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

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We greatly appreciate the time and effort the reviewer has taken and the constructive comments they have provided. We are pleased that the reviewer sees considerable potential in the study.

Firstly, we agree that AIC is a good and widely accepted measure of model performance, which takes into account both the goodness of fit and the parsimoniousness of the model. However, AIC requires distributional assumptions in order to evaluate the log-likelihood. In our case, the parametric formulation of likelihood appears to be very difficult and assumption of normality of residuals or anything similar is highly unlikely to hold. That is one of the reasons we have decided to use cross-validation which does not require any distributional assumptions. The other reason is that we are actually

interested in predictive power more than in explanatory power.

We disagree that the cross-validation does not take parsimony into consideration. A higher number of parameters might mean better fit, but it does not necessarily mean better prediction due to resulting volatility of the estimates. It has in fact been shown in Stone M. (1977) 'An asymptotic equivalence of choice of model by cross-validation and Akaike's criterion.' JRSS 39: 44-7, that the cross validation and AIC are asymptotically equivalent. We have also found Forster M (2000) 'Key concepts in model selection: performance and generalizability' Journal of Mathematical Psychology, 44(1):205-241 as a useful paper comparing various model performance measures. We will endeavor to fully explain the cross-validation methodology in the revised paper and the reasons why we chose this method.

Secondly, we agree that the paper could benefit from an improved description of the methodology. In brief, we did test ALL possible combinations of parameters in our calibration. The initial parameter range was conceived by consulting the existing literature (see Table A1 for references). The step width is indeed the increment listed in Table A1. It is a fairly coarse increment as initial testing indicated that the optimum was fairly insensitive to changes in the values. Furthermore, using a smaller step width greatly increased the computing time required. We have meanwhile addressed this issue thus a finer resolution could easily be applied. The leave 10-out operation was performed a similar amount of times for each model at each tower and only depended upon the amount of data available for each tower. For towers with low amounts of data, this was a larger proportion of the data than towers with many points.

Finally we agree that the current text is somewhat unclear about what is meant by "regional level" and this needs to be clarified. This study only looked at four tower sites spread across Russia. We do however want the reader to keep in mind the possibility/potential to upscale findings. By region we are referring to the landscape level or biome. Within region variation is very important and not negligible, however with only a few observation points we are more interested in the variation between regions in Russia.

Furthermore, the reviewer is correct to caution against reading too much into Figure 2. We certainly did not intend to imply that latitude can explain differences in physiological processes. We will consider removing this from the revised paper, replacing it with perhaps additional tower results.

We will address the detailed technical comments in the revised version of the paper.

Thankyou

Interactive comment on Biogeosciences Discuss., 10, 8919, 2013.