Reply to comments of referee 2

Thanks to the referee for his comments on our manuscript. As he refers to the issues raised by referee 1 we likewise refer to our reply to referee 1 considering our approach, the relevant equations and separation of wetland types. In the following we will focus on the explicit comments of referee 2 (again in bold font with our reply in normal font).

I was not able to relate Figure 1 to the quantities in equation 1. It would be really useful to have the terms of equation 1 clearly shown in Figure 1.

The terms of Eq. 1 are explained in the text. We wrote their full names instead of the abbreviations in the figure to simplify it. Of course, we can mention the abbreviations there as well.

Page 410, line 12. Please explain exactly how "the degree day approach" works.

The degree-day approach is a very common approach and used by many hydrological models (see table 1 in Haddeland et al., 2011). Based on this approach the Max Planck Institute – Hydrology Model (MPI-HM) calculates the daily snowmelt depending on the actual temperature scaled by a sine factor. However, we would not want to give a very detailed explanation of this approach in our manuscript. It is a standard component in the MPI-HM and neither especially related to the dynamical wetland extent scheme (DWES) nor did we change its implementation during the development of the DWES. However, we can give Rango and Martinec (1995) as a reference if a more detailed explanation is required.

Page 410, line 20. It is unclear to me what does "reduction of 10% in the maximum drainage" actually means. Drainage usually depends on the hydraulic conductivity of the soil. Is that what is suggested here? Also, since the wetland fraction is dynamic does this mean that an area of a grid cell has different hydraulic conductivity when it is a wetland and when it is not.

The drainage follows the parametrization of Dümenil and Todini (1992). For a completely saturated soil drainage reaches a certain maximum value. The drainage in wetlands is set to 10% of the maximum value for the standard grid cell. As a result the maximum drainage of the respective grid cell fraction changes when wetland extent is changing. We parametrized wetland drainage in such way because low hydraulic conductivities are often observed in wetland soils. We are aware that soil types do not change their texture such fast. We are using this simplification to account for the low drainage conditions in the wetland water balance in absence of highly resolved soil type information.

Page 411. In equation 2 is I__gb the sum of lateral inflow the sum of inflows from all the up stream grid cells. Please explain the function of parameter z in this equation. It seems that this parameter reduces the lateral inflow into a grid cell when a larger fraction is covered by wetlands.

I_{gb} is the total lateral inflow into a grid cell at a certain time step. This inflow originates from upstream grid cells and is routed to the actual grid cell. I_{lat} is the part of the total inflow that is routed into the wetland reservoir. The remainder is routed into the river flow reservoir. The total inflow is not changed by this formula. It only determines how the total lateral flow is divided into wetland inflow and inflow into the riverflow reservoir. Please find the detailed explanation for this equation and the exponent z on page 4 in the reply to the first referee.

The parameter c in equation 5 and the parameter C in equation 9 are confusing. Please use a different alphabet in any one of these equations.

Thanks for this advice. We will change the parameter names accordingly.

Page 411. Lines 15-17. The solution in figure 2 looks like a solution to two equations. It would be useful to tell explicitly what these two equations are. In addition, is there an analytical solution to these equations.

The blue curve is the sum of all inflows IF into the wetland fraction f_w of grid cell. It is calculated as $IF(f_w) = P(f_w) + I_{lat}(f_w)$. The green curve is the sum of all outflows OF from the wetland fraction of the grid cell. Its equation is $OF(f_w) = ET(f_w)+D(f_w)+O_{lat}(f_w)$. The precipitation P, the evapotranspiration

ET and the drainage D are linearly related to the wetland fraction. The more complex shape of the curve (and thus the existence of a intersection of the curves that indicates the stable wetland extent) is caused by the lateral inflow I_{lat} and the lateral outflow O_{lat}. Both variables depend in a non-linear way on the wetland extent as shown in the equations 2 and 3 in the manuscript.
This is the way how we parametrize this behaviour in the model. In this respect we do not fully understand what the reviewer is meaning with an analytical solution.

Page 412. Line 8. This might be obvious but what is the resolution of the GTOPO30 data set.

It is 30 arc seconds which equals about 1 km horizontal resolution at the equator. We will add this information to the text.

Equation 6 is described as "actual sub-grid slope s for a given grid cell fraction f". However, looking at figure 3 it seems a more appropriate description of s(f) is that it is the fraction of a grid cell that slope less than s. In essence then s(f) is like a cumulative distribution function.

Thanks for the more appropriate description. Eq. 6 works like that but we are looking at it exactly the other way around. We first sort the slope values and then evaluate s(f) to get the maximum slope that occurs for a given grid cell fraction.

Is S__sl a tune-able parameter?

Yes, we optimized it in the second step of our optimization procedure (page 414 in the manuscript).

The simulated river discharge of all simulations was compared to observations from the Global Runoff Data Centre (2011)." Please reword this sentence to say clearly what was done since discharge cannot be compared to runoff. As you know runoff needs to be routed to convert it into discharge.

The GRDC provides time series of discharge for about 3800 stations. We will include this information in the sentence to make clear that we do not compare discharge to runoff.

Page 414. Line 16. "... and, finally, averaged over all river basins". How many river basins?

We use 97 river basin that are represented in the MPI-HM in sufficient quality (the criteria are explained in the following sentence in the manuscript). We will include the number in the paragraph.

Page 416. Lines 6-8. " The model computes increased wetland fractions mostly for the same regions which are wetland focus regions in the observations." I am unable to follow this sentence, please consider rewording.

We will rephrase this complicate sentence. We mean that the wetland observation data and our simulation results agree on the location of large scale wetland clusters.

Page 416. Line 21. I do not think it is right to call the mean of observations as "ensemble-mean". Simply call them "mean of observation-based estimates".

We agree that “ensemble” might indicate a more complete and uniform data set collection and will follow the referee’s advice.

I am confused about the role of snow cover when comparing simulated and observation-based wetland fractions. I am still unclear what does "wetland below snow cover" actually means? Wouldn’t those wetlands be frozen anyway? How does the inability of the model to account for freezing affect its results? The follow-up discussion (page 418, lines 12-13) says that the decreased wetland fractions during DJF are due to decreased wetland inflow during the cold season. Is this a precipitation effect? What about the effect of temperature?

The Papa et al. (2010) satellite observation data do not include snow covered wetlands. Before we can compare area means of their and our data we have to apply their snow mask to account for missing data. We agree that the observed wetlands are most probably frozen when covered by snow. Thus, they might

have a constant extent which could be used for comparison. However, we decided to restrict our analysis to months with actually include observation data instead of filling the gaps with constant or interpolated values.
As the MPI-HM does not compute an energy balance, freezing and melting processes have to be parametrized in a simple way. With the virtual snow layer we allow for snow accumulation on top of the wetland as soon as the snowfall occurs. This snow is melted using the degree-day approach as it is done by the MPI-HM for the remaining grid cell fractions. During this time the vertical water inflow into the wetland is restricted to melt water from the snow storage. Thus, the decrease in wetland extent is a direct result of low temperatures and not precipitation change. Since we cannot freeze the wetland, all wetland processes are still active in the DWES. Thus, we expect to loose some water during the snowfall period due to drainage and lateral outflow. This would enhance the already existing wetland extent decrease.

Page 420. Lines 12-14. Why does the mean flow increases for the Nile, Sao Francisco and the Colorado rivers?

Because for those catchments the water transport in wetlands is more efficient than via overland flow. Please find the more detailed explanation in our manuscript on page 423 line 3 - 10.

Page 421. Lines 21-22. " In our study we concentrate on hydrological feedbacks between wetlands, the atmosphere and the ocean." Since this is an off-line study I do not think you can say that your study concentrates on feedbacks between the wetlands, the atmosphere and ocean.

Our motivation was to estimate whether feedbacks caused by the wetland schemes would be relevant for a coupled Earth System Model (ESM). As both referees pointed out the term feedback leads to some confusion here therefore we will follow referee 1's suggestion and will investigate the impact of wetlands on the components of the hydrological cycle instead.

Page 422. What does "vegetation skin reservoir" means?

This is the canopy water storage. In vegetated areas it is needed to be filled by precipitation before throughfall reaches the ground.

Page 422. Lines 15-16. " Eventually, this results in a wetter and cooler state of the surface and therefore in a stabilization local climate conditions". This vague statement appears completely redundant.

We disagree with this judgement. Although our offline study cannot provide a more specific statement it is an important conclusion which we hope to demonstrate when our model is implemented into an ESM. However, we will rephrase the sentence to: . . . the wetlands will likely stabilize local climate conditions if implemented into an ESM.

Page 423. Lines 1-2. " Here, about 530 km3a-1 less water reach the oceans." Compared to what?

Compared to the ocean inflow in the control simulation. The whole section is concerned with the comparison of our dynamic wetland simulation to the control simulation as stated in the first paragraph of it (page 421, line 22 - 23).

Figure 2. What are the units on the y-axis?

Figure 2 is a conceptual figure that demonstrates the area dependencies. It is based only on the presented equations and not on model data. In this example the water volume flux would correspond to $m^3 s^{-1}$.

Figure 4. What are the units on the x-axis? It would be really easy to interpret this figure is the slope curve (similar to figure 3) was shown for this grid cell. In the caption of the figure the phrase "the wetland covered slope" is unclear.

The x-axis shows the model time step of 1 day. The caption indicates the mean slope of the grid cell fraction that is covered by wetlands. We agree that it is quite hard to understand this figure and will simplify it or replace it with another example of this process.

Figure 6. Does this figure show a maximum wetland fractions? If I am not wrong most observation-based estimates are of the maximum wetland fraction.

We thank the referee for this advice. As written in the caption we show the simulated mean wetland extent but as most observations refer to maximum wetland fraction we will change our analysis to the same measure. In addition, we will not compare to the mean of observation based estimates anymore but follow referee 1's suggestion to compare our results to the wetland observations separately.

Figure 7. Earlier in the manuscript the mean of observation-based estimates has been called "ensemble mean" and in this figure it is being called SIND. Please use consistent wording.

That is because we SIND is used as abbreviation for the Papa et al. (2010) data which is the only data set containing monthly inundation data. In this figure we do not compare to the ensemble mean but to the one data set only.

References

Dümenil, L. and E. Todini (1992). A rainfall-runoff scheme for use in the Hamburg climate model. In: *Advances in theoretical hydrology - a tribute to James Dooge.* Ed. by J.P. Kane. 1 European Geophysical Society Series of Hydrological Sciences. Amsterdam: Elsevier Science, pp. 129–157 (cit. on p. 1).

Haddeland, I., D.B. Clark, W. Franssen, F. Ludwig, F. Voß, N.W. Arnell, N. Bertrand, M. Best, S. Folwell, D. Gerten, S. Gomes, S.N. Gosling, S. Hagemann, N. Hanasaki, R. Harding, J. Heinke, P. Kabat, S. Koirala, T. Oki, J. Polcher, T. Stacke, P. Viterbo, G.P. Weedon, and P. Yeh (2011). Multimodel estimate of the global terrestrial water balance: Setup and first results. *J. Hydrometeorol.* 12(5), pp. 869–884. DOI: `10.1175/2011JHM1324.1` (cit. on p. 1).

Papa, F., C. Prigent, F. Aires, C. Jimenez, W.B. Rossow, and E. Matthews (2010). Interannual variability of surface water extent at the global scale, 1993-2004. *J. Geophys. Res. D Atmos.* 115(12), p. D12111. DOI: `10.1029/2009JD012674` (cit. on pp. 2, 4).

Rango, A. and J. Martinec (1995). Revisiting the degree-day method for snowmelt computations. *Water Resour. Bull.* 31(4), pp. 657–669. DOI: `10.1111/j.1752-1688.1995.tb03392.x` (cit. on p. 1).