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Biogeosciences Discussions

Interactive comment on "Influences of observation errors in eddy flux data on inverse model parameter estimation" *by* G. Lasslop et al.

G. Lasslop et al.

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We would like to thank the anonymous reviewer for the careful reading of the manuscript and the critical comments. In the following we discuss parts of his comments, whenever the referee is cited, the text has been written inside quotation marks.

The reviewer highlights the importance of the first part of the study, specification of the error statistics, but believes that the second part, which addresses how these errors influence parameter estimates in typical ecosystem-level models, is of weak significance, as "model errors dominate the observation error budget for parameter estimation in this field, so that the study does not address the appropriate issue". Supported by the other reviewer and by personal communication from colleagues after

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having read the BGD paper we disagree with this general statement. There has been a recent publication highlighting the need of choosing the correct error model (Trudinger et al., 2007). We furthermore disagree that model errors generally dominate in our field, this depends on the model and on the time scale of interest. For the hyperbolic light response curve used for small timescales for example this does not hold true. Of course the general impact of error characteristics on parameter estimates is well known, but for each data and model type the quantitative effect is not a priori clear and needs to the characterized. This is important information for scientists working with this kind of data. In the past there has been considerable discussion about the appropriate cost function when using eddy data (for instance Richardson et al., 2005), whether the absolute deviations or squared differences should be minimized when using eddy covariance data for parameter estimation. Besides this it was not clear how to describe the varying standard deviation, Richardson et al. (2005) also studied the effect of different weighting schemes on respiration which was higher than the 95% confidence interval. With the second part of the study we used synthetic data for parameter retrieval experiments, to not only be able to quantify the differences between methods but also the distance to the true parameter and fluxes respectively. We compared two weighting schemes and could identify the method that improved the parameter retrieval most.

Furthermore the reviewer criticizes that "the wording is awkward from a statistical point of view". Largely, this criticism reflects obviously the fact that different scientific communities use different terminologies. Our impression is that the reviewer is from the numerical weather prediction/data assimilation community, while our background is biogeochemistry, micrometeorology and (inverse) modeling therein. We used the expressions common in the eddy flux community as this is the target group of the paper.

We thank the reviewer for the technical comments and will modify the text according to them. The specific comments we discuss in the following.

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Abstract, l.23: "show" changed to "illustrate"
p. 753, l.15, l.17, l.19; p. 760, l.19: "model parameters may be inverted, but not models".
Model inversion is a commonly used term in the biogeosciences community, it can be used for the rearrangement of the equations of the model that yields the adjoint model or in a broader sense for using observations of the target variable to derive model parameters.
p. 753, l.16: "why is there no background term (or at least bound information) in the cost function?"
The use of a background term is common in the field of numerical weather prediction, where short term predictions or first guess model runs are used as a background field. The background term is then the difference of the model output to the background field. As we can estimate the parameters for our models from the data, with maybe including prior information on the parameters adding an additional constraint is not necessary. In this study we tried to explore how eddy data can be used most efficiently, thus including additional information in the cost function might have made the interpretation of results more difficult.
p. 753, l.22-23: "the classification has little significance from a statistical point of view because it mixes unconditional and conditional probabilities"
The reviewer refers here to the classification into random, selective systematic and systematic errors. This classification was introduced by Moncrieff et al 1996 and is commonly used for the description of errors occurring in eddy covariance data (Valentini, 2003, Foster and Aber, 2004).
p. 754, l.5: "the statement is general and does not need a specific reference"
Reference deleted.
p. 758: l.10: "why is there no background term (or at least bound information) in the cost function?"
See above.
p. 758: l.15: "What happens if some parameters have values beyond plausible bounds

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at the minimum (e.g. unexpected negative values)"
In summer the parameters estimated usually are reasonable, how to deal with unreasonable parameters depends on the application, and is independent of this work, for instance one could interpolate or include prior information about the parameters in the cost function. Here all parameters had reasonable values, since the problem is well-posed.
p. 760, l.19 until Eq. (7): "I do not understand"
Here we scale the model residuals of NEE and LE to ensure that the NEE misfit contributes with the same magnitude to the cost function as the LE misfit. We reworded this sentence and hope it is better understandable now.
p. 761, l.6: "in that case the study is also of minor importance"
The reviewer refers here to our statement that the model performance is of minor importance. As the study deals with the characterization of data errors and their influence on model parameterization and not with model errors, we used the model output as reference to be independent of potential model errors. Hence, by design of the study the model errors are not important. The reviewer may have misunderstood this point. We reworded the sentence to be more clear.
p. 761, l.23: "this is not consistent with the chosen statistical framework. The prior parameters should follow the prior error statistics."
For the parameters of the model used here the prior error statistics are not known. We concur that setting the parameter uncertainty to 20 % of the parameter value is not consistent with the Bayesian framework, but the problem is that the prior error statistics of parameter are not well-known, i.e. reasonable assumptions approximations have to be made. However, for inverting the model against eddy covariance data additional constraint is needed to yield reasonable parameter estimates for those parameters where the data does not contain constraining information. In this sense it relates to the background value/bounding question mentioned above. The set up we used here has been used in previous studies (Knorr and Kattge, 2005). In any case our conclusions are not affected by this, since again we only look at data errors.

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p. 762, l.20: "This makes little sense. One may assume that some variable (like the NEE error) statistically behaves like a random variable. However, the more it is linked to other variables (like wind speed here), the less random it appears to be."
This seems to be a misunderstanding; we did not say the error itself was linked to the wind speed but the variance of the random error. We reworded the sentence to emphasize this.
p. 766, l.2: "one should not expect the opposite to happen"
The reviewer refers here to our statement that the random error introduces no systematic error to the parameter estimates. Include "as expected"; this was to emphasize the qualitative different behavior of the random and systematic error, respectively.
p. 766, l.10-14: "I do not understand"
We compared two methods of describing the error standard deviation (std and res), one improves the reproduction of the original data (std), the other (res) does not. Res is only based on the flux magnitude, but we mentioned before, that flux magnitude cannot reproduce the whole variability of the error standard deviation.
p. 767, l.1: "this naive statement could be avoided"
The reviewer refers to the statement that the random error complicates the parameter retrieval. We reworded the sentence and deleted this statement.
p. 768, l.14: We changed "indicate" to "illustrate"
p. 768, l.20: "some methods already exist"
The reviewer comments on our statement, that methods need to be developed to account for systematic errors when estimating the uncertainty. We are not aware of such methods for eddy flux data and hope the reviewer may give us more information about it.
p. 769, l.7: "this is no news"
We added "of the eddy flux data". Previous studies emphasize the double exponential distribution of the eddy data (Richardson et al., 2005, Hollinger and Richardson, 2005), thus we don't understand why this can be considered to be "no news" and we hope the misunderstanding is, that we are not doing a general statement but are writing

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about eddy flux data here as in the whole paper.

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Interactive Comment

Interactive comment on Biogeosciences Discuss., 5, 751, 2008.

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