**Response to the reviews:**

***Editor***

Thank you very much for your submission to Ecology Letters. I have received and examined the comments of the reviewers, who provided thorough comments on your work.

They both acknowledged that the study is timely and well conducted, and the manuscript clearly written. I totally agree with them. I also think that this kind of studies based on models results leading to new hypotheses to be tested in the real world is very promising.

Yet the reviewers also raised some concerns that should be responded or taken into account in a future version. Some methodological issues especially deserve clarifications. Following these concerns, I cannot recommend the publication of your paper in Ecology Letters. However, and in agreement with the positive evaluations by the reviewers, I would suggest to carefully revise it according to the comments. Note that if you choose to resubmit, it is likely that I request the assessment by a third reviewer as both reviewers claimed that they were not able to assess all methodological parts of the study.

**We would like to extend our many thanks to the editor for their many positive comments on this manuscript and for their insight in highlighting important issues with the manuscript as it currently stands. We also greatly appreciate the editor’s oversight of such constructive, positive, and helpful criticism of this manuscript. We believe that this oversight and the many constructive comments of the reviewers have greatly improved the quality of this manuscript.**

**We have numbered the editors and reviewers’ comments and respond to them below in bold. In addition to these responses, we also highlight the revisions in the manuscript in green text to make it clear where we have addressed these comments.**

Furthermore, I would like to particularly highlight the following points:

1. As pointed out by a reviewer, I think that the section about the ‘extension to other functions’ should be reconsidered. The authors wrote on p. 16 : ‘Here, we focus on the relationships between realised diversity and three closely related ecosystem functions: aboveground biomass, productivity, and overyielding.’Sensu stricto, aboveground biomass and overyielding are not ‘ecosystem functions’. Aboveground biomass can be seen as a state variable (ie. a stock related to a flux, which is productivity), while overyielding also strongly depends on productivity. Therefore, I would suggest to clearly present the study on the productivity angle (instead of general ecosystem functions).

**We have tried throughout to focus less on biomass as indicative of general ecosystem functioning. We also have tried to incorporate our findings more fully on productivity into the manuscript on lines 256-259, 311-313, 329-332, and 347-350. One of the difficulties in comparing grasslands and forests is that biomass can be the equivalent of productivity in grasslands but not in forests. We chose to focus on biomass here because we believe it is more relevant to some key ecosystem services like carbon storage. We have now clarified this on lines 455-458 in the manuscript (see also our response to comment #2 below).**

1. (Continued from #1) better discuss the possible extension of this kind of study to other ecosystem processes.

**We have now altered this discussion to better incorporate other ecosystem functions and also to highlight some specific conservation relevant functions. This is especially apparent on lines 449 to 474 of the discussion, where we say:**

**“Here, we focus on relationships between realised diversity and aboveground biomass/productivity. Aboveground biomass and productivity are good indicators for many other functions and services, such as root biomass, carbon storage/sequestration, harvestable volume for forests, and ecosystem stability (Allan et al. 2013; Ratcliffe et al. 2017). We therefore expect that our results will hold for this set of functions. However, other functions such as above- and belowground decomposition, nutrient cycling, microbial biomass, or resistance to climate extremes are not easily predicted by biomass or productivity. Although we would not expect the biomass-based function-dominance correlation to be informative for these functions, it may be that function-dominance correlations that are based on these other functions or related functions could still prove to be useful predictors. For example, in microbial systems, if a species comprises a large amount of microbial biomass in monoculture and also plays a dominant role in contributing a large amount of microbial biomass in mixture (positive function-dominance correlation for microbial biomass), then we would predict that the relationship between realised diversity and microbial biomass across species richness treatments would be strongly positive.**

**Function-dominance correlations may be an effective ecosystem assessment tool that could be adopted by applied ecologists interested in maintaining or restoring ecosystem health. Assessing function-dominance relationships with respect to species losses and gains may allow better prioritization of management actions for conservation and more function-driven restoration (Ladouceur et al. 2021). Species loss and gain is commonly observed in many conservation areas, particularly those that lie within successional habitats such as old fields (Walker et al. 2007, Bourgeois et al. 2016). The function-dominance correlation, rather than species identity *per se*, may serve as an indicator of whether assisted community assembly (e.g., through replanting or re-seeding) would be effective at restoring additional ecosystem functioning (Isbell et al. 2019, Ladouceur et al. 2020). The function-dominance correlation may serve as a community profile tool that allows assessment of ecosystem health and the success of management, conservation, or restoration (sensu Matthews & Whittaker 2014).”**

1. Both reviewers raised concerns about how the second hypothesis tested fits in the story. One reason may be that the seed rain issue is key to promote coexistence in the models, but this should be better introduced and discussed.

**We have tried throughout to better incorporate hypothesis 2 in three ways.**

**First, we have rearranged the introduction to introduce the general reasons for why dispersal likely influences the patterns discussed here before we introduce hypothesis 2. This paragraph is now on lines 114 to 123:**

**“In addition to local processes related to species identity and dominance, regional processes such as seed dispersal can affect community composition by maintaining populations that would otherwise go extinct (Thompson & Gonzalez 2016, Leibold & Chase 2018). These regional scale processes influence ecosystem functioning independently of local scale processes (Leibold & Chase 2018, Thompson et al. 2020). For example, if dispersal maintains populations of species that are poorly locally adapted, then dispersal can weaken, or lead to negative, BEF relationships (Thompson et al. 2020). Alternatively, if species that contribute strongly to ecosystem functioning are maintained by dispersal, then dispersal can lead to strong positive BEF relationships (Gonzalez et al. 2009, Shanafelt et al. 2015, Thompson & Gonzalez 2016, Thompson et al. 2020).”**

**Second, we have highlighted and discussed much more literature on the dispersal effect throughout the paper (see the paragraph above for example).**

**Third, we have further specified the hypothesis so that the scenario we describe is more clearly tied to the effects of our seed inflow treatment, rather than discussing “dispersal” in general (lines 147 to 149):**

**“Second, we hypothesise that reducing seed dispersal will increase the importance of the function-dominance correlation for BEF relationships, because seed dispersal decreases the relative abundance of the dominant species in our simulations”**

1. Following one remark regarding the metric about diversity, I think that the results about species richness should be, if not fully included, at least better discussed (by comparing the results of the two metrics for instance).

**We have addressed this comment in three ways.**

**First, we’ve clarified the species richness results in the results of the revised manuscript. For example, on lines 311-313, we state: “This relationship also emerged when using realised species richness as a measure of diversity rather than Shannon’s diversity (Appendix S2), or productivity as a measure of ecosystem functioning (Appendix S3).”**

**Similarly, on lines 329-332, we state: “Similar results were obtained using productivity (rather than biomass) as the measure of ecosystem functioning (Appendix S3) or using realised species richness (rather than realised Shannon’s diversity) as the measure of community diversity (Appendix S2).”**

**Second, we have also better justified our focus on Shannon diversity on lines 251 to 256 where the manuscript now reads:**

**“For the results presented here, we use Shannon diversity to quantify community diversity. We do so because Shannon diversity incorporates information about both richness and evenness, and in several models, species abundances decline to very low levels rather than to zero (i.e. “asymptotic” extinction). Thus, when species became functionally extinct, realised species richness remained unchanged. Note, however, that when analysed in terms of richness, our results are qualitatively similar (see Appendix S2).”**

**Finally, we’ve also added some additional discussion of these results in the discussion on lines 448-452:**

**“Realised species richness was bolstered by seed inflow more than realised Shannon diversity, leading to a much larger increase in the across-treatment slope once seed inflow was removed (Appendix S2). This strong response emerged because seed inflow was uniformly distributed, and thus kept realised richness artificially high and reduced the importance of local competitive dynamics in driving communities’ biomass and productivity.”**

1. A reviewer pointed out that the legacy of the ‘selection effect’ (Loreau & Hector 2001, Loreau et al. 2001) was somewhat hidden. I agree with him/her about the fact that the current purpose of the study should be better linked to this, especially in the discussion.

**With respect to the selection effect, it is true that the correlation that we calculate, for example, between species monoculture biomass vs. their biomass in mixture, is quite similar to the selection effect *sensu* Loreau & Hector 2001 (which measures the covariance between monoculture biomass, vs. the difference between two ratios -- one calculated as the mixture yield divided by the monoculture yield, and the other generally calculated as 1/N, where N is the number of species planted in mixture). Both our metric and the Loreau & Hector selection effect are effectively a summary of dominance -- that is, positive values suggest that species that have high biomass in monoculture also contribute disproportionately strongly to biomass in mixture. An advantage of the Loreau & Hector metric is that it also can be related directly to total observed overyielding -- and indeed, the metric was initially invented in order to partition observed overyielding into components related to dominance (their “selection effect”), and components not related to dominance (their “complementarity effect”). However, this partition comes at the cost of added complexity (e.g. we suspect the verbal description at the top of this paragraph is a bit confusing even to readers familiar with Loreau & Hector 2001), and it also cannot be calculated in cases where species become very rare or go extinct in monoculture, as this drives the ratio in the index towards infinity.**

**Our metric, on the other hand, applies equally well regardless of monoculture biomass, and is substantially easier to compute and explain. Because our paper’s main goal is to explain the variation in BEF relationships across systems without needing to exactly partition observed overyielding, we chose to apply our simpler, more robust metric.**

**We have now clarified how our metric relates to the one of Loreau & Hector on lines 281 to 287 and the more general concept of the “sampling effect” on lines 103 to 110. For example, on lines 281 to 287:**

**“Note that like the ‘selection effect’ of Loreau & Hector 2001, our correlation coefficient effectively summarises overall effects of dominance on yield in mixture -- i.e., it tests whether highly functioning species in monoculture also tend to be highly functioning in mixture. We use this metric, rather than the classic Loreau & Hector metric, both for simplicity, and to avoid issues related to low or zero monoculture biomass that can complicate the classic Loreau & Hector metric (Clark et al. 2019).**

1. I think that the authors could have mentioned former works using the same kind of tools to investigate BEF relationships and their underlying mechansims, e.g.:
   1. Bohn, F. J., and A. Huth. 2017. The importance of forest structure to biodiversity–productivity relationships. Royal Society Open Science
   2. Morin X., Fahse L., Scherer-Lorenzen M. & Bugmann H. 2011. Tree species richness promotes productivity in European temperate forests through a strong complementarity effect. Ecology Letters, 14, 1211–1219.

**We have now added a paragraph to the Introduction that includes this information on lines 128 to 133, it reads:**

**“Although plant community models have been used in the past to explore BEF relationships (Morin et al. 2011, Holzwarth et al. 2015, Bohn & Huth 2017, Maréchaux & Chave 2017), here, we consider a much wider variety of models, systems (grasslands, forests, drylands) and community assembly processes. This diversity of modelling approaches leads to a much wider range of resulting BEF relationships, thereby better mirroring the context dependence observed in natural systems.”**

1. Although I repeat that I really believe in such studies applying Independent models to test ecological hypotheses, I think that the limitations coming with such a study should be mentioned (at least shortly), especially for non-modellers to better understand pros and cons of the approach.

**We discuss the pros of such a standardized model comparison in a few places in the manuscript, e.g., in paragraph above. However, we can think of one specific limitation: Our results depend on how well the model structures and parameterizations capture the community assembly mechanisms that operate in reality. This is the main reason that we included only six carefully models in our comparison. Four of the six models were parameterized with extensive field data, and five models were extensively validated against independent observations (i.e. not used for model parameterization). Just one of the models is a totally theoretical model.**

**We have now mentioned this limitation on two sections of the discussion, lines 395 to 402:**

**“Unlike the function-dominance correlation, the specific slope and sign of the relationship between realised diversity and biomass varied with model structure both between species richness treatments and within species richness treatments. For example, the two tropical forest models show differing results. However, across model types, study systems, and community assembly mechanisms, we were able to identify a common pattern: the link between the function-dominance correlation and the strength and direction of BEF relationships. Thus, we expect that the function-dominance correlation is relevant for understanding variation in the sign and magnitude of BEF relationships across a wide variety of systems.**

**Also, on lines 441 to 448:**

**“Seed inflow in our models is limited to the original species pool, and assumes constant, uniform seed input across species. While this approach is not realistic, our results are consistent with findings from a recent meta-analysis of seed addition experiments (Ladouceur et al. 2020) which found that when dispersal limitation was alleviated by seed addition, species richness increased (also reviewed by Myers & Harms 2009) while biomass was relatively unaffected. Similarly, in our models, communities generally had higher realised diversity with seed inflow than without it, with biomass remaining largely unaffected relative to changes in Shannon diversity (Appendix S5).”**

**We would also like to stress that our next step in future research will be the validation of the findings of this study with data from BEF experiments.**

***Referee: 1***

Findings from empirical experiments conducted across systems ranging from grasslands to forests over close to three decades have shown a generally positive relationship between plant community biodiversity and ecosystem function (usually productivity). Yet, in a minority of cases, data from experiments have also indicated negative biodiversity-ecosystem functioning (BEF) relationships or shown no evidence of BEF relationships. Why is this? The authors of “The function-dominance correlation drives the direction and strength of biodiversity-ecosystem functioning relationships” propose a novel, underlying dynamic within plant communities – one that may explain some of the mixed results coming from BEF studies.

Specifically, these authors propose the “function-dominance relationship,” a correlation (positive or negative) between the dominance of a particular species in a community (e.g., its share of total biomass) and its contribution to community ecosystem functionality (e.g., biomass production). When function and dominance are positively correlated, dominant species contribute most to the ecosystem process in question, with scale-dependent consequences for BEF dynamics. At large taxonomic scales (across communities with different levels of species diversity), positive function-dominance relationships contribute to apparent, positive BEF relationships. At small taxonomic scales (across communities with the same or nearly the same species diversity), a negative counter-gradient emerges, in which the most functional communities are the least diverse (because the dominant, productive species predominates).

The authors justify and provide evidence of this dynamic from six previously published vegetation models. These models vary in system, in data source (e.g., empirical field data, literature values, theoretical values), and many other respects. The authors also use these models to explore a secondary question, which is whether metacommunity dynamics (e.g., seed rain or not) weaken or strengthen the role of the function-dominance relationship in structuring BEF dynamics.

The authors have produced a sophisticated, high-quality, and generally very clearly communicated manuscript. I don’t have any substantive critiques of their work. Below, I review my only concerns, which have to deal with the framing of the manuscript, and provide some minor suggestions for improving the language employed in it.

**We would like to extend our gratitude to the reviewer for their positive comments on this manuscript as well as for their extremely constructive remarks below. We believe that these contributed to substantially improving the readability and clarity of the manuscript and deeply appreciate them.**

1. My main concern (rather than critique) with the authors’ work is that, despite claims of its relevance to conservation/management (e.g., lines 505-19) it will be limited in impact to ecologists working in the BEF field. As a BEF person, I appreciate the proposal of this function-dominance explanation of BEF dynamics in the field. And I am optimistic about the realism of the phenomenon given that three of the models used to elucidate it were parameterized with field data. Yet the proposal of the correlation seems to beg some fairly important questions, which must be answered before it becomes generally relevant to management. The passage in lines 495-7 is particularly telling: “our results suggest that diversity change will have the strongest impact on ecosystem functioning when the dominant species provide the most function.” The question is, then, do they? In the case of biomass production, especially under light-limited conditions, the answer may very well be yes. But without attending to the generality (or lack thereof) of, for instance, positive function-dominance relationships, this work is largely of interest to those in the BEF field rather than managers or even ecologists more generally.

**We have now added context to both the introduction on lines 164 to 168 and the discussion on lines 441 to 452 that highlights the generalizability of these patterns to naturally assembled systems and to other functions. Additionally, on lines 454 to 468, we state:**

**“Here, we focus on relationships between realised diversity and aboveground biomass/productivity. Aboveground biomass and productivity are good indicators for many other functions and services, such as root biomass, carbon storage/sequestration, harvestable volume for forests, and ecosystem stability (Allan et al. 2013; Ratcliffe et al. 2017). We therefore expect that our results will hold for this set of functions. However, other functions such as above- and belowground decomposition, nutrient cycling, microbial biomass, or resistance to climate extremes are not easily predicted by biomass or productivity. Although we would not expect the biomass-based function-dominance correlation to be informative for these functions, it may be that function-dominance correlations that are based on these other functions or related functions could still prove to be useful predictors. For example, in microbial systems, if a species comprises a large amount of microbial biomass in monoculture and also plays a dominant role in contributing a large amount of microbial biomass in mixture (positive function-dominance correlation for microbial biomass), then we would predict that the relationship between realised diversity and microbial biomass across species richness treatments would be strongly positive.”**

**We also examine the conservation consequences of this work on lines 469 to 480:**

**“Function-dominance correlations may be an effective ecosystem assessment tool that could be adopted by applied ecologists interested in maintaining or restoring ecosystem health. Assessing function-dominance relationships with respect to species losses and gains may allow better prioritization of management actions for conservation and more function-driven restoration (Ladouceur et al. 2021). Species loss and gain is commonly observed in many conservation areas, particularly those that lie within successional habitats such as old fields (Walker et al. 2007, Bourgeois et al. 2016). The function-dominance correlation, rather than species identity per se, may serve as an indicator of whether assisted community assembly (e.g., through replanting or re-seeding) would be effective at restoring additional ecosystem functioning (Isbell et al. 2019, Ladouceur et al. 2020). The function-dominance correlation may serve as a community profile tool that allows assessment of ecosystem health and the success of management, conservation, or restoration (sensu Matthews & Whittaker 2014).”**

1. My main critique, which again pertains to framing and not to substance, has to do with claims around extension of this approach to ecosystem functions beyond productivity (lines 477-489). To be honest, I was skeptical that this approach could make sense outside of a focus on productivity and I am still not convinced that it can. Productivity has been the star of BEF research not only because it is a key function in the global carbon cycle, but also because it is associated with the physical production of biomass, a concrete stock that can be measured as a proxy of the flow in question (productivity). This stock (biomass) is used very sensibly in the authors’ work to estimate dominance. But extension to other functions really feels like pushing the envelope. For instance, some of the major ecosystem functions measured in the BEF literature – outside of productivity – include herbivory and decomposition (the latter is mentioned in this manuscript). What is dominance in herbivory or decomposition? A species’ relative share of total community herbivory or mass loss? I ask these questions with a genuine interest in the answers and am open to a satisfactory answer. But as presented, the authors do not provide one and so do not, I think, substantiate their claims that the function-dominance relationship can be useful outside of studies of productivity.

(I will also add, pertinent to the above comment, that it strikes me as tenuous and unnecessary to claim, in lines 278-80 and throughout that biomass, productivity, and overyielding are distinct “but related” ecosystem functions.)

**We have tried to address this comment in three ways.**

**First, we’ve now removed discussions of overyielding from the manuscript to decrease the focus on its distinctness from biomass and productivity.**

**Second, we’ve tried to better highlight the results for productivity in each section of the results in response to this comment, as well as those from the editor (see comment #1 in our response to the editor, for example on lines 311 to 313).**

**Finally, we’ve clarified what results we would expect from other functions on lines 455 to 468, as described in our response to reviewer #1 comment #1.**

1. Finally, there is a counterintuitive nature to the authors’ framing of “diversity” gradients among communities with the same species richness (e.g., Fig. 1A). Strictly speaking, it’s true that communities with the same species richness can vary in community diversity. And the authors use a diversity metric (Shannon diversity) that incorporates species richness and evenness. But the thing that varies in these counter-gradient comparisons, evenness, is rarely thought of as synonymous with diversity, especially in the BEF literature, where it is much less frequently manipulated compared to richness. For me, this makes passages such as lines 157-162 really hard to understand on a first (or second!) reading. For readers who are not steeped in the BEF world, this may be even more confusing. I suggest that the authors find a way of distinguishing between the two “diversity” gradients (e.g., by specifying a taxonomic vs. evenness gradient and/or drawing attention to the differences in taxonomic/phylogenetic scale between the two gradients).

**We’ve tried to address the issue of confusing terminology when discussing the counter-gradients in two ways:**

**First, we have separated the results so that the “across” and “within” patterns are discussed in distinct sections.**

**Second, we have stepped away from using shortcut language like “BEF relationships” and “between treatment slopes”, and instead use language throughout that specifies both the x and y variables in each comparison.**

**For example, on lines 301 to 302 where we before wrote “Within-treatment BEF slopes were negative in four out of the six models.”, we now write: “Within species richness treatments, the slope of the relationship between realised diversity and biomass was negative in four out of the six models.”**

**We hope that it improves the readability of the manuscript for a more general audience.**

***Referee: 2***

Comments for the Authors

Using six established plant community models, this study aims to understand the role of the function-dominance correlation in the biodiversity-ecosystem functioning context. I think that the topic is interesting and appropriate for the journal as it addresses an overlooked but important aspect of the biodiversity-functioning field. Basically, this study shows how the dominance-functioning correlation of species in a community can explain the sampling effect (often simplistically considered just an experimental artefact) and how it can either have positive or negative effects on ecosystem functioning and affect the variability of responses. Overall, I really like the study, but I think it should go through a revision before being ready for publication. Below a series of points that hopefully will help clarifying a few aspects and improve the paper.

**We would like to extend our many thanks to the reviewer for their positive feedback and thoughtful suggestions. We believe that these suggestions have contributed to substantially clarifying the manuscript and greatly appreciate their efforts to help us.**

1. While I think that the presented data is in line with the first hypothesis, there are additional points that need to be clarified before I am convinced that the second hypothesis can be supported by the presented data. Right now, I feel that that the authors are a bit overreaching. The authors state that “We hypothesise that reducing seed dispersal likely increases the importance of the function dominance correlation for BEF relationships since seed dispersal may decrease the abundance of the dominant species”. It is not clear to me why seed inflow may only decrease the abundance of dominant species and this postulate is further used to interpret the results. I’m not a modeler and it is not a lot of detailed on how this seed inflow is parameterized in the model, but I argue that this depends whether the species that enter from the species pool via the seed inflow are dominant species or not. If a very dominant species enters the community it could end up increasing the abundance of dominant species via its contribution.’

**We agree that, as the reviewer suggests, more information on how seed inflow is characterized is imperative to understanding this hypothesis. We have addressed this problem in three ways: 1. By adding a paragraph to the introduction discussing how seed dispersal is likely to influence biodiversity ecosystem functioning relationships (lines 114 to 123), 2. Adding supporting literature for dispersal effects in biodiversity ecosystem functioning research (throughout), and 3. By further specifying the hypothesis which is specific to dispersal as produced in our modeling experiment (lines 147-149).**

**The clarification on seed inflow is now provided in the methods on lines 240 to 248:**

**“During this initial stage, seed inflow from the initial species pool took place (“with seed inflow”). Seed inflow for each species was constant and corresponded to average internal seed production across all equilibrium monocultures, divided by the number of species. This implementation generates equal numbers of saplings (forest models) or equal seed biomass (grassland/dryland models) per species per year, and therefore buoys abundances of poorly performing species. To explore the effect of isolating communities from their respective metacommunities, in a second stage of the same duration, seed inflow was stopped (“without seed inflow”). We measured diversity and ecosystem functioning on the last time steps of each stage.”**

**We have also revised this hypothesis to reflect this specification and clarified the relevance of this second hypothesis throughout, in response to this comment and the guidance of the editor (see our response to the editor’s comment # 3).**

1. Maybe I missed it, but I is not clear to me what is actually computed for the function-dominance numbers. While I understand that it’s based on the correlation between a species dominance and its performance in monoculture, for a given community it is not clear what it is. Does the number refer to the correlation of the most dominant species in that community? or is it an average of the correlations all the species in that community? …something else? Please clarify.

**We have now clarified this in the methods on lines 277 to 287, where we state:**

**“To do this, we bootstrapped species’ mixture biomasses (n=2500) using the 32-species mixtures. Each bootstrap contained 2048 species (32 species per mixture x 64 replicates) selected with replacement. We then calculated the Pearson’s correlation between species’ biomasses in monoculture (‘function’) and their respective mixture biomasses (‘dominance’).”**

1. Although it is mentioned a couple of times, I feel that the authors are somewhat avoiding to use the established “positive or negative sampling effect” and prefer their own terminology. Perhaps they are afraid that it comes across less novel but I think the opposite. I think that the authors should clearly state when the results are in line with a positive or negative sampling effect. It is mentioned in the discussion a bit but it could be also introduced in the abstract and the results.

**Thanks to the reviewer for their comment on this, we were indeed avoiding putting our results in terms of sampling effects and selection effects from the biodiversity-ecosystem functioning literature because we felt that this would be a bit confusing for a reader outside of this field. We have now clarified how our metrics relate to these previously established terms on lines 103 to 110 of the introduction for the sampling effect and 281 to 287 of the methods for the selection effect. Please also see our full response to this in our response to the editor’s comment #5.**

1. Speaking of terminology, I think that “seed inflow” is perhaps a more appropriate term to use instead of “seed rain”.

**We agree that this new term is potentially more intuitive for a general audience and we have changed this throughout.**

1. Also, I suggest that the authors are more consistent with the wording when describing effects “across and within species richness levels” as they use multiple ways to say the same thing throughout the text which can be tiring.

**We have clarified this issue also with respect to the comment #3 of reviewer #1.**

Some minor points:

- Lines 366-367, I would rephrase as it sounds like is the only possible mechanism, which is not the case.

**We have now rephrased these lines to clarify our purpose here.**

- What do you mean by “Underlaying latent variable”? do you mean an alternative explanatory variable?

**We have now revised this section with regards to Simpson’s paradox and hope that this is now clearer on lines 112 to 113 where we state: “This type of counter-gradient is often associated with Simpson’s paradox, which arises when observed relationships vary across different subsets of data (Simpson 1951).”**

- Some correlation coefficients or coefficient of determination on the graphs would be useful.

**We have now added correlation coefficients to the graphs.**