

Review of “Variation in competitive ability with mating system, ploidy and range expansion in four *Capsella* species” by Yang et al.

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The paper by Yang et al. has been largely and adequately modified and corrected following our suggestions. We thus think that the paper is now ok for recommendation.

1 Minor comments

- The conclusions of the abstract must also be softened and more accurate: We suggest that the authors add the information that they detected competition effect on a reproductive traits but not on vegetative traits
- For the information of the authors, there are statistical procedures adapted to cases where there are many zeros in a dataset: zero-inflated models. See for ex. <https://stats.idre.ucla.edu/r/dae/zinb/>
- p11: something is missing after the “:” or something is wrong with the beginning of a paragraph
- p13 & p16: We are not sure to agree with the authors’ statement that since weak differences were obtained for rosette surface then it is a not a good proxy for fitness (but maybe we misunderstood what the authors meant, which calls for a clarification). The authors’ results may also mean that competition effectively does not affect fitness through vegetative traits. The authors’ statement can look like an *ad hoc* argument. An alternative conclusion might be (even if, of course, more reproductive and vegetative traits should be measured) that only reproductive traits are affected. A question would thus emerge: why would selfer vs. outcrosser species mostly differ by reproductive traits rather than vegetative traits? Are there any theoretical reason for that?