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Biogeosciences Discussions

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.

J. Kattge (Referee)

jkattge@bgc-jena.mpg.de

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General comments:

The authors use Eddy-Covariance (EC) data from four boreal coniferous forests to determine estimates of the two parameters quantifying photosynthetic capacity in a canopy photosynthesis model based on the biochemical model of photosynthetic CO2 assimilation according to Farquhar, von Caemmerer and Berry (1980). While the idea in general is highly appreciated the implementation of the inversion and the interpretation of parameter estimates are not straightforward yet. Nevertheless some of their results seem to be novel and reasonable. Therefore, I would suggest to accept the manuscript, but major changes are required.

Specific comments:

The manuscript presents values of Vcmax and Jmax estimated throughout the growth period at a range of different air temperatures from 0°C to 30°C. Within the observed range of air temperatures these values in principle show an exponential relationship to air temperature. Even if the slope of the relationship may be highly uncertain - which will be discussed later - the exponential relationship within the observed range of temperature seems to be robust. This is remarkable, as it is in agreement with the average behavior obtained in a reanalysis by Kattge and Knorr (2007) and in a review by Hikosaka et al. (2006), but it is in contradiction to leaf level measurements on boreal needle leaved trees by Aalto (2002) and Wang (1996), which indicated exceptionally low temperature optima of Jmax below 25°C (Wang, 1996) or even below 20°C (Aalto, 2002). Therefore it is a remarkable result of this inversion against Eddy Covariance data, that Jmax of boreal needle-leaved trees seems to behave rather like the general average than like the exceptional estimates by Wang (1996) and Aalto (2002).

Within the observed range of temperatures it seems reasonable to use the Arrhenius function to describe the relationship of Vcmax and Jmax to ambient temperature, instead of using a more general peaked function, especially when accounting for temperature acclimation obtained for peaked functions (Kattge and Knorr, 2007). Comparing the activation energy determined in this study for the summer periods to the average activation energy in Kattge and Knorr (2007) (about 58000 J/mol for Vcmax and about 45000 J/mol for Jmax, these results based on the Arrhenius model were not published), shows that the activation energy determined in this study on average is higher, but within reasonable ranges.

However, to my mind, the manuscript contains several problems and inconsistencies

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with respect to its focus, the methods used to derive Vcmax and Jmax values and with respect to the interpretation of estimated Vcmax and Jmax values.

Focus: The title does not match the content of the manuscript. While the title is formulated: “Assessing seasonality of boreal coniferous forest CO2 exchange by estimating biochemical model parameters from micrometeorological flux observations”, the topic of the MS is the seasonal variability of canopy photosynthetic capacity. Autotrophic and heterotrophic respiration, which are important parts of the CO2 exchange, are only roughly estimated and subtracted from measured NEE to derive GPP. Calculated GPP, which is a flux on canopy scale, is then used to invert Vcmax and Jmax. Vcmax and Jmax are linked to observed GPP via the canopy module of the model and leaf are index, LAI. LAI seems to be assumed to be constant throughout the year. Therefore seasonal changes of LAI are not considered but ascribed to estimates of Vcmax and Jmax. As the title does not match the content, to my mind, the MS is not well focused yet in general. To my mind several parts of the MS may be omitted (e.g. “different ways to estimate Jmax” based on the correlation of Vcmax to Jmax. The obtained results may be compared to this relationship. But if you do so, please do not use the relationship derived by Wullschleger (1993) from data not corrected for different temperatures, because the relationship is extremely temperature dependent).

Accuracy of formulation: The difference between Vcmax and Jmax at ambient temperature, Vcmax and Jmax at standard temperature and the temperature dependence of Vcmax and Jmax is not well taken into account throughout the text, tables and figures (table 1: f0 and at 17°C, figure 5 -8: daily values of Vcmax and Jmax: which temperature?). The authors should introduce separate names for Vcmax and Jmax corrected to standard temperature (Vcmax,std and Jmax,std) and strictly use the formulations appropriate in the given contexts. For example, whenever Vcmax and Jmax are compared to other estimates, the temperature corrected values should be used. This holds as well for the ratio of Jmax to Vcmax (Jmax,std/Vcmax,std) and for Vcmax and Jmax

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in Figures 5 to 8. Using Jmax and Vcmax without temperature correction does not make sense in these contexts. Being very strict and accurate with these formulations will also help to disentangle inconsistencies, as it will help to separate temperature dependence of model parameters Vcmax and Jmax from other influences like changes in Vcmax,std and Jmax,std e.g. caused by frost damage or variability of LAI (see below). In the Farqhuar model Vcmax and Jmax indeed have a biochemical meaning, and therefore they should, if possible, not be entangled with other influences. If the meaning of Vcmax and Jmax in the model context is switched, this should at least be carefully discussed. I would suggest that the standard temperature should be within the observed range of temperatures (e.g. 17°C). Currently the fitted parameter values (f0 Eq.3 and table 1) is outside the observed range, most probably at 25°C. I would suggest to replace this with the parameter value at 17°C and only provide 20°C, 25°C parameter values for comparison to other data.

Method of flux partitioning: First the authors subtract needle respiration, estimated from chamber measurements and leaf mass, from night time EC data of NEE, then they fit the a soil respiration model based on air temperature against bi-weekly time series of the difference. Leaf respiration and soil respiration are subtracted from NEE to derive GPP, which is then used to estimate Vcmax and Jmax. Here two problems may occur, that at least need to be discussed with respect to uncertainties introduced in the estimates of Vcmax and Jmax: 1) The model of temperature dependence of needle respiration (exponential ?) may introduce major uncertainties for high summer temperatures: is the temperature dependence of needle respiration exponential - or limited by carbohydrates and therefore on seasonal timescales not-exponential? 2) Especially in spring the air temperature may not be a good proxy to derive soil respiration, as at the same air temperature the soil may be frozen or already melted - which, I guess, may make a major difference with respect to soil respiration.

Interpretation of inverted values of Vcmax and Jmax: Vcmax and Jmax are inverted against eddy-covariance data, which are measurements on canopy scale. Therefore

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all influences on canopy scale have to be taken into account, when interpreting the inverted value. On canopy scale at least 3 different processes are involved in the seasonality of photosynthesis: 1) Seasonal variability of leaf photosynthetic capacity, expressed in the model by Vcmax and Jmax at standard temperature: in spring recovery from frost-hardening, summer values constantly high (?), eventually drought effects, in autumn frost-hardening. Frost events during recovery from frost-hardening in spring may have an important impact on photosynthetic capacity (please see Ensminger et al. GCB 2004). 2) Temperature dependence of leaf photosynthesis, in the model expressed by temperature dependence of Vcmax and Jmax. 3) Seasonal variability of leaf area index (LAI) e.g. due to new needle leaves, and some deciduous trees in the footprint of the EC site in Hyytiälä. These influences have not been separated and taken into account appropriately. Therefore, I guess that the observed correlation between temperature and Vcmax and Jmax during summer may not only represent the temperature dependency of Vcmax and Jmax, but may be the result of the combined effect of temperature dependency of Vcmax and Jmax and varying photosynthetic capacity (Vcmax,std and Jmax,std) and varying LAI. Nevertheless, I think that the estimates of Vcmax and Jmax from summer EC data are quite robust, but these influences and the related uncertainties have to be discussed - at least. More important, I assume, that the interpretation of the correlation of spring photosynthetic capacity to air temperature is wrong: the observed correlation between temperature and Vcmax and Jmax is most probably not the effect of the temperature dependency of the model parameters Vcmax and Jmax, but this correlation is n first order the result of a recovery of photosynthetic capacity after frost hardening which is repeatedly intermitted by frost events (please see Ensminger et al. 2004). As the frost events are known, this impact can easily be taken into account.

In some plots some of the fits look spurious (Vcmax April Hyytiälä). Please do not extrapolate the fitted functions to the temperature range of the plot.

Minor comments:

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- Description of method should be more focused and more exactly.

- Please add table with characterization of sites.

- Table 2 is not necessary.

- Figure 5-8: please use temperature corrected parameter values: $V_{cmax,17}$ and $J_{max,17}$. V_{cmax} and J_{max} are higher in summer than in spring and autumn due to higher summer temperatures - this is trivial.

- Figure 9 is not necessary.

Interactive comment on Biogeosciences Discuss., 5, 2707, 2008.