

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

### **Anonymous Referee #1**

Received and published: 26 February 2008

This is an excellent paper in which the authors have used a very large data set to validate the DNDC Forest model using carbon flux data. The paper is well structured and well written with a good account of the methodologies used and provides important messages regarding the influence of site conditions on carbon exchange in northern coniferous forests.

The paper’s main weakness is the lack of any meaningful discussion section. In its current form the discussion adds little useful information to the paper. This section should be revised and expanded to consider the various strengths and weaknesses of the experimental and modelling approaches used within the paper and to discuss this

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in the context of other published work. We need to know what aspects of the approach used in this paper were most successful, and where further development is required. We need to know for example to what extent this model is better than alternative models simulating ecosystem responses. I would suggest that the following topics are included in the discussion section.

The model is validated against carbon flux data collected in the forest sites. However, the model is also able to simulate N<sub>2</sub>O and CH<sub>4</sub> exchange allowing the calculation of global warming potentials. There is no mention of any data been collected for these trace gases, I would guess that it is probably not available. The authors nevertheless present estimated global warming potentials for the sites. If N<sub>2</sub>O and CH<sub>4</sub> data is not available, then the success of the model in simulating the fluxes at other comparable sites should be discussed. Changes in water table in depth would be critical to the magnitude of the fluxes and could cause considerable variability in global warming potential.

The model appears to provide a good simulation of CO<sub>2</sub> fluxes for the site, however with the scale of the graphs that I am looking at, it is difficult to estimate the strength of this relationship. I would suggest that some quantitative estimate of the goodness of fit between modelled and measured data is presented. This could then be used in the discussion to compare the value of the authors approached by comparison with others.

I noticed that water table depth was only measured in 2004, and at the same values were repeatedly used in the other years. We are not told anything about the variability in climate between the six years, but it would seem likely that climatic variability would lead to annual variability in water table depth. Would it mean more reasonable therefore to calculate some relationship between climate and water table debt in 2004 and apply this to other years? Again this is something which could be considered further in the discussion.

The observation that the site conditions can influence net ecosystem exchange of car-

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bon is important. The model will use this information to calculate changes in site carbon storage. It would therefore be interesting to describe this data in the paper in order to understand the relative importance of these differences in NEE on carbon pools. I would suggest also that the authors include a brief table (possibly extending Table 3) summarising monthly precipitation and temperature over the measurement period. If space is short they could consider omitting Figure 1 or 2.

Some other minor comments

Line 8 p 280 van der Molen et al 2007. Reference not cited in reference section

Line 19 p 280. ..."we re-ran" not" we re-run"

Line 9 p 282 "still sparse" not "still spare"

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