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Interactive comment on "Assessing effects of permafrost thaw on C fluxes based on a multi-year modeling across a permafrost thaw gradient at Stordalen, Sweden" *by* J. Deng et al.

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Review of Deng et al. "Assessing effects of permafrost thaw on C fluxes based on a multi-year modeling across a permafrost thaw gradient at Stordalen, Sweden".

General comments

A nice paper that introduces some modifications to DNDC to apply it to assess the fluxes of CO2 and CH4 in a permafrost peatland. The authors use the very well-studied Stordalen peatland in northern Sweden as their test site. This is appropriate because the data sets exist to rigorously evaluate the model output but it would be worthwhile for the author to provide a little more explanation of the physical and climatological setting

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Interactive Discussion
Discussion Paper

of Stordalen. It does not represent permafrost peatlands in general but a class of ice-core peatlands called peat palsas or plateaus. These are normally found in sporadic or discontinuous permafrost regions over fine sediments and near good sources of water. These landforms have their own cycle of growth and decay regardless of climate change – they exist in the climate zone where normally variability creates their inherent instability so small normal variations in temperature and/or snow accumulation cause expansion and thawing. This does not matter for this particular study too much because the work is focussed on does DNDC capture the changes in carbon cycling that occur with thawing but it does influence how much readers can take from this study and apply it to permafrost peatlands in general. This is not a criticism but a few words of caution in the introduction to contextualize this work are warranted. The paper represents a step in the development of a fully functional permafrost peatland DNDC. It examines the changes in the biogeochemistry and fluxes when the hydrological change is provided to the model. DNDC does not simulate the change in hydrology due to changes in surface elevation that occur with permafrost thaws and does not include functions for the vegetation changes that occur because the moisture conditions change dramatically. Again this is not a criticism. The authors deal with the future needs for development of DNDC to make in fully functional for permafrost environments in the discussion but it would be useful to acknowledge in the introduction what the modifications will do and what they will not do – i.e. be more explicit about the current assumptions involved in the application of DNDC to this current study. You do this in the methods but it would be very useful to qualify this work in a general way in the introduction maybe right after the objectives. Also let the readers know what the final modelling objectives are and where this particular study fits along the route to those objectives. Many of my specific comments below come from wondering how this was going to work in the end. My final comment is that this paper assumes readers have a considerable amount of knowledge about how DNDC works and how it handles the relationship between hydrology, and carbon cycling in aerobic and anaerobic conditions. I am not sure those who do not have a good knowledge of DNDC will understand the description of the changes

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to DNDC and what it means to couple DNDC to NEST? I fully recognize the authors do not want to reiterate a full description of DNDC with each manuscript using and developing the model. Think about the reader this is their first experience with DNDC. Because this is the first DNDC runs involving permafrost it is a new audience that will be reading this modelling work – not the main stream DNDC crowd.

The paper is well written. The objectives are clear. The authors attain their stated objectives. The conclusions are justified given the results presented. The paper is a worthwhile contribution to the literature.

More Specific Comments

Pg 3971 ln 4 - How similar are the climate variables between ANS and Stordalen. The temperatures and precipitation (see Olefeldt's work on the hydrology of Stordalen) are quite different and I believe solar radiation is different. When calibrating the model for the hydrology is it not fairly important to have local precipitation.

Pg 3971 Ln 23 - Does this means you calibrated the hydrology of the model? Why not simple use the measured wtd then?

Pg 3974 ln 13 - Did you examine the structure of the residuals to see if there were any particular biases in the model.

Pg 3974 ln 19 - Can you look at the random versus systematic components of the RMSE? The random component could be reduced by better specification of parameters but a systematic error indicates potential structural problems with model components.

Pg 3975 ln 20 - Was this true fir CH4 simulations or also fir NEE? In the methods you discuss how DNDC hydrology was calibrated. It would be good in that section to mention you used the model hydrology only for days without measurements? In Figure 4 a-g there is no distinction between measured versus 'infilled" wtd. How much is measured and how much is infilled?

Pg 3976 ln 14 - In DNDC what are the sources of substrate used for the production

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of methane? Are the substrates only from decomposition of SOM? Strom et al. have shown that around the roots of Eriophorum there is a significant amount of acetate, presumably from roots exudates. Olefeldt et al. have shown that the DOC quality changes significant in the Eriophorum areas of Stordalen. Does DNDC include these pathways? Is there any correlation between CH4 and NEE and is there a lag correlation?

Pg 3977 ln 3 - What fraction of the annual fluxes occur outside the growing season? In other words how much of the annual simulated fluxes are for periods that you have not evaluated?

Pg 3979 ln 25 - Can you quantify these differences? The data exists to see if this is a reasonable explanation. There have been lots of measurements done at Stordalen over the years. You could see what the offsets are and use these factors to adjust the continuous record from ANS and see if this explanation stands up to the test?

Pg 3980 ln 15 – 18 - There have been over winter measurements of NEE and CH4 that you could use to quantify the fraction of the annual exchanges that occur in the winter. This would provide a better quantification the qualification you make here.

Pg 3981 ln 20 – 21 -Due the CH4 exchanges partitioned by vegetation type match what Christensen et al. (2004) estimated? Or is this the same analysis that Johansson et al (2006) did? Pg 3981 ln 24 – 26 - How will DNDC handle this? I do not believe it has dynamic vegetation? Wouldn't you also need to simulate the lateral redistribution of water because of the changes in elevation due to the presence or absence of permafrost? Ok this is discussed below - maybe some signal that you will discuss this below is warranted?

Pg 3982 ln 6- 10 - See comments above: hint that this discussion is coming here earlier in the manuscript. Knowledgeable readers will have these questions a lot earlier in the manuscript.

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Interactive comment on Biogeosciences Discuss., 11, 3963, 2014.