

# ***Interactive comment on “Constraining ecosystem model with Adaptive Metropolis algorithm using boreal forest site eddy covariance measurements” by J. Mäkelä et al.***

## **Anonymous Referee #1**

Received and published: 13 July 2016

### General comments:

Parameters of the JSBACH land surface model are tuned for two forest sites in Finland. Photosynthesis and evapotranspiration estimates derived from eddy covariance measurements are used to calculate cost functions to be minimized. The optimization is able to correct for the main shortcomings of the model in the description of the annual cycle but is not sufficient to improve the representation of extreme events such as droughts. This shows that basic processes are missing in JSBACH. This kind of result is not new. The authors should do a better job in explaining what is new and original in their optimization approach. From a modelling perspective, a discussion is lacking about the reliability/robustness of JSBACH with respect to other models. From

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a methodology point of view, several issues need to be clarified. Spin-up must be performed for any new set of parameter values and it is not clear whether the authors made this effort or not. The purpose of the parameter classification (class I, II, and III) is not clear. The classification itself is not properly described, nor justified. Although the paper is reasonably well written, part of the method description is found in the Result section and should be moved to the Methods section. The Abstract need to be improved.

Recommendation: major revisions.

Particular comments:

P. 1, Abstract: A summary of the main findings regarding the usefulness of the optimization technique used in this study is lacking. Key results and conclusions must be listed.

P. 3, L. 31: Why not including the spin-up into the calibration ? Please clarify.

P. 4, L. 7: Some Class II and Class III parameters can also be "site-specific". For example, soil water retention parameters are highly site-specific. Please clarify what you mean by "site specific".

P. 5, L. 11-12: This argument is not valid as some Class II and Class III parameters listed in Table 1 can be site-specific. Do you mean that Class I parameters are observed and do not need any analysis ?

P. 6, L. 22: Is using a single spin-up valid ?

P. 7, L. 8: Does this mean that class I parameters other than maximum LAI are not considered as site-specific ?

P. 9, L. 4: This paragraph is difficult to understand because the methods were not sufficiently described and symbols were not defined before. Methods, as well as "L1", and all the other symbols of Table 5 (including "HC", "HV", "SV") should be de-

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fined/presented in Sect. 2. Not here in the result Section.

P. 9, L. 6 ("half as large"): Half as large as what ?

P. 9, L. 14 ("ET is a more turbulent flux than GPP"): What do you mean ? GPP is not a turbulent flux at all. The turbulent CO2 flux is NEE, not GPP. GPP is not directly measured by eddy covariance techniques.

P.9, L. 27: The JSBACH model simulations don't look very good. How does JSBACH perform with respect to other models at these two sites ? Please give basic scores in terms of half-hourly fluxes, such as RMSD, ubRMSD and mean bias.

P. 11, L. 1: How can this be explained ? Shortcomings in the representation of the soil moisture stress ? How could these shortcomings be attenuated ? Using another photosynthesis model ?

Editorial comments:

P. 18 (Table 1): Parameters' units are lacking.

P. 19 (Table 4): Parameters' units are lacking.

P. 20 (Table 5, "highlighted values"): I don't see any highlighted value.

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