Round #1

Author's Reply:

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by Sandrine CHARLES, 11 Oct 2022 09:31

Manuscript: **https://doi.org/10.1101/2022.05.31.494006**

Major revisions

Dear authors,

Based on the reviewers' comments, I ask you to resubmit your preprint after the consideration of the major issues raised by the reviewers. Some key points have been pointed out, for example the restructuration of your results and discussion parts, as well as more details about your modelling approach and the underlying hypotheses you chose. Two of the reviewers provided very detailed comments that should really help you in improving your manuscript. So, please benefit from this in preparing a new version, that you will join to a point-by-point reply letter for me.

Best regards,

Sandrine Charles

Dear Sandrine Charles,

Thank you very much for the time spent reading and finding reviewers for our manuscript. We took into account all your comments and comments from the reviewers. We considerably refocused the manuscript by moving to supplementary material every result analysis that did not specifically address the comparison of expansion scenarios. We also added a general figure explaining the simulation study and numerous details about the population dynamics modelling and its relation to the real-world ecology of pests and predators. We hope that you will find that the manuscript was improved by the changes.

Please also note that the data repository has been updated to take into account some of the comments, and can now be found here: https://doi.org/10.5281/zenodo.7576542

Thank you again and best regards,

The authors

Reviews

Reviewed by Lionel Hertzog, 26 Sep 2022 08:10

In this manuscript Delattre et al explore the effect of organic farming expansion on pest and predator densities in virtual landscapes. The topic is timely given the upcoming expansion of organic farming and the uncertainty for practitionners regarding its potential drawbacks. The manuscript is generally well written and the results are interesting. I have the following major issues with the manuscript as it currently stands:

Thank you for these comments and for the time you spent reviewing the manuscript.

- parts of the discussion need to be expanded by comparing the results presented here with published empirical and modelling work. I am also missing a section in the discussion discussing the potential limitations of the simulated results presented here given the assumptions made in the model.

We now referred more to other modelling theoretical studies of spatial pest-predator models on crop mosaics in the discussion (e.g. lines 539-542, 574-578, 585-587). We also added a reference to a review of pest-predator modelling studies (Alexandridis et al. (2021)) and used it to detail the limitations of our approach. To our knowledge, only Bianchi et al. specifically addressed the question of OF expansion for pest-predator interactions and their results are now better compared to ours (lines-594-600). Edwards et al. 2018 addressing pest densities for different levels of spatial grouping of annual crops found that grouping was better for dispersal-limited pests, by mechanisms that are similar to those promoting the IP scenario. This is now discussed also (lines 539-542). We also added a full section to the discussion about “limits and benefits of the modelling approach” in which we discuss the choices we made for modelling, simplifications with regards to real-world ecology of pests and predators and consequences they may have (lines 575-615).

For instance the results show situations with predator to prey ratios above one, this is contradictory to most empirical settings. Another example is the assumption that pesticide will affect pest but not predator.

Predator-to-prey ratios are above 1 only in organic fields, in situations where there is a large spill-over of predators from semi-natural habitats (50% SNH and large fragmentation). This is made possible by the fact that predators are generalists and their density may thus be large, even if pest density is low (Fig. 3 and 5). This is now explained lines 378-379 “Note that in landscapes with 50% SNH predator densities in organic fields were sometimes larger than pest densities (figure 3). This was most prominent when fragmentation was high, an indication that it resulted from spillover of predators from semi-natural habitats.”

We also added examples of specific pest management practices that affect pests but not predators such as the spread of specific viruses (lines 211-214).

- many results are presented: two different type of agricultural fields, within three different landscape context, for three different scenario of organic farming expansion, with four different pesticide effect for two different dispersal intensities ... I would argue to add at least one schematic overview figure summarizing the results. I also wonder whether to streamline the results, based on the aim of the manuscript, by focusing on

the interaction between organic farming expansion (three scenarios) and landscape context (two level of fragmentation) and put the rest in appendices.

We added a new figure (new Fig. 1) explaining the simulation design focusing on the interaction between the spatial scenario of organic farming expansion (Random versus IP or GP) and the landscape context (3 proportions of SNH x 3 fragmentation levels). To better focus the manuscript, as suggested (and also suggested by Julia Astegiano, the second reviewer) we moved the results concerning the impacts of the type of organic farming (intensity and specificity) and the pest dispersal to supplementary material (respectively SM 2 and SM 3). Furthermore, as suggested by Julia Astegiano we also moved the description of landscape changes under the three spatial scenarios of organic expansion, and its discussion, to the supplementary material section (SM1). We hope the manuscript is now more focused.

- the manuscript uses quite some abbreviations, I would recommend at the start of each section to fully name all terms before using the abbreviations.

We recalled abbreviations at the beginning of sections and changed abbreviations to full names in a number of instances, in particular when mentioning organic or conventional fields.

In addition I have the following minor comments:

line 25: remove "the”

Done

line 47: cropping system sounds a bit too restrictive here, I would replace it with farming system.

Done

line 71-72: by investigating the effect of OF on what? Please precise.

The sentence was modified to: “The effect of OF expansion on pests and their natural enemies can be approximated, in a space for time approach (Blois et al 2013), by investigating how pest and natural enemies are affected by the proportion of OF in the landscape.” (line 64-66)

line 88: parenthesis around Petit can go away.

Done

line 88: damage on what? Do you mean yield loss?

Changed to “crop damage” that is more general than yield loss (crop quality rather than quantity may be reduced).

line 100-102: I would re-phrase the second part, something like: "and may support higher spill-over of predators from seminatural habitats into agricultural fields"

Done, thank you for the suggestion.

line 121: I would re-phrase something like: "lack of CBC data in the context of OF expansion"

Done, thank you for the suggestion.

line 142: de-capitalize intensity. Maybe better to use or add the word pesticide here?

This sentence was modified for two reasons. First, we moved to supplementary material results concerning the effects of organic farming type and pest and predator dispersals to supplementary material. Second, we now changed the term 'pesticide' to 'pest management' throughout the manuscript to better include differences in management between organic and conventional fields. The sentence is thus: " We further investigated if the ranking of scenarios was robust to pest management methods in OF fields and pest and predator dispersal abilities." (lines 117-118)

line 152: please provide in the text or in appendices information on this model. Readers should not have to read another paper to know the basics of the model used here.

Done, we provide many more details on the model in the main text (lines 145-240).

line 156: what is HSN?

Sorry, it's a typo for SNH. Done.

line 169: error in formatting of the equation

Our apologies, we did not check the equations thoroughly after formatting to pdf. We now added the equations as an image which also allowed us to add the description of their terms.

line 165-182: I am not an expert of predator-prey LV models, but I find it curious that the effect of the prey on the predator growth is modelled separately from the predator growth function fp. I would have assumed that the function fp also takes as parameters the prey population and the interaction term. I am also not so sure what this interaction term represents, is this another way to specify the conversion efficiency of prey biomass into predator biomass? Could you cite some previous work that used a similar mathematical formulation of spatial predator-prey LV model? I also note that intrisic death rates as controlled by the parameters p are assumed to be solely driven by pesticides.

We added more explanations about the equations to clarify these points.

- We now specify that f_P represents the intrinsic growth of the predator, i.e. in the absence of the pest, which can be positive because the predator is a generalist and can feed on alternative prey (lines 173-174).
- We clarify the meaning of the interaction term "We assume standard Lotka–Volterra interactions between the pest population and its natural enemy, which means that the pest death rate increases linearly with the density of the natural enemy, and conversely the growth rate of the natural enemy increases linearly with the pest population density". This is close to your explanation except that we refer to pest densities rather than biomass (lines 178-181).
- Indeed, we assumed that pest death rate is solely governed by predation and pest management. In contrast, predator death is also governed by intrinsic death in cultivated areas (Table 3). We now added this in the text (lines 190-199 and 230-231).
- A very similar mathematical formulation was used by Martinet and Roques. very recently (Martinet and Roques 2022). The similarity stems from the fact that this

model was developed within the same project. Questions asked, however, are very different. We added this reference in the text (line 158). Ref: Martinet V, Roques L. 2022. An ecological-economic model of land-use decisions,agricultural production and biocontrol. R. Soc.Open Sci. 9: 220169. https://doi.org/10.1098/rsos.220169

line 200-202: how could a pesticide having the same impact on pest species in conventional and OF fields have less or even no impact on predator in OF compared to conventional fields? These two scenarios look like wishful thinking for me. Could you maybe back this up with empirical data showing that pesticides in OF fields have impacts on pests but not on predators?

We agree that the four types of organic farming are somewhat theoretical and were considered to represent a diversity of situations. This is added at the beginning of the paragraph (lines 206-208). Nevertheless, there exists in real life a diversity of organic farming systems that differ in intensity and specificity. We now added a reference to the literature to justify that we consider a diversity of organic farming systems (line 207). Please note that when we consider that pest management is as efficient in organic and conventional farming systems, we do not imply that the same pesticide is used in these two systems. Further, we now changed 'pesticides' to 'pest management' throughout the text. Indeed pest management can be based on other means than pesticides and also induce pest mortality (e.g. trapping). We now provide examples of very efficient pest management measures that can be used in organic systems (e.g. viruses or nets) and have more or less impacts on predators (lines 211-214) ". Examples of efficient and specific pesticides are microorganisms targeting pests such as viruses (Graillot et al., 2016) or other microorganisms (Deshayes et al., 2017). Other pest management measures such as pesticides (*e.g.* spinosad) or nets are efficient on pests but also affect some predators (Dib et al., 2010)."

Table 2: What is CF and BIO?

Sorry, CF stands for conventional farming, BIO is a typo (French word). It was changed in the table.

Figure 1: please use color-blind friendly color palette.

We now used colors from the *colorBlindGrey8* palette in R.

line 340-394: given that the aim of this manuscript is in studying predator-prey dynamics in the context of organic farming extension, I find this paragraph to be beyond the scope. Consider reducing it or removing it.

The whole manuscript is now better focused. In particular, this paragraph is now provided as supplementary information SM1. It was quickly summed up in lines 341-348.

line 406: this is interesting since my limited understanding of LV models is that they lead to a variety of dynamics from system collapse to stable periodicity. Please precise what you mean by stabilized (ie do you mean temporally constant densities?), at what spatio-temporal scale and provide some figures showing these stable dynamics.

We now added information about scale and reworded the term 'stabilized' to be more explicit: "the average landscape scale densities of pests and predators remained stable over time after approximately 15 years (Figure 3, scenario: REF)." (lines 322-324).

Figure 4, 7 and 8: These figures are hard to interprete, they have multiple panels, different colors and line types. I would simplify and re-organize these figures to make the main results more readily stand out. For instance the text associated with figure 8 focus on the interaction between fragmentation and dispersion rates, figure 8 should then have one of these two variables in the x-axis with coloring for the second, patch area is not mentionned in the text associated with this figure so this variable can be averaged over.

We modified figures 4, 7 and 8 and built them all on the same basis: 3 panels that depended on the variable of interest, 3 fragmentation levels on the x axes and 3 colours for the three expansion scenarios. We modified the text describing the results to match these new figures.

line 516: which quartiles are represented in Figure 5?

These are the first and third quartiles. This was added to the legend.

Figure 5:predator to prey ratio seems extremely large, in organic fields with many SNH there were more predator individuals than preys. How do you explain this? And do you think that such a situation can occur in empirical systems?

Average predator-to-prey ratios were above 1 only in organic fields, in situations where there is a large spill-over of predators from semi-natural habitats (50% SNH and large fragmentation). This is made possible by the fact that predators are generalists and their density may thus be large, even if pest density is low (e.g. Fig 3 fr=0.9, SNH=50%). This is now explained in lines 378-379: “Note that in landscapes with 50% SNH, predator densities in organic fields were sometimes larger than pest densities (figure 3). This was most prominent when fragmentation was high, an indication that it resulted from spillover of predators from semi-natural habitats.” We also now explain that the predators are generalists: “ Predators thus behave as generalist predators that feed on the pest prey, and on alternative prey in semi-natural habitats.” (lines 197-198).

line 519-521: convulated sentence, please re-formulate or split in two sentences.

We rephrased this sentence: “More interestingly, we observed a clear ranking of spatial expansion scenarios with IP>RD>GP for the predator to pest ratio in organic fields (Fig. 6). This ranking might be due to the larger increase of predator densities with the IP scenario (Fig. 3) and the somewhat larger pest densities in the GP scenario. “ (lines 425-428).

Figure 7: please use the same typology of pesticide effects as in the method section.

We now changed the typology throughout the text. The Figure was modified and moved to the supplementary material SM2.

line 569-570: the interactive effect of pest dispersal and fragmentation on pest densities appears marginal, the differences between fr=0.1 and fr=0.9 are low. These effects are also hard to see on figure 8, see my other comment.

We changed the figure following your earlier indications. Further results about dispersal were moved to supplementary material (SM3).

line 590: at the start of each sections I would write again the full names of the abreviations, readers do not always read through from start to end but jump around.

Thank you for the suggestion. Done. We also changed to full names when we thought it could be helpful.

line 601-602: Based on what results is this conclusion based?

Please see next answer

line 602: To what does the this refers to? To your results that IP provide most benefits for biological control or to the fact that organic farming extension mostly happens in patterns similar to your IP scenario? I would rephrase this sentence and the next to make it clear.

We modified these sentences as follows to disconnect the better performance of the IP scenario and the fact that it corresponds to the observed trend. The second sentence was furthermore moved later in the discussion in response to a comment by Julia Astegiano. “Our results indicate that, at the landscape scale, the IP (Isolated Plots converted first) scenario would provide the most benefits for conservation biological control in organic fields with little impact on pest densities in conventional fields.(lines 458-461) “ and “This best IP scenario is in accordance with the current trend of OF extension mostly happening in areas already rich in OF fields (Gabriel et al., 2009; Marton & Storm, 2021; Sánchez Herrera & Dimitri, 2019; Zollet & Maharjan, 2021)” (lines 545-547).

line 612: densities of what?

Densities of pests and predators. This is now explicit (line 462)

line 614: SNH can be dropped.

Done.

line 621: predator to pest ratio.

Done, line 469.

line 622: Aha, if the benefits that you mentionned at the beginning of the discussion section refers to the predator to pest ratio as a proxy for biological control then this should be stated also there.

This was added to the beginning of the discussion (line 455)

line 625: using random development as a baseline seems odd especially since you mentionned a few lines above that in real landscapes organic farming expansion rather follows a clustered pattern. So either use IP scenario as baseline (what would happen if OF expansion follows the current trend) or discuss why you think that using random development is better suited as a baseline for this exercice.

The objective here was to compare by simulation three scenarios for the extension of organic agriculture. Two deterministic conversion scenarios were used and the third one, which is random, was used as a baseline to evaluate the gain of our conversions compared to a random conversion. In our minds, this is a declination of the famous no free lunch theorem (NF) which can be summarized as: "Before posting the merits of a method we must therefore prove that it has significantly better results than a random method."

We now made this explicit: “The IP and GP scenarios are two possibly planned scenarios that we compared to the baseline RD scenario in terms of resulting pest densities and predator to pest ratio” (lines 282-283)

line 629: use throughout the same wording for the predator to pest ratio.

Done.

line 631-647: A complex paragraph again focusing on land-use development a bit beyond the focus of this manuscript. I would recommend to remove it.

This paragraph was rephrased and summed up on lines 488-494.

line 645-646: Do you mean that you assume to be able to predict how pest and predator dynamic will be in a given landscapes based on the results presented here? This sounds overconfident, especially given that your results showed interactive effects with landscape contexts.

Sorry, we did not mean that indeed. The sentence was removed, we also now indicate at that point that the smooth increase in pest and predator densities contrasts with results observed by Bianchi et al. for a pest-parasitoid model (lines 494-500).

line 664: What strategies?

This was rephrased to “scenario of organic farming expansion”. Please note that we now use the word ‘scenario’ rather than ‘strategy’ throughout the text. (line 501)

line 669-673: I don't get these sentences first you write that IP is best for predator to pest ratio and then you write that under this strategy it declines. Harmonize the two sentences.

In fact, the IP scenario was best for predator-to-pest ratio in organic fields. In conventional fields, it sometimes performed worse than other scenarios regarding the predator-to-pest ratio but performed better regarding pest density. We now somewhat rephrased this paragraph by insisting on our criteria for ranking scenarios (line 507-509: Here, because conventional fields relied on pesticides for pest control, and pest densities varied little in organic fields, CBC was a target mostly in organic fields while the main target for conventional fields was the density of pests.). We also changed abbreviations to full terms (OF->organic and CF->conventional) so that results would be easier to understand.

line 682-684: A very clear sentence, I would start the paragraph (line 669) with this one and develop from it.

During the revision process, the paragraphs were reorganized somewhat differently from your suggestion. The first paragraph you mention (formerly line 669) synthesizes results. We indicate from the beginning that “ the IP scenario performed better, by improving CBC in organic fields and doing so at the expense of lower CBC, but not higher pest densities, in conventional fields.” (lines 510-511). The second paragraph begins with the sentence you mention (adapted) and we discuss mechanisms: “The best performance of the IP scenario resulted from two distinct mechanisms: a predator spillover improving CBC in organic fields, and a combination of ‘chemical umbrella’ and lesser pest spillover in conventional fields.” (lines 525-527). We hope that is ok.

line 716: What is meant by intensive organic farming?

Intensive OF systems correspond to intensive pest management resulting in high pest mortality (formerly OF 1 and 2, now Int-Spe and Int-Gen). The different OF systems were renamed for clarity and the whole section was moved to supplementary material 2.

line 662-754: There is little comparison of the results presented here with already published work. Please reflect on how the results presented here complement, contradict or present new results compared ot past empirical and modelling work.

As detailed in our answer to your general comment above, we now referred more often to other modelling theoretical studies of spatial pest-predator models on crop mosaics in the discussion (e.g. lines 539-542, 574-578, 585-587). We particularly compared our results with those of Bianchi et al. that address the similar question of organic farming expansion (although for a pest-parasitoid system) and to those of Edwards et al. who question the grouping of fields for pest control. (lines 594-598 and 539-542).

line 755: In the conclusion I would use a term less cold than IP strategy, maybe replacing it by "clustered organic farming expansion".

We now write: “The scenario that consisted in setting the priority on isolated conventional fields for conversion to organic (IP)” (lines 616-617)

Online repository: The model code on the OSF repository would profit from a fully fledged readme explaining how to run the models and either describing the main features of the models or citing the relevant literature.

The zenodo repository is very large (1Gb), I would only put on the repository the post-processed datasets used to draw the figures, the raw data files of 500Mb are not needed there. The zenodo repository would also benefit from a readme file explaining what the code is doing.

Thank your for the suggestion. A readme file has been added to the new repository. Half of the data files were not strictly useful for the figures and have been deleted, which significantly lightens the repository. Files have also been updated to take into accounts the changes in the figures made to take into account the various comments that are addressed in this letter. The new repository version can be found at: https://doi.org/10.5281/zenodo.7576542

Reviewed by Julia Astegiano, 30 Sep 2022 19:11

Dear Sandrine Charles,

Many thanks for the invitation to review this manuscript. After reading carefully this work, I found it very interesting and I think it represents a great contribution to discussions about how to advance with sustainable agricultures. In general terms, the manuscript is very well introduced and methods are precisely described, but results and discussion sections need to be reorganised, with great emphasis on the main objectives of the article. Moreover, including explanatory figures describing the modeling process and scenarios evaluated will certainly allow the manuscript attain a wider public. I made

numerous comments and suggestions to the authors in the original manuscript. I attach the commented version and I hope that my review helps improving the manuscript.

Best,

Julia Astegiano

—- > Attached comments

Dear Julia Astegiano

Thank you for your general comment and for the time you spent reviewing the first version of the manuscript. We totally reorganized the result and discussion sections as you suggested by moving some of the result sections to supplementary material. We also included a figure explaining the simulation design and added many details about the ecological assumptions associated to the modelling. A general point that will be detailed in the answers below is that we focussed the manuscript on the comparison of the three scenarios of organic farming regarding pest density and predator to pest ratio (as a proxy for conservation biological control) in fields only. The evaluation of these scenarios in different landscape types (i.e. landscapes with different levels of proportions and fragmentation of semi-natural habitats) was mainly meant to evaluate if the ranking of scenarios changed in different landscapes. Although we are interested in ecological processes at play in these landscapes, we did not focus the manuscript on the specific role that semi-natural habitats play for pests and predators. We understand this could have been another way to investigate the simulation outputs, but we think this would have been a totally different manuscript. This is particularly true since we moved the results and discussion concerning pest and predator dispersal to the supplementary material SM3. We nevertheless tried to relate our results to ecological processes as you will see in our detailed answers to your comments.

Title: may be you should use spatial distribution instead of deployment?

Thank you for your suggestion. We changed the title to ‘Best organic farming expansion scenarios for pest control: a modeling approach’. We preferred the term ‘expansion scenario’ to refer to the action undertaken instead of ‘spatial distribution’, which is the result (and less active) of the expansion scenario. Furthermore, as explained above we now focus the manuscript on the comparison of the three expansion scenarios.

Abstract

We rewrote the abstract section almost entirely.

L.2 What do mean with “its share of agricultural landscapes”? Is not clear for me.

We reformulated, as follows, to be more clear: “The area under OF is expected to further increase in the future” (line 3).

L. 16 What do mean by “landscape context”? Please, try to be a bit more specific while describing your study.

This means interactions with the surrounding landscape (i.e. in this general sentence both the crop mosaic and semi-natural habitats). We reformulated it as ".... conservation biological control, which depends on the surrounding landscape (i.e. both the crop mosaic and semi-natural habitats)" (line 5).

L. 18 One species of pest, one of predator? Please, be more specific while describing your model.

Thank you. We reformulated to be more specific as "...we modeled the effect of spatial changes in farming practices on population dynamics of a pest and its predator" (lines 7-8).

L.19 Maybe you should use "spatial distribution" instead of deployment?

We changed it into 'progressive conversion of conventional fields' (lines 9-10) as we are dealing with 'OF expansion' (line 7).

L. 21 I suggest you be more specific here as fragmentation can be measured in many different ways.

The description of the calculation of fragmentation was completed in the M&M 'Overview' section. We further insist that it concerns the fragmentation of the semi-natural habitat: "The three OF expansion scenarios were applied to nine landscape contexts differing in their proportion and fragmentation of semi-natural habitat" (lines 13-15).

L 23. what do you mean by different initializations of pop dynamics? Species combination of abundances? Please, be more specific.

This means "different initializations of population dynamics, i.e. species abundances". We did not refer to the initialization conditions in the revised abstract because the results were robust whatever the initialization conditions and because this point is not very important and out of the main objectives of the paper.

L. 23-24 various combinations of pesticides effects… different management scenarios? This last part of the sentence is not clear for me General: It seems to me that a good description of your response variables is lacking (how did you measure CBC?).

We totally revised this part of the abstract. We currently precise the three scenarios of conversion of conventional fields into organic fields (lines 10-12) and how the ranking of the three scenarios was robust to variations of pest management practices in organic fields (line 15). We also specified that results were based on pest densities and predator to pest ratios (line 20).

L. 26 First you used landscape context, here you use landscape types. It is hard to get what you mean.

We referred to 'combinations of landscape contexts and OF expansion scenarios' instead of 'combination of landscape type' in the revised version (lines 17-18).

L 26. "Its" -> I suggest you repeat OF as you have many different elements in your model and is easy to confound them. In this case I do not understand which impact you refer.

Done (line 18)

L. 28 What do you mean? Landscapes with more fragments but similar total forest cover?

We rephrased to: "with large proportions of highly fragmented semi-natural habitats" (lines 19-20). We added a figure in the M&M to explain the combination of parameter values in the landscape model. In total, we simulated 9 contrasted landscape contexts (line 14), i.e. for the combination of 3 fragmentation values and 3 proportions of semi-natural habitats (see also answer to your other comment in the M&M section, lines 155-158) .

L. 32. Here you mention quantity of semi-natural habitats but before you used other forms to refer to landscape effects. I suggest being more consistent with the terms you use

Thank you, we now use 'proportions of semi-natural habitats' instead of 'quantities' (line 24 and elsewhere in the text)

Introduction I really liked your introduction; it's clear enough and provides information on all topics you develop in your study.

Thank you.

L.44 Many thanks for mentioning impacts of industrial agriculture on human and nonhuman health in your introduction. This helps constructing a different narrative based on other values different from the false and hegemonic simplified view behind the sentence "we need to increase food production" (which in general means "commodities production").

Thank you for your positive comment. We slightly modified the text to highlight the multi-performance of organic farming: 'The multi-performances of OF recently received much attention, in terms of yield (Knapp et van der Heijden 2018; Le Gal et al. 2020), of effects on biodiversity (Caprio et al. 2015; Lourenço et al. 2021; Smithet al. 2020), of nutritional value and of global positive impact on human health (Gomiero 2018; Salomé et al. 2021).' (lines 40-43)

Furthermore, we clarified the issue of pest control for organic farming notably based on conservation biological control in agreement with our modeling approach: 'Pest management in OF relies on specific cultural practices and on a restricted number of non-synthetic pesticides. Conservation Biological Control (CBC) methods that enhance natural enemy abundance and activity to reduce pest populations (Heimpel et Mills 2017), are of particular interest for OF to reduce pest populations (Heimpel et Mills 2017; Holland et al. 2016) are of particular interest for OF.' (lines 44-47)

L.68 I do not understand what do you mean with "its share".

We reformulated "the area under OF" and removed "share" (line 54).

L. 90 This is an uncommon word… May be you should use maximum or great?

Change done to ‘maximum’ (line 73). Thank you.

Line 140 “fragmentation” measured as… I suggest you clarify how you measured fragmentation.

The description of the calculation of fragmentation was completed in the M&M ‘Overview’ section. We further insist in the introduction section: “We further investigated if the ranking of scenarios was robust to pest control methods in OF fields and pest and predator dispersal abilities” (lines 117-118).

Lines 139-142. Ok, you are exploring interactions among management practices at different spatial scales (local and landscape). This is not clear in your abstract, at least not as clear as here.

We revised the abstract in order to be as clear as possible.

Lines 155-158. Ok, you are measuring fragmentation as an effect of isolation, it is important to mention that in your abstract and maybe focus your literature review cited in the introduction on isolation effects (which are different from area effects)

We now added that our measure of SNH fragmentation is a measure of fragmentation *per se* and thus not related to the quantity of SNH habitat at the landscape scale. This quantity is controlled by %SNH. The level of fragmentation strongly impacts the mean area of SNH habitat patches as now added lines 153-154 and shown in figures 2 and S1.1 (supplementary materials). We thus would find it misleading to imply that it is only an isolation effect. As you indicate later, *fr* also affects the edge length between SNH and cultivated patches. This was also added in line 154 (and see figure S1.2 the new supplementary material SM1 ‘Effects of semi-natural habitat fragmentation and OF expansion scenarios on landscape structure’, which refer). We also referred to Fahrig et al. 2003 which distinguishes fragmentation per se from habitat loss.

General comment on section 1.2.

Even if the model is described elsewhere, I suggest you construct a figure to easily explain the “steps” and elements of the model. This would facilitate your dialogue with people working with OF that do not use mathematical modelling.

Also considering this point, maybe you can add some biological interpretation to model description (I made some comments below)

We added a figure explaining the simulation design to make the focus of the study clear to the reader. We did not add a figure to describe the population dynamic model but added a significant amount of text to better explain the biology behind the model (material and methods, paragraphs 2.2.1 and 2.2.2) lines 155-200. We hope that this makes the model understandable to non-modelers.

.Lines 139-142. Ok, you are exploring interactions among management practices at different spatial scales (local and landscape). This is not clear in your abstract, at least not as clear as here.

We totally modified the abstract and hope the aim of the study is now clear.

Lines 155-158. Ok, you are measuring fragmentation as an effect of isolation, it is important to mention that in your abstract and maybe focus your literature review cited in the introduction on isolation effects (which are different from area effects)

Please see our answer to your previous comment, just above, for these same lines.

L62 Check the double space “the pest population” Equations: I do not understand how predators only reproduce in SNH… what is the biological explanation for this decision? Indeed, it is not clear how these general equations describe dynamics in different land uses if the pest and the predator are specialized.

First, we apologize, indeed predators may reproduce in crops if the pest is present, this was a mistake in the text.

We added many biological explanations in the text to explain equation 1: The general description of the model was largely augmented and we added information about ecological processes below the terms of the equations. The following text was in particular added (lines 158-165):

‘We modeled the spatio-temporal dynamics of a pest and a generalist predator species interacting over the lattice generated by the landscape model according to Martinet and Roques (2022). The model describes the density of the predator population $P_t(x)$ and of the pest population $N_t(x)$ at each position $x = (i, j)$ over the grid and at each time step t (equations 1). The variation over time (indicated with sign ‘) of pest ($N'_t(x)$) and predator ($P'_t(x)$) densities at each position depends on their dispersal in and out of this position, their intrinsic growth (i.e. population growth in absence of pesticides and of interactions between pests and predators), mortality due to pest management, and mortality (for the pest) or growth (for the predator) due to predation’.

Furthermore we now specify clearly that the predator is a generalist (line 173-174, 197-198) and totally changed the section describing the timing of ecological processes (lines 191-200). .

L 191. This means that you are working with a pest that is specific of a given crop. This is a very important point of your work and I suggest you mention it in different parts of your manuscript. I mean, model dynamics may apply only for specialized predator-prey interactions, This is not a minor point in your work and is key to understand your results on the expansion of OF

L. 193-94 Are they generalists? How predators diet is considered in your model? Clarify this point, please.

We hope that the new description of the model answers these two comments. Indeed it was a mistake to say that the predator reproduced only in semi-natural habitats. We meant ‘in the absence of the pest’. The new description is as follows: “ Pests are specialized on the crop and their phenology matches that of the crop. The first half of the year schematically represents the season where the crop is absent, pests do not reproduce

and there are no pest management practices. Pest densities only depend on their dispersal and predation by predators. During the second half of the year, when the crop is present, pests furthermore reproduce and are affected by pest management practices in the crop. The predators, in contrast, reproduce all year long in semi-natural habitats (loosely mimicking taxa that include both spring and autumn reproduction) and suffer from intrinsic mortality in crops. Their density increases further in both semi-natural habitats and crops when the pest is present. Predators thus behave as generalist predators that feed on the pest prey, and on alternative prey in semi-natural habitats. " (lines 191-200).

I suggest you complement your tables with a figure in which you explain the different modelling scenarios you implemented, emphasizing the two different spatial scales of management that you are evaluating Section "Initial conditions". All this information needs to be summarized in a figure, which certainly will facilitate the understanding of your model and modelling scenarios, and therefore your results

We now added a new figure (Figure 1) as suggested.

L. 244 I suggest you use isolation directly

We preferred to leave 'fragmentation' (please see answers to comments about *fr* being fragmentation *per se* and the relations between fragmentation and patch size and the number of patches in the supplementary materials section 1)

Results

General comment: even I consider your results are very interesting, I think that you need to make a choice about what to share in the main text. For instance, point 1 is not part of your main objective (evaluating CBC at different management scenarios). I suggest more descriptive and exploratory results, that are interesting themselves, should be part of a supplementary material and here you should concentrate on the main results of your model following your specific objectives. I suggest you start your results with section 2, which gives the "control" scenario to make comparisons with the other scenarios. That is why I suggest you a figure explaining the modeling process and the scenarios evaluated.

Following your suggestion we now start our results with section 2 and we moved all description of landscape changes during organic farming expansion to supplementary material SM1. We also focussed the results and discussion on the comparison of organic farming expansion scenarios. We thus moved former sections 5 and 6 of the results to SM2 and SM3.

L. 339 on the dynamics of the number and area of…

This section was moved to supplementary material 1 and the title was changed to include description of edge length, patch number and patch area.

Section 1. I suggest you summarize in a sentence this first description on landscapes dynamics following different ways of increasing OF patches and send it to a SM, to facilitate readers comprehension of your results. I think it is interesting to read such

description to get some elements to discuss your results. Figures Section 1. I suggest you move these figures to a supplementary material

We summed-up the description of landscape dynamics (lines 341-348) and moved the initial text of part 1 and figure 1 to supplementary material SM1. We now refer to this supplementary material when necessary to explain the results.

Line 404. In which scenarios? Initial ones?

This was in absence of organic farming expansion, i.e. in the 9 types of landscapes with the initial proportions of organic farming. This is now explicit (line 323).

Lines 411-417. Maybe I did not understand your biological decision about the model, but as described in methods, this is about specialized predators that only reproduce in SNH which food is only reproduced in crops. Can you give some example of such biological system? I am not sure these results make strong biological sense if the predator is not a generalist one. I mean, according to your model you have a pest specialized in a crop and predators specialized in that pest, but predators only reproduce in SNH, i.e. they use the landscape in a complementary way.

As explained above, the former model description was misleading (sorry) and the model was specified for a generalist predator growing in crop habitats on the pest and in SNH.

Line 420. I suggest you refer to isolation, as is the factor of fragmentation that you are managing with your model parameters.
Line 422. This means that you have an habitat loss effect, not just an effect of isolation.

Indeed, fragmentation is not simply about isolation. We hope that this is now clear thanks to figures S1.1 and S1.2. At the landscape scale it is also not an habitat loss effect since the proportion of SNH is fixed. It is really more an effect of the area of SNH patches, and thus of their edge length with cultivated patches (lines 153-154).

This is interesting! Section 3 describes your main results. I suggest you merge sections 2 and 3, as section 2 describes the “control” landscape that allows understanding the effects of landscape management

The two sections were merged within a new section 2, as suggested.

L 432. Maybe measuring mean edge density in your landscapes can be very informative. Does the model allow you to measure MED?

Thank you, we calculated the edge density (ED) metric and assessed its relation with the fragmentation of semi-natural habitat. For the three land cover types, the number of patches increases and their mean area decreases when the fragmentation of semi-natural habitat increases. We added this information in the supplementary material SM1 and added this information in the text (lines 153-154).

Section “Pest dynamics”. General comment on the significance of results on pest dynamics: these results are very interesting. They mean that pest densities are responding more to habitat loss than to habitat fragmentation and that at least with the percentages of SNH explored you do not show the existence of a threshold, which is commonly reported in fragmentation studies.

Thank you. It would indeed be an interesting result to discuss, although we think it lies beyond the scope of this paper. It is especially true since we modified several of the figures to take out the patch size, following the recommendations of two reviewers. Figure 4 still shows coarse levels of SNH (10, 25 and 50%). Looking at the 'conventional farming' part of this figure 4, one could actually argue that pest densities increase more strongly with SNH% when fragmentation is high, which would be consistent with Andrèn's fragmentation threshold hypothesis. However, this could also be a simple mean/variance relationship. Moreover, the 'organic farming' part of the figure doesn't show any such effect. All in all, we don't believe there is enough material to support one hypothesis or the other.

How such results can be affected by the fact that you incorporate some kind of the landscape heterogeneity perspective?

Figure 4 sums-up the effect of the heterogeneity introduced in the landscape model (3 values of fragmentation and 3 proportions of SNH) for the each scenario of organic farming expansion.

Lines 451-453. This is interesting indeed. Can we say that you have an interaction between habitat loss and fragmentation, in the sense that the way you increased OF (in a more fragmented way) has effects depending on the % of SNH.

We discarded the sentence in revised version in order to focus the paper on the effects of landscape heterogeneity (SNH proportions x SNH fragmentation) on the pest dynamics/densities (lines 394-405)

Lines 455-456. Here your results suggest a threshold associated to the concept of habitat loss, interesting!

Figure 3 shows that the predator dynamics differ between the scenarios of organic expansion in the organic fields with more predators for the IP scenario. Predator densities globally increase with the increase of the proportion and the fragmentation of the semi-natural habitats. Differences between the scenario of OF expansion are less obvious for the highest proportion and highest level of fragmentation of semi-natural habitats, but it remains difficult to identify a threshold. Furthermore we decided to move as supplementary material explanation about the number and the size of SNH patches

We just rewrote the sentence as follow: "In contrast, the three scenarios performed similarly in landscapes with high proportion and high fragmented semi-natural habitat" (line 370)

Lines 464-466. This means that conventional fields are benefited by surrounding OF. What can you say about the other direction? Is OF benefited by conventional farming?

In our opinion, conventional field benefit from high proportion of SNH as already mentioned (line 283)

Line 477. I suspect that in fact is more to the contact among pest and predators sources, that is why I suggest you measuring mean edge density

We moved this part of the discussion to the supplementary material section SM1 with measures of edge lengths as you suggested (see above our answer to comment for line 432).

Lines 484-487. I found this dynamic very interesting. How can we think about local management in OF allowing some pest increase to maintain predators densities, instead of having large conventional farms providing predators food? (considering differences in human health that the different agricultural practices imply).

We moved this part of the discussion to the supplementary material section SM2.

We added the following paragraph in SM2: "As expected, the effect of organic farming intensity and specificity was much less pronounced in conventional fields. The effect of specificity was very weak. The effect of OF intensity was observable mainly in landscapes that were characterized by a low fragmentation (figure S2)." We would prefer not to speculate about extensive OF management allowing to sustain predators in conventional fields, indeed we observed an effect of pest management specificity indicative of such a process but the higher pest densities in conventional fields when OF is intensive and the landscape is fragmented also evoke strong spill over of pests from organic to conventional fields.

Furthermore we wished to keep the focus on the comparison of OF expansion scenarios and also added these sentences in SM2: "Interestingly, the response of pest density to expansion scenarios showed the same pattern whatever the OF type. It was very similar whatever the expansion scenarios in organic fields and pest densities were generally lower for the IP scenario in conventional fields"

Figure 5. This is the main figure and result of your article. I understand the importance of describing pest and predator dynamics first; however, I suggest you summarize your main results in 4-5 paragraph in one result section and then add a more detailed description of those results in a supplementary material to facilitate the understanding of your whole modelling exercise to stakeholders. I do think all your results are interesting, but I feel that in the way you communicate them is too detailed and not that easy to be followed by a more general public. But it is just a suggestion.

We followed your suggestion and we totally restructured the result section. We deleted the first paragraph about the effect of OF expansion on the number of organic and conventional patches (partially move in supplementary material SM1). Reported results mainly focus on the pest and predator densities at the end of the expansion scenarios (t=50). We conserved figure 5 (with the same name) adding pictures describing the landscape type to figure out the effects of the combination the proportion and fragmentation of the SNH on predator to pest ratio.

On the other hand, this figure can be improved by adding some designs that facilitate the lecture of axes (fragmentation in fact is a measure of SNH isolation in your case; and the proportion of SNH a measure of habitat loss; maybe you can add some representative designs of this?).

We added examples of landscapes on figure 5 illustrating the type of habitats that correspond to the nine landscape types (3 levels of % SNH x 3 fragmentation levels).

Lines 525-527. These results are very interesting, because the CBC seems to respond to an interaction among isolation and habitat loss, differently if we only measure pests or predators. I suggest you find a way to organize your result section to highlight these results

We find it difficult to discuss habitat loss because the predator can reproduce in all three types of habitats and the pest can reproduce in both conventional and organic fields, although organic fields are obviously a better habitat. Comparing landscape types, higher proportions of semi-natural habitats may thus loosely be considered as habitat gain for the predator but habitat loss for the pest. The figure was furthermore changed as an answer to reviewer 1 so that we quite modified the description of results.

Nevertheless, we really think that the result formerly described on lines 525-527, i.e. “As a consequence, differences between the GP and IP scenarios were largest in landscapes with few and little fragmented semi-natural habitat and very small in landscapes with a large proportion of highly fragmented semi-natural habitat.” is due to the fact that the IP and GP scenarios mainly differ in the fact that the conventional fields are grouped with the IP scenario and not with the GP scenario. When the landscape is fragmented, groups of conventional fields are small and landscapes are largely similar for the two scenarios. We now rephrased as follows: “Consistent with the larger differences in crop patch area in landscapes with few and little fragmented semi-natural habitat, differences between the GP and IP scenarios were largest in such landscapes and very small in landscapes with a large proportion of highly fragmented semi-natural habitats.” (lines 437-439).

General comment on section 6. These results are interesting, but support your main results. I suggest you summarize them in one or two sentences and move the description to supplementary material

We agree and we moved this section to supplementary material SM3. We also changed the format of most result figures so that they will be easier to read (comment from reviewer 1 Lionel Hertzog).

Lines 569-570. This is why I suggest that it is more an effect of the level of contact among the three different land uses than of SNH isolation and suggest measuring the landscape mean edge density.

We now measured edge length. Please see above. This section was moved to supplementary material SM3.

Line 596-597. I suggest “pest and predator abundances dynamics”

Done thank you.(line 452)

Lines 598-606. Following your main objectives, this is an important but secondary result. Discussing it at this point in your discussion section deviates the focus of your study.

This sentence was moved later in the discussion (lines 540-542)

Please, reorganize your discussion by following your main objectives and then add secondary results that may strengthen that discussion.

As tentatively explained above, we followed your suggestion by focussing the manuscript on the comparison among OF expansion scenarios in different landscape types and assessing the robustness of their ranking to types of OF farming (intensity, specificity - table 3) and dispersal.

Landscape complexity is a too wide term and means so many different things that I suggest you use more specific concepts like patch isolation or amount of SNH (the parameters you explored).

We changed the title to 'landscape type and OF…' since we clearly defined earlier the nine landscape types used in our study. We did not use the 'word' complexity to describe our results later on.

Line 614. You do not need to cite figures that were already showed in your results section in your discussion

We removed references to Figures in the discussion section.

Lines 619-620. This sentence is rare, because in fact deployment strategies are part of landscape characteristics, are part of the construction of that landscape. Moreover, from your results I see a predominance of habitat loss effects (in the sense that dynamics respond more to increases in SNH).

Yes indeed, we modified "landscape characteristics" to " landscape types" to avoid confusion. Looking at new figure 4, you will see that dynamics really respond both to the proportion and the fragmentation of semi-natural habitats (and their consequences for the structure of the two habitat types).

Lines 619-624 and 625-630. In this sense, I wonder how considering coevolutionary dynamics may affect these results. I mean, if conventional fields led to pest resistance and OF drives to more diversified predator-prey coevolutionary dynamics

This is a question that we also have in mind. We indeed wonder how pests and predators may adapt to organic farming, and also more generally to agroecology. We suspect that selection pressures may be less than those from insecticides. We chose, however, not to discuss this question which seems somewhat outside the scope of this study.

Line 649. For me this is the main discussion of your article. I suggest you give priority to these results and discussion

We moved the sections about the changes in landscape structure and the effects of organic farming systems and dispersal to the supplementary materials SM1 to SM3 to better focus the paper. Still we tried to focus the manuscript on the comparison of the three OF expansion scenarios (using pest density and predator to pest ratios as criteria). Therefore we focussed on comparing ecological processes for the three expansion scenarios and assessed how they were impacted by the proportion and fragmentation of

SNH. Nevertheless, we agree that this is an interesting result from an ecological point of view. The discussion section was largely modified and we now wrote a paragraph about the impact of SNH proportion and fragmentation on dynamics (lines 473-487).

Lines 654, 658-661. I agree with your discussion and that is why I suggest you measure mean edge density, to have a measure of contact among different land uses and landscape configurational complexity that may be interacting with the % of SNH

Yes, thank you, it was indeed interesting to calculate edge density (now line 153-154) and supplementary material SM1).

General comment on section 2.1. I understand your results are exciting but I feel that you concentrate too much on it and do not discuss with other literature. Can you add some discussion in this sense?

We added some discussion to a study looking at field grouping impacts on pest and predator dynamics in annual crops. Similar mechanisms likely explain that they observe lower pest densities in grouped fields and we observe lower pest densities in grouped conventional fields (lines 539-543). Most references to other studies were added in a new section dealing with the benefits and limitations of our approach (lines 575-615)

General comment on section 2.3. Please, add discussion with previous work on the subject.

section 2.3 was moved to supplementary material following suggestions by Sandrine Charles and Lionel Hertzog. We thus rather shortened it and only used these results to assess the robustness of the scenario ranking regarding pest management in OF systems (lines 558-569).

General comment: please, include discussion with the literature. Moreover, I think it would be interesting to include the metacommunity perspective here to discuss the importance of species dispersal and its interaction with landscape configuration .

We are not sure to which section this comment is related.

We now referred more to other modelling theoretical studies of spatial pest-predator models on crop mosaics in the discussion (e.g. lines 539-542, 574-578, 585-587). We also added a reference to a review of pest-predator modelling studies (Alexandridis et al., 2021) and used it to detail the limitations of our approach. To our knowledge, only Bianchi et al. specifically addressed the question of OF expansion for pest-predator interactions and their results are now better compared to ours (lines-594-600). Edwards et al. (2018) addressing pest densities for different levels of grouping of annual crops found that grouping was better for dispersal-limited pests, by mechanisms that are similar to those promoting the IP scenario. This is now discussed also (lines 539-542).

Since we only modelled one pest and one predator we did not refer to the metacommunity litterature. However, we acknowledge interactions between dispersal and landscape structure : “Further, while we assumed similar dispersal abilities for the pest and the predator, real species may have different dispersal abilities and thus perceive the landscape at a different grain (Jackson & Fahrig, 2012). Differences among

scenarios would, for example, obviously be reduced for long-distance dispersers that would be less affected by landscape structure." (lines 586-588).

General comment on section 2.2. Same general comment as in the previous section. Moreover, how your results discuss with recent discussions about landscape effects in the context of European agrienvironmental schemes?

We added the following sentence in the conclusion section in agreement with your comment: "Furthermore, landscape contexts with large proportions and fragmentation of semi-natural habitats supported the highest level of CBC" (line 623-625).

Reviewed by Sylvain Bart, 14 Sep 2022 09:29

This manuscript presents a valuable and innovative approach to explore best organic farming deployment scenarios for pest control. It helps to understand how spatial strategies of organic farming deployment impact conservation biological control. The manuscript is well written and organised, methods are clearly explained and results well-presented and discussed. They are a lot of results but the result and discussion section are well organised to account for it.

Thank you very much for the time spent reading our manuscript and for your positive comments. You will see that following your comment about the numerous results and the other reviewers' comments, we largely refocused the manuscript. In particular we moved results about the types of organic farming (formerly section 5) and about dispersal (formerly section 6) to supplementary material.

I miss a paragraph (will probably fit at the end) about the limitation of the modelling approach, and the weight of the choices made in the construction and hypothesis formulated in the model.

We now added such a section at the end of the discussion (lines 573-613).

line 193-195: This is an important choice that may affect the output of the model, what is the rational for this choice? I would suggest authors to add more explanation regarding this choice. Is it base on actual predator life history traits? My first thought was predator do not reproduce during the winter because they don't have resources to do so.

In the model we only considered two seasons and time is continuous, we could thus not match closely real life cycles. The predator that we had in mind would be for example a generic predatory ground beetle. Among species, for example, some reproduce in spring, while others reproduce in autumn after harvest, some even have two breeding periods (e.g. Traugott (1998) European Journal of Soil Biology, 34,189-197)

Our assumptions in the model are now better described (paragraph: timing of ecological processes, lines 188-200). In particular we clearly indicate that " The predators, in contrast, reproduce all year long in semi-natural habitats (loosely mimicking taxa that include both spring and autumn reproduction) and suffer from intrinsic mortality in crops. Their density increases further in both semi-natural habitats and crops when the pest is

present. Predators thus behave as generalist predators that feed on the pest prey, and on alternative prey in semi-natural habitats".

Some of the modeling choices are now discussed in a new section of the discussion "3-limits and benefits of the modelling approach" (lines 573-615). We compare in particular our results to those of Bianchi et al who modelled a pest-parasitoid system.

The conclusion regarding this modelling approach is interesting but caution should be taken because it is not based on actual life history traits of a prey-predator couple. A nice perspective would be to run the model with observed data regarding the life history traits of a prey and a predator (precise season of activity, etc…). A discussion on that matter would longer the discussion part, which is already a bit long, but I think the other parts might be shortened

The discussion was largely shortened by removing the discussion of results concerning the type of organic farming system and the dispersal of pests and predators. We summed-up their conclusion in a paragraph concerning the robustness of our results (lines 559-573). As stated in the previous answer, we also added a large section devoted to the limitations and benefits of our approach. We explain in particular some changes that may be expected in the results for other types of pests and predators (lines 573-615). We also end the conclusion by acknowledging the need to design and calibrate models with real-world data (lines 625-627).