Hydrol. Earth Syst. Sci. Discuss., 9, C589–C599, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C589/2012/


Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Development and validation of a global dynamical wetlands extent scheme" *by* T. Stacke and S. Hagemann

**Anonymous Referee #1**

Received and published: 28 March 2012

* General comments

The "The development and validation of a global dynamical wetlands extent scheme" manuscript deals with an important issue, namely the representation of the wetland extent variability in both time and space in land surface models. This is a relevant scientific question, for instance because wetlands represent a large natural source of $CH_4$ and because it has been shown the wetland extent dynamic contributes to the variability in the $CH_4$ emissions at different time scales. The authors propose very simple parameterizations to represent the wetland dynamic. Evaluating the possibility to use such a simple method rather than more sophisticated hydrological models like the commonly used TOPMODEL (Gedney et al, 2004; Kleinen et al., 2001) is an interesting subject in the scope of HESS. In particular, the authors do not account for the diversity in wetlands (no difference in the modeling treatment for floodplains and saturated wetlands). Evaluate if such a simple concept is able or not to represent rather well the global distribution of wetlands could represent a substantial contribution to scientific progress. However, the scientific approach and the applied methods are not well described in the draft. Lots of details are missing, many points are not clear (problematic, methods) and this makes the evaluation of the scientific reasoning difficult. This is particularly true for the methods and the optimization procedure where the choices made by the authors are not well justified. Moreover, I also found that the presentation of the paper is not clear. The problematic is not well defined (e.g. the fact that the authors do not account for the diversity in wetland, which is both an interesting point of view and a disadvantage, is not clearly explained). I do not find the methods description and results are sufficient to support the interpretation and conclusions.

* Specific comments:

1) I find the scope of the study is not clear. From an hydrological point of view, a distinction/categorization of the wetland types in two main classes could be done: i) the wetland generated by an increase of the water table depth from below, i.e. wetlands resulting from vertical water fluxes (peatlands, swamps, marsh; hereafter called ”saturated wetlands”) and 2) the floodplains, i.e. wetlands associated with a river and resulting from horizontal flows.

The main interest of the paper is to try to simulate these two kinds of wetlands with a common approach and to test if this latter could reproduce with success the big patterns of the wetland distribution at the global scale. Some indications let think it is also the aim of the authors, e.g.:

- p409;l14-15 “Both parts share a joint water storage” with “both parts" referring to the land surface model part computing the vertical water fluxes and a state of the art river routing model.

- p412;l1-5: The authors do a distinction based on the topography between areas



HESSD
9, C589–C599, 2012

where the “water is distributed over a large plain” .vs. areas where a “new water volume is need to raise the water table”.

But this aim is not clearly stated in the draft. Thus, the reader wonders if the authors model floodplains, saturated wetlands or the both until the end of the Method section. I found the problematic is not well described in the draft.

In relation to the above comment, the authors need to be much clearer on their definition of the “wetland” term. For instance, it appears that, in addition to their no-distinction, the authors simulate only wetland with a free surface water. p409;l20: “Wetlands without surface water are not accounted for.” (it is also not clear how they can distinguish wetlands with/without surface water; see next point). Also, the authors consider they can model lake because lake data are used for the validation (section 3.3).

The no-accounting for the diversity of wetlands is not addressed in the discussion while it could explain some mismatches between the data and the model simulation. For instance, the importance of floodplains process is not discussed as a potential reason for disagreement with data in the Tropics (p424;l1-10).

Also, in the discussion, the authors claim the mismatch in boreal regions could be explained by the fact these regions “are dominated by water logged peatlands” (p424;l14) which are not accounted for in the model. But do this wetland type appear in the Papa et al. data, which are based on the detection of inundated areas? The authors have to be more careful not only with the definition of wetland in their model but also in potential difference in the definition for each dataset they used. For instance, p415;l23: “a high std is found for the wetland observation indicating a considerable uncertainty between the observational data sets”: no discussion is given about eventual differences in the accounted wetland (e.g. do all the databases account for lakes?).

2) More important problem is related to the lack of some important informations in the manuscript. Some assumptions on which equations are based, the explanations of the

choose of an equation as well as lot of details in the whole manuscript are missing or are not well described. This prevents to evaluate in an appropriate way the pertinence of the chosen methods as well as its potential success. Lot of points need to be clarified and more details have to be given.

2.1. Methods

At first, the authors have to explain more the strategy and the computation of the different terms in the original version of the MPI-HM model. E.g. p410;l17: the authors should give some words about the methodology of Weedon et al., 2011; in particular because, the crude way in which ET is approached seems to explain a part of the mismatch with the data in the Tropics (p424;l5. Discussion section; "Following Weedon et al., 2011, we use globally constant parameters of short grass for the vegetation height and the surface resistance").

Then, the authors have also to be more explicit on how they adapt the different terms of MPI-HM to DWES (e.g. p410;l15: "when the wetland water table is below the soil surface both water fluxes are scaled according to the actual soil moisture content"; no equations/information are given about this scaling).

Also, we do not know how the authors could compute the the water table depth in the model. And yet, this variable seems to be used to scale some terms in the different equations (p410;l15) and allows to do the distinction between the accounted/no-accounted wetland (p409;l20). How is h in the Equation (5) computed?

The authors need to explain more their equation and the assumption on which they are based: - Eq 2: how can the authors explain the "z" exponential? There are no references to previous study and no justification. - e.g. Eq 3 and p411;l4: why "the lateral outflow is only computed for wetland"? Why is there no distinction between wetland and riverflow for the outflow? - Eq (6) and Fig.3: unclear. Does the grey area on the Fig.3 (or the red curve on the same figure) correspond to the cumulative distribution of the slope index? Also what is the advantage to use the slope rather than



HESSD

9, C589–C599, 2012

Interactive Comment

the topographic index as used in Kaplan et al., 2006 or in TOPMODEL approaches?

2.2. Optimization

The optimization is based on two steps. Three parameters are optimized (inflow exponent, flow coefficient, slope sensitivity); two of them are optimized in the first step during which the wetland extent is fixed; the last parameter is optimized when the dynamic in wetland extent is accounting for.

I find the justification of the optimization process is not clear: what is the reason of the two-steps decomposition of the optimization? Why are static wetland used for the first step? What are the explanations to optimize the inflow exponent and flow coefficient first?

The first step of the optimization is unclear: - p413;l27: “were considered using a static version of the wetland extent scheme (SWES) with prescribed wetland fraction”. Is a seasonality prescribed using Papa et al. data? - the first optimization step is not totally independent to the wetland database - how does the cost function (equation 10) vary depending on z and c? The authors has to give information about the found values for the cost function and how it varies depending to the values of the parameters couple (for instance, thanks to a figure). In fact, the results of this optimization step consists in only one sentence (p414;l22: “when comparing the different maps, the best agreement on a low cost value was found for z=2 and c=1.1”)

The 2nd optimization step is also not well justified: p414;l26: “the river discharge simulation was not compared to observations anymore but directly to the range of discharge curves which were generated by the optimized SWES”. Does it mean the optimization is not based on the reduction of the gap between model and data? What are the justifications of this?

2.3. Validation

A main problem of the validation is related to the use of water level in lakes. This



HESSD

9, C589–C599, 2012

supposes that DWES is able to simulate lake. How could the authors justify this? This remark has to be linked to the first point in the "Specific comments" section. A lake is commonly characterized by a small variability in its horizontal extent. Because no distinction is done between lake and wetland in the model, it could explain why "the DWES underestimates the range of these variations (i.e. vertical variability) for almost every locations" (pp419;l9). It is not clear how the water level is simulated in the model (see previous comment).

A part of the validation is based on the comparison between the modeled/observed seasonality of wetland extent. To do so, they use the Papa et al., data (the only one dataset giving information about the wetland extent variability in time). One important result in boreal regions is the authors need to account for a snow mask as done in the Papa et al. data to match the 'observed' seasonality. p417;l14 "however, the satellites are not able to identify wetlands below snow cover". How is this results linked with the fact that "the MPI-HM is not able to simulate the freezing of wetlands, snowfall would add directly to the wetland water storage and lead to a too high inflow during water. This behaviour is corrected by allowing for a virtual snow layer on top of the wetland" (p410;l9)? Also, do the authors use the same mask as the one used by Papa et al.? Or they compute themselves a snow mask? The comparison of the seasonality is done at very large scale (two latitude bands); it could be interesting at least to separate boreal, temperate an tropical latitudes.

Could the authors justify they do not compare the year-to-year variability of the simulated/"observed" wetland extent (this information is provided in the Papa et al. data). The study of the year-to-year variability could be a way to estimate the accuracy of the model to reproduce a good wetland extent sensitivity to the climate. The authors should justify their choice to not validate the year-to-year variability of the wetland extent.

The validation against riverflow is not clear due to not well-defined separation with the optimization (riverflows have been used for the optimization). Have some catchments been used for optimization and others for validation (p414;l18: "the analysis considered only those river catchments which include at least 40 grid-cell and have a similar area (+- 10%) in model and observations")?? Moreover, it seems the cost function between simulation/observation is not reduced while the same function have been used for the optimization (p420;l15: "as the river discharge has been used to optimize the ..., an improvement ... is expected" .vs. p420;l26: "small improvement of the discharge simulation which is almost balanced by those catchments with degraded results"). Finally, no figures is given which do not help for a better understanding of this draft part.

3) I found the position of the problematic relatively to the state of the art is not well described. For instance, some key studies (in particular the studies of Coe, 1997, 2000; see references below) are not quoted. p408;l28: "while these models usually lack an explicit surface water storage, this feature is included in a dynamic inundation model by Decharme et al.". What is the position of the current study relatively to Decharme et al.?

Coe, M. T. (1997). Simulating Continental Surface Waters: An Application to Holocene Northern Africa. Journal of Climate, 10(7), 1680-1689. doi:10.1175/1520-0442(1997) Coe, M. T. (1998). A linked global model of terrestrial hydrologic processes: Simulation of modern rivers, lakes and wetlands. Journal of Geophysical Research, 103(D8), 8885-8899. Coe, M. T. (2000). Modeling Terrestrial Hydrological Systems at the Continental Scale: Testing the Accuracy of an Atmospheric GCM. Journal of Climate, 13(4), 686-704. doi:10.1175/1520-0442(2000)013

also:

Krinner, G. (2003). Impact of lakes and wetlands on boreal climate. Journal of Geophysical Research, 108(D16). doi:10.1029/2002JD002597

4) The organization of the manuscript presents many problems:

–> p408;l12-27 are not appropriated to an Introduction and are more relevant to Methods section



HESSD

9, C589–C599, 2012

Interactive Comment

–> Methods section: The paper is based on the simulation of the wetland hydrological cycle itself (Eq1). But the authors needs to explain first how the wetland extent is computed before giving information about each term of the water budget. Thus, it would be better to begin the Methods section with the paragraph p411;l15 and the Figure 2. In particular, because the wetland extent is used to scale the different terms of the 1st equation (p410;l22-23: "When converting them into volume fluxes they are multiplied with the wetland area. Thus, they depend linearly on the extent of the wetlands.").

–> p417;l9-17: this paragraph focuses on simulation alone and thus is not appropriate to the "Model validation" section.

–> discussion: As said by the authors, because the model is not coupled with a climate model, it does not allow to estimate any feedbacks on the climate. Instead of presenting the discussion on this way, it would be better to clearly state the discussion will focus on how the accounting for different processes in the model has an effect on the components of the hydrological budget (ET, soil water content, etc).

5) Many syntax problems or not clear sentences: see some examples in the "Technical comments" section.

* Minor comments:

p409;l19: is no spin-up performed to reach a steady state?

p410;l27: "Several approaches were tested to find a valid scheme for this partition" but no information is given about these tests. The authors should either remove this kind of sentence or explicit more the different tests.

p411;l19-25: The term "adapt" is really inappropriate when it is used to describe the sensitivity of the wetland extent to the climatic conditions!!!

p413;l21: "furthermore, time series of river discharge are not available on grid cell scale, but only as integrals over catchments". It is not true; the riverflow are measured at gauging station_



HESSD

9, C589–C599, 2012

Interactive Comment

p414;l4: “other missing processes in the model or biases in the forcing data”: too general sentence

p416;l17: “These simulation results demonstrate that the DWES is able to reproduce the large scale wetland patterns”: the authors should be less affirmative. In particular when one of the following sentence (p416;l23) is “between 10N and 20°S the DWES overestimates the wetland extent by a factor of three”.

p417;l21: “the timing coincides with the SIND data”. It is difficult to say given the differences between the two curves. The authors should show the figure with a relative scale (between 0 and 1)?

p417;l23: “not shown”; please show it

p422;l17 “as more water is evaporating, the runoff from the land surface is decreased”. But there is an opposite effect: the increase of free surface water through the accounting for wetlands leads to increase the Dunne runoff? How do theses effects compensate each other?

p422;l7-8: “the larger vegetation skin reservoir of the land surface in the control runs evaporate water more easily than the wetland soils of the DWES simulation”: why is the vegetation skin smaller in the simulation with DWES ?

* Technical comments:

p407;l2: the first reference of the paper is not well defined: Friedlingstein et al., 2011 is not in the references list at the end of the manuscript

p407: the authors should describe more deeply the biogeochemical .vs. biophysical effects of the wetlands on the climate

p407;l4: not only seasonality but temporal variability in a more general aspect.

p407;l5: climate model → Earth system model; simulation → representation



HESSD

9, C589–C599, 2012

p408;l7: independent of → independent to

p408;l9: should → could

p409;l10: add “of” between “and” and “the Hydrological...”

p409;l20: remove “those”

p410;l17: “For ET the maximum is given”; do the authors speak about the potential ET as defined in p409;l16

p410;l21: “from now on called” ?

p411-412; Equations (3) and (8): use a different name constant (c in the both). Also S and s in Eq1 and 6.

p412;l5: the “here” word is used very often

p412;l12: “the actual sub-grid slope s”; is the “distribution” word missing ?

p413;l2: “The DWES (...) has to parameterize them”: the model does not do the action to “parameterize" but the authors do.

p413;l6: “only the peak flow and the amplitude”: did the authors want to say “only the peak flow month and the amplitude”

p413;l6: “VAR as their monthly variances”; it is different to the amplitude which is mentioned before, p414;l7?

p413;l16: give the letter corresponding to the third parameter

p413;l18: “against”: repetition

p415;l4: “Evaluation” would be more appropriate than “Validation”

p415;l22: please, give the number at global scale (it is in the Table 1 but please give it in the text)



HESSD

9, C589–C599, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

p416;l7 “The model computes increased wetland fractions mostly for the same regions which are wetland focus regions in the observations ” ?

p416;l19: “with deviations up to 10% of the grid-cell area”: at which space scale?

p416;l25: the paragraph begins with ”we also investigate the spatial agreement between simulation and observation“: this is already begun in the previous paragraph

p420;l10: “a minority of catchments show”: please, give the numbers

p426;l14: ”we will aim to extent the DWES into the soil“: what does it mean?

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 405, 2012.



HESSD

9, C589–C599, 2012

Interactive Comment