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Biogeosciences
Discussions

Interactive comment on "Modeling carbon dynamics in two adjacent spruce forests withdifferent soil conditions in Russia" *by* J. Kurbatova et al.

Anonymous Referee #3

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Review of Modeling carbon dynamics in two adjacent spruce forests with different soil conditions in Russia by Kurbatova et al.

General comments

The authors present a study on the influence of soil properties, mostly the water table regime, on the annual C budget of one upland and one wetland boreal forest sites using modelling tools. The authors used eddy covariance data from other upland sites to calibrate their model. They validate the calibrated model using multi-year eddy covariance data collected at their two sites. The authors then perform a sensitivity analysis by evaluating the effects of different scenarios of water table regime on the annual C balance of the wetland site only.

The study represents an interesting modeling exercise to highlight the role of the water table regime on the C dynamics in wetland ecosystems in a context where long-term data are hard to collect. The study addresses a relevant scientific question regarding the link between the water and C cycles in boreal terrestrial ecosystems. The paper reads well and its presentation is clear and well structured. The paper appears to have two major conclusions: (1) the model used can reproduce the measured NEE at both sites, and (2) soil properties differentiate the CO2 exchange dynamic between the sites. The first conclusion is fairly well supported by the results for the wetland site (although there are some issues, see below) but not for the upland site since observational data are sparse. The second major conclusion is currently hard to judge because clarification is needed (see below). Also, the second conclusion is not new as it is very similar in some regards to what has been published by Dunn et al. (2007) based on long-term observational data from a wetland boreal site. The authors should either take advantage of this paper (which they do not refer to) or sell their study in a way that clearly makes it different than or complementary to the study published by Dunn et al.

Specific comments

Firstly, the estimation of annual NEE needs far more description as it is currently hard to judge. How were flux/climate data cleaned/quality controlled? Were the flux data under calm conditions filtered out (ustar threshold filtering)? What proportion of flux data was then available for further analysis? What gap-filling strategies were used to obtain daily and annual values?

Secondly, the authors validate their model by assessing its capacity to reproduce (1) total daily NEE and (2) a multi-year average of annual NEE. Fig. 4 showing the correspondence between modelled and observed total daily NEE for WSF is rather convincing (although it relies solely on visual inspection). However, such a strong validation is missing for DSF and this weakens the two major conclusions of the study listed above. Also, how much confidence can be put in a model if it was validated by comparing only one single observation (multi-year average of annual NEE)? For example, modeled NEE is about 65 % greater than observed NEE at DSF, is this good enough? Can the authors use annual NEE estimates rather than a multi-year average or can they use other quantitative criteria?

Thirdly, I have some concerns regarding the model calibration. Why those German and US sites were chosen to calibrate physiological parameters in your model given they have quite different ecosystem structure and composition, as well as site and climate conditions than the WSF and DSF sites? Also, why no wetland site (according to Table 2) was chosen to validate the model? How could that affect the validation of the model especially given the fact that hydrological process modelling depends on whether the site is an upland or a wetland site (as stated on p. 278, line 21-26)? This would need to be discussed in the text.

Fourthly, the authors conclude that soil properties differentiated the two sites regarding their contributions to atmospheric CO2 (p.282, line 18-20). My understanding is that this statement comes from the fact that their model can reproduce a multi-year average of annual NEE for both sites by changing only the water table regime and the soil carbon stock between WSF and DSF (all other parameters being the same between sites), hence these two factors explain the observed between-site difference in annual NEE. Apart from the concern mentioned above regarding model validation at DSF, tree species composition and structure do appear to differ significantly between the wet and dry spruce forests. For example, at least one third of all trees are deciduous trees at DSF and we know that coniferous and deciduous trees have different CO2 exchange rates and phenology. Can the difference in annual NEE be attributed to the higher proportion of deciduous trees and the presence of younger trees at DSF rather than soil properties? It would help the authors make their point across regarding the importance of soil properties in explaining between-site differences in annual NEE if

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they can convince the reader that no other factor can explain the observed difference.

Finally, the discussion section looks more like a conclusion to me. In addition to the suggestions listed herein, the authors could discuss their model's limits and shortfalls and the implication of their study for wetlands and boreal terrestrial ecosystems in general from an ecological stand point.

Technical comments

p.272, line 18: Replace 'proved' by 'suggested'.

p.273, line 25-27: I would avoid the use of the word 'preliminary' because it could give the reader a false sense that published analyses of observed NEE fluxes are either incomplete or not self-sufficient (i.e. they can not contribute significantly to our understanding of C dynamic in terrestrial ecosystems by themselves). Please rephrase. Also, there are numerous notable analyses published before and particularly after Falge et al. (2002), some of them could be easily added.

p.275, line 15-16: How does the location of the tower in a shallow depression affect the measured CO2 flux (e.g. advection, storage term)? What does 'with a heterogeneous territory' mean?

p.275, line 23-24: Is it Acer platanoides rather than Acer plaNtanoides?

p. 276, line 10-12: Is a flow rate of 4-5 l min-1 high enough to ensure turbulent flow inside the sampling tube and minimize high frequency attenuation, especially if you are to measure at 20 Hz? Ameriflux recommends a Reynold's number (Re) between 3000 and 3500 and I calculated Re above 3000 only under very cold conditions and when the sampling flow rate is at its upper limit (5 l min-1).

p.276, line 16-18: What is the precision of the known CO2 concentrations?

p.276, line 21-24: What was the purpose of measuring multi-level CO2 concentrations? Presumably for storage term calculation to be included in NEE estimation but it is not

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Interactive Comment

mentioned in the text.

p.276-277: Please make sure to include all the appropriate information when referring to instrument manufacturers.

p.277, line1-15: It is not obvious to me how all these variables are used in the study especially given the fact that climate data used in the model are from the local weather station (as stated on p. 279, line 23-25). At line 13-15, a 'comparison with half-hourly eddy flux data' is mentioned, but what comparison? Either add a sentence stating how you used all these climate measurements or edit out this enumeration of instruments.

p.277, line 21-23: How can an average annual NEE be estimated with less than one year of data (July 02 to May 03 which, moreover, badly represents the annual cycle since it includes two different growing seasons)? Are measurements available at other time during the 1999-2004 period? Can a brief description of what was done by Van der Molen et al. to estimate an average annual NEE for the DSF site be provided?

p.278, line 1-2: Water table depths were measured using what probes? Please include this information with all the other information regarding climate measurements.

p.278, line 20-26: Does the one-dimension hydrological routine also apply for wetland sites for the unsaturated zone of the modeled soil profile? If so, can this sentence be rephrased to solve this ambiguity? Also, does the model account for capillary movements of water just above the water table that can affect soil moisture profile?

p.279, line 25-26: Why did this particular ecosystem composition and structure was chosen to use for both DSF and WSF? According to the site description section, ecosystem composition and structure appear to differ significantly between both sites as well as between the actual sites and the modeled sites.

p.279, line 28: How was the 25 tons C/ha determined? Where does that value come from?

p.279, line 28-29: How were climatic conditions in 2004 as compared to other years used in your model? What are the implications in your model of using year-specific climate data except for the water table regime? How can that affect the modelled annual NEE? Please include more discussion on this matter.

p. 280, line 4-5: To me, Fig. 4c shows quite a discrepancy between observed and modelled values, especially in the first half of the growing season. How can this be resolved and\or explained?

p.280, line 10: The annual values reported for WSF and DSF make them more like strong source and sink, respectively (especially given the fact they are boreal sites).

p.280, line 11-13: If I understood correctly, the same climate data and ecosystem composition and structure were used to model NEE at WSF and DSF. The only things differing between WSF and DSF in the model inputs were the amount of soil organic carbon and the water table regime (as stated on p. 279, last paragraph). Given that, is it possible for the model to output different results than what you reported in Table 3 (same modelled photosynthesis rate and autotrophic respiration between sites, different heterotrophic respiration between sites)? In other words, how photosynthesis and autotrophic respiration depend on the amount of soil organic carbon and the water table regime in the model?

Table 1: Few terms need to be defined (e.g. Dtemp, DVPD, VPD, GDD)

Table 1: Why is the spruce specific leaf weight set to zero? It does not make a lot of sense to me, especially since spruce is the dominate species at the studied sites.

Fig. 3-7: The 'field' and 'model' lines are hard to distinguish when the paper is printed in black and white. I would suggest the use of dots for observation and lines for model data.

Fig 3-4: Please change 'field' to 'observation' in the legend as it is more explicit.

Fig. 7: Please change 'plant-CO2' and 'soil-CO2' to something explicit like 'Rautotrophic' and 'Rheterotrophic' or 'Rauto' and 'Rhetero'.

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Reference Dunn AL, Barford CC, Wofsy SC, Goulden ML, Daube BC. 2007. A long-term record of carbon exchange in a boreal black spruce forest: means, responses to interannual variability, and decadal trends. Global Change Biology, 13:577-590.

Interactive comment on Biogeosciences Discuss., 5, 271, 2008.

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