Subject: Journal of Applied Ecology JAPPL-2020-01405
From: Journal of Applied Ecology <onbehalfof@manuscriptcentral.com>
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To: lea.beaumelle@inrae.fr

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JAPPL-2020-01405
Research Article
Plant diversification across scales enhances biological pest control in agricultural landscapes
Beaumelle, Léa; Auriol, Arthur; Grasset, Marie; Pavy, Alice; Thiéry, Denis; Rusch, Adrien

Dear Léa Beaumelle,

Thank you for submitting your manuscript to Journal of Applied Ecology. The manuscript review process was handled by one of our Associate Editors, who has also supplied a recommendation. Copies of the reviewers' reports are included below together with the comments from the Associate Editor.

You will see that while the reviewers are positive about the general aims of your work, at the same time they find much in the work to comment on critically. With such comments, and pressure for space in our Journal so high, I have no alternative but to decline further consideration of your paper.

Reviewers are asked to provide constructive comments and also to assess whether, following revision, a manuscript might be considered for resubmission to the Journal. While the majority of papers can be improved through revision, this, in itself, will not necessarily bring them to the level of excellence required by the Journal. Although the reviewers believe your manuscript has considerable potential, there was no indication that it would attain the necessary standard following revision.

I realise that you will be disappointed with this decision but I am afraid we can only accept outstanding papers that provide novel insight and convey important general lessons for environmental management and policy. Unfortunately, we are in the position where we receive many more manuscripts than we are able to publish and currently decline approximately 80% of submissions, many of which ultimately are publishable in other journals.

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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.

Yours sincerely,

Professor Jos Barlow
Editor
Journal of Applied Ecology
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COMMENTS FROM ASSOCIATE EDITOR: Dr Bret Elderd

Associate Editor
Comments to the Author:
The reviewers and I recognize the time and effort put into conducting this study. In general, it was a well-written manuscript. However, the reviewers pointed out a number of limitations and questions regarding the temporal scale of the study and the contribution of the work to the general literature. Additionally, as Reviewer 2 pointed out, the main argument hinges on Fig. 2C. The reviewer asked what the significance of this effect means to the general farmer and posited that other data relevant to the farmer could have been taken such as pest density or crop yield to answer this question. Additionally, I wondered about the difference between the diversity treatments. It appears from the supplemental that the species from the 2 diversity treatment do not occur in the 20 diversity treatment. Also, all of the species in the 20 diversity treatment are flowering plants that provide nectar and pollen; whereas, the 2 diversity treatment contains grasses. Thus, is the difference between the plots due to diversity or due to differences in the plant groups in the two treatments? In addition to the above, the authors should spend considerable time thinking about ways of addressing the reviewers other comments. Undoubtably, this would help to improve the manuscript as a whole.

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REVIEWERS' COMMENTS
(If no comments are included please see note below)

Reviewer: 1

REVIEWER REPORT FOR THE AUTHORS
Summary
Authors conducted a 1-year study to evaluate the influence of cover crops on natural enemy diversity and abundance, as well as predation of sentinel Lobesia botrana eggs in vineyards. There were 9 vineyard sites situated along a continuum of landscape diversity (within 1 km), and each site had a pair of plots with and without the cover crop treatment. Insects were sampled in the cover crop using pitfall traps and in the vine

canopy using beat samples 3x/season. Sentinel egg cards were placed out in the vineyard 3x/season as well.

Natural enemies were primarily spiders, which is common in vineyards. Data indicate increased natural enemy abundance, but not diversity, in the plots with cover crops. Egg predation was higher in cover crop plots only in low diversity landscapes, indicating some tradeoff between local x landscape diversity. Authors conclude that this is an example of how local diversity impacts can be mediated/interact with changes in landscape diversity – the final conclusion being that landscape context should be considered when implementing local habitat management strategies.

Local x landscape interactions are important and have been the focus of numerous studies over the past 20 years, especially within the context of AES programs in the EU. This is not a new idea and has been demonstrated multiple times by other investigators, so the novelty of this current manuscript is lost on me, especially since it is just a 1-year study with a small sample size of just 9 sites. Furthermore, authors make no effort to specifically relate the natural enemies encountered to the key local and landscape variables. What about the cover crops or landscape would be driving these (mostly spiders) natural enemies? What do we know about their movement between natural habitats and vineyards?

It's not entirely clear which natural enemies were being measured in this study. Suggest authors provide a table with means/variance for each natural enemy on each sample date.

Use of Lobesia botrana as sentinel prey is fine, but suggest authors provide more background on pest management in this region/system – is L. botrana a key pest? Is it the top pest? I'm trying to find the relevance here to farmers.

While I can't reject this for being scientifically flawed, I would be hesitant to publish this due to the lack of novelty, 1-year effort, and small number of sites. I applaud the authors for their work and encourage them to collect an additional year of data and include more exploration/study on the mechanistic aspects of this system.

Specific Comments
L132: What grape varieties? Plant dates? Irrigation setup? Please provide more details about the vineyards. Were pesticides applied at any of the sites? If so, what type and how frequently? Do you think this had any effect on insect response?

Edits
L53: suggest rephrase – "Multiple studies have demonstrated a positive relationship between…"
L70: suggest rephrase – "…crop diversification reduced pest abundance and crop damage by 23% compared to monocultures…"
L90: delete comma before citations
L119: suggest rephrase – "Use of cover crops in vineyard row-middles is however…"
L276: use Chi-square symbol rather than spelling this out, here and throughout the manuscript
L323: suggest rephrase – "…increasing plant diversity did not influence taxonomic…"
L393: California
Figure 1A: Add asterisk to indicate significant difference between Low and High treatments.

Reviewer: 2

REVIEWER REPORT FOR THE AUTHORS
The authors present a 1-year study on the effect of establishing flower strips in vine orchards on the abundance, diversity and predation potential of natural enemies. These relationship have been studied in 9 landscape settings that cover a range in semi-natural habitat. I consider this work relevant, interesting and timely, particularly in the light of the current widespread declines of insects. The paper is generally well written, and the statistical is sound. While I really appreciate the effort that the authors undertook

in establishing the treatments at the farms, I also feel that the study is somewhat limited in scope in the sense that the authors have not reported data on pest densities, crop injury, and vine yield or quality. This makes it difficult to evaluate the usefulness of flower strips for farmers, as they will make most likely inform their crop protection on the density of pests and diseases, rather than on natural enemy densities (if they indeed don't apply calendar sprays). I will detail my comments below.
I find the title a bit misleading because the treatment strictly not does reflect a gradient in diversity (the 2 grass species of the control treatment are not present in the diverse treatment), but rather a gradient in floral resources as most if not all plant species in the diverse treatment offer copious amounts of nectar and pollen for insects. It would be good to reflect this in the title.
Study design: Why was a scale of 1 km chosen? Often landscape scale studies include multiple spatial scales to explore which spatial scale can explain most variation in the response variables. And why was the proportion of semi-natural habitat chosen as a proxy for landscape complexity? Could there be any other relevant land use types that may provide resources for natural enemies (e.g. mass flowering crops)? I presume that the vine orchards were conventionally managed. It would be interesting to also report information on variables that may influence arthropod communities, such as tillage, insecticide and fungicide applications. How often and when were orchards treated with pesticides? How were grass strips (control) and flower strips managed? Were grass strips mown? On line 360 some information is provided but this is too general to be really informative. What were the selection criteria for flowering plant species, e.g. in terms of annuals and perennials, and associated insect groups? Apparently, soil fertility was also a criterion (line 360) but this is not explained. Additional information in the appendix would be helpful for readers.
Line 158: I had difficulty to relate to the sample size N=324. I presume this is derived as: 2 transects x 3 distances x 2 treatments x 9 landscapes x 3 sampling rounds. It would be helpful for readers to provide these details. Also, there seems to be a mismatch between the 8 egg cards reported on line 171 and Fig S1 which depicts 9 release locations per field. Did the cards contain eggs (as stated in the ms) or larvae (as stated in the caption of Fig S1)? Perhaps I am missing something, but please clarify and standardize. Where there any missing observations? Based on line 244 (n=999) I suspect that not all cards could be recovered but it is good to mention this explicitly in the text. It would have been informative if also data on parasitism would have been collected as parasitoids may have benefitted from floral resources in flower strips, but I presume that has not be done.
Results: a large proportion of the specimens (e.g. spiders and harvestmen) seem to be not (or only to a very limited extend) dependent on pollen and nectar resources. It would be informative to give more detailed information on the coleoptera and neuroptera guilds, because I expect that here likely to be groups that will feed on nectar and pollen provided, such as soldier beetles, ladybeetles and lacewings. Fig 2 suggests a negative correlation between predator abundance and predation rate (discussed on lines 377 and 402). This really triggers the question which species have contributed to removing eggs from the cards. Are there any relevant observations or any other clues? The statement on line 377 is in this sense confusing "diverse cover crops had positive effects on natural enemy abundance and predation rates below 50% of semi-natural habitats" because even though the diverse treatment supports a higher abundance of predators than the control treatment, the trend in the relationship between abundance and landscape complexity in this part of the graph is negative.
Line 357: Indeed, one of the limitations of this study that it is a 1-year study. It would be interesting if the experiment could be continued (including assessments on pests and vine production) in a second year when the vegetation structure may be better developed (at least for the perennial plant species).
Conclusions: I was not convinced whether the conclusions were fully supported by the data since eyeballing Fig. 2c the difference in effect size between the 2 treatments seem limited. Even if this difference is statistically significant, will it also be large enough to be meaningful for farmers? Possibly including a discussion on economic damage threshold might be useful, but since no pest densities have been reported the possibilities to evaluate the outcomes of this study to make it really tangible for practitioners seems to be a bit limited.
Minor comments
Line 326: please explain effects on what

Line 410: what “small predator species” are you referring to?

Reviewer: 3

REVIEWER REPORT FOR THE AUTHORS
I have been asked to assess whether this study could be included in the NbS Special Issue, and I have not assessed the manuscript for science quality.

I think the results are interesting and this could be a good contribution to the NbS Special Issue, however I would like to see in the discussion clearer guidelines or recommendation on how stakeholders could modify the land use to improve natural pest predation. This could be in form of a paragraph or so under a clear sub-heading denoting the section.

If a reviewer's comments seem to be missing or the report refers to an attached file, which is missing please e-mail the editorial office (admin@journalofappliedecology.org).

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Reply to:
Tom Pinfield
Journal of Applied Ecology Editorial Office
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