

Anonymous Referee #1

General remarks

Accounting for the impact of soil organic matter on hydraulic properties is a highly relevant and challenging topic. This is definitely the case in the frame of land surface modeling, which aim to function at global scale and are limited by data availability. The author presents a framework to do so, in a clear and transparent manner.

We thank the reviewer for this positive assessment. We appreciate that the relevance of accounting for soil organic matter effects on hydraulic properties in land surface models has been recognized, as well as the clarity and transparency of the proposed framework.

Remark 1 : In soil mixture theory, the well-documented interaction between soil organic and mineral components is ignored. In soil science, OM is typically part of soil texture, and gives shape to soil structure through aggregation and ultimately determines soil hydraulic/thermal properties (e.g. Rawls et al. 2003, De Lannoy et al, 2014, Van Looy 2017). This interaction is not mentioned or discussed in the manuscript, yet it seems crucial. It makes the link to other strategies, which rely rather on PTFs than soil mixture to include the impact of OM. This is touched upon in the discussion, by comparing to the porosity estimates of Weynants and Wösten, but could be extended to other variables.

Response : We thank the reviewer for this comment. We understand this concern, but to our knowledge no study has explicitly suggested that mixture theory ignores the interaction between organic and mineral components. This perception may arise from how the theory is sometimes interpreted, but the literature consistently presents it as a framework that integrates the volumetric contributions of both domains. In fact, this interaction is central to the classical formulations developed or reported by Stewart et al. (1970), Adams (1973), Raats (1987), Rühlmann et al. (2006), and more recently Reynolds et al. (2020). These studies show that soil mass-volume-density-porosity relationships can be analytically expressed as the combination of the effective volumes of organic matter and minerals. Organic matter directly affects bulk density, particle density, and porosity, reflecting its structural interaction with the mineral fraction. Conceptually, pedotransfer functions (PTFs) and mixture theory are two comparable mathematical approaches linking for instance texture, bulk density, and organic matter variables to soil properties. PTFs express these relations statistically (or in other words empirically), whereas mixture theory formalizes them through conservation-based analytical relationships. However, our framework still requires PTFs or specific phase values to characterize the hydraulic and thermal properties of each domain (mineral and organic), so the two approaches should be seen as complementary rather than exclusive.

In addition, for thermal properties, the volumetric fractions of each soil constituent (minerals, organic matter, air, water, ice) must be known. Conduction models (Johansen, De Vries, Balland & Arp 2005) explicitly rely on these volumetric fractions. Mixture theory, by converting gravimetric carbon contents (kg kg^{-1}) into volumetric organic matter contents ($\text{m}^3 \text{ m}^{-3}$), is therefore not only suitable but essential for correctly representing thermal properties. We have clarified this point in the manuscript, emphasizing that mixture theory provides a coherent physical basis, complementary to PTF approaches, and is indispensable for estimating thermal properties.

Finally, we agree that the comparison with Weynants and Wösten could be extended to other variables. We performed these analyses and present them now as Supplementary Figures (S4-S6). They confirm that the Weynants PTF performs poorly for both water retention and saturated conductivity, whereas the Wösten PTF is more promising but shows two major limitations: a strong dispersion at high SOM contents (as also seen for porosity in Fig. 9) and a systematic overestimation of θ_{sat} . By contrast, the mixture-theory approach provides robust and consistent results across all variables. This confirms that our framework is adequate to represent soil hydrodynamic parameters, while the Wösten model remains promising but would clearly benefit from recalibration.

Note that we have added two paragraphs about this remark at the end of the discussion :

L1016 : "...suggesting that this PTF is likely not suited for organic-rich or peat soils. We also extended the comparison to soil water retention (Figures S4 and S5 in the Supplement) and saturated hydraulic conductivity (Figures S6 in the Supplement), for which the mixture-theory approach provided equal or better agreement with observations than existing PTFs. The Weynants PTF performed poorly for both variables, while the Wösten PTF gave more promising results but with two clear limitations: a large dispersion at high SOM contents, as also noted for porosity (Figure \ref{fig_9}), and a systematic overestimation of saturated conductivity. These results highlight that, while..."

L1018 : "One may argue that the well-documented interactions between organic and mineral components are not captured by mixture theory and would only be represented by PTFs. Numerous studies (e.g., Stewart et al., 1970; Adams, 1973; Raats, 1987; Rühlmann et al., 2006; Reynolds et al., 2020) have demonstrated that soil bulk density, particle density, and porosity can be expressed as the sum of the effective volumes of mineral and organic domains, highlighting that these interactions are indeed included in mixture theory. This theory provides mathematical formulations conceptually comparable to PTFs, which rely on empirical regressions between texture, organic matter, and hydraulic properties, while mixture theory captures these relationships through conservation-based analytical expressions. The two approaches should therefore be considered complementary rather than exclusive. Moreover, for porosity our mixture-theory approach provides a closer match to observations than existing PTFs (Fig. 9), and similar behavior is found for retention and saturated conductivity (Figures S4 to S6), indicating that mixture theory can serve as a useful complement or alternative to existing PTFs. For thermal properties, the situation differs. Conduction models (Johansen, 1975; De Vries, 1974; Balland and Arp, 2005) require the volumetric fractions of each soil constituent, which makes mixture theory not only suitable but indispensable in this domain."

Remark 2 : Furthermore, due to the lack of interaction described in the mixture model, this framework might be not appropriate for all soils. However, the aim is applicable for all soils: covering agricultural topsoils, O-horizons in forests, peat-soils, (tropical soils) etc. Addressing the remark above should shed some light on the limitations of the soil mixture theory, depending on soil type/OM content too.

Response : We acknowledge the reviewer's concern. This remark relates to the previous one, and we reiterate that mixture theory does account for the volumetric consequences of interactions between organic and mineral domains, as expressed through bulk density, particle density, and porosity relationships. What mixture theory does not represent explicitly are the mechanistic processes that generate such interactions, for example aggregation or organo-mineral associations. In this respect, mixture theory is conceptually similar to PTFs, which also

do not describe these processes explicitly but capture their integrated effects through empirical relationships. We argue that the proposed framework remains applicable to all soils for three main reasons. First, it relies on conservation relationships between mass, volume, density, and porosity, which are universally valid. Second, it does not depend on datasets calibrated in a specific domain, unlike PTFs, although it is limited by the accuracy of the chosen phase-specific PTFs. Finally, it is adaptable: as long as bulk density, texture, and gravimetric organic matter (or carbon) content are available, the framework can be applied consistently to mineral agricultural soils, organic forest horizons, peat soils, or tropical soils, as also acknowledged by the reviewer.

Remark 3 : An important aspect of the framework is the determination of the bulk density of OM (ρ_{b_om}). It is derived from the bulk density, mineral bulk density and mass fraction of OM. It can be measured from pure peat samples, but becomes a difficult (perhaps meaningless) soil property for soils at low OM content, where the organic and mineral fractions are part of 1 matrix. On the other hand, the particle density can be determined and remains meaningful throughout. It would be good to evaluate this value as well, as a quality check to see whether the OM particle density falls within the expected range (e.g. Rühlmann et al, 2006).

Response : We thank the reviewer for this suggestion. We note first that the organic matter bulk density (ρ_{b_om}), Fig. 5d) inferred in our framework falls within the ranges observed for peat and for mineral soils with a non-negligible SOM content, as already discussed in lines 822-825. In addition, since $\rho_{s_om} = \rho_{b_om} / (1 - w_{sat_om})$ and w_{sat_om} is well predicted in our framework (Fig. 4), it is not surprising that the resulting ρ_{s_om} distribution is also consistent with the literature. The inferred distribution shown in a new Figure S3 that will be added to supplementary (mean 1.38 g cm⁻³, median 1.49 g cm⁻³) is centered within the typical range of 1.1 - 1.5 g cm⁻³ reported by Rühlmann et al. (2006; 2020), which provides an additional, though expected, quality check of our parameterization. The lower tail of the distribution largely corresponds to soils with high bulk density and very low SOM content, where the estimation of ρ_{s_om} is numerically amplified. Such values should therefore be interpreted as “apparent” properties resulting from the bookkeeping of mixture theory, rather than as directly measurable densities. This comment was added line 829 : “A complementary quality check can be provided by the inferred particle density of organic matter (ρ_{s_om}), obtained from ρ_{b_om} and w_{sat_om} (Equation 11). The distribution shown in Figure S3 (mean 1380 kg m⁻³, and median 1490 kg m⁻³) is centered within the typical range of 1100-1500 kg m⁻³ reported by Rühlmann et al. (2006) and Rühlmann (2020) or 900-1550 kg m⁻³ reported by Redding and Devito (2006), further supporting the physical consistency of the framework.”

Remark 4 : In the same vain, if we imagine that the OM content of soils can change over time in LSM (by coupling it to the carbon cycle). What are the implications in this framework? The mineral bulk density will remain constant, the soil bulk density too (?). The OM bulk density will change to keep everything physically sound. However, it can be questioned whether this is really physically sound. Perhaps it should be noted then that this framework is not suitable to handle such a configuration (?).

This remark is highly relevant, but it goes beyond the scope of the present study, which primarily aims to establish a conceptual framework. In a LSM coupled to the carbon cycle, if organic matter content evolves, then the bulk density of organic matter (ρ_{b_om}) would also change, and bulk soil density (ρ_b) would need to adjust accordingly, with only the

mineral bulk density remaining constant. Since such evolution is very slow (several decades), its representation should be facilitated and would not raise numerical difficulties. In other words, ρ_b would become a prognostic variable of the model, analogous to the progressive densification of snow in LSMs, which depends on factors such as gravitational compaction, snow viscosity, and liquid water content. Conversely, our approach, which makes it possible to determine $\rho_{b_{om}}$ from observations or estimates, could also guide carbon cycle schemes in LSMs by providing an additional constraint on carbon accumulation in the different soil horizons.

Remark 5 : The manuscript is well-written, figures are clear. Perhaps some repetitions can be avoided to make the text a bit more concise. This is a matter of taste. Overall, this is a solid work. Even though tests at global scale might indicate a low sensitivity to this improvement (as the author indicates), this is a step forward in land surface modelling.

We thank the reviewer for this positive evaluation of our manuscript and figures. We have followed the suggestion to reduce some repetitions in order to make the text more concise where possible.

Specific remarks

L18: Chemical weathering could be listed too

We thank the reviewer for this suggestion. We have added chemical weathering to the list of processes mentioned in the introduction (line 18).

L50: Impact of SOM on thermal properties is probably mostly indirect, as it affects the soil structure and porosity (?)

We agree that the impact of SOM on thermal properties is also indirect, through its influence on soil structure and porosity. However, as indicated in the literature (Farouki, 1981), SOM also has distinct intrinsic thermal properties, with lower thermal conductivity and higher specific heat capacity than mineral matter. Both direct and indirect effects are therefore relevant, and we will clarify this point in the text. This sentence has been added at the end of line 53 : “In addition to this direct effect, SOM also mainly influences soil thermal behavior indirectly through its impact on soil structure and porosity.”

L65: Worth noting that PTFs that include OM were existing in soil science

We agree that PTFs including organic matter have long existed in soil science, especially for hydrodynamic properties. We have clarified this point by adding the following sentences after L65: “While PTFs including organic matter have long existed in soil science for hydrodynamic properties (Rawls et al., 2004; Weynants et al., 2009; Wösten et al., 1999; Tóth et al., 2015; Van Looy et al., 2017), their use in LSMs has generally remained limited. For thermal properties, such PTFs are nearly absent, with models usually relying on fixed values or simple mixing rules.”

L67: This is a bit confusing. Most pedotransfer functions are designed to work with gravimetric OM content.

It is not entirely clear to us why this remark arises at this point, since our discussion focused on soil physical properties (hydraulic or thermal) that are mostly governed by volumetric rather than gravimetric considerations. We fully agree that most PTFs use gravimetric SOC or SOM content, i.e. expressed as a mass fraction (kg of SOC per kg of dry soil). Our intention was to highlight that many soil physical properties are inherently volumetric, as illustrated by porosity or volumetric heat capacity. Therefore, land surface models require volumetric fractions (m^3 of SOM per m^3 of bulk soil) to compute such properties. Since soil databases usually provide only gravimetric content together with bulk density, volumetric fractions must be derived from these quantities, which we believe is already stated clearly around L66.

L137: unclear what is meant with ‘when $w_{\text{sat_om}}$ reaches 0.9, which corresponds to a few centimeters below the soil surface’ Eq 3: (confusing equation, but ok): ρ_{ms} should be $\rho_{\text{b_ms}}$ (in the numerator).

We thank the reviewer for this remark. The intention was to indicate that in this parameterization $w_{\text{sat_om}}$ is depth dependent, following an idealized profile from 0 to 1 m depth. We have clarified the text accordingly, now stating explicitly that $w_{\text{sat_om}} = w_{\text{sat_om}}(z)$ and that the value 0.9 corresponds to a few centimeters below the surface within this idealized profile. We have also corrected the notation in Eq. (3) by replacing ρ_{ms} with $\rho_{\text{b_ms}}$ in the numerator.

L229 ‘earth fraction’ => ‘solid fraction’

We agree with this remark. We have replaced ‘earth fraction’ by ‘solid fraction’ at L229.

L277 not eq18 should be referred to, but the equation at L267

We agree. The sentence at L277 has been rephrased to clarify the link between the two equations. It now reads: “As expressed above, to pass from Equation (17) to Equation (18), the soil organic volumetric fraction is”

L300 The porosity of the mineral domain is estimated using Cosby et al. 1984 or Clapp & Hornberger, 1978. This is somewhat questionable. Although These PTFs don’t take OM in account, they are trained on soil samples which contain OM. For example, Cosby et al. used data from Holtan et al. (1968) and Rawls et al. (1976), containing samples from (mostly) agricultural soils across the US. The OM content is not described, but roots content is described as ‘abundant’ or ‘common’ in roughly half of the samples. Meaning that the porosity obtained with this PTF is representing a soil with an average OM content and not a pure mineral soil. Perhaps a better approach (still questionable) is using a PTF that accounts for OM, and setting OM to 0.

We acknowledge that PTFs such as Cosby et al. (1984) and Clapp and Hornberger (1978) were calibrated on soil samples that most likely contained some organic matter. However, the

resulting parameter values lie within the range of mineral soils and are far from those of organic soils, which supports their use as a reference for largely mineral domains in land surface models. The same reasoning could apply to PTFs developed for organic soils, such as Liu and Lennartz (2019), which were also derived from soils that are not purely organic. In contrast, using a PTF that includes OM and simply setting OM = 0 would likely step outside the statistical validity of the underlying regression, because the implicit interaction between mineral and organic fractions embedded in the calibration would be lost. This point is consistent with the reviewer's own Remark 1, which emphasizes the role of organo-mineral interactions in shaping soil structure and hydraulic/thermal properties. For these reasons, we retain classical Cosby et al. (1984) PTFs as references for the mineral domain, while recognizing that their calibration may include soils with some organic content. This assumption is indeed clearly validated in Fig. 4, which shows that Cosby's PTF yields porosity values within a narrow range centered on typical mineral soils, and cannot represent the much wider variability associated with organic soils.

L354: Note that aside from arithmetic mixing of parameters, a dual porosity model is perhaps more suitable/compatible with this framework.

We acknowledge the suggestion of using a dual-porosity framework. However, this goes beyond the scope of the present study, which aims at developing a simple and generic approach suitable for land surface models operating at global scale. Dual-porosity models are mainly used for specific problems such as solute transport or macropore flow, and they require additional parameters that are not available in global soil databases. Moreover, separating organic and mineral domains into distinct pore networks is not necessarily more consistent with the role of organo-mineral interactions. As emphasized in Remark 1, these interactions shape soil structure and properties, and our mixture-theory framework already accounts for them in a simple conservation-based way, applicable at global scale..

L355 and L404: same remark as above: Cosby et al is not representative for a pure mineral soil.

At these lines we explicitly wrote that 'hydraulic parameters for mineral soils can be readily estimated using' Cosby's PTF. Our goal is not to represent a chemically pure mineral soil, but to provide parameter values corresponding to the mineral end-member of the soil continuum, clearly distinct from organic soils. As already explained in our response to L300, even though Cosby et al. (1984) was calibrated on samples that may have contained some organic matter, the resulting parameters fall within the range of mineral soils and far from organic soils. We recall that this PTF has been historically used in land surface modeling as a standard reference for mineral soils.

Section 3.1: no information was provided related to how the samples were compacted in the lab.

We acknowledge the reviewer's remark. The three datasets used in Section 3.1 come from published studies, each with their own laboratory procedures. In Walczak et al. (2002), samples were hand-mixed without a standardized compaction; in Arkhangelskaya and Telyatnikova (2023), mixtures were filled into cylinders with manual compaction; and in Willaredt and Nehls (2020), a standardized protocol was applied using a falling weight device according to the German FLL guideline. Rather than reproducing these methodological details in our manuscript, we prefer to refer the reader to the original publications, which are all peer-reviewed A-rank papers where the experimental protocols can thus be considered valid and appropriate.

L584: There is no clear justification for the rescaling of the bulk density. Why was this done? What are the implications for the soil mixture theory?

We thank the reviewer for pointing out this issue. In our framework, soil bulk density (ρ_b) corresponds to the ‘normal bulk density’ as defined by Keller & Håkansson (2010), i.e. $\rho_{b,n}=0.83 \rho_{ref}$ (their Eq. 17). In their study, ρ_{ref} is a reference bulk density obtained from uniaxial compression at 200 kPa, which represents a critical compactness threshold rather than the in-situ bulk density of a soil sample. We therefore applied the 0.83 rescaling factor exactly as proposed by Keller & Håkansson, in order to make our comparison consistent. This has no implication for the soil mixture theory itself, which remains strictly based on conservation of mass and volume, but simply aligns our data with their definition of normal bulk density. This was clarified in the new manuscript.

Fig.3 Dashed and dotted lines not clearly explained in the caption

We thank the reviewer for pointing out this issue. In the original caption of Fig. 3c, the association between line styles and averaging schemes contained an error. The correct correspondence is: arithmetic mean = dash-dotted line, geometric mean = solid line, harmonic mean = dashed line, and geo-harmonic mean = dotted line. We also modified the wording by adding “line” after each style (e.g., “solid line” instead of “solid”) for clarity. In addition, we included the following passage to better describe the main right panel: “In the main right panel, the tested parameterizations are shown with their corresponding averaging schemes, using the same line styles as in the inset: Lawrence and Slater (2008) (blue) and Chen et al. (2012) (green) rely on arithmetic means (dash-dotted line), while the presented framework (red) relies on either a geometric mean (solid line) or a geo-harmonic mean (dotted line).”

Units g/cm³ and kg/m³ are used interchangeably. Stick to kg/m³ for clarity.

We fully agree that unit consistency is important. In the manuscript text, all bulk density values are consistently expressed in kg/m³. The figures use g/cm³ only to shorten axis labels and improve readability. Since the conversion is straightforward and widely understood, we considered this choice acceptable for graphical purposes. However, if the editor prefers full consistency across text and figures, we can readily adapt all figure labels to kg/m³, for instance using the notation 10³ kg/m³ instead of g/cm³ to keep the labels compact.

L653: Perhaps relevant to indicate that using the SOM/SOC factor of 2 and $\rho_{b,oc}$ of 130 kg/m³, the method of Lawrence & Slater is equivalent to using a $\rho_{b,om}$ of 260 kg/m³. This makes it easier to see the impact of the ‘opposing biases’, and to compare it to the newly proposed method (which gave $\rho_{b,om} = 205$ kg/m³, 248 kg/m³ and 240 kg/m³ for the 3 lab datasets).

We thank the reviewer for this constructive suggestion. We agree that explicitly stating the equivalence helps to better illustrate the impact of the opposing biases and to compare with the values obtained from the present framework. We have therefore added the following sentence in Section 3.3 (L653): “In practice, combining a SOC-to-SOM factor of 2 with a SOC fixed bulk density of 130 kg m⁻³ is equivalent to assuming $\rho_{b,om} = 260$ kg m⁻³, which can be

directly compared to the values of 205, 248, and 240 kg m⁻³ obtained for the three laboratory datasets using the present framework.”

L778: It is worth mentioning that the porosity estimates with the soil mixture method are constrained by the bulk density, contrary to the other methods: ρ_{b_om} is changing to match the bulk density. The question which arises then is how realistic is ρ_{b_om} and, more physically: how realistic is the resulting particle density of the organic matter.

We thank the reviewer for this remark. We agree that in the mixture-theory formulation, porosity estimates are constrained by bulk density (ρ_b), which implies that ρ_{b_om} and ρ_{s_om} adjust accordingly. To address this point, we revised the text (line 778) to specify that the observed agreement also reflects the consistency of the inferred ρ_{b_om} and ρ_{s_om} , which fall within reported ranges, as shown in Fig. 4 and the new Fig. S3.

Finally, we corrected a notation error in the sentence at lines 777: it should read w_{sat_om} (porosity of the organic matter domain) instead of w_{sat_ms} .

Anonymous Referee #2

General remarks

The manuscript reviews and synthesizes a wealth of existing related research, proposing a physically consistent framework to represent soil organic matter's influence on key physical properties for land surface models. This framework, grounded in rigorous soil mixture theory, explicitly considers the soil's volumetric composition as a mixture of organic and mineral components. The resulting parameterization addresses a previously overlooked inconsistency in many land surface models and is supported by experimental data. The paper is clear and well-written. However, I noticed several minor issues in the manuscript. As the equations are likely to be directly used by many land surface models, I suggest the author examine them further.

Thank you for your careful reading and positive assessment of our manuscript. We also appreciate your minor comments, which are pertinent and helpful. We will address them to clarify the text, harmonize the notation, and perform a final check of the equations.

Minor comments

Line 141: In Equation (3), ρ_{ms} has not been defined.

We thank the reviewer for this remark. It is a typographical error and we have corrected the notation in Eq. (3) by replacing ρ_{ms} with $\rho_{b_{ms}}$ in the numerator.

Line 251: Deriving Equation (15) involves substituting the definitions of $f_{m_{om}}$ and $f_{m_{ms}}$ from Equation (13a) into Equation (14).

Thank you for the helpful suggestion. The identity $f_{m_{ms}} = 1 - f_{m_{om}}$ is stated just above line 251. To improve clarity, we revised the sentence to explicitly mention both mass fractions and their relation. This does not change any result. We now propose this sentence : “Inserting the definitions of the mass fractions from Equation (13a) into Equation (14), and using $f_{m_{ms}} = (1 - f_{m_{om}})$, leads to expressions for both soil bulk and particle densities as functions of $f_{m_{om}}$ and the densities of the individual soil components.”

Line 260: Please correct this sentence. The ratio of bulk density and particle density is equal to 1 minus the soil porosity.

Thank you for pointing this out. In the text, we replaced “equal to the soil porosity” with “equal to 1 minus the soil porosity”

Supplement, Lines 8-9: Please correct the referenced equations, Equation (14) -> Equation (16)?

Thank you for pointing this out. In the Supplement, several equation cross-references were offset by -2. We corrected them as follows: Eq. (14) → Eq. (16), Eq. (15) → Eq. (17), Eq. (10) → Eq. (12), and Eq. (11) → Eq. (13). These corrections do not affect any derivations, results, or conclusions.