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Manuscript title: Retrieving root-zone soil moisture from land surface modelling and GRACE/-FO and validating its dynamics with in-situ data over West Africa

Response to Reviewers' Comments

We are grateful to the anonymous reviewers for the detailed, insightful, and constructive comments and questions that have been used to improve the quality of our manuscript. We thank the editor for inviting us to respond to reviewers' comments and revise the manuscript.

 # Response to the Editor-in-Chief

We thank the Editor-in-Chief for the careful assessment of our revised responses and for the constructive feedback regarding the presentation quality of the manuscript. We fully acknowledge that, beyond the scientific content, the manuscript requires substantial improvement in structure, formatting, clarity, and overall presentation quality to meet the standards of HESS. We have carefully addressed all these remarks in the revised version of the manuscript and have made substantial efforts to improve its organization, readability, and overall presentation quality.

1. Poor Table Formatting: Tables 1, 2, and 3 are currently presented in an unprofessional format (resembling raw CSV dumps or unformatted text). They must be formatted as proper academic tables with clear headers and standard layout.

Response : We fully agree. Tables 1, 2, and 3 are completely reformatted into proper academic tables with clear headers, consistent notation, standardized layout in accordance with the HESS guidelines.

2. Writing Style and Structure (Avoid "List" Formats): Several sections of the manuscript, particularly Section 4.3, rely heavily on bullet points and list-like structures (e.g., separating results by "Projection depth = 5 cm", then "10 cm" as isolated items). This style resembles an outline (or AI-generated text) rather than a cohesive scientific narrative. Please integrate these findings into continuous, flowing text. Similarly, the definitions for variables in Equation 1 are listed in a vertical strip, which is non-standard; these should be integrated into the text.

Response : We agree with this assessment. We have fully restructured Section 4.3 into a continuous scientific narrative. The previous list-based and depth-labelled presentation has been removed and replaced with flowing text, in line with HESS presentation standards. Similarly, the definitions of variables in Equation (1) have been incorporated directly into the running text, in accordance with standard mathematical writing conventions, instead of being presented as a vertical list. This also considered in section 3.3.1.

3. Mathematical and Terminology Clarifications:

- Eq. 2: The variable "N" is undefined. Please define it clearly.

Response : Thanks for pointing this out. In the revised manuscript, we explicitly define N as the total number of discrete time steps considered in the analysis, each of duration ΔT.

- Eq. 4: It is confusing why theta_wt, theta_fc are associated with the "2nd" or "98th" percentiles.

Response : This issue was already raised by Reviewer 1 and has been fully addressed as suggested in the revised manuscript (Section 3.3.2, Lines 415–430). theta_wt, theta_fc are now clearly defined as soil moisture at the wilting point and field capacity, respectively, and are represented by the 2nd and 98th percentiles of the soil moisture distribution to capture dry and wet extremes while reducing the influence of outliers. The manuscript now adopts the standard term Soil Wetness Index (SWI) instead of wt, with appropriate references added. This formulation expresses soil moisture relative to physical dry–wet limits and is specifically designed to highlight drought signals and relative moisture anomalies, enabling consistent comparison across models, satellite products, and in situ observations.

- Line 41: The term "boxcar" is used without context. Please define this filter method.

Response : We agree that the term “boxcar” was previously used without sufficient explanation. This has now been clarified in the revised manuscript by explicitly defining the filter method (Section 3.3.3, Lines 439 to 443).

- Spell Events: The terms "Dry and wet spell event" are used; please explicitly define what constitutes a "spell" in your methodology.

Response : We agree and thank for pointing out the need for a clearer definition. In the revised manuscript, we now explicitly define dry and wet spell events (Section 4.1, Lines 463 to 467). Dry and wet spells are defined as persistent periods of anomalous soil moisture conditions, corresponding to sequences of consecutive time steps with soil moisture values persistently below or above a defined threshold, respectively. We further clarify that this definition is conceptually consistent with the classical climatological definition of spells as continuous sequences of days with rainfall below or above a given threshold.

4. Narrative Flow and Logic:

- Line 443: The mention of the "Tibetan Plateau" appears abruptly and lacks context. As noted by Reviewer 1, this comparison feels disjointed. Please provide a better transition or move this to a broader discussion context.

Response : The manuscript has been revised to improve the narrative flow as suggested, by first focusing on the influence of the West African monsoon system on soil moisture dynamics in the region, and then placing the Tibetan Plateau example within a broader comparative context of monsoon-driven soil moisture dynamics (Section 4.1.1, Line 477 to 486). This comparison is now framed as a general illustration of the role of large-scale monsoon systems in shaping seasonal soil moisture variability across different climatic regions.

- Figure 4 Labeling: There appears to be a mismatch in labeling. The Senegal site is referred to as panel (a), and should be (c). Please check the entire ms.

Response : We thank the editor for noting this labeling inconsistency. The panel labeling in Figure 4 has been corrected (Senegal is now correctly labeled as panel (c)).

5. Data Availability:

- Line 632: You mention the "limited number" of ISMN sites. However, there are additional observational datasets available in the region. Please verify if more data can be incorporated to strengthen your validation, or better justify why only these specific sites were used.

Response: We thank the reviewer for the suggestion regarding additional observational datasets in West Africa. In this study, only in situ observations that have undergone rigorous quality-control procedures and were flagged as “Good” by the ISMN (Dorigo et al., 2013) were selected for analysis. This ensures that the in situ data used are highly reliable, having passed stringent checks for accuracy and consistency, thereby minimizing uncertainties and enabling robust validation results. These criteria are explicitly described in the Data section (Subsection 2.2.3, Lines 272–275).

We are aware that additional ISMN sites exist in the region (e.g., in Ghana, Nigeria, and Côte d’Ivoire). However, many of these stations were installed relatively recently, primarily within the framework of the Trans-African Hydro-Meteorological Observatory (TAHMO), with most deployments in these countries occurring from 2019 onwards. As a result, these records do not overlap with our analysis period (2003–2019) and therefore do not meet the temporal coverage and quality-control criteria required for inclusion in this study. Consequently, incorporating these datasets would not allow for a temporally consistent validation across the full analysis period. For this reason, we restricted our validation to ISMN stations that satisfy strict data availability and quality requirements, ensuring a robust, homogeneous, and scientifically consistent comparison with the model and satellite products. This is also mentioned in the revised manuscript (Section 2.2.3. lines 278 – 282).

6. Corrections and Improvement:

- Line 603: There is a typo (an extra dot) after "5cm".

Response : In the revised manuscript, the typo on line 603 (extra dot after “5 cm”) has been corrected.

- Figure Captions: The current captions are poorly represented and need to be more descriptive and precise. Figure 1 should also be substantially improved. Please revise.

Response : The caption of Figure 1 has been revised to provide a clearer and more detailed description of the elements displayed in the figure, including the locations of the in situ soil moisture stations, the satellite grid cells used for comparison, and the regional domain used for the gridded validation. In addition, the figure has been improved by clarifying the graphical elements and ensuring that the different symbols and colors used in the map are clearly explained in the caption.

- I invite you to submit a revised manuscript that not only answers the reviewers' scientific questions but also rigorously corrects these presentation and formatting issues. The manuscript will then undergo further review.

Response : We have carefully reviewed the manuscript for presentation and formatting, including table layouts, figure numbering, and consistency throughout the text, to ensure it meets HESS standards.

 # Response to Reviewer 1 Comments

Review of Retrieving root-zone soil moisture from land surface modelling and GRACE/-FO and validating its dynamics with in-situ data over West Africa by Yap et al.

General comments:

This study benchmarked a number of modelling approaches and gridded datasets in West Africa, using in situ observations. The paper is not well written and cannot be published in its current form. In particular, a separate discussion section should be added. The table and figure captions are often incomplete. There is also no Data Availability section at the end of the paper.

Recommendation: major revisions.

Particular comments:

- L. 42: 'SM' is undefined.

Response: "SM" refers to soil moisture. In the Highlights section, the term "soil moisture" is written in full, and the abbreviation "SM" is introduced later in the main text (line 42).

- L. 162: The title of Section 2 is inaccurate. This section also includes a model description.

Response: The title of Section 2 has been revised to "Study area, datasets and models" to accurately reflect the inclusion of both datasets and model descriptions.

- L. 186 (Figure 1): I think that Figure 2 should be merged with Figure 1. The unknown satellite image in Figure 1 should be replaced by the land cover map in Figure 2.

Response: We thank the reviewer for this helpful suggestion. We carefully considered the possibility of merging Figures 1 and 2. However, Figure 1 is designed to highlight the geographical location of the meso-scale sites and the in situ soil moisture stations, including several graphical elements such as background photographs, the locations of the individual stations, and the 0.25° satellite grid cells used for the validation. Integrating the land-cover map into the same figure would likely reduce the readability of both datasets, as these elements would visually obstruct the land-cover information and make the map more difficult to interpret. For this reason, we believe that presenting the site location (Figure 1) and the land-cover distribution of the study area (Figure 2) in separate figures provides a clearer and more effective visualization of the information. Keeping the figures separate allows each map to focus on a specific aspect of the study while maintaining good visual clarity.

- L. 187 (section 2.2): Neither Section 2.2.1 nor Section 2.2.2 provides a description of a dataset. I suggest replacing the title of Section 2.2 with 'Data and Models'. A short introduction to Section 2.2 should mention which atmospheric forcing data are used to perform the model simulations, rather than including this important information in the model description sections. The titles of sections 2.2.1 and 2.2.2 should also make it clear that these sections deal with models.

- L. 223: CORDEX was not defined before.

- L. 238: CRUNCEP was not defined before.

Response: Section 2.2 has been renamed "Datasets and Models", and a short introductory paragraph (Lines 191 – 196) has been added to clearly describe the atmospheric forcing datasets used for the model simulations. CORDEX and CRUNCEP are fully defined at first mention.

We thank the reviewer for these constructive comments, which helped improve the clarity and structure of Section 2.2.

- Regarding the title and structure of Section 2.2 (Line 190): We agree that the previous structure could create confusion. The title of Section 2.2 has been revised to « Data and models » to better reflect its content (Section 2.2, Line 190). We have also added a short introductory paragraph at the beginning of this section clearly specifying the atmospheric forcing datasets used to drive the model simulations (e.g., GSWP3-W5E5, CRUNCEP, and WFDE5) with the corresponding references (Section 2.2, Lines 191 - 196).

- In addition, the titles of Sections 2.2.1 (Line 197) and 2.2.2 (Line 230) have been revised to explicitly indicate that they describe models (e.g., « Hydrological Models: WaterGAP and GLWS2.0 » and « Land Surface Model: CLM5.0 »), thereby improving consistency and readability.
- Regarding L. 223 (CORDEX not defined): We agree with the reviewer. CORDEX is now defined at its first occurrence as the « Coordinated Regional Climate Downscaling Experiment ».
- Regarding L. 238 (CRUNCEP not defined): We also agree. CRUNCEP is now defined at first mention as the combined Climate Research Unit (CRU) Time Series dataset merged with the National Centers for Environmental Prediction (NCEP) reanalysis product. The corresponding references have been clarified accordingly.

- L. 248: Only papers by Dorigo et al. are cited here. Dorigo et al. did not take these measurements. Please cite the papers describing the data and the sites here.

Response: We agree that the original references describing the measurement sites should be cited in addition to the ISMN data reference. Accordingly, we have revised the manuscript to include references describing the observational networks and study sites from which the soil moisture data originate. In particular, the stations in Benin and Niger belong to the AMMA-CATCH observatory, which is described (Cappelaere et al., 2009), while the Dahra station in Senegal is documented (Tagesson et al., 2014). These references have now been added in the revised manuscript (Section 2.2.3) to properly acknowledge the origin of the measurements and provide further information on the site characteristics and instrumentation.

- L. 397: In the description of Eq. 4, you should indicate that theta_wt and theta_fc correspond to wilting point and field capacity, respectively. What you call "wt" has been called SWI (Soil Westness Index) during the last 3 decades. It seems that Tian et al. did not use the standard term 'SWI'. Could you switch to using the term SWI and cite a paper that uses this concept? It should be mentioned that the main purpose of SWI is to highlight the drought signal rather than mapping soil texture-driven volumetric soil moisture. Drought maps can only be produced using SWI.

Response : In the revised manuscript, theta_wt and theta_fc are explicitly defined (Section 3.3.2, Lines 422-431) as the soil moisture at the wilting point (dry conditions) and at field capacity (wet conditions), respectively. Values of “wt,” close to 0 indicating soil moisture conditions approaching the wilting point (dry state) and values close to 1 indicating conditions near field capacity (wet state). The formulation now adopts the standard terminology Soil Wetness Index (SWI) instead of “wt,” in line with long-standing usage in the literature. Foundational and relevant references employing the SWI concept have been added as suggested. The text now clearly states that the primary purpose of SWI is to highlight relative moisture anomalies and drought signals, rather than to represent absolute, soil texture–driven soil moisture.

- L. 414: 'The episodic and seasonal variations': these should be clearly defined, as should the method used to obtain them.

Response: In the revised manuscript, the concepts of “episodic” and “seasonal” variations have been explicitly defined. Seasonal variations are identified as long-term fluctuations with periods longer than one year, obtained by applying a 12-month rolling boxcar filter to smooth the original soil moisture time series. Episodic variations are defined as short-term fluctuations and event-scale anomalies occurring on timescales shorter than one year, derived as the residual signal after subtracting the smoothed seasonal component from the original time series. These clarifications, along with a description of the method used to obtain them, have been incorporated into the Methods section (Section 3.3.3., Lines 442 to 445).

- L. 425: The results and discussion sections should be separate. The current organisation of the paper is unclear.

Response :

We thank the reviewer for this comment and for raising the issue of manuscript structure. After internal discussion among the co-authors, we would like to clarify that the current structure was chosen intentionally. Given that the manuscript already includes a dedicated Conclusions and Implications section, we consider that a fully separated Results and Discussion structure is not strictly necessary for this study. In our view, the close coupling between results and their interpretation is essential for the methodological and regional analyses presented, and an integrated Results–Discussion approach is commonly used in similar hydrological and geophysical studies. That said, we agree as a team that parts of the manuscript would benefit from clearer organization. In the revised version, we will improve the structure by more clearly distinguishing descriptive results from interpretative statements within each subsection and by relocating broader synthesis, cross-regional comparisons, and implications to the Conclusions and Implications section. We believe this approach preserves the scientific coherence of the study while addressing the reviewer's concern regarding clarity. We are open to further restructuring if recommended by the editor.

- L. 432: The methods used to produce these time series should be clearly presented in the 'Methods' section.

Response: The section 3.3.3 (Lines 441 to 446) has been updated to clearly describe the procedures used to produce the deseasonalized and episodic soil moisture time series. Specifically, the deseasonalized component is obtained using a 12-month rolling boxcar filter to isolate long-term variations, while the episodic component is derived as the residual signal after removing the seasonal trend.

- L. 437: The quality of Fig. 3 could be improved. Could each year on the x-axis be clearly identified? Indicating months is not useful. The caption is incomplete.

Response: The x-axis in Figure 3 shows months to provide a clear view of the seasonal cycle within each year, which is essential for interpreting short-term variability and the timing of wet and dry periods. Indicating individual years alone would obscure the intra-annual dynamics that are critical for evaluating the seasonal performance of the soil moisture products. The caption has also been revised.

- L. 442-444: Why have you suddenly moved from Africa to the Tibetan Plateau? This is confusing. Move this part to a discussion section.

Response : As noted by the Editor-in-Chief, this point has been addressed in the revised manuscript by first focusing on the influence of the West African monsoon on soil moisture dynamics within the study region, and then placing the Tibetan Plateau example in a broader comparative context of monsoon-driven soil moisture variability. This discussion is presented in Section 4.1.1 (Lines 479–486)

- L. 444-448: Move to a discussion section.

- L. 464-466: This sentence should be moved to the Methods section or to the Discussion section.

Response: The content in lines 444-448 and 464-466 is already included in the joint Results and Discussion section. We intentionally use an integrated structure to closely couple results with their interpretation, which is common in hydrological studies. In the revised manuscript, we improve clarity by distinguishing descriptive results from interpretative statements within subsections and moving broader synthesis and cross-regional comparisons to the Conclusions and Implications section. This preserves coherence while addressing the reviewer's concern.

- P. 22, Table 2: Use the correct symbols as defined in the main text. 'Corr' should be replaced by 'R', 'RMSE' by 'RMSE', and so on. To improve readability, use fewer digits. For example, replace 0.894 with 0.89.

Response: We have updated Table 2 to use the correct symbols as defined in the main text (e.g., 'Corr' replaced by 'R', 'rmse' by 'RMSE') and correct the number of digits for improved readability

- P. 22, Table 3: Table 3 cannot have the same title as Table 2.

Response: We thank the reviewer for pointing this out. The title of Table 3 has been updated to:

Table 3: Statistics of the episodic and deseasonalized components of the normalized soil moisture time series across the Senegal, Niger, and Benin study sites (Page 23).

- L. 491: The Sahel region should be labelled on either Fig. 5 or Fig. 1.

Response: The Sahelian region is already defined and described in Section 2.1 (Study Area, Lines 166–177) and is explicitly shown on the land cover map presented in Fig. 1 of the manuscript.

- L. 497-506: This part should be moved to a discussion section.

Response: As noted above, the manuscript adopts an integrated Results–Discussion structure, as agreed upon by the co-authors. The content in Lines 497–506 is therefore intentionally included in this section.

- L. 513-523: This part should be moved to a discussion section.

Response : As discussed above, the content in Lines 513–523 is therefore intentionally included in the Results and Discussion section.

- L. 531: "these models": is ESA CCI a model?

Response: This was a misstatement and has been corrected. ESA-CCI is a satellite-derived soil moisture dataset, not a model. Accordingly, "these models" (section 4.2, line 571) in the manuscript have been replaced with "these datasets" to accurately refer to ESA-CCI and GLWS2.0.

- L. 534-554: This part should be moved to a discussion section.

Response : The content in Lines 534–554 is included in the Results and Discussion section.

- L. 555: Section 4.3 should be moved to a discussion section.

Response : The content in Line 555 is included in the Results and Discussion section.

- l. 608: The conclusion section is far too long. Some parts could be moved to a discussion section. This section should focus on the main take-home messages and possible areas for further research.

Response: We agree that the Conclusions section is currently too long. After discussion among the co-authors, we have substantially shortened this section rather than modifying the overall manuscript structure. In particular, the part at the beginning of the Conclusions, which largely repeated results (Lines 647–659), has been reduced, allowing the section to focus more clearly on key take-home messages and directions for future research.

Editorial comments:

- L. 223: “?”

Response: We agree that the “?” in the reference is a mistake. This has been corrected in the revised manuscript (Line 233).

- L. 479: Figure 4 or Figure 5?

Response : This was a mistake. It is Figure 5 ; This has been corrected in the revised manuscript (sub-section 4.1.3, Line 517).

- L. 525: Figure 5 or Figure 6?

Response : This was a mistake. It is Figure 6 ; This has been corrected in the revised manuscript (sub-section 4.2, Line 565).

- L. 563: Figure 6?

Response : This was a mistake. It is Figure 7 ; This has been corrected in the revised manuscript(sub-section 4.3, Line 602).

 # Response to Reviewer 2 Comments

We thank Reviewer 2 for the positive evaluation of the study’s relevance and for the insightful suggestions to strengthen the methodological and interpretative aspects of the manuscript.

This study offers a valuable integration of GRACE/-FO-based water storage anomalies with land surface and hydrological model soil moisture products to estimate root-zone soil moisture. The fusion of these data sources is a significant contribution to understanding soil moisture dynamics. However, the methodological novelty, in comparison to previous GRACE assimilation or analytical approaches, is not fully explained. The paper would benefit from a clearer explanation of how the adopted analytical solution extends beyond existing methods, and what specific improvements in accuracy or process representation have been achieved by this approach.

Response: We thank the reviewer for highlighting this important point. In the revised manuscript, we have clarified the methodological contribution of our study by more explicitly positioning it with respect to previous GRACE-based assimilation and analytical approaches. In particular, we emphasize that our approach enables a consistent comparison between GRACE-informed model estimates of root-zone soil moisture and in situ observations measured at different depths. To our knowledge, this is the first study that evaluates root-zone soil moisture from the GRACE-informed global hydrological model GLWS2.0 against in situ soil moisture observations while explicitly accounting for the measurement depth using an analytical solution of the Richards equation. This step is necessary to ensure a physically consistent comparison between model-derived root-zone soil moisture and in situ measurements collected at specific depths. To make this contribution clearer to the reader, we have added a new sentence in the Introduction (Lines 135-137) highlighting the novelty of this approach and explaining how it allows a consistent evaluation of GRACE-derived root-zone soil moisture estimates from WaterGAP/GLWS2.0 against in situ observations.

While the results are well presented, they remain largely descriptive. The discussion could be expanded to explore the physical reasons behind the observed differences between products, particularly in relation to regional hydro-climatic conditions. Interpreting how these differences manifest in the context of the West African climate would significantly enhance the scientific insight and generalizability of the conclusions.

Response: We thank the reviewer for pointing out this. In the revised manuscript, the Results and Discussion section, has been expanded to provide a deeper interpretation of the observed differences between products. These differences are explicitly linked to regional hydro-climatic conditions in West Africa, including variations in precipitation regimes, and vegetation dynamics.

The workflow is logically structured, but certain analytical steps, particularly the parameterization of the Richards equation, require deeper justification. A more thorough explanation of why specific parameter values were chosen would add robustness to the methodology. Additionally, a stronger uncertainty analysis is needed to make the validation results more convincing. The current approach could benefit from more detailed discussion of the limitations and sources of uncertainty, especially in relation to data assimilation.

Response: We thank the reviewer for this comment. In the revised manuscript (Section 3.1, Lines 358–362), we clarify that $\theta\infty$ represents the long-term mean surface soil moisture, and the analytical solution of Richards' equation is applied iteratively from the surface to the root zone using representative values for the soil hydraulic parameters D and k. These parameters are calibrated using in situ measurements and observed minimum and maximum soil moisture as suggested by Sadeghi et al. (2020).

The use of the GRACE/-FO-based global assimilation model GLWS2.0, which is based on WaterGAP, is an interesting aspect of the study. However, the paper lacks sufficient details on how GRACE data, which includes various components like groundwater, soil moisture, and vegetation weight, is assimilated into the model. It would be helpful to know how the integration of these different components improves soil moisture predictions specifically. For example, how do variations in groundwater levels influence soil moisture estimates? Are these variations significant enough to warrant inclusion in the model?

Response: We thank the reviewer for this comment. A full description of the GLWS2.0 data assimilation framework is provided in Gerdener et al. (2023), which we cite in the revised manuscript. As the reviewer points out, the GRACE data include various components like groundwater storage chnage, soil moisture, surface water body volume change, snow, and vegetation water change. For the region of our study, variability in TWSA at seasonal and longer timescales is dominated by groundwater change, followed by soil moisture, and then followed by surface water changes. It is thus important to deal with groundwater changes in a correct way. In WaterGAP and thus also GLWS, groundwater and surface water abstractions are derived individually for a number of water use sectors. Runoff for each grid cell is calculated from the soil water balance, and then partitioned into fast surface, subsurface runoff and diffuse groundwater recharge (Müller-Schmied et al., 2021). In GLWS, the GRACE information is partitioned into the

WaterGAP storages including groundwater on the basis of weighting model dynamics against observation error covariance. For this, an ensemble of model runs is created while seeking to map model errors.

The application of the analytical solution to Richards' equation, as proposed by Sadeghi et al. (2020), is an interesting approach, but the assumptions underlying the Gardner model need to be more thoroughly examined. This model assumes homogeneous soil and neglects plant water uptake, which may not align with the assumptions of the WaterGAP model. The paper does not sufficiently address whether these assumptions are compatible or if they introduce contradictions. A more detailed discussion of the theoretical framework, particularly how the assumptions in both models align or diverge, would strengthen the overall argument.

Response: We thank the reviewer for this important comment. We acknowledge that the analytical solution of Richards' equation, as applied in this study, represents a simplification of real-world soil processes. In WaterGAP and GLWS2.0, soil moisture and groundwater are represented conceptually, with storages treated as "black boxes" and fluxes such as groundwater recharge explicitly modeled. The WaterGAP model and thus GLWS define soil in a deep layer as comapred to e.g., remotely sensed soil moisture that usually presents the first few centimeters of soil storage. Soil heterogeneity and plant water uptake are included only in a simplified manner in these models. In contrast, the analytical solution does not consider soil type variations or root water uptake explicitly, and CLM provides a more detailed representation of these processes. To clarify this limitation, we have added the following statement at the end of Section 5 (Lines 681-685):

"*We acknowledge that the analytical solution of the Richards' equation represents a simplification of reality. Implementing a more refined (numerical) version, together with more detailed representations of soil types, plant root water uptake, and soil hydraulics, would represent a step towards assimilating GRACE data into more sophisticated land surface hydrology models. We believe this should be explored in future studies (Sprimger et al., 2026).*"

This addition explicitly recognizes the assumptions of the analytical method and contextualizes its use relative to conceptual and physically detailed models.

While the results are presented in detail, there is a need for deeper analysis of the underlying processes that lead to the observed patterns in the data. For instance, how do soil moisture and flux relationships differ across various depths, and how do these differences relate to regional hydro-climatic variability? Expanding this part of the discussion would help connect the empirical findings with broader environmental processes.

Response : We agree with the reviewer that this would be a very useful analysis, but we fear this would change the character of the paper significantly. The relationships between soil moisture and flux across various depths could be analyzed purel from data, but the data are sparse as we describe here. Or they could be analysed from the CLM simulations, but again this is not in the focus of this paper.

Minor Comments:

- The paper would benefit from a clearer structure in terms of presenting the key innovations of the study. It is important to ensure that the reader can easily identify how this work advances current understanding, particularly in relation to model improvements and new methodological contributions.

Response: We thank the reviewer for this helpful suggestion. In the revised manuscript, we have added a short framing paragraph in the Introduction (Lines 60-64) highlighting the importance of soil moisture, the sparsity of in situ networks (Lines 77-80), the limitations of surface-only remote sensing products (Lines 98-102), and the strong dependence of root-zone soil moisture estimates on model assumptions, thereby emphasizing the need for validation. In addition, we have condensed the beginning of the Conclusions section into a concise synthesis, replacing repetitive content and clearly summarizing the main findings relative to initial expectations. These additions aim to make the key innovations and contributions of the study more visible to the reader.

- Additional references to recent work in the field of soil moisture modeling and data assimilation, especially focusing on studies that use GRACE data, would provide more context and help justify the novel aspects of this study.

  Response: We thank the reviewer for this suggestion. In the revised manuscript, we have added references to recent studies on soil moisture modeling and data assimilation, with a particular focus on works that utilize GRACE data. Specifically, we now cite the review by Springer et al. (2026) to provide context for GRACE-based data assimilation approaches.

Once again, we thank both reviewers for their valuable comments, which will significantly improve the quality and clarity of the manuscript.