

Reply to comments of referee #1

**We thank the referee for his/her comment on our manuscript “Modelling sub-grid wetland in the ORCHIDEE global land surface model: evaluation against river discharges and remotely sensed data”. In the following paragraphs, we provided detailed answers to all his/her comments. We repeat the specific comments of the referee before our answers (bold font).**

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 inundation 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 here 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.

**1.1) Almost all the section 2.3 has been rewritten to be clearer on how the variable Fmax is computed.**

**Indeed, Fmax is a function to the average soil moisture computed by ORCHIDEE thus Fmax varies in time. We have added new equations (equations (3) and (4) in the revised version). Briefly, for a given grid-cell, ORCHIDEE computes a soil moisture at each time-step. This means soil water content varies between wmin and wmax. The ORCHIDEE-computed soil moisture is re-distributed into the grid-cell depending on the sub-grid topographic index distribution and following some concepts of the TOPMODEL approach. This re-distribution of the soil moisture into the grid-cell allows us to compute the fraction of the considered grid-cell which reaches wmax. We pasted below (in blue) the rewritten part of the section 2.3.**

**Pages 692; lines 4 to Page 693; line 2 of the current version of the manuscript are replaced in the revised version by (in blue):**

We follow the approach of [Decharme et al., 2006; Habets and Saulnier, 2001] for the ISBA model and [Gedney and Cox, 2003] for the MOSES model to describe a subgrid soil moisture distribution into ORCHIDEE using TOPMODEL concepts [Beven and Kirkby, 1979]. Following [Decharme and Douville, 2007] we also incorporate the bias correction of [Saulnier and Datin, 2004]. This defines the ORCHIDEE-TOP version, which we apply globally and evaluate in this study (Fig. 1) and which allows us to compute at each time-step the fraction of each grid-cell which reaches  $\omega_{\max}$ , further noted  $F_{\max}$ .

TOPMODEL was initially developed at river catchment scale. It attempts to combine the distributed effects of channel network topology and dynamic contributing areas for runoff generation [Beven and Kirkby, 1979; Sivapalan et al. 1987]. This formalism takes into account topographic heterogeneities explicitly based on the spatial distribution of a topographic index  $\lambda_i$  (m), defined at the pixel scale as follows:

$$\lambda_i = \ln(a_i / \tan\beta_i) \quad (2)$$

where  $a_i$  (m) is the drainage area per unit of contour of a local pixel,  $i$ , and  $\tan\beta_i$ , the local topographic slope, approximates the local hydraulic gradient where  $\beta_i$  is the local surface slope. If a given pixel in a catchment has a large drainage area and a low local slope, its topographic index will be large and thus, its ability to be saturated will be high.

TOPMODEL gives a relationship between the mean water deficit in a catchment ( $D_t$ ), the local deficit of a given pixel ( $d_{i,t}$ ) into the considered catchment and the topographic index:

$$D_t - d_{i,t} = -M (\lambda_m - \lambda_i) \quad (3)$$

where  $M$  (m) is a parameter describing the exponential decrease of the soil transmissivity with local deficit and  $\lambda_m$  is the mean topographic index over the catchment. For a given mean deficit over the catchment ( $D_t$ ), a threshold topographic index  $\lambda_{th}$  can be diagnosed in such a way that all pixels with a local topographic index  $\lambda_i > \lambda_{th}$  have no local deficit ( $d_{i,t} = 0$ ).

Then, a fraction of the catchment, noted  $F_{max}$ , defined by the pixels with no water deficit can be estimated from the partial integration of the spatial distribution of the topographic index in the catchment, noted  $\delta$ :

$$F_{max} = \int_{\lambda_{th}}^{\lambda_{max}} \delta(\lambda_i) \cdot d\lambda_i \quad (4)$$

The coupling between TOPMODEL and ORCHIDEE assumes that the relationship between local soil moisture, mean deficit and topography holds within each grid-cell at the LSM resolution [Gedney and Cox, 2003]. It requires the estimation of the grid-cell mean deficit from variables computed by ORCHIDEE. Following [Decharme et al., 2006], we consider that the grid-cell average deficit ( $D_t$ ) and the soil moisture computed by ORCHIDEE ( $\omega_{soil}$ ) are proportional for each time-step, so that the grid-cell average deficit  $D_t$  can be simply expressed as:

$$D_t = ((\omega_{max} - \omega_{min}) - \omega_{soil}) \cdot h_{soil} \quad (5)$$

Here,  $h_{soil}$  is the ORCHIDEE soil depth and  $D_t$  is computed as a deficit with respect to the maximum soil water content  $\omega_{max}$ . As a result,  $F_{max}$  corresponds to the subgrid fraction at  $\omega_{max}$ , and it varies at each time-step, being inversely proportional to the grid-cell mean deficit  $D_t$  deduced from the soil water content  $\omega_{soil}$  computed for each time-step by ORCHIDEE.

Following [Decharme and Douville, 2007], we also incorporate the bias correction of [Saulnier and Datin, 2004]. This correction leads to more complex relationships than given here by equations (3) and (4) for the sake of simplicity. All details can be found in [Decharme et al., 2006].

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.

## 1.2) We agree with the reviewer and improved the part of the manuscript dealing with the definition of Fwet in the revised version.

**Pages 697 Lines 4-5 and 9-16 of the current version of the manuscript are replaced in the revised version by (in blue):**

In the aim to simulate wetland extents that are compatible with P10, we introduce a global parameterization in order to deduce calibrated wetland fractions (Fwet) from fractions at maximum soil water content (Fmax).

[...]

We performed a shift of the topographic index distribution by modifying the topographic index in all grid-cells:

$$\lambda' = \lambda + c \quad (7)$$

where  $c$  is a global constant. This leads to modify the sub-grid topographic index distribution, called hereafter  $\delta'$ . In the idealized case in which the equations of the Section 2.3 are given, i.e. without the bias correction of [Saulnier and Datin, 2004], the wetland fraction,  $F_{\text{wet}}$ , would be computed similarly to the Equation (4):

$$F_{\text{wet}} = \int_{\lambda_{\text{th}}}^{\lambda_{\text{max}}} \delta'(\lambda_i) \cdot d\lambda_i \quad (8)$$

$c$  has been optimized to obtain an annual global  $F_{\text{wet}}$  close to the global annual P10 +29%, i.e. P10 + the estimate of drained wetland extent since the pre-industrial period [Sterling and Ducharne, 2008] This leads to a global “pristine” wetland fraction about ~3.2%. With  $c$  (unit in  $\ln(\text{m})$ ; see [Ducharne, 2009]), the yearly global  $F_{\text{wet}}$  is equal to 3.4% while the mean annual  $F_{\text{max}}$  fraction over 1993-2004 is 9.7%.

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 disagree 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?

**1.3) We agree with this comment. The new parameterisation actually degrades the streamflow performance at many catchments. These shortcomings are openly presented in the results. They are also discussed in the section 6.2.1. In particular, limiting the effect of the coupling with TOPMODEL on the ORCHIDEE modeled soil water budget are presented and discussed.**

**We agree also with the existence of some biases in the simulated wetland extent. Again, we tried to give assumptions to explain these biases in the discussion (mainly section 6.2.1). Focusing our evaluation on the time variability in large world regions allows us to circumvent these limitations. The confrontation of the simulated dynamic against the observed variability is allowed through the Papa et al. data, which is an additional value of our work.**

**The limitation of the use of such modeled wetland extent to simulate CH4 emissions are discussed in the section 6.2. Given the fact that variability in time (and in particular the year-to-year variability) of the wetland extent seems to play a key role in the year-to-year variability of the wetland CH4 emissions (Ringeval et al., 2010, GBC and Bloom et al., 2010, Nature), we discussed also the possibility to use comparison of modeled wetland CH4 emissions with top-down results to indirectly evaluate the time variability of the simulated wetland extent.**

**As underlined by the 2<sup>nd</sup> referee, few approaches for determining wetland extent are published yet and we still think, despite the above cited limitations, the manuscript is a valuable contribution.**

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.

**Please, refer to the above reply.**

