# Response to Reviewer 3

We thank the reviewer for his/her time dedicated to this manuscript. We found the comments highly valuable to improve the quality of our manuscript.

Please see our detailed replies to each comment in blue. Text in bold is text that is copied from the new manuscript. Text in bold and highlighted in yellow is new text added as a result of the review.

This paper describes a new floodplain scheme developed within the framework of the land surface modeling platform ORCHIDEE. The main applications of this new model development are intended to be used at the regional-to-global scale in so-called “offline mode” (decoupled from a regional climate, RCM, or global-scale earth system/climate model, GCM) or coupled to an atmospheric model, thus the level of complexity, process representation and input data are adapted for such applications. As noted by the authors, RCM and GCM spatial resolutions are constantly increasing, thus there is a need to adapt the hydrological parameterizations in such models accordingly. Rather than using a classic grid structure (as many GCMs currently use) dictated by the atmospheric model, the current scheme is based on the Hydrological Transfer Unit (HTU) concept. The implementation of this scheme benefits from numerous relatively high spatial resolution topographical and geomorphological off-the-shelf datasets now available to hydrologists. This paper describes the methodology and mathematical underpinnings of this new floodplain scheme and how it interacts with other components of ORCHIDEE (such as evaporation, river flow, runoff, etc.). The scheme is next used to simulate the floodplains along with the other main components of the surface hydrological cycle over a recent multi-year period over the Pantanal basin in South America, which contains one of the world’s largest floodplains thus making it a very pertinent case study. The model is evaluated at two spatial scales, one representing the approximate scale still used by many GCMs (i.e. 0.5 o) and another representing a scale comparable to RCMs and what more and more GCMs are (or plan to) move to in upcoming years (~25 km). As boundary conditions, so-called atmospheric forcing must be prescribed in offline mode but there are many such products and there are considerable differences among them, especially at different spatial scales as herein. The authors have addressed these uncertainties by using a very standard analysis product as forcing at the more coarse resolution, along with a forcing which has been developed specifically for this region at a higher spatial resolution. The model simulations, notably the floodplain outputs, are evaluated using several standard satellite-based products along with in-situ discharge measurements. Convincing statistical results are used to summarize the performance of the model using the new parameterization for the two input forcing compared to the baseline model (without the new floodplain scheme). A discussion of errors (in terms of the model input, output and the evaluation data), limitations, and gained insights are presented. I find the organization of the paper to be quite good, it is well written: the overall presentation is clear, the results are presented in a very pragmatic manner and future perspectives are discussed. I recommend publication after only some minor revisions as this paper is an important contribution to the rapidly developing region-to-global scale hydrological modeling field, notably improved terrestrial water cycle simulations in RCMs and GCMs.

**General Comments:**

1. Lines 339-358: In my opinion, the only part of this paper which needs some improvement is Calibration of the Parameters. There are no graphics (for example, showing the discharge performance at the calibration station) and only limited statistics (Table 1.).

Lines 347-349 mention that The best combination of parameters has been established through a grid search method which consists in evaluating the different combinations of parameters within their respective interval of definition. I find this a bit vague and it seems to gloss over a very important part of any new parameterization: parameter calibration/estimation/determination. I feel the authors should just give a slightly more detailed description of how exactly the parameters were calibrated. There is some limited information, but more details would be appreciated. Also, plots of discharge before and after calibration would be informative. Also, 1991-1996 was the calibration period: why these 6 years? Is the natural variability adequately represented over these years? And so on. Again, just a few more details on the methods and results. Parameter sensitivity analysis is a critical part of any new model development and a bit more information would be very informative to readers.

We appreciate your comment, and as per your suggestion, we have completely rewritten this section.

We didn't include figures of the before and after calibration process because there is no "initial state" of the parameters, we directly compared the outputs for a range of values which was estimated as physically reasonable. This is why we focused our analysis on the comparison of the discharge with and without the floodplains scheme activated, this would be equivalent to a floodplain time constant equal to the stream reservoir time constant, a very large OF parameter, a flood fraction always equal to zero and a C parameter equal to 1.

The new version of this subsection about the calibration emphasize the role of each parameter, how they affect the simulated discharge and the model in general and the relative sensibility of the simulated discharge to each parameter and then described.

**The different parameters of the floodplains scheme have been calibrated based on the simulated discharge at the *Porto Murtinho* station, which is the reference station at the outflow of the Pantanal (Brazil, lat: 21.7°S, lon: 57.9°W) between 1991 and 1996 in comparison to the observations considering: (1) the variation of the discharge through its correlation with the observations and (2) the mean value and variability of the discharge. The choice of the 6 years calibration period was due to a limited number of available years from the simulations (24 years). Therefore, the model has been calibrated over this reduced period common to both forcing in order that the results analyzed after are not influenced by an overfitting effect. Considering that our model have a reduced number of physical variables, we consider it is not necessary to assess it on large periods as we made the assumption that these parameters are relatively independent of the hydrological cycle variability. However, we agree that performing the calibration over a larger period of time could have been preferable, but**

**we faced 2 limitations for this point: 1) the period of the simulations (AmSud was only available from 1990 to 2019) and 2) a technical limit due to the resources (time and computational resources) needed to run the simulations.**

**The parameter with the largest influence on the variability of the discharge is $\tau_f$, the time constant of the floodplains reservoir. This parameter has an important impact on the annual cycle of the discharge at Porto Murtinho station. The $[\alpha_{stream}, \alpha_{fast}]$ interval is considered as a valid interval for $\tau_f$. This interval has been discretized to select different possible values for $\tau_f$.**

**It has been assessed along with $R_{limit}$ which is the second parameter with the largest influence on the discharge. For $R_{limit}$, we discretized the [0,1] interval to obtain possible values.**

**In a first step, these two parameters have been calibrated together, we performed a grid-search evaluation, which means that we evaluated all the existing combination of possible discretized values over the intervals for $\tau_f$ and $\tau_f$ to select the combination with the best performance to represent the observed discharge.**

**In a second step, we assessed the parameters related to the overflow, which have a limited impact on the discharge $OF$ and $OF_{repeat}$. These parameters slightly influence the temporality of the discharge. In this case, we also assessed these two parameters using a grid-search evaluation considering a discretization of the following intervals: [0.5 day, 2 days] for $OF$ and [1 repetition, 5 repetitions] for $OF_{repeat}$.**

**Finally, the last parameter to calibrate is the infiltration constant ($C$) which determines the loss to soil moisture and, thus, potentially to evaporation. This parameter with a very reduced impact on the discharge and only reduce / increase the level of the discharge at the outflow of the region. We discretized the [0,1] interval to assess it.**

2. The quality of the English is good, however there are a certain number of very small errors, notably the use or lack thereof of “a” or “s” at the end of some words, e.g. Line 48: a South American tropical floodplains. There are just a few small errors like this on nearly every page, so they do not detract from the reading or result in a lack of understanding. But I’d recommend a quick filtering to catch them.

Thank you for your comment, we performed a complete review to identify and correct these issues.

**More specific:**

Line 121: I suggest changing ruling to governing

Thank you, the text has been corrected.

Line 130: Referring to the text: HTU only flows into a single HTU and is acyclic as water cannot return to the original HTU: I assume that backwater effects can be neglected at the spatial resolutions you are modeling here?

Exactly, backwater effects are neglected because they are not relevant at this resolution. However, they can have an impact over larger river such as at the confluence of Paraná and Paraguay river, but this is out of our area of interest.

Eq.2 for evaporation from the floodplains: water surfaces have very low roughness lengths compared to land surfaces: typically Charnock-type parameterizations are used for water bodies. I assume that floodplains are generally fairly smooth...should this effect (or is it?) somehow incorporated into this computation? I suspect that using such a roughness length could reduce the evaporation from floodplains (?).

Thank you for your comment.
The Charnock-type formulation for surface roughness is conceived for open oceans without any surface elements (except waves) which can generate atmospheric turbulence. In the case of the floodplain or a lake, the open water is surrounded by trees or mountains, generating turbulence over the open water. It is thus not a given that the open water of a floodplain or lake has the same effective roughness as the open ocean.
We can evaluate the use of this type of formulation over flooded areas in future works.

Line 172: I am surprised that soil water infiltration can be larger outside of floodplains than within them. Can the authors present some sort of physical arguments or an observational basis for this assumption?

We agree that this point is not clear. The k_litt parameter is the Hydraulic conductivity at saturation over the first layer of soil. We assumed that this parameter can change over the floodplains because these processes at the interaction between flood water and soil can be altered due to the presence of sediments which can decrease the infiltration rate. This is why we decided to open the possibility to calibrate this parameter. The outcome is that this parameter was identical in the higher resolution simulation and has been found lower in the WFDEI_GPCC simulation.

This was not originally clarified, and you are right that we need to be more transparent on our original assumptions. This has been clarified:

**This k_litt parameter has been established for the soil infiltration processes but not specifically for floodplains. Therefore, we assume that the infiltration can be different over the floodplains due to the presence of sediments, which may reduce the infiltration capacity. This is why a reduction factor (C) has been introduced to evaluate changes in the infiltration over flooded areas if necessary. This parameter may**

**depend on the local properties of the region considered such as the type of vegetation or the soil and the sediments which cannot be represented explicitly.**

Line 188: Referring to the text: The time constant of the floodplains (τf) is slower than the stream reservoir time constant (τstream) and faster than the fast reservoir time constant. Can the authors give some sort of physical argument or explanation for this (frictional effects of flooded riparian vegetation and non-riparian vegetation in flooded zones for example? Or some other reason? Or just a reference justifying this choice?)

The floodplains time constant is necessarily higher than the stream reservoir time constant because the floodplains reservoir represents the slow-down of the river discharge flow over the floodplains. Still, the time constant in the floodplains is related to the river flow and, therefore, should be lower than runoff processes. This difference between the stream time constant and floodplains time constant is related to frictional effects of flooded riparian vegetation and non-riparian vegetation in flooded zones due to the locally divergent flow of water sparsing.

The following sentence has been added in the article:
**The time constant of the floodplains (τf) is slower than the stream reservoir time constant (τstream) and faster than the fast reservoir time constant because the dynamic floodplains reservoir represents the slow-down of the river flow over the floodplains due to frictional effects of flooded riparian vegetation and non-riparian vegetation in flooded zones due to the locally divergent flow of water sparsing. The fast reservoir model a slower dynamic related to runoff and therefore is an upper limit for the floodplains reservoir time constant**

Eq.5: It is not quite clear to me why when Sfmax,i > 0 there is no contribution from the upstream stream reservoir to the local stream flow (it is just from the upstream floodplain...)...I am missing something here.

When a certain HTU is considered as a floodplain (Sfmax,i > 0) the water is not coming from the stream reservoir of the upstream HTUs but first flow into its floodplain reservoir. (Qf,i).

Eq.8: It seems that a term is missing on the RHS...the possible addition of overflow from the downstream reservoir?

You are right, thank you for highlighting this omission.
The possible overflow of the current floodplains HTU into the upstream one is not included there. This has been corrected.

Lines 271-273 should probably be placed after Eq.13 since “beta” doesn’t seem to be mentioned until Eq.13.Lines 312-313: Referring to the text: The different values of standard deviation are bounded by lowlim_std = 0.05m and uplim_std = 20m. Why these particular values? Is the model very sensitive to this range?

Concerning the first point, we think that it is better to keep the description of beta along with the other variable used to describe the floodplains geometry in 2.4. This way, beta is also defined before using it in the equations in the following subsection in 2.4.1 (eq. 13).

Concerning the second point, it was difficult to establish a simple relationship between the distribution of the elevation within a HTU and this beta variable. We used clustering methods to analyze the different type of distribution and the corresponding beta. This is how we defined these limits and we are aware that this is a raw approximation and this is something that need to be improved in future development of the model. The simulation of the discharge is not so sensitive to these values, however this will affect the flooded area but in a small extent.

Line 465: It seems that there are only roughly 1 to 2 GRACE pixels covering your zone, likely not with a perfect overlap. Is this really sufficient? Can you say a bit more about the errors involved in this comparison to justify this for readers?

You are right and we are aware of this. However, GRACE was the best tool we had at this moment to perform this type of analysis. Although the area is large enough to justify the use of GRACE, there can be, as you say, overlapping error. It is more of a qualitative comparison than a quantitative one. Hopefully, the new generation of GRACE will allow seeing it more clearly.

We added the following specification:
**Although the area is large enough to justify the use of GRACE, there can be an error related to the overlap of pixels. Still, GRACE is the best tool available at this moment to perform this type of analysis. Also, the comparison GRACE is more of a qualitative than a quantitative one.**

Line 475: Maybe I missed it, but I assume the statistics were made using monthly model outputs and observations?

Exactly, thank you for highlighting this imprecision. This has been specified.

Fig.3 showing the multi-year monthly averages as a single annual cycle is indeed an informative way to convey the quality of the climatological performance of the scheme. But aside from the statistics, it would be good to see some graphical information on the year-to-year variability per month in the main paper: some sort of spread (standard deviation or quantiles, etc.) on these plots would be most informative. Indeed, we wish to see the climatological (average annual cycle), but it is of course the improvement or degradation in terms of model vs observed variability that is also of interest.

Thank you for this suggestion, Figure I has been added in Annex and the following comment has been added in the article:
**The interannual variability of the monthly discharge at Porto Murtinho is shown in Figure I. We can observe that the floodplains scheme reduces the variability of the discharge. Between October and April, the variability of the FP simulations is closed to the observed discharge variability. From May to September, the variability of the**

**monthly discharge is overestimated compared to the observation. This overestimation is higher in WFDEI_GPCC_FP compared to AmSud_GPCC_FP.**

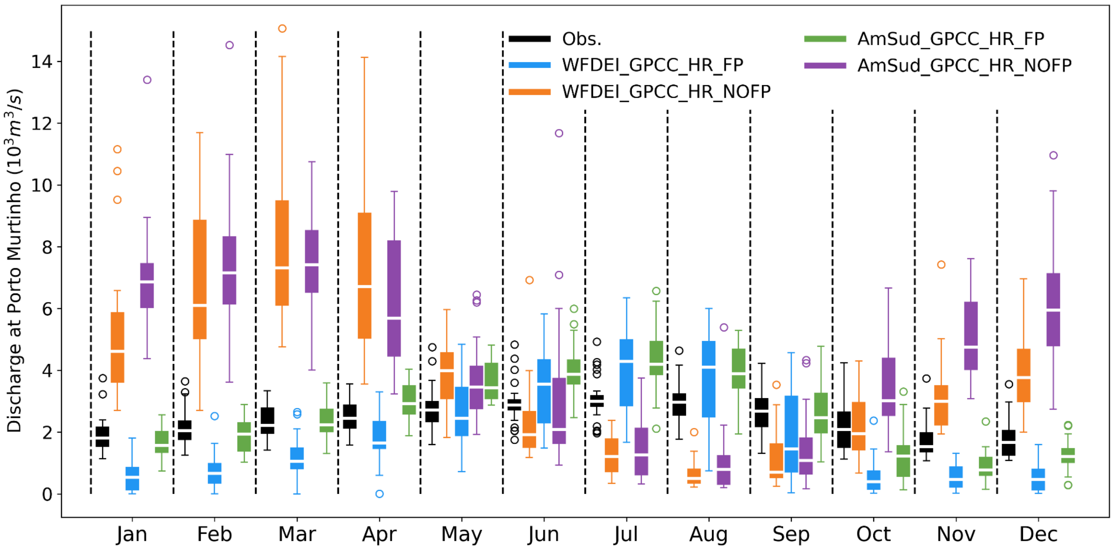

**Figure I: observed and simulated boxplot representing the interannual variability of the average monthly discharge at Porto Murtinho between 1990 and 2013.**

Line 522: Referring to the text: AmSud_FP seems to have more runoff. This sounds a bit speculative and it seems that it would be easy to verify by comparing the modeled runoff with and without floodplains? The authors could just include some numerical values here within the text for example.

Thank you for highlighting this imprecision, the sentence was not clear.

We also quantified this aspect more precisely and calculated the range of order of the runoff in the different simulations over the Pantanal floodplains. The runoff and drainage and is higher in the simulations with floodplains activated. The runoff is 3 times higher (respectively 63 times higher) in the AmSud_GPCC_FP (respectively WFDEI_GPCC_FP) simulation compared to the AmSud_GPCC_NOFP (respectively WFDEI_GPCC_NOFP) simulation.
The higher increase in WFDEI_GPCC can also be observed in the fast reservoir difference between WFDEI_GPCC_FP and WFDEI_GPCC_NOFP in Figure 3.f.

The sentence you mention has been removed and replace by the following :

**This can be explained by the increase of runoff in the FP simulations compared to the NOFP simulations (not shown). This increase is much higher in WFDEI_GPCC than in AmSud_GPCC.**

End of Section 4.2: After reading this section, I am left wondering whether it possible to give a number or show a figure of the contribution of Eflood to the total E? The total E with floodplains will almost certainly increase (when using the same prescribed forcing) over soils which have been wetted once floodwaters retreat, so increases in E will be at least related to this, as discussed in the paper. But Eflood seems to be rather uncertain/difficult to model and observe, I wonder how much Eflood is contributing to the overall E increase. I say this because I wonder if a more surface-water adapted approach for E might be in order, especially if this flux is significant compared to the other E components.

Thank you for this comment. The different components of evapotranspiration can be found in Figure 10. As discussed with your previous comment on the Charnock-type parameterization, the limitation of this type of approach will be the important local surface heterogeneities and also the presence of vegetation over and close to the flooded area.
As observed in Figure 10.c, if the floodplains lead to a significant soil moisture increase (such as it is the case with the WFDEI_GPCC forcing) the transpiration can have a non-negligible role in the increase of the evapotranspiration during the dry season. It is as important as direct evaporation from the flooded area during this season.

Lines 615-620: If I understand correctly, rainfall can lead to greater soil water infiltration that when the same grid element is flooded. This seems a bit counter-intuitive, to me anyway. Are there observational studies which can be referenced etc. to justify this?

This is a side effect of the modeling choices. Over a slightly flooded grid point, the water in the precipitation will go partly to the floodplain reservoir and in the case the flooded area is small, a lower volume of water will infiltrate. When there are no floodplains, all this water goes directly to soil moisture.

Lines 670-674: What about the increase in net radiation over the flood waters? The typical albedo for water surfaces is generally around 0.07, far lower than vegetation or soil. Is this considered?

This is due to the lower surface temperature. The surface albedo is not yet changed by the floodplains, but it should be !

Line 676-677: typo, a phrase is repeated → depending on vegetation type and on soil types (Clay, Sand, Silt). depending on vegetation and soil types (clay, sand, silt)

Thank you for highlighting this repetition. It has been corrected.

Lines 730-731: Can the authors just provide a phrase describing the specific sub-surface component and how this could help solve the mentioned issues?

We agree that this brings values to the discussion to detail a bit more the content of this sub-surface component and clarify how this solves the mentioned issues.

**The use of a specific sub-surface component such as suggested in the framework for LSM described in Hallouin et al. (2022) can be used to solve these issues by providing a tridirectional movement of the water in the ground with a lateral movement driven by topographic and hydraulic head gradients.**

# Reference

Hallouin, T., Ellis, R. J., Clark, D. B., Dadson, S. J., Hughes, A. G., Lawrence, B. N., ... & Polcher, J. (2022). UniFHy v0. 1.1: a community modelling framework for the terrestrial water cycle in Python. Geoscientific Model Development, 15(24), 9177-9196.