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Interactive Comment

Interactive comment on "Two soil hydrology formulations of ORCHIDEE (version Trunk.rev1311) tested for the Amazon basin" *by* M. Guimberteau et al.

Anonymous Referee #2

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Review comments on the paper "Two soil hydrology formulations of ORCHIDEE (version Trunk.rev1311) tested for the Amazon basin" by M. Guimberteau et al.

- 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).

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Interactive Discussion
Discussion Paper

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

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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. 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.

- Model description:

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

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draingage runoff into 5% and 95%? 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.

- Routing model:

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? 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?

- Discharge Validation:

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

- GRACE TWS:

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

- Precipitation (P)

Why didn't you use your improved data set to run the model?

- 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.

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- 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. Amplitude and phase assessment: Do you calculate the amplitude for each year and then average the results? 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.

- Contributions to TWS variation

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?

- ET results:

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?

- Conclusions: Lines 4 to 6: This conclusion about differences in 11LAY and 2LAY is

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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.

- MINOR COMMENTS:

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

Pg. 77. Line 15 The role of floodplains on the delay and attenuation of floodplains can be clearly seen in 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.

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

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?

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.

Figure 3. Please provide a figure with higher resolution.

- References

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