5 would no longer be needed. Moreover, it is unclear why the rosette surface $\times$ treatment interaction is not included into models comparing species (Table 3).
- An alternative explanation about the intermediate results obtained for *C. bursa-pastoris* could be that this tetraploid species is an hybrid of two species, one outcrosser and one selfer, also tested in this study. Hence, this is not necessarily due to a masking effect of deleterious mutations. Their results do not allow for the disentanglement of the various potential mechanisms underlying the observed pattern. The authors should once again be more balanced in their interpretations and conclusions.

2 Minor comments

- It is not clear whether the measurements were performed by a single or several experimentators. This should be indicated in order to eliminate the possibility of an experimentator-effect.
- We suggest to merge figures 1 & 2, and 3 & 4 to gain space (results are quite clear on those figures, smaller figures would be enough).
- Introduction: "Polyploidy is often [...] formation of the polyploid species.". For readers not used to the literature on polyploidization, it is worth briefly explaining here the underlying mechanisms.
- M&M: Define more clearly what are S1 and S2.
- M&M: The authors refer to the interaction using both the "$*$" and the "$\times$" signs. For clarity, the notations should be unified.
- Results: Use past or present tense, but not both.
- Results: "We first compared" instead of "First we compared".
- Results, beginning of last paragraph : in the sentence "We then tested whether... among geographic areas", "differences" instead of "difference".
- Discussion: "the outcrossing species *C. grandiflora*" (add "species")
- Discussion: "row values": What do the authors mean by "row"? or "raw"?
- Discussion: "without demographic consequences": prefer "with small demographic conequences"