Dear Board members, recommenders and reviewers of PCIEcology,

We would like to thank you very much for the very helpful review of our manuscript. Your comments greatly improved our work. We revised our work according to your suggestions and hope you find our response satisfactory.

In brief, the terminology we used raised some questions from all reviewers, so we changed the following terms for more clarity:
- **"Spatial clumping" → "Spatial autocorrelation"**
- **"Clumped" → "Clustered"**
- **"Extinct patches"/ "non-extinct patches" → "Perturbed patches"/ "unperturbed patches"**
- **"Extinction rate" → "Amount of extinctions"**

As suggested by the recommender, we conducted sensitivity analyses of our simulations on *i)* the size of the landscape and *ii)* the dispersal rate. We found our simulation results to be robust to larger landscapes sizes and stronger/weaker dispersal rates (see new supplementary Figure S7 to S16).

We trimmed the introduction and discussion as suggested by the reviewers in order to get a more straightforward text. We also removed the figures S3 and S4 that were redundant with the figure S5 (previously S7).

Lastly, we do not refer to the "relative importance" of parameters anymore since it was a clear point of confusion for readers and did not add a lot of information to the AICc model selection in our case.

Please find our point-by-point response to all comments below.

Thank you again for considering our manuscript.

Yours sincerely,
Camille Saade, on behalf of all authors.

Note: All lines and page numbers below refer to the track changes pdf version of the manuscript.

Dear Camille Saade,

Your article, entitled **Spatial distribution of local patch extinctions drives recovery dynamics in metacommunities**, has now been reviewed. The referees' comments and the

recommender's decision are shown below. As you can see, the recommender found your article very interesting, but suggests certain revisions.

We shall, in principle, be happy to recommend your article as soon as it has been revised in response to the points raised by the referees.

When revising your article, we remind you that:

1) Data must be available to readers, either in the text or through an open data repository such as Zenodo (free), Dryad (to pay) or some other institutional repository. Data must be reusable, thus metadata or accompanying text must carefully describe the data;

Data and code are available on a Zenodo repository:
https://doi.org/10.5281/zenodo.4660016

2) Details on quantitative analyses (e.g., data treatment and statistical scripts in R, bioinformatic pipeline scripts, etc.) and details concerning simulations (scripts, codes) must be available to readers in the text, as appendices, or through an open data repository, such as Zenodo, Dryad or some other institutional repository. The scripts or codes must be carefully described so that they can be reused;

Data and code are available on a Zenodo repository:
https://doi.org/10.5281/zenodo.4660016

3) Details on experimental procedures must be available to readers in the text or as appendices;

4) Authors must have no financial conflict of interest relating to the article. The article must contain a "Conflict of interest disclosure" paragraph before the reference section containing this sentence: "The authors of this article declare that they have no financial conflict of interest with the content of this article.";

We have added a "Conflict of interest disclosure" at the end of the main text (l. 673).

5) This disclosure has to be completed by a sentence indicating, if appropriate, that some of the authors are PCI recommenders: "XY is one of the *PCIEcology* recommenders.".

Emanuel A. Fronhofer is one of the PCIEcology recommenders. We have added this information to the "Conflict of interest disclosure".

When your revised article is ready, please:

1) Upload the new version of your manuscript onto your favorite open archive;

2) Follow this link https://ecology.peercommunityin.org/user/my_articles or logging onto the *PCIEcology* website and go to 'your contributions' section and then to 'Your submitted preprints' subsection in the top menu;

3) Make your changes to the title, summary, link to the article (or its DOI) and keywords if necessary by clicking on the 'Edit Article' button. If not already done, you also need to upload a picture or an illustration for which you own the rights – this picture will be used to illustrate your article, if recommended;

4) Write, copy/paste or upload (as a PDF file) your reply to the recommender's and reviewers' comments by clicking on the 'Write, edit or upload your reply to recommender' button. You can also upload (as a PDF file) a revised version of your article, with the modifications indicated in TrackChanges mode;

5) When you are ready to submit your new version, click on the 'Save & submit your reply' button.

Once the recommender has read the revised version, he/she may decide to recommend it directly, in which case the editorial correspondence (reviews, recommender's decisions, authors' replies) and a recommendation text will be published by *PCIEcology* under the license CC-BY-ND.

Alternatively, other rounds of reviews may be needed before the recommender reaches a favorable conclusion. He/she may also reject your article, in which case the reviews and decision will be sent to you, but they will not be published or publicly released by *PCIEcology*. They will be safely stored in our database, to which only the Managing Board has access. You will be notified by e-mail at each stage in the procedure.

We thank you in advance for submitting your revised version.

Yours sincerely,

The Managing Board of *PCIEcology*

PCIEcology is one of the communities of the parent project Peer Community In. It is a community of researchers in Ecology dedicated to the recommendation of preprints publicly available from open archives (such as bioRxiv, arXiv, PaleorXiv, etc.), based on a high-quality peer-review process. This project was driven by a desire to establish a free, transparent and public scientific publication system based on the review and recommendation of preprints. More information can be found on the website of *PCIEcology* (https://ecology.peercommunityin.org/).

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Spatial distribution of local patch extinctions drives recovery dynamics in metacommunities

Camille Saade, Sonia Kéfi, Claire Gougat-Barbera, Benjamin Rosenbaum, and Emanuel A. Fronhofer

Abstract

Human activities lead more and more to the disturbance of plant and animal communities with local extinctions as a consequence. While these negative effects are clearly visible at a local scale, it is less clear how such local patch extinctions affect regional processes, such as metacommunity dynamics and the distribution of diversity in space. Since local extinctions may not be isolated events in space but rather clump together, it is crucial to investigate their effects in a spatially explicit framework.

Here, we use experimental microcosms and numerical simulations to understand the relationship between local patch extinctions and metacommunity dynamics. More specifically, we investigate the effects of rate and spatial clumping of extinctions in a full factorial design. Experimentally, we found that local patch extinctions increased inter-patch (β-) diversity by creating differences between extinct and non-extinct patches and at the same time increased local (α-) diversity by allowing inferior competitors to persist. Most importantly, recolonization dynamics depended more strongly on the spatial distribution of patch extinctions than on the extinction rate per se. Clumped local patch extinctions reduced mixing between extinct and non-extinct patches which led to slower recovery, lower α-diversity in non-extinct patches and higher β-diversity. Results from a metacommunity model matched the experimental observations best when the model included a competition-colonization trade-off, giving a hint at the underlying mechanisms.

Our results highlight that local patch extinctions can increase the diversity within and between communities, that the strength of these effects depends on the spatial distribution of extinctions and that the effects of local patch extinctions can spread regionally, throughout a landscape. These findings are highly relevant for conservation and management of spatially structured communities under global change.

Keywords: disturbance, spatial clumping, correlation, Moran effect, microcosm, protist, theory

Round #1

Author's Reply:

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minor revisions requested before recommendation

Dear Authors,

I would like to apologize for the delay in getting back to you. I have been waiting for a 3rd review for more than 3 weeks now, so I decided to proceed with the first two reviews only, to avoid keeping you waiting any longer.

Both reviewers were very positive about your work, which they found thorough and clearly written. The elegant link between experimental results and model simulations was particularly appreciated. I reckon that most of the comments we had should be easily addressed, but please pay particular attention to the suggestions concerning results presentation, the analysis of dynamical response variables, and the analysis (or at least discussion) of effects of temporal synchrony in perturbations.

I will be most happy to recommend your manuscript once you have addressed these comments. Thank you for sharing your work with us.

Best regards,

Elodie Vercken.

My own comments:

- I wondered why you used the terms "spatial clumping" rather than "spatial autocorrelation".

Spatial autocorrelation is often used with regards to time (e.g., time series from close-by location tend to vary in the same way). Since we consider events that happen only once and are discrete in time, we used "clumping" to avoid the confusion, but both terms

work. We changed "clumping" to "autocorrelation" and "clumped" to "clustered", which may be more clear for the reader.

- The size of the landscape sets a strict limit to the range of spatial clumping that can be explored. I reckon you could use the model to investigate larger landscapes, to check whether the dominant influence of the spatial distribution of extinctions over extinction rates holds in less constraint configurations.

Thank you for the suggestion, we conducted the same simulation on 16*16 landscapes (16 times more patches) with the same proportion of extinctions (0, ¼ and ½ of all patches -> 0, 64 and 128 patches), and the patterns are remarkably consistent (qualitatively) for all measured variables and all competition scenarios. We added two figures to the supplement to show this (Fig. S7 and S8), and we also added a section in the methods section (l. 271) and in the results section (l. 399)

- Also, I think it would be informative to compare the distributions of some functional indicators between experimental modalities (e.g. distance to the closest extinct patch, proportion of extinct patches in a 1-patch neighbourhood), as it would help to interpret the results (see for instance l. 464-465). Would it be possible to analyze the different response variables with such quantitative covariates, rather than "clumped vs dispersed"?

Thank you very much for your suggestion. We have done some explorations and most of these functional indicators unfortunately covary very much (e.g., distance to the closest unperturbed patch and number of adjacent unperturbed patches) and are also confounded with the experimental treatments (e.g., previously mentioned indicators and spatial autocorrelation of extinctions), making it difficult to disentangle the effects of each one in a statistical model without additional data. Hence, we could not include such variables in our statistical analyses. However, we added the dynamics of biomass and alpha-diversity (Fig. S18 and S19) with different colors and line types for the number of adjacent unperturbed patches and the distance to the closest unperturbed patch for reference, respectively. We also added a table giving the average of these two indicators for each treatment (Tab. S2).

- It seems that the results are sensitive to dispersal (l. 287-290); Maybe it would be interesting to run a sensitivity analysis relative to dispersal on the model outcomes. It might also interact with landscape size, as the influence of higher dispersal rates should be stronger in larger landscapes.

Thank you for this comment. We have now added a sensitivity analysis with varying dispersal (0.2; 0.5; 2; and 5 times the dispersal rates used the main text). While we observe quantitative differences, most qualitative patterns described for the "empirical interactions" and "competition-colonization trade-off" scenarios are coherent for dispersal rates up to 2 times stronger/weaker than our standard scenario (e.g., stronger influence of spatial organisation than number of extinctions, higher beta-diversity for clustered extinctions, higher alpha-diversity spillover and faster biomass recovery for

dispersed extinctions). We added supplementary figures to visualize these new results (Fig. S9 to S16) as well as text to the methods section (l. 271) and results section (l. 399).

- Figures : why is the modality "no extinction" represented on Fig 2 and not on Fig 1? (also, legend says that these represent diversity in extinct patches or extinct landscapes, so something is not right here).

Thank you for catching this. The statistical model associated with Fig. 1 describes what happens in "extinct" (now called "perturbed") patches, so we initially did not plot the data for "non-extinct" (now called "unperturbed") patches. We have now added the data of patches from the control landscapes (Fig. 1 a, b and c) as a reference and for consistency with the figure 2. We have also clarified the legends of figures 1 and 2.

- Fig 3: why are only non-extinct patches adjacent to an extinct one included in the analysis? It does raise again the issue of landscape size, and the potential large-scale influence of extinction events.

We have added a paragraph in the discussion regarding this point (starting from l. 560). Please also see our response above regarding additional simulations which indicate that our results are robust to larger landscapes. The indirect effect of extinctions only reached adjacent patches; we observe no large scale influence in our work. Note that there are more adjacent non-extinct patches in dispersed treatments, which means that dispersed extinctions both have a stronger effect on non-extinct patches and affect more patches.

- I did not understand how you estimate the consistency between experimental results and the different modelling scenarios. For instance, L. 340-341, you state that the "competition-colonization trade-off" scenario is more consistent with experimental results, while based on the effect sizes on Fig 3a and Fig 4a, I would tend to say that the "empirical interactions" scenario is a better fit?

We do not estimate any match formally. We compared empirical data and model outputs qualitatively. We now specify this in the text (added l. 399). We agree that the "empirical interactions" scenario is more consistent and have changed the text accordingly (l. 399 and abstract).

Additional requirements of the managing board:

We would like to receive your revision within 2 months. If you need more time, just tell us.

As indicated in the 'How does it work?' section and in the code of conduct, please make sure that:

-Data are available to readers, either in the text or through an open data repository such as Zenodo (free), Dryad or some other institutional repository. Data must be reusable, thus metadata or accompanying text must carefully describe the data.

Data and code are available on a Zenodo repository:
https://doi.org/10.5281/zenodo.4660016

-Details on quantitative analyses (e.g., data treatment and statistical scripts in R, bioinformatic pipeline scripts, etc.) and details concerning simulations (scripts, codes) are available to readers in the text, as appendices, or through an open data repository, such as Zenodo, Dryad or some other institutional repository. The scripts or codes must be carefully described so that they can be reused.

Data and code are available on a Zenodo repository:
https://doi.org/10.5281/zenodo.4660016

-Details on experimental procedures are available to readers in the text or as appendices.

The experimental procedure is detailed in the "Methods" section.

-Authors have no financial conflict of interest relating to the article. The article must contain a "Conflict of interest disclosure" paragraph before the reference section containing this sentence: "The authors of this preprint declare that they have no financial conflict of interest with the content of this article." If appropriate, this disclosure may be completed by a sentence indicating that some of the authors are PCI recommenders: "XXX is one of the PCI XXX recommenders."

We added a "Conflict of interest disclosure" at the end of the main text (l. 673):

"Conflict of interest disclosure

The authors of this article declare that they have no financial conflict of interest with the content of this article. Emanuel A. Fronhofer is one of the PCI Ecology recommenders."

Reviews

The manuscript "Spatial distribution of local patch extinctions drives recovery dynamics in metacommunities" presents the effects of the extent and spatial clustering of an extinction event on the recovery of local and regional diversity, productivity, and on the recovery rate, in a metacommunity of competing species. It is based on a microcosm experiment and a mathematical model. Spatial clustering had a larger effect than the extent of extinction events.

The introduction is effective in setting the scene for this study, but I believe it could be made clearer, at the end of the intro, which phase of recovery dynamics are targeted here with respect to the diversity and productivity data (just after the disturbance? One generation later? Etc).

We followed the recolonization dynamics just after the disturbance for a duration of two weeks, which is roughly the time it takes for *Blepharisma* sp. to exclude the other species in a single patch co-culture (see Fig. S5 h-j). This temporal window allowed us to capture the transient effects of the disturbance, before the system reached an equilibrium dominated by *Blepharisma* sp.

We reformulated the introduction (l. 81) and added a phrase in the methods (l. 103-107) so that this point is now more highlighted.

That was not clear to me, and I also did not find this back in the methods. The methods are clear and well-described (apart from the time points associated with the diversity and productivity data).

(Concerning the issue of the time points, please find our answer in the responses to your specific comments regarding Fig. 3).

The results are well-structured (although I provide some detailed comments below). I find the discussion repeats the results a bit too much, which makes the discussion section relatively long and renders some of the interesting bits less visible.

The text was shortened to avoid repetitions as much as possible.

As a general comment to the paper, which I generally liked and found to be based on an impressive amount of work and well thought-through designs, I have to say it took me a while to realize that the main variables (e.g. Fig1) were measured during a recovery trajectory. This also raises the question of how representative these measurements are for the 'recovery dynamics' as a whole. If possible (e.g. if the necessary data have been measured in the experiments) I think it would be cool to try to plot the rate of change (or some other summary stat) of (some of) these variables during the recovery period instead of the variables themselves. This might be even more effective in addressing the main questions.

Thank you very much for your positive evaluation of our manuscript. We appreciate all your points a lot.

Rates of change would indeed be very clear and effective in characterizing the dynamics of the system. However, non-linearities in the dynamics of our measured variables make rates both hard to define and not really representative of the dynamics. We have now plotted the raw dynamics of biomass and local diversity inside each patch in fig. S18 and S19. The dynamics of biomass are very non-linear following extinctions (with the classic "s-shape" associated with logistic growth, Fig. S18), hence we use the "recovery time" rather than a rate (which is similar to 1/rate but does not require linear dynamics to make sense). The dynamics of local diversity did not exhibit much slope and usually fluctuated around a given value depending on the treatment (high in perturbed patches, low in unperturbed patches, fig. S19).

Another general comment, which may seem a tad pedantic, but well: I would propose to replace 'extinct patches' with 'disturbed' or 'perturbed' patches. That is because (1) a patch cannot go extinct, only populations or species can; and (2) it prevents some odd constructions like 'alpha diversity in an extinct patch' (which, technically, should be zero).

We are sorry that this was unclear. As suggested, we have replaced "extinct" with "perturbed" (and "non-extinct" with "unperturbed") throughout the manuscript.

Finally, I wonder if it would be possible to compare the measured dispersal rates and the competition parameters to see if the empirical system exhibited features akin to a competition-colonization tradeoff.

Thank you for pointing this out. Classical competition-colonization trade-off literature uses identical or similar protist species (e.g., Cadotte 2007), which is why we included this scenario in our analysis. However, previous work with some of the species used here indicates that the competition-colonization trade-off may be more complex, involving plastic dispersal rates (Fronhofer et al. 2014, 2015 & 2018). Here, we don't have direct measurements of dispersal rates, however, we do have movement speed which usually correlates with dispersal in this system (Fronhofer & Altermatt 2015, Pennekamp et al. 2019; speed: Col > Ble > Tet, see l. 134 and Fig. S3). Movement speed suggests a competition-colonization trade-off between Col and Ble. Finally, we would like to underline that the competition-colonization trade-off scenario indeed seems to match less well with our data than the "empirical" scenario.

Specific comments

L13: 'persist' suggests long-term persistence; not sure if this is what is meant here.

Thank you. We did not imply long term persistence. We have changed the wording throughout the manuscript to "delaying the competitive exclusion of inferior competitors".

L37: In a scenario where all biomass disappears, how can one trophic level benefit? If all are removed, it does not matter which trophic level a species belongs to, right? Please clarify.

We are sorry that this was unclear. We are here referring to sustained local patch extinctions (e.g. one or several patches are made unsuitable to life). If this (local) perturbation drives top predators to lower biomass/to extinction regionally, then top-down control is released and intermediate and basal species can see their biomass (regionally) unaffected or even increased by the local extinctions (see Ryser 2019, Fig. 3). We modified the sentence accordingly l. 41.

L46-52: Although I like the intro, I feel the focus of this bit is less clear and evokes some elements that are not core elements, so my suggestion would be to trim it down. At this point, it was also not clear what was meant by 'spatially structured'.

We are sorry that this was unclear and reformulated. We have replaced "spatially structured" by "made of independent units linked in space by dispersal") and removed the sentence l. 50-52 on species coexistence. We kept the following two sentences on spatial synchrony and perturbation spread which we think are important elements.

L69: I propose to use something like 'extent' instead of 'rate', as there's no time component, right?

Thank you very much for the suggestion. We have now replaced "extinction rate" by "amount of extinctions" throughout the manuscript.

Eq4: I advise to use single-letter expressions for variables (e.g. m instead of n_links).

Thank you. We have changed "n_links" to "c".

L221: I advise to use 'interaction coefficients' or 'competition coefficients' instead of 'competitive abilities'.

Thank you for the suggestion, we replaced it accordingly.

L227: It would be nice to have a rationale for the scenario of no species interactions. I'm sure a few things can be learnt from that additional scenario, but spelling them out would be useful.

Thank you, we have now added more details in lines 285-288. The idea was to investigate which results were due to biological properties of our system and which were more general. For example, the results on biomass and recovery time were well described by this scenario, indicating that biomass dynamics depend mainly on simple diffusion processes and as such should be pretty general. The results on diversity (local and inter-patch) were not described at all by this model, which means that they depend on species interactions and are probably specific to the setting of ranked competitive abilities.

L258: "extinction rate alone only had a marginal effect on the outcome of the experiment". I'd say that for beta diversity it had.

The effect of extinction rate largely depends on the spatial correlation. Here we were trying to convey that the interaction is the most important, but we can see how the phrasing implied that extinction rate had no effect at all. We are sorry for that and have changed the phrasing l. 302 to: *"Except for β -diversity, the amount of extinctions alone only had a marginal effect on the outcome of the experiment as indicated by model selection".*

Fig.1: why isn't there a 'no extinctions' scenario plotted for the experiment?

Thank you for this suggestion, we have added it to figure 1.

Fig.1: the variable 'recovery time' seems to be factorial (5 levels). Was this the case and if so, why, and how was it dealt with in the statistical analyses?

In fact, recovery time was not factorial, measurements were made at fixed time intervals which leads to seemingly discrete times. Therefore, we have used time as a continuous variable.

Fig.2: depending on what you want to convey as the main result, there might be more effective ways to plot these data. One way could be to make a biplot for (some of) the variables at 4 extinctions vs. 8 extinctions, to show they are all located near the 1:1 line, regardless of interaction scenario (coded with a symbol key for example) or the spatial arrangement of extinction (e.g. using a color key). A second panel could do exactly the same but would now compare the variables between the two spatial extinction arrangements. Just a spontaneous thought though.

Thanks a lot for this suggestion. A biplot would be very effective in comparing experimental treatment. Unfortunately, we do not have a 1:1 correspondence of landscapes between treatments, so the only solution would be to plot averages of each treatment (leaving us with only 2 points per scenario). Moreover, we have tried to keep the same representation as in the experimental data for clarity. We have therefore chosen to keep the original representation.

More on Fig.2: It seems interesting that beta diversity is higher in perturbed networks than in nonperturbed networks when there are no species interactions. Is that then purely due to difference in species abundances?

It is indeed due to differences in species abundance, because species densities are more even during the growth phase/recolonisation process that at equilibrium (the equilibrium density differs greatly between Tet, Ble & Col).

In general, it could be insightful to plot one of these beta diversity indices that only look at compositional similarity and not at similarity of abundance.

This would indeed be very interesting if our data allowed it. While it is technically possible to separate our beta-diversity index into a "replacement" (composition) and "richness" (abundance) components, we have a strong uncertainty on presence/absence in empirical data due to errors of identification (Pennekamp et al. 2017 https://doi.org/10.1371/journal.pone.0176682) (i.e., a small percentage of the most abundant species can end up assigned to an absent species). In simulations, we do not have presence/absence data (since density can get arbitrarily small). We could set an arbitrary threshold below which we consider the population to be extinct, but a composition based beta-diversity would then be sensitive to our threshold choice.

L265-266: I would be a bit more precise. What is meant by 'reproduced these results'? What are 'these results'? I suspect it is the relatively small effect of the number of extinctions compared to that of spatial arrangement but it would be good to repeat this here.

We are sorry that this was unclear and have added precisions in l. 312: *"Numerical simulations of our metacommunity model with the same spatial configuration and extinctions patterns reproduced these results (a weak effect of the amount of extinctions compared to that of their spatial arrangement) for all competition scenarios (Fig. 2 and 4)."*

L282-283: It could be interesting to compare the distributions of interaction strengths across model scenarios (possibly scaled by intrinsic growth), as it might explain the results for recovery rate.

We are not sure we understand this comment entirely, our design controls for interaction strength in all scenarios apart from "no interaction".

Fig.3: This caption mentions "at the last two measurement points": what are these and why was this info not mentioned in Fig.1 (and 2)?

All measurements were made at regular time intervals ("measurement points"). We expected indirect effects to be delayed. An increase of alpha-diversity in unperturbed patches (due to dispersal from species-rich perturbed patches) could only happen once biomass was high in perturbed patches, hence we built the statistical model using only data from the end of the experiment (last two measures). The dynamics of alpha-diversity (fig. S19) indeed show that the alpha-diversity in unperturbed patches increases only at the end of the experiment.

This was not mentioned in Fig. 1 and 2 because these figures are concerned with direct effects (effects in perturbed patches) that we investigated over all post-extinction measure points.

For more clarity, we added a paragraph to the methods detailing this l. 215: *"The direct effects of extinctions (i.e., the variations of biomass and α-diversity in perturbed patches as well as the variations of β-diversity; Fig. 1) were estimated using all the measurements obtained in perturbed patches in the two weeks following the extinctions. We expected the indirect effects of extinctions (i.e., the variations of biomass and α-diversity in unperturbed patches; Fig. 3) to be*

much more elusive, so we used only the data from unperturbed patches directly adjacent to perturbed patches. We expected indirect effects on biomass (i.e., a reduction of the biomass of unperturbed patches due to reduced fluxes from perturbed patches) to happen early in the recolonization process, so we estimated them using only the data obtained just after the perturbations (from the two measurements following the extinctions, Fig. 3b). On the contrary, we expected indirect effects on α-diversity to happen late in the recolonization process (once the biomass in perturbed patches was high enough to have an effect on the composition of unperturbed patches) so we estimated them using data obtained near the end of the experiments (from the last two measurements made, Fig. 3a)."

The section starting L357 starts by mentioning the consistency of the results across response variables, but then focuses on the recovery rate only. It might be useful to, even if briefly, mention how exactly effects on the recovery rate explains the effects on the other variables.

Thank you for this suggestion. We have now added text to discuss the effects on alpha and beta diversity, starting l. 445: *"Under the "rescue" regime, dispersal between perturbed and unperturbed patches is marginal compared to local dynamics. Perturbed and unperturbed patches are strongly differentiated, and the recovery dynamics mainly rely on local growth.* <u>*Because of this strong differentiation, β-diversity was higher than in the "clustered extinctions" treatment, but the high α-diversity of perturbed patches did not spill over much to unperturbed patches.*</u> *Under the "mixing" regime, dispersal between perturbed and unperturbed patches is on par with local dynamics. Perturbed and unperturbed patches are well mixed, and both local growth and dispersal from perturbed patches participate substantially to the recovery.* <u>*Because of the mixing between perturbed and unperturbed patches, α-diversity in the "dispersed extinctions" treatment in unperturbed patches increased greatly (due to dispersal from perturbed patches), but β-diversity was lower than in the "clustered extinctions" treatment."*</u>

L366-367: If the data are available to do so, it could be nice to substantiate this further by testing how the number of neighboring non-extinct patches affects recovery rate.

Thank you for this suggestion. We have added figures illustrating the biomass and recovery rate (fig. S18) and alpha-diversity (fig. S19) depending on the distance and connectivity to unperturbed patches. Please find below our response to the recommender (Elodie Vercken) who raised a very similar point:

Thank you very much for your suggestion. We have done some explorations and most of these functional indicators unfortunately covary very much (eg. distance to the closest unperturbed patch and number of adjacent unperturbed patches) and are also confounded with the experimental treatments (e.g. previously mentioned indicators and spatial autocorrelation of extinctions), making it difficult to disentangle the effects of each one in a statistical model without additional data. Hence, we could not include such variables in our statistical analyses. However, we have added the dynamics of biomass and alpha-diversity (Fig. S18 and S19) with different colors and line types for the number

of adjacent unperturbed patches and the distance to the closest unperturbed patch for reference, respectively. We have also added a table giving the average of these two indicators for each treatment (Tab. S2).

L400-401: I would find it more logical to state "extinct patches had a higher biomass".

Thank you for this suggestion, we modified the text accordingly.

Summary

Saade et al. (bioRxiv 2020.12.03.409524) evaluated how the rate and spatial distribution of extinctions affected metacommunity diversity. Using a simplified ciliate metacommunity of three species in a factorial manipulation of extinction rate (0, 4, and 8 patches out of 16) and spatial distribution (dispersed vs. clustered) paired with numerical simulations, Saade et al. demonstrate that recolonization dynamics and community recovered depended more on spatial distribution or the interaction between spatial distribution and extinction rate than extinction rate alone. By combining the experiment with numerical simulations, Saade et al. were further able to identify that interspecific interactions and, specifically, a competition-colonization tradeoff as a possible mechanism. It is important to note that these numerical simulations were based on empirical measurements of the study organisms and more directly link the numerical to experimental (and vice versa) in the study. The authors were also very clear when discussing their results and the broader implications, presenting any uncertainty and tempering any broader claims with appropriate context; in doing so, I think it makes the link between this study to the broader body of work stronger because it accepts limitations while also presenting the strengths. This study contributes very strong evidence of how spatial configuration needs to be considered when looking at extinction or extirpation dynamics. This study also provides an important "proof-of-concept" that spatial distribution of extinctions should be considered to understand community recovery. In an applied sense, this study suggests that exclusion of spatial distribution from analysis or management of extinctions could be faulty. In summary, I found this paper very compelling, clearly written and 'mentally replicable' (i.e., while reading the methods and looking at the figures, I could replicate the study), and with no major criticism or comments. I only have a few minor suggestions to improve clarity and presentation of the results and some comments for the discussion.

Thank you very much for the positive evaluation of our work.

Minor Comments

1. Line 187: What R package was used for the mixed-effects models? It would be good to know whether it was lme4 or nlme, for the sake of reproducibility.

    We are sorry for this omission and have now added the information in line 195. We were using lme4 (version 1.1-23). The script for the analysis is available in the Zenodo archive (https://doi.org/10.5281/zenodo.4660016**).**

    For the points below, please note: The relative importance of an explanatory variable is the sum of the weights of all models containing this variable. It is supposed to give a synthetic view of the model selection process (e.g., Table 1 (resp. 2) is a more synthetic version of Table S2 (resp. S3)). Since the RI does not add any important information in our case and is a likely point of confusion to the

reader, we have decided to remove tables 1 & 2 and to use only model weights (from tables S2 and S3) rather than RIs.

2. Line 195: On which parameters was relative importance calculated and how was it determined? It is likely a simple thing I just missed, but this component spilled over to Table 1 and Table 2 where it was not clear exactly what information was being provided.

   The RI of a parameter is the sum of the weights (from the AICc model selection) of all models comprising this term (e.g., the RI of the "spatial autocorrelation" of extinctions in explaining alpha-diversity (first term in the ex-table 1) is the sum of the weights of the following models:

   - **Alpha-diversity ~ spatial autocorrelation**
   - **Alpha-diversity ~ spatial autocorrelation + extinction rate**
   - **Alpha-diversity ~ spatial autocorrelation * extinction rate**

   that are available in table S2a.)

3. Line 258: Extinction rate is said to have a "marginal effect" on community recovery, and this seems accurate based on Table S2 but not Table 1. Looking at Table 1, I would consider the effect of extinction intermediate but Table S2 I can see how it is marginal. The in-text presentation could therefore appear misleading when calling the effect marginal yet readers only immediately seeing Table 1.

   You are right that the extinction rate had a high RI. However, in this case this is because the model with the interaction clustering*rate had a really low AICc, yet the extinction rate on its own (without an interaction) explained very little of the variance. This should no longer be a problem since we do not use RI anymore.

4. Table 1 and Table 2 captions: As written, I do not know exactly what the relative importance is derived from. Following my second comment (Line 195), a more detailed clarification in the methods would make a brief, if any, clarification in the table caption helpful.

   We have removed the RI to avoid confusion.

5. Line 344: It is reported that "β -diversity was fairly low because the patches ended up being homogeneous" but the results are not shown. This happens again on Line 397. Would it be possible to add a figure to the supplement?

   Thank you. We have now added a figure with the median and quantiles of patch composition in each treatment (Figure S6) to show this point.

6. Line 517: Effects of metacommunity synchrony on extinction dynamics are discussed, but asynchrony and 'portfolio effects' could also be discussed. While the experiment only used synchronized extinction treatments, it would be good to at least mention asynchrony and how this could have affected results and conclusions.
   ○ More broadly, is it possible to use the metacommunity model to parameterize temporal asynchrony in the extinction treatments and numerical responses? Spatial asynchrony would also be interesting, along with the interaction of spatial and temporal asynchrony.
   ○ I completely understand that this is not really the point of the paper and going too far into this topic would dilute the message of the present study, but it would be good to at least note potential effects of asynchrony and potentially parameterize temporally and/or spatial asynchronous models.

Thank you very much for this suggestion. Asynchrony in the extinction treatments would make a very interesting extension of this work and our code could easily be adapted to model asynchronous extinctions. However, this is really beyond the scope of our article, and experimentally exploring this question would require more time and space than we currently have in the lab.

Despite these issues, we added a paragraph in the discussion to explore how asynchronous extinctions could affect our results (l. 610):

"Secondly, the temporal scale of our study is very narrow as we consider a single event of synchronous extinctions. In nature, extinction events can potentially be asynchronous and recurring over time. Both the degree of synchrony and the frequency of extinction events could shape their consequences on metacommunity dynamics. A first intuitive approach to explore these directions would be to use a space-for-time substitution, and to consider the amount of extinctions (in space) as analogous to a frequency of extinctions (in time) and the spatial autocorrelation as analogous to the synchrony of extinctions. However, adding a temporal dimension could also lead to consequences unforseen in our mostly spatial setting, such as the synchrony/asynchrony of extinctions affecting metacommunity stability by affecting the synchrony/asynchrony of local community dynamics (Fox et al., 2017). Exploring these questions would thus require to go beyond a simple space-for-time substitution and to conduct new experiments on a larger temporal scale."

7. Figure S5: It looks like the default graphing parameters re-organized the time points so that the x-axis does not go in chronological order. In ggplot (and wrappers like ggpubr), this could be done with the scale*x*discrete() function.

Thank you for catching this. We have corrected the error.