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Discussions

Interactive comment on "Sensitivity and predictive uncertainty of the ACASA model at a spruce forest site" *by* K. Staudt et al.

Anonymous Referee #1

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General Comments:

This paper presents a sensitivity and uncertainty estimation study of the ACASA model using the Generalized Uncertainty Estimation (GLUE) methods while using 2 5-day periods of half hourly micromet. data. I think the paper is generally well written and structured however before acceptence and publication I have a number of issues and concerns that need to be considered and/or clarified.

1. While limiting their analysis to the vegetation/atmosphere interface, why are the authors only using such a small range of data. From my understanding of FLUXNET sites the required information should be available at much longer time periods.

2. The ACASA model –according to table 2 – has a number of 24 parameters that

need to be specified or that have to be estimated from the lE, H and CO2 flux time series. While I am in general very much in favour of the GLUE method, I have some concerns here. Within GLUE authors run a number of 20,000 MC runs – given the set of 24 parameters this means roughly a sampling density of 1.5 per individual parameter. Authors need to argue for a sufficient sampling density with regard to a "stable" estimation of sensitivities and uncertainty bounds.

3.Also, concerning the GLUE method, authors should argue why they apply the widely use efficiency critieria. There has been a long and intensive debate in recent years within the field of hydrology especially on that issue (see Mantovan, P., Todini, E., 2006. Hydrological forecasting uncertainty assessment: incoherence of the GLUE methodology. J. Hydrol. 330 (1–2), 368–381; Beven, K.J., Smith, P.J., Freer, J., 2007. Comment on hydrological forecasting uncertainty assessment: incoherence of the GLUE methodology by Pietro Mantovan and Ezio Todini. J. Hydrol. 338 (3), 315–318; and following up papers). This discussion and possible consequences should also be included in the discussion of the methodology.

4. Why are uncertainties of individual measurements (see Hollinger et al.,) not included into the analysis. Also, the non-closure of the energy balance, a hot topic and to my knowledge of particular interest to the Foken-group, are not included either. Why?

5. Therefore, this paper "only" presents a straight foreward application of the GLUE method. What are the consequences of the equifinality issue and the uncertainty. Not understanding me wrongly, I am very much in favour of quantifying uncertainties, but I see much more topics within this paper that should be addressed.

6. There is one interesting point that is hardly taken on by the authors. The model seems to have strong difficulties in handling both, NEE and energy fluxes. What is the reason for this? How can the model be improved. Also, why is the optimal parameterization different for different time periods? It seems here that some of the process descriptions are not able to handle different climatic conditions. What are these? To my

understanding these are some of the interesting questions coming out of this analysis.

In summary, after thoroughly considering and addressing these issues (1-6), this paper might be suitable for publication in BG.

Specific/Technical issues

p. 4237, l. 22: from Fig. 3 I do not see the valuesrunning from 0-1.

p. 4238, l. 3: please specify other fluxes.

p. 4238, l. 9: this would be an argument to include G as an uncertain component within GLUE

p. 4238, l. 15: this correlation should be no surprise: Rn-G=H+lE.

Paragraph 3.2 is of some length and after some paragraphs pretty “bouring” to read – perhaps this could condensed in a more exciting way.

p. 4242, l.26: what do you understand by “reasonably well”

p. 4246, l.13: why is 20000 be expected to be enough (see above)?

p. 4247, l.3: a short sentence how Mitchell did it wouldbe nice!

p. 4248, l.10: “reasonably well” ???

Interactive comment on Biogeosciences Discuss., 7, 4223, 2010.