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Comment

Interactive comment on "Assessing seasonality of boreal coniferous forest CO_2 exchange by estimating biochemical model parameters from micrometeorological flux observations" by T. Thum et al.

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General comments

In this manuscript the seasonal patterns of photosynthetic parameters are estimated for 4 coniferous forests using GPP data derived from eddy covariance measurements. To my opinion, this study is very relevant and the use of eddy covariance data to study model (parameter) behaviour is an interesting way to study ecosystem processes. In general this paper is well written but a clear focus is lacking. Moreover, several parts

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need improvement as I mention below in my specific comments. I therefore suggest accepting this paper for publication after major revisions.

Specific comments:

1. The title is not consistent with the content of the paper. The paper is dealing with the seasonality of model (photosynthetic) parameters and not with the seasonality of carbon exchange as the title suggests.

2. I agree with the comment of J. Kattge that this paper is not well focussed in general. I think several things are merged which makes the story a bit confusing. I think the manuscript will improve a lot when the authors make an effort to separate the effects of the seasonality of the parameters (Vcmax, Jmax) at reference temperature, the temperature response of these parameters and the seasonal pattern of the leaf area. I think the use of different symbols for actual parameters and the parameters at reference temperature (as suggested by J Kattge) is a good start to make this clearer. I think it is a real challenge to separate between these effects. The authors show in the 'sensitivity analysis' that a different LAI yields different photosynthetic parameters. This part should be further developed to a sensitivity analysis that studies the different processes determining the seasonal pattern (LAI, temperature, leaf aging ...). In addition, it would be interesting to see if the introduction of seasonality (in the LAI, the parameters or the temperature response) improves the model or not. Does the model perform better when you use different temperature function parameters depending on the season? If you want to test the performance of different model formulations, I suggest using other indices in addition to r^2.

3. Some essential information on the inversion is missing. I suppose that the difference between measured and modelled GPP was minimized. But it was not explained how. Which kind of minimization algorithm was used? I think this is quite essential as the parameter estimation is the core of this paper.

4. It is not clear for me how the changeover dates are determined. Did the authors

estimate the temperature dependence parameters every day and looked for significant changes of the parameters afterwards? Or were the changeover dates determined in another way? Just arbitrarily?

5. There is no estimate of the uncertainty on the estimated parameters. The use of GPP estimated from NEE (page 2713) introduces some uncertainty. At least a short discussion on the error introduced here would be appropriate.

6. The multiplication of the parameter values by 'pi' seems to be quite essential (p 2723, line 17) when you want to compare your parameter values with literature. I suggest to explain this more clearly in the material and methods section. Because this puts the low parameter values that you find for Vcmax and Jmax in another perspective and it is a pity for the reader to find that out only in the discussion.

Minor comments:

1. p. 2717, line 27: I don't see the large drop in figure 7a

2. p. 2718, line 11-12: Does this mean that there is no effect above this threshold?

3. p. 2722, line 19-20: I see only 2 relationships in fig 9 a.

4. Table 2: please mention in the caption which variable is used to calculate r^2: GPP, NEE?

5. fig 1-4: can be merged in one combined plot.

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