Hongkai Gao and co-workers present an interesting contribution to the debate about key elements in the concepts of hydrological modelling. As an opinion paper, the authors argue that the affinity of hydrological model concepts to soil properties are more a relict than a substantial information basis. They propose to shift focus to the rootzone (as manifestation of the ecosystem) as alternative conceptual foundation.

RC1.1: I congratulate the authors for their work and I agree that our community has to keep challenging the conceptual assumptions and traditions. The role of soils in hydrology and land surface modelling is a particularly interesting debate. Recently Novick et al. (2022) have pointed to a "water potential information gap" in similar notion but opposite proposals. Discussing the role of pedotransfer functions (Looy et al. 2017, Vereecken et al. 2022) and soil hydraulic functions (Peters et al. 2023) together with structural adequacy of models (Gupta et al. 2012), perceptual model consistency (Wagener et al. 2021) and the data flow in model building (Gharari et al. 2021) and model analyses (Loritz et al. 2018) is in my view very important and promising. Hence I see the topic of this manuscript as worth an opinion paper.

We thank Conrad Jackisch for endorsing the need of our work, and his pointing out the relevance of this topic in relation to various developments in hydrological science and land surface modelling.

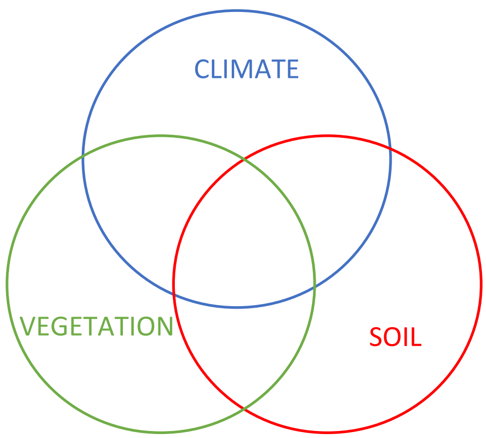
RC1.2: However, I am not really convinced that the current arrangement of the arguments in the manuscript is really substantiating this timely debate. My main concern is that the authors use the word "soil" for different concepts and at different scales without much differentiation. The critical zone concept (Lin et al. 2006) was already much further than this. Also the debate about landscape organisation and hydrologic functioning (Jackisch et al. 2021) including a critical assessment of conceptual assumptions about processes and scaling is more advanced on the topic.

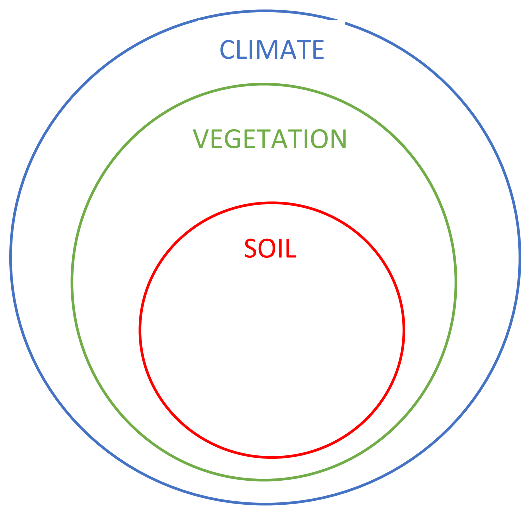
We thank the reviewer for referring to previous work that is relevant to our discussion, and we will include these references in our revised version. However, we generally disagree with statements such that Lin et al. 2006 'goes much further than this' and that Jackisch et al. 2021 'is more advanced on the topic', which imply that our opinion paper is contained in these previous discussions.

The cited papers argue in favor of interdisciplinarity, recognizing that there is an intersection between hydrology and pedology (Lin et al. 2006) or landscape organization and hydrological functioning (Jackisch et al. 2021). We shall acknowledge and cite the relevant papers in our MS, such as on hydropedology (Lin et al. 2006), landscape organization and hydrology (Troch et al., 2015). We fully agree that the cross-fertilization of related disciplines can lead to novel knowledge, and that it is important to encourage a broader earth system science perspective to better understand interactions and feedbacks between different processes. We don't claim that we are the only ones, or that we invented the ecosystem centered approach. There is extensive previous work on these subjects, going from entire special issues in HESS dedicated to catchment co-evolution (https://hess.copernicus.org/articles/special_issue207.html) to the Gaia hypothesis. It's encouraging that there are more people who share this view. It is just that mainstream (often global scale) modellers continue to use the traditional soil-based approach (e.g. the recent paper by Vereecken et al. 2022).

However, our commentary should not be mistaken for an invite to look at things more broadly. It addresses some of the limitations in our ability to integrate different disciplines, by trying to disentangle some specific dependencies that are not fully understood. In particular, we are identify a dependency structure between climate, vegetation and soil hydraulic properties, which clarifies what one should model and for which purpose. Therefore, compared to this previous work, our paper is more specific, but also more purposeful.

The picture below could perhaps help clarify our contribution. The overlapping circles shown on the left reflect the traditional view, which recognizes that climate, vegetation and soil all play in important role in governing catchment hydrological processes and cross influence each other. The nested circles shown on the right are closer to our view, where climate sets the boundaries for vegetation, and in turn, vegetation manages the soil hydraulic properties. We claim that for catchment hydrology it is unnecessary to dig into what happens in the 'soil circle', as the 'vegetation circle' provides the necessary level of detail.





Then there is the question of scale, which is often brought up when debating appropriate process descriptions. The paper of Vereecken et al., 2022 (e.g. in Figure 1) clearly indicates that one needs to understand soil processes at the pore scale in order to understand processes at larger scales (pedological, regional, global). This is a very common view, which we strongly oppose, as it fails to recognize that a natural system exhibits emergent properties, which effectively enable a description of large-scale processes independent on what happens at the smaller scale. We shall clarify that our main focus is describing hydrological behaviour at system scale, ranging from the watershed scale to the catchment scale, hence considering topographic areas that are large enough for a stream channel to be identified, which typically takes at least a few hectares. This scale is clearly too large to enable a detailed characterization of pore scale processes, but fortunately sufficiently large so that considerable process integration takes place, which makes it possible to characterize the overall system behaviour through its emergent properties.

RC1.3: The authors do address such aspects in their manuscript and point to intertwined factors and sub-systems. However, the arguments are not really brought to consistently support the very fundamental claim of the manuscript. Without meaning to offend the authors, I would see many of the claims rather being rooted in conceptual limitations in the view of soil functions by the authors than in the lack of information or importance of soils in hydrological processes and models. I will substantiate this in the more detailed assessment.

We do not agree that our abandoning detailed description of soil processes is caused by a lack of understanding of such processes. The point is that detailed knowledge does not prevent us from seeing the larger scale picture. We reply to the individual points in the more detailed comments.

RC1.4: In general, I do not really see, how the replacement of a soil-centred with a rootzone-centred concept deviated from the critical zone concepts (Lin et al. 2006). I also do not see, why the authors omit the main driving concept for fluxes (depletion of gradients) and thus the whole debate about potentials (Novick et al. 2022). I would have liked to see to which degree their arguments are essentially an expression of the conceptualisation of hydrological models i) as distributed and linked storages, ii) at a broader scale (in the sense of the scale triplet) and iii) with soils expressed by texture classes.

The paper of Lin et al. 2006 introduces hydropedology as an integration of pedology and hydrology. The first difference between Lin's and our work is in the system of interest. Their system is the root and deep vadose zone, where the relevance for hydrology is the description of flow pathways in the unsaturated zone. Vegetation is only mentioned tangentially in their work. Our interest is in the hydrological system, and in particular, the relationship between precipitation and streamflow. Vegetation is an integral part of this system and the ultimate water manager, as an active agent. As a result, we are interested in understanding the overall behaviour of the root zone. They are interested in what happens inside it.

The second difference between Lin's and our work is that pedology and hydrology in Lin's work are seen at the same level, in the sense that they can cross-influence each other, which is true at the scale they are primarily interested in. In our work, we establish a hierarchy, where it is the hydrology – by means of the ecosystem as the active agent – that influences the soil much more than vice-versa. Again, this applies to the overall behaviour of the root zone at the catchment scale.

Novick et al. 2022 is about potentials that govern the water flows throughout the soil–plant–atmosphere. These processes are much more specific than the level of detail we are focusing on in our commentary which is at larger scale hydrological behaviour.

In order to avoid these misunderstandings, we will clarify in our revised version that we are primarily interested in the description of hydrological processes at system scale.

RC1.5: Moreover, I find many claims very strong and confrontative (e.g. L21f, L111) and not well-balanced. To really spark the debate (and not a battle) I would have liked a more balanced and substantiated formulation.

Our paper is meant to trigger debate and definitely not a battle. However, we also feel that an opinion paper should include clear views, personal thoughts, and opinions that are not universally shared. Such opinions may be considered provocative and may not please everyone, but if the arguments are too balanced, then there is not much incentive for debate.

Detailed comments:

RC1.6: L43: I would argue that this is the debate throughout in pedology. At least not only recently.

"Soil forms an ecosystem in itself" is a common sense in pedology. This can be found in many soil textbooks, such as "The Nature and Properties of Soils. 15th edition, Weil and Brady, 2017". As suggested by the reviewer, we will remove "recently".

RC1.7: L49: Why do you limit the perspective to abiotic boundary conditions when you actually argure for an ecosystem perspective? Biodiversity, niches, disturbances, stressors, carbon pools are not all determined by climate and geology. Moreover, at least most temperate soils do not develop directly from bedrock material but on deposited material from rather old geomorphological processes (which include path-dependent development options). Pointing to this, soil degradation and soil loss too is an important and largely irreversible process with severe implications on regional hydrological and biogeochemical cycles.

We shall clarify that we are referring to systems that are vegetated, and where the ecosystem had the time to adapt and is in a condition of a relatively stable equilibrium. Such systems, we would argue, have the ability to adapt to 'disturbances', such as heavy rains that can cause soil erosion, or dry periods that can cause water shortage. The reviewer is right to mention that there can be disturbances that can be destructive and can determine irreversible processes. These extreme cases are not the focus of our commentary. However, we believe that a better understanding of the behaviour of an ecosystem in its 'mature' state can also help understand when tipping points are reached that can cause such irreversible processes.

RC1.8: L59: I think I have an idea what you intend to express, but since this simplistic/reductionist pedotransfer approach has a couple of implications which could be challenged. I suggest to clarify this sentence a little more and link to the debates in soil physics and the pedotransfer community.

We will rephrase. Our intention was not to criticize pedology as a discipline, but rather, the way that concepts developed within soil science are utilized in hydrology.

RC1.9: L63 (opposite): I am not sure if I can follow. The argument before was that simple pedotransfer and soil hydraulic property models are an issue and that they become coupled to rooting depth.

Consequently, rooting depth has to be defined somehow to eventually assess plant-available soil water storage. Why does it matter if in this step plant-available soil water storage becomes the dependent variable of the other involved variables, if it is used as independent variable in the proceeding calculation steps? Isn't this a question about the perceptual model underlying any form of conceptualisation and numerical expression?

L63 (root zone storage): Yes, but maybe at different time scales? Plants and ecosystem may adapt and co-evolve (within a range of their survival). So why should the debate be solved by exchanging the depending/independent variables? Could this actually be a scaling issue?

This is a good question. Whether regarding root zone water storage as a dependent or independent variable, results in very different perceptual hydrological models. Traditional models regard root zone water storage as a function of soil moisture and rooting depth, which is a typical reductionist perspective. Such an approach requires to collect endless spatial and temporal heterogeneities of soil, water, and underground biomass variations to obtain root zone water storage. There are two limitations of this method. 1) Data availability. These small-scale details are usually not available especially at large scale. This limitation “will remain unresolved in the foreseeable future” (Or, 2019). 2) more importantly, the ecosystem is not a stationary item. Ecosystems can and do adapt to changes. Hence, rooting depth is a dynamic variable in response to climatic and anthropogenic drivers.

It is not the combination of the moisture holding capacity of the soil and the rooting depth that determines how much water plants use, but rather the opposite: the ecosystem has a certain water demand and thus requires a certain root zone water storage as a buffer for critical periods of drought. This storage can be reflected in root density, depth and lateral extension, but it boils down to a hydrological volume that can be easily conceptualized in a hydrological model.

Regarding the scaling issue, we refer to our answer to comment RC1.2.

RC1.10: L65: This might depend on what exactly we see as detailed. As you open your argumentation with coevolution, maybe a broad idea about the general type of soil (not texture class) and biome (including its ecohydrological properties) could be sufficiently detailed? If so, remote sensing claims various solutions to gather such data…

We agree that some soil properties such as soil type could in principle be mapped, and there are soil databases that offer such information in some parts of the globe. However, some of the variables that hydrologists need are very difficult to obtain, even with detailed mapping. Concerning vegetation modelling, one of the key variables is rooting depth, which together with wilting point and field capacity, would determine plant available storage. We will further clarify these statements in the revised version.

RC1.11: L67: Again, i would see this as a scale issue: Ecosystem and climate are both terms referring to large scales (in the sense of the scale triplet). Hydrology is not referring to a specific scale.

We agree that we are referring to a larger scale. The scale triplet referred to by the reviewer is the ‘Process scale’, ‘observation scale’ and ‘modelling (working) scale’ defined by Bloeschl and Sivapalan (1995). As clarified earlier, we are considering processes at the system scale. This is also our modelling scale, as we are interested in characterizing the processes directly at this scale. Unfortunately, there is a well-known lack of direct observations at this scale. We will clarify the scale issue in our revised version.

RC1.12: L72f: This assumes that the ecosystem is somewhat in equilibrium with determinable drivers of its development. However, path-dependent trajectories, dynamic deviations from equilibrium more or less buffered by the ecosystem and any application for global changes (climate, land use, cohabitation…) but severe challenges to this view.

It is correct that the method applies under the condition that the ecosystem has converged to some sort of equilibrium. Indeed, it does not apply to agricultural fields, as we specify later in the paper. Regarding predicting the time to equilibrium, this is a difficult issue, as it depends on how fast the change is and how rapidly the ecosystem can adapt to it. Apart from simulating observed spatial variability (Gao et al., 2014, de Boer-Euser et al, 2019), the method has been applied to simulate temporal variability in response to vegetation change (Nijzink et al., 2016) and climate change (Bouaziz et al., 2022). In these case study, it was shown that the method could be used to anticipate how the system adapts to such sources of variability. The case of agricultural fields is an example where the system is not given the time to adapt and where the proposed method does not apply.

Nijzink, R., Hutton, C., Pechlivanidis, I., Capell, R., Arheimer, B., Freer, J., Han, D., Wagener, T., McGuire, K., Savenije, H., and Hrachowitz, M.: The evolution of root-zone moisture capacities after deforestation: a step towards hydrological predictions under change?, Hydrol. Earth Syst. Sci., 20, 4775–4799, https://doi.org/10.5194/hess-20-4775-2016, 2016.

de Boer-Euser, T., Meriö, L.-J., and Marttila, H.: Understanding variability in root zone storage capacity in boreal regions, Hydrol. Earth Syst. Sci., 23, 125–138, https://doi.org/10.5194/hess-23-125-2019, 2019.

Bouaziz, L. J. E., Aalbers, E. E., Weerts, A. H., Hegnauer, M., Buiteveld, H., Lammersen, R., Stam, J., Sprokkereef, E., Savenije, H. H. G., and Hrachowitz, M.: Ecosystem adaptation to climate change: the sensitivity of hydrological predictions to time-dynamic model parameters, Hydrol. Earth Syst. Sci., 26, 1295–1318, https://doi.org/10.5194/hess-26-1295-2022, 2022.

Gao, H., M. Hrachowitz, F. Fenicia, S. Gharari, and H. H. G. Savenije, 2014. Testing the realism of a topography driven model (FLEX-Topo) in the nested catchments of the Upper Heihe, China, Hydrol. Earth Syst. Sci., 18, 1895-1915, 2014.

RC1.13: L78: Why do you refer exactly to these citations? I would think that e.g. the work of Gardner including his famous lab experiments have been far more important for propagating this perception.

We will consider more references in the revised paper.

RC1.14: L87ff: Yes, and this might be one of the actual issues to address here. Linear Darcy filter flow has been coupled to highly non-linear retention properties with the Richards equation and as a first-order diffusive flow model, it does an ok job for diffusive flow in somewhat well-defined porous media. However, especially infiltration (as initial soil water redistribution into the soil during rain events) is often not dominated by diffusive flow but by advection (Newtion Shear Flow equation (Germann, 2020), soil moisture velocity equation (Ogden et al. 2017), particle model (Jackisch and Zehe 2018), non-equilibrium flow (Vogel et al. 2023)). To my understanding, this deficit is rooting back to the very limited means to measure antecedent state-dependent infiltration and to use such data in hydrologic models. But why this is an argument for soils not being central in the question for one of their fundamental services to mediate the local soil water cycle is not clear to me. Especially because infiltration is state dependent, precipitation may not be retained after drought conditions, requiring vast amounts of light rains, slow snow melts or similar to replenish the water stocks, while storm events will simply lead to preferential flow and possibly erosion…

We agree that soil fulfils several important functions. Provided that we are considering catchment scale processes, and therefore integrating processes that take a minimum area to be operative, the question is whether processes such as infiltration, retention or release to subsurface flow depend on soil properties such as texture or can be somehow related to them, as many hydrological models assume. Our suggestion is that the processes that are significant at the catchment scale are conditioned on a multitude of soil properties, such as macropores, rootzone depth, etc, which are themselves conditioned on the vegetation, which ultimately is conditioned by climate, as shown in diagram above.

In order to model the catchment scale processes, in this set of nested dependencies, it is sufficient to stop at the vegetation level. There are other applications, however, where it is necessary to dig into the soil level.

RC1.15: L93: Well, it is not dominant when it comes to storm events, yes. But these experiments use rather steep gradients with a lot of water. The debate about when and to what degree soil water flow is preferential is ongoing. If this was the full story, soils could hardly sustain the ecosystems.

We shall clarify that we do not negate the existence of matrix flow, which is how the soil becomes wet. However, preferential flow is observed ubiquitously, and is critical to sustain ecosystems, as this is the way ecosystems get rid of excess water, which would otherwise favour soil erosion or create waterlogged conditions where roots would deteriorate. For example, in a Mediterranean climate, isotope tracing has shown that at the beginning of the wet season, preferential flow allows precipitation to rapidly replenish ecosystem water deficit. While during wet periods, the excess water after precipitation is drained efficiently by preferential flow and subsurface drainage (Brooks et al., 2009). By the way, this is also a perfect example of “catchment as a living organism”.

Brooks, J.R., Barnard, H. R., Coulombe, R. and McDonnell, J.J. Ecohydrologic separation of water between trees and streams in a Mediterranean climate. Nature Geosciences, 2009

RC1.16: L98: Partly yes. But preferential flow can also simplify our models. Anyways, I suggest to ease the dispute opened by this statement with a slightly more balanced view on achievements towards unifying forms of non-uniform infiltration.

We will consider your suggestion here to make our statement more balanced.

RC1.17: L108: Yes. But this again can be seen as a scaling issue. At the hillslope- and plot-scale, these parameters/concepts have been very central. Only at the catchment-scale they could be easily subsumed as general soil property parameters not requiring for a dual domain definition. And this is true for the hindcast of our observations…

We will clarify that we refer to the catchment scale as mentioned in the comments above.

RC1.18: L111: Again a strong claim. I can agree that the pref flow debate has always struggled to connect to Darcy-scale soil physics. But fog? No progress? Is this claim really needed for your argument?

The progress in preferential flow studies are discussed in L92-108. We will also add what the reviewer recommends about the importance of advection flow. To our best knowledge, the importance of preferential flow has been recognized at least to Schumacher (1864) (Beven 2018). Still until now, we did not see clear improvement of preferential flow models in runoff prediction practice. Hence, we chose on purpose a strong claim.

RC1.19: L113 (a priori assumption): Well, they are ABOUT the description of soil water flow. If they are key for describing hydrological processes is part of the actual model conceptualisation, its numerics and the respective regimes under study. Again, I would argue that this is no other “a priori assumption” as most other parts of the perceptual model. And since its actual effect in the model can be and is challenged (Glaser et al. 2018), I would rather see it as a positive example for advancing hydrologic models.

Yes, we agree that all our perceptual models need experiments to test and sharpen. Both accepting and rejecting a-priori assumptions is good for advancing our understanding of the hydrological system.

RC1.20: L118: So far soil variability has not been motivated. This is especially difficult, because the effect of soil variability is again a matter of scale (including the respective range of processes). After reading subsection 3.1, I can think of quite a number of papers, providing good evidence for the opposite: When you have the average soil right, you can easily reproduce observed hydrologic patterns (e.g. Loritz et al. 2017).

As mentioned above, we will clarify the scale issue.

RC1.21: L123f: Ok. Known and well established. Maybe citing some of the many studies would be nice.

These results are not from other literatures, but they are calculated from the state-of-the-art ERA-5 reanalysis data. There are similar studies, which we will cite in the revised MS.

RC1.22: L131f: This argument is not really sound. Studies fully agree that plants and ecosystems strongly moderate the net ET flux of a stand. But without soil as the part of the ecosystem which can actually store water for weeks and beyond, this percentage cannot be reached. We exactly see this in data based on Budyko-like assessments that more draining locations (sandy, karstic) have very little ETact simply because precipitation is largely drained.

We agree that the soil supports the ecosystem, and we stated this from the outset. However, we also believe that vegetation will adapt to the soi conditions. For example, special geological condition in Karst region, i.e. the carbonate rock, determines very thin soil layer. This is not an exception, but another good example, that soil is the consequence of co-evolution of ecosystem in certain climate and geology. Also our proposed ecosystem-centred rootzone method has been used in Karst and other mountainous regions. The reviewer can refer two recent studies (Gao, 2020; McCormick et al., 2021).

Gao, Y. Modeling Flow and Nitrate Transport in Karst Groundwater Basins. University of Central Florida, PhD thesis, 2020

McCormick, E. L., Dralle, D. N., Hahm, W. J., Tune, A. K., Schmidt, L. M., Chadwick, K. D., and Rempe, D. M.: Widespread woody plant use of water stored in bedrock, Nature, 597, 225–229, https://doi.org/10.1038/s41586-021-03761-3, 2021.

RC1.23: L137f: Yes, difficult but steeply advancing. Please see Peters et al. (2023) and Hohenbrink et al. (submitted to ESSD) for examples. The most critical part might be the reduction of such data to van Genuchten/Mualem SHP model parameters and the weakly informative relation to the broad texture classes, BD and Corg. But the issue of pedotransfer models is a discussion on its own, and which is currently gaining momentum.

We will cite more recent papers.

RC1.24: L144: The issue here might be that soil mapping is not particularly done for hydrological purposes. On the one hand, pedological classes are not always directly convertible to hydrological properties. On the other hand, soil stratification and the respective hydrological properties are rarely conveyed into land surface models with sufficient degree of vertical resolution. Moreover, the uncertainty about the hydrological properties of the mapped soil classes is largely unknown and very different from region to region. Given all of these points, I am not quite sure if “interpolation and upscaling” is the core issue here. Maybe it is more a disconnection between soil mappers and hydrologic modellers?

We agree that soil maps are not particularly useful for hydrology. There have been attempts to develop hydrological soil maps such as ‘the hydrology of soil types’ (Boorman et al, 1995) in UK, but these are not widely available. However, our main argument is that pursuing that route is unnecessary if one is interested in describing catchment scale processes. This does not mean that a connection between soil mappers and hydrological modelers would not be fruitful, particularly if the focus is on describing smaller scale processes.

RC1.25: L148: I fully agree that unnatural lab conditions are a fundamental difficulty. However, many measurements are conducted using “undisturbed” samples for soil hydraulic property analyses. It is unnatural because the samples are extracted from their capillary context, exposed to free evaporation at the surface and a no flow boundary at the bottom (for the standard HYPROP protocol). However pedotransfer functions are then correlating lab measurements (soil hydraulic properties) to lab measurements (texture) and the scaling and transfer involved in its application to field conditions remain hidden.

We agree that with a lot of effort one can obtain reliable pedotransfer functions, however, this is impractical at the catchment scale. Moreover, such functions are typically surface maps, and lack a vertical dimension, which is necessary to model what happens in the underground. The pedotransfer functions approach is plagued with uncertainties and difficulties of various kinds. For this reason, we believe that exploring alternative, potentially easier approaches is worthwhile.

RC1.26: L152: I fully agree, but again this is an issue with quite a bit of literature from hydropedology to cite here.

Will search and read.

RC1.27: L153ff: I do not get this point. The discussion about parameter regionalisation has a long standing in hydrology. E.g. mHM (Samaniego et al. 2017) exactly works because it modifies the initial lab scale parameters to match its distributed effects on fluxes in the landscape. Showing one odd model result can have so many reasons that I find it very difficult to support your argument through it.

The Netherlands likely benefits from the most detailed soil survey in the world, thanks to its very advanced agricultural science and technology. But even with such detailed soil data, the soil-based evaporation model produced such a large discrepancy. This is a strong indication that collecting detailed soil data does not benefit hydrological studies. This is not one odd model result, but a strong example showing the dead-end track of this methodology.

RC1.28: L157: Which is a nice example for model extrapolation and the shift in parameter sensitivity under climate change (Melsen and Guse 2021).

Thank you for sharing this literature. We will read it.

RC1.29: L177: Again, a difficult claim. They test if texture classes and soil depth is informative. However Novick et al. (2022) point nicely to soil water potential being most informative and often omitted in LSMs. The model you are referring to are not particularly strong in soil physics as they conceptualise soils as stores instead of any framework of potentials as drivers. So your assessment might actually pinpoint that soil hydrology based on a storage concept is not very informative? As stated in the general section, I find it very difficult that you do not discern between weak conceptualisations of soils and the actual physical properties and dynamics linked to soils.

Please refer to our reply to RC1.4.

RC1.30: L188: These intertwined factors mostly manifest at "soil scales", which are not necessarily very small.

Our attitude to terminology in this opinion paper can be found in RC1.13.

RC1.31: L194f: Again, I would argue that the concept of infiltration capacity as rigid site property maybe the root of the issue here? Infiltration capacity to my understanding does not necessarily entail a constant or any specific model (e.g. Horton which is subsuming site properties and antecedent condition into an exponential decay function for infiltration rate or Green and Ampt which indeed is rarely proven in natural soils). Since infiltration is the passage of water into the soil domain, I would argue that soil structures (draining macropores and storing finer pores) facilitate it and that antecedent conditions plus the rainfall supply dynamics govern the individual initial (non-uniform) soil water redistribution (see comment to L87ff). The ecosystem modifies the boundary conditions, state dynamics and structure formation in the long run (Lange et al. 2015 and other publications from the Jena experiment).

The reviewer's thought on infiltration capacity is interesting. We agree that "infiltration capacity… does not necessarily entail a constant or any specific model", which is in line with our augment to question the long-term held belief that soil determines infiltration capacity. The concept of infiltration capacity is still important in hydrology, especially for storm events. But in natural hillslope and catchments, vegetation, topography and other land surface are all indispensable factors in storm event modelling and may play a more significant role controlling the infiltration capacity than soil properties.

RC1.32: L203ff: I agree and I admire the authors for their very nice contributions to these examples. However, this comparison is not fair since the intended applications of more complex models are often more than rainfall-runoff modelling. Especially when models are used to analyse effects of changes in land use , climate regime, management etc. the stationarity assumption collapses and we require parameters and submodels with physical meaning. Once we have a good understanding about how the modified hydrologic system can be conceptualised, the simple models are much more efficient and maybe even less error-prone again. But the transition (in system characteristics or scale) remains very challenging for these kind of models.

The reviewer might not get our points. We believe the reviewer is also with us. Both of us agree that we should not develop our model based on stationary assumption. But the soil-based model is a typical stationary model, since soil properties are mostly stable and unchanged with climate and human activities in short term. What changes dramatically are the land use and land cover and belowground biomass in the background of both human activities and climate change. Our proposed ecosystem-based model can deal with this issue much better than soil-based models. Because ecosystem-based model intrinsically regards catchment as a living organism. The reviewer may refer to many published papers (Nijzink et al., 2016; de Boer-Euser et al., 2019; Bouaziz et al., 2022).

RC1.33: L210f and Fig. 2: I do not find it a logical proof of your argument that some models can succeed without soil information. If soil information is only texture class and porosity maybe it is more telling that these properties are not very informative for hydropedological characteristics and that the variable for the most frequent antecedent conditions (aridity) has far more influence because it is more informative for hydrological functioning? Hohenbrink et al. (submitted to ESSD, soon at https://doi.org/10.5194/essd-2023-74) show very nicely how these standard properties and texture-based soil classes do not inform hydropedologic functioning.

Soil texture is the most easily accessible soil information, that could be the reason Addor et al (2018) chose these characteristics to compare with hydrological signatures.

RC1.34: L217ff: I find it difficult to discern your "ecosystem"/"rootzone" approach from the hydropedology concepts (Lin et al. 2006).

Root zone and critical zone have strong connections, but with obvious differences. The lower and upper boundary of critical zone is still debated. But usually, Earth's critical zone includes air, soil, water, rock and organisms. For hydrology, the ecosystem with its rootzone is the most active layer in the critical zone. For example, in the Loess Plateau where soil is thick, root zone is merely the active layer on 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 cropland, where irrigation provides an extra water supply to rootzone during dry seasons, the rootzone water storage capacity is often smaller than under natural conditions with similar climate background. The rootzone is the most active layer in the critical zone (with as much or even more biomass than above ground) controlling land surface processes, including hydrology.

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

RC1.35: L226ff: Within the lines of arguments, I think you are jumping through different scales here (with concepts and properties which are known NOT to be scale-invariant). The assumption that the ecosystem will be able to become the dominant driver is only true if the system has sufficient degrees of freedom to do so. Mediterranean basins have been deforested long ago, soil has been lost and there is no sign of spontaneous ecosystem replenishing under the current climate conditions. Badlands, crusts, long-term unstable debris are examples contradicting your claim. Hence a more differentiated analysis would be more insightful?

We will clarify the scale issue, as mentioned in the comments above.

RC1.36: L232: I fully agree that water can bypass the rootzone but is not necessarily reaching groundwater. In many soil systems of the mid latitudes we find laterally conductive layers formed by more distant ice ages leading to relatively quick drainage or even interflow. Your FLEX approach has nicely shown this for the Ardennes…

We will add subsurface storm flow in the revised MS.

RC1.37: L235ff: With having FLEX in mind I can understand your reasoning but I find your PERCEPTUAL model rather inflexible in the first place. The notion to simplify as much as possible is fully legit but deterministic concepts are in my understanding rather a thing of the past when we were limited in computational powers. And I find that this stiffness weakens your argumentation.

We use FLEX merely as an example, but our approach is not constrained by the modelling concept and is not an advertisement for simple models. The reviewer misunderstood the intention of the paper. We state that the ecosystem should be central in all hydrological modelling at whichever scale, since the ecosystem is the active agent that reacts to and modifies the impact of external drivers. A nice consequence of this fact is that models can often be much simpler than the modeler intuitively assumes, but it is not a requirement.

RC1.38: L245 (and the paragraphs before and after): I do not see why this is an argument against the importance of soils. Just because modellers use non-informative variables about soils and just because they have not found laws to scale the scale-dependent concepts/models does not mean that soils are not important. If these observations are biased, this does actually point to a misconception of the soil system rather than serving as an argument for omitting soils altogether. I would claim that this only shows that soil function cannot be described by texture classes (alone).

It is worthwhile to note again that we did not intend to omit soils altogether. We claimed that "Soil is important" at the very beginning of our paper. We proposed to considering root zone as an integrated system, rather than simply treating soil and roots as isolated parts.

RC1.39: L251: I find it very difficult to agree to your arguments at this too general level of characterisation of somewhat arbitrarily selected model examples. I suggest to build the arguments based on the state of the art about structural adequacy and model conceptualisation (see general comments)

Please see our response to your general comments.

RC1.40: L282ff (and the whole subsection): You are proposing a new conceptualisation in which you omit various central properties governing water retention and drainage, which are not only governed by vegetation alone. With most of the terrestrial surface of our planet being actively managed by humans and a massively changing climate and biosphere, I find it not very helpful from a physical and system perspective. Moreover, your concept does not evade the scale issues. Quite to the contrary the active rootzone is not a static thing (at many scales). When we look at root water uptake alone, the sourcing depth of water within the root zone is dynamic over the year and very different from site to site (with the very same tree species and ages) (Jackisch et al. 2020). Giving reference to ERA5 data for this is maybe a little too large scale to substantiate your arguments with?

We agree that active rootzone is not a static thing (at many scales), thus we need to develop an alive model to take these changes into account. This is exactly what we are saying in our opinion paper. Please refer to some of more references (Nijzink et al., 2016; de Boer-Euser et al., 2019; Bouaziz et al., 2022).

Our argument is mainly for catchment scales. The concept of preferential flow was proposed in small scale soil profiles, but hydrologists found preferential flow is everywhere for all hydrological processes at multi-scales (Uhlenbrook, 2006). Also the root zone is not only important for catchment hydrology, but also for land surface processes, and is essential for ecosystem's resilience to drought at multiple scales, including landscape, regional and global scale.

Nijzink, R., Hutton, C., Pechlivanidis, I., Capell, R., Arheimer, B., Freer, J., Han, D., Wagener, T., McGuire, K., Savenije, H., and Hrachowitz, M.: The evolution of root-zone moisture capacities after deforestation: a step towards hydrological predictions under change?, Hydrol. Earth Syst. Sci., 20, 4775–4799, https://doi.org/10.5194/hess-20-4775-2016, 2016.

de Boer-Euser, T., Meriö, L.-J., and Marttila, H.: Understanding variability in root zone storage capacity in boreal regions, Hydrol. Earth Syst. Sci., 23, 125–138, https://doi.org/10.5194/hess-23-125-2019, 2019.

Bouaziz, L. J. E., Aalbers, E. E., Weerts, A. H., Hegnauer, M., Buiteveld, H., Lammersen, R., Stam, J., Sprokkereef, E., Savenije, H. H. G., and Hrachowitz, M.: Ecosystem adaptation to climate change: the sensitivity of hydrological predictions to time-dynamic model parameters, Hydrol. Earth Syst. Sci., 26, 1295–1318, https://doi.org/10.5194/hess-26-1295-2022, 2022.

Uhlenbrook, S.: Catchment hydrology–A science in which all processes are preferential, Hydrol. Process., 20, 3581–3585, https://doi.org/10.1002/hyp.6564, 2006.

RC1.41: L295ff: From a (soil and hydrologic) physics perspective the main fundament might be that fluxes are driven by gradient depletion and that the degrees of freedom for these fluxes are state dependent (including subscale properties subsumed as hysteresis). The fill-and-spill concept (McDonnell et al. 2021) is a very powerful description of dynamic connectivity and threshold behaviour resulting from the strong non-linearities in soils. However, the depletion of gradients is largely omitted in such models. You might argue (L298f?) that storage-based models do not require an explicit treatment of gradients since it is all implicitly covered by the individual storage and transfer functions. However, this is not an argument against the importance of soils nor does it solve the standing issue to be capable to convey changing landscape properties into the required storage characteristics.

Please see our response to your general comment on gradient and fluxes.

RC1.42: L308ff: Why do you jump from the debate about the concepts back to the debate about available data (which has so far not been really opened)?

The landscape-based model we are proposing requires quite different data than soil-based models. What we are trying to say is that the data supporting landscape-based model development is booming. The increasing data accessibility allows us to test the model realism not only in terms of runoff, but also internal fluxes with more complementary data (Gao et al., 2020; Hulsman et al., 2021).

Gao, H., Dong, J., Chen, X., Cai, H., Liu, Z., Jin, Z., Mao, D., Yang, Z., Duan, Z. (2020). Stepwise modeling and the importance of internal variables validation to test model realism in a data scarce glacier basin. Journal of Hydrology. 591, 125457

Hulsman, P., Savenije, H. H. G., and Hrachowitz, M.: Learning from satellite observations: increased understanding of catchment processes through stepwise model improvement, Hydrol. Earth Syst. Sci., 25, 957–982, https://doi.org/10.5194/hess-25-957-2021, 2021.

RC1.43: L320ff: Since I read your manuscript as a strong claim for a simplified hydropedologic perceptual model, I find the argument with Occams razor very problematic. I would claim that we are in a situation with plenty of data to challenge our perceptual models and we have the tools to do this (e.g. Höge et al. 2020, Guthke 2017). Occams razor is a perceptual assumption, too.

We don't agree. Firstly, our model is NOT a simplified hydropedologic perceptual model. It regards root zone as an integrated system, rather than simply summarizing isolated parts together, e.g. soil, water, and roots etc. It is controlled by ecosystem's adaption to climate.

Secondly, Occams razor is not a perceptual assumption which can be tested, but a problem-solving principle in philosophy, which means "The simplest explanation is usually the best one." Although it is not a science hypothesis, it is widely used as a heuristic to guide scientific theory development. We believe this is very appropriate in an opinion paper to stimulate thoughts on model complexity or parsimony.

RC1.44: Again, I sincerely thank the authors for raising this debate. I hope that my review can contribute to sharpening the arguments and to raise awareness about the many aspects that might have fallen a little too short in preparing this manuscript.

We are very grateful to Conrad Jackisch for his very detailed and well-argued comments and for taking ample time to enter in this debate with us. We think and hope that our slightly provocative approach has stimulated the convergence of different viewpoints and hydrological schools.

We will incorporate all the valuable suggestions for improvement and will address omissions in the literature.

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