Response to Referee #2, 28.2.2017 by Thum et al.

The authors use GPP data derived from CO2 fluxes measured at 4 boreal forest sites, together with SIF derived from the GOME satellite and leaf-level active fluorescence data to test a new version of the land surface model JS-Bach, which has been updated with a description of ChlF fluorescence. Finally, JS-Bach is applied at regional scale.

The authors demonstrate overall good correspondence between measured and simulated GPP (which was calibrated though) and satellite SIF and site-level GPP and reasonable correspondence to leaf-level active fluorescence data. SIF compares better to measured GPP compared to remotely sensed fAPAR.

I think this is a useful and original contribution. My comments are mostly meant to improve clarity, which the ms frequently lacks.

We thank the referee for these comments and hope that our responses to the comments are able to clarify the manuscript.

Detailed comments:

p. 3, l. 6: as ecosystems exchange various forms of carbon, use carbon dioxide if you actually refer to carbon dioxide

Indeed, the aim was here to refer only to carbon dioxide, not to methane or VOCs, so we made this addition to clarify the text.

p. 3, l. 11: strictly speaking this is only true for fAPAR, while NDVI is just the normalized difference between reflectances in NIR and red, which happens to correlate with fAPAR

We totally agree with the referee and this was a language issue. We replaced "which" by "describing", hopefully now clarifying the issue.

p. 4, l. 1: I would contradict the "readily", given that we are still far from a truly process-based description of SIF; the Farquhar model though offers most of the interfaces for coupling to SIF

We agree with the referee. We took away the word "readily".

p. 4, l. 12: "Both these regions . . ."

Thank you, this is now corrected in the text.

p. 4, l. 15-15: here you might explain why you focus on spring and autumn

A good point. We added here the following text: "*Forests in the boreal zone experience strong seasonal cycle with cold winters and warm summers (Bonan, 2008). The transition periods of spring and autumn influence the carbon balances in these northern ecosystems (Bergh et al., 1998). In changing climate the conditions in spring and autumn will change (Ruosteenoja et al., 2011)and cause changes to carbon balances. It is anyhow during these times that the carbon cycle models have difficulties in performance (Schaefer et al., 2012). Therefore it is important to find ways to improve carbon cycle models in these time periods.*"

p. 4, l. 19: here you haven't mentioned yet that you did implement SIF into your LSM

Thank you, we changed the text so that we separately mention the implementation.

p. 4-5, section 2.1: while this section clarifies some of the basics, it entirely lacks details, such as which instrument was used for active measurements in the field and how the experimental protocol was, etc. – I see this comes later, so an appropriate header reflecting this is required here

A good point again, we added "in general" to the title in order to clarify the issue.

p. 6, l. 6-7: the acronyms/abbreviations do not make sense – maybe use subscripts like dir and dif to distinguish between direct (beam) and diffuse radiation;

Indeed, the used abbreviations were not that clear. We used referee's suggestion to distinguish between direct and diffuse radiation.

Wouldn't the equation be easier to understand if fAPAR was calculated as the difference between the radiation balance at the top of canopy (layer 1) minus the radiation balance below the lowermost layer (layer 3);

We understand the referee's point, but we have a three –layer canopy and here we wanted to show how to calculate fAPAR for each layer. As said in the text, the canopy fAPAR is the sum of fAPAR from different layers.

replace "transmitted" by "used" or similar

We made that.

p. 6, l. 12: ". . . is used .."

Thank you, corrected.

p. 6, l. 16: typically the temperature dependency of Jmax is either exponential or even follows an optimum shape

We agree, but in this study we decided not to make any changes to the original formulation of the JSBACH model, that would have required then some additional evaluations of the model performance.

p. 6, l. 17: isn't the value of alpha typically around 0.05 (mol CO_2/mol photons)

We're talking here about apparent quantum yield, not the intrinsic quantum yield. We modified the text to say apparent quantum yield, which is the true quantum yield multiplied by the light absorption in the leaf (Walker et al., 2014). This value (0.28) is the default value of JSBACH for all the different plant functional types. The parameter optimization study by Mäkelä et al. (2016) done by JSBACH at site level for FI-Hyy and FI-Sod, showed this value to be good for the two sites.

p. 7, l. 19: "obtained" – use past tense throughout

Thank you, we corrected that.

p. 8, l. 19, 24: two times same header numbering

Thank you, we corrected that.

p. 9, l. 5: is it a good idea to introduce a bias into the data? Isn't there some other way to deal with the negative values?

We have redone the analysis by including the negative values in the analysis.

p. 9, l. 9: does this explain how fAPAR is derived? I mean in the sense that a reader should be able to repeat the author's approach?

What the author did related to fAPAR was to ask Thomas Kaminski for these data, that he kindly provided and even took out data for each site. These values were obtained by partitioning the solar radiation fluxes that were based on inversion of the MODIS broadband white sky surface albedos and the reference for those data is given here. As we did not do the laborious processing it takes to obtain those data, we did not go into details here.

p. 9, l. 18: what does "adjusted" exactly mean? Which metric did you use to measured the success of the "adjustment"?

We didn't perform here any rigorous tuning with profound mathematical methods, like done by Mäkelä et al. (2016). Instead, we matched the averaged LAI value to the observations. We did not consider building a model tuning framework for this work, as we're not dealing with the absolute GPP values from the sites, but instead we use our modelled time series to assess the seasonal behavior of the model. We added sentence: "*No rigorous parameter inversion methods were used, as we did not use the absolute GPP values in our study, but focused more on the seasonal behavior.*" to the text.

typically, Jmax is linked to Vcmax through the ratio of the two – was that done here too, i.e. only Vcmax adjusted and Jmax "followed" based on the relatively conservative ratio of the two?

Yes, the original ratio between Vcmax and Jmax was 2.1, and we kept this same ratio, by changing first Vcmax and then always Jmax accordingly. We added this point to the text.

p. 10, l. 14: what exactly means "most" in this context?

Apologies, that was unclear. We did the analysis for points having larger fraction than 0.5 for the vegetation, but plotted all the points to the map in Fig. 4. We clarified this.

p. 11, l. 1: doesn't the term "midday depression" refer to the drought-related midday decrease in leaf net photosynthesis and stomatal conductance?

Yes, we took that away, as it's not in the right context here.

p. 16, l. 2: "wider footprint" – be more precise . . .

We added (e.g. for GOME-2 default footprint is 80 km x 40 km) in parenthesis to be more explicit.

Fig. 1: might be worth commenting on the negative measured GPP values

This is a good addition. The negative values are originating from measurement uncertainty. The GPP is obtained from the observed NEE by subtracting the respiration fitted by temperature regression. The temperature fit to respiration adds some systematic error to the GPP estimate. We wrote to the manuscript:

"*Some negative GPP values are present in Fig. 1. The random nature of turbulence and instrument uncertainty add to measurement uncertainty (Rannik et al., 2016). The GPP is obtained from the observed net ecosystem exchange (NEE) by subtracting the respiration that has been estimated by a regression fit to temperature (Wohlfahrt and Galvano, 2017). Thus the random measurement uncertainty leads to some negative GPP values that are compensated by equal amount of too high positive values, additionally the temperature fit to respiration causes some systematic error in the values."*

<u>References</u>

Bergh, J., McMurtrie, R. E., and Linder, S.: Climatic factors controlling the productivity of Norway spruce: a model-based analysis, Forest Ecol. Manag., 110, 127–139, 1998.

Bonan, G.B.: Forests and climate change: Forcings, feedbacks, and the climate benefits of forests, Science, 320, 1444–1449, 2008.

Mäkelä, J., Susiluoto, J., Markkanen, T., Aurela, M., Järvinen, H., Mammarella, I., Hagemann, S., and Aalto, T.: Constraining ecosystem model with adaptive Metropolis algorithm using boreal forest site eddy covariance measurements, Nonlin. Processes Geophys., 23, 447-465, doi:10.5194/npg-23-447-2016, 2016.

Rannik, Ü., Peltola, O., and Mammarella, I.: Random uncertainties of flux measurements by the eddy covariance technique, Atmos. Meas. Tech., 9, 5163-5181, doi:10.5194/amt-9-5163-2016, 2016.

Ruosteenoja, K., Raisanen, J. and Pirinen, P.: Projected changes in thermal seasons and the growing season in Finland, Int. J. Climatol., 31, 1473–1487, 2011.

Schaefer, K., et al.: A model-data comparison of gross primary productivity: Results from the North American Carbon Program site synthesis, J. Geophys. Res., 117, G03010, doi:10.1029/2012JG001960, 2012.

Walker A. P., Beckerman A. P., Gu L., Kattge J., Cernusak L. A., Domingues T. F., Scales J. C., Wohlfahrt G., Wullschleger S. D., Woodward F. I. (2014) The relationship of leaf photosynthetic traits – Vcmax and Jmax - to leaf nitrogen, leaf phosphorus, and specific leaf area: a meta-analysis and modeling study. Ecology and Evolution 4, 3218-3235, doi: 10.1002/ece3.1173.

Wohlfahrt, G. and Galvano, M.: Revisiting the choice of the driving temperature for eddy covariance CO_2 flux partitioning, Agric. Forest. Met., 237-238, 135-142, 2017.