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Geoscientific Model Development Discussions

Interactive comment on "A user-friendly forest model with a multiplicative mathematical structure: a Bayesian approach to calibration" *by* M. Bagnara et al.

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Response to 'gmd-2014-179 comments', Anonymous Referee #1.

Referee #1: Abstract is the place to clearly concisely show what you have done, why do you think the work is important. What are your results and why are they significant? The abstract is somehow not clear and concise enough to me; may be also confuse the potential readers. For example, the first paragraph basically says: the Bayesian approach is widely used to calibrate forest model, which has already been well accepted (hundreds of published studies). No need to spend entire paragraph to clarify this point. Please consider reconstruct your abstract to be as concise as possible.

Authors: We thank the reviewer for raising this point. We agree the abstract is not concise enough and we will rewrite it in the revised version of the manuscript, including the new results and conclusions we will find as a consequence of the several changes suggested in the review process, and considering the reviewer's comments including but not limiting to the size of the abstract, main novelties of this work and their impact.

Referee #1: The presentation is not complete. For example, the author started the introduction with the definition of GPP and followed by the observation of GPP, GPP modeling and model calibration. Lots of important information are missing, including but not limiting to: (1) Besides Eddy Covariance GPP data, MODIS-GPP is another famous GPP product; (2) Eddy Covariance network only measure NEP, GPP is derived from their model; (3) There are several other ways to model GPP besides LUE model (e.g., Farquhar 1980 type model). (4) Dislike LUE model, in Farquhar model GPP associated parameters have physical meaning, thus they are relatively easy to infer from observations.

Authors: We agree with the points raised by the reviewer, and we will adjust the introduction consequently according to these points. We are also well aware that there are several ways to simulate GPP, and Farquhar (1980) is one of the most commonly found in forest modelling. However it is not free of disadvantages (Yin et al., 2004; Van Oijen et al., 2004): the Farquhar model parameters have no physical meaning at the canopy scale since they are chloroplast parameters with at best some validity up to the leaf level, but not more. Its parameters are also not easy to infer: A-Ci measurements with leaves sampled from all across the canopy are needed, with young leaves having much higher values of Vcmax and Jmax than old ones. We will include a more complete presentation of GPP in the revised introduction, including advantages and disadvantages of the most common methods to estimate it.

Referee #1: In the second part of introduction, the author presented the idea of Bayesian Calibration. It worth to mention that Bayesian calibration is not necessary rely on MCMC method. Bayesian approach relying on adjoint method is also an ef-

fective calibration method (Zhu 2014). Also it worth to mention other important type of ecosystem model calibration method: Kalman filter (Gao 2011). And the author need to justify the reason why they decide to use MCMC methods, given that other two types of calibration methods (adjoint method and kalman filter) could be much more efficient (e.g., adjoint method is a local optimization method, while this study needs a global optimization method? I believe the authors have their own reasons).

Authors: We will add a paragraph in the introduction of the revised manuscript, referring to the two techniques mentioned by the reviewer and justifying our choice to apply a MCMC-based method. These reasons will read as follows: "The data assimilation techniques mentioned above are special cases of Bayesian calibration (Wikle & Berliner 2007), where a prior probability distribution for parameters is specified and updated using Bayes Theorem. However, in contrast to our MCMC approach, the old data assimilation methods – though computationally efficient - require assumptions of linearity and Gaussian distributions that are restrictive and inappropriate in the case of the highly nonlinear models that we study here. Therefore such methods are common in state estimation of computationally demanding models such as GCMs, but they are not common in parameter estimation of ecosystem models. Our MCMC method allows for any type of prior and posterior distribution, including asymmetric and multimodal ones, in contrast to the other methods. Moreover, the sample from the posterior distribution generated by MCMC represents the full posterior probability distribution, in contrast to the adjoint method which only provides an estimate of the mode, and uncertainties can only be assessed fully with such global methods, not local ones."

Referee #1: The purpose of model calibration is to improve the posterior model predictability. This study only presented the calibrations, but miss the posterior model evaluation.

Authors: We carried out a posterior model evaluation for the approaches that resulted in proper convergence. We decided not to include this result in the paper since its main focus is on Bayesian calibration. The model results were insensitive to the algorithm

used or to the procedure applied. We will include the results of model evaluation in the revised version of the manuscript, focusing on the differences (or similarities) between the different calibration procedures.

Referee #1: One common approach is that: the model should be first calibrated at one EC tower site and then apply to another site that has the same plant function type. The cross-site evaluation is necessary to ensure the efficacy of model calibration. I suggest that the author should apply the posterior model parameters derived by different calibration methods to another tower site, in order to fairly compare the goodness of different calibration methods.

Authors: The reviewer is correct in describing the common approach to forest modelling. We did indeed implement this approach in a recently published paper (Bagnara et al. 2014). The latter work focuses on the same model analysed here and tests it on several EC sites. The focus of the present study is on evidencing potential issues in calibrating a simple but highly non-linear model, characterized by a commonly applied mathematical structure. We will refer to Bagnara et al. (2014) in the Discussion of the revised manuscript, focusing on the impact of our results to their findings, but we think a model validation on different EC sites is beyond the scope of this paper.

Referee #1: Broad impact of this study is not well discussed. It is not clear to me how their findings interest our molding community and facilitate future studies in terms of forest model calibration.

Authors: We thank the reviewer for raising this very important point, and we will emphasize the impact of our findings in the revised manuscript. We think our results are important because they focus on issues that have never been discussed before in the field of forest modelling: there are no studies on the difficulties in calibrating this kind of models, which are widely applied to forest research and management. Several well accepted studies and models could be affected by this kind of issues, and we are stressing the need of a more careful approach to calibration to solve potential problems

which have been rarely mentioned before.

Referee #1: Another issue worth discussing is that the parameter calibration could only reduce model parameter uncertainty, however, is not able to constrain model structure uncertainty. There are two LUE models with different model structure used in this study, which might provide insight into the uncertainty in model structure.

Authors: We will apply the procedure described in Van Oijen et al. (2013), based on the ratio of posterior model probabilities, to assess the importance of model structure uncertainty. The results and discussion about this interesting point will be included in the revised manuscript.

Referee #1: Minor comments: Page 6998 Line 2: Remove in very different forest all over the world. Do you mean different forest functional type? Line 4: “easy to use” is not a rigid scientific term. Define it more appropriately. Maybe “pragmatic”? Line 13: what does “user-friendly” mean? Line 19: this sentence needs to be rephrased. Line 22: calibration did not

Authors: As stated in the previous point, the abstract will be entirely rewritten according to the reviewer’s comments and to the changes to the revised manuscript. These comments will be taken into account while doing so, replacing " very different forest all over the world " with "different biomes and PFTs all over the world" and defining "easy to use" and "user-friendly" as in Landsberg & Waring (1997), that is models available to a broader public then models with strictly research and academic purposes, as they are based on a few equations, few parameters, and they do not require high computational power nor lot of data to be run.

Referee #1: Page 6999 Line 2: terrestrial ecosystem carbon balance Line 11. Cite paper here Reichstein 2005

Authors: We will include these last two comments in the revised manuscript.

Referee #1: Page 7000 Line 13: sentence needs to be rephrased.

Authors: The sentence will be rephrased as: "There are several LUE-based models in the existing literature: a few examples are C-Fix (Veroustraete et al., 1994), 3PG (Landsberg and Waring, 1997), Prelued (Mäkelä et al., 2008a), and the model described in Horn and Schulz (2011b)."

Referee #1: Line 18: compared with Line 21 daily time step, based on Page 7001 Line 1: The Bayesian model calibration approach

Authors: We will include these last three comments in the revised manuscript.

Referee #1: Line 15: The efficiency of the MCMC technique highly depends on the model structure. Is it true in general? How about other factors?

Authors: The dependency of the MCMC efficiency on the model structure has been proven, among others, by Gilks & Roberts (1996) and Browne et al. (2009). The effect of model structure on efficiency of Bayesian calibration is the main point of the paper, as we show that commonly used models with a simple but highly nonlinear structure can be hard to calibrate. Other important factors are the uninformative prior distributions (Hartig et al., 2012) and the heavy use of empirical parameters in the model formulation, as discussed in the submitted version of the manuscript. We will discuss on the importance of these factors more in detail and include the previous references in the revised version of the manuscript.

Referee #1: Page 7002 Line 26: Why only use one year data? Lavarone site has multiple-year data (2000-2006). Perhaps, it is a good practice to use part of the data as calibration dataset, and use the rest for model validation.

Authors: We believe that one year of data is more than enough to successfully calibrate a 6-parameters model. Therefore we saw no need to run a longer calibration on several years that would be heavier in terms of computational power required. We did use a different part of the available dataset to evaluate the model performances, and this results will be added to the revised manuscript as stated in the previous comments.

Referee #1: Page 7005 Line 4: Why do you chose 0.3*GPP as a upper bound of GPP data uncertainty? Any reference or reasons?

Authors: Very few examples can be found in the literature of uncertainty estimates of daily GPP. Moreover, these are not consistent across studies: Mo et al. (2008) set daily uncertainties on GPP as 15% of its value, while Duursma et al. (2009) estimated them to be 5% of GPP. We set them to 30% of GPP as done by Williams et al. (2005), as a conservative estimate for calibration purposes, also to be sure that the information content of the data was not overestimated.

Referee #1: Page 7011 Line 16: "multiplicative structure of Prelued was probably the main factor responsible for the difficulty in the calibration." Is it true?

Authors: The reviewer raises a good point. We don't have enough evidence to blame the mathematical structure of Prelued as the only or main responsible for the difficulties in calibration, but we consider also several other factors, like poor a-priori knowledge on the parameter values and their empirical nature. It is crucial that these points are clear to the reader, therefore we will emphasize them in the Discussion section of the revised manuscript

Referee #1: (1) First of all, photosynthesis (GPP) is a very complex biology process, a certain level of model complexity is needed. The difficulty of model calibration might be simply due to the fact that LUE model is too simple (model structure) to capture the GPP response to environmental changes.

Authors: If that were true, the model would have difficulties in reproducing the data, even after calibration, on the same site and period of simulation, which is not the case. Including the model evaluation will show that, even for algorithms that did not reach proper convergence, the model results are pretty good. The model does not have difficulties in reproducing GPP, so its structure does not seem to be inadequate for that purpose.

Referee #1: (2) Multiplicative structure is common in other GPP models (such as abovementioned Farquhar model), there is no evidence that the multiplicative structure hinders model calibrations.

Authors: That relates to the point already mentioned above: the fact that there is no evidence of that does not mean it does not exist, only that it has not been studied so far. Such behavior from a simple model was very surprising as we did not expect such difficulties in calibration, but it gave us the possibility to study an issue that, to our knowledge, has never been studied before in the field of forest modelling. That is the main novelty of this work. Moreover, as the model by H&S had the same problems, this work raises an important question: can we really trust the well-accepted models with similar structure developed so far, or are they all affected by the same calibration issues we described? We will add and highlight this concept in the revised manuscript, as it is one of the main central points of the paper and it should be emphasized to be brought to the reader's attention.

Referee #1: Page 7013 line 14: Any suggestion of future development of LUE model? At least, based on your findings, the LUE model needs a better mathematical structure? Which structure should it be?

Authors: Our recommendation, that will be included in the Conclusions of the revised paper, is that a more complicated structure should be applied to LUE-models. For example, including Prelued as a module in a more structured model could reduce the difficult in calibration (like PRELES, successor of Prelued), also to avoid estimating only one variable very complicated as GPP. It should also be pointed out that this kind of models does not allow to compare model estimates against actual data: GPP is not measured, it is derived from NEP. So NEP should be the model output against which the calibration should be performed, and it should be included in LUE models via combination with a respiration model. Another important point relates to the empirical nature of the parameters: when possible, a large use of parameters with no physical or physiological meaning should be avoided, in order to rely on the physiological basis

of GPP as much as possible.

References: Yin, X., Van Oijen, M., & Schapendonk, A. H. C. M. (2004). Extension of a biochemical model for the generalized stoichiometry of electron transport limited C3 photosynthesis. Plant, Cell & Environment, 27(10), 1211-1222. Van Oijen, M., Dreccer, M. F., Firsching, K. H., & Schnieders, B. J. (2004). Simple equations for dynamic models of the effects of CO2 and O3 on light-use efficiency and growth of crops. Ecological Modelling, 179(1), 39-60. Wikle, C. K., & Berliner, L. M. (2007). A Bayesian tutorial for data assimilation. Physica D: Nonlinear Phenomena, 230(1), 1-16. Bagnara, M., Sottocornola, M., Cescatti, A., Minerbi, S., Montagnani, L., Gianelle, D., & Magnani, F. (2014). Bayesian optimization of a light use efficiency model for the estimation of daily gross primary productivity in a range of Italian forest ecosystems. Ecological Modelling, in press. Van Oijen, M., Reyer, C., Bohn, F. J., Cameron, D. R., Deckmyn, G., Flechsig, M., Härkönene S., Hartig F., Huth A., Kiviste A., Lasch P., Mäkelä A., Mette T., Minunno F. & Rammer, W. (2013). Bayesian calibration, comparison and averaging of six forest models, using data from Scots pine stands across Europe. Forest Ecology and Management, 289, 255-268. Landsberg, J. J., & Waring, R. H. (1997). A generalised model of forest productivity using simplified concepts of radiation-use efficiency, carbon balance and partitioning. Forest Ecology and Management, 95(3), 209-228. Gilks, W. & Roberts, G. (1996). "Strategies for Improving MCMC." In W Gilks, S Richardson, D Spiegelhalter (eds.), Markov Chain Monte Carlo in Practice, p. 89-114. Chapman & Hall, Boca Raton, FL. Browne, W. J., Steele, F., Golalizadeh, M., & Green, M. J. (2009). The use of simple reparameterizations to improve the efficiency of Markov chain Monte Carlo estimation for multilevel models with applications to discrete time survival models. Journal of the Royal Statistical Society. Series A, (Statistics in Society), 172(3), 579–598. doi:10.1111/j.1467-985X.2009.00586.x Hartig, F., Dyke, J., Hickler, T., Higgins, S. I., O'Hara, R. B., Scheiter, S., & Huth, A. (2012). Connecting dynamic vegetation models to data–an inverse perspective. Journal of Biogeography, 39(12), 2240-2252. Mo, X., Chen, J.M., Ju, W., Black, T.A., 2008. Optimization of ecosystem model parameters through assim-

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Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C3164/2015/gmdd-7-C3164-2015-supplement.pdf

Interactive comment on Geosci. Model Dev. Discuss., 7, 6997, 2014.