

There are clear community interests in this review article. We received four expert reviews and one community comment on this manuscript. All the reviewers think the topic is interesting and important. However, reviewers raised issues regarding organization, novelty, and reference to prior work. Reviewers expressed concerns regarding the definition of “root zone”. I concur with the reviewers' assessment. I would recommend a thorough revision of this manuscript with reviewers' comments in mind.

Yours sincerely,
-Lixin Wang

Dear Editor,

We thank the Editor for handling our paper and leading the fruitful discussion. We also thank all referees for their very constructive comments. In the revision, we thoroughly reorganized the structure of our manuscript. The main changes are in these parts:

1. We rewrote and shorten the Introduction, to make it more concise and well-organized.
2. In section 2, we provided a holistic definition of the root zone, its storage, deficit and storage capacity. To make root zone as an entity, we compared it with other similar concepts.
3. In Section 4, we added the subsections on heterogeneities within root zone, especially “plants' belowground zone of influence” and “rhizosphere and mycorrhizal fungi”. All these heterogeneities underscored the limitations of the traditional reductionist approach in modeling this complex and dynamic zone.
4. In Section 5, we discussed the emergent behavior of root zone as a holistic system, and advocated for a shift towards an ecosystem-centered root zone definition and modeling.
5. In Section 7, we added the discussion on “limitation and outlook”, including limited observations, root zone biogeochemistry, making models alive for future prediction, and root zone in planetary stewardship.

We believe this thorough revision answered all the concerns raised by the reviewers.

Best regards,
Hongkai Gao on behalf of all authors

Anonymous Referee #1

Thank you for the opportunity to review this manuscript. It is evident that the authors have made significant contributions to this field, notably through their work on remote-sensing methodologies for monitoring root zone water storage. The concept of emphasizing the root zone as a distinct entity is commendable. However, the manuscript often reads more like a textbook chapter or a commentary rather than a comprehensive review article.

We thank Anonymous Referee #1's endorsement on our “significant contributions to this field”, and his/her commendation on “the concept of emphasizing the root zone as a distinct entity”. We thoroughly improved the structure and writing in our revision.

The structure and content of this paper are reminiscent of the work by Klos et al. (2017), which, surprisingly, is not cited. The authors seem to be aiming to present a similar vision, albeit with a

lesser focus on hydrology. Nevertheless, the level of detail presented does not match that of the referenced work. The manuscript successfully delineates what the root zone is not, through numerous "not equals" subsections. Yet, it falls short in providing a clear conceptual figure or a definitive definition of the root zone. The reliance on a definition from Sprenger et al. (2019) – "The root zone is the upper part of the vadose zone" – raises questions about the exclusion of various types of vegetation, such as phreatophytes, from having a root zone. This definition, alongside the conceptual figures provided, does little to advance the discourse beyond the existing literature.

Thank you for recommending the work by Klos et al. (2017). We have read and made citation. Moreover, in the revision, we gave more detailed discussion on the existing definitions, and provided "a definitive definition of the root zone" in the entire Section 2.

Conversely, the authors demonstrate a deep understanding of calculating remotely sensed water storage deficits. It is curious why this expertise was not leveraged to further explore the potential advancements in this methodology. Questions regarding the nature of the deficit, methods of measurement, spatial and temporal variations, subsurface implications, and the potential for methodological failures warrant a more thorough investigation.

This is a good suggestion. Except for the general review of the importance of root zone across all the spheres of the Earth system, we added more details to further explore the potential advancements of our proposed holistic methodology to investigate root zone storage by landsurface observations, including "the nature of the deficit, methods of measurement, spatial and temporal variations, subsurface implications, and the potential for methodological failures". Please find the details in Section 5 and 7.

Furthermore, the manuscript does not adequately address a range of root zone variables, which are crucial given the ambitious title "Root Zone in the Earth System". The section titled "Root zone estimation approaches" particularly falls short of expectations, focusing solely on storage rather than exploring other vital aspects such as carbon, weathering processes, and water transit times within the root zone.

Thank you for this constructive comment. We added more discussion on carbon and weathering processes in Section 3.3 and 7.2.

In summary, while the authors' expertise is evident, the manuscript could greatly benefit from a more structured review of the literature, a clearer definition and conceptualization of the root zone, and a more comprehensive exploration of the methodologies and variables pertinent to the root zone's role in the earth system.

We thank Anonymous Referee #1's endorsement on our expertise in root zone. We added more literatures in reference, gave a clearer definition of root zone, and made more comprehensive discussion on the importance of root zone in the earth system. Please find the detailed changes in the track-changes file.

Anonymous Referee #2

The authors highlight the importance of the root zone in different disciplines, reviewing the concepts

that can be easily confused with it and the methods used to estimate it. Although I am convinced of the importance of understanding the processes occurring in the root zone to advance in the solution of a wide variety of problems of high research interest, I do not see a clear objective in this manuscript. The authors mentioned a possible confusion in defining the root zone but did not contrast them or show examples. Most importantly, they did not define the root zone clearly but posed many “root zone is not”. Besides, I did not find clearly which type of article this manuscript pretends to be since it is not detailed enough to be a review and is too general to be a perspective or opinion paper. In my opinion, the manuscript will be highly improved if the authors delineate the intention of the paper and focus on describing and discussing it. If the objective is to provide a unique and clear definition of the root zone, the structure and much of the information written in the manuscript could be inadequate. If the objective is to claim a holistic perspective for calculating the root zone, it should be better described this perspective and compared with the reductionist one. Moreover, I found several affirmations confusing, repetitive and not adequately referenced, and most of the figures were not well connected to the text. Please see the attached pdf for more details.

Reply: We thank Anonymous Referee #2's endorsement for the “importance” and “high research interest” of this paper. There are three objectives of this manuscript: 1) provide a holistic definition of the root zone; 2) define the root zone as a living, evolving, adapting and most essential part of the Earth system; 3) advocate for a shift from the traditional reductionist approach towards a holistic ecosystem-centered perspective that offers a more realistic, simplified, and dynamic representation of the root zone in Earth system science.

In the revision, we gave a clear definition of the root zone, and meanwhile further sharpened our argument on our proposed holistic method to estimate root zone spatial and temporal variations.

Major comments:

- The abstract does not include the results or conclusions of the manuscript. Having the "correct" definition of the root zone would be helpful.

We gave a clear definition of the root zone in Section 2.

- In the introduction, I expect more development of the need to clarify the definition of the root zone and the problems resulting from using different definitions.

We clarified the problems of using erroneous root zone definitions and provided a clear definition of the root zone in Section 2.

- L116. Please explain Sumax better. What do the authors mean by “maximum water deficit in the root zone”? What is a maximum water deficit? Furthermore, it is not clear to me how this can happen when all the available water has been consumed after a critical period of drought. What is a critical drought period? I understand the water storage capacity as the volume of water that the root zone can contain (Rodríguez-Iturbe & Porporato 2004).

Indeed, the root zone storage capacity is the accessible volume of water that the root zone can contain. We cited the paper by R-I and P, and made clear definition in Section 2.3.

- L245-L247. It is not clear how the figure shows that. Consider numbering the different panels in the figure and associating them with what they are intended to indicate.

We added more description about Figure 5 (Figure 3 in the revised manuscript) in its caption.

- L356. Is De Roo et al. The last quantification? It was made 30 years ago.

We used more recent references, e.g. Zhong et al. (2022).

Zhong, F., Jiang, S., van Dijk, A. I. J. M., Ren, L., Schellekens, J., and Miralles, D. G.: Revisiting large-scale interception patterns constrained by a synthesis of global experimental data, *Hydrol. Earth Syst. Sci.*, 26, 5647–5667, <https://doi.org/10.5194/hess-26-5647-2022>, 2022.

- L369. Is it possible to make this very general statement? Wouldn't the importance of the site and its conditions depend?

Before this sentence, we discussed the results of global calculation, which is a quite general statement. But as Anonymous Referee #2 said, this could be site- and condition-specific. Generally, soil evaporation plays a more important role in arid rather than in humid regions. We clarified that and improved the narrative in the fifth paragraph in Section 3.1.

- L428-L429. What do the authors mean by “Changes in the root zone are generally cumulative, which may be introduced by slow, gradual or abrupt changes.” Please be more specific.

What we meant to express here is that the root zone has a memory (cf. van Oorschot et al. 2021,2024), and the water deficit is cumulative. All changes, irrespective of their time scale and thus including slow, gradual or abrupt changes, will eventually be accumulated in the root zone storage. We added Figure 5, and Equations 3-5, to demonstrate our approach to estimate water deficit cumulative.

- L437. Please elaborate in more detail why ecosystems can increase their Sumax while reducing tree cover.

This is a trade-off between above ground and belowground biomass in a water-stressed ecosystem. With limited rainfall and increasing water-stress, vegetation may allocate more biomass to the root zone, and simultaneously shed leaves in order to avoid water loss through transpiration. More details can be found in Singh et al. (2020).

Singh, C., Wang-Erlandsson, L., Fetzer, I., Rockstroem, J., and van der Ent, R.: Rootzone storage capacity reveals drought coping strategies along rainforest-savanna transitions, *Environ. Res. Lett.*, 15, <https://doi.org/10.1088/1748-9326/abc377>, 2020.

- L455. Not in all cases, some ecosystems are currently emitting more CO₂ than they assimilate. Please provide references.

We modified, and made more discussion on root zone in biogeochemistry studies in Section 7.2.

- L458. Please elaborate if carbon in the root zone has the greatest uncertainty, how is it possible to claim that it has a large influence on carbon neutrality and sequestration?

There is consensus that the root zone has a large carbon stock, but its specific amount is highly uncertain. Due to its large stock, a relatively small variation may have an important influence on the absolute value, the carbon neutrality and sequestration, even allowing for uncertainty.

- L603. How large is the difference?

We removed this sentence.

- Figures. In general, the figures show much more information than what is considered and described in the text. Their link and appearance in the manuscript do not seem clear to me.

We presented more information about all figures in their captions and in the main text.

Minor comments:

Abstract

- Since this is a specialised magazine, I suggest eliminating the words in parentheses (water, soil, etc.). The same goes for the expression “water-centered perspective.”

Modified.

Introduction

- L72. Homogenize micro-biotic (microbiotic) and macro-biotic (macrobiotic).

Modified.

- L87. What do the authors mean by “different reasons”? Please be more specific.

We removed this statement.

Section 2

- L98. Storage of water. Moisture is a relationship or ratio.

Both the absolute value and ratio are difficult to determine.

- L115. Why “reversely”?

We thoroughly improved Section 2.

- L120. Sumax is a variable, not a parameter.

This is an interesting point. In hydrology and land surface models, Sumax describes a vegetation controlled property of the subsurface in terrestrial systems and defines the *maximum* subsurface water volume accessible to roots of vegetation and is at the time scales of typical water management problems typically treated as a parameter in these models (fixed in time). But in an “alive model” with long term evolution, it is changing in response to climate variability and human intervention, and therefore becomes a variable.

Section 3

- L147. Is the root zone the only one that determines the ecosystem’s resilience to droughts and climate change?

Except for the root zone storage capacity, there are other properties that affect the resilience of an ecosystem, such as changing the composition of species in the ecosystem, but also adjustment of species themselves, such as by the shape of the leaves and stomata distribution and regulation. There is ample indication that the root zone of ecosystems can react relatively fast to climate change, and determines the roots water uptake to overcome droughts, thus we believe “determines” is a proper word.

- L148. What about nutrient availability?

Under natural condition, nutrient availability itself is also a result of long-time coevolution between ecosystem and climate, geology and topography.

- L149. As it is a specialized journal, I do not think it is necessary to define “vegetation”.
This is not a definition of “vegetation”, but we emphasize: 1) that it does not refer to an individual plant; 2) that “present at any moment” indicates that it is the result of adaptation.

- L151-L156. Please provide appropriate references.
We made changes and added references in Section 2.3.

- L168. It is not necessary/possible to sample all trees. Isn't it better to compare in terms of species?
This sentence intends to demonstrate that “we have very limited knowledge of the root system”.

- L181. Please provide appropriate references.
We rephrased this sentence and added references in the second paragraph in Section 5.2.

- L185. What do the authors mean by “accidental discoveries”?
This is not our main message. We removed this statement in the revision.

- L186. Please specify which type of flexibility the authors refer to.
The flexibility of rooting depth to its environment.

- L184-L186. Please provide appropriate references.
We removed this statement in the revision.

- L195-L205. I found this paragraph repetitive.
Agreed, we reorganized the part on rooting depth thoroughly in the revision.

- L215-L216. Please provide appropriate references.
Please find the changes in the second paragraph in Section 7.2.

- L232-L236. Consider to rewrite this paragraph.
We did. We almost rewrote the entire manuscript.

- L238-L239. Please provide appropriate references.
We removed this statement in the revision.

- L259-L260. Why is this sentence quoted?
This quoted sentence is from the literature of National Research Council in the US.

- L270-L271. What do the authors mean by “precipitation of moisture”? Do they mean rainfall?
Moisture cannot precipitate.
Changed to “precipitation”.

- L275. What do the authors mean by “are to a larger or lesser extent”? Please be specific.
We removed this sentence in the revision.
- L276. Yes, it has been widely studied. Consider to review the work by I. Rodríguez-Iturbe.
We did.
- L280. Figure 6 does not indicate that.
We added more description about Figure 6 in the main text and its caption.
- L297. I don't agree that it's always mainly driven by topography. Please provide references.
Agreed. We toned this down and added some references on the variable contributing area theory and observations. Please see the changes in second paragraph in Section 5.2.
- L297. What do the authors mean by “runoff threshold has a spatial distribution function”? That is confusing. Runoff can vary in space and that variation can be represented by a function.
We mean that to generate runoff water storage thresholds need to be overcome. These storage thresholds can and do vary in space. Please see the changes in second paragraph in Section 5.2.
- L298-L299. This sentence is not clear. Please indicate first what.
We removed this sentence in the revision.
- L301-L302. Is called by who? Please provide the references.
This is a classic runoff generation theory, and widely used in hydrological modeling studies. We added this reference.
Ambrose, B. Variable ‘active’ versus ‘contributing’ areas or periods: a necessary distinction. *Hydrol. Process.* 18, 1149–1155 (2004)
- L304. Is P intensity, depth, frequency? Precipitation can be quantified with different variables.
 P represents the amount of precipitation in a given time t .
- L305-L306. In Gao et al, 2017 beta is defined as a parameter of the storage capacity curve function. It determines the shape of this curve.
We removed this sentence in the revision.
- L313-L314. Please define “matrix infiltration capacity” and “preferential infiltration capacity”.
There are two types of land surface infiltration processes, i.e. matrix flow and preferential flow. Infiltration capacity is the root zone’s property determining the infiltration rate of rain water, controlled by matrix infiltration capacity in homogenous soil materials, and preferential infiltration capacity in heterogenous conditions characteristic of soils with roots and rocks. Please find the changes in Section 4.1.3.
- L335. Please differentiate between superficial and depth runoff.
We thoroughly improved our narrative on runoff generation in Section 5.1.1

- L344. Are the authors referring to flow in streams?

Yes, runoff is the flow in streams.

- L351-L355. Please provide appropriate references.

We published these results in Gao et al. (2023). Thus we removed this part in the revision.

Gao, H., Liu, J., Gao, G., and Xia, J.: Ecological and hydrological perspectives of the water retention concept, *Acta Geogr. Sin.*, 78, 139-148, 2023.

- L376-L377. Please provide appropriate references.

Added references in Section 4.2.1.

- L387. Why “inversely”?

We thoroughly rephrased Section 3.4.

- L400-L402. Please clarify this sentence.

We removed this sentence in the revision.

- L407. What do the authors mean by “climate change”? That is very general.

We believe the reviewer meant L417. Climate change mostly means global warming, but also includes other changes, such as precipitation, snow cover, radiation, albedo etc. All these changes in climatic variables will impact active layer thaws in permafrost region.

Section 4

- L425. The upper boundary is the atmosphere not climate.

Climate refers to atmospheric changes, and “has the dominant impact on root zone dynamics”. Thus, we prefer to use “climate”.

- L427-L428. That is not such general. Please provide the references.

We removed this sentence in the revision.

- L433-L435. Please provide the link between this statement and the manuscript.

This statement is very relevant to the manuscript. The transition patterns across tropics and subtropics found by Singh et al. (2020) provide a potential option to infer temporal root zone trajectories in future climate change, with the “space-for-time” assumption.

- L435. What do the authors mean by “By comparing Sumax with aboveground tree cover”? This sentence is not clear.

We removed this sentence in the revision.

- L436. Please specify which type of transitions the authors are referring to.

We referred to “rainforest-savanna transitions”.

Anonymous Referee #3

The paper, titled "Root zone in the Earth system," addresses the critical but underexplored concept of the root zone within Earth system science. It begins by acknowledging the historical significance of roots in plant evolution and their transformative impact on the biosphere and abiotic environment. The study aims to clarify the definition of the root zone, differentiate it from similar terms (e.g., rooting depth, rhizosphere, critical zone), and highlight its profound implications across Earth's spheres (biosphere, hydrosphere, pedosphere, lithosphere, atmosphere). The authors advocate for a shift from traditional, reductionist approaches to a holistic, ecosystem-centered perspective for understanding and modeling the root zone. This perspective considers the root zone as a dynamic entity influenced by and influencing hydrological processes, plant water uptake, and atmospheric moisture cycling.

Reply: We thank Anonymous Referee #3 for this positive summary of our work.

However, I believe there is no need to newly state anything regarding the comparison between holistic methods and reductionist approaches, as there are already many research examples on this topic. Especially in many Earth system models, the holistic method is predominantly used, and reductionist methods seem to be virtually non-existent.

Reply: Reductionist approaches remain dominant in hydrological and Earth System Models. Such mainstream hydrological and Earth System Models, including but not limited to MIKE SHE, SWAT, VIC, CLM, JULES, MATSIRO, Noah, ORCHIDEE, all use reductionist soil hydraulic schemes, such as the Richard's equation, to calculate the storage and transmission of water through soils, and its relevant ecohydrological processes (Clark et al., 2015; Danabasoglu et al., 2020).

Clark, M. P., Y. Fan, D. M. Lawrence, J. C. Adam, D. Bolster, D. J. Gochis, R. P. Hooper, M. Kumar, L. R. Leung, D. S. Mackay, R. M. Maxwell, C. Shen, S. C. Swenson, and X. Zeng (2015), Improving the representation of hydrologic processes in Earth System Models, *Water Resour. Res.*, 51, 5929–5956, doi:10.1002/2015WR017096.

Danabasoglu, G., Lamarque, J.-F., Bacmeister, J., Bailey, D. A., DuVivier, A. K., Edwards, J., et al. (2020). The Community Earth System Model Version 2 (CESM2). *Journal of Advances in Modeling Earth Systems*, 12, e2019MS001916. <https://doi.org/10.1029/2019MS001916>

On the other hand, the concept of the root zone might differ between models with dynamic vegetation and those with static vegetation. In this study, it would be beneficial to review how the root zone is handled in dynamic versus static vegetation models.

Reply: We thank the reviewer for raising this highly relevant point. We added more detailed discussion about dynamic vegetation models in Section 4.2.

Furthermore, although Figure 7 explains using a single tree, in Earth system models, discussing on the scale of individual trees is nonsensical, and it is more common to discuss in terms of forest communities. Especially on these points, I feel the review is incomplete. Before publication, I believe the paper would benefit from revisions in Chapter 5, taking into account the above comments. I trust that making these revisions will improve the quality of the paper.

Reply: We improved all figures and their captions. Section 5 is thoroughly improved as Section 4 and 5 in the revised manuscript.

My main concerns are as follows:

I think it would be beneficial to include a review on the differences between dynamic vegetation models and static vegetation models, especially in terms of root zone estimation. Additionally, considering vegetation as a community is an approach that aligns with the holistic method, so it would be useful to mention and review this aspect as well.

Reply: In the revision, we added more discussion about dynamic and static vegetation models, especially comparing their root zone estimation in Section 4. In Section 5 about the holistic method, we mentioned the approach of considering vegetation as a community in large scale ecology and Earth system models.

At line 111, Sumax suddenly appears, but I would like it to be expressed in an equation, specifying in which model this variable is defined. Doing so would allow for a better understanding of the content of the paper.

Reply: Agreed. In the revision, we made the definition of the root zone storage capacity in Section 2.3 and the associated model parameter Sumax much clearer in Section 5.1.1.

I believe there are papers in the literature that have been overlooked. It would be appreciated if you could consider adding them. In the early studies mentioned at line 178, there is Tanaka et al. (2004, DOI: 10.1029/2004JD004865). In addition to the references at line 411, I find the lag mechanism caused by soil freeze-thaw cycles in permafrost regions interesting. I recommend citing literature such as Sugimoto et al. (2002, DOI: 10.1046/j.1440-1703.2002.00506.x) and Suzuki et al. (2021, DOI: 10.3390/rs13214389).

Reply: We thank Anonymous Referee #3 for sharing the references. We added these relevant references in the revision in Section 3.5.

These comments would aim to enhance the paper's contribution to understanding the root zone's role in Earth system science and encourage further research in this important but underexplored area.

Reply: We thank Anonymous Referee #3's constructive comments and endorsement for the importance and underexplored study of root zone.

Anonymous Referee #4

While this review paper demonstrates promise and importance, it predominantly focuses on listing previous research findings, with each section serving as a basic introduction, lacking deeper analysis and synthesis. Here are some suggestions for improvement:

We thank Anonymous Referee #3's endorsement on the "promise and importance" of our review paper. In the revised version, we deepened our analysis, and sharpened our perspectives. We believe the frame of this paper is more like an opinion paper.

In line 77, it is recommended to revise "Plants" to "Plant roots."

Modified.

Section 2 should be reorganized to enhance coherence.

We thoroughly reorganized Section 2.

Section 3.2.2 would benefit from additional content to provide greater depth.

We thoroughly improved Section 3.2.2. Please find the changes in Section 3.3 and 4.1.2.

Figure 6 should have the first letter of "Capillary" capitalized.

Modified.

Consider removing Section 4.3.3 from the manuscript, as it may not be directly relevant to the main topic.

We reorganized the entire Section 4.3 in the revision. Please see Section 6.2.

Figures 1, 3, and 4 are cited from published papers, which may be excessive for this manuscript. Please consider creating some original figures for inclusion.

We made five original figures, including Figures 2, 3, 4, 5, 6.