Interactive comment on: "Roots induce hydraulic redistribution to promote nutrient uptake and nutrient cycling in nutrient-rich but dry near-surface layers" by Jing Yan and Teamrat Ghezzehei.

Response to Reviewer Comments #2

June 15, 2022

General Comment

In the paper entitled "Roots induce hydraulic redistribution to promote nutrient uptake and nutrient cycling in nutrient-rich but dry near-surface layers", the authors present a mathematical model of soil hydraulics, aimed to investigate the effects of soil modification by rhizosphere deposition on water and nutrient uptake. I think the mathematical model and the analyses presented in this paper are sound and of scientific value, but I do have two main issues with the presentation of the work. First, I think the framing of the work is not consistent with the methodology, and should be reconsidered. Second, I think that the writing is not up to the standard I expect for publication, and that the paper requires a thorough proof read to improve the text.

The authors present the following knowledge gap in the abstract of the paper: "whether hydraulic redistribution is a passive happy accident or a process controlled by plants remains unclear". In the introduction, the authors state: "The exact mechanism by which roots can induce HR is, however, not known. Here, we present a modelling study that demonstrates that alteration of rhizosphere soil by rhizodeposition facilitates HR". I am of the opinion that this knowledge gap is not one that can be filled with the mathematical modelling approach presented in this paper. The fact that one can simulate HR by changing rhizosphere properties in a mathematical model does not provide evidence that real plants can do the same, and it will this not shed light on the mechanisms by which roots can induce HR. I am of the opinion that the paper should be reframed to focus on the results that show how changes in rhizosphere properties differentially affect water uptake, nutrient uptake, and nutrient mineralisation in different soils and soil water conditions. This can then be framed as a series of hypotheses that should be tested with experiments: namely that plants

are able to modify soil properties in the way that was tested with the model through rhizosphere depositions, and that the exudation of these deposits is more common or more pronounced in soil types and soil water conditions where the model predicts the largest increase in the uptake of either water (under wet conditions and in sandy loams) or nutrients (under dry conditions and in sandy loam). However, I think that the authors currently overextend the impact of this work in sections 4.3 and 4.4. I think framing this work in the context of climate change is not appropriate in the paper's current form, and a statement such as the one made in L. 363-365 ("Our results indicate that answering questions about plant adaptation to complex and changing soil and environmental conditions requires integrating biotic and abiotic feedback in the soil-plant-atmosphere continuum") does not match up with the model presented in this paper. Either the modelling should be extended significantly to include more than just a hydraulic model, or the framing of this work should not go beyond the metrics simulated by the model (i.e. water and nutrient uptake). This reframing should lead to significant re-writing of the introduction and the discussion (sections 4.3 and 4.4 in particular).

> **Response**: We thank the reviewer for the constructive criticisms and suggestions. We agree that the presentation of the model, its design philosophy, and what it can and cannot answer were not presented clearly. The revised manuscript, including the introduction and discussion will be much clearer and take into consideration the above criticisms.
>
> The main motivation for this modeling work was to provide a mechanistic explanation for (a) whether roots can actively regulate HR via exudation of rhizodeposits and (b) whether the primary function of HR is for water uptake or nutrient uptake. A significant amount of work done on HR has focused on the water-stress benefit of HR, including a modeling study co-authored by one of us [2] and the references cited therein. Although there are not that many studies that focused on the nutrient cycle/uptake function of HR, an elegant field study by Cardon and co-workers [1] showed evidence for enhanced surface soil nitrogen cycling and nitrogen uptake under HR in sagebrush.
>
> In this study, we set out to develop a physics-based model that retains what we deemed only the essential components of the soil-root-plant system to explore cause and effect relationships that are difficult to conduct in experimental settings. We agree with the reviewer that the model does necessarily reflect the full plant behavior. However, provided that the assumptions and parameterization of a model are consistent with the current knowledge, we believe models (including ours) are effective tools for providing explanations that are direct outcomes of the current understanding. Specifically, the conclusions that we presented in this paper are direct consequences

of the rationale and assumptions that we used in constructing the model.

As simple as it is, our model captured the complex feedback by which plants can adapt to heterogeneous resource distributions in soil profile. In the context of global change modeling, this model provide a more nuanced explanation of the potential resilience than what is represented by modeling vegetation migration/adaptation models that rely primarily on correlations of abundance with mean precipitation and temperature.

In the revised manuscript, we will provide clear distinctions between conclusions that can be reached from this work alone and open questions and suggestions for future considerations.

I would suggest that the paper is carefully proofread to improve the text, which is currently full of grammatical flaws, especially in the introduction and discussion sections. I will mention a few examples I found in the first couple of paragraphs, but have not put the entire text to this level of scrutiny.

Response: We corrected the issues listed in the examples and thoroughly reviewed the manuscripts to correct the grammatical flaws.

Specific Comments

L. 27: I guess this is the explanation of hydraulic redistribution? Please cue the term here if that is the case.

Response: Yes, we added the definition, "previously defined as hydraulic redistribution (HR)".

L. 28: The current sentence structure reads as if roots modify their immediate surroundings and also modify hydraulic redistribution. Change the order of these two statements to correctly follow up on "... separate advances in our understanding of... '

Response: We modified the sentence to "This study builds upon separate advances in our understanding of how roots modify their immediate surrounding to facilitate HR and HR-driven benefits".

L. 29: Replace '-an' with ', which is', and follow this with a better definition of the rhizosphere.

Response: We changed to sentence to "...the rhizosphere, which is a narrow region of soil in direct proximity to root surfaces where roots and soils interact".

L. 32: Rewrite by switching the order of plant water uptake and wetness of the rhizosphere.

Response: We changed the sentence by switching the orders of those phrases.

L. 33: 'the rhizosphere's carbon investment' suggests that it is the rhizosphere that is doing the investing.

Response: We changed the sentence to "...plant roots' carbon investment in the rhizosphere...".

L. 36: Change 'peculiar' to 'specific'

Response: Changed.

Specific Comment: Finally, I have a couple of minor comments.
In the current analysis, do the results of changes in nutrient uptake include the combined effect of increased nutrient uptake and increased nutrient mineralisation? If so, it would be insightful to decouple the two, presenting both their individual effects and their combined effect.

Response: Yes, it's decoupled. We re-clarified that in the introduction and methodology by specifying our modeling goals and procedures of the decoupling. Please also see our reply to the general comment.

L. 302: What is a rhizoshealth? (L. 302) I presume this should read rhizosheath, but I would still like an explanation of the term.

Response: Changed to "rhizosheath" and added more details on the definition, which is "...the formation of rhizosheath, a portion of the soil that adheres to the roots upon excavation of the root systems, which is considered as a typical...".

Specific Comment: The reasoning behind some of the model's design is explained in detail in the discussion, but I feel this should have been explained in the introduction: L. 305-308 – 'Increased rhizodeposits ... nutrient mineralisation.'

Response: We followed the suggestion by adding more details on the modeling goals in the introduction. Please see our reply to the general comments.

References

[1] Z. G. Cardon, J. M. Stark, P. M. Herron, and J. A. Rasmussen. Sagebrush carrying out hydraulic lift enhances surface soil nitrogen cycling and nitrogen uptake into inflorescences. *Proceedings of the National Academy of Sciences*, 110(47):18988–18993, Nov. 2013. ISSN 0027-8424, 1091-6490. doi: 10.1073/pnas.1311314110. URL `http://www.pnas.org/cgi/doi/10.1073/pnas.1311314110`.

[2] A. Carminati, E. Kroener, M. A. Ahmed, M. Zarebanadkouki, M. Holz, and T. Ghezzehei. Water for Carbon, Carbon for Water. *Vadose Zone Journal*, 15(2):1–10, 2016. doi: 10.2136/vzj2015.04.0060. URL `http://dx.doi.org/10.2136/vzj2015.04.0060`.