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Interactive comment on "Modelling sub-grid wetland in the ORCHIDEE global land surface model: evaluation against river discharges and remotely sensed data" *by* B. Ringeval et al.

Anonymous Referee #1

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This paper discusses some modifications to certain parameterizations governing the hydrological cycle in the ORCHIDEE land model. Phase change of soil water is added, which leads to a modification of the infiltration parameterization. Also, some TOPMODEL concepts are included to diagnose the saturated area within a gridcell. Comparisons are made between modeled and observed river discharge and wetland extent.

The value of this paper is hard to assess for two reasons. First, the parameter FMAX, which is central to the validation of the model against indundation extent (P10), is never defined in an equation. On page 692, it is defined as the "fraction at maximum soil water content within each gridcell". This would seem to be temporally constant, as

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there is only one maximum value of soil water content. Yet at the same time, FMAX is said to be "a function of the average soil moisture" which would vary in time. An equation defining FMAX would help correct this confusion.

Because the magnitude of FMAX is much larger than inundated areas from the P10 dataset, a second variable, FWET, is introduced. The authors also fail to define FWET. It is tuned to match P10, but it is not clear how. For example, on page 697 it says "the constant c has been optimized to obtain a annual global Fwet close to the global annual P10 +29 %". But then it says "yearly global Fwet is equal to 3.4 %". Again, I do not understand these statements.

The second reason why I am concerned that this paper may not benefit the land surface / hydrological modeling community is that the modifications either degrade the simulation or do not improve the match to observations. From figure 3, it appears that the ORCHIDEE-TOP simulations all degrade the simulation of river discharge, except perhaps for the Ganges. This implies that the model runoff is unrealistic, and therefore the soil infiltration is also unrealistic. One would then question the quality of the soil moisture simulations, which (I believe) directly affect the calculation of FMAX and FWET. The comparisons between P10 and FWET agree in a few places (north-central Siberia, eastern Canada, and India) but also disgree strongly in NW Canada, SE United States, and Brazil. This implies that the processes governing the spatial distribution of the modeled wetlands are not well understood. If that is true, why should the reader have confidence in the model's ability to simulate future changes in wetlands, and the possible GHG emissions from those wetlands?

In summary, I think the paper would be improved by better defining the important quantities FMAX and FWET. Secondly, while the addition of the TOPMODEL parameterization allows the model to diagnose saturated areas, it is not clear from these results that the reader should have confidence in the simulations from the new model, given the degradations in performance in river discharge, and the significant biases in the FWET spatial distribution.

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5, C66–C68, 2012

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5, C66–C68, 2012