

## ***Interactive comment on “Towards operational remote sensing of forest carbon balance across Northern Europe” by P. Olofsson et al.***

**P. Olofsson et al.**

Received and published: 5 November 2007

We would like to express our gratitude to the anonymous referees for their insightful comments and suggestions for improvement. Your participation is much appreciated. A revised paper will be submitted reflecting the comments as described below.

### **Referee 1**

**Comment 1.** *The authors shortly describe the dominant species at each sampling site (in Table 1). I have some problems with this, because there is no information about how species dominance was defined. Additionally, nothing is said about structural differences that certainly exist between the sites (species composition of non-dominant*

*species, vidual densities, canopy heights, coverages, etc). [...]*

**Author reply.** A very good point; and point taken. Will try to find the necessary data for inclusion in the revised paper.

**Comment 2.** *I suggest to include a map of Northern Europe showing the location of the sampled sites.*

**Author reply.** Good idea. A map will be added to revised the version.

**Comment 3.** *P3150, L12-15: needs clarification: in one case, respiration was derived by relationships between average night NEE and night T; in the other case between night NEE and air T ?*

**Author reply.** Yes, different methods were used at the different sites for deriving respiration. It would of course be better if the same methods were applied to all data, but since we used existing data sets of the parameters in question this processing had already been performed and we lacked the resources to reprocess the measurements.

**Comment 4.** *I found the different resolutions of NDVI and EVI used at the different sites (250 m, 1 km) confusing. The highly resoluted 250m data are not able to detect the seasonal dynamics, but the lower resoluted 1km data display the expected seasonal trend clearly ? Needs clarification. In addition, 250m data were only used at Sorø site, because 1 km data detected other vegetation types than deciduous forest. Excluding this site from the data bases may would help to standardize the satellite data sets (all 1 km), which would facilitate the interpretation of the results.*

**Author reply.** Yes, it appears as if the 0.25 km data fails to detect the seasonal dynamics, as apart from the 1 km. This does not make sense and we have been in contact with the people responsible for the EVI product and they confirm our findings. We

have some preliminary explanations (last five lines at P3152) and a study is planned exploring this phenomenon further.

Excluding Sorø would, as pointed out, result in the use 1 km data exclusively, but also leave us with only one deciduous site. Furthermore, the observed difference between 0.25 and 1 km EVI applies to the coniferous sites, not the deciduous (however, if this is consequence of species composition or different angle geometry due to lower latitude is unclear).

**Comment 5.** *P3157, L7-10: The authors mention that one reason for the stronger correlations between EVI and GPP in deciduous sites is that only two sites were considered, whereas five sites were considered in coniferous forests. Is there no possibility to perform standards that permit direct comparisons ? In fact, I expected a contrary result: stronger correlations (less variability) between EVI and GPP in coniferous forests, and weaker correlations (more variability) in deciduous forests.*

**Author reply.** I think this relates to the first comment – the two deciduous sites are rather homogenous beech forests and not necessarily representative of a "generic" Northern deciduous forest. Therefore, a stronger EVI-GPP relationship is observed compared to the coniferous sites. A pointed out, a more thorough description of the sites would help clarify this.

**Comment 6.** *Include the  $r^2$  of the exponential relationships in Figs. 6a, 6b, 7, and 8.*

**Author reply.** OK, will be done.

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**General Comment** *I have some problems with the conclusion that this study illustrates the potential that remote sensing can be used for assessing the carbon balance of forested areas in Northern Europe. Remote sensing based approaches can tell us a lot about GPP and also NPP but not so much about soil respiration and thus NEE. Since there is currently no solution to the problem of capturing spatially varying base respiration rates from space I would expect a more differentiated statement in the conclusion.*

**Author reply.** We have discuss the issue and we agree; we should be a bit more careful when it comes to the conclusion on the applicability of the results. This will be toned down in the revised version.

**Comment 1.** *Abstract line 11 to 14: not clear which reported correlation coefficients refer to which carbon fluxes*

**Author reply.** True, refers to NEE and GPP. Will fix for revised version.

**Comment 2.** *Introduction - p. 3146, line 23: remote sensing does not provide a direct estimate of the carbon balance or carbon fluxes! Better call it data-driven models or diagnostic models*

**Author reply.** True. Will fix.

**Comment 3.** *- It is several times mentioned that process-oriented models are primarily limited by accurate input data. I do not agree with it. It is true that process models can generally be tuned to accurately predict fluxes for the site level and small regions but when it comes to the large scale they are much more uncertain. Input data, esp. meteo input have large effects here but uncertainties related to model structure are at*

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least equally important. Otherwise, there would be no difference between models in modelintercomparison studies when all are driven by the same input. I would consider removing this line of argumentation in the paper.

**Author reply.** Our point is that it may be difficult to apply process-oriented models for larger areas. The structure of a model will affect the performance, but the problem with large scale estimations will remain.

**Comment 4.** - p. 3146, l. 24 to p.3147, l. 6: *I find it a strange argumentation. First the authors say that modelling soil respiration from space data is very difficult (I think impossible) then they say it is worth doing it anyway. One does not need a justification if one wants to model NEE given that this is the crucial flux everybody is interested in; and this paper explores an approach using remote sensing data.*

**Author reply.** We wanted to see how far one can reach in obtaining the total respiration using the variables we had at our disposal. But yes, as mentioned in the reply to the general comment, we will tone down the parts on remote sensing of respiration.

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Interactive comment on Biogeosciences Discuss., 4, 3143, 2007.

**BGD**

4, S1823–S1827, 2007

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