Anonymous Referee #1

We first thank the reviewer for his very insightful comments, which helped us a lot to clarify and improve the paper.

<u>General comments</u>:

This paper compares two versions of the ORCHIDEE land surface model over the Amazon basin, focusing on the hydrological and to phenological impacts. These two versions do not differs only by the use of two different soil hydrology, as the title would have us believe, but also in the parameterization of the river routing module (page 83, line 25-27). It is therefore difficult to clearly attribute the very very slight difference between the two versions only to soil module. In addition, this paper is very long and very descriptive. This article would be clearer if the sections 3 and 4 were reduced or if only the ORCHIDEE 11LAY was used. Finally, I regret that there is no direct comparison with time-series of observed discharge. Principally for all these reasons, I propose that this article should be in major review.

<u>Major comments</u>:

As already mentioned, the two versions of ORCHIDEE do not differs only by the use of two different soil hydrology, but also in the parameterization of the river routing module. It is very surprising that the river routing module depends on the soil hydrology and then requires a specific tuning for the both versions. Why that ? Is it physical ? I don't think, these two module should be independents. Please clarify this fact and use the same river routing module (and the same tuning) to compare your simulation. If not, then you can change the title and the history of your paper because to focus only on soil module difference will not be justify, especially for seasonal TWS results.

There has been a misunderstanding about the parameterization of the river routing module. Both soil models are coupled with the same river routing module, with the exact same parameters. The focus of the paper, which is to compare results from the two hydrological schemes, is thus justified. Yet, because of this misunderstanding, and to answer questions from Reviewer 2, we completely rewrote Section 2.4 "River routing module". The main point is that : "*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, as defined by Ngo-Duc et al. (2007).*"

This remark also leads me to wonder if this comparison between two soil modules in ORCHIDEE is not vein. It is now well know that multi-layer schemes are superior to old bucket schemes.

Despite many advantages of multilayer schemes to implement processes that depend on soil properties or soil moisture profiles (soil infiltration and surface runoff generation, root water uptake for transpiration, water table coupling, surface soil moisture assimilation), there have been very little studies on the practical difference between conceptual bucket-type models and multilayer models, when looking at the simulated water fluxes involved in the terrestrial water budget. This is the starting point of our work, and it has been made clearer in the Introduction:

"*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).*"

In addition, because this paper is very long and generally very descriptive, it looks more like a report than a scientific article with a clear message. This report is certainly very interesting for your colleagues in your laboratory, but is it the case for the entire community ? Perhaps your work would benefit to focus only on the ORCHIDEE 11LAY. This would be an effective way to shorten this work. Whatever your choice, this article would be clearer if the sections 3 and 4 were reduced.

Given the agreement between the two reviewers about the paper's length, we worked a lot to reduce it. We kept the same focus of the paper ie the comparison of 2LAY/11LAY, but we reduced the length in many places, and more particularly in the sections dealing with description of the results. The corrections are highlighted in the revised manuscript. We put also 4 tables (included a new one called Table 3) and 2 figures in the Supplementary Material section.

Another major comment is that there is no comparison between observed and simulated river discharges while daily observations exist over this basin in the HYBAM database (Guimberteau et al. 2012). For me, annual comparison is not sufficient and some skill scores, like nash criterion and/or deseasonalized root mean square error, should be used as in Guimberteau et al. (2012).

Comparison between observed and simulated river discharges already exists in the paper with the Figure 7. The time step used here is monthly. Daily outputs have not been written during the simulation process. But you are right, an objective comparison is missing. Thus, we added skill scores in the Table 3 in the Supplementary Material section.

Minor comments:

Page 76, Line 16-17: **Are you sure that TWS plays an important role in regulating the global climate ? TWS is it more or equally important than the ocean? Me, I am not sure. This sentence is not adequate. TWS plays a non negligible role in modulating (and not in regulating) the climate in some regions but certainly not the global climate.**

You are right, this sentence is not adequate. We removed this sentence.

Page 77, Lines 23-25: **The paper of De Rosnay et al. (2002) can be applied to ORCHIDEE, but is it universal ? Please add more references to this affirmation or delete it.**

We believe that the results obtained in one LSM can be generalized, at least partially, to other LSMs, if the methods are clear, and this is the main justification of our paper. Yet, generalization is stronger if similar studies conducted with other LSMs give similar results. To this end, we added more references dealing with comparison between multilayer schemes and old bucket schemes in the Introduction:

"*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).*"

Page 77-78, Lines 28-1: **According to previous remarks, this question is not addressed in this paper because the routing module is not the same according to soil module. So improve your article or delete this sentence.**

We kept this sentence because we do use the exact same routing module, with the exact same parameters, for the two soil hydrology schemes, as explained above.

Page 81, Line 26: **The fact that ORCHIDEE uses only a soil depth of 2m appears not realistic. Observations of root depth over tropical forest shows that this depth is much close to 6-8m (Canadell et al., 1996: Maximum rooting depth of vegetation types at the global scale, Oecologia, 108, 583-595). Please discuss about that in your paper. If you choose to rewrite this article in focusing only on ORCHIDEE 11 LAY, it should be interesting to test one version of your model with such soil depth. If not, please discuss about that in your paper even if it is difficult to justify that roots of tropical forest stop to only 2m depth.**

You are right, several field studies have shown that roots are occurring much deeper than 2 meters. (8, 10, 12 meters depending on the location). Canadell et al. (1996) suggested an average rooting depth of 7.3 m, based on 5 measurements with a maximum of 18 m. Moreover, several modeling studies showed that a deep soil (and deep roots) is needed in models in order to represent realistic ET and GPP during the dry season (e.g. Baker et al. 2008, Verbeeck et al. 2011). This kind of discussion already existed in the conclusion of our study (lines 2-8 page 103). We agree that taking only 2 meters for soil depth for the entire basin is not realistic. On the other hand, taking a deeper soil (eg 8 meters) for the entire amazon is not realistic as well. In the south west of the basin, in the Jarú fluxtower site, there is a much shallower soil and roots only up to 3.5 meters. Rooting depth changes spatially. The problem is that we do not have good spatial information on soil depth over the whole Amazon basin.

We have tested the impact of different soil depths in ORCHIDEE. We attached to this report the spatial results of ET simulated in JJA, for a 2-m soil depth (upper left) and a 8-m soil depth (upper right). Differences between both are also illustrated by the two maps on the bottom. ET variation is negligible when soil depth is prescribed to 8 meters over the Amazon basin. Only small increase (+0.4mm/d) occurs in some small regions in the south. We changed the text in the conclusion to introduce more discussion dealing with soil depth uncertainties:

"*More attention should be also paid to the soil depth, which was fixed to 2 meters for the entire basin in both soil hydrology schemes, given the lack of geospatial information across the entire basin. Several field studies showed that roots can be present much deeper than 2 meters. For tropical evergreen forest, Canadell et al. (1996) estimated an average rooting depth of 7.3 m, and a maximum of 18 m, based on data from 5 sites. Deep roots observed by Nepstad et al. (1994) in northeastern Pará enable evergreen forests to maintain dry-season ET (Verbeeck et al., 2011) which feeds back on climate (Kleidon and Heimann, 2000). Several modeling studies concluded that deep soils and deep roots are needed in models, in order to represent realistic ET and GPP in Amazon forests during the dry season (e.g. Baker et al., 2008; Verbeeck et al., 2011). With the 2LAY, Verbeeck et al. (2011) showed that the soil depth had a significant effect on the seasonal cycle of water fluxes. We tested a soil depth of 8 meters in the 11LAY but found only a negligible effect owing to high soil water holding capacity in the 11LAY.*"

Table 6: **a Taylor diagram could be used instead of this table.**

Table 6 is put now in the Supplementary Material section (called Table 4) in order to reduce the length of the paper.