The authors wish thank the editor for following up on this manuscript and accepting our slight delay for the author comments since the main author (Pierre Tiengou) was defending his PhD last week.
They also thank the three reviewers for their very relevant suggestions for this article, which were all taken into account and included in the revised version.

The main changes regard the introduction, to better cover the existing state-of-the-art and state the scientific questions addressed. This also led to a slight change of the title to make the purpose of the article clearer for the reader.
Another major change was the evaluation of surface soil moisture (Fig 6) which was not included in the first version of the article but provides additional insights to connect the evaluation of precipitation and evapotranspiration, and clarify the impacts of irrigation obtained in the simulations.

Moreover, several appendices were added to the manuscript to address questions and concerns that had been looked into but not included in the initial article. These will provide more information on the assumptions and methods of this study, and help the most interested readers to gain a better understanding of the results presented in the article.
Explanations are given for the preliminary offline parameter tuning of the irrigation scheme, and the spin-up period considered for the simulations.
Additional figures are also provided to look into the seasonal biases compared with reference products used, changes in surface soil moisture in the model, the impact of snowfall and snow melt, and assess impacts on atmospheric variables in other seasons since only summer was shown in Fig. 7.

The authors believe that the robustness of the analysis and the clarity of the results are enhanced thanks to these changes in the revised version, providing a clearer selling point and additional information for readers looking for an in-depth understanding of the methods used to obtain the results.

Although they are available on the Copernicus interactive discussion section, the specific responses to each of the reviewers comments have been included below.

Reviewer 1

We would like to thank the reviewer for this thorough review of our article. The precisions demanded and complements suggested greatly helped us to improve the introduction, and should make the article more relevant, particularly for readers who want to have an in-depth understanding of the methodology and simulation experiments used.

In the following, **reviewer comments are shown in bold** and our answers in normal text, with *quotes from the revised version in italics*.

Comment

1. **Due to the large regional variability in climate and irrigation effects, consider explicitly naming the Iberian Peninsula in the title. Besides, the term "physics" in the current title is confusing. I suggest authors could use "Earth System Model" or "coupled land-atmosphere model" instead.**

The phrasing of the title was also brought forward by reviewer 2 and changing it should indeed make the article more relevant to the readers. We propose a new title for the revised article:

Regional impacts of irrigation on the atmospheric and terrestrial water cycle of the Iberian Peninsula in a climate model.

1. **The introduction needs updated references, especially in the first four paragraphs, which currently cite only one paper from the last five years.**
2. **Line 33: Please update the reference from CMIP5 to CMIP6.**

Thank you for these remarks. It is true that the literature review from which this study originated had not been updated enough when writing the first draft of the paper. In the revised version of the article, more recent references will complement the foundational work presented in the first version, including studies that used CMIP6 simulations.

1. **Lines 48-53: This paragraph emphasizes that "irrigation can create strong spatial heterogeneities in SM". However, the subsequent content focuses on effects of irrigation on other climatic variables rather than soil moisture (SM), which seems unrelated to aforementioned statement. The authors should clarify or connect these points.**

The mention of "SM heterogeneity" here was indeed an imperfect choice, especially as SM itself was not studied in the first version of the manuscript. This will be discussed more specifically on point n°10 but a brief explanation can be given here: although SM is directly impacted by irrigation, most of the additional water input in our simulations is evaporated very quickly, which is why the focus was quickly shifted to changes in ET and the consequences on other coupling variables. Efforts will be made to make this clearer, including comparisons of SM in the two simulations.

Back to this point, this sentence was an attempt to make a link between irrigation and the literature on SM-precipitation feedback loops (which is not specifically focused on irrigation) but will definitely benefit from rephrasing, to emphasize that irrigation artificially increases SM in irrigated areas. The following formulation should be clearer and enable more relevant links with the rest of the introduction (which will also undergo multiple other changes):

A great source of complexity in land-atmosphere processes comes from the spatial heterogeneity of SM, which may be derived from the diversity of vegetation, soil types, orographic features, and anthropogenic processes. In particular, in semiarid climates, artificial water inputs in irrigated fields create strong contrasts with the surrounding environment. Observational studies have shown that it increases the latent heat flux and decreases the sensible heat flux in irrigated areas.

1. **The authors cited so many studies regarding regional and global climatic impacts of irrigation, but didn't clearly summarize their limitations, or explained how the authors' study addresses them. For example, in Lines 55-68, the authors summarized that previous studies were limited to "short simulations over a limited domain", yet this study also used only a short (13 model-year) simulation over a limited domain (Iberian Peninsula). Similarly, in Lines 70-75, global-scale studies were mentioned without clarifying their shortcomings or relevance to this study. The Introduction should be reorganized to improve logical flow and to highlight the novelty and necessity of this research.**

Thank you for bringing this to our attention, it would be a shame if this aspect was not clear. The introduction will be partly restructured to better reflect the literature novelties. I will not paste all the new text here but recall the reasons why we consider this study relevant.

One the one hand, compared to kilometer-scale modelling studies, we aim to study impacts of irrigation over a decadal timescale, using a water-conservative modelling of irrigation that enables a study of the full water cycle (including withdrawals in rivers and groundwater, and associated impacts on river discharge). This is not the case of the existing WRF or MesoNH studies on the topic, which often focus on specific campaigns or on a few months at most with idealized representation of irrigation.

On the other hand, compared to global climate studies, it aims to provide an in-depth assessment of irrigation's impact over a region (the Iberian Peninsula) which has rarely been looked into despite its semi-arid climate and high reliance on irrigation. The regional setup enables a higher resolution over the region than in global simulations, allowing a more detailed analysis of the behaviour of our irrigation parametrization (which has only been used at the global scale so far, with coarser resolutions like in CMIP setups), including the study of regional specificities and multiple land-atmosphere coupling variables (precipitation, latent and sensible heat fluxes, 2-m temperature, atmospheric moisture convergence, CAPE, LCL and ABL height). We also see this study (the first of its kind with this new regional climate model) as a first step towards a comparison of the model to measurements of the LIAISE campaign, that will help understand atmospheric impacts and possible feedbacks of irrigation, and evaluate the IPSL-CM in irrigated areas.

I hope all these aspects will be better reflected in the revised version of the introduction.

1. **Line 80: The stated scientific question "which regional impacts of irrigation on surface-atmosphere couplings and the water cycle can be represented by a climate model." does not align with the focus on that "regional climatic impacts of irrigation" indicated by the title and discussion.**

Thank you for this remark, this was also brought forward by reviewer 3, and is the result of a confusing choice of words. The revised article will use the following formulation to specify the focus:

This study investigates the regional impacts of irrigation on the atmospheric and terrestrial water cycle of the Iberian Peninsula over a decadal timescale and analyses effects on land-atmosphere coupling variables.

1. **Parameter choices. The target SM is set to 60% of the SM at field capacity when calculating the moisture deficit. This proportion significantly affects the results, as different irrigation conditions could alter their climate response. Why was 60% chosen, and how would other values change the results?**

   Besides, the assumption "Only 90 % of the volume can be withdrawn from each reservoir to maintain a minimum environmental flow" in Line 151 requires justification. The authors should perform sensitivity tests for these parameters or add uncertainty discussion.

The choice of the target soil moisture parameter was also pointed out by reviewer 2 and must indeed be explained. It stems from the discussion of the paper in which the irrigation scheme was first presented and evaluated at the global scale (Arboleda et al. 2024), which explored different values of this parameter (called β). The default value of 0.9 (90%) was identified as a good option on average at the global scale but it was found that using different values depending on the regions considered would be more appropriate, as illustrated by this quote: "The results roughly show at least two classes for β, with the first with values of 1.2 and 1.4 (for instance, China and north India) and the second with values of 0.6. Using at least two β values is not enough to reduce the irrigation bias at a global scale, but it has an important effect on the spatial distribution of the irrigation bias in Southern Asia, the region with the largest paddy rice area. These results suggest that the β parameter should have at least two values, namely 1.3 in areas with paddy rice and 0.6 in the rest of the irrigated areas."

Based on this discussion, preliminary tests were conducted over the Iberian Peninsula in offline simulations with the ORCHIDEE LSM before the study presented in our article. A figure will be added as an appendix to the revised article (see supplement to this comment), showing three simulations: without irrigation, with the default target (β=0.9) and with a reduced target (β=0.6). These tests showed that the reduced target avoids excessive river reservoir depletion, and leads to a more reasonable seasonal cycle of simulated irrigation, justifying the use of the target used in this study (β = 0.6, meaning that the SM target is 60% of field capacity SM).

In the revised version of the article, more details will be added in the presentation of the ORCHIDEE LSM (Section 2.1.2) to explain this choice and point to the dedicated appendix.

In the default version of the global model, this target is set to 90 % of the SM at field capacity, but this parameter was calibrated on global average to reflect a wide variety of irrigation practices, including flooding in rice paddies. In the Iberian Peninsula, the irrigation methods are less water-intensive, and after calibrating the routing and irrigation schemes, the target value was adjusted to 60 % of the SM at field capacity (see Appendix A). This value had already been identified in Arboleda-Obando et al. (2024) as a more suitable value for irrigated areas with other methods than paddy irrigation, confirming its relevance for the study area.

To answer your question, a higher value of the target would likely increase irrigated volumes in the region where water is still available in the rivers, but not in regions where irrigation is already limited by the available water supply in the groundwater reservoir. Moreover, river discharge in summer (and possibly spring) would be excessively lowered, leading to a depletion that would eventually cap the increase in irrigation.

Regarding the limitation on withdrawals at 90% of the available water in the reservoirs to maintain a minimal environmental flow, this is the default value identified for the irrigation scheme in Arboleda et al. (2024): "To prevent the complete depletion of these reservoirs, which all feed streamflow and support aquatic ecosystems, we mimic an environmental flow regulation by reducing the available volume, owing to a user-defined parameter aj, to between 0 and 1. It is set here at 0.9 for all three reservoirs, so as to keep at least 10% of the available water at each time step." In Arboleda et al. (2024), a sensitivity analysis was conducted which showed that the choice of the beta parameter (discussed above) largely dominated irrigation withdrawals, whereas the limitation for environmental flow had a very limited influence. Therefore, for our study, the preliminary sensitivity analysis focused on the soil moisture target, and the default value of this parameter was used for the environmental flow limitation. In the revised version, a comment on this aspect will be added to make this choice clearer to the reader.

This parameter value was selected after a sensitivity analysis in Arboleda-Obando et al. (2024) and found to have a much lower impact on withdrawals than the target SM parameter, so it was not tuned again for this study.

1. **Line 160: When first referring to the Ebro Valley, please mark this region on Figure or a related map.**

In addition, basin boundaries should be included in all relevant figures.

A reference to Figure 2 will be added to facilitate identification of the Ebro valley.

Figure 2 will also include river basin boundaries.

1. **Line 170: It is unclear whether a 3-year spin-up (or warm-up) is sufficient for soil moisture and other variables to "reach a satisfactory equilibrium". The authors should provide evidence to prove it.**

We understand this concern over the spin up duration to ensure the relevance of the results, although we did not think it was essential for the reader in the initial draft of the article. The revised version of the article will include details and a specific figure in the Appendix (see supplement to this comment) presenting annual means of several variables on average over the peninsula (precipitation, total runoff, ET, total soil moisture in the 2-m soil column, ET, routing reservoirs, irrigation, mean LAI) that include the spinup period to show that the state reached after three years was acceptable.

The simulations started after a 3-year spin-up run to enable the stabilization of the vegetation and hydrological variables, in particular irrigation and ET. The ORCHIDEE model starts with a saturated soil and no vegetation and therefore the first year of the spin-up shows no irrigation on average over the Iberian Peninsula, and a very small mean value of LAI (Fig. B1g-h). After three years, variations of total runoff, ET, total soil moisture in the ORCHIDEE 2-metre soil column, and routing reservoirs are largely driven by the inter-annual variability of precipitation, as illustrated by the correlation coefficients of Tables B1 and B2. It must be noted that over the final years, the simulations exhibit a decrease in precipitation and therefore in other hydrological variables. This trend in precipitation is also present in ERA5 and mostly dictated by the lateral boundary conditions of the simulation, which is why it was not considered as an indicator of an incomplete spin-up process.

A specific reference to this appendix section will be added:

Before this 13 year simulation, 3 years were run as a spin-up to allow for the hydrological and vegetation variables to reach a satisfactory equilibrium (see Appendix B for more details).

1. **Section 2.3: Given that SM is a core variable for this study, the authors should validate it against observations or reanalysis data.**

   Likewise, river discharge validation (Line 191) should be added to Table 2.

Thank you for bringing this point forward (it was also mentioned by reviewer 2). Although we did look into soil moisture and initially chose not to include it in the article, we might have overlooked the importance of this for the reader. As explained in point n°4, SM is obviously impacted by irrigation in the real world, but in our simulations, most of the additional water input is evaporated (a sign that we are in the transition regime of Budyko where ET is limited by SM) and SM does not change a lot between the irr and no_irr simulations. In particular, although surface soil moisture is increased in *irr*, no clear relationship was found between irrigation and changes in runoff and drainage, showing that this has little effect on the surface water balance other than the increase in ET shown in Fig. 8a.

This might be a consequence of our irrigation parametrization which continually adds small amounts of water (at every ORCHIDEE time step so every 15mn) making them available for immediate ET increases. We are aware that this does not perfectly reflect actual practices as, for instance, fields in the LIAISE sites of the Ebro valley are irrigated approximately every 10 days, with much larger amounts of water. This modelling choice makes it less likely to represent changes in soil moisture, but still allows us to study irrigation's impacts on the surface energy balance. This is why our study quickly focused on changes in ET and the consequences on other land-atmosphere coupling variables, rather than SM. We agree, however, that this needs to be explained to the reader and that including more details on SM will improve the quality of the study. An evaluation of surface soil moisture using the combined CCI product will be added in Figure 6 (see supplement to this comment) and commented in Section 3.3, with a description of the data and procedure used in Section 2.3.

The evaluation of surface soil moisture uses the COMBINED product from the European Space Agency Climate Change Initiative (ESA CCI, Dorigo et al., 2017; Gruber et al., 2019), which merges multiple active and passive satellite remote sensing instruments in the microwave domain (Preimesberger et al., 2021) to provide daily SSM (in m3 m−3) at 0.25° resolution. This product contains low-quality flags (e.g. in the presence of snow, dense vegetation, or radio-frequency interference in the measurements) and an estimated uncertainty of the measurement. A screening of the data for the Iberian Peninsula over the period 2010-2022 was conducted, following the procedure described in Mizuochi et al. (2020). Data records with quality flags other than zero were excluded (representing 19% of daily data records), as well as those with an uncertainty larger than 0.6 m3 m−3 (representing only 0.1% of the remaining data records). The remaining daily values were then averaged to monthly time steps. Direct comparison of the CCI SSM product with the ORCHIDEE model is limited by the inherent differences in soil representation between ORCHIDEE and the LSM used to scale the satellite measurements (GLDAS-Noah), as analysed in Raoult et al. (2018). Therefore, a spatio-temporal normalization was applied following Mizuochi et al. (2020); Polcher et al. (2016) to compare statistically normalized values rather than absolute values.

The seasonal cycle of the normalized SSM matches the CCI product in both simulations (Fig. 6g). It is very similar in the two simulations, although in absolute value, SSM is slightly higher in summer in irr, and annual increases up to 11% are visible in the most irrigated grid cells (Fig. E1a-b). Compared to the increase in ET, this indicates that the water added by irrigation is mostly evaporated, as will be further illustrated in Section 3.5 and Fig. 8. This confirms that the transition regime described by Budyko (1961) is dominant in the region. Spatial biases in normalized SSM compared the the CCI product are very similar in the two simulations and match the precipitation underestimation in the Ebro valley, Guadalquivir river mouth,and overestimation in the Pyrenees, but show a contrasting pattern in the West compared to precipitation and ET, due to a positive bias in summer (Fig. D3).

An appendix figure will also be included (see supplement to this comment), showing the mean seasonal cycle of SM and the difference in absolute values between the two simulations. This will show the impact of irrigation (up to +11% on annual mean for the most irrigated grid cells) and hopefully make it clearer why the analysis focuses more on ET and other coupling variables.

As for your second point, the GRDC product was initially set aside because we only included gridded datasets in Table 1, but we agree that adding it to the Table will indeed give a better overview of the reference data used. It will be done in the revised version of the article.

1. **Figure 7: Use a different color bar for variables unrelated to the water cycle (e.g., temperature), so they are clearly distinguished from the water-related variables.**

Figure 7 will be updated, keeping the green-brown colormap for evaporative fraction, precipitation and moisture convergence only (see supplement to this comment).

1. **Section 3.5: Although the analysis focuses on summer, the authors should at least briefly present results for other seasons or include the relevant figures in the appendix.**

Note: I assumed you were referring to section 3.4 and Fig.7 (since Section 3.5 presents results at the annual scale).

We understand that seeing only results in JJA for this section raises the question of the other seasons, however we did not consider them to bring much more information on the impacts of irrigation and chose not to include them in the manuscript for the sake of conciseness. In the revised version, a comment will be added after the presentation of the results in JJA to provide some additional insight, and additional figures analog to Fig. 7 will be added in the appendix.

I have included them in the supplement to this comment, note that a new scale (common for all 3 seasons) had to be used for the colorbars, because using the same scales as in Fig. 7 (JJA) did not lead to anything visually interesting.

In other seasons (see Appendix G for equivalent figures), almost no significant impacts are visible in winter (DJF) since irrigation is very low and no clear pattern emerges in other variables. In spring (MAM) and autumn (SON), the impacts resemble those of JJA with lower values for all variables and less statistical significance over the domain, and a greater amplitude in SON than MAM. Precipitation increases in the Pyrenees are also present in these seasons, but the area where changes are significant is reduced, and no clear signal is found in the southern rim of the Ebro Valley. The only difference in spatial pattern is found for CAPE in the spring, with increases at the centre of the Peninsula rather than the mountainous areas, although the amplitude remains almost ten times lower than in summer.

Reviewer 2

We would like to thank Xabier Pedruzo Bagazgoitia for this encouraging assessment of our work and for the relevant suggestions and discussion points raised. The remarks were very helpful in making the article more suitable for an external reader, by lifting ambiguities and avoiding shortcuts in the presentation of our objectives, methods and results.

In the following, reviewer comments are shown in **bold** and our answers in normal text, with quotes from the revised version in *italics*.

- Some reference to the actual region studied should be mentioned in the title. Either South-Western Europe, Iberian Peninsula, or similar. Also, while I understand what is meant by 'Earth System Model physics', it is a bit vague of a term for the title. Please rephrase that too.

Thanks for the suggestion, this was also brought forward by reviewer 1 and will help make the article more relevant to the readers. We propose a new title for the revised article:

Regional impacts of irrigation on the atmospheric and terrestrial water cycle of the Iberian Peninsula in a climate model.

- L54: This paragraph presents the state of the art on regional modelling studies to study irrigation and atmospheric impacts. There, the LIAISE initiative, with a special focus over the Ebro basin, is mentioned but little reference is made to the studies that have been produced so far within the project. They appear vaguely in the discussion, but I believe that some of the already published studies could be present in the introduction as they shed light on the specific role of irrigation over the region of focus in this manuscript.

Thank you for bringing this to our attention, several recent papers are indeed missing from this introduction (some of which were only preprints or not available when writing the first draft) and they will be included in the revised version of the article, which is more suitable than just mentioning them in the conclusion.

In the Ebro Valley (northern Spain), the Land surface Interactions with the Atmosphere over the Iberian Semiarid Environment campaign (LIAISE Boone, 2019) highlighted strongly contrasting ABL properties between an irrigated and a rain-fed site, separated by 14 km (Boone et al., 2025). They were partly attributed to a high impact of the contrasts induced by irrigation on the development of the ABL through the morning transition (Brooke et al., 2023). Simulations without irrigation over the LIAISE study area exhibited strong limitations over irrigated areas (Jiménez et al., 2025), and near-surface conditions and vertical profiles over the area were greatly improved by representing irrigation (Lunel *et al., 2024a; Udina et al., 2024). A lower, more stable ABL was identified over irrigated and surrounding areas (Udina et al., 2024), as well as a weakening of the regional sea-breeze regime due to irrigation (Lunel et al., 2024b).*

Some other recent results of the GRAINEX campaign will also be included.

- L100: 2 or 3 sentences on the surface layer would help the reader to understand its basics without having to go to the references mentioned.

This is a good point, we were trying to remain concise in the model description but this might have been a little too concise considering the importance of the surface layer for the processes studied here. In the revised version of the article, this description will be enriched with the following description.

A surface layer description based on Monin-Obukhov similarity theory (Monin and Obukhov, 1954). Turbulent fluxes are expressed using the specific humidity and temperature gradients between the surface and the lowest atmospheric level and the aerodynamic resistance. A neutral drag coefficient is computed from the surface roughness, and the formulations of Louis (1979) and King et al. (2001) are used to account for stability conditions.

- L146 The calibration of routing and irrigation to from 90% to 60% of the SM at field capacity should be explained in more detail as currently there is almost no explanation of it.

The choice of the target soil moisture parameter was also pointed out by reviewer 1 and must indeed be explained. It stems from the discussion of the paper in which the irrigation scheme was first presented and evaluated at the global scale (Arboleda et al. 2024), which explored different values of this parameter (called β). The default value of 0.9 (90%) was identified as a good option on average at the global scale but it was found that using different values depending on the regions considered would be more appropriate, as illustrated by this quote: "The results roughly show at least two classes for β, with the first with values of 1.2 and 1.4 (for instance, China and north India) and the second with values of 0.6. Using at least two β values is not enough to reduce the irrigation bias at a global scale, but it has an important effect on the spatial distribution of the irrigation bias in Southern Asia, the region with the largest paddy rice area. These results suggest that the β parameter should have at least two values, namely 1.3 in areas with paddy rice and 0.6 in the rest of the irrigated areas."

Based on this discussion, preliminary tests were conducted over the Iberian Peninsula in offline simulations with the ORCHIDEE LSM before the study presented in our article. A figure will be added as an appendix to the revised article (see supplement to this comment), showing three simulations: without irrigation, with the default target (β=0.9) and with a reduced target (β=0.6). These tests showed that the reduced target avoids excessive river reservoir depletion, and leads to a more reasonable seasonal cycle of simulated irrigation, justifying the use of the target used in this study (β = 0.6, meaning that the SM target is 60% of field capacity SM).

In the revised version of the article, more details will be added in the presentation of the ORCHIDEE LSM (Section 2.1.2) to explain this choice and point to the dedicated appendix.

In the default version of the global model, this target is set to 90 % of the SM at field capacity, but this parameter was calibrated on global average to reflect a wide variety of irrigation practices, including flooding in rice paddies. In the Iberian Peninsula, the irrigation methods are less water-intensive, and after calibrating the routing and irrigation schemes, the target value was adjusted to 60 % of the SM at field capacity (see Appendix A). This value had already been identified in Arboleda-Obando et al. (2024) as a more suitable value for irrigated areas with other methods than paddy irrigation, confirming its relevance for the study area.

- L174 I am aware this may be out of the scope of this work, but it would have been interesting to briefly look at whether there is any impact North of the Pyrenees from the irrigation south of them.

Regarding impacts of irrigation north of the Pyrenees, the analysis is not straightforward because there is also some irrigation in southwestern France, that is simulated by the model. Without a specific moisture tracking algorithm (not available in the ICOLMDZOR LAM) impacts cannot be directly linked to irrigation from the Peninsula. The dominant low-level westerly winds suggest that these impacts would not be very significant. Moreover, the domain is centered in the Iberian Peninsula so several geographical features in France are not fully included in the domain (river catchments and Alps mountain chain for instance), or are partly in the transition zone of the LAM where the model is nudged towards lateral boundary conditions. All these reasons explain why no analysis of the impacts on the water cycle or climate were conducted outside the Peninsula.

- Table 1. A validation of Soil moisture, given its critical role in evapotranspiration and the need for irrigation, would increase confidence in the simulations.

Thank you for bringing this point forward (it was also mentioned by reviewer 1). Although we did look into soil moisture and initially chose not to include it in the article, we might have overlooked the importance of this for the reader and will change that in the revised version.

While SM is obviously impacted by irrigation in the real world, in our simulations, most of the additional water input is evaporated (a sign that we are in the transition regime of Budyko where ET is limited by SM) and SM does not change a lot between the irr and no_irr simulations. In particular, although surface soil moisture is increased in irr, no clear relationship was found between irrigation and changes in runoff and drainage, showing that this has little effect on the surface water balance other than the increase in ET shown in Fig. 8a. This might be a consequence of our irrigation parametrization which continually adds small amounts of water (at every ORCHIDEE time step so every 15mn) making them available for immediate ET increases. We are aware that this does not perfectly reflect actual practices as, for instance, fields in the LIAISE sites of the Ebro valley are irrigated approximately every 10 days, with much larger amounts of water. This modelling choice makes it less likely to represent changes in soil moisture, but still allows us to study irrigation's impacts on the surface energy balance. This is why our study quickly focused on changes in ET and the consequences on other land-atmosphere coupling variables, rather than SM. We agree, however, that this needs to be explained to the reader and that including more details on SM will improve the quality of the study. An evaluation of surface soil moisture using the combined CCI product will be added in Figure 6 (see supplement to this comment) and commented in Section 3.3, with a description of the data and procedure used in Section 2.3.

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The seasonal cycle of the normalized SSM matches the CCI product in both simulations (Fig. 6g). It is very similar in the two simulations, although in absolute value, SSM is slightly higher in summer in irr, and annual increases up to 11% are visible in the most irrigated grid cells (Fig. E1a-b). Compared to the increase in ET, this indicates that the water added by irrigation is mostly evaporated, as will be further illustrated in Section 3.5 and Fig. 8. This confirms that the transition regime described by Budyko (1961) is dominant in the region. Spatial biases in normalized SSM compared the the CCI product are very similar in the two simulations and match the precipitation underestimation in the Ebro valley, Guadalquivir river mouth, and overestimation in the Pyrenees, but show a contrasting pattern in the West compared to precipitation and ET, due to a positive bias in summer (Fig. D3).

An appendix figure will also be included (see supplement to this comment), showing the mean seasonal cycle of SM and the difference in absolute values between the two simulations. This will show the impact of irrigation (up to +11% on annual mean for the most irrigated grid cells) and hopefully make it clearer why the analysis focuses more on ET and other coupling variables.

- -L245 The ‘much better match with observations’ may be for the wrong reasons. The presence of dams is used as an explanation for discharge increases in the summer that the simulation fails to catch for Figure 5e. If dams play a role there, and acknowledging that dams may partly explain the strong overestimation of discharge in winter and spring, it is also fair to admit that over summer the irr simulations have a larger positive bias in most of the other stations than what is apparent in Fig5. In those, irr seems to agree with observations over summer but it is in fact overestimating the discharge since dams, that increased discharge at the time, are not accounted for in the simulations.

This is an interesting point and I think this question can be looked at in different ways. The use of water stored in dam reservoirs to sustain stream flow in summer is clearly visible only for two stations from the Guadalquivir basin, which have rather small average discharge (30.25 and 6.19 $m^3 s^{-1}$) and an unexpected peak in summer. I am not sure to what extent this practice is used in the other four basins, and if it significantly affects the observed discharge in other stations. This would indeed mean that the better match with observations partly relies on error compensation and that the irrigation parametrization should withdraw more water. On the positive side, it would also mean that in the cases where the irr simulation underestimates summer discharge, irrigation withdrawals are not necessarily overestimated since we know that another process is missing.

From a more practical point of view, since our model only represents one anthropic process out of the two discussed here (dams and irrigation, which are connected to each other in the real world), it still seems a good choice to aim for a match with observations. Things would be different if there was an explicit dam representation alongside the irrigation parametrization, providing a representation of discharge increases in summer by river dams (as done with the “ecological flow” in Baratgin et al. (2024) or with the “minimum environmental flow” in Sadki et al. (2023)). In that case, the irrigation scheme could be tuned using discharge observations with more confidence than what is done at the moment.

In the revised article, the discussion of river dams will be modified to mention this aspect.

Explicit dam representation could generally improve the fidelity of the model by limiting the winter and spring overestimation of river discharge in anthropized areas (since water would be stored in the dam reservoirs during this season instead of flowing in the rivers), and by accounting for environmental flow regulations that increase discharge in summer using water stored in dam reservoirs (Sadki et al., 2023). This would disentangle the impacts of irrigation on river discharge from those of dam operation, and likely require a new parameter tuning of the irrigation scheme.

- L263. What about the role of modelled snow cover and snow melt in river discharge? There is not a single mention to this process in the whole manuscript.

Thank you for this reminder that snow processes are indeed very relevant for the analysis of river discharge. We had briefly looked into it during the analysis but did not include anything in the article, and now realize that this might seem incomplete for the reader, and that more details help understand the discharge biases near the Pyrenees. In the revised version of the article, a brief description of snow processes in ORCHIDEE will be included in the Methods (Section 2.1.2)

Snow is represented with three layers of varying density and thermal conductivity (T. Wang et al., 2013). The energy budget of the snowpack is computed, and when a layer's simulated temperature exceeds freezing, it is automatically reset to 0°C in the snow, and the excess energy is used to melt snow. The resulting liquid water may then percolate through the snowpack or be refrozen. If it reaches the soil surface, it is integrated into the surface water budget, contributing to infiltration and runoff in the same manner as rainwater.

To discuss the impact on river discharge and the links to precipitation, a figure will be added in the appendix (see supplement to this comment) showing the average seasonal cycle of snowfall and snow melt next to rainfall and total precipitation, as well as a map of the share of snowfall in total precipitation (rainfall+snowfall). Comments on the influence of snow melt on discharge will be added in the presentation of the results, pointing to that figure.

In most cases, the model shows a slight delay and a large overestimation of river discharge compared with observations, particularly in winter and spring. Snow melt does not seem to play a large role in the delay since it occurs rather concurrently with snowfall (Fig. F1a).

It must be mentioned here that in the Pyrenees, snowfall represents up to 35% of annual precipitation in the simulation (Fig. F1) and largely contributes to river discharge in these two stations via snow melt.

The influence of this process will also be reminded in the discussion among other sources of uncertainty.

Nevertheless, biases induced by precipitation and the melting of snow in mountainous regions are very likely to remain major drivers of discharge biases, largely independent of irrigation or dam representation.

- L278: Fig 6 shows a strong overestimation of precipitation over the peninsula North-West, and a strong underestimation of ET at the same time. What is the explanation for this? The validation of soil moisture mentioned before may be helpful here.

In the northwest of the peninsula, precipitation is overestimated and ET underestimated on annual scales, but this is the consequence of seasonal disparities. The excess in precipitation occurs in winter and spring and dominates the annual mean, but in summer and autumn, precipitation is not overestimated, and is even underestimated in several grid cells. ET is not very biased in winter and spring in this region (ET-SM coupling is in a wet regime), but more largely underestimated in summer and autumn, which is not inconsistent with precipitation. However, comparisons of normalized soil moisture with the CCI product do not bring much more insight on this region since biases are small and do not exhibit important seasonal variations.

In the revised article, an appendix figure presenting the biases of the no_irr simulation to the reference products for P, ET, SSM in the 4 seasons will be included to illustrate this (see supplement to this comment). A comment on Figure 6 will also be added to discuss this region and point to the appendix figures.

The biases in ET and precipitation are not consistent in the north-west of the Peninsula, which is explained by seasonal differences. Indeed, the overestimation of precipitation mostly occurs in winter and spring and does not strongly affect ET since soils are already wet in this season, whereas the underestimation of ET is dominated by summer (see appendix Fig. D1 and D2).

-L337: I'd still mention the underestimation (overestimation) in thalwegs (hillslopes)

The sentence will be modified in the revised version of the article, to mention this aspect, using the following formulation:

It first shows that the ORCHIDEE irrigation scheme simulates realistic values from April to September in areas where surface water withdrawals are most important, such as the Ebro Valley, although it is partly due to a compensation between an overestimation of irrigation in the thalwegs and an underestimation in the hillslopes.

Reviewer 3

We would like to thank the reviewer for this very positive assessment of our work. All the comments will be taken into account in the revised version of the article.

In the following, reviewer comments are shown in bold and our answers in normal text, with quotes from the revised version in italics.

Specific comments

L78-85: The paragraph is confusing. I wonder if in this sentence "this study aims to understand which regional impacts of irrigation on surface-atmosphere couplings and the water cycle can be represented by a climate model." To me the impacts that are represented are the ones that are parameterised. Would it be better to say that "the study investigates the impacts of irrigation on surface-atmosphere coupling using a climate model". But perhaps I am missing the meaning. Either way please look and revise to make the motivation and the unique selling point clear.

Thank you for this suggestion, a similar remark was made by reviewer 1, and the revised version will have a simpler formulation to clarify the objective of this study.

This study investigates the regional impacts of irrigation on the on land-atmosphere coupling variables and the water cycle of the Iberian Peninsula over a decadal timescale.

To clarify what was initially meant in response to your comment, the model has a parametrization for irrigation that adds water at the surface in the land surface model. This impact is indeed directly parametrized, but indirect impacts of irrigation in the model may arise from the interaction with the other parametrizations (infiltration in the soil, surface fluxes, boundary layer, convection and precipitation…). This study aims at quantifying how the parametrization affects these variables and processes, and if interactions with the atmosphere create secondary impacts or feedbacks.

Fig 7: The figure would benefit from more appropriate selection of colourbars. For example, blue-red (rather than green brown) for temperature difference would be more intuitive.

Figure 7 was updated, keeping the green-brown colormap for evaporative fraction, precipitation and moisture convergence only (see attached supplement).

L455: Missing info for Budyko 1956 reference.

The 1956 citation will be replaced by the 1961 English version (which has more information and a doi).