

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-52

Anonymous Referee #2

Referee 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, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-52-RC2>, 2022

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).

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.

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

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...”

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

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

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

L. 36: Change ‘peculiar’ to ‘specific’

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.

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

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.'