

## ***Interactive comment on “Sensitivity of the West African hydrological cycle in ORCHIDEE to infiltration processes” by T. d’Orgeval et al.***

**T. d’Orgeval et al.**

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The authors would like to thank the referee for his interesting remarks that allowed to clarify some important points of the article.

Major Remarks - In the paper, the sensitivity of the ORCHIDEE to infiltration processes is tested. If all sensitivities are very specific to ORCHIDEE, then the results would not really justify a publication. Well, in my opinion, this is not the case as several results of the present study seem to be of general importance. But such general results should be clearly pointed out. And it also should become clear how much of the sensitivities found depend on the model structure of ORCHIDEE. The separation of ORCHIDEE specific and more generally applicable results should be made much clearer in the text. Currently the manuscript is partially oriented too much towards ORCHIDEE, thereby

missing a broader scope in the implication of its results.

ANSWER: We agree with the reviewer that despite the fact that the experiments were conducted with ORCHIDEE, the sensitivities found do not mainly rely on the structure of ORCHIDEE. The respective role of the different processes identified with ORCHIDEE would be the one observed if sufficient observations were available to quantify the different processes through field experiments. The importance of the results beyond the ORCHIDEE model is more clearly stated in the discussion and conclusion section of the revised version.

- Is ORCHIDEE always newly calibrated for each sensitivity experiment? Calibration is only mentioned in the abstract and the conclusions. Please clarify for which experiments ORCHIDEE is calibrated, and whether this affects the results or not.

ANSWER: There was no specific calibration of ORCHIDEE for this article. The version used here is the same as in d'Orgeval and Polcher (2008). However, a calibration was performed to obtain the version of ORCHIDEE used in both articles. The calibration followed the introduction of the 3 parameterizations for infiltration processes. More details are given in the answer to reviewer 1. In the revised version of the article, only the calibration of the infiltration parameterizations is mentioned and explained.

- I was somewhat confused by the use of two different forcing datasets in the sections 3 and 4. I suggest including a common section on used data before Sect. 3 and 4 instead of a few sentences on data in each of these sections.

ANSWER: Section 3 provides a comparison of ORCHIDEE with other LSMs before testing the sensitivity of ORCHIDEE to infiltration processes (with GSWP2 forcing), whereas section 4 provides a comparison with river discharge observations before testing the sensitivity to the surface water parameterizations (with NCC forcing). As no multi-model experiment was carried out with NCC, NCC could not be used in section 3. On the opposite, as GSWP2 is a forcing for 1986-1995, and discharge observations are mainly available for the mid 1950s until the late 1970s, it was not possible to use

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GSWP2 in the second part. The use of 2 different datasets (GSWP2 and NCC), and 2 different methodologies (evapotranspiration and river discharge comparisons) explains the fact that there is a specific "data and methodology" sub-section in each section. To clarify this point, the fact that 2 different datasets are used in section 3 and 4 is stated at the end of the introduction in the revised version.

Minor Remarks In the following suggestions for editorial corrections are marked in *Italic*.

Abstract &#8211; par. 1 &#8211; p. 2251 - line 2-4 It is written: The aim of this article is to test the sensitivity of the Land Surface Model (LSM) ORCHIDEE . . . This aim sounds rather technical. I would prefer a rephrasing of the aim according to scientific questions to be addressed. Sect. 1 &#8211; p. 2254 - line 27 . . . the conclusions in . . . Sect. 3 &#8211; p. 2262 - line 16 These two features . . .

ANSWER: These changes are made in the revised version.

Sect. 4.1 &#8211; p. 2263 - line 4-15 I don&#8217;t understand why only 2\*2 years are used for the validation. Shouldn&#8217;t one use as much observations of adequate quality as possible for this purpose?

ANSWER: As there are many missing data in the river discharge datasets for Africa, we chose the largest sets of years with dry and humid years and for which there was a maximum number of available discharge observations.

It is written in line 14-15: . . . . does not allow for the identification of errors in the annual cycle. I don&#8217;t understand this statement as information on the annual cycle is available from observations, thus its validation should possible.

ANSWER: Indeed, the observations allow for the validation of the annual cycle. The sentence only states that the chosen methodology does not. Indeed, the focus of the article is on errors and uncertainty in terms of annual means. The reason for this choice is to clarify the message of the article. Should the representation of the annual cycle be a main focus for the article, the role of the time constants of the routing would have

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to be explored more thoroughly. This could be the aim of another article.

Sect. 4.1 &#8211; p. 2263 - line 20-21 It is written: . . . or if ORCHIDEE&#8217;s error on the catchment area exceeds 20%. Why this is occurring? Couldn&#8217;t you correct this error before applying ORCHIDEE? Please clarify.

ANSWER: The error may be high for two reasons: 1. the routing map is too coarse. Indeed the routing is computed at a scale of 0.5x0.5 degree. As the border between two basins does not follow the 0.5x0.5degree limits, the partitioning of water at the edge of a catchment may be inaccurate. However, this is negligible for large basins such as the ones used in this study. Moreover, the misplaced areas are generally not areas with ponds or floodplains, but consist in mountaineous areas. 2. the border of large catchments in the desert are not well defined. Therefore, the most uncertain catchment sizes are obtained for Dongola (Nile) and Malanville (Niger) for their border in the Sahara. However, this has no impact on the river flows and on the analysis because no water is received from the misplaced area.

Sect. 4.3 &#8211; p. 2267 - line 14-15 . . . fraction of more than 1.5% of the catchment area for . . . . Sect. 5 &#8211; p. 2269 - line 14 . . . similar to the classification of . . . . Sect. 5 &#8211; p. 2270 - line 8 . . . infiltration has a . . . . Sect. 6 &#8211; p. 2273 - line 4 So, the AMMA project . . .

ANSWER: These changes are made in the revised version.

Table 6 caption &#8211; p. 2284 - line 3 It is written: R mean total runoff (runoff plus discharge) . . . . Runoff plus discharge does not make sense. Usually total runoff refers to the amount of water that may flow laterally off (i.e. before it is routed), and discharge refers to the amount of water after it is routed and that can be directly compared to measured streamflow.

ANSWER: Runoff is replaced in the article by "surface runoff" and drainage by "sub-surface runoff" to make things clearer.

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Fig.4 &#8211; p. 2288 Why do you not just plot the anomaly relative to evapotranspiration from the control simulation? I don't understand this figure (without carefully reading the text).

ANSWER: The role of this graph is to show the relative importance of different parameterizations for different regions. We normalized the effects of the parameterizations because we wanted to focus on the comparison between the regions for a given parameterizations and not on the individual role of a given parameterization. Indeed, the absolute evapotranspiration anomalies obtained in each sensitivity experiment has no specific meaning as it strongly depends on how the sensitivity test is specified.

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