Report of: "Attracting pollinators vs escaping herbivores: eco-evolutionary dynamics of plants confronted with an ecological trade-off" submitted for Recommendation by *Peer Community In Ecology*

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Suggested major revisions

1. Trade-off, dissimilarity: Trade-offs and dissimilarity are at the core of your paper and model but it is difficult to clearly understand how you define and modelize them. I agree with one reviewer who suggested to move App. B to the main text. I agree with the other reviewer that the presentation and justification of the link between dissimilarity and the trade-off should be improved.

2. Modelling choices: Several strong assumptions are made and not explicitly highlighted: as the existence of an extrinsic intraspecific competition, none-evolution of the animals, and the absence of limit to the evolution of the plant trait. These assumptions should be made explicit, biologically justified or interpreted (e.g. the assumption that the animals do not evolve can be interpreted as much shorter time evolutionary scales for the plant then the animals because of differences in reproduction rate or lifespan), or at least discussed. In addition to the reviewer's suggestions I would add other implicit assumptions to be highlighted: the animals species are supposed to be either ultra-specialist of the plant, or obligate mutualist.

3. Cited literature: I agree with one of the reviewer who strongly suggested to better choose and more thoroughly present the empirical and experimental literature you cited in the discussion, and to better position in the introduction the present article in relation to the one you previously published (Yacine and Loeuille 2022). I suggest to improve the former point in the introduction (e.g. L97-98 and L103: a list of references rather than a single example is expected; also provide a more accurate and synthetic presentation of what has been shown in these papers that justify you own work). I also concur with the reviewer that you should position your model with the literature which consider mutualism as a product of a continuum of between-species interactions rather than as an input.

4. Paper organisation: I strongly suggest to modify one aspect of the paper with which I am uncomfortable. Your presentation of verbal 'predictions' (Fig. 1) that are later 'confirmed' by your results gives me the impression of something similar to 'Harking', something I would call 'Parking': 'Predicting after the results are known'. Of course I might be wrong and you might have effectively provided predictions before doing the model, and of course such a bias would be less important than in the case of an experiment. One drawback of your choice of presenting verbal 'predictions' is, when starting the "Results" section with "as predicted", to trivialise your results. In addition, one can argue that your verbal "predictions" are confirmed only because you introduced the necessary elements in your model to do so. I really do not think the organisation

of your paper needs to present Fig 1 as something you 'predicted'. I think Fig 1A and 1B. are good graphical summary of your work and results but should not be presented as a prediction. I think that it would be more important to replace Fig 1 and the associated text by Fig B1 and a text better explaining your definition and use of the trade-off.

5. Global clarity: I found that the text lacks clarity in many places:
   - L94: "stronger mutualism" is not clear. You mean obligate vs. facultative? Specialism vs. generalism? Is "stronger mutualism" synonymous to "better match"? If so this sentence is somewhat tautological.
   - L95-106: your justification why mutualistic interactions should give stabilising selection whereas predation should give runaway or disruptive selection should be improved. Some situations can justify the reverse: for instance how would it be possible to explain the dozens centimetres tongue length in some pollinators with stabilising selection rather than runaway selection? Or one can imagine that some chemical component produced by a plant to repel its predator should exactly match its vision, olfaction or taste and thus should evolve because of stabilising selection. Overall, I think you might want to explicitly present this as an assumption of your paper, supported by some empirical literature, rather than as an "obvious" and exclusive element.
   - L38, L108, L180, L206: "constrain": be more explicit, do you mean "shrinks the parameters space"?
   - L110, L137, L141, L143, L177, L179 and in the whole text: "balance between", "correlated interactions": this should be better defined, what do you mean by balance, similar predation and mutualistic interaction strengths? But at the individual or population level? (regarding the individual interaction rate or the global interaction rate?)
   - L112-L113: The question you are addressing seems to be only about species coexistence when the reader arrives at the end of the introduction. However, your model addresses more general and numerous other questions, as suggested by the beginning of your introduction when you presented some phylogenetic works. You might want to better present the whole set of questions addressed in your paper especially in regards with the literature you already cited.
   - L117-118: claim 3) seems contradictory to what was explained L95-106. Does stabilising or disruptive selection depend on the type of interactions? Or on the trade-offs? Is it an output or an input of your model?
   - L138: "favored", relatively to what? What does "coexistence favored" mean?

Minor comments

- L171: "heteroroph": seems too restrictive. Might your model also be interpreted for specialist mutualistic or predator species? For obligate mutualistic species?
- A direct and explicit link to the Appendix should be given within the text, and also for the files on the Dryad deposit server.
- Table 1: The variable length L only appears in this table if I am not wrong. It should be defined and related to the model. Some notations would need to be changed to avoid confusion: M is used for mass and pollinator density, t for time and trait. $t_h - t_m$ described as "strength of ecological trade-off" in the table but as dissimilarity everywhere else (e.g. L205), which is confusing and inconsistent.
- I am not sure to understand why you only talk about CSS and not ESS when the singular strategy are non-invasible but convergent.

- Fig 1.: What you mean by "temporality" is unclear.
- Fig. 5: "orgy" is not defined and not a classical term in Adaptive dynamics as far as I know.
- L458-463, Eq. 5: this part is very unclear. α is not clearly defined, is it an input parameter, an output of a statistical analysis? Regarding Eq. 5, some elements are not defined as c_{sim}. I was also unable to understand where does this equation come from, how it is used.
- App. III, L320-325: rep_1, rep_2, css, etc. are not defined.
- It is not easy to make the link between Fig.B3 and Figs. 1A, 2A, 5. I would suggest to combine both Fig 1A and Fig3B to better explain how you measured the proportion of the different regions, or even maybe replace Fig3B in 1D by a similar toy figure but in 2D.