Kristina Luus and John Lin present a study that develop, calibrate, validate and analyze a high latitude ecosystem carbon dynamics model. The model has several promising features, *e.g.*, take advantage of satellite information to capture the snow dynamics and vegetation dynamics; the treatment of subnivean respiration. Overall, the presentation is smooth, the methodology is valid, the analysis is convincing and the content fits the scope of GMD.

Comments:

1. How to do prediction? The model is limited to the year when MODIS data are available. Is this model designed to be a diagnostic tool?

2. Respiration temperature sensitivity. The treatment of temperature sensitivity needs to be improved, or at least needs more discussion. The model assumes "linear temperature sensitivity", which might be suitable for this application (simulation over high latitude from 2001 to 2012). But the linear sensitivity is not a favorable assumption under most circumstances. Macro-scale soil microbial respiration is commonly assumed to be exponential, using Q10 framework [Lloyd 1994]. Micro-scale soil heterotrophic respiration is even more complex, which is closely coupled to environmental fluctuations as well as resources supplies (carbon and nutrients) [Manzoni 2009]. Plant maintenance respiration is generally linked to plant nutrient content, modified by exponential Q10 function [Ryan 1991]. Plant growth respiration is usually assumed to be a fraction of newly fixed photosynthate, based on the construction cost of woody tissue [Larcher 2003]. In summary, the respiration temperature sensitivity is much more complex than linear, especially

when the plant respiration and soil respiration are combined in one variable (R in PolarVPRM). I suggest that the author should explore the linear temperature sensitivity assumption more carefully and at least discuss this issue and make some arguments for their model assumption.

3. Why not use soil temperature for growing season R calculation? Without snow over during growing season, soil temperature is closely coupled with air temperature. In Equ. (6), why did the author use air temperature to calculate growing season respiration? My understanding is that R has two components, plant respiration and soil respiration. Plant respiration relies more on air temperature, soil respiration relies more on soil temperature. During snow season, soil respiration is the dominant flux in R, therefore, it's reasonable to use soil temperature to calculate R (this part is reasonable in Equ. 6). During growing season, however, both soil respiration and plant respiration are important. Using air temperature to calculate growing season R, may bias the soil respiration estimates. Another issue is that growing season air temperature is more variable than soil temperature. Intuitively, using air temperature to calculate growing season soil respiration could result in a wider range of soil respiration.

4. Forest sites calibration and validation. It model was calibrated and validated at several shrub tundra, graminoid tundra and wetland/barren sites. The model-data comparison showed that PolarVPRM was successful. But the evergreen forest, deciduous forest, mixed forest, shrubland were not calibrated using EC tower data. The model was applied to the high-latitude North America (north of 55° N) to model the diurnal, seasonal, interannual variation of NEE. Figure 1 showed that

about half of this research area was covered by forest and shrubland. My question is that how to determine the forests and shrubland parameters. FLUXNET has lots of boreal forests sites. Why not using those data to calibrate the PolarVPRM. The authored stated that they used original VPRM parameterization for forest. Then it looks like that the latter part of this paper is comparing a model (a mixture of PolarVPRM (tundra and wetland) and original VPRM (forests)) with CARBONTRACKER and FLUXNET-MTE.

Other minor comments:

1. Page 2 L22-26, High-latitude permafrost regions …. Positive feedback … from thawing permafrost…. The first two sentences really confused me as I started reading the manuscript. Actually, the paper has noting to do with permafrost carbon dynamics. So, maybe it's better to start the introduction with something like "high latitude carbon dynamics" but not "permafrost".

2. Page 3 L21. Is "R" equal to plant autotrophic respiration plus soil heterotrophic respiration or just soil respiration?

3. Page 4 L15. P_{scale} range in values from 0 to 1. Is it true? In Equation 2, P_{scale} =(1+LSWI)/2, in which LSWI ranges from 0 to 1. Therefore, P_{scale} ranges from 0.5 to one.

4. Page 12 L14-16. 2008 and 2011 were selected, as these were closest years to 2005. Should you use a year that is closest in terms of climate forcing (temperature and precipitation), but not just numerically closest?

5. Page 19 L 23. Relative to “CarbonTracker”. CarbonTracker is an atmospheric CO2 inversion model. This modeling approach relies on a prior flux of land surface carbon exchange, atmospheric CO2 concentrations, as well as background CO2 emissions (e.g., fossil fuel burning). In addition, the model is transport-oriented. Even though the inversed NEE could be sampled at the fine-scale resolution, the fine-scale NEE has such large uncertainties, since the fine-scale NEE is so sensitive to local synoptic events. That’s one of the reasons why transport inversion inter-comparisons usually compared sub-continental scale integrated NEE. I would suggest the author compare original VPRM, Polar-VPRM, FLUXNET-MTE.

6. Page 38 Table 3. Add one column to show which years of ECA data are used.

7. Page 42 Figure 2. Several like overlaps. Visually, I can not find “Sim NEE”, “PAR0_GEE”, “T_R”, “R_all” in the right panel. If some lines are overlapped each other, please use arrows to indicate where are the lines.

Reference

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