Anonymous Referee #1

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 $\rho_{bms}{}^c$ does not necessarily lead to a proportional decrease in bulk soil porosity. Through the mixture inversion, a larger $\rho_{bms}{}^c$ 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* $\rho_{bms}{}^c$ *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 $\rho_{bms}/\rho_{bms}{}^c$ 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.

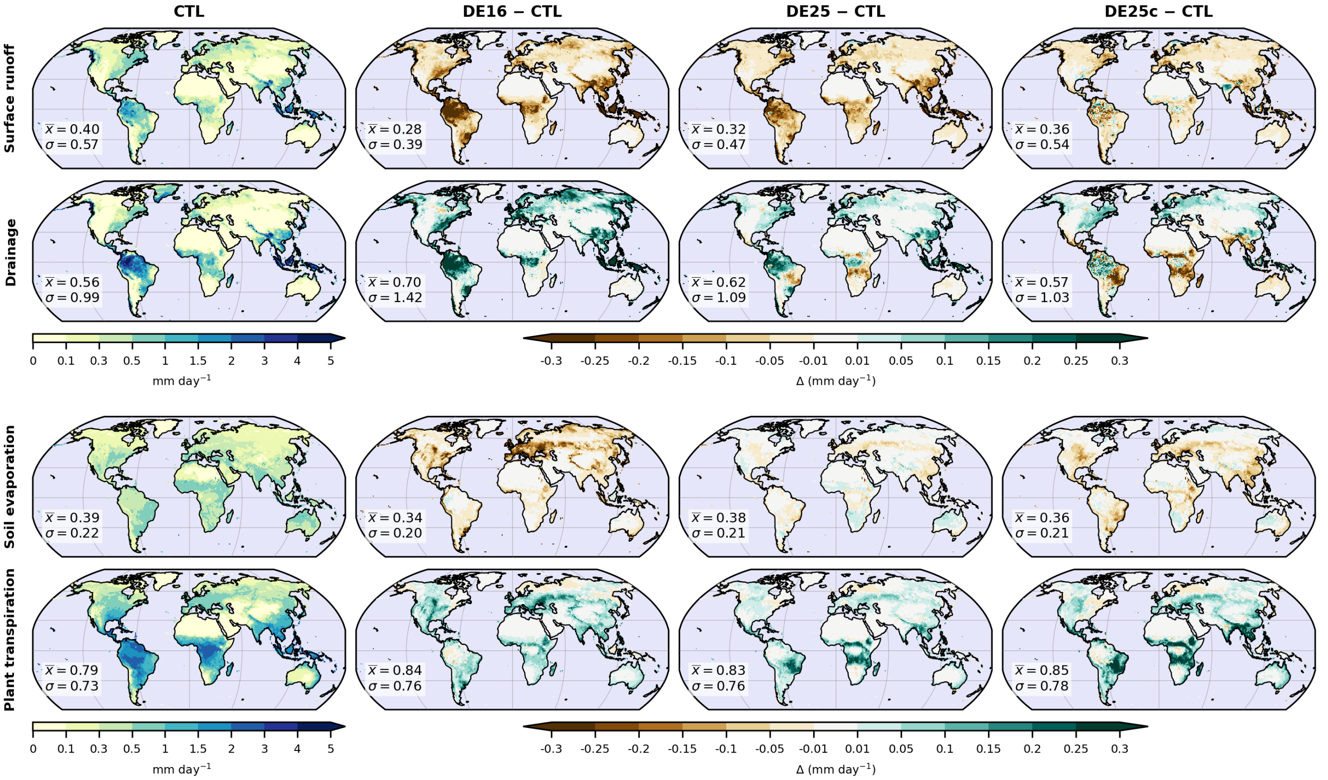

Figure S15. As in Figure 9, but for the 1979–2010 mean surface runoff, drainage, bare soil evaporation, and plant transpiration. Surface runoff and drainage are the two components of total runoff considered here, while bare soil evaporation and plant transpiration are shown to document the main land-surface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

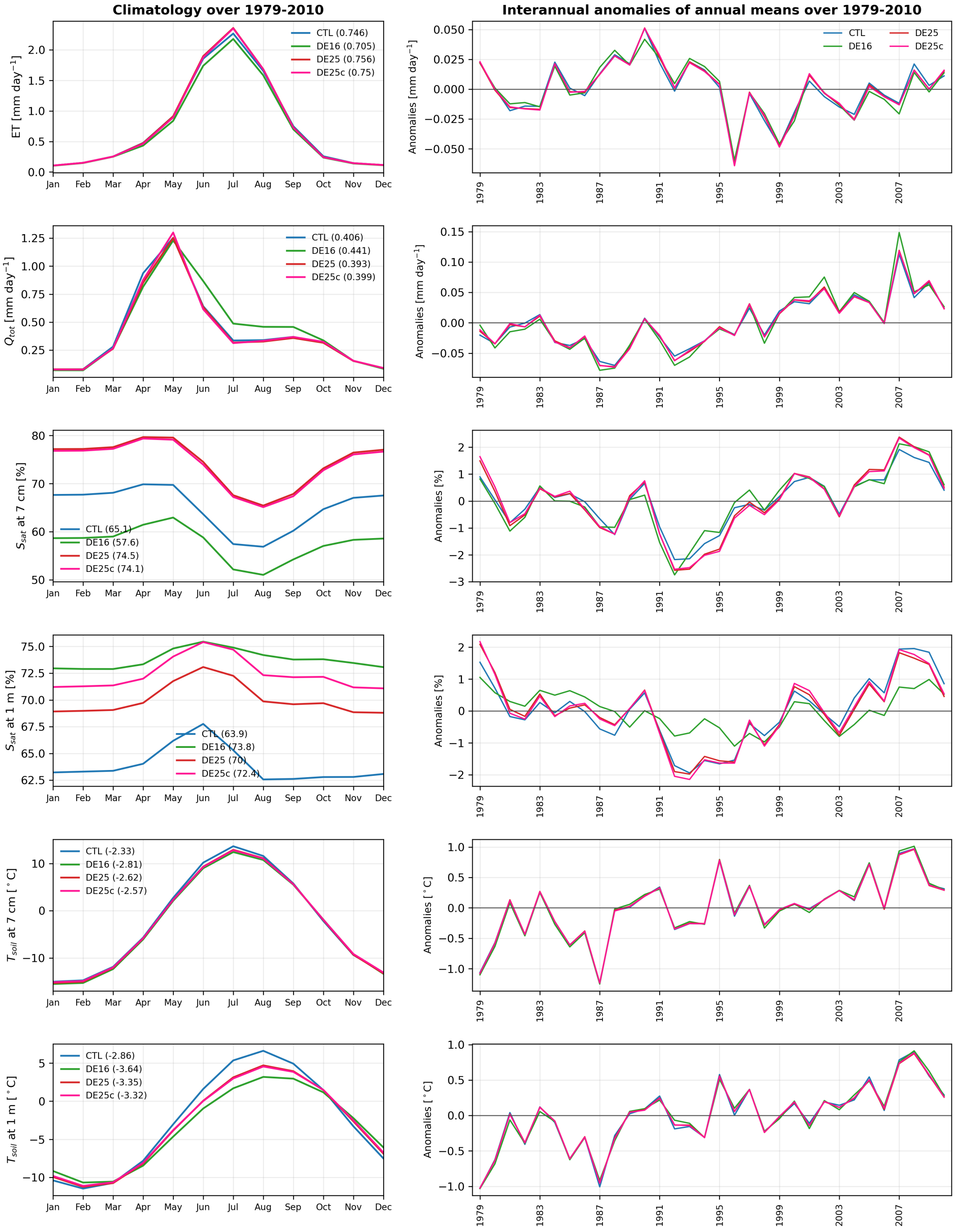

Figure S16. Regional temporal diagnostics over a SOC-rich Siberian domain, 40^oE to 180^oE and 45^oN to 85^oN, for CTL, DE16, DE25, and DE25c over 1979 to 2010. The left column shows monthly climatological cycles of evapotranspiration, ET, total runoff, Q_{tot}, soil saturation degree, S_{sat}, at 7 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.