

Interactive comment on “Improved light and temperature responses for light use efficiency based GPP models” by I. McCallum et al.

Anonymous Referee #2

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The manuscript "Improved light and temperature responses for light use efficiency based GPP models" presents a study comparing different models of gross primary productivity fitted to four eddy covariance sites in Russia. The authors show that accounting for temperature acclimation and non-linear response to radiation improve the performance of light use efficiency models compared to the original formulation (relying on an instantaneous temperature response ramp and linear radiation response). The study is a timely contribution as the C uptake processes of vegetation are important for the climate system and their integration into global circulation models is currently an important topic of research. The contribution also falls well within the scope of Biogeosciences.

The study is well conceived and executed. My major concern is that as it is, the paper

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is more or less stating the obvious, i.e. that more complex, parameter-rich models are better able to reproduce data after fitting than more parsimonious approaches. Don't get me wrong: I see considerable potential in the study. However, I think that its value could be improved considerably by not only looking at GOF indicators but also keeping parsimony trade-offs in mind. E.g. a penalization of parameter-rich models (like in AIC) would help to inform the reader on whether using a more complex model is a good trade-off for the respective increase in GOF. It appears, for instance, that including temperature acclimation improves the original formulation quite a lot, while the additional improvements by the BL formulation (which is considerably more complex than LUE-TA) are only minor.

My second issue is with the current description of model fitting and evaluation. Although the authors claim that they want to "present a methodology for comparing diagnostic modeling approaches" the actual methodology is not very well described - I'd certainly not be able to reproduce it from the information given in the manuscript, which forfeits the original claim and intention. To be more specific: Did you test ALL the combinations of possible parameters in your calibration, or did you use a form of stratified sampling, prior distributions, etc.? How was the initial parameter range conceived and what was the step width in the search? Table A1 is not very clear but if the "increment" column is indeed the step width in your search, is 3°C really fine enough to identify temperature thresholds (re the analysis by Mäkelä et al. 2008, GCB)? In the evaluation, how often was the "leave n out" operation performed? How was the n of 10 determined (and what percentage of the overall N is this)? Were all models evaluated against the same N minus n dataset, or was the sampling done separately for every model? If you really want to demonstrate a methodology to compare different model formulations - great, but please demonstrate a little bit more clearly and don't spare the details necessary for replication.

My final point is on the reoccurring theme that the results are important for studies on "the regional level". I tend to agree in general that a global parameter set is likely

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inferior to regional parameterizations, but then again it's not clear what "regional level" means in the context of this paper (which looks at four sites (= stand level) distributed over the large land mass that is Russia (= continental scale)). What is the regional level that we're talking about here? Provinces? Landscapes? Stands? And are you concerned more with the within-region variation, or with the variation between regions on the continent? Please be more specific. Secondly, it seems that the authors imply that their (very tentative) latitudinal parameter gradient (Figure 2) could be used to determine parameters for regional applications of models (re: last sentence, page 8931). This is contradictory to the previously stated importance of regional parameterizations, and implies that latitude can explain differences in physiological processes, which I'd be highly doubtful about (and the four-point regression in Fig.2 doesn't do much to dispel my doubts in this regard).

More detailed technical comments

Abstract, l5: "applying global results at regional levels"... unclear - do you mean global parameterizations to local applications?

p8921 l3: what is a "spatial frequency" in this context?

p8924 l11: the authors name is actually Mäkelä (with umlauts)

p8924 l11: can you show what X_0 and t do in the model? They are not contained in Eq. 2

p8926 l15: parenthesis before Reichstein

p8930 l1: generally outperform

p8930 l10: refinement in measurements... well, your work does definitely not address this; should it rather be refinement in models/ model parameterizations?

p8930 l15: this is nothing new, as many previous analyses have shown that the linearity of the relationship only applies at temporal scales of >3 weeks (which is also why many

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LUE models, such as 3-PG operate on monthly time scales) - see the synthesis by Medlyn et al. (2003, *Funct. Plant Biol.* 30, 153-169).

p8931 I9: gross primary productivity estimates

Table A1 is not very clear and well explained. I at first didn't understand what 0:2500 ment - could you introduce two columns for the min and max range? Also, the heading should be self-explanatory, so please elaborate on the abbreviations. It would also be great to somehow see which parameters are used in which model versions.

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