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Interactive comment on "The importance of micrometeorological variations for photosynthesis and transpiration in a boreal coniferous forest" *by* G. Schurgers et al.

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We would like to thank the reviewer for the accurate and positive summary of the work and for the constructive remarks. Below, we address the reviewer's comments in detail. For a submission of a revised version, a detailed list of changes to the manuscript will be provided together with these comments.

Introduction, paragraph starting from line 17
I think you need more justification for the critics of the large scale models. Which models are you really referring to? Many of the large scale models do have a radiative transfer scheme, and the canopy is represented as layers (e.g. as in

the Mercado et al. paper that is in your references). Now you are missing new references to the present state of the large scale models. It would be beneficial, if you'd justify your claims with literature.
We agree that the statement about the large-scale models was too vague and warrants further clarification. We will address this by describing existing models and provide examples, both of models that fail to capture heterogeneity, and of models that have addressed these types of heterogeneity.

Section 2, 1st paragraph
You do give the reference to Lundin et al. paper, but it would be nice to have the annual precipitation and air temperature, as well as a description of the understory vegetation for the site. You show later the distribution of LAI in a plot, but you could mention here the total LAI, and the LAI of the understory vegetation, if you have that.
The long-term mean annual temperature is 5.5°C and annual precipitation is 527 mm y^{-1}. The leaf area index is 4.5. A short summary on the site characteristics will be added.

Section 2.1.1 Appendix A
In this study the vertical profile of radiation is one of the main variables studied. A detailed light extinction scheme is represented in the appendix. It is said to be building on earlier work, with new addition of not averaging of intermediate results over the canopy. I wish that you provide better background for this and how this new scheme really differs (e.g. some of that is visible in Fig. 6 and you could discuss that there) and what is the importance of this new addition. In Appendix A the presentation of the scheme does not include really references to other work, except in the last paragraph of the appendix. It might be easier for the reader, if you would start with the references.
The text of the appendix will be altered to introduce the main elements (and references to the early studies that develop these concepts) that are part of this scheme in the first

paragraph. Further differences due to the use of distributions will be discussed when presenting the impact of these in the results, as suggested by the reviewer.

Did you evaluate the light extinction model? Now it is not that clear in the text. You're not having below-canopy observations of PAR, but you had other radiation measurements. How is the light attenuation compared to literature? Why did you not include clumping? It is generally considered to be important for coniferous forests (e.g. works by Stenberg & Smolander).

The light extinction model has been tested against analytical solutions for a number of standardized cases, e.g. cases with a spherical or horizontal leaf angle distribution and no scattering (for which analytical solutions exist). However, the main feature of the light extinction scheme, which is the distribution of light intensities at the leaf level, is hard to evaluate because of a lack of observations – a proper evaluation would require a large amount of sensors to capture the distribution. The objective with the light extinction scheme was to be comparable with the schemes used in large-scale models, so without detailed site information on e.g. leaf area distribution or clumping, hence this is ignored in the scheme. Compared with the large-scale models, it uses the same information on the canopy (LAI), but it computes the distribution of light instead of a mere average condition. This point will be highlighted in the revised manuscript.

p. 12450, l. 1: Do you assume constant O2 concentration?

Yes, the O_2 concentration is kept constant at 21

p. 12450, l. 9: There are different alternatives for the formulation of J, are you using the "standard" non-rectangular hyperbola or something else?

We use the standard description:

$$J = \frac{I + J_{max} - \sqrt{(I + J_{max})^2 - 4\theta I J_{max}}}{2\theta} \tag{1}$$

The equation will be added to the manuscript.

p. 12450: You should mention how you calculate transpiration. Now you only

mention aerodynamic conductance...

The transpiration flux was computed as a function of the concentration gradient of water vapour between the stomata (assumed to be saturated) and the canopy air, applying the stomatal resistance (based on the stomatal conductance) and the aerodynamic resistance in series. This will be added to the manuscript.

p. 12452, l. 9: It might be clearer, if you also say in the text what you mean by annual variability. It is explained in the table, but would be good to be in the text too.

Additional explanation will be added to the sentence to read: "These simulations were driven without annual heterogeneity (labeled as AHET in Table 2, applying an annually averaged vertical profile and diurnal cycle) for all parameters except one. Similarly, the simulations without diurnal heterogeneity (labeled as DHET, applying average daily conditions while maintaining the annual cycle and vertical profile) had the diurnal heterogeneity removed for all parameters except one. "

p. 12452, l. 25: You mention here the drought period in 1999. Do you have any explanation for the overestimation of modeled GPP around doy 180 in 2001?

This is a drought period as well. A remark about this will be added to the text.

p. 12453, l. 15: In the figure 4 you have negative values of GPP, which is basically unphysical, but is due to the method used to estimate the GPP from flux measurements. You could mention this.

A sentence will be added in the new manuscript to emphasize this: "Negative fluxes of CO_2 assimilation in the observations (Fig. 4g) are due to the method used to separate the net flux into CO_2 assimilation and ecosystem respiration, and represent the noise in the observation-based flux."

p. 12453, l. 25: You could mention that wind speed had no effect also in conclusions.

Will be added to the conclusions.

p. 12454, Section 3.2: You are here talking about differences between the tests, but you could introduce first how the model is doing during this time period, as there is a discrepancy between the observations and simulations in the beginning of this period. You could mention what is causing this.

A sentence introducing the results will be added: "For the period of the first case study, 18-22 May 1999, the CO_2 assimilation flux was captured well by the model, and the simulated transpiration flux was slightly underestimated for 18-19 May, whereas it was captured well for 20-22 May."

p. 12457, l. 2: It's not clear to me, what you mean by "optimal" in this case.

"Optimal" was not chosen well here. What we meant to say was a homogeneous (or even) distribution of the light, which results in the highest (or optimal) light use efficiency. This will be altered.

p. 12457: For the transpiration cases, you mentioned the effect on the annual balances. What is the effect of different tests for the annual GPP?

The full heterogeneity simulation is reasonably close to the observations with a 3% overestimation of the annual GPP (based on the part of the year for which data are available, in total 262 days). The difference between the full heterogeneity simulations and the ones that have more homogeneous PAR distributions is considerably larger: GPP is overestimated by 44% in case vertical heterogeneity of light is completely ignored (simulation HOM_PAR). For the simulation that ignores the sunlit-shaded distinction, but includes the layering (HOM_PAR_LAYER), the overestimation is 14% - much less, but noteworthy. We agree that this is important information and will add these numbers to the manuscript.

p. 12458, l. 3: I would rephrase this sentence. You have soil respiration occurring all the time, you only have less mixing and no CO2 sink during night... And you'd also have autotrophic respiration.

Sentence will be altered to "... during nighttime, when CO_2 assimilation has stopped, but heterotrophic and autotrophic respiration continue, while vertical mixing is reduced

in the canopy."

p. 12458 Fig. 9: In the figure, for d) and e) you are only showing the daytime graphs, because there is no photosynthesis taking place in early morning and late evening? Maybe you could mention this in the caption or in the text, it would clarify the figure.

Yes, sun rises shortly after 6.00 AM, and sets around 7.30 PM (Panel a), so the periods for which there is no light and hence no CO_2 assimilation were left out in (d) and (e). Clarification will be added to the figure caption.

Discussion, first paragraph

You are representing a summary of this study in the beginning of the conclusions. I'd rather see this in the beginning of the conclusions, maybe.

The discussion will be revised.

p. 12460, l. 10: Here you mention for the first time, that the nighttime fluxes did not always show clear temperature dependence. I think you could mention this earlier and tell here what are the implications of this. Did you subtract a constant value of respiration from NEE, or how did you do it?

In the cases where no clear variation with temperature was observed, the sensitivity to temperature (given by E_0 in Reichstein et al., 2005) tends to 0, which effectively means subtracting a constant respiration (determined as average for the period). We agree that this could have been mentioned earlier, a remark will be added to the explanation of the correction in section 2.1.2.

p. 12461, 2nd and 3rd chapter: It would be better, if you'd tie your own results with these results from literature.

More emphasis will be put in the comparison of the model simulations and analysis with the literature results in the revised manuscript.

p. 12462, 2nd chapter: Having a vertical gradient for the biochemical parameters is very widely used and might affect your results. I'd suggest that you do a

sensitivity test with the light heterogeneity test, where you implement vertical profile for the biochemical parameters to see, what is their importance.
We appreciate the suggestion by the reviewer and will perform such a sensitivity test for the revised manuscript.

p. 12462, l. 16: How big is the difference in the values biochemical parameters of Scots pine and Norway spruce? Do you have measurements of their different LAI distribution? If so, and if the difference is pronounced, you could make a sensitivity study of how the light distribution changes for the two cases and what's the importance.
The biogeochemical parameters are indeed quite different (with Pine having higher rates than Spruce both for the Rubisco-limited and for the electron transport limited CO_2 assimilation, see e.g. Wullschleger, 1993; Thum et al., 2008). This has been clarified in the discussion. However, testing the model's performance when accounting for the two tree species individually, as suggested by the reviewer, is unfortunately not feasible. Information on the LAI distribution of Pine and Spruce would be available, but the model is not set up to distribute the light between the two species, and doing so would require a major reformulation of the model, so that the absorption and scattering, as described in the appendix, is indeed performed for two (or more) species in parallel, to allow for mutual interaction between the two species. We keep the suggestion in mind for future studies (potentially with a simplified version of the light extinction code), and will clarify this in the text.

Conclusions

In the large scale models the atmospheric CO2 concentration is often taken to be annual mean. This is of course not really a topic about vertical heterogeneity, but it might be interesting to check, how large influence this has on the results (instead of using observed CO2 concentration).
The simulations used in this paper do not give an exact answer to this, although the AHET_CO2 simulation (for which CO_2 was the only factor varying annually – all other

drivers were kept at their annual mean – see Fig. 10) gives a suggestion that it is of little importance. For testing this, we have performed an additional simulation doing exactly the opposite (all drivers varying annually except for CO_2), which resulted in an overestimation in GPP of 1.5% (and an even smaller overestimation when using diurnally averaged CO_2). Although this result is interesting as background information, we consider it too far outside the scope of this paper to be added, in particular because it would need to introduce yet another simulation setup to the reader.

Can you give some kind of estimate of the contribution of the ground vegetation to the observed GPP? It is now not mentioned, but it would likely contribute to the GPP, even though it's likely a small contribution.
According to a rough calculation based on turnover estimates, NPP from ground vegetation contributes less than 10% to the total NPP (Fredrik Lagergren, unpublished results), which is in the same range as for similar Swedish sites (Berggren et al., 2002). We expect the contribution to total GPP to be a similar fraction, and will add this to the manuscript.

Technical corrections
We thank the reviewer for the technical correction and will adjust the manuscript accordingly. We will adopt "CO_2 assimilation" rather than "assimilation" in the text.

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