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

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We would like to thank the anonymous referee #1 for the time and effort taken to review our manuscript. We will reply to the comments one by one, quoting the comments for convenience.

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.

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Interactive Discussion

Discussion Paper

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."

The limiting factor to the range of data is the number of model runs for the GLUE methodology. The number should be very large and therefore the number of days must be reduced to have an acceptable CPU time (calculations were done on a single Linux computational node at the University of California, Davis). Furthermore, during both EGER IOPs more data (e.g. from SODAR/RASS and sap flux measurements) for comparison were available than during other time periods, when only the usual instrumentation of the FLUXNET station DE-Bay was available. A comparison of ACASA model simulations and data from the FLUXNET station was made for the whole year of 2003 within a Diploma thesis (Schäfer, 2010), which showed a good agreement in all seasons except during the extreme heat of August. A publication of these results is in preparation.

"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."

We are aware of 20000 model runs being a very low number of parameter sets given the number of 24 input parameters (p.4246, lines 8-15). 'However, as with Prihodko et al. (2008), who had an even larger number of parameters, we expect that an important range of the parameter space is already covered by 20000 model runs.' Thus, the significance of this study is limited in that it is conditional not only on the meteorological conditions covered by the input data but also on the sets of parameters. On the one hand we wanted to assess the general ability of the ACASA model to reproduce measured fluxes for our site. Thereby, indications for weaknesses in the model structure were revealed. On the other hand, the observed parameter sensitivities could be used as basis for a refined parameter estimation study by fixing parameters that appeared to be not influential and by studying the sensitivity of the influential parameters in more detail. The revised version will emphasize these limitations in the last paragraph of Chapter 4.1 of the discussion.

"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."

We will include the debate about the use of informal likelihood measures such as the Nash and Sutcliffe coefficient of efficiency within the GLUE methodology by Mantovan and Todini (2006), Beven et al. (2007), Mantovan et al. (2007) and Beven et al. (2008) in our description of the methodology (Chap. 2.4.2). As discussed by Beven et al. (2008), the choice of an informal likelihood such as the coefficient of efficiency does not require the definition of an explicit statistical error model resulting from e.g. input errors and model structural errors. In real applications the structure of these errors is mostly unknown, as shown there for hydrological applications but as also holds true for SVAT modeling. The use of an informal likelihood prevents the user from making any wrong assumptions about the modeling errors resulting in well-defined but incorrect parameter distributions. However, the parameter distributions derived with the informal likelihood was shown to be flatter, but included the right parameter value.

"4. Why are uncertainties of individual measurements (see Hollinger et al.,) not in-

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cluded 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?"

The uncertainty analysis by Hollinger and Richardson has some disadvantages. Either a second measuring instrument is necessary (Hollinger and Richardson, 2005) or a time separated data set (Richardson et al., 2006). Instead of this we used the widely applied data quality analysis by Foken and Wichura (1996) in the version of Foken et al. (2004). This method gives the opportunity to determine for each time series (independent from another instrument or the conditions on another day) the data quality or in combination with the instrument type (Foken and Oncley, 1995) a quantitative accuracy (Mauder et al., 2006). We will combine our separated formulations on pages 4228 and 4243 to make this clearer. Thus, the accuracy of the eddy-covariance data measured with our sonic anemometer (USA 1 Metek GmbH, type B) after applying the quality scheme after Foken et al. (2004) and only considering flux data with a quality flag of 6 and better is 10% to 15% for the sensible heat flux and 15% to 20% for the latent heat flux and the NEE, depending on the quality flag (Mauder et al., 2006). This accuracy was added to Figure 7 and Figure 8 (see response to reviewer 2 for the figures and response to comment 5 for an explanation of these uncertainties).

The energy balance closure problem certainly adds non-random uncertainties to the measurements. The missing energy was always found to be about 20% of the available energy without larger variations in time, e.g. 23% (1997 1999, Aubinet et al., 2000; Foken, 2008), 19% (IOP-1) and 21% (IOP-2). The problem of the unclosed energy balance is still an open question. Probably, large-scale processes in heterogeneous landscapes and the corresponding large scale eddies that are missed by the eddy-covariance technique cause the residuum (Foken, 2008). One suggested method to close the energy balance according to the Bowen ratio (Twine et al., 2000) is only a first approximation (Foken, 2008), as this method assumes a similar Bowen ratio for small- and large-scale eddies, which could not be confirmed by measurements (Ruppert et al., 2006). Therefore, we decided not to close the energy balance in eddy-covariance measurements. By doing so we hope to avoid adding more uncertainties to the measured fluxes due to the selected closure method but also from uncertainties of the soil heat flux measurements and estimates of storage heat fluxes. Additional problems might arise from weaknesses of the ‘Bowen-ratio closure’ for negative Bowen ratios. We have made no further investigations on this topic in our paper because the energy balance closure problem is more complex than the scope of the paper, and deserves more detailed studies: In this respect, a special experiment is in preparation for 2011 to investigate the influence of secondary circulations on the energy balance closure problems (Inagaki et al., 2006; Kanda et al., 2004). We will add a paragraph highlighting this issue.

"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."

We will include the uncertainties of the measurements in the figures showing the uncertainty bounds of the models (Fig. 7 and Fig. 8, see response to reviewer 2 for updated figures). Thus, the comparison of uncertainty bounds of the modeled fluxes to the uncertainty of the measurements allows for a meaningful assessment of parameter induced model uncertainty. Here, the uncertainty bounds of the ACASA model results are of a similar width as the range of uncertainties of the eddy-covariance measurements for the sensible heat flux during both periods and the latent heat flux during the fall period. Furthermore, for these fluxes, uncertainty bounds of the measurements and the models overlap largely. Only for the latent heat flux during the summer period were uncertainty bounds of the model much larger than uncertainties of the eddy-covariance measurements, resulting from the generally lower coefficients of efficiency for the latent heat flux for the summer period than for the fall period. Thus, the ACASA model seems to have problems in simulating the latent heat flux for warm periods. This problem together with possible reasons is further discussed in the response to the following

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comment (comment 6). There, also the difficulties of ACASA in handling both, NEE and energy fluxes are discussed in more detail. These problems are an important result of this GLUE analysis which should be considered in future model developments.

For many parameters, the equifinality problem was observed, meaning that very good as well as very poor results were possible across the whole range of parameter values. Thus, identification of an optimal parameter value for a single parameter will always depend on the values of all other parameters (Schulz and Beven, 2003). Furthermore, parameter equifinality could indicate that the model is over-parameterized, as no robust parameter estimation is possible with the employed data set (Franks et al. 1999). Consequently, one needs to either include longer data sets for calibration that also comprise different meteorological conditions or seasons or fix as many parameters as possible to values determined from independent measurements (Schulz et al., 2001; Schulz and Beven, 2003). It has been argued by Franks et al. (1999) that the complexity of SVAT models should be reduced to a level that copes with the available calibration data and thus reduces the problem of parameter equifinality. We will include these issues about parameter equifinality in our revised discussion.

"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."

It certainly is a result of this study that the model has difficulties in achieving good results for NEE and energy fluxes concurrently. Optimal parameterization was different for the different fluxes mainly for plant physiological parameters (cm, iqe, jmax25). Thus, weaknesses of the model will most probably be located in the plant physiological sub models and their coupling to other parts of the ACASA model. The exchange of carbon dioxide and water of the leaves is mainly coupled by stomatal conductance calculated with the Ball-Berry equation. Sensitivity graphs for the slope of the Ball-Berry formula cm suggested optimal parameter values from the lower part of the parameter range for the latent and sensible heat fluxes, whereas more parameter values of the behavioural parameter sets for NEE came from the upper half of the parameter range (Fig. 6e and 6f). These discrepancies highlight that the problem of the model not being able to get good results for the NEE and the energy fluxes concurrently could stem from an insufficient representation of stomatal conductance that couples these fluxes. Furthermore, the strong but different sensitivity of the modeled fluxes to the leaf area index, particularly in relation to NEE sensitivity, suggests the need to review leaf area index within different parts of the model (i.e. radiation regimes, soil respiration). These results will be considered in future model developments by the authors of the model.

Different optimal parameterization for different time periods was especially evident for the slope of the Ball-Berry formula cm for the calculation of stomatal conductance. In our study, the latent heat flux during dry and warm conditions was not as well represented as during the cold and wet fall conditions (in terms of the values of the coefficients of efficiency and as reflected by the very large uncertainty bounds for the summer period, Fig. 8, see also response to comment 5). This finding was confirmed by a study applying the ACASA model for the full annual cycle for the year 2003 by Schäfer (2010). Thus, one problem of the model seems to be the representation of the latent heat flux for warm and dry periods. The different optimal parameter ranges for cm for the latent heat flux for the two periods suggests the need to reduce stomatal conductance for drier conditions. As discussed in Chap. 4.1, Tenhunen et al. (1990) and Baldocchi (1997) suggested a reduction of cm values with decreasing water availability. From our study, a similar suggestion could be drawn, but this problem could also be addressed by including a mechanism to handle dry conditions. Further work should study this problem more thoroughly.

We will include further discussions of these problems and highlight improvements needed to be made in future model versions.

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Specific/Technical issues:

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

This was probably unclearly expressed. What we wanted to say is that the coefficients of efficiency for all fluxes but the NEE during IOP-2 were positive. We will clarify this in the updated manuscript.

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

'Other fluxes' are the sensible and latent heat fluxes and the NEE. We will include this in the updated manuscript.

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

We did not include a more extensive discussion of the ground heat flux into this paper for two reasons (as will be stated in the revised manuscript): For such an investigation, a different experimental setup with a high resolution of radiation measurements in the trunk space and soil measurements would be needed in the investigated forest. Furthermore, the ground heat flux is only about 5% of net radiation and much smaller than all other turbulent fluxes. Thus, the ground heat flux was excluded from further analysis.

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

This certainly is no surprise. We will include this statement in the updated manuscript.

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

We will condense paragraph 3.2.

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

Here, by judging the agreement of calculated uncertainty bounds and measured val-

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ues as ’reasonably well’, we wanted to express that there are some periods when the uncertainty bounds captured measured values very well, but that there are also some periods when less agreement between model uncertainty bounds and measurements was observed. We will change this statement to: ’In general, the calculated uncertainty bounds capture the measured values for all three fluxes during most of the time.’ This statement is based on the details about agreement/disagreement of model results and measurements following this general statement at p. 4242, line 27 to p. 4243, line 11.

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

Response see above (comment 2).

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

They extended their study based on the results of the GLUE analysis on annual NEE to further explore the reasons for model failure, for example by analyzing the feedbacks of problems within the simulation of soil hydrology and total ecosystem respiration on annual NEE. We will include this statement in the updated manuscript.

"p. 4248, l.10: “reasonably well” ???"

To clarify this expression we will write in the revised manuscript: ’The ACASA model was capable of reproducing all fluxes during most of the time as reflected by the uncertainty bounds. . .’. See also response above.

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