Response to Reviewer 1

January 29, 2026

We thank anonymous Reviewer 1 for taking the time in reading and giving valuable feedback on our manuscript: "From Soil to Stream: Modeling the Catchment-Scale Hydrological Effects of Increased Soil Organic Carbon". In the following, we address each of your comments (in italic) along with our responses and indicate how we will adapt the manuscript in the next phase of a potentially revised manuscript.

1) *"I am not convinced that assuming a uniform increase in SOC across the entire catchment is appropriate. SOC can either increase or decrease across the catchment. I understand that SOC changes can be significant in agricultural areas, whereas SOC changes may be less pronounced in unmanaged forests and grasslands, which constitute important parts of the catchment. Reference: Terrer, et al. (2021). A trade-off between plant and soil carbon storage under elevated CO2. Nature, 591(7851), 599-603."*

Thank you for this comment. We would like to clarify that SOC is not increased uniformly across the entire catchment. As stated in Lines 299–301, the increases in SOC are applied to agricultural land, while SOC in forested areas remains unchanged. We agree that this distinction should be stated more clearly, and to further emphasis this point we have added a map with the land cover classes in Figure 3, which we now also refer to in the revised version of the manuscript.

We further acknowledge that assuming a uniform SOC increase across all agricultural land, including grasslands and meadows, is a simplification - which is further articulated and discussed in the manuscript (Lines 531–533 and 587–588). This simplification arises from the underlying model structure, which currently represents land use using a limited number of discrete classes with no specification for representing spatially heterogeneous SOC changes within a given land-use type. As with any modelling framework, this represents a trade-off; and we accepted this simplification because mHM is otherwise well suited and widely established for catchment-scale hydrological analysis.

We acknowledge that conventional agriculture can lead and has been observed to lead to SOC losses (Lal et al. 2013, Sanderman et al. 2017, Keel et al. 2019), and that these losses are likely to accelerate under climate warming (Crowther et al. 2016, Walker et al. 2018, García- Palacios et al. 2021, Poeplau & Dechow 2023). Assessing SOC losses, however, is beyond the scope of the present study and will be addressed in future work incorporating climate projections. The implemented scenarios in the present study represent a plausible, literature-supported range of SOC increases through agricultural management (Table 1). Accordingly, this study focuses on SOC enhancement scenarios, which align with current policy and management initiatives promoting soil carbon sequestration (lines 536–545).

To make this clearer, we will revise the discussion and add: "It should be noted that conventional agriculture can lead to SOC losses, if more carbon is removed from the soil than is returned (Lal et al. 2013, Sanderman et al. 2017, Keel et al. 2019). These losses are expected to accelerate under climate change, because higher temperatures enhance SOC mineralization (Crowther et al. 2016, Walker et al. 2018, García- Palacios et al. 2021). Exploring scenarios with decreasing SOC could provide additional insights in future studies. Herein, we focus on an optimistic SOC increase scenario to illustrate potential upper-bound effects on soil water retention and catchment hydrological responses."

2) *"I am doubtful about the accuracy of the pedotransfer function linking SOC changes to soil hydraulic properties.. Equations (1-3) and Section 4.2. Given that SOC change effects are reflected by soil hydraulic properties, it is important to estimate these functions for the catchment investigated. If these equations are not suitable for the current catchment, the numerical results will be biased. In addition, I would suggest to discuss why SOC change effects can be adequately captured solely by changes in soil hydraulic properties."*

Thank you for this comment. We agree that the use of pedotransfer functions (PTFs) deserves careful discussion as using and choosing PTFs is always associated with uncertainty. We explicitly discuss these limitations and the uncertainty related to the choice of the PTF in the manuscript (lines 450–456). The selection of PTFs in our case is however strongly constrained by data availability and physical realism of tested PTFs - especially for soil hydraulic parameters related functions.

We agree that ideally region-specific PTFs would be used; however, no or little measurements of soil hydraulic properties are available for our study area. Conducting additional measurements was not in the scope of this study. PTFs, such as the ones used in this study, are trained on large, diverse datasets encompassing soils similar to those in our catchment. Given the complete lack or very limited local data on SOC, ρ_b, or K_{sat}, using PTFs is a common and justified approach.

Regarding the specific equations questioned by the reviewer: Equation (1), which estimates θ_{sat} is implemented in mHM; which is well established for applications in Central Europe. Equation (2) represents a simple PTF describing the relationship between SOC and bulk density (ρ_b). This formulation is supported by the findings of Minasny and McBratney (2018), who showed that SOC consistently affects ρ_b largely independent of soil texture. On this basis, we consider the use of this PTF justified, despite its original calibration on U.S. soils. Equation (3), used to estimate K_{sat}, is based on the PTF by Vereecken et al. (1990), which was developed using Belgian soils that include dominant textural classes also present in our catchment (see Fig. 3b and Vereecken et al., 1990). We propose to add these points in section 2.3 of the revised manuscript (Parameterization of mHM soil moisture dynamics related to SOC changes).

Finally, we will expand the discussion to more explicitly justify why SOC effects are represented primarily through changes in soil hydraulic properties in Section 5.3 while genuinely recognizing the study limitations (Section: Model suitability and structural limitations for representing SOC-induced changes). In our framework, SOC influences hydrological fluxes mainly via bulk density and soil water retention, which determine soil moisture availability and, consequently, actual evapotranspiration. The model also accounts for some vegetation responses, such as adjustments in rooting depth (Chapter 5.2.3), but in general, mHM represents dynamic aspects of vegetation related process at a coarse level. This is a limitation, which we already discuss in lines 588-599. While SOC may influence vegetation-related processes (plant productivity, leaf respiration), the dominant hydrological effects are captured through soil hydraulic properties, which is the focus of this study.

3) *"Model calibration and evaluation and Figure 5. The numerical model works well in reproducing the discharge. However, it would be better if the authors can provide the comparison between numerical results and measurements for ET, soil water content, and subsurface runoff."*

Thank you for this comment. We agree that evaluating the model against additional observed fluxes and states would be highly desirable. However, such an evaluation is strongly constrained by data availability in the study catchment. As shown in Appendix A7 and discussed in lines 322–325, we compare simulated soil moisture with a soil moisture time series measured in the catchment. This comparison provides at least a partial evaluation of simulated soil water. To the best of our knowledge, no observations of ET or subsurface runoff are available for the study area. We would, however, like to emphasize that the model is well established and has been extensively evaluated in multiple European studies against diverse fluxes and state variables, such as soil moisture, evapotranspiration, and total water storage anomalies (see https://mhm-ufz.org/about/publications/ for detailed list).

4) *"Section 4.3 and 4.4. The authors show the difference of ET, soil water content, and subsurface runoff at the grid scale, while the difference of discharge at the catchment scale. Given that the difference of discharge is very small at the catchment scale (lines 364-366), I would suggest showing the difference of ET, soil water content, and subsurface runoff at the catchment scale too. In addition, the SOC increase seems to have a negligible effect on hydrological processes, considering the uncertainty from assuming the uniform SOC change."*

Thank you for these comments. We agree that it would be informative to show the effects of SOC increase on ET, soil water content, and subsurface runoff aggregated at the catchment scale. We will include these aspects in the revised manuscript, possibly in a Tabular format, as well as in the abstract, results, discussion and conclusion.

We also agree with the observation that SOC increase does have a marginal effect on hydrological processes (as mentioned in lines 459-460), but we do not agree that it is fully negligible. As discussed in lines 545, larger SOC increases can influence low flow periods, especially in smaller and relatively dry catchments with a high share of agricultural land, where even small changes in the frequency of low flows can meaningfully limit water availability for agricultural use and ecosystem functioning.

5) Our replies to the minor comments/responses:

5.1) *"Lines 6-8. The authors only report result at the grid scale, but the title involves the catchment scale. I would suggest mentioning catchment-scale results as well."*

Thank you for this observation, we agree and will adjust the abstract accordingly.

5.2) *"Section 2.2. The time step is missing."*

That is correct, we will add this information in Section 3.3 "Model set-up and evaluation".

5.3) *"Line 205. Spaces are needed for "CKsat1 to CKsat6 are"."*

Thank you, we will correct this.

5.4) *"Lines 232-233 and Figure 3. 2017 appears dry and hot."*

Thank you, 2017 is indeed dry, but not as hot 2018, 2020 or 2022. We will adjust the text accordingly.

5.5) *"Lines 264-268. "selected" to "select"."*
Thank you, we will adjust this.

5.6) *"Line 330. "dont" to "do not"."*
Thank you, we will adjust this.

5.7) *"Line 332. Suggest removing this sentence."*
Thank you, the sentence is indeed redundant and will thus be removed.

5.8) *"Lines 334-335. Please correct this sentence."*

We will revise the sentence as follows: "The decrease in ρ_b propagates through the model (Fig. 1), leading to average increases of 3.2 %, 6.2 %, and 8.8 % in both θ_{Sat} and θ_{FC}".

5.9) *"Figure 7a. The legend is missing. Change "scenario" to "base scenario" in the legend of Figure 7c."*

Thank you, we realize that this was unclear. We will revise in the figure caption that the legend actually applies for all panels. In panel *a* the lines just overlap so much that we see this in grey-ish colour. And the word "scenario"in the legend simply shows that these 5 objects are the scenarios. To better clarify this part, we will change it to "scenarios: ".

5.10) *"Line 367. Figure 9b is missing."*
Thank you for the comment. Panel *b* in Figure 9 does not exist anymore, we will correct line 367 accordingly.

5.11) *"Lines 459-460. Does this mean that we can ignore SOC effects on hydrological processes?"*
We would like to refer to our response above to reviewer comment 4)

5.12) *"Line 467. This is misleading. ET is widely measured by flux towers (see https://fluxnet.org/)."*

Thank you for this comment, we see that the way we phrased this was misleading, but what we mean is, that in experiments where SOC is increased, ET is not measured, since this is not the target variable easy to measure. We will therefore adjust the sentence to: "Direct comparison with experiments is difficult since ET is rarely measured in field experiments; PAWC is often used as a proxy due to its influence on transpiration and plant productivity (Feifel et al., 2023)."

5.13) *"Line 619. "takeaways" to "findings"."*
Thank you, we will adjust this.

5.14) *"Line 672. The content of Appendix A5 is missing."*

Thank you, we will correct this.

5.15) *"Lines 785-790. Repeated reference."*
Thank you, we will correct this.

5.16) *"There are too many figures in the appendix. I would suggest moving some of them to the supporting information."*

Thank you. We will carefully re-consider the option on what is actually needed in the Appendix; and will move some figures into the Supplementary Information (e.g., A4, A5, A6, A8 and A14).

6) Sources mentioned in our replies:

Vereecken, H., Maes, J., & Feyen, J. (1990). Estimating unsaturated hydraulic conductivity from easily measured soil properties. Soil Science 149(1), 1-12.

Minasny, B., McBratney, A. B. (2018). Limited effect of organic matter on soil available water capacity. European Journal of Soil Science, 69(1), 39-47. https://doi.org/10.1111/ejss.12475

García-Palacios, P., Crowther, T. W., Dacal, M., Hartley, I. P., Reinsch, S., Rinnan, R., Rousk, J., van den Hoogen, J., Ye, J.-S., Bradford, M. A. (2021). Evidence for large microbial-mediated losses of soil carbon under anthropogenic warming. Nature Reviews Earth Environment, 2(7), 507-517. https://doi.org/10.1038/s43017-021-00178-4

Keel, S. G., Anken, T., Büchi, L., Chervet, A., Fliessbach, A., Flisch, R., Huguenin-Elie, O., Mäder, P., Mayer, J., Sinaj, S., Sturny, W., Wüst-Galley, C., Zihlmann, U., Leifeld, J. (2019). Loss of soil organic carbon in Swiss long-term agricultural experiments over a wide range of management practices. Agriculture, Ecosystems Environment, 286. https://doi.org/10.1016/j.agee.2019.106654

Lal, R. (2013). Intensive Agriculture and the Soil Carbon Pool. Journal of Crop Improvement, 27(6), 735-751. https://doi.org/10.1080/15427528.2013.845053
Poeplau, C., Dechow, R. (2023). The legacy of one hundred years of climate

change for organic carbon stocks in global agricultural topsoils. Sci Rep, 13(1), 7483. https://doi.org/10.1038/s41598-023-34753-0

Sanderman, J., Hengl, T., Fiske, G. J. (2017). Soil carbon debt of 12,000 years of human land use. Proc Natl Acad Sci U S A, 114(36), 9575-9580. https://doi.org/10.1073/pnas.1706103114

Walker, T. W. N., Kaiser, C., Strasser, F., Herbold, C. W., Leblans, N. I. W., Woebken, D., Janssens, I. A., Sigurdsson, B. D., Richter, A. (2018). Microbial temperature sensitivity and biomass change explain soil carbon loss with warming. Nat Clim Chang, 8(10), 885-889. https://doi.org/10.1038/s41558-018-0259-x

Response to Reviewer 2

January 29, 2026

We thank anonymous Reviewer 2 for taking the time in reading and giving valuable feedback on our manuscript: “From Soil to Stream: Modeling the Catchment-Scale Hydrological Effects of Increased Soil Organic Carbon”. In the following, we will address of your comments (in italic) along with our responses and indicate how we will adapt the manuscript in the next phase of a potentially revised manuscript.

1) *”Lines 3: Please correct the sentence as “We investigated how increasing SOC affected catchment-scale hydrology including extremes.”*

Thank you, we will correct the sentence in the revised manuscript.

2) *”Line 7: Correct as “At the plot scale, increment of SOC resulted in higher net soil water content (2.99–8.13%) and in slightly higher evapotranspiration (0.15–0.4%),. . . .”*

Thank you, we will correct this.

3) *”Lines 15: “Overall our analysis suggest that a large-scale increase in SOC, while benefiting agricultural productivity and peak flow attenuation, may also induce trade-offs by potentially reducing groundwater recharge and downstream water.” Since this is the significant conclusion of abstract; try to rewrite the sentence though it this form it is ambiguous.”*

Thank you, we agree and will adjust the sentence as follows: “Overall, our analysis suggests that large-scale increases in SOC can provide hydrological benefits such as enhanced agricultural productivity and reduced peak flows, but may involve trade-offs through reduced groundwater recharge and thus water availability." However, since the impact depends on the catchment properties, as explained the preceding section, we still prefer to use the formulation "can".

4) *"Keywords: "SOC enhancement; Broye catchment, Western Switzerland"*

Thank you for pointing that out, we will add these keywords.

5) *The sequence of the "Introduction" does not directly comply with the core aim of this study. In fact, it focuses mainly on the agriculture and effects rather than the modelling targets. Please try to adapt and connect SOC and modelling procedures. Also, it involves general explanations such as "However, irrigation increasingly competes with ecological needs and other sectors (Brunner et al., 2019)."*

Thank you, we agree that the Introduction could more clearly connect SOC changes with the modeling objectives. Following your suggestion, we will adjust the Introduction in the revised manuscript. We propose to shorten the sections on drought, irrigation, and management practices so to focus on the core messages. We will revise the texts on improve the transition from management to modeling, by 1) stating more clearly why we consider SOC increase following management adaptations, 2) how this might influence hydraulic properties; 3) how in other studies the impact of land use changes on catchment hydrology has been investigated; 4) why modeling is often the only possible option to do so; and lastly 5) how we will use our modeling scheme to answer the research question i.e., How will increases in SOC at a larger scale affect catchment-scale hydrological processes, including extremes?. We hope that this restructuring now better reflect the rationale for using a model-based approach, thereby aligning the Introduction more closely with the core aims of the study. We will also carefully consider generic statements e.g., we will remove the statement on irrigation.

6) *Line 21: Order of the references must follow the chronology.; E.g. ; Tijdeman et al., 2022; Hou et al., 2024*

Thank you, we will correct this and make sure that the order of references follows the chronology throughout the manuscript.

7) *Line 30: "When precipitation and soil moisture deficits coincide with high air temperatures and high evaporative demand, plants close their stomata to limit water loss (Gupta et al., 2020)." What does this sentence intend to mean?*

Thank you, this sentence will be omitted in the revised manuscript.

8) *Line 50: Check the order of the references' chronology.*

We will check this and make sure that the order of references follows the chronology throughout the manuscript.

9) *Also try to revise English.*

Thank you, we will try our best to improve language, while also taking the help of copy-editing service provided by the journal.

10) *Line: 108: "A range of management practices are reported that increase SOC:" What do you mean?*

While revising the Introduction, this sentence was deleted.

11) *Line 230: Here the study area is not well-defined; we learn only its size. The following expressions are time inputs etc.*

Thank you, we agree, most properties of the study area are not listed in the text, since Figure 3 provides a more detailed overview, including elevation, land cover, soil texture, climate and discharge. As we see the need for those aspects to be explicitly mentioned, we will include the following text to compliment and connect to Figure 3:

"The landscape within the study area is dominated by cropland interspersed with small forest patches. Underlying soils are primarily loams, clay loams or sandy loams, with rather low SOC contents (averaging around 2.2% in the topsoil, as shown in Figure 3). The region has a temperate climate, with mean annual precipitation of 1142 mm and mean annual temperature of 9.12 °C, averaged over 1993-2022). The streams exhibit a typical pluvial flow regime, with discharge peaks in winter and low flows in summer, characteristic of lowland Swiss agricultural catchments."

12) *Line 267: "(following Jarvis (1989) as implemented and documented by Demirel et al. (2018))"?*

Thank you, we agree this sentence was not very clear. We will revise it as such "For the soil moisture routine, we selected the process option in mHM where ET in each soil layer is regulated by the relative available soil moisture and fractions of roots in each soil layer following the formulation of Demirel et al. (2018)."

13) *Results & Discussion: The other catchments are included here. However, they are not mentioned at the "Abstact".*

Thank you for this clarification. We realize, that we have to clarify that we applied the model to the whole Broye catchment, but also evaluate the results on the scale of the 4 sub-catchments within the larger catchment. We will add the following sentence to subsection 3.1: "For clarity, we refer to the full modeled domain as the Broye catchment and to the gauged subcatchments as Broye (subcatchment), Flon, Arbogne, and Petit Glâne". The subcatchments are not named explicitly in the abstract, but we do mention them in lines 12-14: "In warmer and drier subcatchments, low flow frequency increased in some years, whereas in cooler and wetter subcatchments, conditions in spring and early summer produced a beneficial effect, slightly reducing low flow frequency".

14) *In the Figure 6, the scenario should be plural: "scenarios".*

Thank you, we will correct this.

15) *Discussion: Discuss also the performance of the model particularly for this study and catchment.*

This is of-course an important point, but we believe that we already thoroughly discuss the model performance for this study and catchments and even compare the results to other studies done in the same area with different models (section 5.1.1, lines 422-432: "For streamflow, the calibrated mHM model performs very well for the Broye (Payerne) subcatchment (KGE = 0.91, NSE = 0.86), outperforming previous applications of conceptual models (SWAT, Zarrineh et al. 2018; PREVAH, Muelchi et al. 2021) and a physics-based model (Alpine3D, Lehning et al. 2006). Despite relatively short calibration and evaluation periods (four and three years), these performance values are high (see Supplementary Material for a more detailed comparison), underlining the model's ability to reproduce observed discharge dynamics. Seasonal low-flow regimes are fairly well reproduced for the Broye and Petit Glâne subcatchments, while the frequency of low flows is underestimated for the Flon and overestimated for the Arbogne. The differences in performance can be traced to biases in the precipitation input fields. Such biases were already reported in earlier studies using the same precipitation data product (Muelchi et al. 2021, Brunner et al. 2019)). While our adjustments to the precipitation input substantially reduced these biases, they were not fully eliminated. A more systematic bias correction would be required. Still, this adaptation was essential to reliably simulate soil moisture dynamics. Here, we were able to reproduce observed soil moisture time series with relatively good performance (Appendix C2).").

16) *Check the references. Exp.: "Fry, E. L., Evans, A. L., Sturrock, C. J., Bullock, J. M., and Bardgett, R. D.: Root architecture governs plasticity in response to drought, Plant Soil, 433, 189–200, https://doi.org/10.1007/s11104-018-3824-1, fry, Ellen L Evans, Amy L Sturrock, Craig J Bullock, James M Bardgett, Richard D eng Netherlands 2018/01/01 Plant Soil. 2018;433(1):189-200. doi: 10.1007/s11104-018-3824-1. Epub 2018 Oct 25., 2018." I think wrong-written. Or automatically referencing procedure was wrong-applied. Check the others.*

Thank you, we will correct this reference and throughly check the entire bibliography again for errors.

17) Sources mentioned in our replies:

Brunner, M. I., Bjornsen Gurung, A., Zappa, M., Zekollari, H., Farinotti, D., Stahli, M. (2019). Present and future water scarcity in Switzerland: Potential for alleviation through reservoirs and lakes. Sci Total Environ, 666, 1033-1047. https://doi.org/10.1016/j.scitotenv.2019.02.169

Lehning, M., Völksch, I., Gustafsson, D., Nguyen, T. A., Stähli, M., Zappa, M. (2006). ALPINE3D: a detailed model of mountain surface processes and its application to snow hydrology. Hydrological Processes, 20(10), 2111-2128. https://doi.org/10.1002/hyp.6204

Muelchi, R., Rössler, O., Schwanbeck, J., Weingartner, R., Martius, O. (2021). An ensemble of daily simulated runoff data (1981–2099) under climate change conditions for 93 catchments in Switzerland (Hydro-CH2018-Runoff ensemble). Geoscience Data Journal, 9(1), 46-57. https://doi.org/10.1002/gdj3.117

Zarrineh, N., Abbaspour, K., Van Griensven, A., Jeangros, B., Holzkämper, A. (2018). Model-Based Evaluation of Land Management Strategies with Regard to Multiple Ecosystem Services. Sustainability, 10(11). https://doi.org/10.3390/su10113844