Peer Community In Ecology

Unravelling plant diversity in agricultural field margins in France: plant species better adapted to climate change need *other* agricultures to persist

Julia Astegiano *based on peer reviews by* ***Ignasi Bartomeus***, ***Clélia Sirami*** *and* ***Diego Gurvich***

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Agricultural field margin plants, often referred to as "spontaneous" species, are key for the stabilization of several social-ecological processes related to crop production such as pollination or pest control (Tamburini et al. 2020). Because of its beneficial function, increasing the diversity of field margin flora becomes as important as crop diversity in process-based agricultures such as agroecology. Contrary, supply-dependent intensive agricultures produce monocultures and homogenized environments that might benefit their productivity, which generally includes the control or elimination of the field margin flora (Emmerson et al. 2016, Aligner 2018). Considering that different agricultural practices are produced by (and produce) different territories (Moore 2020) and that they are also been shaped by current climate change, we urgently need to understand how agricultural intensification constrains the potential of territories to develop agriculture more resilient to such change (Altieri et al., 2015). Thus, studies unraveling how agricultural practices' effects on agricultural field margin flora interact with those of climate change is of main importance, as plant strategies better adapted to such social-ecological processes may differ. In this vein, the study of Poinas et al. (2024) can be considered a key contribution. It exemplifies how agricultural intensification practiced in the context of climate change can constrain the potential of agricultural field margin flora to cope with climatic variations. The authors found that the incidence of plant strategies better adapted to climate change (conservative/stress-tolerant and Mediterranean species) increased with higher temperatures and lower soil moisture, and with lower intensity

of margin management. In contrast, the incidence of ruderal species decreased with climate change. Thus, increasing or even maintaining current levels of agricultural intensification may affect the potential of French agriculture to move to sustainable process-based agricultures because of the reduction of plant diversity, particularly of vegetation better adapted to climate change. By using an impressive dataset spanning 9 years and 555 agricultural margins in continental France, Poinas et al. (2024) investigated temporal changes in climatic variables (temperature and soil moisture), agricultural practices (herbicide and fertilizers quantity, the frequency of margin mowing or grinding), plant taxonomical and functional diversity, plant strategies (Grime 1977, 1988) and relationships between these temporal changes. Temporal changes in plant strategies were associated with those observed in climatic variables and agricultural practices. Even such associations seem to be mediated by spatial changes, as described in the supplementary material and in their most recent article (Poinas et al. 2023), changes in climatic variables registered in a decade shaped plant strategies and therefore the diversity and functional potential of agricultural field margins. These results are clearly synthesized in Figures 6 and 7 of the present contribution. As shown by Poinas et al. (2024), in the context of climate change, decreasing agricultural intensification will produce more diverse agricultural field margins by promoting the persistence of plant species better adapted to higher temperatures and lower soil moisture. Thus, adopting other agricultural practices (e.g., agroforestry, agroecology) will produce territories with a higher potential to move to sustainable processes-based agricultures that may better cope with climate change by harboring higher biocultural diversity (Altieri et al. 2015).

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Reviews

Evaluation round #3

DOI or URL of the preprint: https://doi.org/10.1101/2023.03.03.530956
Version of the preprint: 3

Authors' reply, 09 February 2024

Download author's reply

Decision by Julia Astegiano, posted 05 February 2024, validated 05 February 2024

minor changes

Dear Isis Poinas (and coauthors), Many thanks for your work on this new version of your manuscript. You provided detailed answers to our comments and suggestions, modified the text in a way that made it more accessible and added key figures (Fig. 2 and 6) that clearly improved its understanding. I am ready to recommend your article but first I will need to ask you for a few minor changes (listed below). Thank you in advance. Best Julia Minor changes
Line 35: I suggest you define field margin management here (i.e., mostly mowing) to clarify your results, and adding "also" between intensity and resulted, as this result is in line with your previous one (both changes favor conservative strategies).
Line 36: "there was no impact" on what? It seems to me that your sentence is incomplete. Please, at least add a generality as "vegetation changes" or "on all vegetation variables we measured".
Line 37: "practices within the field" (within cultivated areas?)
Table 1: write "Number of margin field management events" instead of "number of management events"
Fig. 5. I suggest you add "(stress-tolerance)" to axis 1 and "(ruderality)" to axis 2
Line 512: please, add the information on supplementary material location

Evaluation round #2

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Version of the preprint: 2

Authors' reply, 21 January 2024

Download author's reply

Decision by Julia Astegiano, posted 22 November 2023, validated 22 November 2023

the preprint needs revision

Dear Isis Poinas

Many thanks for sending the revised version of your manuscript to PCI Ecology. Two reviewers (Dr. Ignasi Bartomeus and Dr. Claelia Sirami) and I read carefully this new version. We all agree that you made a great work to incorporate our comments or provided arguments about the suggestions you disagreed. Dr. Sirami still has some minor comments on your work, but both reviewers are very positive about your contribution. I still have a major point to discuss with you and some other suggestions to improve your manuscript.

Your work is impressive because of the dataset you analyzed to answer key questions related with the influence of climate change and agricultural practices on the taxonomic and functional diversity of spontaneous vegetation in agricultural systems. Your main question is related to temporal changes on such predictors and their influence on spontaneous vegetation. As you recognize the spatial correlation of your samples, you also present a complementary analysis that show how considering such spatiality may influence your key question (that is still on climate variables and agricultural practices and their temporal variation). I have no doubts about the timing and relevance of such work and I want to recommend it, but there is still a key issue of your study design (or the way you chose to communicate it) that I think that need to be addressed.

Now you provided answers to different questions that we made on your first version and that the manuscript has been improved, during my second (and deep) read I realized that what you presented as a "spatial model" (Figure 4) is more related to controlling spatial autocorrelation among samples -given your questions on climate change and agricultural practices- rather than a specific question related to spatiality itself. For the different comments that the reviewers and I made related to your analyses, you provided the reasons underpinning each analysis and I understand that there is no way to consider all spatiotemporal dependencies on your data in just one analysis (the ideal analysis to answer your original question). I am ok with that. However, now I feel that presenting your "average" analysis as a spatial one is not correct. What makes me think about this point is that your introduction clearly describes temporal changes in both climate and agricultural practices, but "the spatial problem/question" is not really introduced; readers may have no idea about how spatiality may affect variability in climate or agricultural practices. In fact, you do not have hypotheses related to spatiality and your Table 1 describes expected temporal patterns and hypothesis. Moreover, considering that you have a clear pattern related to temporal changes, I think that collapsing information to an average just to understand how controlling spatial autocorrelation changes the statistical effects of climate variables and agricultural practices and showing that as a spatial analysis as important as the temporal one basically adds confusion to your work. For me, your main question is related to temporal trends in climate variables and agricultural practices patterns and on their effects on spontaneous vegetation; then, you realized that you need to control for spatial autocorrelation to understand such changes in a more robust way. This is very different from having a spatial analysis itself, with an introduction and associated hypotheses. Based on this reasoning, I will suggest you simplify your results, focus on the temporal analysis (for which you have a clear background) and mention your results considering spatiality as a secondary, complementary analysis, and not as a one as central as the temporal analysis. This implies changing the way you describe some results and some of your figures, and clearly stating that your focus is on temporal changes that in turn may be affected by spatiality. In this vein, it is important to mention that you do not have temporal patterns in agricultural practices; thus, you are analyzing the effect of the intensity of agricultural practices by considering spatio-temporal samples. In the case of climate variables, their variation follows a temporal pattern, i.e. the value of climate variables can be associated to temporal changes. In the following, I will exemplify across your manuscript how considering spatiality as important as temporality may confound readers and suggest changes that I feel may improve the manuscript.

Another major comment is that your abstract still needs more work to be clear. As stated by Dr. Sirami, you

use different words and expressions to talk about each variable and that may confound readers. Moreover, I think that there are some results that I highlighted in my first review that need to be clarified or written in a different way as they remain unclear.

Hope you find that our comments and suggestions will improve your manuscript.

Finally, apologies for the time we took to review this new version.

Best wishes

Julia Specific comments

ABSTRACT

Lines 23-25. You state "Here we used a standardized yearly 23 monitoring effort of agricultural field margin flora at the national scale to assess the spatial-temporal response of diversity and functional traits to climatic and agricultural variations". As I explained before, you do not assess a given hypothesis related with spatiality, you control for spatial patterns to assess how mean climate and agricultural practices values modulate species diversity. Following my suggestion, you should focus your abstract on temporal changes and state your spatial analysis in a separated sentence. That will clarify your abstract. Moreover, I think that you need to clarify what do you mean by climatic and agricultural variations here and use the same words or expressions to refer to these factors in order to clarify your abstract.

Lines 25-29. You state "We examined temporal trends in climate (temperature, soil moisture), intensity of agricultural practices (herbicides, fertilization, margin management), plant species richness, and community-weighted means and variances of traits expected to vary both with climate and practices (e.g., seed mass, specific leaf area), across 555 sites in France between 2013 and 2021." This is, again, related to a temporal analysis. You do not mention details on your "spatial analysis" (which should be briefly explained) nor a hypothesis related to spatiality.

Line 31: Functional changes… temporal? spatial? mainly explained by temporal variation in climate variables instead of climate change? Why you do not introduce results on plant species richness, and community-weighted means and variances of traits, before going directly to an interpretation of results according to Grime's strategies? For me, introducing some variables in methods and then mentioning results related to something that you have not mentioned before may totally confound readers. The same for the next sentence, you go directly to ruderal species, without any mention on species richness/diversity.

Lines 33-34. "The impact of agricultural practices was more limited". What do you mean with "was more limited"? In which sense? I also suggest you write "The impact of temporal changes in the intensity of agricultural practices", as this is the focus of your work. Maybe you can add just one sentence summarizing what is the effect of considering spatiality in the effect of mean values of climate and agricultural practices, also stating clearly that you analyse mean values of response variables.

Line 34: Please, add "the intensity" of field margin management and fertilization.

Lines 33-35. "The impact of agricultural practices was more limited and mainly exerted through field margin management and fertilization that shifted vegetation towards species with a ruderal syndrome." This sentence is confusing as before you stated "whereas the intensity of agricultural practices did not show clear temporal trends over the past decade". Maybe it is not clear because of the use of different words/expressions to talk about the same variables.

Lines 35-36. "Responses to climate change differed according to crop type (vineyards versus annual crops), region (Mediterranean versus continental)". These analyses were not introduced before and thus it may confound readers. I suggest you briefly introduce them before or here, and then reveal your results.

Lines 37-38. "Our findings suggest that species adapted to climate change (including Mediterranean and conservative species) have [add "temporally"] increased in proportion."

Lines 40-42. We put these results into the conceptual framework of Grime's CSR triangle and revealed a [add temporal] decline of competitive and ruderal species in favor of stress-tolerant species better adapted to

climate change.
Lines 43-44. Why more diverse communities if CC is favoring a given strategy?
General comment: It is not clear for me which results are associated to the analyses that controls for spatiality. As the abstract should work independently from the text, I strongly suggest you work hard to make it more transparent and associated with your full set of results, even if you cannot mention all results.
INTRODUCTION
Lines 62-65. Spatiality is introduced in the first paragraph in the last sentence but it isn't clear what do you mean wit "spatiality". Maybe regions where pesticide use has decreased? If you really think you have a spatial analysis, I strongly suggest you introduce spatiality in this section and review spatial changes as you did for temporal changes.
Line 68. "these changes"-> are you referring to temporal change in biodiversity? That will be "this change" (draw by climate or agricultural practices changes). Spatial changes are not contextualized in your first paragraph. Maybe you can explicit what changes you are you referring here.
Lines 84-85. You state "To understand the complex interactions between climate change and agricultural practices, it is thus essential to examine the temporal dimension of functional inter-specific trait variations". However, agricultural practices do not show a temporal trend. Then, what you are evaluating is the effect of the intensity in agricultural practices (either, spatially or temporally sampled). Is that correct? This will be different from having a temporal trend in agricultural intensification. This point is definitely unclear for me. I think that trying to simplify your work and messages you confounded your factors. Climate variables have temporal samples that show a temporal trend, so your climatic gradient is temporal. But your gradient of agricultural practices it is not. Then, should temporality be considered in the same way as for climate? Are temporal changes in agricultural practices or the intensity of agricultural practices across different years what is being evaluated? This is a major point of your manuscript and you need to make a strong effort to make it clear. Maybe that is why you do not use the word temporal in your Table 1?
Lines 93-94. "These temporal variations in functional traits reveal patterns that cannot be assessed solely with a space-for-time approach". This sentence is really confusing. What do you mean by a space-for-time approach, as this is not clear in your work?
Lines 95-97. "In this study, we aimed at deciphering how spatio-temporal variations of climate (temperature, soil moisture) and agricultural practices (frequency of herbicide use, margin management and nitrogen dose in fertilizers)." This sentence is confusing, because you do not really have a hypothesis for spatial changes, you control spatiality in order to evaluate how variations in climate and agricultural practices modulate vegetation changes. Moreover, as I stated before, agricultural practices in fact do not show a temporal trend, so I suppose that temporality here is just a way to recognize that you have different values of agricultural practices because you studied different years. Hope you can see the difference between the two forms of analysis and argument.
Lines 101-103. "Our study stands as one of the first to investigate the temporal trends in agricultural practices and climate, and explore the spatial and temporal drivers of species richness and functional traits at such extensive scales." Here again, I see very problematic the way you present your study.
Lines 104-106. "while agricultural practices would have a greater spatial than temporal influence on plant communities, as we did not expect clear temporal trends in these practices." There is no such spatial effect, there is an effect that emerges because you controlled by spatial autocorrelation. If you consider that there is such spatial effect, which factor will explain it? Your question will be "how species richness, functional diversity and strategies change with the intensity of agricultural practices (evaluated across different regions and years)? Hope you can see the difference between the two alternatives. METHODS
Table 1.
(1) I suggest you edit your table 1 according to my previous comments. As it is, the headers may confound readers. For example, the word "factor" in the first column, even if you put response variables in different colors. Maybe you can add something like "Factors/response variables". On the other hand, the fact that readers will find temporal hypotheses -not mentioned as temporal yet- for climate and agricultural practices in

the column “hypothesis of response to the agricultural/climate gradient “, i.e. the gradient responding to the gradient itself. This sounds rare for me.
(2) Please, make clear which hypothesis are temporal here. As I mentioned before, from your introduction I understand that you have a hypothesis for climate, which is temporal, and two different hypotheses for agricultural practices (temporal and about their intensity). Is that correct?
(3) Your legend will also need to be modified accordingly. Lines 179-181. Can you give a more detailed explanation of these data-entry? How did you collapse information for site? (across all observations?).
Lines 207-220. As I explained before, you do not have a spatial analyses per se. That’s why in fact you do not have a spatial framework in your introduction or even a hypothesis linked to the spatial variation of your factors or variables (something that you have for the temporal analyses). You have an analysis in which you control for spatial correlation in order to make your samples more or less independent accordingly to their spatial distribution in the territory you are analyzing. It is important to note that in fact, you have a design to evaluate how mean species richness and mean trait variation of a given site (averaged across years) change with climate variables and agricultural practices (also collapsed across years by using averaged values). I strongly recommend you keeping the temporal analysis, as the spatial analyses seems secondary to me (collapsing values across years while you have an explicit analysis of temporal changes does not makes sense for me; but I understand that is the best analysis that you can introduce to better approach your results). I think that will simplify your article and make your contribution more valuable, since you have temporal trends in climate variables, which also seem to have the most important effect.
Lines 222-223. You do not have a spatial analysis, see my previous comment.
Fig.2. I strongly recommend you rethink this figure and your proposal (see my previous comment and general proposition). Just keep the temporal analysis as the main one and show how diversity patterns related to climate variables and agricultural practices change when space is considered (recognizing the limitations of this spatial analyses, which I think merits a paragraph in the discussion section). RESULTS
Line 250. Why you do not present a spatial analysis of vegetation as the one you present for the temporal analyses? I think that this is in line with my argument that you want to control for spatial correlation but you do not have the same interest/background/hypothesis as with temporal changes in climate i.e., higher temperatures and lower soil moisture. In case of having those hypotheses, I would like to see them separated in your Table 1 too.
Lines 288-313. Here you confound a spatial analysis with an analysis based on the subgroups of species that you mentioned in your introduction. It does not seem to me that this is a spatial analysis per se, you are focusing on two regions that you already identified as different regions (in your introduction) and with the PCA you look for sites described as continental or Mediterranean being more similar. In fact, in your methods section you do not mention these analyses as spatial analyses (which will include lines 299-313). In fact, at lines 197-199 you said: We performed a normed PCA on the CWM of traits (by site for spatial analyses, by observation for temporal analyses) to classify each community based on its average trait combination or ecological strategy, which is reflected by its position on the first two axes. I suggest that you revise carefully all parts of your manuscript referring to a “spatial analysis”.
Line 299. In line with my previous comments, models evaluating factors and considering the spatial autocorrelation among samples are different from models that explicitly have an hypothesis about space. Again, for me you are controlling spatial autocorrelation to evaluate the effect of the factors you are interested in. PCA analysis and spatial analyses are different and provide different (and complementary) information, as far as I understand. PCAs allowed you to talk about Grime strategies, which is different from what the analyses mentioned as “spatial analyses” in the methods section show. I suggest you to show these results in a separated subsection in the Results section.
Lines 327-331. Please, provide this results in a separated section, or explain clearly in the methods the relationship between your PCA analyses and the temporal analysis.
Line 333. In what part of the methods you mention this analysis with the ”stress-tolerance” axis? DISCUSSION

GENERAL: I agree with Dr. Sirami's comment about starting with a general summary of your results.
Fig. 6. Maybe adding your factors here will make the figure clearer; for instance, Climate change (increasing T, decreasing soil humidity), Resources (fertilization practices?), Disturbance (Mowing, herbicide use?). That will link more directly your figure to your specific results.
Line 382. It does not seem to me that you have an analysis to say that climate drives the spatial variation of vegetation; instead, once you have controlled by spatial autocorrelation, you found a given relationship between climatic factors and species composition. In fact, you do not present a section related to the spatial variation of the factors or vegetation as you do with the temporal part.
Line 383. Is a temporal shift, isn´t?
Lines 386-389. This is a temporal increase, not a spatial one, as you have collapsed information on many years. As explained before, your spatial analysis is controlling for spatial autocorrelation in order to make your mean values of everything more comparable.

Reviewed by Ignasi Bartomeus, 28 September 2023

Thank you for your detailed response. I read this new version, and the message is much clearer, I especially like the use of effect sizes to describe the magnitude of changes. The dataset is impressive but complex, and while some observed patterns are still challenging to interpret, the main messages are clearly stated and I have no further comments on the text.

Best,

Ignasi Bartomeus.

Reviewed by Clélia Sirami, 31 October 2023

The authors have thoroughly addressed all comments, conducting additional analyses when needed, and providing important clarification on methods. I only have minor comments to improve readability because the analyses remain complex and sometimes difficult to follow. Overall, I suggest that the authors standardize terms throughout the whole ms, add a synthesis of the results at the beginning of the discussion section, and use more specific wording to avoid any confusion, in particular in the discussion. Below, I am providing detailed comments to illustrate these general suggestions.

L34 Replace "that with ", which"

L51 Replace "are observable" with "have been observed"

L52 Add "temporal" before "trend"

L56 Replace "by taxonomic diversity" with "using taxonomic approaches"

L58 Replace "dimension" with "approaches"

L85 Replace "the temporal dimension of functional inter-specific trait variations" with "temporal changes in species trait distribution"

L90 Since the authors use many traits and associated adjectives, it would be useful to specify the link between traits and adjectives the first time they are being used, e.g. more acquisitive species means species with a higher SLA.

L103 Use the same wording as above "species richness, trait composition and ecological strategies"

L111 When possible, use the same wording throughout the ms, e.g. here "soil moisture" instead of "water resource", to help readers.

L170 Add "and ecological requirements"

L215 Try to use the same wording, e.g. here you call CWV "divergence"

L262 Use the words "significant", "strong", "non-significant" or "weak" rather than "clear" and "uncertain". The word "uncertain" may suggest that the statistical power is not sufficient or that confidence intervals are too large to conclude. It would be great if the authors can clarify this point.

L265 Since this study only focuses on French data, delete “In France,”

L280 Fig 3 would be easier to read if legends for y-axes are indicated along the axis rather than above

L296 As suggested above, it would be useful to specify at least once the link between adjectives and traits (here conservative and acquisitive)

L332 Specify “temporal models” on what?

L333 “Effect sizes” rather than “regressions”?

L339 and 342 Replace “community changes” with “community trait composition”

L349 Explicit the meaning of CWM in the legend.

L363 The way this sentence is constructed suggests that you ran a model on a data subset for “frequently managed margins of the MZ”.

L366 What does “seasonal effects” means? Date of observation?

L371-380 This whole section explains Grime’s theory and belongs to the method section rather than the beginning of the discussion. I believe a summary of the results would be more useful at the beginning of the discussion section.

L390 What is “functional variety”? Does it refer to “trait divergence”?

L394-396 It would be good to start the discussion with this kind of sentence on the main results.

L403 Reword “dry and heat stress”

L417 The authors suggest that “plant communities are also responding to past landscape diversification”. However, landscape simplification has been ongoing for more than 70 years in most agricultural landscapes in France (except in the Mediterranean region). Do they refer to a heritage effect from past landscape diversity rather than diversification?

L419 The authors mentions “arable lands decrease” whereas it has increased in most agricultural landscapes in France.

L423 The authors did not study “functional diversity”

L427 Farmers will certainly adapt to climate change so this sentence sounds a bit naïve.

L429 Replace “diversity and species assembly” with “field margin plant communities”

L435 Replace “can only have” with “are likely to have only”

L437 I do not understand what “functionally similar subset of communities without herbicide application” means

L438 Add “some” in front of “traits”

L444 Replace “this” with “fertilization”

L446-447 Delete “high nitrogen levels can favour” and “, which”. Replace “, explaining that” with “, which explains why”

L450 Specify which vegetation

L463 Change to “and implications for communities response to ongoing global changes”

L467 Are trade-offs evident due to the scale of the size of the species pool considered?

L468 Change to “occur”

L470 Specify “field-margin plant communities”

L479 Replace “space” with “spatial analyses” and “identify” with “imagine”

L483 Replace “specialized to their habitat” with “habitat specialists”

L485 Which “restricted areas”?

L491 Add “to understand the temporal dynamics of field margin plant communities” or something similar at the end of this sentence.

L497 The authors mention “at the national level” but they did not study field margin plant communities at that level.

L498 Change to “Our study suggests that species selected by…”

L500 Specify which agricultural practices

L510 Replace “some perspectives” with “other important research questions”

Evaluation round #1

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Version of the preprint: 1

Authors' reply, 01 August 2023

Dear editors and reviewers,

we have carefully considered the feedback provided by the reviewers and have made necessary revisions to address their concerns. We believe that the revised manuscript now meets the high standards set by the journal.

In the attached document, you will find our point-by-point response to the reviewers' comments, along with the PDF file that Julia has directly commented on.

Thank you for your time and valuable feedback. We look forward to hearing from you soon regarding the status of our submission.

Best regards,

Isis Poinas

Download author's reply

Download tracked changes file

Decision by Julia Astegiano, posted 21 April 2023, validated 24 April 2023

your contribution needs major changes

Dear Isis Poinas and co-authors, Many thanks for submitting your manuscript "Functional trade-offs: exploring the effects of climate change and agricultural practices as drivers of field margin plant communities". I found your work timely and relevant for current discussions on the role of agricultural practices in the face of the food and biodiversity crisis. After receiving the review of three colleagues (Dr. Clelia Sirami, Dr. Ignasi Bartomeus and Dr. Diego Gurvich) and my own review, I decided to ask you for a major revision of your manuscript. You will see that all reviewers found your article very interesting and highlight the impressive dataset that you analysed as well as the importance of sharing your results to discuss the effects of climate change and intensive agriculture on biodiversity of field margins. I agree with such positive conclusion on your work. However, Dr. Sirami, Dr. Bartomeus and I made several major comments, observations and suggestions that I expect you will consider or provide specific arguments in case you decide to not include them. I hope you will find our contributions valuable and that they will improve your manuscript. In general, we all had concerns on methodological, design and analytical decisions that lacked a clear explanation or arguments. These decisions may affect your results so need to be clarified (e.g. decisions on data collection, selection of common species, information on landscape heterogeneity, crop type, history of production of farms). There is also general agreement on a lack of centrality of Grime's strategies background in your introduction, which should be changed as it became the core of your discussion. Figures 4 and 5, which ar central to your contribution, were not clear ofr Dr. Bartomeous and I, and we made suggestions that I hope will improve them. In that vein, I suggest you try to simplify your results keeping in mind your interest in sharing your work with stakeholders. Dr. Sirami and I also made several suggestions to your discussion section, in order to include discussions on other results beyond Grime's strategies (e.g., trait variance at community scale), the interaction among climate and agricultural practices, implications/perspectives on landscapes management and perspectives/feedback on the public program that guides data collection. You will find more specific comments on the reviews of Dr. Sirami, Dr. Bartomeus and Dr. Gurvich. I Attach a pdf with my review. I will wait for a new version of your manuscript and your specific responses to reviewers to make a decision about recommending your article for PCI Ecology. Thank you very much again for sending your contribution to PCI Ecology. Best wishes Julia **Download recommender's annotations**

Reviewed by Ignasi Bartomeus, 30 March 2023

The manuscript presents an impressive dataset to ask how ruderal plants have changed in the last decade across space in France. The challenge is to explain such complex patterns effectively. While the manuscript is overall sound, my main concern is that some details or decisions are unclear or not justified. Most of my comments are in this direction.

First, given the several analysis conducted, I would recommend focusing the discussion on the effect sizes of the different models and avoiding using p-values. The authors are aware of that and already report p-values at alpha 0.01, and highlight the R2 of the models, but I miss a clear interpretation of the models in terms of the effect observed. How many e.g. species on average were lost over years? If the authors can translate the results into clear units, the paper will be much easier to follow. A clear example is Fig 4, which is a nice summary of ”important” variables, but it tells nothing about how big were each of the effects.

Second, while I like the distinction between temporal and spatial patterns, I am not sure if using a single modeling technique would help explain better the story. First, temporal correlations are shown, but those treat all site-year combinations as independent. I would expect ”site” to be modeled e.g. through a random structure as done for other analyses. Second, spatial trends are done by pooling across years, but I am not sure if this pooling can mask some interesting effects. Finally, a more detailed temporal analysis is done, this time considering ”site” as random (this is in part redundant with the first analysis?). Moreover, all these models are run for different subsets of data. Would it be possible to tackle all questions within a single analytical framework? For example, by modeling the interaction between time and environment e.g. CWM ~ Time * Region would avoid doing a test for each region. I am not suggesting that all analyses need to be re-done, but if you can simplify the number of tests done, without losing insights the paper would gain clarity and robustness.

Finally, while I appreciate the context of Grime’s triangle, this appears only in the discussion, and for me was very hard to relate the analysis performed to this framework. For example line 333 says your analysis shows changes in stress-tolerant species, but you did not previously relate your analyzed variables with the CSR strategies. This can be easily fixed by introducing this earlier on, or alternatively, this comparison can be deleted, as the manuscript is already quite complex. I have to say I did not fully understand Fig 5. Calculating CWM on CSR scores, and averaging across space is oversimplifying the story, and the two data points (a mean of means) are very close to each other precisely because this averaging masks more subtle trends, but the variation across species might explain a better story. In any case, this should be integrated into methods and in your modeling framework. Otherwise, sentences such as 417-418 are not convincing.

In-line comments:

Line 21-22: This justification is poor, and mixes a technique (meta-analysis), with an index (taxonomic diversity). I think it can be safely deleted. The abstract can be enhanced for clarity.

Line 79: I think there is an extra s in traits.

Line 111: You mention 4 crop types, but mention only three.

Line 123: By removing rare species your analysis focuses on changes in common species. This is fine but should be discussed and interpreted as such.

Table 1. The underlines, italics etc... are really hard to keep in mind, especially as afterward you refer to e.g. ”explanatory variables” without mentioning them explicitly again. I am not sure which is the best solution, but maybe adding a column to indicate predictors, explanatory variables, etc... and mentioning them again when you specify the models might help.

Line 177: It’s unclear if you always use this ”richness detrended” CWM index, or if you also use the raw index in your models. I think both are valuable and tell you different things. In fact, the species richness detrended CWM is hard to interpret, and if this is the one you report in the analysis, this should be made very explicit and help the reader interpret it with phrases such as ”smaller CWM than expected given a community of this richness”.

Line 179: Why perform the PCA on CWM values, and not in the pool of species values first, and then calculate metrics by communities (e.g. as done by Villager, Laliberte et al. in the FD package you use to calculate the

metrics)? My concern is that CWM is losing information by focusing on the mean (you lose the variance), but maybe there is a rationale to do this. Explaining it would help the reader.

Line 195: Adding the number of observers does not fix observer bias (differences among observers). You do this later on by using observe id as random, but I don't think here is accomplished. Maybe I am losing something.

Line 263: Here is an example of the complexity for the reader to follow up on all the details. By using Ellemberg T, the reader needs to track what this variable meant. I would prefer to be redundant and explain what variables should be interpreted every now and then.

Best,

Ignasi Bartomeus

Reviewed by Clélia Sirami, 04 April 2023

General comment

The manuscript entitled "Functional trade-offs: exploring the effects of climate change and agricultural practices as drivers of field margin plant communities" presents a very interesting study of the interactive effects of climate changes and land-use changes on plant diversity in agricultural landscapes. It develops a spatial and temporal analysis of a large national dataset, based on a functional approach (species traits and the conceptual framework of Grime's CSR triangle), to assess potential trade-offs in the response of plant species to climate changes and land-use changes. The results of this study may contribute to better predictive models and improved conservation strategies for plant species in agricultural landscapes.

The manuscript is well written. The introduction convincingly argues for the need to use a functional approach to understand the interactive effects of climate changes and land-use changes on biodiversity and develops sound hypotheses based on the literature. The material and method section presents the complex dataset collected within the 500 ENI monitoring program and the set of statistical analyses used to test hypotheses. Results are explained clearly. The discussion is structured around three straightforward messages: the effect of climate, the effect of agricultural practices and their interactive effects on field margin plant communities.

I believe there is still a margin for improvement in terms of clarity of the study design and in terms of interpretation of the results. I therefore provide two sets of comments that will hopefully help the authors to improve their manuscript, as well as a third set of comments on other specific points.

Study design

This study is based on a national dataset collected within the 500 ENI monitoring program. Such programs tend to produce large but complex datasets. It seems to me like some key variables (based on the literature) were not considered in the study and some methodological choices were presented only briefly. Since these variables and methods have the potential to influence the outcomes of this study, it seems important to clarify their consequences (or lack therefore). Below, I develop four main comments regarding the study design.

1) The herbaceous field margin represents the uncultivated vegetated area located between the cultivated strip and the adjacent habitat. The manuscript mentions that crop type varied within the ENI dataset, with a clear latitudinal gradient (wheat in the north, vineyard in the south; see Appendix A). Similarly, the type of adjacent habitat is likely to vary within the ENI dataset (e.g. crop, grassland, woodland, road, hedgerow…). Yet,

these two variables were not considered. What is the likely influence of the type of crop and adjacent habitat on plant communities in the field margin? Could the authors explain why these effects do not create a bias in the study?

2) Landscape structure is a major determinant of biodiversity. Andrade et al. (2021) actually explains that landscape variables were measured around each field within the ENI programme. Moreover, the effects of landscape variables on biodiversity have been shown to interact with the effects of agricultural practices (e.g. Ricci et al. 2019). Yet, landscape variables were not considered in the present study (unlike in another manuscript from the same authors on the same dataset, which is currently under review). There may be some valuable reasons for this omission, but I believe these reasons need to be provided and, here again, the absence of bias needs to be justified.

3) The manuscript develops both a spatial and a temporal approach. However, it does not provide a clear description of the data available for these two approaches. It states L190 "To analyze the effects of spatial variations in agricultural practices, explanatory factors and abundances were averaged across years within sites having at least five years of data and five floristic observations, leaving a total of 349 sites". This sentence suggests that some sites were not surveyed every year. Could the authors provide some statistics about the data available and explain why the absence of data for some years does not influence the results of the temporal analysis? Moreover, it is not clear how using the average value across years may influence the spatial analysis. It would certainly be useful to know more about the temporal variability of vegetation and practices at the field level.

4) A subset of well-known species (142 species over 855 taxa) was used for this study. I fully understand that this selection was necessary to avoid observer biases. However, it is not clear whether this choice may have triggered other biases. For instance, this may have resulted in a stronger underestimation of species richness in species-rich field margins. It may also have resulted in the selection of a subset of species with specific traits. Could the authors present the traits of selected species compared to the whole species pool? Could the authors explain why using this subset have not influenced the results, on either species richness or traits?

Discussion of results and perspectives

This study focuses on field margin plant communities but also aims to discuss its broader implications regarding the interactive effects of climate changes and land-use changes on plant communities in agricultural landscapes. The authors discuss their results in a very straightforward way but I believe they could further discuss some unexpected results, the implications for other plant communities within agricultural landscapes and possible avenues to further explore the ENI dataset. Below, I develop four main comments regarding the discussion and perspectives.

1) The authors state L416 "Our results show little effect of agricultural practices, likely because of their limited temporal trend and great range of spatial variability". It is however hard for the reader to grasp this spatial variability. Could the authors provide more detailed information on the spatial variability of agricultural practices? and further discuss why this great range of spatial variability did not result in a stronger effect of agricultural practices in spatial analyses? The reader may also wonder to which extent this lack of effect of agricultural practices may result in underestimating the interactive effects of climate changes and land-use changes. This may be harder to assess but it may warrant a comment in the discussion.

2) The authors conclude that agricultural practices have a weak effect, yet they claim that "the persistence

of intensive agricultural practices and accelerating climate change would have critical consequences for the conservation of floristic diversity in agroecosystems". If I play devil's advocate, based on the lack of effect of in-field practices, I could argue that increasing fertilization and the number of herbicide treatments is likely to have a limited effect (or even no effect) on field margin plant communities, as long as the number of management actions in field margins remains low. This would correspond to a land sparing strategy. I certainly don't have the full picture but it feels like these results could actually be discussed in a more nuanced way than the usual conclusion "intensive agriculture is a threat for biodiversity".

3) The authors provide a very valuable analysis of spatial and temporal variations based on a functional approach, which may allow more generic discussions based on Grime's CSR triangle. Are the functional trade-offs observed in this study likely to apply to other plant communities within agricultural landscapes, in particular weeds? Are the conclusions on the interactive effects of climate changes and land-use changes likely to apply to them too? At present, the authors tend to draw broad conclusions about plant communities in agricultural landscapes without discussing to which extend their results can be generalized. I believe adding such discussion would strengthen the overall conclusion of the manuscript.

4) The 500 ENI monitoring program has produced (and keeps producing) a huge dataset on plant communities as well as other taxonomic groups. The present study is among the first ones to analyse part of this dataset. I believe this study opens the way for very exciting research questions and it would be a shame not to discuss these perspectives (non linearity, time-lag, trophic networks, etc..). It may also be useful to provide a feedback on this program (are all relevant data on agricultural practices available, e.g. dosage, toxicity, type of adjacent habitat? Is the distribution of fields and years sampled adequate? Etc...)

Other specific comments

L21 "However, these impacts are often studied using meta-analyses at large scales, or using taxonomic diversity" Why is the fact that they are studied "using meta-analyses at large scales" is a problem? It seems to me like the knowledge gap primarily comes from not considering functional traits in order to understand the interactive effects of land use changes and climate changes.

L30 "Changes in plant communities were noticeable, especially as they relate to climate change" This phrasing sounds ambiguous to me. It would be clearer to state whether changes are significant and what the key drivers of these changes are.

L58 The amount of pesticide use is not the only dimension of agricultural practices that influences biodiversity. The toxicity of pesticides has been shown to be a key aspect, as well as crop diversification, field margin/boundary management and landscape structure.

L98 It would be useful to clarify somewhere in your hypotheses (and not only in the discussion L374) that the impacts of in-field farming practices on vegetation in field margins are likely to be limited (more limited than for weeds).

L101 I believe there are only two zones considered: Mediterranean and Continental.

L108 How were fields selected within the ENI programme? Are they representative of practices in France or are practices less intensive on average because farmers are volunteering? This might also contribute to the lack of temporal trend in agricultural practices.

L149 It would be useful to make a clear link between Ellenberg codes and their meaning, probably both in Table 1 and in the text the first times they appear in a section, because it is not trivial for non-botanist.

L155 I may have missed it but I couldn't find any information on where chorology data comes from.

L174-177 I find these two sentences hard to understand. It may be easier to follow if these sentences were moved to the relevant sub-section on temporal and spatial analyses.

L191 What is the difference between a "year of data" and a "floristic observation"?

L222 The term "elusive" does not seem appropriate here since you did observe some significant trends.

L242 It may also be useful to specify the meaning of Ellenberg-F in the legend

L251 The two PCA represent either sites or observations and not species. Please rephrase all sentences accordingly.

L288 "Regressions between Ellenberg values and climatic factors". This sentence gives the impression that you conducted several regressions rather than a single one with all explanatory variables. Please reword to avoid any confusion.

L356 "These trait values (low SLA, high seed mass) are also known to be linked to less intensive agriculture" It is not clear whether the ENI dataset is consistent with this relationship?

L407 Change "varies" to "vary"

L419 Is a trend towards more intensive practices not as likely, if not more likely, than a trend towards more extensive practices? This hypothesis also contrasts with the following sentences focusing on agriculture intensification.

L426 "Mediterranean species could face severe agricultural intensification in northern France". Did you really mean "northern France"?

In the data provided, the first row has shifted in files flora_temp and flora_spat

Reviewed by **Diego Gurvich**, 28 March 2023

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