Anonymous Referee #2

This opinion paper makes interesting and bold claims about the importance of soil properties for hydrology. I agree with many of the statements for natural soils and mature ecosystems. However, the majority of our earth is no longer a natural mature ecosystem. We have changed the surface cover drastically and very large areas are under agriculture or are so badly degraded and not in a mature "steady" state that the ecosystem perspective that is advocated in this paper is possibly no longer applicable. I think that this has to be mentioned in the text and that the reader needs to be reminded more frequently that these statements are made for mature natural ecosystems.

Reply: We thank Anonymous Referee #2's endorsement for the scientific significance of our opinion paper, and the agreement on our statements for natural soils and mature ecosystems. We agree that we are living in a new geological epoch, i.e. the Anthropocene, which means human impacts on essential planetary processes have become profound. Also we agree with the referee's suggestion to say more about the soils in human modified systems, including agriculture, urbanization and deforestation. We did so in the original paper, but we shall bring it out more clearly. However, it is still relevant to emphasize the importance of ecosystem understanding. There are two reasons: 1) also for human modified systems the ecological approach applies, albeit at different and often smaller time scales; 2) the majority of our earth, and particularly the uphill runoff generating parts of catchments, is still dominated by natural ecosystems, although human modification has modified 14.5% or 18.5 M km^2 of land (Theobald et al., 2020).

Reference:

Theobald, D. M., Kennedy, C., Chen, B., Oakleaf, J., Baruch-Mordo, S., and Kiesecker, J.: Earth transformed: detailed mapping of global human modification from 1990 to 2017, Earth Syst. Sci. Data, 12, 1953–1972, https://doi.org/10.5194/essd-12-1953-2020, 2020.

That soil is important is clear in situations where severely degraded ecosystems are restored. It is the restoration of the soil that leads to the very large changes in the flow pathways (from overland flow to subsurface flow) and thus streamflow responses. Indeed, it is the ecosystem that changes the soil properties that lead to the changes in the hydrological flow pathways and runoff responses, but this does not mean that the soil itself is not important at all. It means that the ecosystem has such a large effect on soil that the ecosystem would be a better predictor to be used in models (because ecosystem and soil properties become correlated as the ecosystem matures and the ecosystem is easier to observe), but it does not mean that soil is not important at all, especially not when one wants to understand processes. I think that some of the statements about soil not being important therefore require a bit more nuance. In particular, the model perspective (rather than process perspective) for some of the claims should be made clearer.

Reply: As the referee will have noticed, the first sentence of the paper is: “Soil is important. It forms the substrate of the terrestrial ecosystem and hence is a crucial element of the critical zone of life on Earth”. We do agree with the referee and we shall adjust the paper in other locations if this statement is contradicted elsewhere.

One of the confusing parts of the paper is that the authors state that the rooting zone is important but that soil is not important. This seems to suggest that they think that the rooting zone is not part of the soil. I think that what they mean is that soil texture is not important. To me it seems that most of the time when the authors say that soil is not important, they mean that soil texture is not important. For example when the authors refer to soil in the top down approach of catchment comparisons (Section 3.3), they actually refer to texture, not soil hydraulic properties. I urge the authors to more explicitly state that they focus on the soil texture. A better description of what parts of the soil they think are not important would be really helpful. It will also help if they give their definition of soil early in the paper.

Reply: Root zone and soil have strong connections, but with obvious differences. Root zone is the active layer in land surface processes (with as much or even more biomass than above ground) controlling hydrology. Soil is part of the substrate of the ecosystem, but only the root zone is the active part. For example, in the Loess Plateau where soil is thick, only the root zone is the active layer in the topsoil. In Karst and other mountainous regions, rootzone includes not only the soil water storage, but also the fissure water storage in bedrock. In very dry climates, roots can even reach the deep groundwater, thus in this case, the rootzone also includes some part of the groundwater (see Singh et al., 2020). In seasonal cropland, if ploughed, the active part of the soil is limited to the ploughed upper layer and otherwise the rooting depth that a crop can develop within one season. In that case soil properties are indeed dominant. We did mention this in our paper, but we shall make it more clearly since apparently the referee missed that point. For permanent crops, the ecosystem has time to develop its preferred hydrological conditions and our approach applies. If irrigation is provided, human interference comes into play. Also our method has taken irrigation, as extra water supply in dry seasons, into account to estimate root zone storage capacity (Wang-Erlandsson et al., 2016).

Regarding the point “To me it seems that most of the time when the authors say that soil is not important, they mean that soil texture is not important”, we agree that if soil hydraulic properties and moisture conditions were known, it would in principle be possible to determine water movement in the soil. But this approach is impractical if not infeasible due to limitations in data, processes understanding, and computational resources. Soil texture is typically the only soil characteristic that is available at useful scales, which leads to the typical approach of relating soil hydraulic properties to texture. We think that this approach is bound to failure, as soil texture has little to do with soil hydraulic properties at the catchment scale. Our approach relates soil hydraulic properties to the ecosystem, effectively bypassing the need of soil characterization.

Where we talk about soil, we mean soil in general, including soil texture and soil hydraulic properties. Soil texture is widely used in hydrological studies, likely because it is the most easily accessible soil information. Soil itself and its characteristics, such as soil texture and soil hydraulic properties, are the results of a variety of environmental variables, including climate, base material, topography, and, most importantly, biota. This has been well documented by soil scientists in 19th century, such as Vasily Dokuchaev, one of the most renowned pedologists in history.

Reference:

Singh, C., Wang-Erlandsson, L., Fetzer, I., Rockström, J., and Van der Ent, R., 2020. Rootzone storage capacity reveals drought coping strategies along rainforest-savanna transitions, Environ. Res. Lett. 15 (2020) 124021

Wang-Erlandsson, L., Bastiaanssen, W. G. M., Gao, H., Jaegermeyr, J., Senay, G. B., van Dijk, A. I. J. M., Guerschman, J. P., Keys, P. W., Gordon, L. J., and Savenije, H. H. G.: Global root zone storage capacity from satellite-based evaporation, Hydrol. Earth Syst. Sci., 20, 1459–1481, https://doi.org/10.5194/hess-20-1459-2016, 2016.

The authors should point out much more clearly (and explicitly) that a major problem is that we use texture in pedotransfer functions to derive the soil characteristics that are related to water flow and storage, especially because these pedotransfer functions were developed for agricultural soils. The sand or silt content of a soil do not affect water flow or storage. We only attribute such an effect when we use pedotransfer functions to derive properties related to water flow and storage based on the texture. Because the pedotransfer functions were largely derived for agricultural soils, they do not take the effects of structure (and preferential flow) into account.

Reply: We agree completely with this comment and will follow up on this suggestion

The writing of the manuscript could be a bit sharper. At several places, the authors make a good argument for why the ecosystem is important and then conclude that the soil is not important. I think that these sections need to be improved for two reasons. First, reasons are given for why the ecosystem is important but not for why the soil is not important. In particular, no references are given for this second part. In other words, the authors provide arguments for the first part (the ecosystem is important) but not for the second part (soil is not important). Thus either the second part (soil is not important) has to be taken out or arguments and references need to be included for the second part as well. Second, ecosystem and soil are interconnected. It is the ecosystem that changes the soil properties. So one can not directly argue that because the ecosystem is important, the soil is not important. It is still important but the ecosystem is perhaps the better predicting variable to be used in models because it is easier to observe and has a large effect on the soil properties that actually affect how water moves through the soil.

Reply: Thank you for your suggestions. We will improve the writing accordingly.

Other parts of the writing could also be improved. In several sentences words are missing and some other sentences are not clear and should be reformulated. The structure of the paper and individual sections was sometimes unclear to me. For example, section 4.1 consists of four paragraphs. Paragraph one highlights the importance of ET and states that hydrologists focus on discharge instead (but this point was already made on L128). The second paragraph then describes that ecosystems maximize storage and drainage. This section is interesting and fits the caption of this section. One would expect the next paragraph to get deeper into this but the third paragraph describes that the numbers for soil properties used in models don't match the actual measurement values, and the fourth paragraph describes the rebalancing of soil properties that needs to be done in models. While the first two paragraphs sort of fit together and the last two paragraphs as well, the link between the first two and last two is not obvious. It also means that the second paragraph ends abruptly and this line of thinking could use some more elaboration. In addition, the part on the soil properties and the rebalancing starts abruptly without an introduction. The latter two paragraphs would probably better fit in a separate section on the problematic part of using pedotransfer functions based on texture (see comments above). This is just one example, there are other sections where the flow was unclear and I expect other readers to also wonder how the paragraphs are connected. I made some suggestions in the annotated pdf but there are more places where text could be reordered for a better flow. I don't request that the authors use the suggested order but I do recommend that they carefully read through the paper to see if the order is logical for a reader.

Reply: We will make the suggested changes to make them more logical for the readers.

Oter specific points:

- L56/139: I think that the problematic part of the use of pedotransfer functions based on texture to derive properties about pores should be described in more detail. Especially knowing that these pedotransfer functions were developed based on cores from agricultural fields and that texture does not really influence the hydraulic conducitivity (e.g., Jarvis et al., 2013; Gupta et al., 2021). See also comments above.
- Reply: Thank you. We will follow up on your suggestion and also add relevant references in the revised MS.
- Section 3.1: I don't think that anyone claims that soil affects the long term water balance more than climate and vegetation. So, I think that it is fine to use this section to highlight that the ecosystem and climate are the main factors that determine the long term water balance but it makes less sense to use this as an argument that soils are not important.
- Reply: The logic is we separately discussed the role of soil in both long term water balance and short term hydrological processes. We believe it is relevant to clarify the unimportance of soil for the long term water balance, but we shall not overemphasize this point.
- L129-132: Yes, land use change (if severe) alters runoff generation, exactly because of the large effect it has on soils. So, I don't think that you can use this argument here to say that soils don't matter. You can use it to make the argument that vegetation has a large effect on the soil properties that actually matter for water flow and storage. Also, it would be good to reference some field studies here (not only model studies).

Reply: We will follow your excellent suggestion to rephrase this sentence and add more references about filed studies.

- L158: But the comparison is basically between a model and a model with more data. I don't think that one should call this observations.
- Reply: Thank you for pointing out this issue. We will change the term "observed" by "remote sensing derived".
- L162: But it also mimics the depth to the groundwater – maybe this has a different effect in the two models?
- Reply: Yes, the depth to the groundwater also impacts the evaporation in dry seasons in the Netherlands. The soil-based model heavily relied on detailed soil observations, but did not consider groundwater replenishment, and underestimated evaporation.
- L181: The problem is in part that we use texture here. Texture does not describe the soil pores that are important for storage or flow of water. The problem is that we use pedotransfer functions that are largely based on data

from agricultural soils and are not appropriate for forested systems. See also the comments above. Furthermore, soil depth data is usually very rough and not very reliable. Maps of soil properties that actually describe water flow and storage are rarely available. Thus, one could also argue that the big problem is that we don't have soil maps with sufficient information on the properties that actually matter and are related to water flow, and that instead we rely too much on texture and pedotransfer functions.

- Reply: Yes, it is a technical issue to rely too much on soil texture and pedotransfer functions. The more fundamental issue though is whether we understand and model hydrological processes based on the substrate (the soil) or on the active agent (the ecosystems). It is an issue of cause and effect. We have focused too much on the effect (the soil properties that we observe locally but cannot observe at the relevant scale) instead of trying to understand the agent that creates the soil properties, which acts at the appropriate and observable ecosystem scale.
- L188: I agree that all these processes are intertwined or connected. Therefore, I think that the opinion paper should use more careful wording. It is OK to say that for hydrological modeling it is more useful to look at the ecosystem because the soil properties that matter for hydrology are highly correlated with land cover, and ecosystem properties are much easier to observe or measure. However, if we want to actually understand processes and the factors that affect these processes, it is important to look at the processes. In other words, then we have to look at the partitioning of rainfall into infiltration, overland flow, deeper drainage, etc. and soils are important. I think that this distinction between model application and process understanding should be made more clearly throughout the text.

Reply: Our opinion paper discussed the role of soil in both model development and process understanding. We found that soil is overrated, not only in model development but also in process understanding. The role of soil is overrated not only in catchment hydrology, but also in hillslope runoff generation under natural condition, land surface evaporation and energy interaction. Even small-scale water movement and pathways are not mainly driven by soil properties but by soil structure, controlled mostly by the ecosystem. Moreover, our argument is about what is the active manager and main driving force, and what is the substrate? What is the dependent variable and what the independent? What is cause, and what is consequence? And eventually what is intuition, and what is realism?

We also believe, and this is hard to prove as yet, that partitioning is controlled by the ecosystem, firstly by interception and throughfall concentration in dripping points where infiltration is facilitated, next by preferential infiltration patterns that are created by biota, and third by subsurface drainage and percolation. From an evolutionary perspective it is reasonable to assume that ecosystems evolve towards survival (a Darwinian hypothesis). This implies that surface runoff is prevented (causing loss of nutrients and fertile topsoil), that depleted moisture stocks are quickly replenished, and that excess water is drained below the root zone (rapid subsurface flow). From a larger ecosystem perspective, one could even go as far as assuming that recharge of groundwater is beneficial to the ecosystem at larger scale, sustaining base flow. We have not even touched upon all the intricacies of how ecosystems manipulate the substrate to its advantage. We have merely shown convincingly (by several model applications) that the root zone storage that ecosystems create are better predictors of hydrological behaviour than soil texture derived storage. In this opinion paper we do not claim that we have all the knowledge required to explain partitioning, we merely point a more promising research direction to untangle hydrological complexity, where the basic assumption is that an active agent, with a clear purpose, creates its own conditions for survival. As a bonus, models based on this approach appear to be simpler, cheaper, less time and resource demanding and better at the job for which they are developed, see for instance Mao and Liu (2019).

Mao, G. and Liu, J.: WAYS v1: a hydrological model for root zone water storage simulation on a global scale, Geosci. Model Dev., 12, 5267–5289, https://doi.org/10.5194/gmd-12-5267-2019, 2019.

- Section 4.2: I am sorry but I don’t understand what these ERA5 storage volumes contribute to the arguments of the opinion paper. The volume is one thing, the total flux from repeated filling and emptying is another. Certainly, I agree that the total storage is highest in the root zone but I consider the root zone to be part of the soil. So why is the root zone important but soil not? The paragraph on 277-284 goes some way into explaining this but it could have been added to section 4.1. It would be good if the authors give a definition of soil early in the paper. I have the feeling that often the authors mean soil texture instead of the soil itself.
- Reply: For the relationship between soil and root zone, the Anonymous Referee #2 can find our replies to your main comment. We will add the definition of soil in the revised MS.
- Several minor comments and suggestions are given in the annotated pdf.

Reply: We thank Anonymous Referee #2’s comments and suggestions which are greatly helpful to improve the quality of this manuscript.

References:

Jarvis, N., Koestel, J., Messing, I., Moeys, J., and Lindahl, A.: Influence of soil, land use and climatic factors on the hydraulic conductivity of soil, Hydrol. Earth Syst. Sci., 17, 5185–5195, https://doi.org/10.5194/hess-17-5185-2013, 2013.

Gupta, S., Hengl, T., Lehmann, P., Bonetti, S., and Or, D.: SoilKsatDB: global database of soil saturated hydraulic conductivity measurements for geoscience applications, Earth Syst. Sci. Data, 13, 1593–1612, https://doi.org/10.5194/essd-13-1593-2021, 2021.