Response to referee comments: Anonymous Referee #1

We appreciate the reviewer for dedicating their time and offering valuable suggestions to enhance the manuscript. Below, we address each specific comment and suggest revisions to address the concerns.

Comment: Line 42. For the Ebro basin several methodologies for deriving irrigation amounts also have been published: https://essd.copernicus.org/articles/15/1555/2023/ https://www.sciencedirect.com/science/article/pii/S0378377424001082 https://hess.copernicus.org/articles/28/441/2024/;

Response: Thank you for providing published methodologies for deriving irrigation amounts. We will discuss relevant methodologies and their application in the Ebro basin in our manuscript.

Comment: Line 52. Irrigation mapping through optical remote sensing is a rapidly evolving topic… maybe more recent works could be cited;

Response: Recent works related optical remote sensing would be cited.

Comment: Lines 59-78. The end of the Introduction generally presents the purpose of the study, but in this case this part is too long and detailed, more than what is required in the introduction section. Also, I believe that it could be mentioned the use of MODIS data as a source of satellite LST estimates;

Response: The last paragraph of the introduction will be revised.

Comment: Line 81. The wflow_sbm should be briefly introduced earlier;

Response: "wflow_sbm" will be changed to "a hydrological model".

Comment: Line 118. You may add a reference here;

Response: Reference will be added.

Comment: Lines 121 and 123. I believe the reference should be moved to the end of each sentence;

Response: The reference will be moved to the end of each sentence.

Comment: Lines 138-141. I believe this is not needed. Please refer to the specific comment about this part;

Response: We will review your suggestions and reconsider the relevance of this section to the overall focus of the manuscript.

Comment: Line 150. The meaning of the terms of Eq (1) should be explained here (net radiation, sensible heat flux, etc.), not later in the text as in the current form;

Response: The explanations of terms of Rn, LE, H, and G in Eq (1) will be explained earlier.

Comment: Figure 2 is not recalled in the main text;

Response: Figure 2 will be recalled in Section 2.3.

Comment: Line 181. The passage from Eq (12) to Eq (13) is not straightforward. It seems you are expressing H according to the bulk transfer equation (Monteith, 1973) and equalizing to Eq (12) to derive LST, if I am not wrong. However, this should be specified. Also, ρa and cp are not defined.

Response: Thank you for pointing that out. Additional information will be provided before Eq (13). ρa and cp will be defined.

Comment: Lines 198-202. You are actually using remote sensing observations to derive the modeled LST to be used as a baseline. Can irrigation effects be present in such observations (e.g., lower albedo)?

Response: While we acknowledge that irrigation effects may influence the observations, the attribution of latent heat flux derived from water balance model plays a more significant role in our case. To provide some perspective, the difference in albedo between the assumed irrigated and non-irrigated pixels results in a small temperature change. Throughout the growing season, this average difference on albedo is 0.00172, which has a weak effect on the Land Surface Temperature (LST), contributing to a change of approximately 0.0116 K. This effect will be mentioned in the revised version.

Comment: Line 218. Sentence 2 sounds as a bit redundant at this point;

Response: The redundant sentence in Line 218 will be removed.

Comment: Lines 221-222. Please quantify the magnitude of data gaps due to cloud coverage (% rate);

Response: We will calculate cloud cover percentage of MODIS LST products.

Comment: Line 225. Please note that the methodology proposed by Dari et al. (2021) has been implemented with vegetation indices also (https://www.mdpi.com/2073-4441/16/5/644);
Response: Thank you for providing the work by Dari et al. (2021). We will discuss how it relates and complements our current work.

Comment: Line 227-230. If spatiotemporal features have been considered, why not applying the methodology to satellite LST directly? The authors should stress more the rationale of using a baseline approach

Response: The spatial features identified between 227 and 230 are based on differences in land surface temperature (LST). The reasoning behind the chosen method is that, in humid areas, the signals from rainfed and irrigated land often overlap, making it difficult to perform continuous classification for irrigation mapping at the catchment scale. By combining LST observations with water balance data from hydrological modeling, this method helps to eliminate the influence of primary evapotranspiration driven by precipitation. As a result, the remaining temperature differences reflect evapotranspiration specifically related to irrigation. This information obtained derived from LST differences improves the accuracy of identifying irrigated areas compared to using LST and evapotranspiration ET observations alone. We will elaborate more about the rationale of using the approach in the introduction section.

Comment: Section 2.4.2. One may argue that it is a Landsat-based irrigation mapping method;

Response: We note that Landsat and land surface temperature derived from hydrological model are both used in the manual labeling. Thus, information may influence evaluation of the classifier evaluation on the manually labeled dataset. However, these manually labeled datasets mainly depend on visual cues which were not included in the random forest classification that produces the classification results.

Comment: Lines 244-249. In this way, uncertainties associated to Landsat and land cover data are embedded in the irrigation maps produced. Have they been assessed/quantified somehow?

Response: Thank you for your observation. We acknowledge that uncertainties related to cover data are inherent in the production of the irrigation maps. However, the uncertainties associated with Landsat imagery embedded in the produced irrigation maps are negligible, as the random forest classifier was trained on land surface temperature differences. In the revised version, uncertainties related to land cover will be discussed.

Comment: Section 3.1. I believe this section does not add value to the paper. I suggest to move it to Appendix and enclose random forest performance instead in the main text, as it has surely impact on the irrigation maps developed;

Response: We will evaluate this section and consider moving random forest performance to one of the main sections.

Comment: Lines 298-299. Please rephrase;

Response: Lines 298-299 will be rephrased.

Comment: Lines 300-306. It sounds a bit as a discussion; -

Response: We will investigate these lines.

Comment: Lines 315-320. Yes, it is definitely a matter of spatial resolution;

Response: We agree that this is indeed a matter of spatial resolution.

Comment: Lines 320-330. This part also seems to be a discussion rather than presentation of results;

Response: We will review the section and consider reworking it to make the distinction between the presentation of results and discussion clearer in future revisions.

Comment: Lines 344-345. This is interesting. I also appreciate the related discussion later on. To fully understand if less rainfall actually means lower water availability for irrigation is reasonable in this case, one should have more information on the irrigation infrastructure (i.e., source of irrigation water, presence of reservoir, etc.);

Response: Thank you for your suggestions. We agree that having more detailed information on the irrigation infrastructure, such as the sources of irrigation water or the presence of reservoirs, would provide complete insights. In future studies, this data could indeed offer a more comprehensive understanding of how reduced rainfall impacts water availability for irrigation. We appreciate your input and will consider this for upcoming work.

Comment: Figure 9. Can crop rotation explain the variability found in the irrigation frequency?

Response: Thank you for your suggestions. We will try to investigate if crop rotation had influence on the irrigated area for the year 2010 to 2019.

Comment: Lines 351-354. This sounds again as discussion;

Response: These lines will be evaluated.

Comment: Lines 393-395. This is a valuable result.

Response: Thank you for your feedback.

Comment: Line 423. The study of Deines et al. (2019) is focused on the High Plains Aquifer, not on the Ebro basin;

Response: "the Ebro basin" will be changed to "High Plains Aquifer".

Comment: Lines 438. 10 years? N=10 is not clear;

Response: "N=10" will be changed to "over a simulation period of ten years".

Comment: Lines 445. This is a known issue, corroborated by outcomes of several papers. Maybe some work could be cited. Also, I would say "irrigation maps" rather than "irrigation products"

Response: Outcomes of several papers will be added and "irrigation products" will be replaced with "irrigation maps".