<u>Anonymous Referee #1</u>

General remarks

Summary. In a companion paper, a new framework for the parametrization of soil was proposed, accounting for the presence of soil organic matter (SOM) using the soil mixture theory. Here, this framework is tested in the context of global climatological simulations, using the ISBA-CTRIP land surface modelling scheme. Additionally, a correction for the compaction of mineral soil was evaluated as well.

The results indicate that the new scheme has an overall modest, yet positive impact on the terrestrial water and energy cycle. Furthermore, it reveals some inconsistencies in the input soil maps (SoilGrids).

General comments. This work is a significant and qualitative contribution to the land surface modelling community. Soil organic matter and its impact on soil properties is highly complex to account for at global scale, and is often overlooked. This follow-up to the companion paper is showing the impact of accounting for SOM with a pragmatic process-based scheme, and sheds light on the associated uncertainties and challenges. It is clearly presented and contains all the details to reproduce the results.

Response: We thank the reviewer for this positive assessment. We appreciate that the relevance of accounting for soil organic matter effects on hydraulic and thermal properties in land surface models has been recognized, together with the clarity and reproducibility of the proposed framework.

Remark 1: Overall, the metrics used to evaluate the new framework show a modest improvement of the model. Most of these metrics are dominated by the global climatological behavior of the model, but the temporal variability, seasonal cycles, and anomalies are largely masked. Perhaps this could be considered out of scope, but the presented results leave the reader wondering what the impact is on temporal dynamics from this new framework, especially since some soil parameters are modified rather drastically.

Response: We agree that several metrics used in the manuscript emphasize climatological states and mean spatial patterns. However, temporal behavior was not fully absent from the submitted version. The seasonal ΔTWS cycle from GRACE, the daily discharge skill scores, and the all-month distributions of evapotranspiration and near-surface soil temperature already provide constraints on seasonal and temporal variability. To address this point more explicitly, we added a new supplementary figure showing monthly climatologies and annual-mean anomalies over a SOC-rich Siberian domain, where the impact of SOM parameterizations is expected to be clear. This diagnostic shows that the SOM parameterizations mainly alter the mean hydrothermal state and its seasonal expression. The largest changes occur during the warm season for evapotranspiration and soil temperature, while annual anomalies remain close across experiments. For soil moisture and saturation degree, the response mostly reflects a shift in the model equilibrium state rather than a change in interannual variability. We also performed an additional ANOVA-like decomposition of monthly differences relative to CTL into mean-state, seasonal-cycle, and interannual components. This confirmed that the interannual component is generally small compared with the mean-state and seasonal-cycle components, except for runoff, where the interannual contribution is larger and more uncertain. Because this additional decomposition does not change the interpretation and would broaden the scope of the paper, we did not include it as a new main analysis. Instead, we added the regional temporal diagnostic to the Supplement and inserted a short clarification in the Discussion.

Change made: We added Fig. S16 to the Supplement and added the following paragraph to the Discussion: “Although the evaluation mainly focuses on climatological states, several diagnostics also constrain temporal behavior, including the seasonal ΔTWS cycle from GRACE, daily discharge skill scores, and all-month distributions of evapotranspiration and near-surface soil temperature. Additional regional diagnostics over a SOC-rich Siberian domain indicate that the SOM parameterizations mainly alter the mean hydrothermal state and its seasonal expression, with larger effects during the warm season for evapotranspiration and soil temperature, while annual anomalies remain close across experiments (Supplementary Figure S16). For saturation degree, the response mostly reflects a shift in the model equilibrium state rather than a change in interannual variability. This supports the interpretation that the new framework affects mean hydrothermal conditions and their seasonal modulation more than the temporal variability itself."

Remark 2: As the authors indicate, the methodology relies on correct input values from SoilGrids. Since it is known that there is a substantial uncertainty associated with bulk density of organic soils, it might be reasonable to constrain the bulk density of organic matter more strongly. See below.

Response: We agree that uncertainty in soil bulk density, especially for organic-rich soils, is a key issue for the present implementation. This limitation is already identified in the manuscript through the comparison between SoilGrids and WoSIS bulk-density profiles in Fig. 13. This comparison shows that the discrepancy between the two datasets increases with f_{moc}, suggesting that SoilGrids may maintain bulk densities that are too high under high-SOC conditions. However, strongly constraining ρ_{bom} is not neutral in the proposed framework (Decharme 2025). First, it could compensate for inconsistencies in the input pair (f_{moc}, ρ_b) without correcting their origin. Second, it would move the method closer to empirical approaches such as DE16, where the organic bulk density is prescribed a priori. In contrast, the objective of the DE25 framework, following the companion theoretical study, is to infer ρ_{bom} and f_{vom} from mass-volume relationships, using the available SOC, bulk density, and texture information, while only applying broad physical bounds to avoid non-physical values. We therefore consider that the more consistent way to address this issue is to improve or correct the input bulk-density product, rather than impose a narrow constraint on the diagnosed organic-phase density.

Specific comments

Remark 1: Litter layer – Is it only activated for forest soils? Is there a risk that the impact of soil organic matter in the topsoil is double accounted for in the soil properties?

Response: In the present configuration, the litter option is activated only for forest tiles. It represents an explicit surface layer located above the SoilGrids-derived soil column. In contrast, the SOM parameterizations discussed in Sect. 2.2 are applied to the ISBA soil layers themselves, using SoilGrids SOC and bulk density, whose first prescribed interval is 0 to 5 cm. Therefore, litter and topsoil SOM effects can physically combine, as expected in forested soils, but they are not applied twice to the same numerical layer. We therefore do not expect a direct double counting of topsoil organic matter effects. We clarified this point in Appendix A2.

Change made: We added the following clarification in Appendix A2: "*In the present configuration, this litter option is activated only for forest tiles. It represents an explicit surface layer located above the SoilGrids-derived soil column. The SOM parameterizations described in Section 2.2 are instead applied to the soil layers themselves, using SoilGrids SOC and bulk density, whose first prescribed interval is 0 to 5 cm. Thus, litter and topsoil SOM effects can physically combine in forested soils, but they are not applied twice to the same numerical layer.*"

Remark 2: To justify the mineral soil compaction correction, it is stated that the soil samples used in the development of the Cosby 1984 PTF were "not explicitly subjected to mechanical compaction", whereas the samples used to build the SoilGrids dataset are assumed to reflect "closer to compacted than to ideal, noncompacted conditions". While it is appreciated that subsoil compaction is taken in consideration, these assumptions and the logic should probably be rephrased or clarified. A large fraction of the dataset used in Cosby 1984 are subsoil (B and even C-horizon) samples, mostly from agricultural fields, so a certain degree of compaction can be assumed. Furthermore, it is not clear why the SoilGrids samples are assumed to have a stronger degree of compaction. This might be true for deeper subsoil samples, but is probably not the case for topsoil layers. Finally, the correction used here is actually the computation of the packing density, which is a correction to allow comparison of bulk densities and eliminate the influence of soil texture. To my knowledge, using these equations to compute a "compacted" bulk density is not their intended use, at least, that is not how it is used in the cited references either. Although it is demonstrated in the results that this correction seems to improve the estimated porosity, the justification of this approach needs some work.

Response: We agree that the original wording could be read as a binary distinction between "non-compacted" Cosby et al. (1984) samples and "compacted" SoilGrids samples, which was not the intended interpretation. We have therefore revised the rationale of Sect. 2.2.3 to clarify this point. Our assumption is not that the samples used to derive the Cosby et al. (1984) PTFs were free of any compaction. Rather, the relevant point is that these PTFs use texture only and do not include bulk density or any compactness index as predictors. Their predicted porosity and hydraulic parameters therefore define a texture-based mineral reference state in which the in situ compactness state is not explicitly represented. In contrast, field bulk-density measurements represent the dry mass per unit volume of soil sampled in situ and therefore include the resulting compactness state of the soil. This state may arise from natural packing, horizon development, pedogenic consolidation, land management, or mechanical compaction. It is therefore reasonable to assume that gridded bulk-density products derived from in situ soil observations, such as SoilGrids or HWSD, reflect in situ compactness states rather than ideal non-compacted reference conditions. We also clarified the role of the Renger et al. relationships. We do not use these equations as a prognostic or process-based model of mechanical compaction. We use them as an empirical packing-density-based constraint to adjust the mineral reference bulk density before recomputing the mixture variables. This is consistent with using packing-density relationships as texture-dependent indicators of compactness state. To avoid ambiguity, the section and the corresponding terminology now refer to a "mineral soil compactness adjustment", and the corrected variables are described as compactness-adjusted mineral properties. The relevance of this adjustment is also supported by Figure 2. DE25c reduces the remaining overestimation of w_{sat} found in DE25, especially for low-SOM and high-bulk-density samples, where the mineral phase controls most of the total pore volume. This empirical behavior supports the need to constrain the mineral reference state with field bulk density. At the same time, the global simulations show that this adjustment remains secondary relative to the dominant SOM-related changes, acting mainly as a local modulation of mineral pore space and associated hydraulic properties.

Change made: We revised Sect. 2.2.3 to clarify the rationale for DE25c. In addition, we replaced "mineral soil compaction adjustment" by "mineral soil compactness adjustment" throughout the manuscript, including in the title. The corrected variables are now described as compactness-adjusted mineral properties. The revised text states that the Cosby et al. (1984) PTFs are texture-based and do not explicitly represent an in situ compactness state, whereas gridded bulk-density products derived from field observations reflect such a state. We also specify that compactness refers to the density state reflected by ρ_b, not necessarily to a specific mechanical process such as traffic-induced compaction. This terminology change does not alter the DE25c formulation. It clarifies that the adjustment constrains the mineral reference state using field bulk density, and Figure 2 shows that this correction reduces the remaining overestimation of w_{sat} in low-SOM and high-bulk-density samples.

Remark 3: A detail, but it could be mentioned that this is the white-sky albedo (I think?).

Response: We agree. The albedo used here is the snow-free white-sky albedo. We have clarified this in Sect. 2.3.1.

Change made: We add the following sentence describing the MODIS-derived snow-free albedo in section 2.3.1: "*The resulting albedo corresponds to the snow-free white-sky albedo.*"

Remark 4: Fig. 3: top row plots, low-organic soil. It is at first sight surprising that the compacted soil, with substantially higher mineral bulk density, has roughly the same total porosity as the non-compacted soil, despite the low organic material content. This is due to the compensation in the organic bulk density, which is much lower for the compacted soil, yielding a higher volumetric fraction of organic material. Due to the freedom in bulk density of organic matter, the consequence of accounting for compaction is that more weight is given to the organic matter in the estimation of soil properties. The same can be observed in the other rows and in Fig. 4. It could be highlighted in the manuscript that compacting the mineral soil doesn't necessarily result in a more compacted soil, but rather gives more weight to the organic component of the soil.

Response: We agree with this interpretation. In DE25c, the mineral soil compactness adjustment is applied to the mineral reference phase, while the grid-scale bulk density ρ_b remains prescribed by SoilGrids. Therefore, increasing ρ_{bms}^c does not necessarily lead to a proportional decrease in bulk soil porosity. Through the mixture inversion, a larger ρ_{bms}^c reduces the inferred organic bulk density ρ_{bom} for a given input pair (f_{moc}, ρ_b), which in turn increases the volumetric organic fraction f_{vom}. The resulting bulk porosity therefore reflects the balance between a lower mineral porosity and a larger weight of the organic component in the mixture. This mechanism was already described in Sect. 3.1.2, where Figure 3 is discussed, but we agree that it could be highlighted more explicitly. We therefore added a short clarification to state that the net change in w_{sat} can remain limited when the increased organic contribution partly compensates for the lower mineral porosity.

Change made: We revised Sect. 3.1.2 and we added the following clarification: "…is consistent with the fact that a denser mineral domain implies a larger inferred organic volumetric contribution for a given bulk density. *This compensation is a direct consequence of using the grid-scale ρ_b as a constraint in the mixture inversion: increasing ρ_{bms}^c reduces the mineral pore volume, but it also increases the inferred weight of the organic component in the bulk soil properties.*" ; "…between organic-rich surface layers and mineral-dominated deeper layers remains similar. *However, because the same adjustment also increases the inferred f_{vom} for a prescribed ρ_b, the net change in bulk w_{sat} can remain limited when the increased organic contribution partly compensates for the lower mineral porosity.*"

Remark 5: Associated to the previous remark, the reference to optimum degree of compactness seems not fully appropriate here. First, this does not refer to the mineral fraction only, but to the total bulk density, which is unaffected by the correction. Second, it is the ratio between the bulk density and the reference bulk density, and is an indication for the degree of compaction. In this manuscript, it is the ratio between the mineral bulk density and the compacted mineral bulk density, where the latter is supposedly representing field conditions more accurately. It is not clear how to match these two approaches.

Response: We agree that the ratio ρ_{bms}/ρ_{bms}^c should not be interpreted as directly equivalent to the optimum degree of compactness defined by (Håkansson, 1990). The latter refers to total soil bulk density relative to a reference bulk density, whereas our ratio is computed only for the mineral reference phase within the DE25c mixture framework. Our intention was only to provide an order-of-magnitude comparison for the amplitude of the mineral-phase adjustment. We have therefore clarified this point in Sect. 3.1.3. The comparison with reported optimum degrees of compactness is now presented only as an indicative benchmark, not as a direct estimate of the bulk-soil degree of compactness.

Change made: We revised the relevant sentence in Sect. 3.1.3 as follows: "Averaged over land, this ratio remains close to 0.87 across depths, corresponding to an average mineral-phase densification of about 13%. *This value is close to reported optimum degrees of compactness (Håkansson, 1990; Håkansson and Lipiec, 2000; Keller and Håkansson, 201), but the comparison is only indicative because the ratio considered here applies to the mineral reference phase, not to total bulk soil density. It should therefore be interpreted as a qualitative benchmark for the magnitude of the DE25c adjustment, not as a direct estimate of the bulk-soil degree of compactness.*"

Remark 6: Why do we see only small w_{sat} differences in Fig. 3 and 4, whereas they are more evident in Fig. 2? Is it due to the imposed limit of 1 g cm^{-3} for the bulk density of organic matter?

Response: The imposed upper bound of 1 g cm^{-3} on ρ_{bom} is not the reason for the smaller apparent differences in w_{sat} in Figures 3 and 4 compared with Figure 2. This bound is only a numerical safeguard that prevents unrealistically high organic bulk densities when the mixture inversion becomes ill-conditioned. The contrast between the figures mostly reflects the nature of the data used. Figure 2 is based on in situ datasets, where organic matter content, bulk density, particle density, and observed porosity are constrained at the sample scale.

The differences between parameterizations are therefore more directly visible, especially for samples with low measured porosity and high bulk density. In contrast, Figures 3 and 4 show properties diagnosed from SoilGrids inputs. These inputs are spatially gridded products and can include inconsistencies in the pair (f_{moc},ρ_b), especially in organic-rich soils. When high SOC is combined with relatively high ρ_b, the mixture inversion can diagnose high ρ_{bom}, which reduces the inferred porosity of the organic domain and limits the increase in bulk w_{sat}. In organic-poor soils, f_{vom} remains small, so bulk w_{sat} remains close to the mineral value. The differences are therefore not absent, but they are more spatially localized in the global simulations. This is shown in Supplementary Figure S6, where changes in w_{sat} are visible near the surface, especially in SOC-rich regions with low ρ_b. Figures 3 and 4 summarize selected profiles and global distributions, which tend to smooth these localized near-surface contrasts.

Remark 7: Fig. 4: Some important differences between DE16 and DE25(c) are observed in the bulk density of organic matter and the air entry value. DE16 was based on idealized profiles from literature. It is known that properties of organic matter can be highly variable and strongly depend on peat type, e.g. Liu and Lennartz (2018). It would be relevant to elaborate a bit on the differences between DE16 and DE25, and what they reflect.

Response: The differences in ρ_{bom} and ψ_{sat} mainly reflect the conceptual difference between DE16 and DE25,DE25c already analyzed in the companion paper. DE16 corresponds to the ISBA implementation of the empirical approach derived from Lawrence and Slater (2008) and Decharme et al. (2016), in which organic soil properties follow idealized depth profiles. In contrast, DE25 and DE25c diagnose ρ_{bom} from the input pair (f_{moc},ρ_b) through the mixture equations, and then derive the organic hydraulic properties from density-dependent relationships. Figure 4 therefore shows the global propagation of these two different assumptions rather than a new calibration of organic matter properties. To avoid repeating the detailed discussion of Part 1, we added a short clarification in Sect. 3.1.3 indicating that the differences observed in ρ_{bom} and ψ_{sat} reflect this distinction between prescribed empirical organic profiles in DE16 and density-dependent diagnosed organic properties in DE25.

Change made: We clarified Sect. 3.1.3 as follows: "In DE16, ρ_{bom} is prescribed as an idealized depth-dependent profile and therefore shows no spatial variability. This reflects the empirical, non data-driven nature of the formulation, *inherited from the ISBA implementation of the Lawrence and Slater (2008) approach and already discussed in detail in (Decharme, 2025).*" We also revised the interpretation of the air-entry pressure response: "This contrast illustrates the uncertainty associated with empirical SOC-based parameterizations such as DE16*, and reflects the difference between prescribed organic hydraulic profiles in DE16 and density-dependent organic hydraulic relationships in DE25*".

Remark 8: As discussed in the manuscript, DE25 is prone to inconsistencies in the input. Could it be made more robust by more strongly constraining the bulk density of organic matter? Or would this be detrimental to capture the spatial variability of OM? Given the uncertainty associated with bulk density in organic soils from SoilGrids, it might be reasonable to constrain the organic matter bulk density to correct potential errors in the bulk density from SoilGrids.

Response: This point is related to the general comment on the uncertainty of bulk density in organic soils. A stronger empirical constraint on ρ_{bom} could make the formulation less sensitive to inconsistent input values. However, this would not be neutral. In DE25, ρ_{bom} is not prescribed, but diagnosed from the input pair (f_{moc}, ρ_b) through the soil mixture equations. Strongly constraining ρ_{bom} could therefore mask inconsistencies in this input pair rather than reveal them.

This is especially relevant for SoilGrids, for which Figure 13 shows that ρ_b can remain too high in high-SOC classes compared with WoSIS. Such inconsistencies should preferably be addressed at the level of the input bulk-density fields. This is why the manuscript identifies corrected SoilGrids bulk-density fields, such as those proposed by Fan et al. (2020), as a priority avenue for future sensitivity analyses. A stronger constraint on ρ_{bom}

would also reduce the ability of DE25 to represent spatial variability in organic matter properties. If ρ_{bom} were forced toward a narrow prescribed range, the approach would become closer to empirical schemes such as DE16, where organic properties are imposed from idealized profiles. This would partly weaken the purpose of the mixture-based framework, which is to diagnose organic and mineral phase properties from mass-volume relationships rather than prescribe them a priori.

Change made: We added the following clarification at the end of the discussion: "Because DE25 explicitly combines f_{moc} and ρ_b to constrain mineral and organic phase properties, such inconsistencies can propagate into simulated porosity, water storage, and hydrothermal behavior, particularly where organic soils dominate the near-surface response. *In the present framework, this propagation occurs partly through the diagnosed organic bulk density, ρ_{bom}, which controls both the volumetric organic matter fraction and the hydraulic properties assigned to the organic phase.* This sensitivity does not challenge the soil mixture formulation itself, but it delineates its dependence on the quality and internal consistency of input datasets. *A stronger empirical constraint on ρ_{bom} could reduce this sensitivity, but it would also mask inconsistencies in the input pair (f_{moc},ρ_b), reduce the spatial variability of diagnosed organic matter properties, and partly reintroduce an a priori constraint on organic matter density, closer in spirit to empirical approaches such as DE16. We therefore retain the current diagnostic formulation and identify improved or corrected bulk-density input fields as the preferred way to address this limitation.*"

Remark 9: Consider to exchange Figure 5, water content, with Figure S15, saturation degree. The latter seems more informative to understand differences in evaporation/runoff, though this figure links well with Figure 10.

Response: We agree that the saturation degree is useful to interpret the hydrological response, especially the partitioning between evapotranspiration and runoff. We nevertheless prefer to keep the water content figure in the main text because w_{gtot} is the prognostic soil water storage variable and provides the direct link with terrestrial water storage and soil thermal behavior. To address the reviewer's suggestion, we have promoted the saturation-degree diagnostic from the Supplement to the main manuscript, where it is now Figure 6.

Change made: Former Supplementary Figure S15 has been moved to the main manuscript as Figure 6. All subsequent figures have been renumbered accordingly.

Remark 10: The impact on some components of the water cycle could be elaborated on: impact on deep drainage? baseflow? Bare soil evaporation/transpiration partitioning?

Response: We agree that decomposing the water cycle response provides useful information. We added Supplementary Figure S15, which shows the 1979–2010 mean surface runoff, drainage, bare soil evaporation, and plant transpiration for CTL, together with the differences relative to CTL for DE16, DE25, and DE25c. This diagnostic shows that the changes in total runoff result from a decrease in surface runoff and a tendency toward higher drainage, especially in DE16 and DE25. This behavior is consistent with the two runoff-generation mechanisms represented in ISBA. The Dunne component decreases because higher near-surface w_{sat} and higher AWC increase soil water storage capacity and reduce the tendency of the soil to reach saturation. The Horton component also decreases where higher near-surface w_{sat} and k_{sat} increase infiltration capacity. A larger fraction of incoming water can therefore infiltrate instead of being converted into surface runoff. Part of this additional infiltrated water first increases soil water content and can then be transferred downward, thereby favoring higher drainage. The drainage diagnostic is the land surface flux component most directly related to subsurface water input relevant to groundwater storage and baseflow generation in ISBA-CTRIP. For evapotranspiration, the decomposition shows that the response is mainly expressed through higher plant transpiration and lower or weakly modified bare soil evaporation. The increase in AWC, mainly driven by the increase in w_{fc}, enhances root-zone water retention and water availability where transpiration is water-limited. In contrast, bare soil evaporation depends mainly on water availability in the uppermost soil layers. The additional infiltrated water is therefore preferentially partitioned toward root-zone storage and plant uptake, or transferred downward as drainage, rather than remaining as mobile near-surface water available for direct evaporation from the soil surface.

Change made: We added Supplementary Figure S15 and inserted an explanatory paragraph at the end of Sect. 3.2, after the discussion of total runoff and evapotranspiration

Remark 11: L676, it seems to me that the anomalies are larger in the deeper layers?

Response: Yes, the original statement was incorrect. Figure 10 does not show a systematic weakening of temperature anomalies with depth. Instead, the cooling signal persists through the soil column, and the spatial variability of the anomalies slightly increases with depth. We have therefore revised the interpretation of Figure 10. The text no longer states that the largest anomalies occur in the upper layers or that the response shows a gradual vertical decay.

Change made: We revised the paragraph describing Figure 10. The manuscript now states that the cooling is not confined to the upper soil layers and that the spatial variability of the temperature anomalies slightly increases with depth. The interpretation was also modified to emphasize that the response reflects changes in heat propagation through the soil column, in addition to changes in near-surface soil properties.

Remark 12: A side-note: On a more local scale, OM is strongly dependent on land cover. I wonder whether the impact is notable at this scale.

Response: We agree that SOM can show strong local variability and that present-day land cover may contribute to this variability. This local variability can lead to locally notable hydrothermal impacts where SOC content and bulk density differ strongly over short distances. However, this dependence is only one component of a broader set of controls. SOM also reflects climate, vegetation history, land-use history, drainage conditions, topography, parent material, soil texture, decomposition state, disturbance, and time. In the present study, SOC and bulk density are prescribed from SoilGrids as gridded soil inputs at the model scale. These fields implicitly integrate several environmental and historical controls, but they are not separated by present-day land-cover class within each grid cell. The simulations can therefore quantify the large-scale response to gridded SOM-related soil properties, but they are not designed to isolate or quantify local land-cover-specific impacts. A local attribution of SOM-related hydrothermal effects to land cover would require a dedicated analysis using higher-resolution soil data and land-use or land-cover history information, which is beyond the scope of this global evaluation.

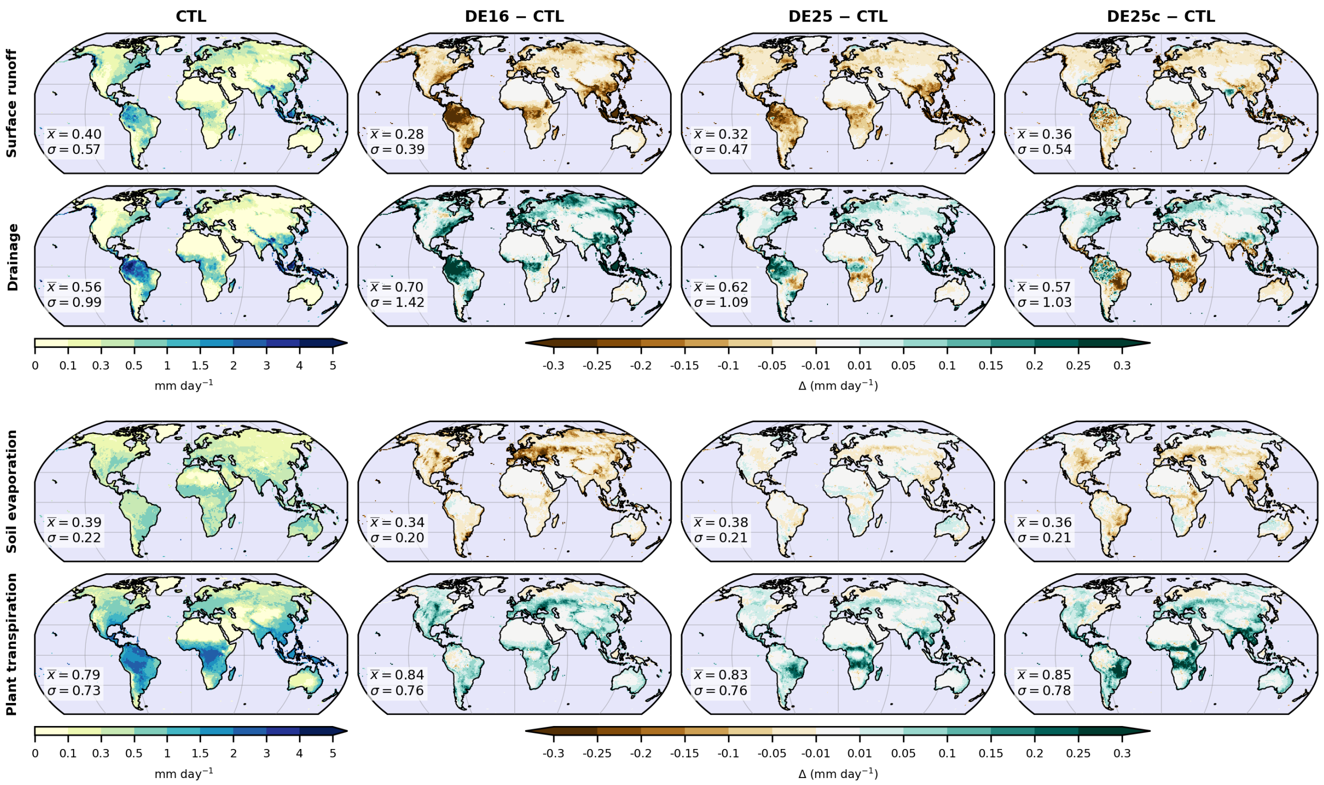

Figure S15. As in Figure 9, but for the 1979–2010 mean surface runoff, drainage, bare soil evaporation, and plant transpiration. Surface runoff and drainage are the two components of total runoff considered here, while bare soil evaporation and plant transpiration are shown to document the main land-surface partitioning of total evapotranspiration.

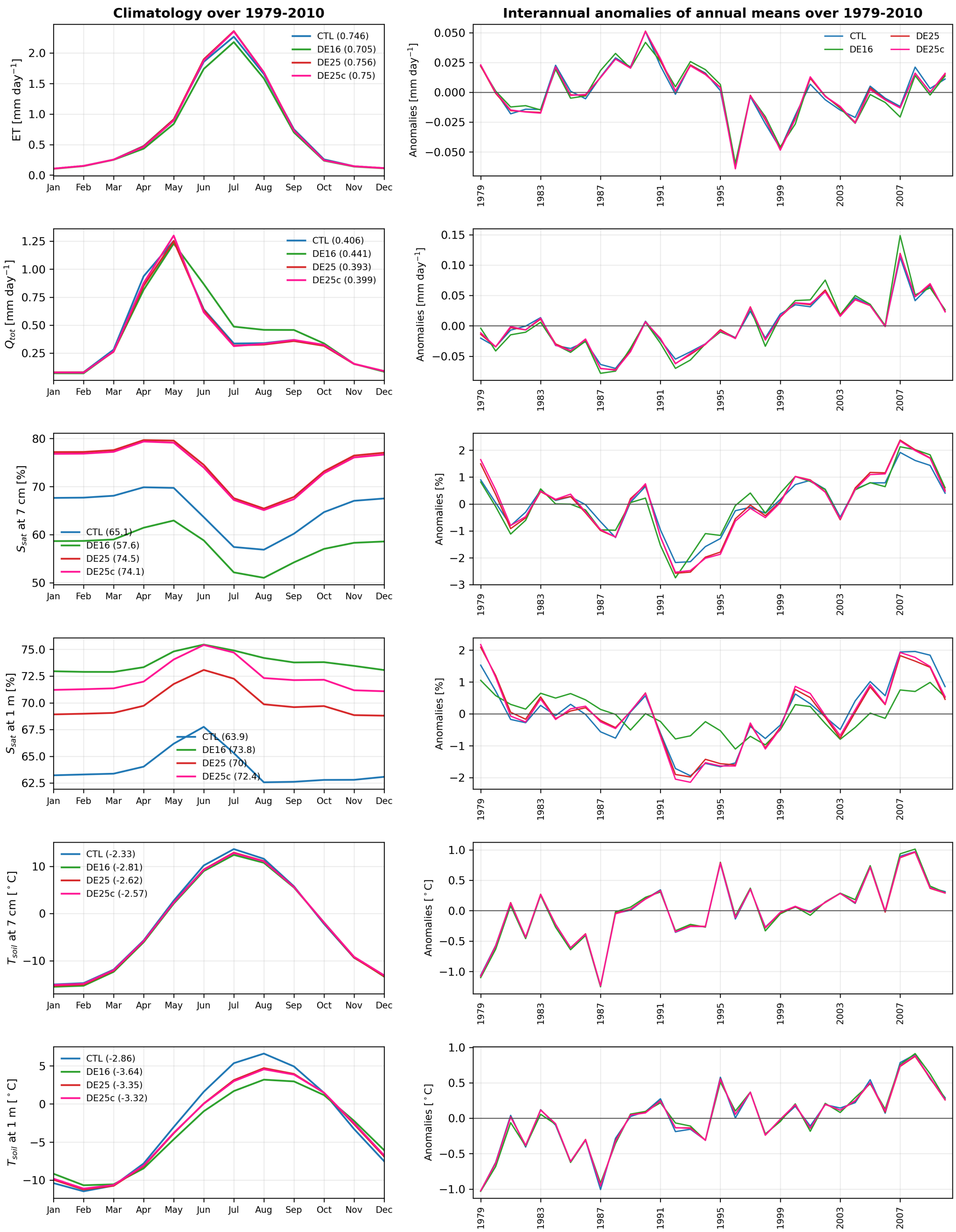

Figure S16. Regional temporal diagnostics over a SOC-rich Siberian domain, 40^{o}E to 180^{o}E and 45^{o}N to 85^{o}N, for CTL, DE16, DE25, and DE25c over 1979 to 2010. The left column shows monthly climatological cycles of evapotranspiration, ET, total runoff, Q_{tot}, soil saturation degree, S_{sat}, at 7 cm and 1 m depth, and soil temperature, T_{soil}, at 7 cm and 1 m depth. Values in parentheses in the legends indicate the corresponding 1979 to 2010 means. The right column shows interannual anomalies of annual means, computed after removing the 1979 to 2010 mean of each experiment.

<u>**Anonymous Referee #2**</u>

General remarks

This manuscript investigates how the revised representation of soil organic matter, together with a mineral compaction adjustment, influences land surface processes within the ISBA-CTRIP modeling system. The study addresses an important and often overlooked inconsistency in compaction status between pedotransfer-function-based soil properties and bulk density estimates derived from SoilGrids, and proposes a more physically consistent framework to account for compaction effects. Overall, the manuscript makes a valuable contribution by quantifying the impact of these processes on global land simulations.

Response: We thank the reviewer for this positive assessment of the manuscript and for recognizing the contribution of the proposed framework to the representation of soil organic matter and mineral soil compactness effects in land surface modeling.

Major comments

Remark 1: The manuscript discusses the analytical formulation used to diagnose organic matter bulk density (ρ_{bom}), noting that the function may become numerically unstable when ρ_b approaches ρ_{bms}, potentially leading to divergence or nonphysical values. To mitigate this issue, a lower bound is applied to f_{mom} in the computation of ρ_{bom}. However, the implementation in Part 1 (Decharme, 2025) uses the uncorrected f_{mom}, which introduces an inconsistency between the theoretical formulation and its numerical application. The relatively large values of DE25c compared to DE25 in Figure 3 suggest that this bound does indeed exert a strong influence. It would be helpful if the authors could provide a spatial diagnostic showing where the lower bound is active (i.e., grid cells where f_{mom} is constrained), in order to better assess the robustness of this bound. In addition, a brief sensitivity analysis of key results using the corrected f_{mom} would help evaluate how much the prescribed lower bound affects the main conclusions, and whether this numerical treatment could alter any of the conclusions presented in Part 1.

Response: We agree that the numerical treatment used in the diagnostic computation of ρ_{bom} needed to be documented more explicitly. The lower bound applied to f_{mom} is not intended to represent a physical change in SOM content. It is a numerical safeguard used only in the computation of ρ_{bom} when Equation 2 becomes ill-conditioned, which occurs when f_{mom} is very small and ρ_b approaches the mineral-phase bulk density. In this limit, the organic contribution to the bulk soil properties is expected to remain small, but the analytical expression for ρ_{bom} can diverge or produce nonphysical values. We therefore clarified in Sect. 2.2.2 that f_{mom}^* is the regularized value of f_{mom}, introduced as a numerical safeguard in the diagnostic computation of ρ_{bom}. We also revised Sect. 3.1.3 to quantify where this safeguard is activated and to show that it remains confined to low-SOC, mineral-dominated conditions. To document this point, we added hatching in the ρ_{bom} panels of Supplementary Figure S4 and in the corresponding f_{vom} panels of Supplementary Figure S5. In DE25, the safeguard is activated over 0.97, 1.88, 9.98, 14.04, and 15.67% of valid land grid points at 0.5, 7, 50, 90, and 175 cm depth, respectively. In DE25c, the corresponding fractions are 0, 0, 0.12, 1.11, and 1.59%. These fractions show that the compactness-adjusted formulation strongly reduces the occurrence of this numerical safeguard. We further characterized the affected grid points. In DE25, they are mainly located in desert areas and other low-SOC regions. Their median f_{moc} decreases from 0.0103 kg kg^{-1} near the surface to 0.0041 kg kg^{-1} at 175 cm, while their median ρ_b remains close to 1.55 g cm^{-3} across the selected depths. These conditions result in small median f_{vom} values, decreasing from 3.45% near the surface to 1.43% at 175 cm. In DE25c, affected grid points occur only from 50 cm downward, with median f_{moc} values between 0.0027 and 0.0042 kg kg^{-1}, median ρ_b values between 1.69 and 1.73 g cm^{-3}, and median f_{vom} values between 1.09 and 1.59%. These diagnostics confirm that the safeguard is activated in dense, low-SOC, mineral-dominated soils, where the volumetric organic fraction remains small. Its effect on the effective bulk properties is therefore limited at the global scale. The larger f_{vom} values obtained in DE25c relative to DE25 should consequently not be interpreted as the result of a stronger activation of the safeguard. They mainly arise from the mineral soil compactness adjustment itself. Increasing ρ_{bms}^{c} modifies the mixture inversion, shifts the diagnosed ρ_{bom} downward, and increases the inferred f_{vom} for a given (f_{moc},ρ_b) pair. This mechanism is independent of the numerical safeguard and explains why DE25c can show slightly larger f_{vom} than DE25 while activating the safeguard much less frequently.

Change made: We revised Sect. 2.2.2 to describe f_{mom}^{*} as the regularized value of f_{mom} introduced as a numerical safeguard in the diagnostic computation of ρ_{bom}. We also revised Sect. 3.1.3 to quantify the occurrence of this safeguard, to report the corresponding median f_{moc}, ρ_b, and f_{vom} values, and to clarify that the affected grid points correspond to dense, low-SOC, mineral-dominated soils. Finally, we added hatching to Supplementary Figures S4 and S5 to show where the safeguard is active.

Remark 2: The mineral compaction parameterization introduced in Section 2.2.3 is an important component of the proposed framework, but it appears to be formulated primarily as a function of soil texture, without explicitly considering the thermodynamic state of the soil. In practice, soil compaction processes may depend on temperature-dependent properties such as liquid water and ice content. Frozen soils, for instance, are generally less compressible than unfrozen soils, which could affect the expression for compacted bulk density, particularly the values of the empirical parameters involved. It would therefore be helpful if the authors could clarify the assumptions behind the current formulation, especially whether it is intended to represent long-term equilibrium conditions. A brief discussion of the potential implications of ignoring temperature- or phase-dependent effects would strengthen the physical interpretation of the scheme. If possible, even a qualitative sensitivity discussion of this simplification would be valuable for assessing its robustness across climate regimes.

Response: We agree that mechanical soil compaction can depend on water content, ice content, temperature, clay swelling, and external loading. However, this is not the process represented by the DE25c adjustment. DE25c does not simulate dynamic mechanical compaction during the land surface simulation. It applies a static adjustment to the dry bulk density of the mineral fraction before the simulation, in order to diagnose dry-reference mineral hydraulic and thermal parameters that are more consistent with the gridded bulk-density constraint. The purpose of this adjustment is to account for the compactness state implied by the input dry bulk density when using purely mineral pedotransfer functions, such as CO84. As shown in Part 1 of this study, the residual overestimation of saturated water content mainly occurs for low-SOM samples with observed porosities below about 0.4 $m^3\ m^{-3}$. This bias was attributed to the texture-based Cosby-SC mineral PTF, which rarely predicts mineral saturated water contents below 0.4 $m^3\ m^{-3}$, even for dense mineral soils (Decharme 2025). This provides the main motivation for the DE25c adjustment, which uses the gridded dry bulk-density constraint to shift the mineral reference porosity toward lower, more realistic values in dense mineral-dominated soils. We therefore clarified in the Methods that DE25c represents a dry-reference mineral compactness adjustment, not a dynamic mechanical compaction scheme. The adjustment is used only to diagnose dry-reference mineral properties before the prognostic simulation. During the simulation, ISBA still computes the time-varying soil water, ice, and thermal states from these diagnosed parameters.

Change made: We revised Sect. 2.2.3 to clarify that the DE25c adjustment does not simulate dynamic mechanical compaction, but uses the gridded dry bulk density as a static compactness constraint on the mineral reference state. We also added that the companion paper showed that the texture-based Cosby-SC mineral PTF rarely predicts mineral saturated water contents below about 0.4 $m^3\ m^{-3}$, which motivates the compactness adjustment in dense mineral soils.

Remark 3: In several parts of the manuscript, the model evaluation is done primarily against the CTL experiment, rather than against independent observations. While such relative comparisons are useful for isolating the effects of the proposed parameterization changes, they do not necessarily tell us whether the modified configurations are more realistic. Including observational or reanalysis-based benchmarks would therefore strengthen the manuscript. For example, in Figure 5, comparing total water content against observational datasets would help assess whether the simulated changes are genuine improvements. In Figure 10, where clear differences in soil temperature over the northern high latitudes appear between DE25 and DE25c, incorporating observed data would help determine which configuration performs more realistically. Also, the interpretation of standard deviation in Figures 5, 9, and 10 is not entirely clear, as reduced variability does not necessarily imply improved performance. Clarifying the diagnostic purpose would be helpful.

Response: We agree that differences relative to CTL should not be interpreted as observation-based performance metrics. This is also how they are used in the manuscript. The CTL-based diagnostics are not used to evaluate model performance, but as mechanistic diagnostics to isolate the hydrothermal response of the model to the different SOM parameterizations under the same atmospheric forcing, soil input data, vegetation distribution, and model configuration. This is the purpose of Figures 5 and 10: they show the internal sensitivity of the simulated soil water and soil temperature fields to the new parameterizations, not their agreement with observations. The observational evaluation is provided separately, using variables for which large-scale independent constraints are available. These include GRACE ΔTWS, river discharge, evapotranspiration, near-surface soil temperature, active-layer thickness, and the comparison of SoilGrids bulk density with WoSIS. In particular, the thermal response over cold regions is not assessed only from Figure 10, because the manuscript also evaluates active-layer thickness, which provides an independent constraint on the simulated ground thermal regime in permafrost-affected regions. We therefore do not add an observational benchmark to Figure 5. Direct observations of total soil water content are not available at the continental to global scale with the vertical resolution, soil-depth coverage, temporal consistency, and spatial coverage required for comparison with the ISBA soil column. Existing gridded soil moisture products either mostly constrain the near-surface layer or rely on land surface models and data assimilation systems. Such products are therefore not necessarily more independent or more reliable than the simulations analyzed here for evaluating vertically resolved total soil water content. The soil water response is instead interpreted through process consistency and through available integrated constraints, especially GRACE ΔTWS, discharge, and evapotranspiration. Similarly, Figure 10 (now Figure 11 after figure renumbering, see RC1) is intended to diagnose the vertical and spatial structure of the soil thermal response to the SOM parameterizations. The observational thermal evaluation is provided separately through near-surface soil temperature and active-layer thickness. Adding a direct observational comparison to Figure 10 would require a global vertically resolved soil temperature benchmark with consistent depth coverage and temporal sampling, which is not available for this purpose. The reported σ values are descriptive spatial standard deviations of the plotted fields or of the differences relative to CTL. They are not used as uncertainty estimates or skill metrics, and no conclusion in the manuscript is based on interpreting a lower σ as an improvement.

Minor comments

Remark 1: L125-140: the sentences describing SoilGrids 2.0 and the Princeton Global Forcing dataset read more like a methods section. It might work better to move part of this description to Section 2 to improve the flow and readability. The remaining text could be trimmed into shorter, more focused sentences.

Response: We respectfully disagree that these sentences reduce the readability of the Introduction. They do not provide a detailed methodological description of the experimental setup, but only identify the two datasets that define the global simulation framework: SoilGrids 2.0 for soil inputs and PGF for meteorological forcing. This information is useful at this stage because it provides the general context of the simulations presented later in the manuscript. The sentences are also short and do not include excessive technical detail. The detailed processing of the SoilGrids variables, their vertical remapping onto the ISBA grid, and the full forcing setup are described later in the Methods section.

Remark 2: In Figure 4, the uncertainty ranges (e.g., red versus pink shading) overlap heavily, making it hard to tell the different configurations apart. Adjusting the color scheme would improve interpretability.

Response: We agree that the overlap between uncertainty envelopes in Figure 4 could make the visual distinction between configurations less direct. We revised the figure by increasing the contrast between the DE25 and DE25c configurations, using a brighter red for DE25 and a distinct yellow tone for DE25c. The data and percentile ranges are unchanged.

Change made: We revised Figure 4 to improve the readability of overlapping uncertainty envelopes by increasing the color contrast between DE25 and DE25c

Remark 3: L678-680: the manuscript attributes the soil cooling in Figure 10 to increased total water content (Figure 5) and reduced thermal conductivity (Supplementary Figures S13-S14). However, the vertical patterns in Figure 5 and Figure 10 do not seem fully consistent. In Figure 5, DE16 shows increasing soil water content with depth relative to CTL, while DE25 and DE25c show differences that decrease with depth. In contrast, Figure 10 indicates that all configurations (DE16, DE25, DE25c) become increasingly colder with depth relative to CTL, a pattern more consistent with the reduced thermal conductivity shown in the supplementary figures S13-S14. It would therefore be helpful if the authors could clarify how soil moisture and thermal conductivity work together to produce the simulated vertical temperature structure.

Response: We agree that the vertical soil temperature response should not be interpreted as a direct mirror of the vertical changes in w_{gtot}. We revised this interpretation to clarify the respective roles of soil water and thermal conductivity. The increase in w_{gtot} modifies heat storage through the volumetric heat capacity and also affects the moisture-dependent component of λ_{soil}. However, the widespread decrease in soil thermal conductivity induced by mineral-organic mixing, shown in Supplementary Figures S13 and S14, provides the more direct mechanism limiting vertical heat transfer within the soil column. The colder subsurface response in Figure 10 (now Figure 11 after figure renumbering, see RC1) is therefore interpreted as the result of coupled changes in heat storage and heat transfer, not as a direct consequence of the vertical pattern of w_{gtot} alone.

Change made: We revised the interpretation of Figure 10 (now Figure 11) to clarify that the vertical temperature response results from coupled changes in heat storage and heat transfer. The text now distinguishes the role of increased w_{gtot} from the more direct role of reduced soil thermal conductivity in limiting vertical heat transfer.

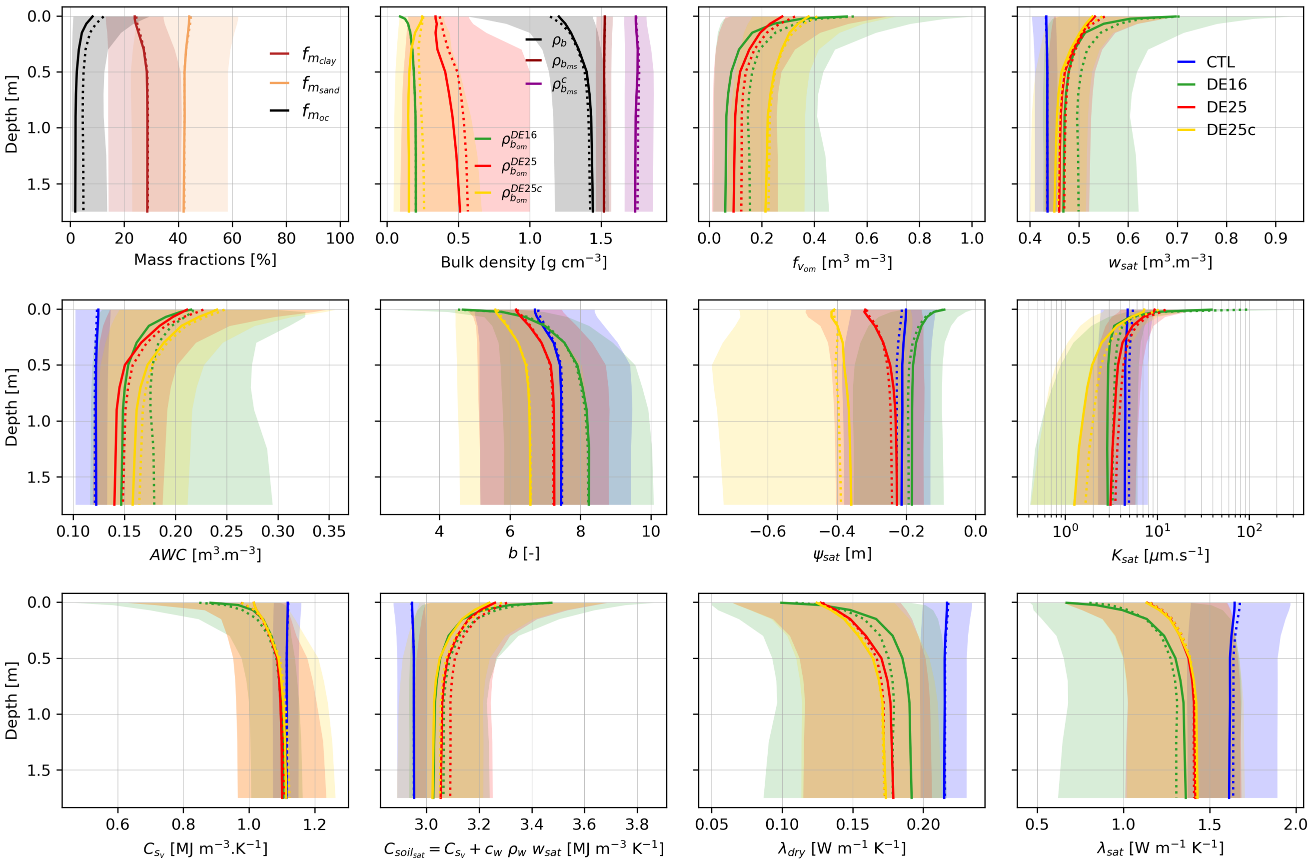

Figure 4. Global distributions of vertical profiles over the upper 2 m of the ISBA soil column, showing SoilGrids parameters ($f_{m_{clay}}$, $f_{m_{sand}}$, $f_{m_{oc}}$, and ρ_b), the derived phase-specific bulk densities ($\rho_{b_{ms}}$, $\rho^c_{b_{ms}}$, and $\rho_{b_{om}}$), and hydrological and thermal variables diagnosed in CTL, DE16, DE25, and DE25c. At each depth (positive downward), shaded envelopes indicate the 10th to 90th percentiles across land grid cells, solid lines show the median, and dashed lines show the mean. Variable definitions are provided in Figure 3 and Table B1.

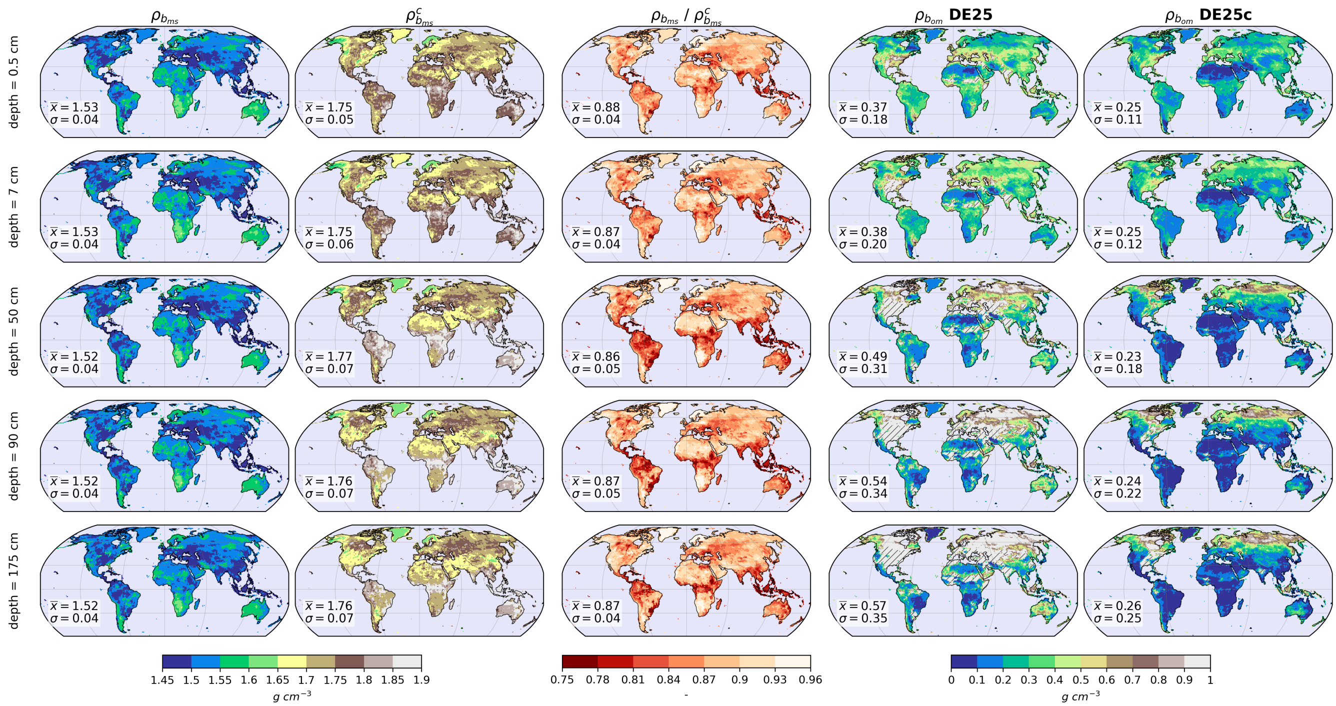

Figure S4. Global distributions of the mineral-phase bulk density $\rho_{b_{ms}}$ diagnosed in DE25, the compacted mineral-phase bulk density $\rho^c_{b_{ms}}$ diagnosed in DE25c, and their ratio $\rho_{b_{ms}}/\rho^c_{b_{ms}}$ (dimensionless), together with the diagnosed organic-phase bulk density $\rho_{b_{om}}$ for DE25 and DE25c, following the same layout and annotations as Supplementary Figure S3. Bulk densities are expressed in g cm^{-3}. Hatched areas in the $\rho_{b_{om}}$ panels indicate grid cells where the numerical regularization is active, i.e., where $f^*_{m_{om}} > f_{m_{om}}$ in the computation of $\rho_{b_{om}}$ (Equations 2 and 3).

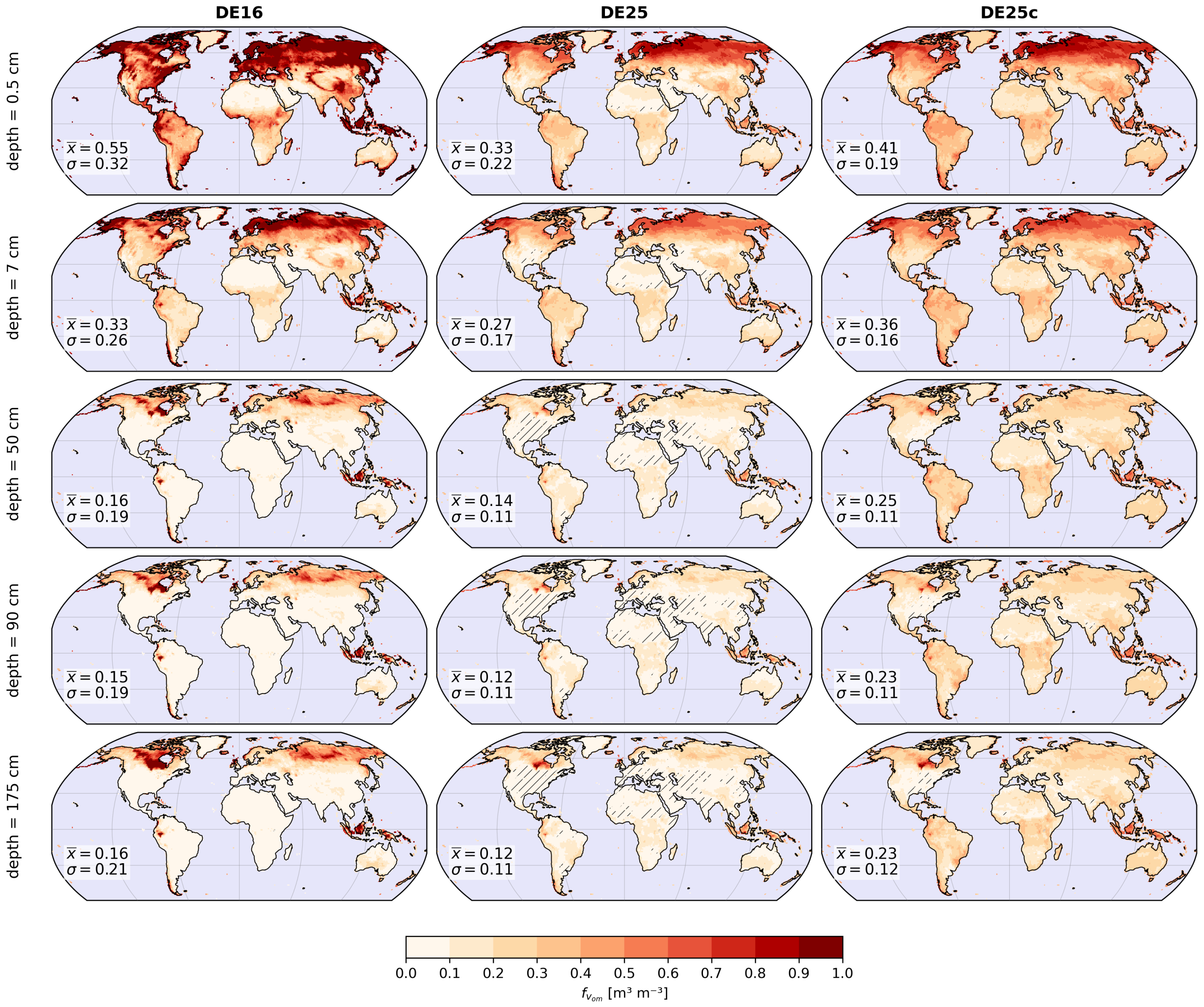

Figure S5. Global distributions of the volumetric organic matter fraction, $f_{v_{om}}$ (m^3,m^{-3}), diagnosed on the ISBA grid for the three parameterizations considered here (columns: DE16, DE25, and DE25c), using the same layout and annotations as Supplementary Figure S3. Hatched areas in the DE25 and DE25c panels indicate grid cells where the numerical regularization $f^*_{m_{om}}$ is active as in Figure S4.

Anonymous Referee #3

General remarks

This paper presents a method to improve the parameterization of the physical and hydraulic properties of organic soils in land surface models. The work seems thorough and paper is interesting and well written.

Response: We thank the reviewer for the positive assessment of the manuscript. We would like to clarify that the proposed framework is not restricted to organic soils. It is designed to represent the contribution of soil organic matter to physical, hydraulic, and thermal properties across the full range of mineral-organic soil mixtures, from mineral-dominated soils with low SOM content to organic-rich soils. The impacts are naturally larger in organic-rich regions, but the formulation is applied globally to all land grid cells where SOC is prescribed.

Remark 1: One criticism is that the authors perhaps somewhat overplay the novelty of the work by not citing some previous studies that are highly relevant. Firstly, although I know that the two early papers developing the theory (e.g. Stewart et al., 1970; Adams, 1973) were cited and discussed in an earlier paper by the same author, I think they should be recognized and cited in this paper too (for example, at lines 59 and 66). Second, there are some other later studies testing and applying the theory (e.g. Federer et al., 1993; Tranter et al., 2007) that the authors seem to have overlooked. For completeness, I think these could also be mentioned and their findings discussed somewhere in this paper.

Response: We thank the reviewer for this comment. We first note that this manuscript is Part 2 of a broader study. Part 1 introduced and evaluated the mineral-organic soil mixture framework, relying on mass-volume-density-porosity relationships from soil physics, on a SOC-to-SOM conversion, and on observational constraints for the properties of the organic matter domain. The present manuscript does not aim to repeat this full theoretical basis, but to apply it in ISBA-CTRIP to assess its impacts on soil physical, hydraulic, and thermal properties at the global scale. We agree that some historical references on mass-volume-density-porosity relationships can also be cited in Part 2 to make the manuscript more self-contained. We therefore added Stewart et al. (1970), Adams (1973), Raats (1987), Rühlmann et al. (2006), and Reynolds et al. (2020) in the Introduction as background references for the mass-volume relationships used in the proposed framework. We also added Ruehlmann (2020) where the SOC-to-SOM conversion is mentioned. We would like to clarify, however, that the novelty of the present work does not lie in the existence of these mass-volume relationships themselves. It lies in their operational implementation within a land surface model, their coupling with global fields of SOC, dry bulk density, and texture, and their use to diagnose mineral-organic soil physical, hydraulic, and thermal properties in global ISBA-CTRIP simulations. Regarding Federer et al. (1993) and Tranter et al. (2007), we acknowledge that they belong to the broader context of relationships between organic matter and bulk density. However, their main objective differs from that of the present manuscript. These studies mainly aim to describe or predict bulk density from organic matter and other soil properties. For example, Tranter et al. developed pedotransfer functions to predict bulk density from variables such as texture, depth, and organic carbon. In our framework, bulk density is not the predicted variable. It is used as an input constraint to diagnose the properties of the mineral-organic soil mixture. For this reason, we did not expand the manuscript with a detailed discussion of these two studies, which are less directly connected to the objective of the present work.

Change made: We added references to Stewart et al. (1970), Adams (1973), Raats (1987), Rühlmann et al. (2006), and Reynolds et al. (2020) in the Introduction as background references for the mass-volume relationships used in the companion paper. We also added Ruehlmann (2020) where the SOC-to-SOM conversion derived from observational data is mentioned. L59-61: "*To address these limitations, Decharme (2025) proposed a physically based framework relying on mass–volume relationships (Stewart et al., 1970; Adams, 1973; Raats, 1987; Rühlmann et al., 2006; Reynolds et al., 2020) and a SOC-to-SOM conversion* *derived from observational data (Ruehlmann, 2020), accounting for vertical variations in porosity and hydraulic behavior.*"

Remark 2: On the same theme, the authors suggest in their section Conclusions and Perspectives at lines 840 to 844 that it would be important in the future to investigate how interactions between soil physical properties and soil organic turnover can be modelled, i.e., not only to capture how organic matter affects soil physical properties like porosity and pore size distribution, but also how these properties in turn influence organic carbon turnover, as we know they do. In fact, quite a lot of work in this direction has already been accomplished. In particular, the two studies by Meurer et al. (2020) and Coucheney et al. (2025) could be acknowledged and cited here.

Response: We agree that Meurer et al. (2020) and Coucheney et al. (2025) are relevant references for this perspective, and we added them to the revised manuscript. We clarify, however, that our statement was not intended to imply that feedbacks between SOM turnover, soil structure, porosity, and physical protection have not been modeled before. These processes have already been represented in soil-crop and soil-profile modeling frameworks, including the studies mentioned by the reviewer. The open question raised here is more specific to the implementation of mixture-based soil property diagnostics in large-scale LSMs and ESMs with interactive soil carbon schemes. In such a framework, dynamically evolving carbon pools may affect diagnostic soil physical properties, such as porosity, hydraulic parameters, and thermal parameters, while these properties can in turn affect soil moisture, soil temperature, and carbon turnover. The challenge is therefore to define a coupling strategy that remains physically consistent and avoids circular dependencies between prognostic carbon state variables and derived soil parameters.

Change made: We added references to Meurer et al. (2020) and Coucheney et al. (2025) in the Conclusions and Perspectives section. The revised text is now : "*Another important perspective is to clarify how mixture-based diagnostics can interface with interactive soil carbon schemes. Recent soil-crop and soil-profile models have started to represent feedbacks between SOM turnover, aggregation, bulk density, porosity, and physical protection (Meurer et al., 2020; Coucheney et al., 2025). However, translating such developments into large-scale LSMs requires ensuring consistency between soil physical properties and dynamically evolving carbon pools, without introducing circular dependencies between prognostic state variables and derived parameters.*"

Remark 3: I don't think the authors should use the phrase mineral soil compaction adjustment, as it may give the wrong impression. It would be better to simply write bulk density correction (or adjustment) because man-made compaction may not be the only explanation for what the authors find. Most subsoils are naturally denser than topsoils and I can imagine that texture-based pedotransfer functions are derived from databases that are dominated by topsoil samples.

Response: We agree that the term "mineral soil compaction adjustment" could suggest a specific mechanical compaction process, which was not our intent. This point also overlaps with RC1 Remark 2 and RC2 Remark 2, for which we revised the terminology and clarified the rationale of the DE25c formulation. We therefore replaced this terminology by "mineral soil compactness adjustment" throughout the manuscript, including in the title. This revised terminology better reflects the purpose of the DE25c formulation. We did not adopt the terms "bulk density correction" or "bulk density adjustment", because they would imply that the gridded bulk density itself is corrected. This is not the case. In DE25c, the gridded dry bulk density is used as an input constraint to adjust the mineral reference state diagnosed from texture-based PTFs before recomputing the mixture variables. The adjustment is therefore applied to the mineral reference properties, not to the input bulk-density field. We also clarified in Sect. 2.2.3 that DE25c does not simulate dynamic mechanical compaction driven by water-ice phase transitions, temperature, clay swelling, or external loading. Instead, it defines a static compactness constraint on the mineral reference state before the land surface simulation. The revised text further specifies that compactness refers to the density state reflected by ρ_b, irrespective of whether it arises from natural packing, horizon development, pedogenic consolidation, land management, or mechanical compaction.

Change made: We replaced "mineral soil compaction adjustment" by "mineral soil compactness adjustment" throughout the manuscript, including in the title, as also indicated in our responses to RC1 Remark 2 and RC2 Remark 2. We also revised Sect. 2.2.3 to clarify that the DE25c adjustment does not modify the input bulk-density field and does not represent dynamic mechanical compaction. It uses the gridded dry bulk density as a static compactness constraint on the mineral reference state before recomputing the mineral-organic mixture properties.

Minor points

Remark 1: Lines 6-8. This should be re-phrased. This finding is actually specific to the pedotransfer function (PTF) presented by Cosby et al. (1984) and it may not be generally the case. I haven't checked, but my best guess is that the Cosby PTF was based on a dataset dominated by topsoil samples, which will tend to have lower bulk densities than subsoils.

Response: We agree that the previous wording could be read as a general statement about texture-based PTFs and about the compaction state of the samples used to derive them. This was not our intent. This point also overlaps with RC1 and RC2 remarks, for which we revised the terminology and clarified the rationale of the DE25c formulation. We therefore reformulated the sentence in the Abstract.

Change made: The sentence now reads: "*We also introduce an optional mineral soil compactness adjustment, under the assumption that texture-based pedotransfer functions define a mineral reference state that is not explicitly constrained by bulk density, whereas gridded bulk density products mostly reflect in situ compactness states.*"

Remark 2: Line 53: this doesn't seem correct (typo?): I guess it should be "… prescribed apparent specific density of organic matter …" Organic volume fraction = ((Mo/Vt))/((Mo/Vo)) = Vo/Vt, where M is mass, V is volume, and subscripts o and t stand for organic and total respectively. The denominator is the specific density of organic matter, not the apparent bulk density.

Response: We do not think that replacing "apparent bulk density" by "specific density" would be appropriate here. The sentence refers to previous empirical LSM parameterizations reviewed in Sect. 2.1.1 of Part 1, not to the mixture-theory diagnostics introduced later in that paper. In Eq. (1) of Part 1, the Lawrence and Slater type formulation estimates the SOM volumetric fraction as the ratio between soil carbon density and a prescribed maximum soil carbon density, $\rho_{sc,max}$. A related formulation is given in Part 1 Eq. (3) for the Chen et al. approach. In both cases, the denominator is used as a bulk-density-like threshold for an organic-rich material, not as the particle or specific density of the organic solid phase. This distinction is precisely part of the conceptual issue discussed in Part 1. If $\rho_{sc,max}$ is interpreted as a SOC density, the equation may be formally valid but describes a SOC volumetric fraction rather than a SOM volumetric fraction. Conversely, if it is interpreted as the bulk density of peat or organic matter, then the equation combines quantities of different nature because the numerator is based on SOC while the denominator refers to total organic matter. Replacing "apparent bulk density" by "specific density" would not solve this issue and would introduce another ambiguity, because specific density refers to the density of the solid organic material, excluding pore space. We therefore retained the wording "apparent bulk density of organic matter", which is consistent with the discussion of previous empirical formulations in Sect. 2.1.1 of Part 1.

Change made: We clarified the reference to the companion paper by replacing "As discussed in Decharme (2025)" with "As discussed in Sect. 2.1.1 of Decharme (2025)". The statement itself was retained because it summarizes the conceptual issue identified in Part 1 for previous empirical SOC-based parameterizations, especially those described by Eqs. (1) and (3).

Remark 3: Lines 56-57: I don't understand this. It should be clarified.

Response: The sentence refers to previous empirical LSM parameterizations reviewed in Sect. 2.1.1 of Part 1, not to the mixture-theory framework introduced in the present study. As discussed in Part 1, some previous formulations estimate a volumetric organic fraction from SOC density using a prescribed threshold such as $\rho_{sc,max}$. If this threshold is interpreted as a SOC density, the expression may be dimensionally valid but describes a volumetric fraction of SOC rather than total SOM. If, instead, the same threshold is interpreted as an apparent bulk density of peat or organic matter, then the numerator and denominator refer to quantities of different physical nature, because the numerator is based on SOC whereas the denominator refers to total organic matter or peat material. This is the inconsistency summarized in the sentence mentioned by the reviewer. We therefore retained this statement. To make the reference to the companion paper more explicit, we revised the text to point directly to Sect. 2.1.1 of Part 1.

Change made: as remark 2, we clarified the reference to the companion paper by replacing "As discussed in Decharme (2025)" with "As discussed in Sect. 2.1.1 of Decharme (2025)".

Remark 4: Lines 81-82: It would be helpful to cite one or two studies that support this.

Response: We agree that this statement should be supported by references. We have revised the sentence to cite studies that document the limitation of texture-only PTFs when soil structure, bulk density, and scale effects are not included among the predictors. Pachepsky and Rawls (2003) showed that texture-based estimates of soil hydraulic properties have limited accuracy because water retention and saturated hydraulic conductivity can vary substantially within the same textural class. They also showed that structural information at the horizon scale can help explain water retention, and emphasized that bulk density, aggregate distribution, penetration resistance, and soil structure are relevant predictors for improving PTFs. Van Looy et al. (2017) reviewed the role of PTFs in Earth system science and emphasized that PTFs translate available soil information into properties required by process-based models. Their review also highlighted that PTF performance depends on the selected predictors and that PTFs used in large-scale models should better account for the environmental and structural controls on soil properties. Vereecken et al. (2019) reviewed infiltration processes from the pedon to the global grid scale and discussed how land surface models represent infiltration and soil hydraulic properties. Their study emphasizes that transferring soil hydraulic relationships from local measurements to grid-scale LSM applications requires attention to spatial heterogeneity, scale dependence, and parameter uncertainty. Weber et al. (2024) provided a recent roadmap for hydro-pedotransfer functions. They noted that current PTFs raise concerns related to their scope and adequacy, especially when applied to field-scale and larger-scale models, and that many PTFs do not fully represent the processes shaping soil hydraulic properties, including land use, vegetation, parent material, and soil structure. Together, these studies support our interpretation that the residual bias identified in the companion paper is consistent with a limitation of texture-only PTFs such as the Cosby-SC PTF used here. Such PTFs cannot represent the effect of dense mineral horizons on porosity and hydraulic behavior when bulk density or compactness is not included among their predictors.

Change made: We revised the sentence as follows: "*This bias is consistent with evidence that texture-only PTFs have limited ability to represent the effects of soil structure, bulk density, and scale on porosity and hydraulic behavior, especially when dense mineral horizons fall outside the structural range represented by their predictor set (Pachepsky and Rawls, 2003; Van Looy et al., 2017; Vereecken et al., 2019; Weber et al., 2024).*"

Remark 5: Line 202: Should this be … "reduction of SOM content" not "decomposition of SOM"?

Response: We agree that the original wording could be ambiguous. The intended meaning was not a reduction in SOM content with depth, but the increasing degree of decomposition of the organic material with depth, from fibric to more decomposed sapric peat. We therefore clarified the sentence while keeping the description of the DE16 parameterization unchanged.

Change made: We revised "*the gradual decomposition of SOM with depth*" to "*the increasing degree of decomposition of SOM with depth.*"