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Request to Transfer Manuscript, JAE-2021-00171

Journal of Animal Ecology <onbehalf@manuscriptcentral.com>

Mon, Jun 28, 2021 at 10:44 PM

Reply-To: admin@journalofanimalecology.org

To: molly.hayes.martin@gmail.com

JAE-2021-00171 - Predicting the Resistance of Plant-Pollinator Assemblages to Fire

Dear Martin,

Your manuscript "Predicting the Resistance of Plant-Pollinator Assemblages to Fire" received a referral decision from Journal of Animal Ecology on 31-Mar-2021. This automated email is a reminder that the referral offer is still available.

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With best wishes

Samantha Ponton
Journal of Animal Ecology Editorial Office

Copy of the decision letter rendered for your manuscript:

Dear Molly Martin

Thank you for submitting your re-worked manuscript to the Journal of Animal Ecology.

Your manuscript has now been assessed by the Associate Editor as well as referees.

Based on their comments I am afraid that I am unable to accept your manuscript for publication in the Journal of Animal Ecology.

While your manuscript is improved and we appreciate your hard work, I'm afraid it still fell just short of the threshold. We realize this decision may be disappointing however we receive many more manuscripts than we can publish.

However, I hope that the reviewers' comments have been constructive.

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Thank you for allowing us to consider your manuscript. I hope you find the editor's and reviewers' comments helpful as you prepare your paper for submission to another journal.

Please do not be discouraged from submitting your work to us in the future.

Yours sincerely

Professor Darren Evans
Journal of Animal Ecology

ASSOCIATE EDITOR COMMENTS TO THE AUTHORS

Associate Editor

Associate Editor Comments for Authors:

The resubmitted manuscript was assessed by a reviewer of the previous version and a new expert in plant-pollinator networks. Whilst both could see merit, they also identified major conceptual and analytical points. Given the lack of support from reviewers I cannot recommend it for publication in the journal of animal ecology. However, with further work based on the reviewers comments, the manuscript could potentially be published elsewhere.

REFEREES' COMMENTS TO AUTHORS

(NB. If there is no comment from a Reviewer listed below, this probably means that they have uploaded a separate 'file for author' to the Central Site. You can see these comments in your Author's Centre by clicking 'manuscripts with decisions' and then using the 'files attached' link at the bottom of the decision letter).

Reviewer: 1

CONFIDENTIAL COMMENTS TO AUTHORS

General assessment:

This is an interesting study on the effects of fire on the structure of pollination networks. The rationale behind the investigation is surely relevant and network science undoubtedly is a good tool to operationalize concepts like complexity, structure, and stability. Nevertheless, although this study has a lot of potential, it also has some shortcomings, mainly conceptual, which should be addressed before it can become a relevant contribution to the field. The main problem is how the arguments are built, as the logics is flawed in some cases due to invalid form or false assumptions.

Specific comments:

L85: "Therefore, disturbance leads to decreased modularity." That's not necessarily true. Actually, this was an old paradigm in the field of ecological networks, which recent evidence has proved to be not so solid. The authors built this argument based on a series of assumptions, which are not well concatenated. In addition, some of these assumptions are false. Let's check the argument:

1. Disturbances increase network generality ;
2. Disturbances increase nestedness;
3. Disturbances increase connectance;
4. Disturbances decrease modularity;
5. Modularity increases as a result of decreased connectance and generality;
6. Lower levels of modularity may increase resistance to disturbance;

7. Nested networks are thought to be more stable;
8. This stability likely leads to higher resistance to disturbance;
9. More generalized networks are inherently more connected;
10. The more modular a network is, the less generalized and connected it is;

Conclusion: Therefore, disturbance leads to decreased modularity

First, the argument is too complex, involving too many assumptions (10 in total). It's incredibly difficult to build a valid deductive or abductive argument from such a long list. Therefore, it's strongly advised to break large arguments into smaller, better concatenated ones. The the conclusions of simpler arguments can be used as assumptions to build more complex arguments.

Second, what is generality for the authors? How does it differ from generalism or generalization? Generality is usually a philosophical concept, while the other two are indeed used in the realm of ecological networks.

Third, assumptions 6 and 7 are not necessarily true. Evidence points out that both nested and modular networks can be stable. Stability is not a simple matter of topology, and other criteria need to be taken into account.

Fourth, assumption 8 is weak. Stability is a broad concept that encompasses different facets, such as robustness, resistance, elasticity, and resilience. The authors need to specify what exactly they are talking about.

Fifth, assumption 9 is false. Generalization is a really broad concept, which can be operationalized in many different ways. A network with low connectance can be generalized, if you consider a different metric of generalization.

Sixth, the conclusion does not derive from the assumptions. There is clearly some information missing. Furthermore, the way the assumptions are concatenated is very confusing and does not lead to the conclusion, even if gaps are filled.

Seventh, nestedness, modularity, and other topological archetypes are not mutually excluding. They emerge at different network organization levels, as a result from the balance between different processes. Read:

- Lewinsohn, T.M., Inácio Prado, P., Jordano, P., Bascompte, J. & M. Olesen, J. (2006) Structure in plant-animal interaction assemblages. *Oikos*, 113, 174-184.
- Blüthgen, N. 2010. Why network analysis is often disconnected from community ecology: A critique and an ecologist's guide. *Basic Appl. Ecol.* 11: 185–195.
- Staniczenko, P. P. A., J. C. Kopp, and S. Allesina. 2013. The ghost of nestedness in ecological networks. *Nat. Commun.* 4: 1391.
- Flores, C.O., Valverde, S. & Weitz, J.S. (2013) Multi-scale structure and geographic drivers of cross-infection within marine bacteria and phages. *The ISME Journal*, 7, 520-532.
- Minoarivelo, H.O. & Hui, C. (2016) Trait-mediated interaction leads to structural emergence in mutualistic networks. *Evolutionary Ecology*, 30, 105-121.
- Payrató-Borràs, C., L. Hernández, and Y. Moreno. 2019. Breaking the Spell of Nestedness: The Entropic Origin of Nestedness in Mutualistic Systems. *Phys. Rev. X* 9: 031024.
- Pinheiro, R. B. P., G. M. F. Felix, C. F. Dormann, and M. A. R. Mello. 2019. A new model explaining the origin of different topologies in interaction networks. *Ecology* 100: e02796.

L98: "Based on past findings we hypothesized that fire would influence community structure". OK, but this is a very vague statement and you need to explain how precisely each prediction was derived from which assumptions.

L107: "pre-disturbance communities with these attributes are more resistant to change." This is a confusing and dangerous statement. Explain your argument more precisely.

L217: "To address changes in network characteristics, we compared the bipartite plant-pollinator networks and associated network metrics of generality, nestedness, connectance (both unweighted and weighted), and modularity estimated for each site in each year using the same BACI analysis." Why did the authors chose those particular metrics? What biological concepts do they operationalize? Why not use weighted versions of the metrics in all cases? What algorithms were selected to optimize calculation? There is too much information missing in this section, which makes the methods impossible to reproduce.

L539, Fig1: bipartite plots are a very poor representation of interaction networks. They help only visually inspect for nestedness, but they make life much harder, if you are interested in modularity, generalism, and other concepts. Try plotting the networks as energy-minimization graphs, as suggested by:

- Pocock, M. J. O., Evans, D. M., Fontaine, C., Harvey, M., Julliard, R., McLaughlin, Ó., ... Bohan, D. A. (2016). The Visualisation of Ecological Networks, and Their Use as a Tool for Engagement, Advocacy and Management. In G. Woodward & D. A. Bohan (Eds.), *Advances in Ecological Research* (1st ed., pp. 41–85). Cambridge: Academic Press. doi: 10.1016/bs.aecr.2015.10.006

- Marai, G. E., Pinaud, B., Bühler, K., Lex, A., & Morris, J. H. (2019). Ten simple rules to create biological network figures for communication. *PLOS Computational Biology*, 15(9), e1007244. doi: 10.1371/journal.pcbi.1007244

Reviewer: 2

CONFIDENTIAL COMMENTS TO AUTHORS

After reading the changes made in the manuscript, I found that the authors have taken care to amend their manuscript in accordance with suggestions. However, further effort is required as some parts still need to be revised. I am still concerned about the significant differences observed using one baseline year and non-significant differences observed with the other baseline year. I think that the authors should compare the two baseline years and provide a possible explanation for any observed differences. Additionally, the authors state that their data follow Gaussian distribution, but abundance and species richness are count data, and thus follow Poisson distribution. This could be solved if authors performed Generalized Linear Mixed Effect Models using as terms BA, CI, and their interaction. Furthermore, I would like to see a description of the sampling sites in the Material and Methods section e.g. the vegetation and also the characteristics of the fire that impacted the sites. I think this type of information is available to the authors as they refer that fire severity was similar across sites and photos were taken at each site.

Specific Comments (lines refer to the “track-changes” manuscript):

- Lines 39-40: “increase in bee abundance and species richness in the first post-fire years”. Furthermore, references are required here.

- Line 65: I don't understand how Figure 1 supports the argument that changes in plant and pollinator assemblages are reflected in the network's structure.

-Lines 74-76: Reference is required for this argument.

-Lines 116-117: The authors should mention the data and the study system used here, prior mentioning the role of fires in the Mediterranean ecosystems.

-Lines 122-123: The authors should remove this sentence and use a different intro sentence

-Line 138: Was the disturbance that impacted the sampling sites acute? In the discussion, the authors mention that “the fire may have moved through our sites quickly and unevenly, leaving enough of the bee and plant communities undisturbed to compensate for the effects of fire”.

-Lines 183-184 and Lines 191-192: Did the authors use this bee and plant abundance and richness in their analysis? In Lines 231-233 is mentioned that abundance and richness were extracted by the network.

-Lines 217-220: This should be moved at the beginning of the data analysis and the result of the analysis should be the first given in the Results section.

-Lines 251-254: The authors should mention the specific metrics estimated e.g. was nestedness quantified by (weighted or unweighted NODF)?

-Lines 256-261: By performing linear model analysis, the authors assume Gaussian distribution for count data.

-Lines 285-287: Generalists increased and specialists decreased in the 2003-2018 comparison, but not in the 2002-2018 comparison. How can this finding be explained? What about the 2002-2003 comparison?

- Discussion: I think that the patchy nature of the fire and the incomplete sampling of pollination network might explain the observed patterns. Assuming that fire did not affect either vegetation or pollinators in the specific sampling sites could explain the non-significant differences and the different results using different baseline years. Perhaps, the often reported year-to-year variation of pollination networks in Mediterranean ecosystems drives the observed patterns.

Table 1: The authors should format the table (this applies to all tables in the manuscript). In their present form, all tables are given as pictures, the titles of the columns are a different font and lightly distorted. Furthermore, all the captions should provide more information about the results that tables (or figures) present.

End of Comments

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