Rebuttal letter review of manuscript “Soil depth and nutrient status are stronger drivers for short-term production and decomposition in temperate fens than water regime in a climatically dry year”

29 July 2026

“**new lines xx-xx**” refers to the line numbering of the revised manuscript of 29 August 2026

Respond to RC1: 'Comment on egusphere-2025-5352', Anonymous Referee #1, 16 Dec 2025

General comments

1. The manuscript by Aggenbach et al. addresses a timely and important topic: the effects of drainage and rewetting on biomass production and decomposition, key processes underlying peat formation and carbon storage in temperate fens. The study is thorough, with a large number of sites and well-executed methods. The finding that nutrient availability and soil depth had a stronger influence on productivity than hydrological status, particularly during an unusually dry and warm season, is insightful and highly relevant given the increasing frequency of European heatwaves. This result has important implications for peatland restoration, suggesting that rewetting alone may not be sufficient to restore carbon sequestration potential and that nutrient regimes and peat depth should also be considered.
2. A clearer introduction and a more concise, confident conclusion would strengthen the abstract and better communicate the significance of the study. The methods are generally sound, though a justification of the ingrowth core substrate for belowground production and clarification of how the small number of replicates per site were incorporated into statistical analyses would strengthen the manuscript.
3. Overall, the study provides valuable insights into the drivers of fen ecosystem functioning under changing climate conditions and will be of interest to the peatland and carbon cycling research community.

   Reply:
   We thank the reviewer for the compliments. Our reaction to comments is covered in our reaction to specific comments below.

Specific comments

Abstract:

1. The abstract would benefit from a clearer introduction to the broader context and importance of the research topic. It currently opens with the study hypotheses but does not provide sufficient background on why the topic matters. I recommend beginning with a few sentences that highlight the role of peatlands as major carbon sinks, the existing gaps in our understanding of carbon cycling in these systems, and how this study aims to address those gaps. For instance, *in-situ* measurements of biomass production and decomposition, particularly belowground, are still

underrepresented in fen peatland studies, even though they are crucial for understanding the effects of drainage and rewetting on biogeochemical cycling and carbon storage in these managed ecosystems.

Reply:
We thank the reviewer for pointing this out and agree that it is important to include the broader context. We have added a new introductory part to the abstract (**new lines 30-41**).

2. The results section of the abstract is detailed, but the emphasis on hypotheses could be reduced in favor of clearly presenting the most important findings and their implications.

   Reply:
   We followed this recommendation and changed the text (**new lines 45-55**).

3. The final sentences currently sound overly cautious and may unintentionally undermine confidence in the results. Rather than framing the extremely dry and warm growing season as a limitation, I encourage the authors to consider discussing the relevance of these conditions, given that many peatlands have been experiencing similarly warm and dry summers over the past decade(s). These observations are not simply anomalies but reflect ongoing climate change, which makes your results particularly timely and valuable.

   Reply:
   We agree and thank the reviewer for this recommendation. We have now changed the part about influence of the hot-dry summer and added some reflection on effects of such events and climate change (**new lines 56-52**) which were addressed in the discussion (**new lines 802-825**).

Introduction:

4. **L59–60:** Please clarify whether this statement refers to a global context.

   Reply:
   We have added “on a global scale” (**new line 67**).

5. **L72–73:** Reference is missing for this statement.

   Reply:
   Reference Jaszczuk et al. (2024) has been added (**new line 83-84**).

6. **L72–73:** The end of this sentence is unclear. I suggest rephrasing as: “In fens, peat accumulation is strongly influenced by belowground biomass from vascular plants, especially sedges (*Carex spp.*), which often provide the largest share of organic inputs.”

   Reply:
   We reformulate the sentence (**new lines 82-84**)

7. **L76–77:** End the sentence after “rare” and start a new sentence afterwards.

   Reply:
   Done (**new lines 89**).

8. **L76–80:** Some redundancy exists regarding the scarcity of belowground data. I suggest merging these lines into one sentence: “In situ observations of belowground biomass production and root responses in fens are generally rare…”

   Reply:
   Done (**new lines 83-84**).

9. **L83:** Reference is missing.

   Reply:
   We have added two references (**new lines 96**).

10. **L88–89:** Consider naming some of the abiotic factors you investigated, providing examples, or simply referring to Fig. 1.

    Reply:
    We have added main factor groups (**new lines 111-113**), but we do not yet refer to Fig. 1, because another figure is inserted before this figure, and prefer not to refer to a figure before others with a lower number are referred (**see comment 12**)

11. **L93–94:** Rather than “displacement peat,” you might say “less peat is formed by root ingrowth”, which is more straightforward and emphasizes the relevance of your study.

    Reply:
    Because of a comment of reviewer 2 we removed hypotheses 3 (**comment 11 reviewer 2**).

Methods:

12. The large number of sites and the broad geographic and climatic coverage are a major strength of the study. A map showing the spatial distribution of the 39 sites across Europe would greatly help readers visualize this gradient.

    Reply:
    We included a map with the research sites (**Fig. 1**).

13. I suggest citing Table S1 of the supplements already in the opening sentence of the site description, so readers immediately know where to find detailed metadata on site characteristics and history. This would facilitate understanding of the regional grouping and site categorization.

    Reply:

We have added a reference to Table S1 at the start of the site description in order to indicate directly where meta-information for the single sites can be found. (**new line 133**).

14. Further, the description of rewetted sites would benefit from more explicit information on management history, such as the approximate time since rewetting for individual sites or site groups. While the authors note that rewetting occurred “a few years to several decades”

    We agree and have added details on the rewetting date of all sites (**new lines 165-169**).

15. (**L116-117**) prior to sampling, a more constrained range or categorical grouping would improve transparency and allow readers to better assess potential legacy effects of drainage and rewetting.

    Reply:
    Done, also compared to response to the previous comment (**new lines 153-156**).

16. **L144:** I acknowledge the substantial logistical effort involved in implementing ingrowth cores across such a large number of sites. I also understand that using autochthonous peat material is often not feasible on this scale (three replicates per site across 39 sites makes n=117 cores in total?; please confirm).

    Reply
    Yes, is right. an changed a sentence to: “After re-collection in 2018, each of the 113 (of 117) ingrowth cores retrieved intact” (**new lines 209-210**).

17. However, the sand–nylon substrate differs markedly from peat in key properties relevant for root growth, despite the authors' attempts to resemble the average peat bulk density of all sites. Variables such as water-holding capacity, nutrient availability, oxygen conditions and microbial activity must differ substantially between peat and sand substrates. Justifying this choice and discussing how these differences may influence root and rhizome ingrowth relative to the surrounding peat would help readers to better interpret the belowground production estimates.

    Reply:
    We did some validation of our measurements with respect to the substrate in the ingrowth cores, but did not add the result to the initial text in order to limit the length of the manuscript. Now, we have added it to respond to the clear demand for explanation. We also refer to a methodological study of Laiho et al. (2014) who compared the effect of substrate type (**new lines 198-201**) and address his findings in the discussion (**new lines 646-651**).

18. **L145:** Since the dominant species are sedges (**L258-269**), please explain how vertically installed ingrowth cores capture realistic belowground production in species that have horizontal rhizome growth but also, to a great extent, vertical root growth. Could that have led to an underestimation of root compared to rhizome growth or

even an underestimation of root production in deeper soil depth (**L450-452)**. If yes, was this accounted for?

Reply:
We agree that vertical in-growth cores can potentially cause bias with regards to root orientation. To our knowledge, though, there are no direct comparisons published between vertical and angled or even horizontal cores which would allow for a quantification of this potential bias. We now have added a statement addressing this to the text (**new lines 221-225**).

19. **L160-161:** Please also indicate the mesh size of the Rooibos bags then.

    Reply:
    Added (**new line 231-232**).

20. **L200-206:** Please add a formula to this description of calculating the peat formation potential. Is this method based on any reference?

    Reply:
    We have added formula and a reference (**new lines 304-307**).

21. **L275-302:** The authors state that replicate measurements were averaged per site and that these site means were used for subsequent statistical analyses. This approach is appropriate for avoiding pseudoreplication, however, it would be helpful to clarify consistently which explanations apply to which analyses. In particular, for the random forest analyses it remains unclear whether site-averaged values or individual replicate measurements were used, and how within-site dependence was handled in this context. Given the relatively small number of replicates per site and the high spatial heterogeneity typical of fens, a brief clarification of the unit of replication for each analysis would strengthen confidence in the robustness of the results.

    Reply:
    With our analyses we focused on between-site variability, and with three or five replicates it makes not much sense to account for intra-site variability. We have added to section 2.8 (Analyses of the effect of hydrological status and soil depth) and section 2.9 (Impact of environmental factors on plant production and plant biomass decomposition) that we used site-averaged values of productivity and decomposition (**new lines 398 and 420**).
    We have added to section 2.3 (**new lines 215-219**) and 2.4 (**new lines 265-268**) the number of replicates per site for productivity and decomposition measurements and its derived variables.

22. Just a site note, why was the ANOVA performed with a different R version (4.2.) than the random forest analysis (4.3.1)?

    Reply:
    This had a practical reason: ANOVA was done by another person on another computer than the random forest analysis.

Results:

23. **L325:** “mass loss of local/root belowground biomass”

    Reply:
    It is not clear what the comment is.

24. **Figure 2 and 3: Several** figures in the manuscript use color schemes that may be difficult to distinguish for readers with color vision deficiencies. Ensuring figures are colorblind-friendly is important because it improves accessibility, allows all readers to accurately interpret the data, and avoids misinterpretation of patterns or trends. I recommend using color palettes that are designed for colorblind accessibility (e.g., viridis, RColorBrewer, scico or colorspaces are some examples for R packages I can recommend) or adding distinct symbols/patterns in addition to color to convey information. Very helpful tool: https://www.color-blindness.com/coblis-color-blindness-simulator/

    Reply:
    Thanks for the useful tip. We have changed the colours to the Okabe–Ito palette which was specifically designed to be colour-blind friendly and remains distinguishable when printed in grayscale because the colours differ strongly in luminance. Because of comments of reviewer 2 we combined Fig 2 and 3 into a new **Fig. 3**

Discussion:

25. **L539-556:** I assume the conditions observed during the study year were extreme compared to long-term averages calculated over several decades. It would be helpful to put these conditions into context relative to the last decade as well. Were such warm and dry conditions still very unusual, or have similar events become more frequent? Providing this context would help readers interpret the observed patterns of productivity and decomposition in light of ongoing climate trends.

    Reply:
    Thank you for this suggestion. We agree and have added a dedicated section to the discussion (**section 4.4**). There we also make clear the meteorological conditions were exceptional, and will become more frequent (**new lines 787-840**).

26. **L502:** A reference is missing.

    Reply:
    We have added two references (**new lines 738**).

Technical corrections

27. *in-situ* in italics throughout the text

    Reply:
    Has been done.

28. Please use spp. instead of spec., as it is the correct and widely accepted abbreviation for multiple species within a genus.

    Reply:
    Has been modified accordingly.

29. **L106:** Delete one “a” or move the first “a” in brackets.

    Reply:
    Done (**new line 136**)

30. **L271-272:** Please introduce abbreviations of carbon and nitrogen and phosphorous, potassium, FAAS as well as CFA. Check the document for more of these unexplained abbreviations.

    Reply:
    We did and explained other abbreviations

Respond to RC2: 'Comment on egusphere-2025-5352', Anonymous Referee #2, 14 May 2026

General

1. I was excited to review a manuscript on this important topic that has thus far received too little attention. Also, I was delighted to see the number of sites in the material. An acknowledged complication of the one-year study was that the year happened to be exceptionally dry. In peatlands, where the water level is a critical process regulator, this likely affected the results. The title framed this nicely, the practical issues such as, e.g., the decomposition frame upheaval were described in Methods, and the authors discuss the potential impacts at the end of Discussion. However, I found the discussion part to remain quite disconnected, and suggest that the potential impacts of drought are already concisely discussed in the previous sections of Discussion along with the results of the study. The subject matter in the specific section (lines 539-556) can easily be split to be presented with the general discussion on biomass production, decomposition, and – ultimately – peat formation potential.

   Reply:
   We thank the reviewer for this positive evaluation of our study.

2. Also, I was wondering if the drought could be brought up in the Introduction, including the hypotheses. It seems to me that the hypotheses remained on a general level, not considering the drought, even though the title would allow, and even suggest, drought effects to be incorporated in this part of the manuscript, too, even though they cannot be directly estimated based on the one-year data with no “regular year” as comparison.

   Reply:
   We agree with the reviewer that contrasting effects of drought with a non-drought year would be interesting. We have conducted our study in an exceptionally dry and hot year (acknowledging that these exceptions will likely happen more often in the future), and have made this transparent by including this already in the title. At the same time, we can only speculate as to the differences in comparison to a “regular” year, as we indeed have no direct comparison. We have included respective speculations on drought/ hot-specific effects in the Discussion, e.g., ll.:
   As we do not have a direct comparison between a drought and a non-drought year, this precludes formulation and testing a hypothesis on the importance of drought vs. non-drought.

3. The Material and Methods section would benefit from some added information; see detailed comments. Also, please describe on which grounds you have estimated that the undrained, drained, and rewetted sites represent originally similar fen types to the extent that comparing them is relevant.

   Reply:
   Please refer to point 14 below for details.

4. It is good that pairwise comparisons of the sites were not focused on in the analysis, but a more general evaluation of patterns between the hydrological status groups was done. The extensive data were rigorously analyzed with RF. Yet, it occurred to me that the use of some further analytical method, such as structural equation modeling, to analyze the direct and indirect effects of your impressive explanatory variable set on production and decomposition, and, ultimately, the organic matter accumulation potential, might give further insights into the functioning of the fens. With such an analysis the authors could evaluate a conceptual model on how the different variables are linked to each other, formulated based on the their ecological knowledge of the systems. Not sure how well the data would facilitate the use of that method, but it might be worth exploring (if not tested yet).

   Reply:
   We agree with the reviewer that structural equation modelling or even representation learning would be promising research direction. At the same time, in this study, we are aiming to derive general patterns, as stated by the reviewer. Further modelling goes beyond the scope of this study, and should take into account non-drought years as well. Within the framework of the joint project WETSCAPES2.0, we are currently running similar trials across years, and also have the required modelling expertise on board. We therefore refrain from adding more analyses to the paper under review and keep the suggestion for future papers!

5. Concerning presentation, the language needs some polishing: for instance, word order is not always the clearest for easy understanding the statements, words or punctuation are missing in some sentences, and the spelling of some words, such as phosphorus, need checking. I will not be detailing these, since easy tools for language check are available. Restructuring some paragraphs would improve the flow of the text and understanding the progress, both in Methods and in Results.

   Reply:
   We thank the reviewer for pointing this out and have thoroughly reviewed the manuscript for correct and clear language use.

   Detailed

6. Line 50: "…this difference will not…" – I would tune this down a bit ("may not") as the decomposition rates measured were likely affected by the drought.

   Reply:
   We have changed this statement, also in the light of reviewer #1 comments **(new lines: 45-55)**

7. Lines 53-55: rather than the bit vague last sentence, you could simply state something like "This should be borne in mind when interpreting the results".

   Reply:
   Thank you for this remark. We have added such a statement **(new lines 57-58)**

8. Line 60: perhaps specify “carbon sink function through peat accumulation”, as in some cases, drained peatland forests may remain a carbon sink at the ecosystem level and even show small soil C sink.

   Reply:
   Thank you for raising this. Evidence on long-term climate benefits from afforestation on drained peatlands is lacking and it is unclear whether CO2 sequestration of forest on drained peatland can offset the carbon loss from the peat over the long-term. While afforestation may offer short-term gains in certain cases, it compromises the sustainability of peatland carbon storage (Jurasinski et al. 2024, https://doi.org/10.1007/s13280-024-02016-5). As we must be brief and concise in the introduction, we prefer to keep the more general original statement.

9. Lines 69-70: The phrase “Whether rewetting…” repeats the end of the previous paragraph.

   Reply:
   Indeed, thank you for pointing at this. We have deleted this phrase **(new line 79-80)**

10. Lines 78-81: While these statements are still generally valid, there is literature on these topics that you seem to have missed both here and in Discussion. The reference from 26 years ago paints a somewhat gloomier picture than what is currently available. I am listing here some papers that might be of interest, but please note that I am not suggesting that you necessarily need to cite any of these:
    https://doi.org/10.1139/b96-116
    https://doi.org/10.1139/b03-043
    https://doi.org/10.1111/gcb.13934
    https://doi.org/10.1016/j.scitotenv.2022.159683
    https://doi.org/10.1007/s10021-005-0070-1
    https://doi.org/10.1890/0012-9658(2000)081[3464:ROBAFP]2.0.CO;2
    https://doi.org/10.1111/jvs.12912

    Reply:
    We are very grateful for pointing us at several related studies that were not yet known to us. Also, in the light of the studies listed above, we find it still justified to mention the scarcity of in-situ below-ground studies. This is particularly true for studies on rewetted fens, especially in the temperate region. We have modified this section and integrated more evidence from literature. (**new lines 89-92)**

11. Line 93: I think that you are not able to examine hypothesis 3 as currently formulated, since one year is too short for examining peat formation, and also your results will only reflect drought conditions. Please revise.

    Reply:
    We removed hypothesis 3 from the text, have rephrased this that we assess the effect of rewetting on peat formation potential by evaluating the patterns in above-

and belowground production and decomposition between undrained, drained, and rewetted fens (**new lines 107-108)**.

12. Line 97: Instead of “in the top, as well in the subsoil” you could define the actual depth range.

    Reply:
    We have indicated the depths (**new lines 94).**

13. Lines 103-104: Please refer to Table S1. Also, I suggest adding a figure with a map and a schematic presentation of the study design, including the different measurement points. The latter would help following the method description, which is a bit complex simply because you measured so many parameters – which is of course excellent as such.

    Reply:
    We have added a reference to Table S1 (**new lines 103)** and added to the Supplement a schematic figure depicting the layout of measurements (**Fig. S1**).

14. Line 108: Classification into hydrological states may be of less concern than including sites that are actually comparable (originally the same ecosystem type), so please add here also a description of how you ensured that was the case.

    Reply:
    We have added a description how we ensured that selected sites were originally the same ecosystem type (**new lines 139-145)**. In short, all sites were checked for being groundwater-fed fens with original peat-formation before drainage by small sedges and brown mosses. Furthermore, distance between the triplets per region was kept to a minimum in order to achieve comparability by spatial proximity.

15. Lines 123-124: Mowing is likely to impact the ecosystem structure, including plant biomass production, so please tell some more (frequency, timing) about that, in the Supplement if it cannot be done concisely. Mowing of undrained sites means that they are not in a fully natural state, which is important to bear in mind when comparing them to unmanaged fens.

    Reply:
    We have added in the main text information on frequency, timing of and type of machinery (**new lines 165-169)**. We acknowledge that fens in a natural state are not mown and that mowing and grazing affects input of aboveground biomass litter, and may also affect input of belowground biomass. Although we aimed for selecting drained sites with homogenous vegetation management across Northwest-Europe, we could not fully standardise this. In many parts of or study’s extent, mainly Northwest Europe, even undrained fens are mown. In contrast important examples of undrained fens in Central-Eastern-Europe (Poland) are not mown. Site management also varied among rewetted and drained sites across the 13 regions. In the discussion, we therefore now have added some (potential) effects of vegetation management on peat forming potential (Section 4.3, **new lines 759-764**).

16. Line 127: the subheading remains a bit uninformative – could you rephrase to exclude the vague “other variables”?

    Reply:
    We have changed the heading to “Variables included for plant biomass production and decomposition, and for abiotic and biotic conditions” (**new line 176-178).**

17. Line 129: Are your functional plant groups the same as the more commonly used plant functional types? In any case, please give some more information on these, e.g., in a supplementary table. Could you actually list the contents of these for all sites? The information that you currently give on vegetation composition is very scarce, just the dominant plant species in Table S1.

    Reply:
    Yes, we are using ‘functional plant group’ as a synonym for ‘plant functional types’. We have added a table to the supplement with species cover of the sites and corresponding functional plant groups for each species (**table S8**).

18. Line 144: Perhaps a discussion matter: Laiho et al. (2014; https://doi.org/10.1007/s11104-014-2225-3) compared peat versus inorganic substrates in ingrowth cores in boreal sedge fens, finding that under dry conditions inorganic substrates yielded lower belowground production estimates than peat. This suggests that you may have somewhat underestimated belowground production.

    Reply:
    Thank you for pointing out this interesting reference. We have now referred to the findings of Laiho et al. (2014) on the effect of ingrowth filling material in the methods section **(new lines 199-201),** and also in the discussion **(new lines 646-651).**

19. Line 147: The pooling of the two layers probably means that you cannot visualize the depth distribution of belowground production? That would have been interesting to see as a supplementary figure.

    Reply:
    Yes, indeed, pooling the two layers for dry weight measurements impedes insights into the depth distribution. We acknowledge that this would have been interesting, but simply lacked the time to separate the samples by all depth layers.

20. Line 155 onwards, section 2.4: This section, especially, could become easier to follow through some reorganization. Also, could you please describe what the “representative plant materials” were for each site, and provide a schematic figure of the frames, in the supplement? On line 183, please specify the length of the incubation period(s). Perhaps note in the Discussion how the exclusion of meso- and

macrofauna may have affected the mass loss rates. Also please discuss a bit the limitations of one-year decomposition data, which does not necessarily correlate with longer-term mass loss dynamics (e.g., Latter et al. 1998: https://doi.org/10.1007/s004420050357 ; Moore et al. 2017: https://doi.org/10.1007/s11104-017-3228-7).

Reply:
Following the reviewer's suggestion, we have substantially re-organized the section 2.4, including mentioning the length of incubation (ca. 365 days). We have now skipped "representative" from the description of the plant material **(new lines 245-247).** We assume that the applied method results in reasonably representative samples of the local biomass.
We have added the implications of measuring decomposition only for one year to the discussion **(new lines 727-735)** and are careful with not to extrapolate to long term peat formation but focus on relative comparisons between sites and conditions. We did not add a schematic figure of the frames in the supplement, in order not to expand the supplement to much.

21. Line 204 ,"we used the averaged…": please explain this specifically for the two different depth segments. Or did you combine these two (which would make no sense)?

    Reply:
    We pooled the two layers without mass loss measurements (5-15 and 20-45 cm) for dry weight measurements (see comment 19). We used this pooled biomass also to calculate the total peat formation potential across the 0-50 cm depth. As we had no corresponding mass loss measurements for these depths, we used the average of mass loss measurements for the 0-5, 20-25 and 45-50 layer. I.e., we fully agree with the reviewer that calculating a peat formation potential for the aggregated 5-15 and 20-45 cm would not make sense We have re-formulated the respective paragraph to better reflect our calculation, please see **(new lines 311-313)**. We acknowledge that separate measurements per depth layer would have been preferable, but simply lacked the time to do so when processing all described samples for biomass, and when setting up mass loss experiments.

22. Line 261: can you give a reference for TURBOVEG?

    Reply:
    The following has been added: Hennekens, S. M. & Schaminée, J. H. J. (2001): Turboveg, a comprehensive database management system for vegetation data. Journal of Vegetation Science, 12: 589–591 **(new lines 378).**

23. Line 263: What was the division of nutrient-rich versus nutrient-poor conditions based on? I mostly agree, but some species were categorized differently from what I would expect.

Reply:
The division of Cyperaceae into preference for nutrient-rich versus nutrient-poor conditions was based on N indicator values of GermanSL (Jansen & Dengler 2008).

24. Line 266, “feather mosses”: earlier, you call these “brown mosses”. I would stick to that term, since feather mosses commonly refer to species like Pleurozium schreberi and Hylocomium splendens.

    Reply:
    We agree and have changed ‘feather mosses’ into ‘brown mosses’. **(new line 384)**

25. Lines 279-282: one-way ANOVA – so there was no other hierarchy in the data that would have required the use of mixed model even in the simpler case? Can you present the two-factor model structure somewhere?

    Reply:
    We reformulate the description of the two-factor model as “Triplet number identifying the region of the plot triplets was included in the two-factor model as a random effect to account for potential region-specific differences.” **(new lines 404-404).**

26. Results, section 3.1: I am not certain that the first paragraph is needed at all: the detailed results follow in the next paragraphs, and those suffice. For any non-significant patterns that you wish to mention (e.g., lack of interactions), you could add the information after presenting the significant patterns. If the readers need a reminder of which variables were measured, they can check Fig. 1. However, if you choose to keep it, please restructure it so that it simply states for which variables significant patterns were, or were not, found, putting the lists in the actual results statements, not in separate phrases.

    Reply:
    We agree and have deleted the first paragraph of the results section **(new line 446-453).**

27. I would suggest restructuring the section on decomposition. Start with aboveground decomposition, stating whether there were or were not significant patterns. Then tell the same for belowground. That is easier for the reader, and follows the structure of the production paragraph above. It is good to maintain consistent structure where possible. Also, I suggest presenting all production-related results, including the depth patterns of belowground production, before moving to decomposition and ultimately, the accumulation potential.

    Reply:
    We agree and have restructured/ reformulated the respective sentences accordingly **(new lines 455-482).**

28. Lines 329-330: PFP from aboveground plant biomass would be a better phrasing: similarly, PFP from belowground biomass. Please present the accumulation potential results separately, after first presenting also the decomposition results, as those are also needed for the calculation, and understanding of, the accumulation values. Please provide also a total PFP value from both aboveground and belowground biomass.

    Reply:
    We agree with the suggested terminology and restructuring of the content and have applied both to the text **(new lines 485-492).**

29. Line 346: The paragraph starting here currently seems to hold all depth-related results mixed together. While that is one option, I would quite strongly recommend presenting the results rather as per variable type: production, decomposition, accumulation potential. These are the factors of interest, and depth-related patterns are just detailed features of those. Mention if the hydrological status affected the depth-related patterns or not.

    Reply:
    We agree and have restructured the chapter 3.1 accordingly. As a positive side-effect, this also fits perfectly to the merging and restructuring of former figures 2 and 3 to new figure 3 (see point 31 for details) **(new lines 518-531).**

30. Line 360, caption of Fig. 2: b) plant biomass, or plant biomass production? e) and f) Did you pool these that across hydrological status because hydrological status did not significantly affect the patterns, or for what reason? Please tell; do not make the reader to guess. Please rather use an abbreviation that more intuitively makes sense in the English language. A quick reader will think that RT means Rooibos tea. Also, as the end part of the term is the same for both aboveground and belowground, make their abbreviations similarly corresponding as well. On the other hand, I do not see a strong reason for using the abbreviations at all, as it seems that Aboveground and Belowground, defined in the caption to relate to vascular plant biomass, are just as clear without the abbreviations added.

    Reply:
    Thanks for pointing out potential reasons for misunderstandings. We have now clarified all points mentioned in the caption of new figure 3, which combines the former figures 2 and 3 (see point 31) **(new lines 518-531).**

31. Figures 2 and 3: I am quite puzzled why you present the production and accumulation results always together, even though accumulation also depends fundamentally of decomposition. Another option for organizing the figures would be putting all depth-related patterns in a specific figure, including current panels 2 e) 2 f), 3 e) and 3 f). That would be visually more pleasing, and present accumulation in the context of both production and decomposition. While the current structuring would align better with my suggestion of presenting the results in the text as per variable type (and not combining all depth-related patterns together), that is not decisive as you can refer to

multiple figures from one result statement, as also from new statements to figures already mentioned.

Reply:
Thank you for this comment. We have now combined both figures in order to benefit from both options. We now present horizontally each response variable first by hydrological status and then separated by depth and vertically allow for the direct comparison of production, decomposition, and their difference, i.e. the peat-forming potential **(new lines 518-531).**

32. Line 384: “Belowground plant production” – “additionally”, as you have already described the depth patterns in the previous sentence? “Positively linked” or “negatively linked” – I would see it as the latter?

    Reply:
    The statement was doubled in the initial version, and we now have reformulated it without mentioning the depth relationship a second time **(new lines 550-551).**

33. Line 385: Rather than “exceeded”, it would be clearer to state whether the water levels were higher than, or lower than, 10 cm above surface. Further, 10% of what? Bulk density is often presented as kg/m3, or some other mass-per-volume unit, so % may not be intuitively understandable for all readers. You actually have kg per liter as the unit in the figure.

    Reply:
    We have replaced “exceeded” by “were higher than” and “was higher than” in the two occurrences in that sentence **(new lines 550-552)**. Thanks for spotting the inconsistency in units for bulk density, it has now been corrected to kg/L also in the text, here to 0.1 kg/L instead of 10 % **(new line 552).**

34. Discussion, general: Please bear in mind all through the Discussion that you are dealing with an exceptionally dry year, and not necessarily general patterns. Otherwise, you may be unintentionally a bit misleading. I would suggest that you add in each section a concise consideration how the drought summer may have affected the results, and which patterns may be considered more generally valid. You have a lot of knowledge of these systems in the author team and that, with support from earlier published research it should be doable. Whereever you really do not feel that you can estimate the directions of the drought effects, please tell that, too. As all the sites were mowed or grazed, please also mention whether you think that may have affected plant biomass production and if so, how.

    Reply:
    Please compare to point 2 above. A statement on the potential importance of the extreme year for each single response variable would be highly speculative, as we lack data from “normal” years for comparison. Therefore, we prefer to combine this important topic in one section of the discussion and not in every section. In order to set the stage for this general discussion and to raise awareness from the start on, though, we now clearly point out this fact already within the first paragraph of the

discussion. Please note that literature on effects of extreme low water tables and high temperature on productivity and decomposition in fens is rare, making it hard to confirm or reject speculations on the importance of the extreme conditions. Consequently, we are now explicitly marking this a knowledge gap that requires future attention **(new lines 59-62 in the summary and 838-839 in the discussion).**

35. Lines 441-446: Could these patterns be caused by the dry year? Please refer to earlier published results on the relationship between biomass production and WL for fens for a bit more insight on this.

    Reply:
    We looked for literature on this relationship but did not find respective publications for fens. However, some studies indicate that aboveground productivity in the fen types we studied is unrelated or not only weakly correlated with the water level gradient **(new lines 619-624)**.

36. Lines 451-452: How was the depth distribution compared to other studies in fens?

    Reply:
    We have now compared the depth distribution with other studies from fens using ingrowth cores/ bags over one year or one growing season. These studies indicate a similar pattern **(new lines 634-635)**.

37. Lines 464-468: This should rather be in Results than Discussion.

    Reply:
    The specified sentences indeed do only describe results. Therefore, we changed the text by shortly wrapping up these results **(new lines 665-669).**

38. Line 482: “overrules” - Could this be because the water levels were generally low? How does this relate to other studies that examined the effects of WL or hydrological status on decomposition? E.g., https://doi.org/10.4141/cjss88-073 ; https://doi.org/10.1111/j.1365-2486.2011.02503.x

    Reply:
    Thanks for this remark. We doubt that lower water levels during summer and autumn caused by the extreme dry weather can account for the complete absence of any influence of water level on decomposition. Not all sites suffered from strong water level dropdowns and a part of the rewetted and undrained sites still had high water levels close to soil level (**new lines 691-694).** These were sites where groundwater flow to the fen system strongly buffered water loss through evapotranspiration. We have now compared our results to the publications suggested (**new lines 683-691**).

39. Line 483: What might these ”other factors” be?

Reply:
We have now added some examples of possible factors that were unaccounted in our study **(new lines 695-696).**

40. Line 487: Based on other studies utilizing rooibos, what does rooibos mass loss generally describe? How does it generally relate to native litter mass loss?

    Reply:
    Due to its low hydrolysable fraction, Rooibos tea decomposes slowly and therefore serves as a standardised proxy for recalcitrant local litter. We have added this info in the method section with references to studies that characterize Rooibos tea decomposition **(new lines 232-234).**

41. Lines 494-495: This is quite surprising, and may suggest that the surface was too dry for efficient decomposition because of the drought. Can you please check other studies on belowground decomposition in peatlands and discuss a bit further?

    Reply:
    Thank you for this valuable remark. We have now compared our result with the vertical pattern in one of the few available studies. The pattern in the study of Straková et al. (2011) surprisingly is opposite to the pattern we found. We have now evaluated some possible explanations (like using sand-nylon grain filled ingrowth cores, extreme high soil temperature, low soil moisture) and have added this to the discussion **(new lines 707-716).**

42. Line 501, heading: Please rephrase to peat formation potential

    Reply:
    We have rephrased the header **(new line 736).**

43. Lines 519-520: Is this in Results? If not, please add; even more generally, the belowground to aboveground ratios are of interest.

    Reply:
    We have added the respective values and statistical tests of the ratio of belowground to aboveground vascular plant peat formation potential to the results section **(new lines 488-489)**. The respective results on the belowground:aboveground ratio for production were already listed in the result section **(new lines 461-463)**.

44. Line 521: Rather “likely contributes”, bearing in mind that your year was exceptional.

    Reply:
    We agree and have reformulated the text **(new lines 757-758)**.

45. Lines 524-525: Again, what might they be?

    Reply:
    We have added a reference to section 4.2, where we have added some possible unaccounted factors (**see comment 39**).

46. Lines 528-529: How about the potential impact of the drought?

    Reply:
    We have added the possible impact of drought on the below ground decomposition pattern with a reference to section 4.4, where the impact of extreme dry and hot weather is discussed more extensively **(new lines 795-825).**

47. Lines 545-550: Perhaps condense this to a simple statement that respiration-based results have varied, since you did not measure respiration; rather focus on discussing factors included in your data.

    Reply:
    We have shortened the statements on ecosystem respiration and have added statements from other research in fens on effects on productivity, decomposition and peat-formation potential. For this, we also used the references suggested in comment 10 **(new lines 797-825).**