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 We agree that some historical references on mass-volume-density-porosity relationships can also be cited in Part 2 to make the manuscript more self-contained.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.*"