Author Response to Referee #3 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 Referee #3 for their constructive and insightful comments. Below, we provide a detailed, point-by-point response (referees' comments are shown in black, and our responses in blue).

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).