Anonymous Referee #2

We first thank the reviewer for his encouraging and very insightful comments, which helped us a lot to make the paper more straightforward, and of broader interest for the LSM community.

- GENERAL COMMENT

This paper evaluates the performance of two soil model formulations into a Land-Surface/Plant Phenology/River routing model of the Amazon (ORCHIDEE model). The soil models consist of a 2 layer bucket model and an 11 layer diffusive model. Model results are compared to estimates of terrestrial water storage (TWS) from GRACE mission, discharge (Q) from in situ data, evapotranspiration (ET) from a global scale dataset and leaf area index (LAI) and vegetation gross primary production (GPP). According to the authors, results from both soil models are similar. However, the 11 layer model could better represent ET, GPP, LAI, TWS and Q in southeastern sub-basins during dry season. Consequently, using the 11 layer soil model should be important to better represent hydrological processes in the drier sub-basins of the amazon, especially during dry seasons. The paper works on an important scientific question: how important is the use of multi-layer soil models if compared to simple bucket models to better represent hydrological storages and fluxes? It is always important to know how complex earth system models should be to represent important physical processes. This question is especially important for the case of the Amazon basin, where a wide range of hydrology models have been applied in the past. That's why the paper has great potential. However, some issues still need to be carefully addressed before publication. The first issue is that the 2 soil models don't seem fully comparable. It is not clear if their differences are mostly the number of layers or the several other hidden assumptions (Horton vs Dunne surface runoff, criteria for water percolation, parameters, etc...). These differences should be clearer to make it easier to extrapolate results from this paper to research outside ORCHIDEE context. Second, some of the validation datasets, as ET, are somehow uncertain. It would be necessary a better justification for the validation data. Third, the paper seems too long and descriptive, what makes it hard to read and less objective/conclusive. I present comments on these and some other issues bellow. For these reasons, I think that the paper should be published after major reviews. I hope that these comments can be useful to improve this paper/research.

- MAJOR COMMENTS

- Introduction/objectives:

The main question that the paper address is: "Does the use of an 11 layer soil diffusion scheme, rather than a simpler 2 layer scheme, improve the simulation of water storage dynamics and water fluxes?" I'd like to suggest some modifications to this question. It would be easier to extrapolate the conclusions to other research outside ORCHIDEE context if the paper compares "multi-layer soil diffusion schemes" vs "simple bucket schemes". I also think that it would be important to better clarify to which extent this question was already answered by previous research. Paragraph from lines 9 to 26 show several arguments showing the importance of accurate/multilayer soil modeling. It may be important for some things but not for others. For example, is it important for simulation ET and sensible heat fluxes? Is it important for land-atmosphere feedbacks? Discharge simulation? CO2? Total soil storage?... Which of these questions were already answered? Please make it clearer. On the other hand, you could clarify if your goal is to understand the importance of soil modeling at the Amazon basin.

You are right, thank you for these constructive remarks. We largely rewrote the introduction and added much more references to show the importance of accurate multilayer soil modeling:

"*As reviewed by Pitman (2003), soil hydrology parameterizations have evolved from conceptual bucket-type models, with one or two layers, with soil moisture described in terms of available moisture between the wilting-point and the field capacity, to physically-based models solving the Richards equation for water flow in unsaturated soil, and relying on volumetric water content up to full saturation (Abramopoulos et al., 1988; Thompson and Pollard, 1995; Viterbo and Beljaars, 1995; Chen et al., 1997; Cox et al., 1999; Boone et al., 2000; De Rosnay et al., 2000; Dai et al., 2003; Decharme et al., 2011). The latter approach offers many advantages, (i) to better account for spatial variability of soil properties (Gutmann and Small, 2005; Guillod et al., 2013), (ii) to implement processes that control soil moisture profiles, such as soil water infiltration and surface runoff generation (D'Orgeval et al., 2008), root water uptake for transpiration (Feddes et al., 2001), or hydraulic coupling to a water table (Liang et al., 2003; Gulden et al., 2007; Campoy et al., 2013), and (iii) to be comparable to available satellite observations of soil moisture in the top zone (Reichle and Koster, 2005; Draper et al., 2011; De Rosnay et al., 2013). There have been very few studies, however, to quantify the differences between conceptual bucket-type models and multilayer models, for simulated water fluxes involved in the terrestrial water budget. Confrontations to local-scale measurements have shown improved soil moisture control on ET in multilayer schemes in different domains (Mahfouf et al., 1996; De Rosnay et al., 2002; Decharme et al., 2011), including in the Amazon basin (Baker et al., 2008). Hagemann and Stacke (sub) also analyzed the influence of soil moisture vertical discretization on soil moisture memory and land-atmosphere coupling in the ECHAM6/JSBACH climate model. Finally, in a study coupling the ORCHIDEE (ORganizing Carbon and Hydrology in Dynamic EcosystEms, Krinner et al., 2005) LSM to the IPSL (Institut Pierre Simon Laplace) climate model, Cheruy et al. (2013) showed that the multilayer version of ORCHIDEE increased ET over Europe, in better agreement with local observations, and thus alleviated the summer warm bias of many climate models in the mid-latitudes (Boberg and Christensen, 2012; Mueller and Seneviratne, 2014).*"

Do you think that your conclusions should be extrapolated to other regions? If yes, you should clarify that the Amazon is only a case study. If not, clarify that the Amazon is the object of your study.

Our work is the very first study of comparison of the two hydrological models in ORCHIDEE. But a very recent study by Traoré et al. (under review in JGR-biogeosciences) also found over Africa that 11-layer version of the model outperforms the 2-layer version for simulating inter-annual variability of ET and soil moisture (see the following figure for ET comparison). Thus, our conclusions can be extrapolated to other regions. We have already specified it in conclusion (lines 20-23 page 102) that our study is currently extrapolated to the global scale and that we expected a signal in transition zones.

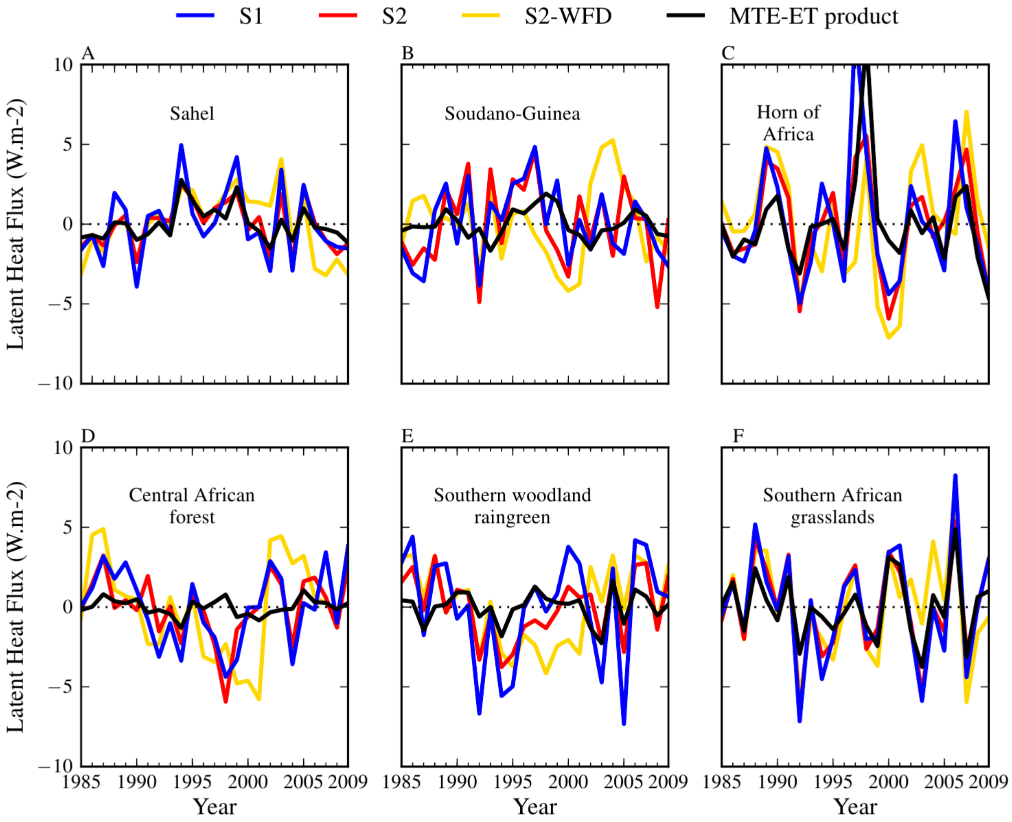

Figure 4: Comparison between interannual anomalies of ORCHIDEE evapotranspiration and MTE-ET product from fluxnet data product.

S1 corresponds to simulation with 11LAY, S2 with 2LAY and MTE-ET to the same ET product that we used for our paper (Jung et al. 2011).

- **<u>Model description:</u>**

I missed a more clear description about the differences between the two soil formulations. It was difficult to understand all about the model functioning by this explanation. It seems that the use of multi-layer diffusive model vs a 2 layer bucket model is not the only difference. Other differences include: 1. Dunne (2LAY) vs Horton surface runoff (11LAY). 2. Predefined runoff portioning of 5% to surface runoff and 95% to deep drainage (2LAY) vs surface runoff given by infiltration model and deep drainage given by free gravitational drainage model (11LAY) 3. Different parameters. 4. Among others... How can we know if the differences in the results are due to using 11 vs 2 soil layers or due to different parameters? Or due to different criteria for surface and deep drainage runoff? If the differences are not clear, and especially if different parameters are used, then the results get non conclusive. Some other issues: Are the parameters of both models equivalent? How the choice of the parameters could change your conclusions? Why portioning surface and deep drainage runoff into 5% and 95%?

You are perfectly right and your questions helped us a lot to achieve a useful synthesis of the differences and resemblances between the two tested soil hydrology schemes. To this end, we largely rewrote Section 2 “Model description”:

- inclusion of a new Table (called Table 1) comparing the main features of the two soil hydrology schemes, cited in introduction of Section 2.2 “Soil hydrology modeling in SECHIBA”, now renumbered 2.3
- thorough rewriting of the two subsections devoted to the description of 2LAY and 11LAY

- inclusion of a final subsection 2.5 “Synthetic comparison of the two soil hydrology schemes”, discussing the relationships between (i) the soil hydrology schemes and their parameters, which are intimately linked, and (ii) the soil hydrology and routing schemes, especially in the case of the 2LAY, which calls for an arbitrary partitioning of total runoff into the input flows of the fast and slow routing reservoirs (also stated in the section describing the routing module). We specifically mention, at the end of subsection 2.5, that this choice “*has an impact on the relative contribution of these fast and slow reservoirs to TWS*”, and this point will be further discussed in the conclusion. This is a paragraph added in section 2.5 in relation to this point:

  “*In the present case, additional differences between the simulations arise from the way total runoff is transferred to the fast and slow reservoirs of the routing scheme, supposed to receive surface runoff and drainage, respectively. The 11LAY makes a clear physical distinction between these two fluxes, contrarily to the 2LAY, which only creates total runoff when the soil reservoir is full, with no clear surface or bottom localization, as in the bucket scheme of Manabe (1969). In this case, the routing scheme has always been used with a 5-95% redistribution of total runoff to the fast and slow routing reservoirs. In this paper, we follow this choice, steming from Ngo-Duc et al. (2007), which has an impact on the relative contribution of these fast and slow reservoirs to TWS (as analyzed in Sect. 4.2).*”

You use free gravity criteria for bottom boundary conditions for the 11 LAY. Is it really how it should work in the amazon?? I guess that in some regions, vegetation may access water from shallow aquifers.

You are right, using a free gravity drainage could be a limitation to simulate hydrology over the Amazon basin, in particular in northwestern Amazonia and floodplains elsewhere. By contrast, in southeastern Amazonia, where we find the largest effect between the two soil hydrology schemes, there are deep aquifers, and free drainage seems appropriate. Campoy et al. (2013) introduced a new boundary conditions in the 11LAY of ORCHIDEE, namely impermeable bottom, and negative drainage at the soil bottom to sustain a fixed water table inside the soil column. None of these configurations was not tested in our study over the Amazon basin. Note that this question of shallow water tables sustaining ET is tightly connected to the one of soil depth, raised by reviewer 1.

- <u>Routing model:</u>

The routing model explanation needs some clarification. For example, why using manning concept to deep drainage? Manning’s equation deal with channel flow and it has no relation to deep drainage flow. What do these velocities mean? Is it related to river-channel flow velocity?

The formulation of the topographic water retention index does indeed stem from an approximation of the Manning formula, proposed by Ducharne et al. (2003) for stream reservoirs only. In this framework, the effect of stream length and slope is explicit, and the one of channel roughness and cross-sectional shape is carried by a constant, called g in the routing scheme described in the present paper. This formulation has been generalized to the fast and slow reservoirs under the assumption that the tuning of the time constant compensates for the fact that the Manning formula is not designed for overland and groundwater flow modeling, and that the various parameters do not have the same values than in streams. For the stream reservoir, the time constant can be related to a stream velocity if the stream slope is known. We give an example in the paper: “*which leads to a stream velocity of around 0.5 m/s assuming a slope of 1%, both values being typical of large rivers.*”

Section 2.4 has been rewritten for the sake of clarity:

“Travel time within the reservoirs depends on a characteristic time scale, which is the product of a topographical water retention index k (in m) and a time constant g (in d.m^{-1}). The latter does not vary horizontally but distinguishes the three reservoirs, while the water retention index k characterizes the impact of topography on travel time in each sub-basin, and is assumed to be the same in the three reservoirs of a given grid cell, eventhough it derives from stream routing principles introduced by Ducharne et al. (2003). This travel time is thus assumed to be proportional to stream length in the sub-basin, and inversely proportional the square root of stream slope. This can be seen as a simplification of the Manning formula (Manning, 1895), where the time constant g compensates for the missing terms. The lengths and slopes are first computed at the 0.5°x0.5° resolution from the topographical map of Vörösmarty et al. (2000), then upscaled at the ORCHIDEE grid cell resolution, of 1°x1° in the present study (Sect. 3.1). The values of the time constants g were initially calibrated over the Senegal basin, using the 2LAY parameterization with the 5/95% partitioning of total runoff towards the fast/slow reservoirs, then generalized for all the basins of the world (Ngo-Duc et al., 2007). The stream reservoir has the lowest constant (0.24 d m^{-1}), which leads to a stream velocity of around 0.5 m.s^{-1} assuming a slope of 1%, both values being typical of large rivers. The corresponding velocities are lower in the other two reservoirs, with a time constant g of 3.0 and 25 d m^{-1} in the fast and slow reservoirs respectively. In former studies using the 11LAY, the time constants of these two reservoirs have been set equal to the one of the fast reservoir (g = 3.0 d m^{-1}) to balance a higher water residence time in the soil with the 11LAY (D’Orgeval, 2006; D’Orgeval et al., 2008; Gouttevin et al., 2012; Guimberteau et al., 2012a, 2013). In the present study, however, to restrict the difference sources to the soil hydrology schemes alone, we used the same set of time constants with both the 2LAY and 11LAY: g = 0.24, 3.0, 25 d m^{-1}, as defined by Ngo-Duc et al. (2007).”

Do you apply the same floodplain parameter for all grid cells? As flooding is variable in space and time in the amazon, the velocity constant of the floodplain reservoir should be variable as well. What is the impact of this simplistic assumption on the TWS results?

The residence time for the floodplain reservoir (and also for all the routing reservoir) depends on a time constant which, indeed, is constant in space, but depends also on a topographic water retention index which varies in space (see lines 16 to 21 page 83 and see page 915 in Guimberteau et al. (2012)). Thus, floodplain parameterization of ORCHIDEE enables a spatial variation of the water storage in the Amazon basin.

- <u>Discharge Validation:</u>

It would be interesting to provide an objective evaluation of model discharge time series versus observations.

Comparison between observed and simulated river discharges already exists in the paper with the Figure 7. For an objective comparison, we added skill scores in the new Table 3 in the Supplementary Material section.

- <u>GRACE TWS:</u>

GRACE Tellus released a new RL05 version. Check it there are important differences between RL04 and RL05 that could change your conclusions.

Thank you for this suggestion. We use now this new version of GRACE in the paper and all our results were updated in the text, figures or tables. The bias of the TWS amplitude between ORCHIDEE and GRACE RL05 becomes slightly higher over the Amazon basin. This is explained by the lower TWS increase in the Madeira basin, between February and April, in RL05 product compared to RL04. However, comparison of ORCHIDEE results with the new product of GRACE did not change our conclusions which were made with the RL04 products.

- Precipitation (P):
Why didn't you use your improved data set to run the model?

You are right, we could have used HYBAM dataset but we showed that Princeton's dataset corrected by GPCC is not far from HYBAM. This is also found by Getirana et al. (submitted in Journal of Hydrometeorology) who compared several LSMs simulations on the Amazon basin according to three different corrections on Princeton's precipitation dataset: GPCC, GPCP and HYBAM. HYBAM is shown to be the best product to represent water budget over the Amazon but results with GPCC are close to that with HYBAM.

- ET:
Several other ET global datasets are available. For example, Azarderakhsh et al (2013) looked at ET from 3 different datasets over the Amazon and the estimates do not agree between each dataset. So, why did you choose Jung et al. 2010 dataset? Why it is better than the others? Please clarify it in the manuscript.

We are aware that it exists other ET products and that Jung et al. 2010 dataset may not be the best one when compared to the others. But we used Jung et al's dataset for ET because the authors provided also GPP product with the same methodology. Thus, this two products are expected to be consistent with each other. We added in the Figure 7 (now Figure 6), ET results from 3 other products to show the spread existing between the ET estimations.
We also added some modifications in the text:

- section 3.2.2 "Evapotranspiration (ET) and Gross Primary Productivity (GPP)":

  "*Here, Jung et al.'s product is chosen to evaluate ET simulated by ORCHIDEE because it also provides a consistent GPP product. Uncertainties around this ET estimate is assessed by comparison with 3 other products: GLEAM-ET (Miralles et al., 2011), NTSG-ET (Zhang et al., 2010) and PKU-ET (Zeng et al., 2014).*"

- Conclusion:

  "*But ET observations uncertainties are of the same magnitude than the misfit between any of the schemes and the observations, so that a particular model scheme cannot be ruled out from these data only.*"

- Residual water balance:
The residual P-ET-Q over a basin equals the change in total water storage DS, including soil, ground water and rivers and floodplains. It is not clear how using shifted Q (Q*) makes that ground water and surface water storage can be neglected. Please clarify it.

You are right, P-ET-Q represents the change in total water storage DS and not only the change in soil water storage as written in the paper. Thus, we corrected in the text and in the caption of Figure 2.

- <u>TWS amplitude and phase assessment</u>:
Do you calculate the amplitude for each year and then average the results?

No, the amplitude is calculated by the mean seasonal cycle of TWS during 2003-2008 (average of monthly value during the six years then calculate amplitude, not calculate amplitude of each year then average).

If you simply use maximum and minimum values from the time series you can be more susceptible to errors due to noise in the data. You could work with percentiles, instead of maximum and minimum values. Or as you are fitting this cosine function, you could be computing the amplitude of TWS from the p coefficients.

I agree the reviewer's suggestion, the amplitude of TWS can be extracted from the p coefficient. In the new version of the manuscript, we updated the results of amplitude from the new definition as 2*p1.

- <u>Contributions to TWS variation:</u>
Some recent research (e.g. Paiva et al. 2013) show that most of TWS variability in the amazon is regulated by surface waters. I guess that your results should show more importance in the floodplain reservoir than the slow reservoir that is supposedly related to subsurface/groundwater flow. What is the reason for such difference? Is it because you are using a simplistic model that considers constant floodplain parameter in space?

Paiva et al. (2013) found that 56% of the Amazon TWS changes is governed by surface waters (corresponding to the sum of the stream, the fast and the floodplain reservoirs for ORCHIDEE), 27% by soil water and 8% for ground water (corresponding to the slow reservoir for ORCHIDEE) (we notice that one cannot find what correspond the remaining 9% !)

In our study, with the 11LAY of ORCHIDEE, we have these proportions: 35% by surface waters, 19% by the soil water and 46% by the groundwater. In ORCHIDEE, more importance is attributed to the slow reservoir in term of TWS contribution which is clearly in contradiction with the results of Paiva et al. (2013). However, uncertainties in storage contribution to TWS are large in the literature. Pokhrel et al. (2013) found that subsurface storage (soil water in the vadose zone and groundwater below the water table) contribution (71%) is far greater than surface water contribution (29%) to TWS changes. The large contribution of the groundwater to TWS variation is also found by the groundwater model of Niu et al. (2007).

Difference between our results and Paiva et al. (2013)'s cannot be attributed to the no-variation in space of the water in the floodplain reservoir of ORCHIDEE, as explained above. One of the uncertainties in ORCHIDEE could be the parameterization of the time constant g for the slow reservoir which has been calibrated over the Senegal basin and generalized for all the basins of the world. Re-parameterization of the time constants for the three routing reservoirs are being re-calibrated in ORCHIDEE.

In order to introduce a discussion dealing with surface or subsurface contribution to TWS in the Amazon basin, we modified:

- Section 4.2.1 "Seasonal variation":

"The annual amplitude in water storage in the slow reservoir, which collects drainage, is lower with the 11LAY (46% of the total annual amplitude of TWS) than with the 2LAY (66%). Sub-surface water

contribution (sum of the fast, slow and soil reservoirs) to TWS variation simulated by the 11LAY (71%) is in agreement with Pokhrel et al. (2013)'s estimations (71%) over the Amazon basin. The physical distinction between surface runoff and drainage with the 11LAY leads to a lower drainage contribution to the total runoff over the Amazon basin (∼ 69%), which is more realistic when compared to the estimations of Mortatti et al. (1997) (68.1%), than with the 2LAY (95%) (see Table 3 in Supplementary Material)."

- Conclusion:

"*By comparing the bucket model, the first property of the 11LAY leads to less drainage, which contribution to the total runoff over the Amazon basin is more realistic (69%) than the 2LAY (95%), when compared to the estimates of Mortatti et al. (1997) (68.1%). Less water is stored in the slow reservoir of the routing scheme (which represents a groundwater reservoir) with the 11LAY. We found the same contribution of subsurface water (including groundwater) to TWS over the Amazon basin (71%) than Pokhrel et al. (2013), and this result is also in line with Niu et al. (2007). However, the attribution of TWS to sub-surface versus surface water remains uncertain since other studies (Paiva et al., 2013) suggested that most of the TWS variability was regulated by surface waters.*"

- <u>ET results:</u>
I'm not sure how accurate the global ET estimates are and to which extent should we trust it. You should really compare it with other datasets. Also, if the data uncertainty is large, it is difficult to argue that 11LAY is better that 2LAY based on such small difference between model results if compared to differences to observed data and uncertainty from ET observations. Also, the vegetation model could not capture GPP and LAI dynamics. So, if the vegetation model is wrong, how can one clearly differentiate between the two soil formulations?

As said above, we added in the Figure 7 (now Figure 6), ET results from 3 other products to show the spread existing between the ET estimations.

- <u>Conclusions:</u>
Lines 4 to 6: This conclusion about differences in 11LAY and 2LAY is may be more related to the assumption of the 2LAY of portioning runoff as 5% surface runoff and 95% for deep drainage. This may be the cause of more water storage in the slow routing reservoir for the 2LAY. Consequently, it is difficult to say if the differences between the models are due to using 11 or 2 layers or due to all the others hidden assumptions of these models. This fact makes the study non conclusive.

In the subsection 2.5 "Synthetic comparison of the two soil hydrology schemes", we discussed the relationships between the soil hydrology and routing schemes, especially in the case of the 2LAY, which calls for an arbitrary partitioning of total runoff into the input flows of the fast and slow routing reservoirs (also stated in the section describing the routing module). In conclusion, we clearly distinguishes now the two properties of the soil models that give differences between the 2LAY and the 11LAY:

"The better simulation of the water budget and TWS with the 11LAY, in most of the sub-basins of the Amazon, owes to the combination of two of its properties: (i) the physical distinction between surface runoff and drainage and (ii) the physically-based description of soil water storage.

By comparing the bucket model, the first property of the 11LAY leads to less drainage, which contribution to the total runoff over the Amazon basin is more realistic (69%) than the 2LAY (95%), *when compared to the estimates of Mortatti et al. (1997) (68.1%). Less water is stored in the slow reservoir of the routing scheme (which represents a groundwater reservoir) with the 11LAY. We found the same contribution of subsurface water (including groundwater) to TWS over the Amazon basin (71%) than Pokhrel et al. (2013), and this result is also in line with Niu et al. (2007). However, the attribution of TWS to sub-surface versus surface water remains uncertain since other studies (Paiva et al., 2013) suggested that most of the TWS variability was regulated by surface waters.*

The second property of the 11LAY enables a higher water holding capacity by soils, resulting into a higher soil moisture level than in the 2LAY. Lower drought stress in the 11LAY scheme sustains ET, which suggests that soil moisture parameterizations are critical in LSMs over the southern part of the Amazon that has strong seasonality in precipitation and marked transition periods between wet and dry soils. Our analysis is being extended to the global scale with the objective of identifying whether differences in water budget components can be found in the transition zones identified by Koster et al. (2004a), where soil moisture is expected to influence precipitation."

- <u>MINOR COMMENTS:</u>

Section 2.1. What is the spatial resolution of the model?

ORCHIDEE can take different spatial resolution given the resolution of the forcing. The model takes the same spatial resolution than that of the forcing. Thus, in our study, it is 1°x1°.

Pg. 76. Line 15 The role of floodplains on the delay and attenuation of floodplains can be clearly seen in Paiva et al. [2013].

Thank you. The reference is added in the introduction:

"*The seasonality of Q is further modulated by floodplains (Paiva et al., 2013)*"

Pg. 77. Line 9 - 15 According to Costa et al., 2010, ET in the Amazon is driven mostly by radiation and not by soil water availability.

Indeed, Costa et al. (2010) found that ET is driven mostly by radiation in the Amazon. However, this was found only in wet equatorial sites. Costa et al's results were different in the seasonally dry southern tropical forests where ET seasonality is controlled with the surface conductance and thus with the water availability (response of the plants to water stress). We modified in the introduction:

" *In the Amazon basin, a particularly important land-atmosphere feedback is precipitation recycling (Shuttleworth, 1988; Marengo, 2006), which is affected by soil moisture in the southern parts of the basin, as they experience a marked dry season, during which soil moisture availability limits ET.* "

Table 5. Present the observed amplitude and error as %. Use % along the text as well.

Corrected in the text.

Figures. All the figures showing spatial results should be reviewed (4 and 6). The amazon basin domain seems to be cut close to the boundaries. For example, the northern part of Negro river basin is not shown in the figures. Is this affecting results from tables 4 and 6, for example?

We modified the Figure 4 (now Figure 1 in Supplementary Material) and Figure 6 (now Figure 5). The spatial results are now shown over all the northern South America and the Amazon basin boundaries were added. The results are given on average over this Amazon domain. However, little difference of the domain cut with reality does not significantly affect the results.

Figure 4. It seems that large amplitude errors are concentrated along the Amazon floodplains (floodplains at Solimoes /Amazon river, Madeira River and Bolivia). These errors are compensated in other regions. Maybe it is caused by model limitations in representing floodplain storage. For example, a previous section says that the model uses a constant (in space and time) floodplain related parameter. Such assumption may be causing these large errors.

As we explained above, floodplain parameterization of ORCHIDEE enables a spatial variation of the water storage in the Amazon basin. The underestimation of the the maximal fraction of flooded areas (MFF), and thus the overestimation of the water level amplitude in the floodplain reservoir, could explain the large amplitude errors concentrated along the Amazon floodplains that we obtained in this study. This has been previously found with ORCHIDEE by Guimberteau et al. (2012). Over the main stem of the Amazon, they have shown an overestimation in water height level, even after the calibration of the time constant of the floodplain reservoir (simulation ORCH4, page 931). They attributed this error to an underestimation of the MFF used in ORCHIDEE, when compared to Hess et al. (2003), even after using a better map of MFF.

Figure 3. Please provide a figure with higher resolution.

We will contact the team from the production office of GMD journal to improve the resolution of the Figure 3.