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 2

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

The authors improved an irrigation scheme in LSM ORCHIDEE and evaluated the improvement. Using reported irrigation statistics for the year 2000, they globally-uniformly tuned key parameters of the irrigation scheme used in ORCHIDEE to achieve a better balance in estimating the global total irrigation volume and spatially minimizing irrigation bias. It is also investigated how each of these parameters can change the irrigation estimate. In addition to modifying their irrigation scheme, this study shows how much irrigation can affect simulations on hydrological processes in terms of several hydrological variables: evapotranspiration, leaf area index, river discharge and total water storage. In addition, their factor analyses indicate potential research directions to further improve the ORCHIDEE irrigation model, such as the explicit inclusion of paddy rice.

Such model improvement is essential for a better understanding of land surface processes in Earth system science. Considering the fact that human activities have influenced the Earth system, irrigation should also be a critical component to be further investigated. I understand that this is an important step for ORCHIDEE. However, I have some major concerns that the authors need to address.

We thank the reviewer for his time on reading and reviewing this manuscript. 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 2 are numbered from 1 to 19.

< Major comments >

1. (1) Why do the authors insist on globally uniform parameters (tuning)? While better estimation of total global irrigation withdrawals is an important challenge, better estimation of irrigation in heavily irrigated regions should also be a priority in a global study. The results show that this irrigation water estimate is relatively small compared to other irrigation estimates (Section5.1), and this should be related to the underestimation of irrigation volume in heavily irrigated countries (Fig4b-c). On the other hand, the globally uniform parameter tuning reduced irrigation volume to exacerbate the underestimation in these regions (Fig5c-1, Fig9). The authors also state that this is a drawback (Line.331). Therefore, I wonder if this tuning is sufficient to improve the simulation skill of ORCHIDEE. Since the authors already present

spatially varying beta value, which is a key tuned parameter, to minimize the irrigation bias in this study (Fig9), I wonder why the authors did not apply this spatially varying parameter to estimate the main irrigation estimate. I assume that there are reasons (perhaps, related to modeling philosophy) for this decision to apply globally uniform parameters and their tuning. If so, I expect the authors to clarify their thoughts in an earlier part of this manuscript.

We agree with the reviewer, better estimations of irrigation withdrawals at global scale are not incompatible with better estimations of withdrawals in those heavily irrigated regions. But we insist on globally uniform parameters, because: it is a first step in the implementation of irrigation at global scale; it is a tradeoff between representation of first-order effects and inclusion of local irrigation strategies; despite the shortcoming, we show that the scheme adequately represent main effects; finally we show that there is an interesting clue to improve irrigation representation by including a spatially varying 'beta' value, using the presence of paddy rice fields.

The latter (using paddy rice to spatially vary 'beta') is important since punctual optimization is not considered in ORCHIDEE due to scale dependence, so the LSM needs to use observed spatial features (e.g., fractions of PFT, soil texture, or paddy rice fractions).

We propose to include the drawback of globally uniform parameters and the limitation on the parameter tuning at the beginning of the discussion (section 6 of the original manuscript), as described in the answer to observation 13, reviewer 1.

2. (2) Another concern related to the parameter tuning is the reference year, 2000. Given the spatiotemporal uncertainty in the meteorological forcing data (even with reanalysis-based forcing data), I wonder if the single reference year allows the authors to robustly tune the parameter. I require an explanation or methodological modification in this regard.

These two concerns (uncertainty in meteorological data, and uncertainty in the tuning process with a single reference year) are important. We agree with the reviewer that these two issues could affect the robustness of the parameter tuning.

On the first issue, it was out of the paper scope to assess the effect of changing the meteorological input on irrigation estimates, although we expect to include this effect in the future. On the second issue, the year 2000 for tuning is often used as reference, because there is more data for that year (see for instance Pokhrel et al., 2016, table 2)

We propose to include these two issues at the beginning of the discussion (section 6 of the original manuscript), as **described in the answer to observation 13, reviewer 1**.

We propose to add a sentence on the concern about the use of a single reference year and the reasons to use data from year 2000 (l. 295 of the original manuscript).

We ran a total of 23 simulations with varying parameters, plus a reference simulation with no irrigation. All of them were run with the same initial conditions for three years (1998 - 2000), and a comparison of irrigation amount and ET increase was performed for the year 2000. By using the last simulation year, we reduce the effect of the common initial conditions on the *simulation results, and the year 2000 corresponds to the values given in AQUASTAT and Sacks et al. (2009).* ***Note that we use a single meteorological forcing dataset and compare our estimates to a single set of observed AQUASTAT data for the period around the year 2000. We choose to compare our estimates for the year 2000 because this year is commonly used as the reference period in the literature concerning the estimation of the amount of irrigation on a global scale (see, e.g., Pokhrel et al., 2016, Table 2). The choice of the year 2000 is mainly due to the existence of more complete reported or observed values for that year, as well as simulated estimates. We use the same reference period to compare our results with independent data.*** *A brief description of each parameter as well as the unit, range, and values used in the sensitivity analysis is shown in Table 2.*

3. (3) I would like the authors to revisit irrigation efficiency and describe in more detail how they account for this factor in their irrigation estimate. I may be wrong, but as far as I have read Section 2.2, evaporative, infiltration, and seepage losses during conveyance, distribution, and application processes, are not considered in the calculation of irrigation water withdrawal from irrigation requirement. Other models that use soil moisture target methods generally consider irrigation efficiency (such as CLM5, LPJmL, H08, and HiGWMAT etc.). Although irrigation efficiency has a large uncertainty, if irrigation efficiency is not considered, irrigation withdrawal cannot be properly estimated from irrigation requirements. Thus, the relatively smaller global total irrigation volume estimated in this study may be related to this point. (Note that this is the different irrigation efficiency defined in line316.)

The scheme does not consider conveyance losses, but we do allow the model to calculate losses related to the application of irrigation, as we simply put the withdrawn water over the soil surface for infiltration. This means that the model decides if the added water runs off or not, then if the infiltrated water is used by the plant or evaporates from the bare soil, or if ultimately it increases deep drainage. It also implies that the water demand is rarely fulfilled during a single time step, because not all the added water is used by the plant. That is why we fit our values to reported irrigation amounts from AQUASTAT, but check the effect in evapotranspiration increase as well.

We propose to show how irrigation efficiency is considered in some models, in introduction, line 45 of the original manuscript:

Some models *also consider* ***conveyance*** *losses and* ***return flows to rivers and aquifers****, i.e. they consider the total water withdrawal* ***(water demand plus losses)****, by using empirical ratios of* ***irrigation efficiency (ratio of ET increase to water withdrawal)*** *or specific rules according to the irrigation* ***technique*** *(Rost et al., 2008, Jägermeyr et al., 2015).*

We propose to highlight the different types of shortcoming and flaws in the modelling framework, and its effects on irrigation efficiency, in discussion (section 6 of the original manuscript), as **described in the answer to observation 13, reviewer 1**.

We propose to clarify what is included in LSMs (return flows) and what is not included (conveyance losses) by changing the current sentence, so it is clearer (line 56 of original manuscript):

Some LSMs prescribe irrigation rates estimated offline (Lo and Famiglietti, 2013; Cook et al., 2015), but most of LSMs and some GHMs estimate irrigation demand by calculating a deficit, for instance, a soil moisture deficit between actual and a target soil moisture (Haddeland et al., 2006; Leng et al., 2014; Pokhrel et al., 2015; Jägermeyr et al., 2015). ***Some LSMs, which benefit from a physically based description of surface runoff and drainage, can explicitly calculate return flow****, but* ***conveyance*** *losses are not explicitly included (Yin et al., 2020; Leng et al., 2017).*

We propose to add a point at the end of section 4. Sensitivity analysis and parameter tuning, so it is clear that our modelling framework lacks some important characteristics of irrigation that could affect water withdrawal, ET increase and ultimately irrigation efficiency, in line 334 of the original submitted paper:

After this analysis we underline four points. First, this process does not correspond to a proper calibration, as we assumed uniform parameter values, the number of simulations is low and the observed data is sparse. The objective of the sensitivity analysis and parameter tuning was to identify key parameters and reduce the underestimation of irrigation by tuning the uniform parameter values. ***Second, our scheme does not include conveyance losses although application losses and return flows are represented. As ORCHIDEE determines the water partitioning, some model flaws in hydrologic processes like infiltration or bare soil evaporation could bias the effect in return flows, in the increase of ET, and ultimately in the irrigation efficiency.***

4. (4) Could you add a description about the crop calendar? It should be explained how the authors defined the irrigation period. The authors mention that they did not consider double cropping, but there does not seem to be any explanation about the crop calendar in the current manuscript.

We propose to include the information on crop calendars after line 102 of the original manuscript:

(...) so that C3 and C4 crops are simply assumed to have the same phenology as natural grasslands, but with higher carboxylation rates and adapted maximum possible LAI (Krinner et al., 2005). ***The crop growing season depends on mean annual air temperature, as detailed in (Krinner et al., 2005). In cool regions it starts after a predefined number of growing degree days, while in warm regions, it starts a predefined number of days after soil moisture has reached its minimum during the dry season. In intermediate zones, the two criteria have to be fulfilled. The end of the growing season also depends on temperature and water stress, and on leaf age.***

We propose to include the differences in crop calendar and growing season as a source leading to differences in irrigation amount, efficiency and irrigation dynamics (section 6 of the original manuscript), as **described in the answer to observation 13, reviewer 1**.

5. (5) How does ORCHIDEE define “renewable”-groundwater resource?

It corresponds to shallow aquifers recharged by drainage at the soil bottom. We include this information in the manuscript, see response to observation 9 reviewer 1.

6. (6) All figures are blurred. It seems that dpi needs to be higher.

We propose to explore this problem with the edition staff. Currently we are using 350 dpi, but we can increase the resolution of our figures if needed.

7. (7) Regarding Fig4b-c, could you provide (supplementary) figures in % compared to Sackes et al. 2009? It is difficult to see how the irrigation bias is critical compared to the reference values.

We propose to include the difference in % in the supplementary as Fig. S11. We also propose to include a map of irrigation efficiency (ratio of ET increase to water withdrawal) by country.

We also propose to include the following text in the supplementary:

In Fig. S11-a we can observe a strong overestimation (red). These areas depict a small irrigation rate (0.01 to 0.05 mm/d) that is strongly surpassed by the simulations, but the absolute value remains small. On the other hand, we observe areas with a strong underestimation (blue). These areas show higher irrigation rates than the areas in red (over 0.1) and in general, fit well with regions where paddy rice is important.

Irrigation efficiency map by country (in Fig. S11-b) show values over 100% in some countries. These high irrigation efficiency values mean that the crops increase ET by using a higher fraction of rainfall, even when there is not irrigation in the area. This is the result of suppressing part of the crop water stress, and lacking a specialized phenology module with crop stages like germination and harvesting. As crops are not harvested, even if there is not irrigation, there is more ET.

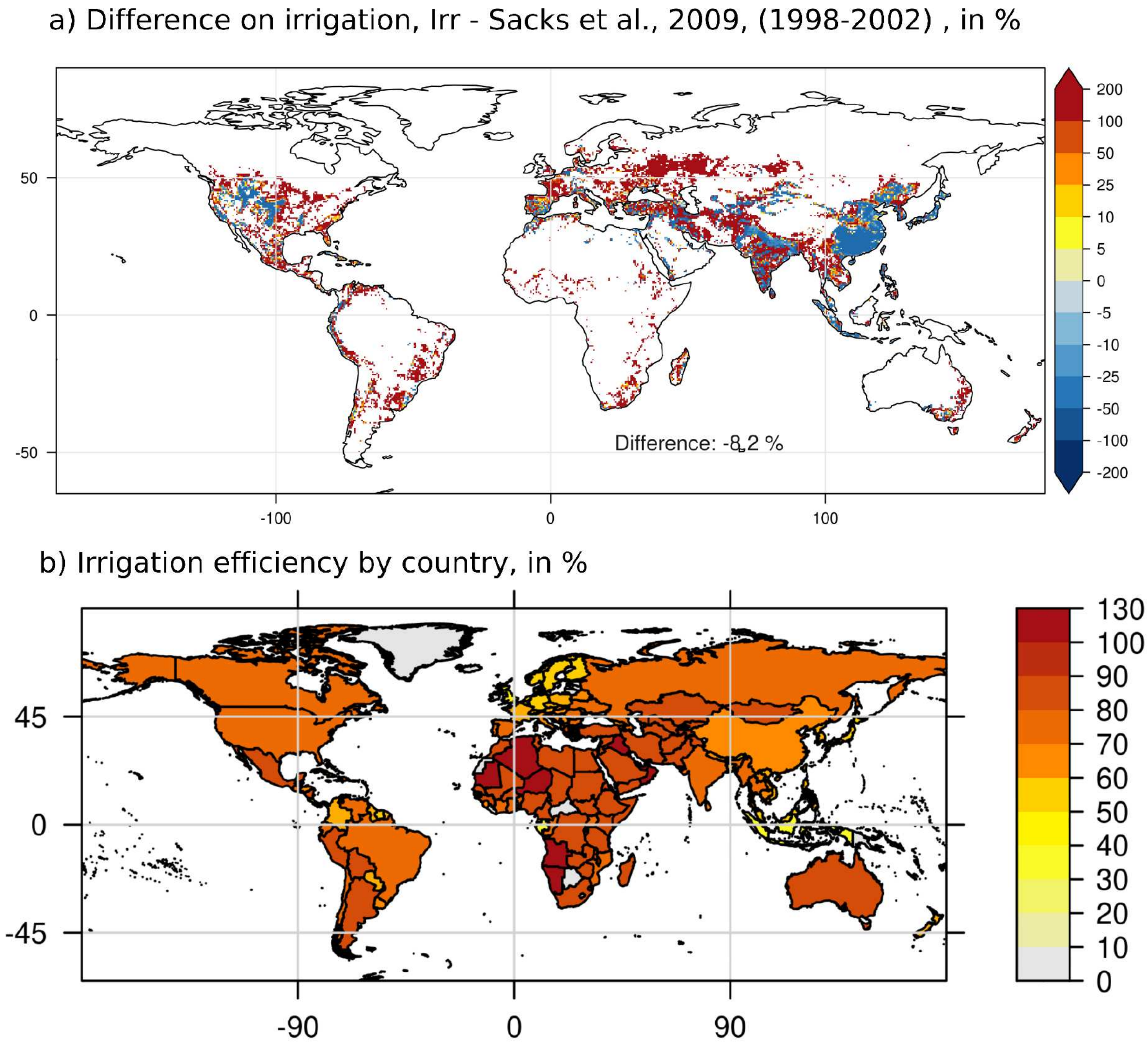

Figure S11 Difference in % in water withdrawal between Irr (yearly average 1998-2002) and dataset from Sacks et al., 2009 (a). Irrigation efficiency from Irr simulation (yearly average 1998-2002) as the ratio of increase of evapotranspiration to irrigation withdrawal (b).

We propose to cite this figure in line 347 of the original manuscript, and line 479 of the original manuscript:

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).***

When we compare the simulations with the FLUXCOM product, the activation of irrigation leads to a reduction of the negative evapotranspiration bias, but the use of a single soil column in ORCHIDEE for both rainfed and irrigated crops could induce an overestimation of ET increase ***(See Fig. S11, in some cases the irrigation efficiency by country is too high).***

< Minor comments >

8. Line 43: PET needs to be spelled out here.

We propose to change this sentence, please see response to observation 3 and 4 from reviewer 1.

9. Line 44: If I remember correctly, H08 applies the soil moisture target method.

The reviewer is right, we will retire 'Hanasaki et al., 2018' from this citation, as H08 applies the soil moisture target method.

10. Line 60: In this context, GHM should also be included.

We agree, we propose to slightly change the sentence:

In addition, irrigation shortage due to water availability is not well represented in ***those*** *LSMs* ***(and GHMs) including this feature****, as some of them include a virtual infinite reservoir to fulfill irrigation demand (Ozdogan et al., 2010; Leng et al., 2014; Pokhrel et al., 2012).*

11. Table1: Probably, Ai should be ai.

Thanks for this typo, we will correct it

12. Line 271: It would be better to explain the original spatial resolution of the observed data in Section 3.2.

We will add the lacking resolutions for FLUXCOM, GLEAM and LAI3g

We use two datasets: the first product is GLEAM v3.3a, which combines satellite-observed *values of soil moisture, vegetation optical depth, and snow-water equivalent, reanalysis of air temperature and radiation, and a multisource precipitation product* ***at 0.25º of gridcell size*** *(Martens et al., 2017). The second dataset is FLUXCOM (Jung et al.,2019), which merges Fluxnet eddy covariance towers with remote sensing (RS) and meteorological (METEO) data using machine learning algorithms* ***at 0.5º of gridcell size****. Here we use RS+METEO products, specifically the averages of RS+METEO*$_{WFDEI}$ *and RS+METEO*$_{cruncep,v8}$*, to cover the analysis period.*

We use the LAI3g dataseT (Zhu et al., 2013) climatological values for the period 1983-2015. This dataset applies a neural network algorithm on satellite observations of the Normalized Difference Vegetation Index (NDVI) 3g to estimate LAI ***at 1/12 degrees of spatial resolution****.*

13. Fig5a: Could it be possible to add a reference plot(s) (AQUASTAT or other models' estimate)?

We propose to add a dashed line showing the AQUASTAT estimate for the year 2000.

14. Line 407, “we observe higher peaks and low values In Huang He when irrigation is activated”: I can not understand which “low values” is about. Could you rephrase this?

Thanks for this observation. We propose to reformulate as follows:

We now focus on the average TWSA value at the basin scale (Fig. 7). Activation of irrigation induces small changes in TWSA, which is coherent with changes in TWS between both simulations (Figure S2). For instance, we observe higher peaks in the TWSA values ***in Huang He when irrigation is activated. Low values also become lower for the Irr simulation in Huang He basin.*** *In the Ganges river basin, low values are lower* ***as well*** *in the Irr simulation than in NoIrr.*

15. Line 418-420: I could not understand the point of this sentence in my first reading. Could you exemplify basins in Fig8 in this sentence?

We propose to put some examples, as suggested by the reviewer:

The main effect of irrigation over the seasonal variations is that peak discharge can occurs before in the Irr simulation ***(for instance Missouri river or Yellow river)****, or that the decrease after the peak is more rapid and low values are lower in Irr than in NoIrr* ***(for instance Colorado river or the Danube river)****.*

16. Fig 6: Add x-axis label.

We propose to add the x-axis label as ‘Irrigation classes’.

17. Line 452: Hanasaki et al. 2008b seems to be a wrong reference here because H08 uses groundwater resource when surface water availability is not sufficient to meet irrigation demand.

We disagree with the reviewer. In Hanasaki et al., 2008, in simulation IRG they use a single imaginary water source. In FUL simulation, they make the hypothesis that all the water withdrawn comes from the rivers. In Hanasaki et al., 2018, the model includes a new groundwater recharge, and allows groundwater abstraction (Section 2.1, Newly added schemes). We propose to leave the citation as it is.

18. Line 459, “... like topography and environmental flow”: Refer Hanasaki et al. 2018 here.

We will refer to Hanasaki et al. 2018 in this line.

19. Line 461: The following models also include detailed irrigation schemes: doi:10.5194/hess-19-3073-2015, doi:10.1029/2022MS003074.

We thank the reviewer for this paper, we propose to include the reference in our paper, line 460 for Yao et al., 2022. The reference Jägermeyr et al., 2015 has been already cited in the manuscript.

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.*

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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