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Interactive comment on "A simplified gross primary production and evapotranspiration model for boreal coniferous forests – is a generic calibration sufficient?" *by* F. Minunno et al.

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Minunno et al., presented a study that assess PRELES model GPP, ET predictability using different calibration data. The modeling exercise was carried out successfully, and the paper itself is well written. The sensitivity analysis, calibration and uncertainty evaluation are comprehensive, worthy of publishing. However, several issues still warrant further explanation and investigation, before the paper could be published on GMD. Reply: We thank the reviewer for finding the work interesting and for providing us with helpful comments.

The primary concerning is the novelty of this study. A similar work was published

early this year. Peltoniemi et al., 2015 used two boreal forest EC tower data (Hyytiala and Sodankyla) to calibrate PRELES model. They highlighted the difference in model parameters and predictions when the model was calibrated by different EC tower data. I didn't see much added value of this calibration study, compared with that similar study (Peltoniemi 2015) (same model, similar calibration data and method). I also want to mention that this study drew a conclusion different from Peltoniemi 2015, even though the two EC sites involved in Peltoniemi 2015 were also included in this study. Reply: (*) In this work we went a step further from Peltoniemi et al. (2015) since we were testing the regional applicability of PRELES. We analyzed model behavior using a much bigger dataset that covered higher variability in terms of species composition, soil characteristics, climatic conditions, stand age, etc . . .Thanks to the insights gained now we can draw stronger conclusions about the regional applicability of the model. We also used a wider set of analysis techniques, including Bayesian model comparison, global sensitivity analysis and model-data mismatch analysis.

I am quite surprised by the major results. I expected that (site-specific) S-S calibration and (multiple-site) M-S calibration should be different due to spatial heterogeneity of the 10 boreal forest EC tower measurements. And intuitively, more data should lead to a better model, unless the data are redundant, which should not be the case for EC tower measurements. The author found no significant difference of (site-specific) S-S calibration and (multiple-site) M-S calibration, in terms of parameters estimations and predictions. This is the most important message the paper delivered. I think the author should discuss such conclusion more thoroughly. Reply: This is definitely one of the main findings of this study and we emphasized more this concept in the manuscript. Please see the Discussion section.

I have done lots of model calibration. My experience is that plant functional type (PFT) (in this case is boreal coniferous forest) is basically not enough; large variations exist at species level. The 10 EC tower are classified as the same PFT, but they are different in terms of plant species. The model parameters for different plant species should be more or less different. Reply: This aspect also makes this work interesting and novel, since we are showing that GPP and ET of boreal coniferous forests can be modelled using the same parameterization. We expect that there is more variability in terms of carbon allocation and growth within the PFT and also at intra-species level, but our results support the fact that photosynthesis and water fluxes can be well represented by a simple model like PRELES. This is not to say that all parameters are the same, only that a quantitatively sufficient representation can be achieved using a single set of parameters. In a closer look of some parameter values as a function of species, such as the rate of spring recovery for example, one may find distinct differences.

This is because that different plant species reside in different ecological niches; therefore, they have different tolerance to environmental changes, different temperature sensitivity and different light response. This is part of the reason why site-level calibrated model often failed when it was extrapolated to a large-scale region with the same plant functional type. Related to that, my second major concerning is that: Why the model calibrated at one EC tower could be representative at other sites, given that the representativeness of EC tower is just about a few kilometers (scale of its footprint)? Please dig into this question thoroughly in the discussion section. In addition, PRELES model is a simple flux model, detail complex mechanistic interactions (e.g., nutrient constraint), physiological responses (e.g., drought tolerance) are simplified. However, EC tower data are heterogeneous and they intrinsically contain those types of information (detail interactions and responses). Then the question become: why a model that does not consider those mechanisms could reproduce EC tower observation reasonably. In order to reproduce the 10 EC tower heterogeneous observations, I would expect that PRELES need site-specific calibration for site-level application multiple-site calibration for multiple site calibration due to the model simplicity, unless those complex interactions are just second order significance at those EC tower sites. Reply: These are very good questions and we thank the reviewer for pointing them out. We can think of several possible reasons why this could be, and have now included them in the revised Discussion of the manuscript. The eddy measurement as such has a ten-

dency to average over sites, because the footprint area covers a wider area than the actual central site used for model parameterisation. The forest landscape in Finland and Sweden is quite heterogeneous, such that the footprint area could cover different soil types and species. We believe that this could explain some but certainly not all of the similarity of the sites included here. A more thorough analysis of the footprint area and the contribution of the surroundings would be required to asses this quantitatively, which we believe is beyond the scope of this study. The relative independence of GPP and ET of site and species has also been found in previous studies (e.g. Medlyn et al. 2005, Duursma et al. 2009), although their respective canopy structure and tolerance of stresses such as drought and shade has been found rather different (e.g. Woodward and Williams 1987). One should note however that the effects of stresses on trees are not only mediated through GPP and ET but also involve other processes such as growth and turnover which are not included in this study. These reactions probably differ more among species than their primary physiological rates. Our results indeed suggest that at the observed error level, the factors not represented by PRELES (e.g species, soil type etc) are of second order importance for GPP and ET (given the current leaf area index or fAPAR of the stand). We might interpret this as a quantification of the information you can gain by increasing model complexity. In this case, we could say that any conifer forest in Fenno-Scandia can be modelled with that accuracy with a big leaf type of model.

Other minor comments: Page 3. Line 2. material and energy -> mass and energy Reply: Changed as suggested.

Page 3. Line 4. weather variables -> climate forcing Reply: Changed as suggested.

Page 4. Line 5. applied in -> applied to Reply: Changed as suggested.

Page 4. Line 9. And the resulting model was found to describe daily GPP rather generally and independently of site, rephrase Reply: The sentence has been rephrased.

Page 5. Line 10. test and calibration -> calibrate and test Reply: Changed as suggested.

Page 5. Line 27. to -> against Reply: Changed as suggested.

Page 8. Eqn. 14. What is S_k? Should it be just "S"? What does the k represent? Reply: Yes the k index is not needed and was deleted.

Page 9. Line 19. a and x are empirical parameters. Are they just scaling parameters? No physical meaning? Am I right? Please explain them a little bit more. Reply: Yes they are scaling parameters with no physical meaning.

Page 10. Line 4. Table 2 appears before the appearance of Table 1. Reply: The numbers of the tables were changed.

Page 10. Line 26. Eq. (22). Should it be Eq. (21)? Reply: The equation number has been corrected

Page 11. Line 5. Eqn. (22) -> Eq. (21) The equation number has been corrected

Page 13. Line 14. also -> removed Reply: Changed as suggested.

Page 16. Line 23. The results for Hyytiala are representative of all sites except Norunda. Figure 2 only show the SA result for Hyytiala and Norunda. Could you plot out SA results for the other 8 sites (e.g., in supplementary material)? Why Hyytiala is representative? In Page 13, you stated that sensitivity is regulated by soil moisture status, site conditions and weather inputs. Does it mean that Hyytiala's soil moisture, weather inputs are representative of the other 8 EC sites? Reply: Even though μ^* and σ assume different values across the different sites parameter rankings are the same (except for Norunda), therefore Figure 2 is well representative of the importance that parameters have on model output. Added new plots will not give any additional information about model sensitivity.

Page 18. Line 9. In MS- calibration the highest correlations were between beta and gama. Any implications? Does high correlation affect model calibration? If does,

in a good way or bad way? Does it mean that variations of those parameters could cancel out each other? Reply: Bayesian calibration allows to handle with parameter correlations since it provides a joint posterior distribution. We included in the paper a section, giving information about how to retrieve model code and parameter posterior distribution.

References Duursma, R. A., Kolari, P., Perämäki, M., Pulkkinen, M., Mäkelä, A., Nikinmaa, E., Hari, P., Aurela, M., Berbigier, P., Bernhofer, Ch., Grunwald, T.,Loustau, D., Molder, M., Verbeeck, H., Vesala, T. 2009. Contributions of climate, leaf area index and leaf physiology to variation in gross primary production of six coniferous forests across Europe: a model-based analysis. Tree Phys. 29: 621-639. Medlyn, B.E., P. Berbigier, R. Clement, A. Grelle, D. Loustau, S. Linder, L. Wingate, P.G. Jarvis, B.D. Sigurdsson and R.E. McMurtrie. 2005a. Carbon balance of coniferous forests growing in contrasting climates: model-based analysis. Agric. For. Meteorol. 131:97–124. Woodward F.I. and Williams B.G. 1987. Climate and plant distribution at global and local scales. Vegetatio 69:189-197.

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