Response to Reviewers' observations on manuscript "Validation of a new global irrigation scheme in the land surface model ORCHIDEE v2.2"

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Reply to anonymous reviewer 1

Review comments on 'Validation of a new global irrigation scheme in the land surface model ORCHIDEE v2.2" by Arboleda-Obando et al.

The authors present a new global irrigation scheme inside the ORCHIDEE land surface model. The irrigation model calculates the irrigation water demand based on soil moisture deficit against their target soil moisture after irrigation, and the irrigation rate is constrained by the available water supply from three major reservoirs (stream, overland, and groundwater). Irrigation model parameter beta that controls the irrigation target soil moisture was tuned to match some existing global irrigation estimates. Global-scale irrigation estimate from the model is comparable with other existing estimates, e.g., FAO's AQUASTAT and Sacks et al. (2009), but its regional estimates show noticeable differences, particularly, underestimated irrigation in some irrigation hotspots in China, India and the US is notable. However, with the irrigation on, negative biases in ET over irrigated areas improved.

The new irrigation scheme shares common features with other some existing irrigation schemes that adopt similar concepts of adding irrigation water to soil up to a (tuneable) target value during the prescribed cropping seasons. But this work convincingly shows the importance of including irrigation scheme in global land surface (or hydrological) modelling to correctly reproduce evapotranspiration, which has important implications to relevant land surface processes and land-atmosphere interaction. Moreover, thanks to the explicit representations of irrigation water source, the authors argue the possible role of irrigation sourced from non-renewable groundwater storage in explaining the gap between modelled TWS and GRACE-derived TWS. The manuscript is well written and the topic is within the interests of EGUsphere's readers. I recommend that the manuscript is considered for publication in EGUsphere once some technical concerns listed in the following section are addressed.

We thank the anonymous reviewers for the time he spent to read and comment our paper. Below, we provide a point-by-point response to these comments. Sentences from the original submitted manuscript are presented in italic, while the proposition to respond to the observations are presented in bold. Observations from reviewer 1 are numbered from 1 to 20.

1. According to the description of irrigation scheme (Section 2.2), the soil moisture deific D is set to zero when crops and grasses are below a certain threshold value, LAI_lim. Although this might be a practical choice for the latter part of a crop growth cycle (maturity stage to harvest), irrigations from sowing the emergence stages, would be missed. Given that most crops require sufficient irrigation in the early stage of their growth cycle, this would lead to an underestimated irrigation overall.

We agree that the LAI threshold value could prevent irrigation during the early stages of the plant growth. As stated in the paper, we do so to prevent irrigation when there is no plant development, for example during boreal winter in the northern hemisphere. But we agree that it overlook irrigation during emergence, and could lead to underestimation. We propose to add the following sentence (in bold) to the paper, line 147 of the original submitted document:

To prevent irrigation when there is no plant development, for example during winter, we set the deficit D to zero if all crops and grasses are below a certain LAI threshold, LAIlim. ***By doing so, we overlook irrigation used to enhance germination, and tend to underestimate irrigation amounts.***

Besides, we add a sentence in the discussion, line 459, to take into account this observation and observation 3 from reviewer 1 (see response to observation 3 from reviewer 1)

2. In addition, it is stated in Section 2.2 that "*we do not separate the irrigated area into a separate soil column, i.e. the soil column includes crops (both irrigated and rainfed) and grasses…The effective irrigation (I, see below) is uniformly applied over the crops and grasses soil column.*" This implies that if a fraction (<1) of crop/grass column is irrigated to meet the target soil moisture content (beta x field capacity), the added water is spread over the whole column, leading to the soil moisture content still under the target soil moisture. This will in turn make the model add more water for irrigation until the entire column that contains fractional crops/grasses receives water up to the target soil moisture. Is this the case? The authors mention overestimated evapotranspiration as a possible result of the simplified water addition scheme, but in combination with the additional irrigation caused by the uniform spread of water to the entire crop/grass column, over-irrigation effect can be fairly significant, particularly when a crop/grass column represent sparse cropping area with a small crop fraction.

The reviewer is right. In the case that the irrigated area is much smaller than the crops and grasses soil column fraction, the added water does not necessarily lead to increase the soil moisture content over the target soil moisture. The result is continuous irrigation (if there is soil deficit and available water) during the growing season. But note that the irrigated fraction and the maximum irrigation rate per hour also control the water demand. So even if overestimation is a likely output, underestimation is also possible.

We propose to change the sentence in line 159 of the original submitted document, so this particular case is its consequences are better explained:

If *the fraction of irrigated area is* ***much*** *smaller than the fraction of* the crops and grasses fraction of soil column***,*** *irrigation will eventually be* ***spread*** *over a larger area than the actually irrigated surface.* ***This particular case (an important difference in irrigated fraction and soil column fraction)*** *could* ***likely*** *result in* ***overestimation of the amount of irrigation (mainly because the water put on the surface will not be sufficient to reach the soil moisture target, see S9). Besides, the*** *fraction of irrigation water* ***that*** *actually* ***evaporates could be larger*** *than in reality.* ***The latter*** *could lead to an overestimation of the evapotranspiration increase, especially in areas that are energy-controlled \citep{Puma2010}* ***and an overestimation of irrigation efficiency.***

Also, we propose to add a new figure in supplementary, Fig. S9. Fig. S9-b shows the irrigation bias by class of irrigated surface over crop and grass soil column. In comparison, in Fig. S9-a we show the same plot by class of irrigated fraction. While sparse irrigation in croplands could likely lead to overestimation in our model, it is not always the case. Also, underestimation in intensively irrigated areas is more important.

The new Figure S9 is shown below:

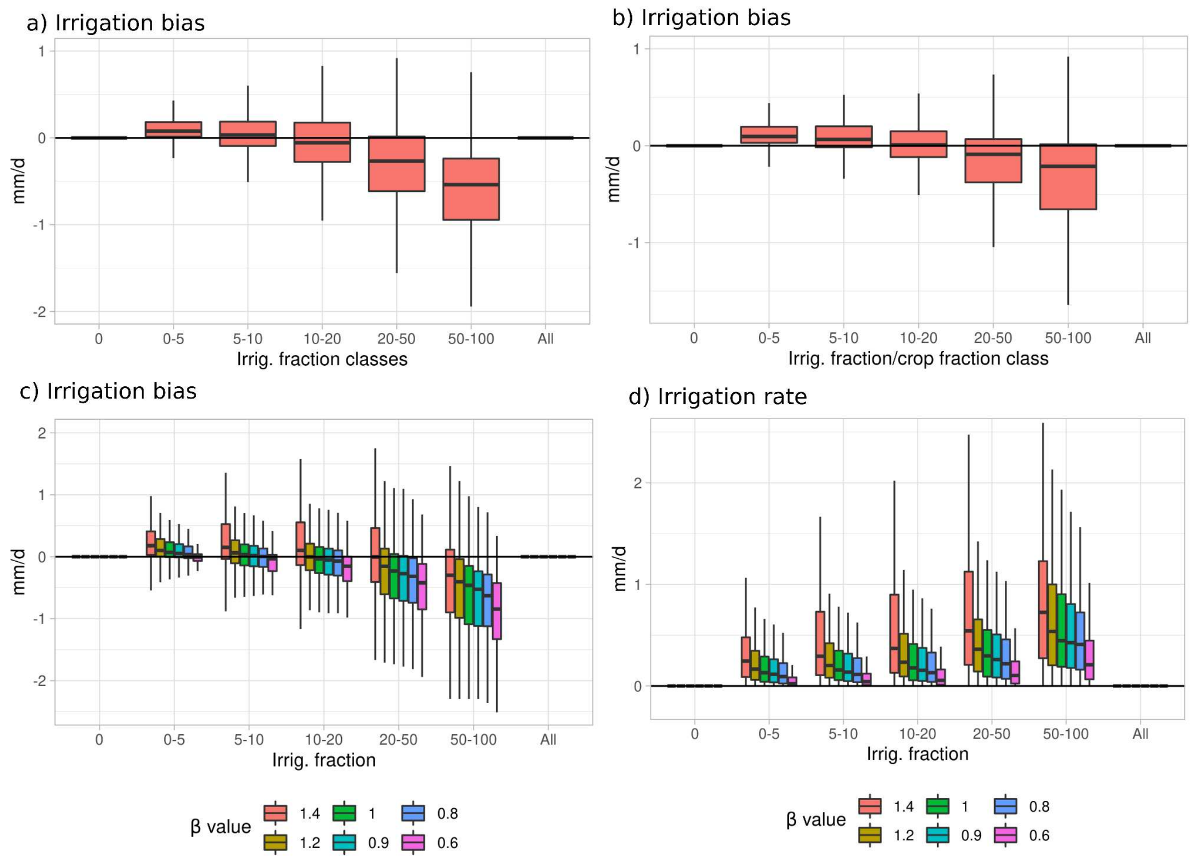

Figure S9 Use of factor analysis against irrigation bias. Irrigation rate bias against data from Sacks et al. (2009), as a function of irrigated fraction classes (a) and classes of the ratio of irrigated fraction and crops and grasses soil column fraction (b). Both plots use data from the Irr simulation for 2000. Irrigation rate bias against data from Sacks et al. (2009), as a function of irrigated fraction classes and 'beta' parameter values (c). Irrigation rate as a function of irrigated fraction classes and 'beta' parameter values (d). Both plots (c) and (d) use data from short simulations used for the tuning parameter analysis, for 2000.

3. The new irrigation scheme add irrigation water to close to soil moisture deficit, the difference between the actual soil moisture and 'beta x field capacity', but the manuscript does not provide the soil moisture value that triggers irrigation (this is a different trigger than the LAI_min). Does this mean that irrigation is triggered whenever soil moisture drops below 'beta x field capacity'? This would result in continuous irrigation over the whole duration of the cropping season (when LAI >

LAI_min), leading to unrealistic emulation of irrigation (and likely overestimation of irrigation).

The reviewer is right, the use of a ‘beta x field capacity SM’ might result in a continuous irrigation during the whole growing season (if there is enough available water), unless soil moisture becomes greater than the target due to precipitation.

To address this observation, we propose to include all the limitations of the scheme at the beginning of discussion (see response to observation 13 of the first reviewer).

We propose to add some clarifications in line 147 of the original submitted document:

where Wi and Wfc i(both in mm) are the actual and field capacity soil moisture in soil layer i, respectively, and β is a user dependent parameter that controls the target value ***with respect to field capacity (see Fig. S10 in the supplementary for information regarding soil texture and field capacity soil moisture in the root zone). When soil moisture drops below the target, irrigation is triggered.***

Besides, in order to take into account observations 1 and 3 from reviewer 1, we add a sentence in the discussion, line 459 of the original submitted document:

Finally, the conditions to trigger irrigation, although controlled by four parameters, may seem too simple in our scheme, especially compared to specialized irrigation models, ***the new irrigation scheme in LSM CLM5 (Yao et al., 2022), or the ISBA LSM (Druel et al., 2022),*** *which implement complex sets of rules to represent different irrigation strategies.* ***Some rules could change the moment when irrigation is triggered and increase the amount (for instance allowing irrigation some days before the crop emergence) or decrease the irrigation amount (for instance, preventing irrigation during maturity of the crop, or preventing continuous irrigation during more than a certain number of days).***

Finally, we propose to add a map of soil texture as used by the model, and the corresponding field capacity SM, in Fig. S10 of the supplementary, and cite it in line 147.

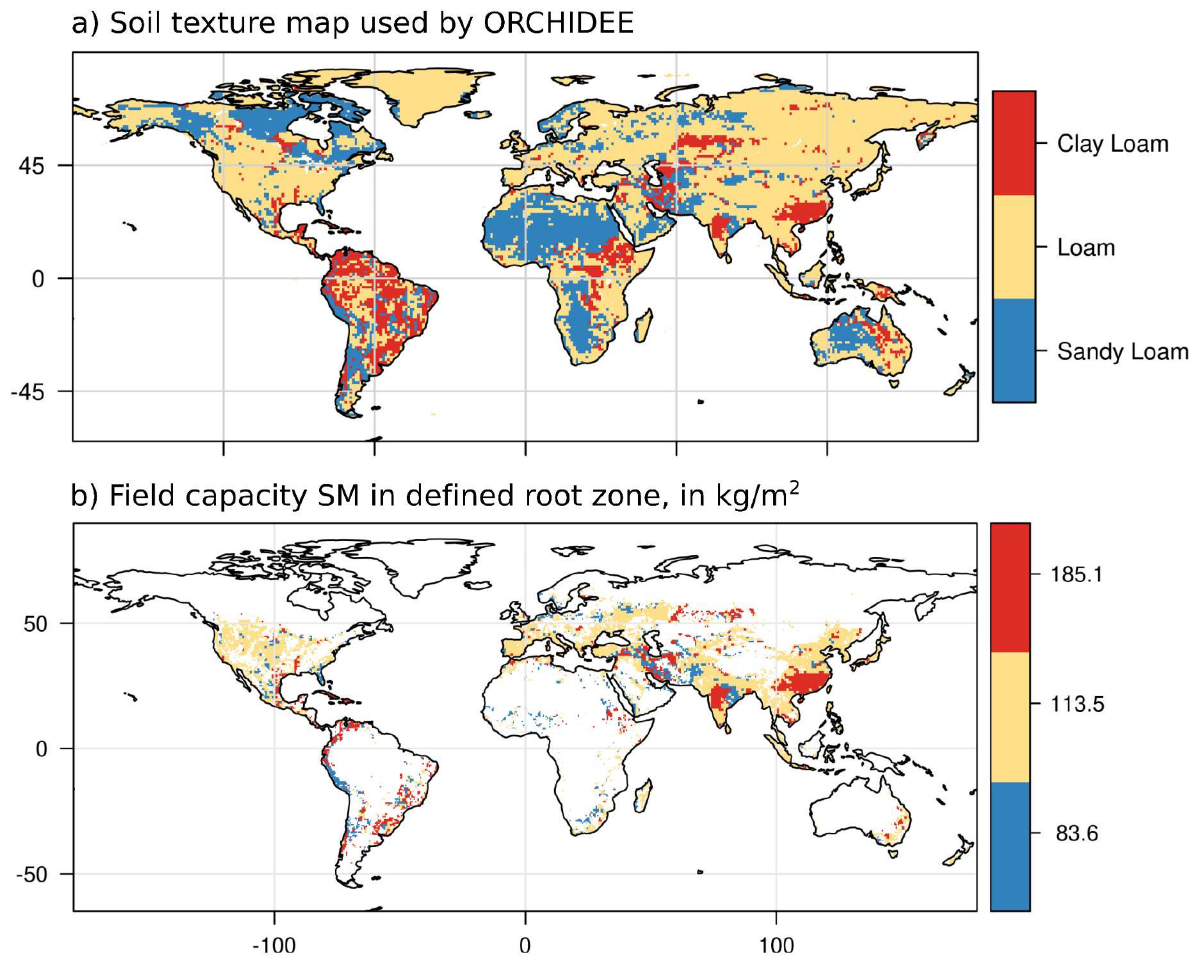

Figure S10 Soil texture map used by ORCHIDEE from Zobler (a) and field capacity soil moisture in the root zone defined according to the new irrigation scheme, in kg/m2 or mm. Both maps are at the simulation resolution (0.5º x 0.5º). White in (b) means that there is no irrigated fraction according to HID map for year 2000.

Specific Comments

4. Line 43: "…potential evapotranspiration PET, ET0…" -> (PET)?

The ETc corresponds to the crop-specific potential evapotranspiration, and is equal to $ET0 = kc \cdot PET$, where kc is a crop-type and growing stages dependent parameter, and PET is the atmospheric evaporative demand. We propose to slightly change this paragraph so it is more comprehensive:

This ET increase is estimated as the differences between crop-specific potential ***ET*** *and actual ET with no irrigation (Siebert and Döll, 2010; Mekonnen and Hoekstra, 2011; Wada and Bierkens, 2014; Chiarelli et al., 2020).* ***Following Allen et al., 1998, the crop specific potential ET is defined as*** ***$ETc = kc \cdot ET_0$****, where* ***parameter*** *kc* ***depends on*** *crop-type and growing stage,* ***and ET_0 is the reference crop ET, corresponding to the atmospheric evaporative demand****.*

5. Line 44-45: “This reference PET…parameter.” This sentence needs to be rewritten for clarity.

We propose to change this phrase, see the response 4. below.

6. Line 125: “Tree” -> Three

We propose to correct the typo in the new version of the paper.

7. Line 138: Correct “crop- grass soil column”

We propose to change the typo in the new version of the paper.

8. Line 156-158: For the reason described in the general comment section, I think ‘the fraction irrigated is greater than the crop/grass soil column’ would be less of a concern for correction irrigation simulation.

We agree, but it belongs to the checks currently made by the scheme, so we prefer to leave it in the manuscript.

9. Line 166: What is the definition of ‘renewable-groundwater reservoirs’ in this work?

It corresponds to shallow aquifers recharged by drainage at the soil bottom. We propose to add a sentence in the discussion, line 166 of the original submitted document:

In this equation, Sj [mm] is the volume storage in each routing reservoir, with index j equal 1, 2 and 3 for the stream, overland, and renewable-groundwater ***(i.e. shallow aquifers that are recharged by drainage at the soil bottom)*** *reservoirs, respectively.*

10. Line 193: “100x 100 km” -> “100 x 100 km”

We propose to change the typo in the new version of the paper

11. Line 214-217: 3.0 x 10^6, 2.5 x 10^6. Does HID include LUHv2 or separate? It appears to be assumed in two different ways?

If both datasets shared the same spatial distribution, AEI should include AAI and so, HID should include LUHv2. But as both datasets rely on different information sources, processed with different methods, inclusion of AAI in AAI is not the case, in fact it is easy to verify the important differences in the spatial distribution of both datasets.

We include a sentence to clarify these differences between both datasets:

The main difference between the HID and LUHv2 maps is that HID prescribes the area that is equipped for irrigation (AEI), while LUHv2 prescribes the area that is actually irrigated (AAI). As a result, the HID dataset has a greater irrigated surface (3.0 $10^6 km^2$ for HID, 2.5 $10^6 km^2$ for LUHv2 at global scale around 2000). It also means that AAI should be included in the AEI if the two datasets shared a similar spatial distribution. ***But this is not the case, as the two datasets rely on different information sources, processed with different methods (Oliveira 2022).***

12. Line 221: “a-priori” -> *a priroi*

We propose to change the typo (from a-priori to *a priori*) in the new version of the paper

13. Line 320-321: This justifies a close exam of threshold SM triggering irrigation and possible flaws in irrigation application to the entire crop/grass column with a fractional coverage, particularly when the fraction is small.

    It is related to the effect of beta on water amount and irrigation efficiency

    It could be related to some flaws on the irrigation scheme, but also to the representation of infiltration, or to other model flaws, like an overestimation of bare soil evaporation.

We agree with the reviewer, it is possible that part of the irrigation bias is probably due to flaws in the scheme, to the effect of the tuned ‘beta’ value, and to other flaws in the model, for instance infiltration.

We propose to include a classification of the type of limitations that could impact our estimates of irrigation amount or the effect of irrigation on other variables, at the beginning of the discussion, line 445 of the original manuscript:

In this study, we implemented a new global irrigation scheme inside the ORCHIDEE land surface model based on previous work from Yin et al. (2020) in China. ***While we found a reduction in some modelling biases when irrigation is activated, we also identified at least four types of limitations in our modelling framework that can affect the estimates of irrigation or the effects of irrigation on other variables inside the land surface model:***

1. ***The irrigation scheme exhibits some shortcomings that may bias the estimated irrigation amount: the use of a single irrigation technique; simplified rules to trigger irrigation and allocate the available water; the joint representation of rainfed and irrigated crops within the same soil column; the non-representation of conveyance losses, although losses due to return flows are represented.***
2. ***The parameter tuning is overly simplistic. As a first step, we considered globally uniform parameters, which is overly simplistic, although spatially distributed values would allow us to better describe the local features of irrigation systems, as shown by the spatial variations in optimized β map, and the dependence of the local irrigation bias on the fraction of paddy rice.***
3. ***We also use a single meteorological forcing dataset and a single year to characterize observed irrigation values. This contributes to biasing the parameter adjustment process by taking uncertain data (meteorological forcing and reference irrigation) as certain.***
4. ***The ORCHIDEE model exhibits many uncertainties that are not related to the irrigation scheme, but ultimately impact the irrigation withdrawals and efficiency (defined here as the ratio of additional ET due to irrigation to water withdrawal) and the temporal dynamics of irrigation. One particular uncertainty comes from the overestimation of bare soil evaporation (Cheruy et al., 2022),***

> ***that we presently try to correct in ORCHIDEE. Other uncertainties result from the inherent simplifications of any model. In ORCHIDEE, they include the use of a single soil texture in each grid cell, of only two kinds of crops with simplified phenology and crop calendars, and the choices made to simulate infiltration and evaporative processes.***

Finally, we propose to add two additional plots to Fig. S9. Fig. S9-c shows the effect of changing beta on irrigation bias, by class of irrigated fraction. Fig. S9-d shows the effect of changing beta on the irrigation rate, by class of irrigated fraction (see Fig. S9 in response to observation 2 reviewer 1). Both show that higher beta increases the irrigation rate, but the effect on irrigation bias is different according to the class.

We propose to include the next sentence in bold in line 328 of the original manuscript to cite the new Figure:

When comparing the irrigation water amount at global scale (in km3 for the year 325 2000, Fig. 3-a) we observe that a value of 1.2 maximizes the irrigation and minimizes the irrigation bias. When we assess the distribution of bias using grid-cell values (in mm/d, Fig. 3-b) we observe that for β equal to 0.8, 0.9, or 1, the bias distribution is centered around 0, while it starts to move up for values 1.2 and 1.4. ***This behavior can be slightly different depending on the irrigated fraction (see Fig S9)****.*

14. Line 341-342: Irr simulation result, 2452.5 km^3/y appears to be closer to the higher end of 3755-2465? Also results in Figure 4 indicate that the difference in global irrigation may not reflect that large continental/country scale difference.

We do not fully understand, but our result for total irrigation withdrawal, 2452, is in the low part of the range 3755-2465 km^3/y. We agree that the differences in global irrigation (around -10% compared to the irrigation amount of ~2700 km^3/y from AQUASTAT) does not necessarily reflect large differences at continental/country scale because the over and underestimations largely balance each other. That is why we include an evaluation at country level, and at grid-cell level.

To make this point clearer, especially for the differences within a country, we propose to add a sentence in the discussion, line 347 of the original submitted document, and cite Fig S11 (See response to observation 7 reviewer 2 for the Figure):

The other regions present in general an overestimation of irrigation withdrawals, which is especially important in some small areas in Africa, in Eastern Europe and north to the Caspian sea, and in some areas of central Asia. ***Finally, we note that within a country, it is possible to observe areas with positive and negative bias, for instance in the USA or India. This could also be partially explained by the use of globally uniform values, as there could be important local differences on irrigation strategies within the same country, and it remarks the need to assess the irrigation bias at different scales (See Fig. S11).***

15. Figure 3 caption: The caption describes what authors did with the beta vs. irr, but it does not properly describe what the figures are about.

To clarify the figure's description, we propose to change the caption to the following one:

Figure 3. Calibration of β value with Sacks et al. (2009) dataset as observed value, using outputs from the short simulations. ***Bias in total irrigation volume in km³ by β value (a), boxplot of the bias of irrigation rates in gridcells in mm/d by β value (b).***

16. Line 349-350: This is not a convincing explanation because Figure 4c shows noticeable contrasts between over- and under-irrigation within a country, for example in the US and India.

We agree with the reviewer, and we included a clarification in that sense (see response to observation 14 reviewer 1)

17. Line 358-359: Again, global annual irrigation may not be the best way to quantify errors in irr when they show strong biases with different signs between continents/countries.

We agree with the reviewer, as said in response 14 and 16, and that is why we include an evaluation at different scales.

We propose to add the following two sentence after line 359 of the original submitted paper so the spatial heterogeneity of the variability is highlighted in this part of the manuscript:

*The global annual irrigation volumes (Fig. 5-a) show a large uncertainty across the simulations due to changes in the parameter values (for instance, -24.7% between Irr_NoTunned and Irr)****, but note that the change on irrigation rates at gridcell scale can have a strong spatial heterogeneity within a country (Fig. 5-c) for instance in India or the USA.*** *The parameter set used in the Irr simulation manages to increase the irrigation rate and to markedly reduce the irrigation bias when compared to the Irr_NoTuned simulation* ***at global scale, even if locally we may observe both an increase or a decrease on the irrigation rate in the same country, for instance in China (with a marked north-south difference, Fig. 5-c, Irr_NoTuned-Irr) or the Indus river basin in Pakistan and India (see Fig. 5-c, Irr_NoTuned-Irr).***

18. Line 436-437: I am not sure what the model would do with a beta > 1. Since soil moisture would be max at the (effective) porosity, if beta x field capacity > porosity, would the irrigation scheme keep adding water every time step?

The reviewer is right, if beta x field capacity > porosity, the scheme would keep adding water every time step (if water is available), but we care that beta x field capacity < porosity by choosing the beta value. We propose to show the theoretical maximum 'beta' by texture in the supplementary. The maximum 'beta' results of the ratio of saturated soil moisture to field capacity soil moisture, as shown in table S4.

We cite the new table in line 304 of the submitted manuscript

Theoretically the upper limit is infinite, but values above 1.5 may exceed the saturated soil moisture for some soil textures, the lower limit is zero ***(see Table S4)****.*

Table S4 Parameters for soil textures used in ORCHIDEE. θs is saturated soil moisture and θfc is the field capacity soil moisture, in volumetric content. β max is the theoretical maximum β value that can be used, where β = θs/θfc , so the target soil moisture does not surpass saturated conditions.

Parameter	Sandy Loam	Loam	Clay Loam
θs (m3/m3)	0.41	0.43	0.41
θfc (m3/m3)	0.1218	0.1654	0.2697
β max (-)	3.4	2.6	1.5

19. Line 445: “basedon” -> based on

We will correct this typo in the new version of the manuscript.

20. Figure 9 inset text: Reduce the font size to make the whole text visible.

We corrected a small typo in the inset text, but we do not fully understand which text is not visible in this figure. We propose to discuss with the editor to correct this observation.

Final note:

We also corrected the style of some sentences, so they were more understandable, and some orthographic and grammar errors.

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