

Interactive comment on “Modeling carbon dynamics in two adjacent spruce forests with different soil conditions in Russia” by J. Kurbatova et al.

Anonymous Referee #2

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General comments

Kurbatova et al address an important issue that is relevant to our present day need for a better understanding of the biogeochemical interactions between the biosphere and atmosphere. They describe their experience in simulating the C and N exchange from two Russian forests varying in soil quality and hydrology. The modeling tool employed for the purpose is the Forest DNDC model developed by Dr. Changsheng Li. The long term C exchange data reported in the paper represent a region with diverse soil, plant and climatic conditions and high variability and uncertainty in biome C balance estimates. Moreover, we would like to know how the existing biogeochemical models

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fare when evaluated against the data from a region that is very different from the one the model was originally developed for. In view of this, this is an interesting paper and the topic is appropriate for publication in this journal.

Specific comments

1. To begin with, I noticed a certain discrepancy with regard to authorship. According to the Editor's message asking me to be a referee for this paper, this paper was introduced to me as being authored by Li et al. However, when I downloaded the full paper for reviewing, I noticed a different authorship order. I think this needs to be clarified, for the sake of records.
2. Use 'was' instead of 'were' in the first line on page 272.
3. Insert 'such' after 'programs' in line 18 on page 273.
4. Lines 5-7 on 273 - Vegetation composition and structure in natural forest ecosystems are far too complex. I do not believe that any of the existing biogeochemical models are adequate enough yet to treat these issues with a fare degree of accuracy.
5. Replace 'governing' by 'govern the' in line 11 on page 274
6. Remove the extra space after the cited reference 18 on page 274.
7. Correct the sentence in line 6 on page 275.
8. Provide a measure of dispersion around the WSF mean NEE.
9. I do not think we can accept the DSF mean value as the site average as it is based on to limited data. If the authors have a sound reasoning to treat the value as the site average, please elaborate.
10. Are there any chamber measurements of respiration from the forest floor? The modeled results indicate that processes leading to soil C turnover govern the net C balance in the studied ecosystems. Eddy covariance NEE data are not easily amenable

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to partitioning into component C flow processes, especially for complex ecosystems such as forests.

11. After briefly describing the NEE data, the authors quickly switch over to model description. What would be more important before explaining the NEE data would be a brief description of the climate and soil hydrology (precipitation) variability over the measurements years. As for the model description, a short note on the model with a reference to the Forest DNDC website would be enough.

12. Page 280 line 19 - replace 're-run' with 're-ran'. 13. The authors have attempted to provide a complete GHG balance of the forests through the GWP analyses. Yes, indeed the DNDC model can simulate methane and N dynamics as well. However, it is not mentioned whether there were any methane and N₂O data available within the study to compare with the modeled data.

14. Also, the simulated N₂O emissions seem to be rather overestimated (about 14 kg N). At this point, I would like to draw the attention of the authors to the soil C and N values for the WSF site given on page 275. What is the unit of soil N mentioned here? If it is kg N per ha, the values given in the paper result in an extremely high range of C:N ratios that is unrealistic. If the unit is ton N per ha, then the site can no more be considered as being low in N content. Please check the soil C and N values and units and report C:N ratios for the two soil types. If possible, the modeled n₂o data should be compared with measurements from the site or other similar sites in the region. I do realise that N dynamics is not a major part of the paper. However, the objective of the paper is to evaluate DNDC under these forest conditions. Therefore, it would be important and rather necessary to establish here that the modeled data are in agreement with the measured data with respect to both C and N dynamics in these ecosystems.

15. And finally, the discussion section is rather very general, short and hence seems vague.

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16. Figures, especially the ones that compare the observed and modeled data, are not quite clear when the printer friendly version of the submitted MS is printed on A4 sheets. Could the authors present these model comparisons in a better way, probably as scatter plots with some statistical analysis to indicate the degree of agreement between the observed and modeled data? Also, please see if Fig 2 and Fig 5 are absolutely needed.

17. Check the year of publication for Kiese et al reference - line 15 page 274 and line 27 page 284.

18. line 9 page 282 - replace 'spare' by 'sparse'.

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

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