Dear editor,

We hereby submit a revised version of our manuscript entitled "Phosphorus stress strongly reduced plant physiological activity, but only temporarily, in a mesocosm experiment with Zea mays colonized by arbuscular mycorrhizal fungi". We thank the editor and the two reviewers for their positive and constructive comments. Below you can find our point-by-point answers (in blue). Line numbers refer to the revised manuscript with tracked changes.

Sincerely,
Melanie Verlinden, on behalf of all co-authors

REPLIES TO THE EDITOR

Two expert reviewers agree that the revisions of this paper have taken care of the reviewer's comments on the original version, and I concur. Both reviewers, however, still have a few minor comments that the authors should address before final acceptance. In addition, since the question of suitability to BG did come up again, it seems that in addition to adding a par to the Intro, the relevance to BG should also be revisited in the Discussion. Also, the links to the companion paper of Verlinden et al., 2018 could be emphasized.

REPLY: We have modified the manuscript in line with the reviewer comments. We have also expanded the conclusion of the revised manuscript to reiterate the relevance for Biogeosciences (l.348ff):

To conclude, low P availability significantly decreased photosynthetic capacity, associated with reduced concentrations of photosynthetic enzymes and pigments. In contrast to the expected increase in nutrient stress because of further depletion of the soil as the growing season progressed, nutrient stress decreased over time and for most leaf processes, pigments and enzymes under study, the fertilization effect had disappeared two months after planting. Our results point towards a key role for the AMF-symbiosis and consequent increase of P uptake in explaining the vanishing P stress. These results add to the mounting evidence of a key role of mycorrhizal fungi in mediating plant responses to environmental changes (e.g., Vicca et al., 2009; Terrer et al., 2016; Parihar et al., 2020). This emphasizes the need to take into account not only nutrient availability, but also mycorrhizal symbionts when studying and modelling photosynthesis and carbon cycling in terrestrial ecosystems.

The link with the paper of Verlinden et al. (2018) was further emphasized by removing the reference to the follow-up experiment in the methods section (following the suggestion of reviewer 3) and through some rewording in the first paragraph of the discussion (l. 244):

End-of-season dry biomass reached 81 (± 7) and 510 (± 24) g m-2 for the non-P-fertilized and P-fertilized treatments, respectively (as reported in an earlier publication of this experiment; Verlinden et al., 2018).

REPLIES TO REVIEWER 3

This is the first time I am reviewing this manuscript. The authors have fully addressed the concerns that were raised during the first review round. As such, I only have a couple of minor suggestions:

REPLY: We thank the reviewer for this positive assessment of our work and have taken into account the minor suggestions.

L89-92. While I appreciate that the authors tried to address comments by the previous reviewer, I don't think that mentioning the Ven et al. 2020b experiment helps to improve clarity. If anything, readers might wonder why you bring up this experiment if you are not going to talk about it. If the authors feel that the results of the Ven et al. 2020b experiment help to put the current results in context, then I suggest they bring up this study in the discussion; please make sure to emphasize the difference between both studies ("these results were supported by Ven et al. 2020b, who found in a separate experiment using .... that..." or something along those lines. Alternatively, the authors could choose to just leave out this reference altogether.

REPLY: We agree with the reviewer and have removed the reference to the Ven et al. 2020b publication to avoid confusion. The Ven et al. study is still mentioned in the discussion, where we clarified that this is a follow-up experiment (l. 346).

- In the methods section, please mention the date at which the maize was planted. This will help the reader to figure out at which growth stage the campaigns were conducted

REPLY: The planting date was 20 May 2016, as indicated on l. 88-89.

REPLIES TO REVIEWER 4

I like the comments from both reviewers, and I think that the authors have done a good job for the revised manuscript. I may have the following minor comments.

REPLY: We thank the reviewer for this positive assessment of our work and have taken into account the minor suggestions.

Line 20-25. “Previous research indicated that N addition did not affect plant growth and also the leaf measurements in the current study were unaffected by N addition.” It is interesting that N addition had no effects on plant growth, which is contrasting to many other related studies. Not sure if they want to keep this sentence in the abstract. Any more discussion is required.

REPLY: We modified this sentence in the abstract to better clarify the (lack of) N effect. We decided to keep this information in the abstract because the reader would otherwise wonder what the N effect was. The revised text (l. 22-24): *A previous study on this experiment indicated that N availability was generally high and as a consequence, N addition did not affect plant growth and also the leaf measurements in the current study were unaffected by N addition.*

A little more discussion on how plant and soil microorganisms will adapt to P stress is needed. See some examples here, Chen et al., 2020, https://doi.org/10.1111/gcb.15218.

REPLY: It is unclear what extra information the reviewer wishes to see included here. The publication referred to is a meta-analysis on how nitrogen addition can alleviate phosphorus limitation in terrestrial ecosystems. It does not specifically deal with adaptation to P stress. It does mention that plants can adapt to/overcome P limitation through e.g. changes in root morphology, root exudation and mycorrhizal fungi. In our manuscript, we already discussed the role of AMF for plant P uptake. This mycorrhizal symbioses is believed to be one of the most successful strategies to maximize plant access to available P and thus overcome P stress. We now added that plants may also adapt to P stress through changes in root morphology and root exudation, but that we have no data to confirm this. This information is included in the revised manuscript (l.336ff):
Also other adaptations to P stress (e.g. changes in root exudation and root morphology may have occurred, but these were not investigated in this experiment. In any case, given that the increase in photosynthetic parameters in the non-P-fertilized plants was associated with AMF colonization, while in the absence of AMF the plants that did not receive extra P died prematurely, strongly indicates that the AMF-strategy was critical for plants to overcome P stress in our experiment.

Here is another evidence on the role of AMF and the associated effects on P cycling, which can be considered in your discussion. Hu et al., 2022, https://doi.org/10.1111/gcb.16135

REPLY: We thank the reviewer for pointing us to this interesting publication and have referred to it in the discussion (l.326).

The authors may need some modifications on their writing, particularly the writing assurance.

REPLY: We reworded a some sentences in the discussion to improve the writing assurance. See e.g. l. 289 and l.293.

It is very difficult to read C1 in your figure 2.

REPLY: The values for C1 were (close to) zero and are therefore not visible on the figure. Because we want to emphasize the difference between C1 and C2, we prefer not to add axis breaks (which would be needed to visualize the details of both C1 and C2 values). Instead, we added a note to the legend to clarify that C1 values were negligible (l.241): *Note that the values for C1 were (close to) zero, indicating that root colonization with AMF was negligible..*