**GCB reviewer's comments on precipitation change paper**

**Response to reviewers’ comments**

We thank the two anonymous reviewers for their helpful and encouraging comments. In response to their comments we have now (i) collated more studies and extracted more data; (ii) rerun statistical analyses and added statistical analyses to test a new hypothesis; (iii) and added clarifying text where requested. In addition, we have improved our analyses of bias relating to precipitation manipulation by including predictions of future precipitation from 25 different climate models, rather than just one, thereby accounting for differences between these models. In summary, we believe following our revisions that the manuscript is significantly improved. Below, we provide details of the specific changes we made in response to the reviewers’ comments.

**Reviewer 1**

1. This meta-analysis provides an intriguing overview of the current state of research on the effects of climate change on forest soil and litter fauna. The study outlines three different objectives, all of which are successfully achieved. I have to admit that I really enjoyed reading this paper, but even I am not an expert in meta-analysis, I doubt whether 30 studies could be not enough for a meta-analysis. I don't know how long a new search would take and how useful it would be, but I did a quick search in WOS and I found at least 4 papers that could meet the requirements for this meta-analysis that were not included in it.

**Response to reviewer**: We are pleased that Reviewer 1 enjoyed reading our work, and thank them for conducting a search in WoS. We believe the reason for omitting potentially relevant studies is because our final literature search was conducted in 2021. To rectify this, we repeated searches to include papers published since 2021. This additional search retrieved 1967 articles, which we screened for their eligibility for inclusion. Of these 1967 articles, four further articles matched our inclusion criteria. This increased the number of primary articles that we synthesised from 34 to 38, comprising 430 pairwise comparisons between control groups and those where precipitation was altered.

Reviewer 1 doubts “whether 30 studies could be not enough for a meta-analysis”. While we understand the reviewer’s concerns about sample size, we would like to note that, there is no prerequisite number of studies that make a meta-analysis robust. Technically, at least two effect sizes are required to perform a meta-analysis. The number of studies that a synthesis includes is constrained by the inclusion criteria that are set, which places minimum standards on the quality of studies included (their internal validity), and also defines the target population to which inferences will be generalised - in our case soil and litter invertebrate fauna in forests. Relaxing our inclusion criteria, for example by allowing the inclusion of studies that focus on any invertebrate group (not just those dwelling in soil or litter) or those carried out in non-forest ecosystems, would have increased the number of studies included, but the pool of studies would have become more heterogeneous, and mean effect sizes would have been less interpretable [(Spake et al., 2022)](https://paperpile.com/c/h2W6U1/QKQt). We also note that a number of meta-analyses published recently in Global Change Biology use similar sample sizes to ours [(Dahms & Killen, 2023; He et al., 2023, 2024)](https://paperpile.com/c/h2W6U1/Onav+VSj2+HZPK).

In addition, we would expect the number of primary studies needed to make solid inferential statements about the effects of stressors (such as prescription change) in a meta-analysis to be higher when using observational studies than when using experimental data. These observational studies typically have greater unexplained between-study variability caused in part by difficulties in controlling for nuisance variables. However, in our study 34 of the 38 studies we used were experimental and so the statistical power of our meta-analysis is relatively high compared to meta-analyses that solely use observational studies - as is commonly the case in ecology.

1. I also think the paper could be greatly improved including other key aspects of climate change, such as warming, in addition to precipitation changes, or at least I consider a clear explanation is needed as to why you decide to exclude this key aspect of climate change.

**Response to reviewer**: We agree with the reviewer that other aspects of climate change such as warming have important effects on soil communities. We note that including studies that investigated temperature change would also have doubled the effort required for screening of papers - from 5,345 papers found on Scopus (using current search) to 9,488 by including terms relating to temperature increase.

In response to this comment by Reviewer 1, we have added a section justifying our focus on precipitation change in the Materials and Methods (see lines 159-163). We have also included a new section in our Discussion acknowledging the potential interactions between precipitation change and other global change drivers and recommend further research into this topic (See lines 657-670)

1. Another weakness I found is that the results are highly dependent on the large variability between studies and I think it would be appropriate to try to disentangle this variability, for example by doing new analyses taking into account the geographical origin and biomes of the studies where possible.

**Response to reviewer**: Thank you for this comment. It is common for ecological meta-analyses to explain variation among effect sizes by using coarse proxies of environmental variation such as biome and geographical region. In response to this comment from Reviewer 1, we thought about what aspects of biome or geographical origin might make most sense to investigate for a synthesis of precipitation change impacts. We decided that the aridity of the ecosystem would be useful to investigate since aridity has direct relevance to soil ecosystem dynamics [(Hu et al., 2021)](https://paperpile.com/c/h2W6U1/Jsn8), especially in the context of precipitation change. Indeed, one could hypothesise that due to greater historical exposure to droughts, organisms in arid ecosystems might be preadapted to reductions in precipitation as opposed to organisms in wetter ecosystems [(Balmford, 1996; Kraft et al., 2015)](https://paperpile.com/c/h2W6U1/gpVS+Hz7P).

To address the comment of Reviewer 1 and to test this new hypothesis we divided between arid and humid ecosystems based on the Global Aridity Index [(Zomer et al., 2022)](https://paperpile.com/c/h2W6U1/zrB1) and used this classification as an explanatory variable in our models. To do this, we examined whether there was an interaction between the magnitude of precipitation changes and the aridity of the ecosystem. However, the models in which ecosystem aridity was included as an explanatory variable were poorly supported and thus the qualitative results of our manuscript remain unchanged.

Based on this new hypothesis and set of analyses, we have now added details about the theory underlying the hypothesis to the introduction (lines 100-111), updated our hypotheses (lines 134-136 and Figure 1), added details to our methods (lines 258-263 and line 282), and added detail to the discussion (lines 492-493).

1. However, I see this manuscript as a notable advance in the understanding of a typically overlooked group that is as the same time crucial to ecosystem function. This meta-analysis makes a significant contribution to future research in this area and serves to improve our knowledge of the future functioning of soils.

**Response to reviewers**: We are pleased that Reviewer 1 thinks our work makes an important contribution to the field. Thank you.

1. L56: I think you should use a more recent reference to remark- the lack of studies on precipitation changes effects on forest soil biodiversity.

**Response to reviewer**: Thank you for pointing this out. We have now altered the sentence (see lines 55-59) so that it points out the more general lack of knowledge about soil and litter fauna responses to disturbance (as opposed to just precipitation changes). We have a reference to Pressler et al. [(2019)](https://paperpile.com/c/h2W6U1/8JVB/?noauthor=1) and Winding et al. [(2020)](https://paperpile.com/c/h2W6U1/6ZXs/?noauthor=1) as evidence of this claim.

1. L59-61: I suggest rewording the sentence

**Response to reviewer**: We have now reworded this sentence (see lines 60-62) to make this clearer. It now reads: “Since soil moisture is a key limiting factor to the fitness and behaviour of many soil and litter fauna, precipitation changes may threaten the processes to which they contribute [(Coyle et al., 2017)](https://paperpile.com/c/h2W6U1/IPehN).”

1. L61-66: These 2 sentences could be reduced

**Response to reviewer:** We have now shortened these sentences into one sentence (see lines 62-66) that now reads: “Precipitation changes and associated changes in soil moisture can alter the movement of microfauna such as nematodes, and therefore their access to food sources, or the humidity in pores which represent the habitat of mesofauna such as Collembola (Coyle et al., 2017; Erktan et al., 2020).”

1. L93: I like to use the size of the soil fauna as a factor involved in the response to precipitation manipulation, but I think it might be interesting to take into account the differences within groups (i.e. commonly, predator nematodes are much larger than herbivores, and they will respond differently to precipitation changes).

**Response to reviewer**: We agree that there is likely a more nuanced relationship between body size and response to precipitation than we have been able to explore in our manuscript. Unfortunately however, we lack the taxonomic resolution that would allow us to define trophic groups for most of the studies included in our analyses. Although we do possess information about the trophic groups of nematodes for studies of drought impacts, we only have this for two field studies. In response to the reviewer’s comment we ran an additional analysis (Figure 1 in this document) to test how drought impact might vary by trophic group. However, given the small sample size for this analysis we do not consider these results to be robust. This is because (i) addition of future studies on this topic would likely alter the results of our analysis [(Koricheva & Kulinskaya, 2019)](https://paperpile.com/c/h2W6U1/wIdY) and (ii) estimates of the impact of stressors on biodiversity taken from individual studies are likely to be poor estimates of general changes due to random variation [(Christie et al., 2019)](https://paperpile.com/c/h2W6U1/JrGn). As such, we chose not to include this analysis in our manuscript. We have however added detail regarding the possibility of a more nuanced relationship between size and precipitation response, modified by other functional traits, to our discussion (see lines 608-610).

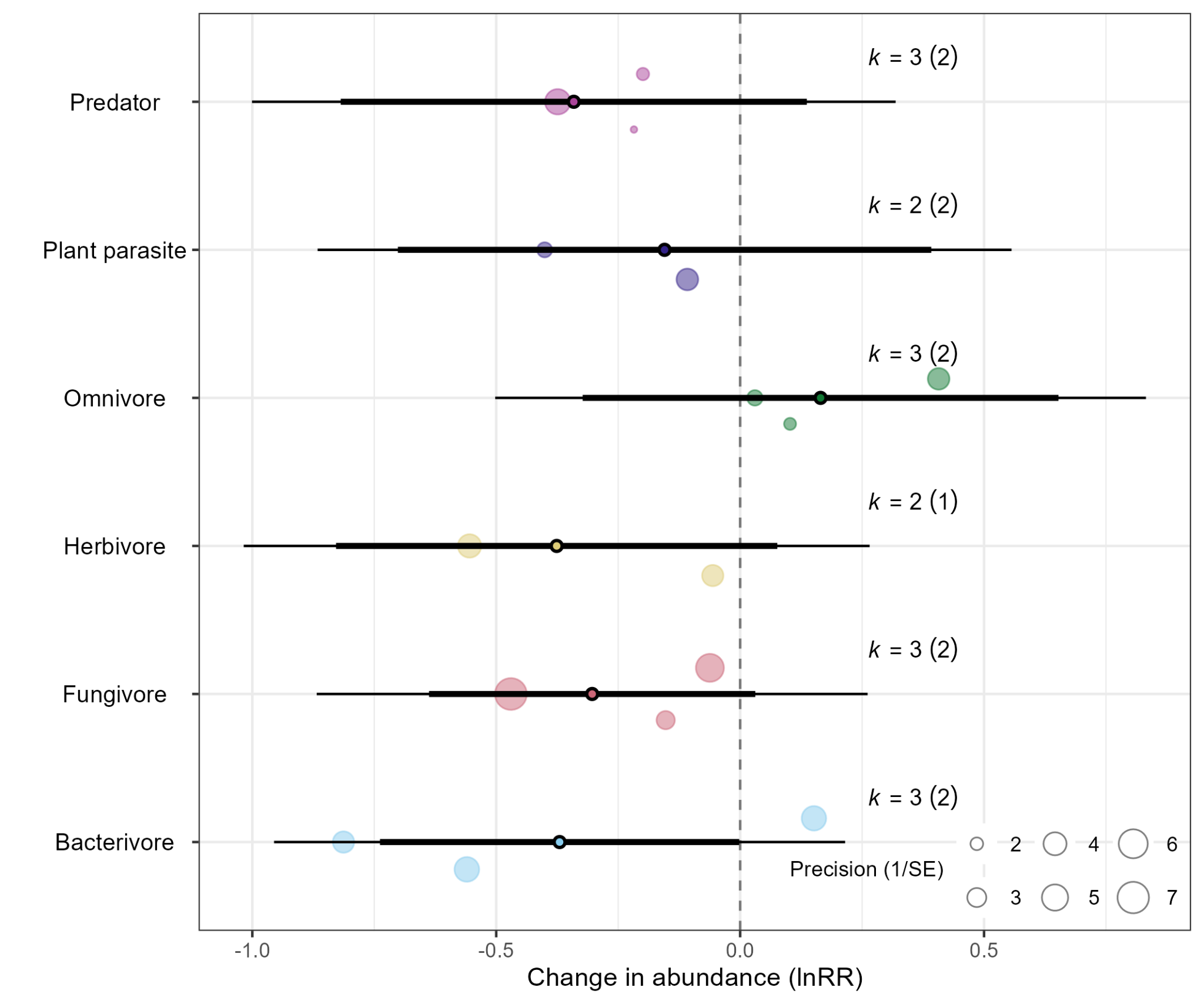


Figure 1 - Results from an additional analysis of impacts of precipitation reduction on the abundance of different nematode trophic groups.

1. L214: Here you explain that you looked for studies that performed spatial and temporal comparisons. However, I noticed that in the rest of the manuscript you almost ignore the origin of the study and the season in which the samples were taken.

We have now clarified that spatial and temporal comparisons refer to the study design employed - whether units of replication were distributed across space or time (see lines 226-228). The text now reads “We therefore determined whether studies (i) consisted of both spatial (i.e. comparisons between spatially distinct control and treatment groups) and temporal comparisons (i.e. comparisons before and after a precipitation change)...”

We used this spatio-temporal classification of study design, in addition to other design features including whether studies were experimental, whether their designs showed signs of confounding, and whether the designs were randomised to assign scores of study robustness, as part of a critical appraisal step. We used these scores in a sensitivity analysis that explored whether studies of differing robustness varied in the impacts of precipitation changes on biodiversity that were observed. To clarify this, we have added information to our methods (see lines 273-276).

In addition, we have added a section to the beginning of the results section that provides an overview of the dataset we used for our analysis, which includes information on the season in which the samples were taken and information about study designs, as well as details about the geographic regions from which data originates, taxa included, the biodiversity metrics we extracted information for, and the robustness of the studies included (see lines 324 - 343).

1. L234: In line with the previous comment, you do not provide information on the period of the year of sampling for the different studies

**Response to reviewer**: We now include this information in a newly added piece of text - please see our response to the previous comment.

1. L362: You found effects of the year of publication on taxonomic richness and diversity, however you did not discuss these results.

**Response to reviewer**: Originally we did not add much detail about this because, although variables relating to publication year and study size were included in some of the most parsimonious models, their effects were not statistically significant. We have now added extra detail explaining these effects (see lines 396-399, 420-421,425-426, 494-495).

1. L475: I think a more fine-grained approach to the size of soil animals could be interesting and provide interesting results.

**Response to reviewer**: We agree with the reviewer that this would be great to do and offer a much more nuanced way of examining size-dependent effects of precipitation change. We undertook some alternative analyses testing the impact of using data on body width measured in mm rather than categorical information on body size. However, we found that models including this variable provided a worse fit and were less parsimonious than other models. We think that this might be the result of the relatively low taxonomic resolution of the data we had access to, meaning that groups such as Collembola are represented by one value despite body size being quite variable within this group. In addition, including body size as a continuous variable interacting with the magnitude of precipitation change, meant that impacts had to change linearly with body size, while our analysis using body size groups allows for more flexibility in this relationship. To address the general point about the need for more detailed analyses on the interaction between size and precipitation changes, we have added text to reflect the need for future studies to do this (see lines 603-613).

1. L509: A reduction in the abundance of some fungi will reduce the population of fungivorous nematodes, but not the population of any mesofauna group?

**Response to reviewer:** We have now modified this sentence (lines 584-588) to clarify that it is not only fungivorous nematodes that would be impacted by these changes. This now reads: “Meanwhile, for microfauna such as nematodes, increased precipitation may reduce the overall fungal biomass, reducing populations of fungivorous nematodes (Liu et al. 2020). At the same time some fungal groups, such as saprotrophic fungi, on which mesofauna such as Collembola preferentially feed, are expected to increase under wetter conditions (Sanders et al., 2024).”

**Reviewer 2**

1. In this contribution Martin et al present a meta-analysis to assess the impact of changes in precipitation on soil fauna in forest ecosystems. Their main finding is that a reduction in precipitation results in decreases in soil fauna abundance of -38%, with the opposite trend (+36%) for precipitation increases, and with minor or no effect at all on diversity or taxonomic richness of these communities. Further analyses show that these responses are related with the size of these organisms, with the smaller mesofauna (i.e. body size below 2 mm) being the most sensitive group. Another important aspect of the meta-analysis presented here is the finding of generalized methodological biases, particularly regarding the magnitude of the climate manipulation effects (with experiments closer to future extreme climatic events than to predicted average changes) and also to the size of the experimental plots used (in general smaller than what could be needed to detect changes in population dynamics of bigger and more mobile soil fauna organisms).

**Response to reviewer**: We are pleased that Reviewer 2 considers our findings important and thank them for their considered review.

1. The paper is well written and the analyses performed seem robust. The topic is timely and the results are appealing. My only concern is that their findings largely overlap with the results presented in two recent meta-analyses dealing with the same topic. It is noteworthy that the authors acknowledge and properly cite them in the discussion (Peng et al. 2022 and Bristol et al. 2023).

**Response to reviewer**: We agree that it is important to acknowledge the overlap with the two recent related meta-analyses. However, we also think that it is important to note the differences in methodological robustness of these studies when compared to ours. For another project, we are in the process of undertaking a review where we collate and compare the results from different meta-analyses on the impacts of natural disturbances on soils. For part of this we have assessed the robustness of different meta-analyses, including those of Peng and Bristol, using a standardised tool consisting of 16 questions that has been used broadly [(Haddaway et al., 2020; Romanelli et al., 2021; Woodcock et al., 2014)](https://paperpile.com/c/h2W6U1/PKvk+bF0d+TaYj). Our assessment scores the robustness of both the studies of Peng and Bristol as ‘low’ whereas the current study scores ‘very high’ under the same criteria. Particularly important differences include broader searches that included grey literature, the use of more than one reviewer to check replicability at multiple stages of our study process, and the use of critical appraisal to assess the robustness of results generated by primary studies. Taken together this makes our results considerably more robust than those of previous meta-analyses.

1. The novelty lies, in my opinion, in their focus on differential responses across the body size spectrum of soil fauna. This is interesting although the term mesofauna is an arbitrary grouping based on a 2 mm threshold that conflates a wide diversity of species with disparate ecological niches. While some species within the mesofauna range can be negatively impacted by reductions or increases in rainfall, others can be benefited and it can be difficult to treat them as whole as authors often do in their discussion. For instance, there is an ample literature linking collembola traits, particularly those related with their ability to live at different soil depths, and their sensitivity to drought (e.g. Ferrin et al. 2023 Functional Ecology). The same applies for micro and macrofauna, which are rather arbitrary groupings. Nematodes for instance present multiple trophic levels, which may present contrasting sensitivities to drought. There is just one reference to that in the discussion (line 507). Perhaps more nuance on this diversity of responses within microfauna and macrofauna as well as mesofauna could enrich the discussion and provide additional value to the body size focus of the paper.

**Response to reviewer:** We thank the reviewer for this helpful comment and agree with their point. We have added a new paragraph that argues the need for more nuanced analyses that incorporate multiple traits in the future (see lines 603-613).

1. Finally, another aspect that I miss in the whole manuscript is the lack of references to microbial communities. Although I understand that this is a meta-analysis on soil fauna, it is also clear that their responses to a changing environment are necessarily coordinated with the responses of microbial communities. There are several observational and experimental articles dealing with these trophic connections in soil food-webs. Moreover, water availability directly affects both the amount and the quality of the organic matter entering in the soil. Likewise, water availability also directly affects the performance and the composition and structure of microbial communities. So, a better integration of these aspects, particularly in the discussion but also in the introduction, could strengthen the impact of their findings.

**Response to reviewer:** We thank the reviewer for this useful comment. We have now modified part of the discussion to reflect some of these changes in microbial communities (see lines 560-577).

1. L 32 for clarity provide the exact numbers of change in abundance (-38 and +36 according to figure 2)

**Response to reviewer:** We have now added this detail for the updated results section (see Lines 33-35).

1. L 422: I suggest to avoid writing several times “Hipothesis 1”, “Hipothesis 2”, “Hipothesis 3” etc., simply remind the hypothesis briefly.

**Response to reviewer:** We have now shortened this section and refer to the respective hypotheses as H1, H2, etc. See (see lines 479-493) for details

1. L 545: Peng et al. collated data from 66 primary studies, more than twice of the papers included here. The focus of Peng et al.’s, however, was broader including several global change drivers. I suggest to clarify here that this meta-analysis focused only on forest ecosystems and precipitation changes, and in this sense is perhaps more comprehensive than previous reviews.

**Response to reviewer**: We agree that there is a need for greater clarity in the differences between our study and that of Peng et al. We have now added text that emphasises key differences, including: (i) the focal ecosystem, and (ii) the methodological robustness of our work (see lines 631-635). However, it is also worth noting that despite our sole focus on forest ecosystems , we found more studies on precipitation change than Peng et al, who found 23 studies for all ecosystems, while we found 38 studies solely for forests. Peng et al. only found five studies on the impacts of precipitation changes on soil fauna in forests, compared to our 38 studies. This is reflective of our more comprehensive search strategy, which captures a higher proportion of existing relevant studies, thus making our results more robust and trustworthy than those of other similar studies [(Konno & Pullin, 2020)](https://paperpile.com/c/h2W6U1/pXzV).

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