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July 31, 2023

Ignasi Bartomeus, recommender
PCI Ecology
Dear. Dr. Bartomeus,

Enclosed is the revision of my manuscript with Gabriela Gleiser, Thomas Kitzberger, and Rubén Milla titled “Being a tree crop increases the odds of experiencing yield declines irrespective of pollinator dependence” (MS #537), which we submitted for publication in the PCI journal. We are pleased with the positive response of both reviewers to our work and their appraisal of the conceptual and practical implications of our study in the context of ongoing human-driven, global changes. We are also grateful for their and your helpful comments, which stimulated improvements in the contents and presentation of our manuscript.

In revising the MS, we have considered all comments and suggestions to the detail and provide the following responses:

Recommender, Ignasi Bartomeus

1. “This is a very nice and compelling paper challenging the mechanisms behind previously reported trends showing that pollinator-dependent crops are declining in yield growth faster than non-dependent crops. Exploring confounding variables such as growth form is a clever and needed addition. As pointed out by Reviewer 1, global analyses are powerful, but data management is complex, and the devil is in the details. Hence, both reviewers make minor but fair questions about the choices taken to manipulate the original data. I think backing up some of their concerns with additional supporting analysis (when data quality and quantity permits) will ensure the results are robust.” – We thank you and the reviewers for your constructive and thoughtful comments and suggestions. We hope that we succeeded in addressing all the comments raised by you and the reviewers (see our responses to each of the comments below).
2. “In particular, I am concerned with the interpretation of growth rate. In the text is mentioned that “a negative growth rate can be taken as evidence of long-term yield decline”. In parts of the text is unclear what "yield decline" refers to.” – For the sake of clarity, we reworded our working definition of “yield decline” in the Abstract (line 27) and provide a clarification of the concept early in the Introduction (lines 59-60). In addition, we checked that “yield decline” conveys the intended meaning throughout the MS and clarified it when needed.

3. “My understanding is that behind these trends, we can find changes in the area occupied by the crop, changes in management practices, and changes in production per hectare. I understand that this last mechanism (changes in production per area) is the most tightly linked to pollinator declines or climate change effects, which is the focus of the introduction/discussion. Is there any elegant way to get closer to analyzing trends in yield per cultivated area (i.e. is area cultivated per year and crop available)? Alternatively, I think this should be noted early in the introduction to avoid any misinterpretation of the results.” – We apologize if this point was unclear in our previous version, but note that the effect of total cultivated area per crop (and per country) is accounted for in our analyses (lines 213-218), and that the results of these effects are reported (lines 319-320, Fig. S5) and discussed (lines 423-427). Although area effects are not presented at length in the Introduction, we now clarify in the Introduction that we assessed the effects of pollinator dependence and growth after accounting for any confounding effect associated with total cultivated area per crop (lines 140-143).

4. “Also, Fig 1 is intriguing, as it seems the extreme years are highly influential. Do you think starting in 1962 or cutting off in 2019 would change the results?” – Following this suggestion, we reanalyzed the data leaving out the years 1961 and 2020 and compared the results of the analyses based on the complete vs. curtailed time series. This reinforced the robustness of our results and conclusions as the strength of the effect of pollinator dependence changed very little in model GLMM_1a (X^2=6.71, df=2, P=0.035, vs. X^2=5.55, df=2, P=0.062 for the complete and curtailed time series, respectively); and likewise with the strength of the effect of growth form analyzed in model GLMM_1b (X^2=15.95, df=2, P=0.00034, vs. X^2=16.08, df=2, P=0.00032). Therefore, the conclusions derived from model GLMM_2 remained unchanged based on the results for both factors, pollinator dependence (X^2=2.08, df=2, P=0.35, and X^2=1.57, df=2, P=0.46) and growth form (X^2=11.93, df=2, P=0.0026, and X^2=11.85, df=2, P=0.0027). These statistical results are now reported in Table S3 and briefly commented in the text (lines 305-308). In addition, in exploring the robustness of our results, we analyzed time series in slices as short as 30 years. In all instances, we concluded that yield decline was more strongly associated with growth form than with pollinator dependence and that any relation between pollinator dependence and yield decline was an indirect consequence of the association between this variable and growth form. Therefore, we are confident about the robustness of our results and conclusions (see also our response to comment 6 by Nicolas Deguines).

5. “I also have a suggestion to reinforce the main analysis. I think it will be illustrative to add a variance partitioning plot. This can be done using a Venn diagram, where you calculate the proportion of variance explained by the model with only one variable (model 1a), the proportion of variance explained by the other one (model 1b), and the joint explained variance (model 2). I might miss some technical details here, but I think exploring this would help the story.” – Thank you for this thoughtful suggestion. Visualizing variance partitioning is key to the main message of this contribution and it was missing in the previous version of the manuscript. By comparing models, we estimated the proportion of the among-crop variance that can be attributed to pollinator dependence and growth form, resulting in values of 5.6 and 12.9%, respectively, when analyzing them separately (lines 301-303). We limited this variance partitioning estimation to the two focal factors to not lose focus on the main objectives of our study. We provide a visualization of variance partitioning by means of a stacked bar figure (new Fig. 5), which we think provides a more intuitive

visualization of variance partitioning in our case than Venn diagrams. This figure shows in just one graphic and comprehensibly the fraction of all the among-crop variation variance explained by each of these two factors in model 1a and model 1b, respectively, and the independent and shared variance in model 2.

6. "Lastly, I miss some discussions on recent advances in plant trait correlations and trade-offs, where the growth form and pollinator dependence are discussed along with other traits. e.g. Lanuza et al. 2023 (10.1111/1365-2435.14340), Friedman, 2020 (https:// doi.org/10.1146/annurev-ecolsys-110218-024638); Paterno et al., 2020 (https://doi.org/10.1073/pnas.19106 31117); Roddy et al., 2021 (https:// doi.org/10.1111/nph.16823)." – Thanks for pointing out to this relevant omission. We now include a paragraph in the Discussion on plant trait correlations (lines 342-357.

7. "I think the changes proposed by reviewers and myself should be viewed as a robustness check, but they are mostly minor concerns. I am looking forward to seeing the final version of this stimulating paper." – We hope that we have been successful in addressing all the concerns raised by you and the reviewers.

Reviewer # 1

1. "In this manuscript, Aizen et al., use a big dataset with temporal series in crop yields to study how yields changed in the last decades, aiming to examine if crop pollinator dependence and crop type (tree, bush or herb) can predict the probability of the sign of this trend (i.e., decrease or increase). I find that the study is interesting, it is framed and presented in a very clever manner, and nicely written. By the type of analyses conducted and the complexity of the issue (i.e., many possible confounding factors understandably not considered due to the lack of data at this temporal and spatial scale), I would take the results with caution. Still, I think that the study represents a nice try to disentangle some of the potential drivers affecting trends in crop yields worldwide." – We thank the reviewer for her/his appraisal of our contribution.

##Abstract

2. "L. 27. Impressive numbers!" – We agree. We are fortunate to count on the open access FAO dataset, an extremely valuable resource.

3. "L 37. Why climate change? Could be due to a number of different reasons, such as plant diseases, more pest species or pest outbreaks, loss of natural enemies, loss of soil fertility, lack of water in recent years (linked or not to climate change), or just the lack of what you mention in your first introduction paragraph. In Africa there is a less mechanized and intensified type of agriculture, so some of the possible reasons might affect more to this region too. In the study, explanatory variables related to climate change are not tested, then why not finishing with something more related to the actual results such as: "These results suggest that pollinator decline is not the main reason behind tree crop productivity loss"." – We agree. However, as argued in-depth in the Discussion, direct or indirect related to climate change seem to provide plausible explanations for the observed association between yield decline and the tree growth form. In any event, we replaced the last sentence of the Abstract with a statement that includes the reviewer's suggestion and the possibility of alternative factors such as climate change (lines 38-39).

##Introduction##

4. “L 77. Remove “to any extent”. Some of these crops seem to have some benefits if they are cross pollinated by pollinators (e.g., https://doi.org/10.17660/ActaHortic.2012.949.27).” –Removed.

5. “L130. “Because of”, or “due to”?” – Changed as “because the incidence of” (line 133).

6. “L 139-143. Good choice! Also, another important reason to calculate trends at the country level, is the influence of political aspects such as subsidies to implement watering systems or to increase mechanization. These can impact trends in yields strongly and depend on country. I would even hypothesize that country GDP (or PPP) might be related to long term trends in yield.” – Right, we agree. We now mention that factors causing spatial heterogeneity can be also political and economic (line 147).

7. “L 139-143. I know that yield is by definition the “production by area”, but I would define this somewhere just to make it clear from the beginning.” – Both yield and yield decline are now defined in the Abstract and early in the Introduction (line 19, lines 26-27, lines 49-50, and lines 59-60).

##Methods

8. “L161-163. Not sure this is the best, because you leave out the uncertainty about the trend (the error), which might be very high in some cases, and low in others. Could probably be a better approach and worth exploring to use a triple interaction in the models (you have a big dataset that should be able to handle it), something like this (simplified): Yield * Year * Pollinator dependence, or Yield * Year * Crop type (tree vs. bush. vs. herb). With this model you will also account for the uncertainty in the trends.” – We appreciate the reviewer’s advice. However, it is not clear what would be the response variable in the proposed model if not yield. If yield is the response variable, as we assume, the problem with this model is that it will depict differences in the magnitude of the slope of yield as a function of time (i.e. yield growth), which, on average, will be positive for all categories and be mute in relation to the question of yield decline. One way around this would be to consider the incidence of negative slopes in the random component “Year * Crop type/ crop”. However, in addition to potential convergence problems of such complex hierarchical models, this seems to be a highly convoluted way to go in view of the alternative of considering decline or not as the response variable itself.

9. “I am not a fan of this approach (categorize mean trend into “increase” or “decline”). You miss all the information about the effect size by categorizing trends into “decline” or “increase”, leaving out, in addition, the uncertainty of the trends. Basically, there will be a number of non-significant trends that you are forcing into the models as instances (or replicates) of “Full probability of yield decline” and “Full probability of yield increase”.” – We understand this concern. We gave a lot of thought to how to characterize “yield decline” at the onset of the study and came out with the conclusion that the sign, either negative or positive, of the long-term growth rate in yield was the simplest, and most direct and reliable way to characterize whether yield was declining or not, without imposing any extra arbitrary criteria (lines 188-191). First, yield growth estimates are based on long-term series (60 years). This large number of years should provide highly accurate point average estimates of growth rates for the vast majority of trends. Second, in terms of the goals of the

study, the absolute value of growth rate (i.e., the effect size) is little informative, as, for instance, an average yield growth rate of, let's say, 0.2%/year can be considered low for an intensively managed and bred crop but high for a less domesticated crop. Instead, the sign of long-term yield growth provides an absolute indication of whether a given crop in a given country is declining or not independent of domestication effort and other factors that may influence growth rates, a choice that is also reinforced by the results shown in Fig. 1 (lines 264-271). Finally, the validity of our dichotomous classification can be visually corroborated by inspecting actual trends, even when depicted on a log scale, as shown in Fig. S1. Straightforward quantile estimations showed that 90% of all time series (n=964) in the category of yield decline had a reduction of >5%, and 82.5% a reduction of >10% in yield over the whole period. This indicates that reductions in yield are agronomically meaningful in the long run for the vast majority of trends classified in the category of decline. This point is now included in the Results section (lines 267-269).

10. "L162: Note that "an average growth rate <0" means that a slope of -0.4 was rounded to 0 (no decline). Specify the number of decimals used." – As stated in the Methods section, "an average growth rate <0 over the period 1961-2020 was considered evidence of yield decline independent of the absolute value of the growth rate" (lines 188-189). In fact, slope values estimating annual growth rates were estimated on a log scale and thus took very small values (all of them <0.1 in absolute value; see also our response to the comment right above, and the datafile "growth" in the Zenodo repository).

11. "L190: The crop species as random factor is key here, because the slope estimates that you are using as response variable will depend strongly on the yields and crop type, very nice!´ – We thank the reviewer for this comment.

12. "L 194: I think that including the country random factor nested within the region random factor would be more accurate." – Region was not included as a random factor for the practical reason that there are too few levels associated with this factor (i.e., five; see for example Gelman & Hill 2006, p. 247), and the more conceptual one that we were interested in making comparisons between regions as shown in Figs. 6-7 and S4. This point is now clarified in the text (lines 207-209).

13. "L198-202: "All the mixed models […] included the (log10) cumulative total harvested area (in square kilometres) […] as covariates to account for variation among crops and among countries in the probability of yield decline that could relate to the agricultural relevance of the crop and country, respectively" Regarding the harvested area, total harvested area by country might reflect more strongly the size of the country than the relevance of the crop in a particular country. For instance, apple production is very important in Switzerland and in China, but the total harvested area is orders of magnitude smaller in the former country. Maybe a better indicator for what you want is the amount of cumulative harvested area relative to the cumulative agricultural area in the country (also provided by FAO). Regarding country, I understand that you want to correct for the importance of agriculture in a given country. I do not fully understand why you expect that this will be related to the probability of yield decline, but I would correct at least for the size of the country here. Agriculture can be very important in a small country. If you want to explore the proposed triple interaction model I suggested before and it does not work, I would remove these covariates, that are taking many degrees of freedom and are probably not fulfilling their purpose as they enter the model currently." – We appreciate this thoughtful suggestion. It should be noted, however, that the probability of yield

decline was in fact strongly influenced by the total cultivated area per country and per crop (Fig. S5, Table 1), and that the inclusion of these variables was important for the correct estimation and interpretation of the focal factors because of potential confounding area effects (e.g., tree crops tend to have smaller cultivated areas that herb crops; Fig. S5). In terms of degrees of freedom, area covariates take a minimum number of degrees of freedom (i.e., only one degree of freedom each; Table 1).

14. "L219-220: Wow! I am not sure that this extra analysis was necessary. It seems to have added a lot of complexity to the project and you were not looking for eco-evolutionary reasons of yield declines. I cannot think of strong reasons to argue that the phylogeny of a species per se is a main driver of yield increases or decline in the last 60 years. Still, I congratulate the authors for their great effort to account for a wide arrange of different aspects." – We thank the reviewer for her/his positive appraisal of the phylogenetic control we carried out. This control was not intended as a test of phylogenetic effects per se, but, as stated in the text, to control for phylogenetic non-independence due to shared evolutionary history that could bias results and inflate type-I errors (lines 237-238).

##Results

15. "244-255: This statement is not really correct if the trends were not free of strong uncertainty." – See our response to comments 9 and 10 by this reviewer.

16. "248: "The first year in the time series". A bit confusing sentence, maybe try to clarify it?" – Clarified (lines 264-271).

17. "Confidence intervals are only provided for the model 0 and line 296. It would be nice to see significance tests or confidence intervals for the rest of the figures provided. Otherwise, it might seem that your results are merely descriptive and I do not think that is the case." – Model 0 evaluates the relevance of random effects, so it is proper to use a confidence interval to portray uncertainty, as the main issue here is whether zero is included in the 95% confidence intervals for the overall effect (line 273) and the random variance estimates (Table 1). Line 296 (now line 325) and Figs. 4, 6-7 and S4 involve mean comparisons where the relevant issue is the extent of overlap between the intervals defined by the mean -/+ 1SE. As it can be appreciated, all these results are accompanied by significance tests.

##Discussion

18. "Nice and carefully worded discussion! "Pollinator dependence cannot be considered the primary driver of yield decline in most pollinator-dependent crops". This might be a nice take home message of your paper." – We thank the reviewer for this comment.

19. "L 342: Yes, I think that there are many confounding factors, such as the degree of management intensification, or new varieties commercialized that are much more important in terms of yield when comparing many different crops at the global scale." – Yes, we concur this is probably the case. The discussion on this point cannot go further from sensible speculation. It is difficult to determine the relevance of these factors in comparison with the effects of environmental degradation, but crop breeding and management might be offsetting some of these effects such as increasing pollination deficits due to, for instance, dwindling pollinator populations as stated in the Discussion (lines 384-388).

##Concluding remarks

20. “L 421-423: “For example, we might have reached a misleading conclusion about the relationship between pollinator dependence and yield decline if we did not consider that pollinator dependence is associated with growth form.” Good point, I agree. However, the same applies for some factors that could not be considered (e.g., degree of intensification or mechanization).” – Yes, we concur. However, yield decline, rather than just yield growth deceleration, is likely to reflect the consequences of widespread environmental degradation and not just the reach of human management skills, as we stated in this last section (lines 454-458).

##Figures

21. “Figure 2 is very interesting. Together with some data shown in the paper makes me think that crop yield decline is not really a general problem. The cut-off in the map is set to 0.6, which means that very few countries will have an overall probability of crop yield decline over 50%. Good to see that probability of declining crop yields has been overall very low worldwide.” – Yes, though this depends on viewpoint and expectations on the food system. In fact, given a priori expectations of positive yield growth rates for most crops and countries in the long run due to improvements in breeding and crop management, even probabilities of yield decline >0.10 might be considered high.

22. “Fig 4. Interesting that even when the probability of yield decline is higher in trees, it is still quite low.” – Yes, but, as stated in the comment above, we are unsure that an estimated probability of yield decline in tree crops of ~0.27 can be considered low.

23. “Fig S2. Name of crops is too blurred.” – We have increased the font size of the names of crops and countries to the maximum possible. Additionally, Figs. S2 and S3 are now provided in vector-based pdf format so that the names of the crops and countries can be clearly read after zooming them in.

Reviewer # 2, Nicolas Deguines

1. “I really enjoyed reading this very interesting and well-written manuscript authored by Aizen and colleagues. I rarely had so few things to say in a review, and I think this paper will be a very nice contribution. In their study, they analyzed six decades of crop yield data at a global scale (136 crops in total, from 163 countries) and investigated if declines in long-term yield growth rates are associated with pollinator-dependence (“None”, “Modest”, “High”) or growth form (“Herb”, “Shrub”, “Tree”). This is an important consideration because previous study investigating long-term global yields did not consider other plant traits that could be non-randomly associated with pollinator dependence (such as growth form, as is shown here) and be the main factor of yield growth decline. The aim of the study is to untangle these factors (pollinator dependence and growth form). The study is really well presented from the beginning to the end. The methodology is very well explained and sound.” – We are glad at the reviewer’s positive appraisal of our study. Thanks!

2. “All these positive points being said, there are a few points that might need to be addressed. Details are provided below and I only here introduce the main one. I wonder to what extent the simplification of the pollinator-dependence variable into three categories was necessary and whether or not keeping the full gradient could

change the result (keeping the details might be possible and could be relevant; I suggest a way to do that in the detailed comment below about L169)." – As it is explained below, most of the thoughtful and useful comments made by the reviewer were addressed in the revision. In particular, we explain in detail the reasons why we treated pollinator dependence as a categorical variable with three levels. However, as it can be appreciated treating this variable as numeric did not change qualitatively any of our results or conclusions (see our response to comment 6 by this reviewer).

Introduction

3. "L69, 71, 97-125: At first, only 'growth form' is mentioned (L69). In the next sentence, it becomes 'growth form and other functional traits' and I rather agree with this (and so do the authors as, later, in the paragraph really introducing the literature about this aspect, growth form and associated traits are considered, given how plant traits are often correlated). Perhaps just modify at the first mention ?" – We added the word "correlated" at the second mention (line 74).

4. "L120: I would cut this long sentence in two, between "outbreaks" and "whereas"." – This sentence is now cut in two and the second part has been reordered for better flow (lines 121-126).

Methods

5. "L159-163: This part is already about how data are analyzed; it is the first step. I think it should be moved at the beginning of the following section (i.e. L182 Data analyses)." – Following the reviewer's advice, we moved the estimation of growth rates and explanation of how these growth rates were classified into the decline and non-decline categories to the beginning of "Data analyses" (lines 185-193).

6. "L169: about the unbalance in number of crops per combination of "growth form" and "5-levels pollinator dependence". First, it would be useful to show the table (appendix) so that readers could make their opinion. Or at least give some numbers to back-up your decision to simplify the pollinator dependence gradient into three categories. Second, I wonder if an alternative could be to keep the details and analyze this variable as a numeric instead of as categorical. Doing so, the 'unbalance' would not be much of an issue (at least for running the glmm's). And perhaps the effect of pollinator dependence is a gradual one that could be weaken/hidden when lumping categories. In a previous paper, my co-authors and I followed an approach where categories where not lumped and considered as a numeric variable (Deguines et al. 2014). Could author consider a change in their use of the pollinator-dependence variable and, if not, provide reasons why?" – We thank the reviewer for raising this point on decision-making during the data analyses process of this project. We now provide the table required by the reviewer (see new Table S1 in the Appendix), which clearly shows a lack of shrub crops in the highest pollinator-dependent category. Considering pollinator dependence as a categorical factor with the same number of categories and associated degrees of freedom as growth form put these two focal factors on a similar footing in terms of inferential testing and associated statistical power (lines 176-179). This was key to the aim of this paper, which was to put these two explanatory factors to compete with each other. In any event and to ensure our results' robustness, we reanalyzed data using the levels of pollinator dependence considered in Deguines et al. 2014 (0, 5, 25, 65, and 95%). The strength of the effect of pollinator dependence changed very little in

model GLMM_1a (X^2=6.71, df=2, P=0.035, vs. X^2=6.14, df=1, P=0.013, when considering pollinator dependence as categorical vs. numerical, respectively). Therefore, the conclusions derived from model GLMM_2 remained unchanged based on the results of the joint analysis of both factors, pollinator dependence (X^2=2.08, df=2, P=0.35, and X^2=1.54, df=1, P=0.21) and growth form (X^2=11.93, df=2, P=0.0026, and X^2=11.04, df=2, P=0.004). These statistical results are now reported in Table S3 and briefly commented on in the Results section (lines 305-308).

7. "L196-197: I was not sure that the 3 two-way interactions were included before having a look at the table in the appendix summarizing the models' results. I even thought that perhaps the three-way interaction was included. Perhaps this sentence should be rephrased to make this clearer?" – Now we are specific about what interactions are included in model GLMM_3 (lines 209-213).

8. "Also, I strongly suggest to move Table S1 from the Appendix to the main text. I think it helps in seeing the broad methodological approach.´ – We were dubitative about including this bulky table in the main text or in the Appendix. We thank the reviewer for assisting us in this decision. Table S1 is now Table 1.

9. "L198-200: All but model GLMM_0 include as fixed effects the (log10) cumulative total harvested area per crop and country. Why not include these effects in GLMM_0? I think it's to be able to get "raw" descriptive estimates of growth rate decline by crop and country, without them being adjusted to these two effects. But I am not sure and I suggest to clarify this point." – The reviewer is right as this model was aimed at obtaining "raw" descriptive estimates of probability yield decline by crop and country. This point is now clarified (lines 199-203).

10. "L199: why was the area variables log10 transformed? Was this done to deal with convergence issue of the GLMM models? (I suppose that these variables reached quite high values, which sometimes make glmmTMB models fail to converge). Authors described very well the rest of the methods but not that point." – The log-transformation of these two variables was necessary because the raw data encompass about six orders of magnitude with an extremely right-skewed distribution. This is now clarified in the text (lines 208-210).

11. "L219-241: all this is very well-explained. Yet, authors start to explain that it is not necessary to account for phylogeny if model residuals are not associated with the phylogeny. But, in the end, authors did make models that do include 'a correlation matrix based on the phylogenetic distances between crops", in order to compare their AIC's with models without the phylogenetic distances included. On one hand, it's nice because it shows how this can be done and authors make a double-check of the robustness of their results to phylogenetical influences. But on the other hand, one wonder why doing the phylogeny-accounted-for models at all and not just check the residuals." – Good point. Meeting both conditions is reassuring: (1) phylogenetic regression models do not outperform equivalent non-phylogenetic regression models, and (2) there is no evidence of phylogenetic structure in the residuals of the non-phylogenetic models. Actually, both conditions are considered in Revell (2010) to evaluate phylogenetic dependence. The text is now modified to reflect this (lines 238-241).

12. "If keeping all this (which can make sense), I would only suggest to change the beginning of that paragraph and keep the part about "checking model residuals

against phylogeny is enough" for the end (after writing that AIC are compared)." – See our response to the comment right above.

13. "L241+: were there any checks of model residuals (under/over-dispersion, …), for example using the DHARMa package (Hartig 2021)?" – We thank the reviewer for this comment. However, as far as we know, the concept of over (or under) dispersion does not apply to a response variable that can take only two values, 0 or 1 (Zuur et al. 2009, p. 253).

Results

14. "L269: I would use "pollinator-independent" instead of "pollinator-nondependent", simply for consistency throughout the manuscript." – We thanks for pointing out this comment. Changed as suggested when appropriate.

15. "L277: I am not entirely sure of the need of GLMM_1 models. One of the advantage of models over 'simple' tests is that multiple explanatory variables can be considered together, i.e. their effects be tested and adjusted for one another. I think authors want to insist on the fact that their final model do account for the fact that two main variables tested are associated (L264 and Figure 3). In my opinion, it is not necessary. But I can see that it can be convincing for readers not very familiar with statistical analyses. Also, I must admit that the paper is very well-written and it did not feel complicated to read through these multiple models. So, I would leave it to the authors to keep it as is or reduce their MS by cutting-off GLMM_1 and 2 models." – We appreciate this comment. However, the GLMM_1 models are an integral part of our conceptual-building strategy, which in our study demonstrates that an increasing probability of yield decline with increasing pollinator dependence, as detected in model GLMM_1a, is, in fact, an indirect effect, i.e., a by-product of the association of pollinator dependence with growth form, as revealed by models GLMM_1b and GLMM_2.

16. "L279: I would remove "practically" to avoid confusion (it may be my level of English ; but I understood it as perhaps the effect was still slightly statistically significant, whereas its associated P-value is well-above)" – The word "practically" was removed and the word "disappeared" replaced by the word "almost vanished" (lines 303-304).

17. "L285: I suggest removing "some". It sounds like the effect of the interaction may be weaker than the first presented, while, if anything, it rather is the opposite." – The word "some" was removed and replaced by "also an" (line 313).

18. "L293: replace "corrected by" with "adjusted to", and potentially complete the sentence by adding after effects ", as they were all included as fixed effects in our models." "– We replaced "corrected by" with "independent of", and then completed the sentence as suggested (line 322).

19. "L297-302: I would incorporate in the methods the results of multicollinearity checks (L215) and phylogenetic inclusion in the models (L241) rather than having that at the end of the Results. Perhaps this was done to insist on the fact that these were no issue in your analyses and that the two factors are well untangled. But it seems to me it would be better in the Methods (readers are then reassured there)." – We appreciate this comment. However, after some thought we decided to leave the results of the collinearity tests and tests of phylogenetic effects in the Results section, as we feel they fit well within the sub-section "Accounting for potentially confounding factors".

Discussion

20. “L333: is human pollination a common practice over all Asia? A hand pollination picture is often used and re-used over the media, but I have never read any numbers about it and I considered this practice to be anecdotical and limited to a very local part of China. Is there a reference to justify that it is used over a large area / number of crops? If not, perhaps reformulate the sentence to better frame this information (or remove it).” – Even though human pollination of crops is a practice employed worldwide and recommended for at least 20 crops, it seems to be a common practice in several Asian countries according to Wurz et al. (2021) cited in the text (lines 377-379).

21. “L390: add “long-term” before “decline”? “– Added.

Conclusion

22. “L428-430: In this last sentence, what do authors mean here by ‘reductionist’ (need for research looking into the physiological/biological aspects of why the yield of tree growth form are more sensitive to climate change)? I think this needs to be clarified.” – The reviewer is right and this is now clarified (lines 471-473).

23. “Also, as is, the sentence is – I think – not very logical. Perhaps a word was forgotten between “In addition to” and “the need for more”, such as “highlighting” (it would match the 2nd part of the sentence: ‘our study identified…’” – We thank the reviewer for pointing out this lack of logic, with which we agree. This sentence is now rewritten including the recommendations (lines 471-473).

##Figures

24. “Figure 1: To avoid misinterpretation, I suggest making the Y-axis of the two panel of the same range, i.e. from 0% to about 15% (enough for the bottom panel and the top panel). Also, the font used for both zeros differ.” - We thank the reviewer for pointing out these format inconsistencies. Figure 1 has been redone.

25. “Figure 2: Could author find a way to show on this figure the number of crops per country? I don’t have in mind a nice way to do that. Perhaps a 2nd map, below that one (or in the appendix)? If that is not possible, it would be useful to provide some information in the text about the range of number of crops per country, and the overall median.” – Instead of a figure, we provide a new supplementary table (Table S2) that lists the countries and the number of crops per country. The range and the median of number of crops per country are provided in the main text (lines 276-278).

Supplementary information

26. “Fig S1 & S2: printed on a A4 page, the y axis is not readable because the font is too small. The figure could be wider on the page and/or find a way to use a greater font size.” – We have increased the font size of the names of crops and countries to the maximum possible. Additionally, Figs. S2 and S3 are now provided in vector-based pdf format so that the names of the crops and countries can be clearly read after zooming them in.

27. “Table S1: I already said it above but, just in case, I suggest to move this into the main text of the manuscript.” – Table S1 is now Table 1.

28. “Also, in part D.: add “phylogenetic” between “Residuals’” and “signal”?” – Modified as suggested: “Phylogenetic signal in the model residuals”.

We trust you will find this revision suitable for publication in the PCI journal.

Sincerely,

Marcelo A.Aizen

References cited by the reviewers

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Zuur, A. F., Ieno, E. N., Walker, N. J., Saveliev, A. A., & Smith, G. M. (2009). Mixed effects models and extensions in ecology with R. Springer, New York.