We thank the reviewer for the comments, and we provide responses below.

1. Please note that we responded to most comments of this reviewer on our initial submission (https://editor.copernicus.org/HESS/ms-records/egusphere-2024-3205), but our earlier responses might not have been visible to the public for technical reasons - it was embedded in the response to the editor. We have therefore uploaded our earlier responses again in **part 1 of our reply**. In the public review of our initial submission (https://egusphere.copernicus.org/preprints/2024/egusphere-2024-3205/), the reviewer stated that "the paper does an excellent job comparing AquaCrop and Noah-MP", and only minor comments were given on (a) data assimilation (DA; our team works on data assimilation indeed, but this paper is not about DA), (b) irrigation losses (addressed in the responses and resubmission), and (c) a request to cite a specific paper, which we added.
2. We would like to kindly ask the reviewer to specify which exact analysis is requested if some of our earlier (and now rephrased) responses would remain unclear**:** please see our new responses **and questions below**.
3. We are aware that our paper has likely been uploaded in an AI platform, leading to circular comments and misinformation. We hinted to this in our earlier responses, and we hope that the reviewers and editorial team will thus act with care.

The original reviewer questions are in blue italic fonts, our responses and questions are in black normal fonts.

1. *The study highlights that Noah-MP simulates higher irrigation rates (434 mm yr $^{-1}$) than AquaCrop (268 mm yr $^{-1}$) due to differences in accounting for losses (e.g., runoff, interception, and soil evaporation). However, the manuscript could better clarify how these losses are quantified and modeled in each system. For instance, the absence of canopy interception in AquaCrop is noted, but the implications for irrigation efficiency and model performance are not thoroughly discussed.*

The model performance and the impact of irrigation losses on irrigation, and all water balance parts has been extensively discussed in sections 3 (L.331, L.338-345, L.357-359, L.426, L.432, L.441, L.472) and 4 (L.506-508, L.590-591) – it is indeed a key aspect of the paper and therefore addressed in many parts of the paper (L.10-12, L.189, L.194). <u>See also our earlier responses</u>. We are convinced that this covers everything, but if we missed anything, can the reviewer specify which exact analysis is requested?

2. *The study uses default soil hydraulic parameters (SHPs) for both models, which differ significantly (e.g., Noah-MP's higher total available water, TAW). While Section 4.2 acknowledges this limitation, the manuscript lacks a sensitivity analysis to assess how variations in SHPs (e.g., θ_FC, θ_WP) impact irrigation estimates.*

This comment was very thoroughly addressed in previous review, please see our earlier responses. Both models are expecting their own set of parameters by design (fluxes are tuned to them) and we use the recommended parameterization. We have also clarified that using the same parameters in both models has led to unrealistic results.

3. *The comparison with satellite-based irrigation estimates from Dari et al. (2023) shows discrepancies, particularly in interannual variability (Section 3.1.2). The manuscript suggests potential inaccuracies in the satellite data but does not explore this further or provide evidence to support this claim.*

Please refer to Dari et al. (2023) for a discussion of the errors. We are convinced that this is scientifically rigorous. Which exact other analysis would the reviewer suggest to add in our paper, keeping in mind that we need to maintain our focus on a model comparison?

4. *Both models show significant overestimation of vegetation (DMP) compared to CGLS data, attributed to simplistic vegetation modules and suboptimal parameterization (Section 3.1.3). The manuscript could improve by discussing specific parameterization choices (e.g., maximum canopy cover in AquaCrop, vegetation module options in Noah-MP) and their impact on results.*

Sensitivity studies on vegetation modules are beyond the scope of this paper, and vegetation modeling is hard to validate due to uncertain reference data (DMP). For both models, we use the recommended settings, in line with our response to comment 2, and as already mentioned in the manuscript. We can add 1 sentence reiterating that dynamic vegetation modeling is still in its infancy in general (e.g. Mahmud et al., 2021, https://doi.org/10.1029/2021JG006400,)

5. *While the authors briefly mention data assimilation (DA) as a potential approach for integrating observations (e.g., Abolafia-Rosenzweig et al., 2019; Busschaert et al., 2024; Igder et al., 2022; Maina et al., 2024; Modanesi et al., 2022; Nie et al., 2022), the discussion could benefit from the inclusion of additional relevant studies. For example, the work titled "Assimilation of Sentinel-Based Leaf Area Index for Modeling Surface-Ground Water Interactions in Irrigation Districts" presents a valuable application of DA in a coupled surface–subsurface hydrological context, specifically within agricultural settings. Including such studies would help strengthen the section by illustrating the diversity of DA techniques and their relevance to integrated hydrologic modeling in real-world systems.*

We have already added the earlier suggested papers by this reviewer. Please see our earlier responses.

6. *The study finds that irrigation estimates correlate better with in situ data at longer temporal aggregations (e.g., monthly, two-monthly; Section 3.2). However, the*

manuscript does not adequately address how this aggregation might mask short-term inaccuracies in irrigation timing, which is critical for real-world applications.

The results for daily, 7-day and 15-day irrigation are included in the manuscript (Fig 8, 9,...) which might have been overlooked by the reviewer and thus we assume this comment as solved. Furthermore, we emphasize that for comparison with independent data, aggregation is necessary to smooth out short-term discrepancies because also benchmark irrigation data are not free from uncertainty (Massari et al. 2021). This is also further explained and discussed in the text L.249, L.365-366, L.483, and L.508-512.

7. *The models rely on MERRA2 meteorological forcings, downscaled via bilinear interpolation (Section 2.2.1). The manuscript does not address potential uncertainties introduced by this downscaling or the coarse resolution of MERRA2 (0.5° × 0.625°). A brief analysis or reference to studies evaluating MERRA2's accuracy in the Po Valley would enhance confidence in the input data.*

MERRA2 precipitation is uncertain without even downscaling it. An evaluation of MERRA2 precipitation uncertainty is done in earlier literature and does not change the key findings of our paper. We are looking for the differences in 2 models, both forced with the same forcings. <u>We will add 1 sentence to highlight uncertainties in forcings in the discussion.</u>

8. *The study excludes paddy rice areas and clay soils due to AquaCrop's limitations in simulating sprinkler irrigation on low-conductivity soils (Section 2.1). This exclusion reduces the study's scope, as rice paddies are significant in the Po Valley. The manuscript should discuss the implications of this exclusion for regional irrigation estimates and propose how future studies could incorporate these areas (e.g., by adapting AquaCrop for flood irrigation).*

To our knowledge rice paddies occupy a small portion of the Po River valley (north western) which is only 10% with respect to the entire agricultural area and less than 15% of the total irrigated area (https://www.adbpo.it/download/PdGPo_24febbraio2010/PDGPo_ELABORATO_14_DocumentiTecniciDiRiferimento/Agricoltura2010_MIPAF.pdf).

In case the reviewer owns a different and more detailed information of rice paddy distribution in the Po River we would be happy to mention it in the paper. We also want to clarify that the other irrigation methods and implications of not simulating the paddy rice areas were already mentioned around e.g. L.389, and again in the discussion (Section 4.2). In the revised version <u>we will add 1 sentence explicitly referring to flood irrigation.</u>