

This study experimentally explores how intraspecific genotypic and functional diversity of consumers affect ecosystem functioning at lower trophic levels. I found this especially interesting given that I have rarely come across studies that account for intraspecific diversity, let alone considering both genotypic and functional diversity, and (!) that also considers the effects of intraspecific diversity at different trophic levels. Therefore, I found this a very exciting and especially novel test of BEF theory that potentially brings to light some very important findings.

That said, I also have a number of concerns about the level of detail provided in the methods and also discussion, as well as with the analytical approach, in particular with regards to the development of hypotheses and how they have been tested (or perhaps not) in the path models. In particular, clearer justification is needed for the hypothesized relationships. For example, why do the authors expect main effects of genotypic and functional richness on zooplankton abundance and richness, but only an interactive effect on zooplankton diversity (Fig S3b)? This is assuming that the diagram in Fig S3 indicates an interaction between genotypic and functional richness, though that is also not explained anywhere. In that vein, why would the authors expect an interaction between genotypic and functional richness in (b) but not in (a)? It would also be helpful to see a list of the 'independence claims' (the basis set) used to construct the path model, to ensure this matches up with the hypothetical models presented in Fig S3. Furthermore, there is no explanation of the 'mechanisms' tested in these path models; that is, the modulating effects of invertebrate and zooplankton abundance/diversity on decomposition and algae. Therefore, it is still unclear exactly what hypotheses the authors are trying to test, which is critical for confirmatory analyses such as these. In addition, the path models shown in Figure 3 appear to have weighted arrows, but it is not explained how the arrows have been weighted. Are these standardised coefficient weightings? Or are they unstandardized coefficients? This is also crucial for interpreting these path models.

In addition, it was not at all clear to me what level of variation in genotypic richness there was between the 'low' and 'high' genotypic richness treatments, although there are details given in the supplement on variation in body mass and functional richness. Was genotypic variation actually quantified, or just assumed given the geospatial separation among sites where the minnows were collected? I see this might be available information from another study, but this should be included in this paper as it is rather fundamental to the experimental design of this study.

Specific comments:

Line 38 – 39: Strange wording "The loss of genotypic richness was similarly prejudicial than the loss of functional richness". I'm not quite sure what the authors mean by this – please clarify.

Line 55: The 'iBEF' abbreviation does not really work if plural, as it stands for "intraspecific biodiversity-ecosystem functioning", without "relationship". This should rather be written as "iBEF relationships". Please check throughout manuscript.

Line 64 – 65: How does that approach not "provide mechanisms"? Which mechanisms? The concepts here are quite vague and need some more development.

Line 76: Change "and a better mechanistic understanding" to "and could provide a better mechanistic understanding".

Line 78 – 79: One might argue that biodiversity effects can be just as strong with changes in lower trophic levels, because of bottom-up effects that could resonate through to higher trophic levels.

Line 91: I am not sure about the terminology of "essential" ecosystem functions. What would be considered a 'nonessential' ecosystem function?

Line 93 – 94: “Predatory and consumer species” – this is redundant and simpler to just say “consumer species”.

Line 109: “At the opposite” – change to e.g. “Alternatively”.

Lines 111 – 114: Why would the relationship differ in the ways presented in Fig 1? Are these hypothetical scenarios based on any previous research? Some more development of these hypotheses is needed to give some conceptual foundation underlying these expectations.

Line 114 – 115: Perhaps this is true, but until this point there is no explanation of these mechanisms. What are they, and how would they modulate the effects of genotypic and functional richness on ecosystem functioning? Also, change “bases” to “basis”.

Line 116: “affected directly populations’ biomass production” – change to “directly affected population biomass production”

Line 117: I think the authors need to more clearly define what they mean by 'ecosystem function'. Biomass production is typically referred to as an ecosystem function in the BEF literature so this could be rather confusing if a clear definition is not provided earlier in the introduction.

Line 118: Can the authors give more information on the study system here? E.g., freshwater or marine?

Line 169: Was it four or six genotypes?

Line 187: “1 - nb. survival fish/nb. introduced fish” – what does this mean and what is it for? This requires some more explanation.

Line 226 – 227: More detail is needed here, and perhaps show results prior to outlier removal so it is possible to assess why the authors felt this should be removed.

Line 242: “and effect sizes were averaged” – can the authors please explain how the averages were calculated? Specifically, which values were averaged (average across which effect size values)?

Line 255: Strictly speaking, I'm not really sure the term 'null models' should be used to describe these. They are simplified models that exclude the relationship between both genotypic/functional richness and decomposition/algae stock. However, they are not the simplest possible models.

Line 277: “At the opposite” – change to e.g. “In contrast”.

Lines 290 – 291: As opposed to? This is written as though an alternative was tested, but that the authors found the strongest support for a trophic cascade. However, the way the analysis is designed, this is the only possible outcome that could have been found. If the path models also allowed for direct effects of minnow richness on ecosystem functions, significant direct effects might indicate that there are other factors at play, other than indirect/cascading effects on ecosystem functioning. Of course, it could also mean that other measures of invertebrate/zooplankton community structure are important (aside from abundance or diversity), that were not quantified in this study. This seems important to include in the analyses in order to confidently make statements such as this.

Line 296 – 297: Again, this statement is unfortunately not supported by the path models, as it appears that direct effects of intraspecific diversity were not included in the full models. Thus, the results could also potentially arise from correlations of invertebrate and zooplankton abundance/diversity with other unmeasured (but potentially more important) variables.

Line 306: “iBEFs” – change to “iBEF relationships”.

Line 335: “extent” – change to e.g. “expand on”.

Line 337: “diversities” – change to “diversity”.

Line 345: “diversity benthic invertebrates” – change to “benthic invertebrate diversity”.

Line 357 – 358: It is not very clear how this relates to, or explains, the results presented in the preceding sentence.

Line 360 – 362: I would still be interested to hear the authors’ thoughts on why enhanced intraspecific diversity of fish caused an increase in invertebrates and zooplankton. Until now, there is no clear explanations provided that could be tested in future experiments.

Line 364: “basis” – change to “base”.