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Earth System
Dynamics
Discussions

Interactive comment on "A new model of Holocene peatland net primary production, decomposition, water balance, and peat accumulation" *by* S. Frolking et al.

Anonymous Referee #2

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The manuscript by Frolking et al. describes a new model to simulate long-term changes in plant production, decomposition, water balance and carbon accumulation in northern peatlands. Under a simplifying assumption that peatland hydrology controls NPP and decomposition, the authors have done an admirable job in describing and formulating some important ecological relations and interactions in this new model with impressive details. This model allows the authors to focus on carbon-water feedbacks and interactions that generate some interesting results. The validation of the simulation results using field data from Mer Bleue is impressive, considering the complex factors, parameters and relationships built in the model. This model represents a major achievement in peatland modeling efforts, being one of the most detailed and thoughtful treatments

of peat carbon – hydrology interactions.

I have some general comments and suggestions below that the authors may want to consider in revising the manuscript. Some of these points may be on issues beyond the scope of the present manuscript and are more related to possible future research directions (including points 1-3) that the authors themselves have discussed in the manuscript. In any case these comments may help the authors to calibrate their stated goals and objectives. In general the manuscript is well and clearly written. I recommend the publication of the manuscript after considering these and other specific comments listed below.

General comments:

1. The authors have done a nice job in addressing the roles of hydrology and hydroclimate in controlling peatland ecosystem processes. However, hydrology alone may not be adequate in reaching some of the goals as stated by the authors, including the incorporation of peatlands into earth system models to investigate climate C cycle interactions over long timescales and the projections of future climate impacts on peat carbon and water dynamics. Temperature would be an important, if not more important, climate factor in driving carbon-cycle processes in peatlands as well as in many other types of ecosystems. Also, temperature is one of the climate variables that can be most robustly inferred for the past and projected for the future. I understand that incorporating temperature and temperature-driven processes would be a major undertaking that may require one or more manuscripts. The authors recognize this in the last paragraph of Discussion. Thus, for the present manuscript the authors may want to point out that considering temperature could be essential in achieving the stated goals.

2. The model as presented and described in this manuscript provides valuable insights into the PFT-specific C dynamics over long-term scales, including the percentages of plant mass remained after millennia. However, the detail and the number of PFTs (12)

may not be practical for incorporating the model and formulations into earth system models as intended (and of course desired). Could it be possible to generate one or fewer surrogate PFTs (called it “peat plants”, or “moss-like plants”) from these described ecological niches that could capture the first order behaviors or responses in the model? Such a simplification may be needed eventually for global scale simulations in an earth system model. Again, this aspect is beyond the scope of the present manuscript, but the authors may want to comment on this possibility.

3. Although the validation results of the model simulations at Mer Bleue are impressive, it would be useful to test the model to see if it and the associated parameterization would be reasonably used to simulate peatlands under different climate conditions, such as for oceanic peatlands in Scotland with annual precipitation of 1-2 m, and for continental peatlands in Siberia and western Canada with annual precipitation of about 0.5 m. If so, we would have more confidence in using the model and parameterization for global-scale simulations, alone or as a component of earth system model. Also, for oceanic bogs the resultant simulated peat cores would be used to test if the age-depth relations are concave as observed oftentimes in these bogs (see Clymo 1984). This exercise may allow us to gain some insights about what fundamental factors or processes controls concave vs. convex patterns.

4. The manuscript can be better organized. For example, Section 3 is more than just model description, so the authors should change its heading to “Model description and simulations”. Also, the authors may want to add a new subsection at the end of section 3, say “3.5 Sensitivity analysis”, in which the authors can include part of materials (related to sensitivity analysis design/method, including Table 5) in 4.2 here. So in subsection 4.2 the authors can mostly focus on evaluating sensitivity analysis results and implications. As now, 4.2 is a bit hard to follow.

5. Another suggestion related to the organization is to divide Section 5 into 2 or more subsections. This may help reorganize the paragraphs in this long section and make it easier for readers to follow. I cannot make specific suggestions about these subsection

topics. Also, the authors may want to consider adding a last subsection in Discussion to address “Outstanding issues and future directions”, which could be expanded from the last paragraph of Discussion and may include some issues mentioned above (points 1-3), if appropriate.

Specific comments:

P116, L18: Change “310 kg C” to “310 kg C/m2”. Otherwise the figure makes no sense.

p.117, l3-6: This sentence is a bit confusing, as the figures refer to mass of C, or the compounds (CO2, CH4 and DOC). Clearer wordings may help. For example, change to “ they appear to take up CO2 at a rate of between 40 and 80 g C /m2/yr”, if that is the case (I think it is the case). For CH4, it is for CH4, not C.

p.117, l11: Change to “MacDonald”

p.117, l.21: Do the authors really think that the model with that much detail and with lack of temperature as presented can be coupled to earth system model? How likely would the data required be available on a large or global scale? See general comments above.

p.118, l8: change subheading to “Background – modeling peatland processes”? (as the model does more than just peat accumulation.)

p.119, l6: it is not necessary to convert mass to depth in order to discuss depth-age relations, but it is actually preferable to use mass-age curves (depth is expressed as cumulative mass per unit area).

p.119, l10: change “Eq. (3)” to “Eq. (2)”

p. 122, l.15: Change heading to “Model description and simulations”?

P. 122, l25: change to “The vegetation sub-model in HPM is based on the assumption that . . ..”, as I think the word “assumption” is more accurate than “idea” in this context.

p. 123, l.17-18: It is great that the model can generate species composition on the basis of precipitation and the optima and tolerance formulations. Could this provide another means to test the model performance using modern plant community data (and water table and precipitation data) from peatalnds around the world?

p.131, l1: typo, delete “a peat”

p. 131, l.23: add “respectively” between “organic mater content” and “(Dean, 1974,. . .”

p.131, l.25: Does “46%” here refer to C % in organic matter (peat)? Unclear. Reword.

p.131, l.27: change “thereafter” to “below that”

p. 131, l.29: change “dates” to “ages” or “age determinations”

p. 132, l. 3-4: add a new subsection on sensitivity analysis and move and reorganize some materials from subsection 4.2. See general comments above.

p. 133, l.16: change “more then” to “more than”

p.134, l.9-11: Here the difference between total NPP and total decomposition is referred as NECB, while in Fig. 5c it is referred as NEE. Perhaps the authors want to be consistent throughout the manuscript, as both terms refer to different ecosystem processes, though the simulations cannot tell them apart (as CH4, DOC and other fluxes are not explicitly considered).

p. 138, l.17: Add subsections in Section 5? With last subsection focusing on uncertainties and outstanding issues, etc. See general comments above.

p. 139, l. 17: Change “300 kg” to “300 kg/m2”, otherwise the figure makes no sense. The cumulative mass per unit area and depth in m should be interchangeable with bulk density information.

p.139, l. 19-20: It is kind of unusual that simulations at 1-yr timestep are less variable than the observed peat-core data that were sampled at much lower resolution. Could

that be due to the smoothed precipitation time series, even with added random variability? Fig. 4a shows that the reconstructed annual precipitation ranges from 0.8 to 1.1 m over the last 8000 years. How that range compares with the instrumental records in Montreal or Ottawa? If similar, should we expect to see much larger magnitude of precipitation change during the Holocene? If so, what impacts of the reduced precipitation driver variability on the results?

References from p. 146: There are some errors in the references cited. Below are just a few examples. The authors should check thoroughly.

Dise: Should be published in year 2009 in Science, not 1998.

Re-order references around Forrest and Fraser, between other Frolking refs.

McGuire 2009. The journal should be “Ecological Monograph”, not “Ecological Appl.”

Yu 2003. The co-authors may not be correct for that cited paper.

Table 1. Superscripts for some terms may be confused with the terms themselves. Spell out the terms in the footnotes? For example, for (a): NPPmax are the maximum NPP values. . ..

Table 2: Footnote b: “-25” needs unit (per mil)

Fig. 2: need pane label for c.

Fig. 6: Add appropriate x-axis labels for all panels?

Interactive comment on Earth Syst. Dynam. Discuss., 1, 115, 2010.