

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

Anonymous Referee #1

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General comments

Schurgers et al. have made a study at a Swedish coniferous forest, where they have vertical profiles of atmospheric CO₂ concentration, air temperature and air humidity available, in addition to eddy covariance measurements. They have built a canopy level model and a radiative transfer model. They infer, whether using the observed profiles (or modelled PAR levels) at different heights of canopy has a lot of influence to the modelled values of GPP and transpiration instead of using average values (or above canopy value for humidity/CO₂ concentration). They find out that their modeling results are improved when they have what they call “vertical heterogeneity” in their model, i.e., they are using vertically observed variables as model inputs.

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The study addresses issues that are worthwhile to study. Overall, the analysis is good, the paper is well-written and the graphs are illustrative and clear. I recommend the paper to be published, after the authors have addressed some of the suggestions I present below.

Specific 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.

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.

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.

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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).

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

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

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

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.

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

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.

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

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

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?

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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.

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.

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.

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?

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

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.

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.

Conclusions

In the large scale models the atmospheric CO₂ concentration is often taken to be

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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).

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.

Technical corrections

You are talking a lot about "assimilation." This term itself is confusing, I suggest replacing it by "CO₂ assimilation."

p. 12446, l. 11: You have defined GPP in the abstract but maybe not in the main body of the text. If so, please do it here. And you could show NEE with net ecosystem exchange, because you're using it later.

p. 12449, l. 10: Instead of "driving forces", would "drivers" be better?

p. 12450, l. 15: After "stomatal conductance" it might be good to add g_s , as you're defining that elsewhere.

p. 12456, l. 14: It's not that clear immediately to what you're referring with "This." Rephrasing would make the beginning of the paragraph clearer.

p. 12459, l. 4: Might be clearer to say: "has therefore negligible impact"

p. 12466, l. 21: "gives" might be better than "return"

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