Author Response to Referee Comments

Long-term irrigation water use datasets from multiple Earth Observation-based methods in major irrigated regions

Laluet et al., essd-2025-737

We thank the Referees for their constructive and insightful comments. We have carefully revised the manuscript in response to these comments. All changes are marked using track changes in the revised manuscript. Below, we provide a detailed, point-by-point response (referees' comments are shown in black, and our responses in blue).

Referee #3

Comment 1:

My first concern is regarding snow-dominated basins and how the delayed effect of snow melt on soil moisture (SM) is tackled. Some of the study regions such as CONUS and Ebro Basin (as mentioned by the authors) contain snow dominated basins. Since the Delta and Inversion approaches attribute SM anomalies to irrigation after accounting for precipitation, it is important to clarify how snowmelt-driven soil moisture increases are distinguished from irrigation signals. Some clarity on how the various methods deal with quantifying snow melt is warranted.

Response:

We thank the referee for raising this important point. Several aspects of our methodology reduce the risk of confusing snowmelt-driven soil moisture variations with irrigation signals.

First, the IWU retrievals are spatially restricted to areas equipped for irrigation using the GMIA dataset, which effectively excludes mountainous areas where snow accumulation and melt predominantly occur. This spatial filtering provides the primary safeguard against snowmelt contamination.

Second, even within the retained pixels, snow-dominated areas and irrigated lowlands are largely spatially separated in the study regions. In the Ebro Basin, for example, snowmelt in the Pyrenees primarily feeds reservoir storage and river discharge, which then supplies irrigation water to downstream plains through canal systems. It does not directly generate soil moisture increases over irrigated croplands.

Third, the temporal restriction of IWU retrievals to the irrigation season defined for each region further reduces the overlap with the main snowmelt period, although some late snowmelt may still occur in transitional areas during the early months of the irrigation window.

While we cannot fully exclude residual snowmelt contributions in such transitional areas, the combined effect of the spatial filtering, the physical separation between snowmelt and irrigation zones, and the temporal restriction is expected to minimize such contamination to a level that does not materially affect the IWU estimates.

To clarify these aspects, we have added a sentence at the end of the spatial and temporal processing description in Section 2.3.4.

Changes in manuscript (Section 2.3.4):

"These spatial and temporal masks also serve as safeguards against potential confusion between snowmelt-driven soil moisture increases and irrigation signals, as snow accumulation and melt predominantly occur in mountainous areas and periods that fall outside the retained pixels and time windows."

Comment 2:

In line with comment #1, how do the assumptions made in each method affect the impact of snow-melt? For instance, the SM-based inversion method assumed runoff is negligible. Will this assumption hold for such basins? Further, is the precipitation dataset used the total accumulated precipitation, containing both rainfall and snowfall components?

Response:

We thank the referee for this follow-up question.

As discussed in our response to Comment 1, snow-dominated areas in the study regions are mainly located in mountainous zones, whereas irrigation occurs primarily in lowland agricultural plains. Snowmelt processes are therefore largely spatially separated from the irrigated areas where IWU retrieval is performed.

Regarding the precipitation forcing, the SM-based Inversion approach uses ERA5 forecast precipitation fields, which represent total precipitation and therefore include both rainfall and snowfall components.

Concerning the assumption of negligible surface runoff, this simplification is justified here on physical grounds: the major irrigated areas analysed in this study are predominantly located in flat agricultural regions, where surface runoff is generally small compared to evapotranspiration and infiltration. At the 0.25° spatial resolution considered here, local runoff processes are further spatially averaged within grid cells. This assumption is consistent with previous studies using similar approaches (e.g., Brocca et al., 2014; Dari et al., 2020; Laluet et al., 2024; Olivera-Guerra et al., 2023).

We have added a short clarification in Section 2.2.2.

Changes in manuscript (Section 2.2.2):

"Surface runoff is assumed to be negligible, as the major irrigated areas analysed here are predominantly located in flat agricultural regions."

Additional correction (Section 2.2.2):

During revision, we also corrected an error in the description of the SM-based Inversion calibration procedure. The original text stated that the parameters are "calibrated jointly during rainfall-free periods", whereas in practice the calibration uses the non-irrigated season and rainfall days within the irrigation season. This correction does not affect the results, as the actual implementation was correct.

"The parameters a, b, F, and nZ are calibrated jointly over the non-irrigated season and rainfall days within the irrigation season."

Comment 3 (see also Referee 2, general comment):

In the Results/Technical Validation section, the author's spend considerable length reiterating values that readers can infer by looking at the visual figures. Not much is mentioned about why these patterns or discrepancies across methods occur. For example, in section 3.1.2, lines 267-274, numerous IWU values are stated for specific regions in CONUS, without accompanying discussion of the possible underlying drivers. These values are easily inferred from Figure 3. However, the readers are left with questions such as why IWU in California is significantly higher in Method 3 vs Methods 2 and 1? I realize that the authors might have touched upon the reasons in section 4 and 5. However, in its present form, it is difficult to infer the scientific explanations of the observations in Section 3. I therefore encourage the authors to modify Section 3 to include some discussion of the mechanistic or structural reasons behind various observations. This will transform it from primarily descriptive reporting into a mix of reporting observations and interpretive analysis, which I believe will improve readability.

Response:

We thank both referees for highlighting this point (Referee 2 raised a similar concern in their general comment). We agree that Section 3 would benefit from linking the observed differences to methodological characteristics.

In the revised manuscript, we have integrated interpretive elements directly into the existing descriptive paragraphs throughout Section 3, explaining that:

- The SM-based Inversion approach relies primarily on soil moisture, which at 0.25° may provide a spatially averaged and potentially weak irrigation signal, leading to smoother patterns and lower magnitudes.
- The SM-based Delta approach additionally exploits ET differences, which are generally more sensitive to irrigation than near-surface soil moisture at this scale (Crow et al., 2025 https://doi.org/10.1016/j.jag.2025.104773) producing stronger spatial gradients.
- The Model-observation integration dataset inherits its spatial heterogeneity from the LGRIP30 irrigated-area fraction used to scale simulated irrigation within each 0.25° grid cell, while regional magnitude differences partly reflect the Noah-MP parameterisation.
- For the Ebro, Murray-Darling, and India sections, we added concise region-specific interpretations with cross-references to the CONUS discussion (Sect. 3.1.2) to avoid repetition.

Changes in manuscript:

- Section 3.1.2: Interpretive elements integrated into the existing paragraph describing spatial patterns.

- Section 3.2.2: Short addition linking Ebro patterns to Sect. 3.1.2, with a note on signal dilution in small fragmented districts.
- Section 3.3.2: Short addition referencing Sect. 3.1.2, with a note on Noah-MP parameterisation.
- Section 3.4.2: Short addition explaining the wider inter-method spread in India.

Comment 4:

The manuscript contains several figures that convey similar statistical comparisons (Figures 2,5,7). Consider consolidating them into one multi-panel figure to reduce repetitiveness.

Response:

We appreciate the suggestion and considered it carefully. We agree that Figures 2, 5, and 7 present similar types of performance metrics. However, we chose to keep them separate because each figure is introduced within its corresponding regional subsection, allowing readers to interpret the evaluation metrics in direct connection with the spatial patterns and time series that follow. Combining all metrics into a single multi-panel figure would make it considerably more complex and require readers to navigate between panels when reading the different regional analyses. For these reasons, we prefer to retain the current structure.

Comment 5:

In section 2.2.1, the authors assume that EO-based and model-simulated ET agree under non-irrigated conditions. How important is this assumption? Consider adding a plot of bias values or descriptive statistics table for a sample case in the supplementary to support.

Response:

We thank the referee for this helpful suggestion. The assumption that EO-based and model-simulated evapotranspiration agree under non-irrigated conditions is indeed an important element of the SM-based Delta approach.

Rather than adding a supplementary figure for a single illustrative case, we have added a reference to Laluet et al. 2026 ("Assessing the suitability of global evapotranspiration products over irrigated areas", https://doi.org/10.5194/hess-30-1779-2026), who evaluated multiple ET products over irrigated regions and showed that EO-based and model-based ET products exhibit similar values outside the irrigation season, while substantial differences emerge during summer when irrigation is active (their Fig. 5), which supports the validity of this assumption.

Changes in manuscript (Section 2.2.1):

"This assumption is consistent with Laluet et al. (2026, their Fig. 5), who showed that EO-based and model-based ET products exhibit similar values over irrigated regions outside the irrigation season, while substantial differences emerge when irrigation is active."

Minor comments

Minor comment 1:

Eq. 1 and 2 have repeating P(t) terms. Did the author's mean R(t)?

We thank the referee for spotting this error. The second P(t) term in Eq. 1 and 2 should read R(t) for runoff. This has been corrected.

Minor comment 2:

Section 1 Line 27 "With global demand for irrigation projected to rise due to population growth, dietary shifts, and climate change, .." requires a supporting reference.

We have added a supporting reference: Elliott et al. (2014, https://doi.org/10.1073/pnas.1222474110), who explicitly discuss projected increases in irrigation water demand driven by population growth, dietary shifts, and climate change.

Minor comment 3:

Are any precipitation datasets used. If so, append Table 1 with relevant details.

ERA5 forecast precipitation fields are used as input for the SM-based Inversion and Model-observation integration approaches. We have added this information to Table 1 and updated the table caption accordingly.

Minor comment 4:

Line 231-233. Details about what the dash indicates can move to Table 2 description.

The explanation of the dash and check mark symbols was already present in the Table 3 caption. We have removed the redundant sentence from the text body (Section 2.5).

Referee #2

Comment 1:

The three methods rely on different assumptions, and their performance varies across regions. It would be helpful if the authors could further clarify how these assumptions influence uncertainty and dataset differences. A more systematic comparison linking methodological assumptions to regional climate, irrigation practices, soil conditions, and water sources would strengthen the manuscript.

Response:

We thank the referee for this comment. This point is closely related to Referee #3's Comment 3, and we have addressed both together. In the revised manuscript, we have integrated interpretive elements throughout Section 3 that explicitly link the observed spatial and magnitude differences to the methodological characteristics of each approach. Please refer to our detailed response to Referee #3, Comment 3.

We note that a fully systematic analysis linking methodological assumptions to regional factors such as irrigation practices (e.g., drip vs. flood), water sources (groundwater vs. surface water), or soil properties would require dedicated investigations beyond the scope of this dataset description study. Nevertheless, the interpretive elements added in Section 3 provide a first-order explanation of the main differences observed across methods and regions.

Comment 2:

In several regions, correlations are moderate and biases remain noticeable. It would be beneficial for the authors to further discuss whether the current level of accuracy is sufficient for intended applications, such as climate modeling or trend analysis.

Response:

We thank the referee for this comment. We have revised the Recommendations for users section (Section 4.5) to better specify the intended scope of the datasets. We now clarify that the datasets are primarily intended as a coordinated benchmarking resource for the irrigation research community, rather than as ready-to-use products for specific applications, and we direct users to the evaluation results in Section 3 when assessing fitness for use.

Changes in manuscript (Section 4.5):

"Beyond these interpretation guidelines, the datasets are primarily designed as a coordinated benchmarking resource for the irrigation research community, supporting large-scale analyses, intercomparison exercises (e.g., Dari et al., 2025a), and model benchmarking, rather than as ready-to-use products for specific applications. Combining them with complementary observations or model outputs can further support robust interpretation and help mitigate uncertainties associated with individual datasets. Users should consider the evaluation results presented in Section 3 when assessing the suitability of specific datasets for their intended use."

Comment 3:

The study applies static irrigated-area maps over long time periods. In regions where irrigated area has changed over time, this may introduce bias. A clearer discussion of how this limitation may influence long-term analysis would improve the paper.

Response:

We thank the referee for pointing out this important limitation. We have added a new subsection in Section 4 (Section 4.3) discussing this issue.

Changes in manuscript (new Section 4.3):

"An additional source of uncertainty relates to the irrigated-area maps used across the three approaches. The SM-based Delta and SM-based Inversion methods use GMIA v5, representing conditions around 2005, while the Model-observation integration approach uses the Landsat-derived LGRIP30 map for the year 2015. Neither map captures temporal changes in irrigation extent, which may affect long-term IWU estimates in regions where irrigated areas have evolved over the study period. The main irrigated regions analysed here have generally exhibited relatively stable irrigation extents over the last two decades (Mehta et al., 2024), although local changes may still have occurred. Future developments could benefit from recently available temporally resolved irrigation maps (e.g., Kebede et al., 2025) to better account for such changes."

Comment 4:

The results vary depending on the soil moisture, ET, and irrigation datasets used. The manuscript would benefit from a clearer explanation of why certain input datasets perform better than others, including a discussion of their respective strengths and limitations.

Response:

We agree that the choice of input datasets influences the resulting IWU estimates, and the evaluation presented in Section 3 (heatmaps in Figures 2, 5, and 7, and Table 4 for India) documents how performance varies across all SM and ET input combinations. In the revised manuscript, we have added interpretive elements in Section 3 that explain how the methodological characteristics of each approach, as well as the type of input data used (e.g., SM and ET for the Delta approach vs. primarily SM for the Inversion approach), contribute to the main differences observed.

However, explaining why specific SM products (e.g., CCI Passive vs. ASCAT) perform differently across regions would require a dedicated assessment of each product's retrieval characteristics over irrigated areas, which is beyond the scope of this dataset description study. The influence of irrigated-area maps is discussed in the new Section 4.3. Previous evaluations of ET products over irrigated regions (Laluet et al., 2026; https://doi.org/10.5194/hess-30-1779-2026) provide useful guidance on this aspect.

Comment 5:

Several key thresholds and parameters are applied uniformly across all regions. The authors may consider evaluating the sensitivity of the results to these settings and discussing whether region-specific calibration could further improve performance.

Response:

We thank the referee for this suggestion. We have added a short acknowledgement of this limitation in Section 4.1 for the SM-based approaches. The sensitivity of the Noah-MP irrigation-triggering thresholds is already discussed in Section 4.2.

Changes in manuscript (Section 4.1):

"The SM-based approaches also rely on fixed thresholds, such as the 0.12 relative SM increase filter in the Delta method (Zaussinger et al., 2019) or the 5% GMIA irrigation mask, which were adopted from previous studies and applied uniformly across regions. Assessing the sensitivity of the IWU estimates to these settings would help further characterise the robustness of the retrieved irrigation signal."

Comment 6:

The discussion section could be strengthened by providing a more integrated cross-regional synthesis that highlights common patterns, key differences, and broader implications of the results.

Response:

We thank the referee for this suggestion. The interpretive elements added throughout Section 3 (see response to Comment 3 from Referee #3) now provide cross-regional context. Several consistent patterns emerge across regions: (i) SM-based approaches systematically produce smoother spatial patterns than the Model-observation integration approach, reflecting the coarser and more spatially averaged nature of the soil moisture signal at 0.25°; (ii) the Model-observation integration approach yields the strongest spatial correlations with reference data but also the largest biases, reflecting the combined effect of detailed irrigated-area mapping and model parameterisation uncertainties; and (iii) no single SM input dataset consistently outperforms the others across regions, as relative performance depends on regional conditions. These cross-regional patterns are also reflected in the Conclusions section. We believe these elements address the referee's concern without requiring a separate synthesis section.

Comment 7:

The readability of the heatmaps in Figures 2, 5, and 7 could be improved by enlarging axis labels, legends, and color scales.

Response:

We thank the referee for this suggestion. The axis labels, legends, and color scales in Figures 2, 5, and 7 have been enlarged in the revised manuscript.