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Discussions

Interactive comment on "Development of an ensemble-adjoint optimization approach to derive uncertainties in net carbon fluxes" *by* T. Ziehn et al.

Anonymous Referee #2

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The paper by Ziehn et al. provides estimates of terrestrial ecosystem model parameters and the net carbon flux between the atmosphere and terrestrial biosphere across two decades. They nicely describe the implementation of an ensemble-adjoint scheme, which assimilates measurements to estimate a certain set of parameters in the model and uses monte-carlo methods to represent the uncertainty in the parameters that are not inverted. Thus the final posterior distributions completely represent the uncertainty in the model. The carbon flux estimates that result are consistent with previously published estimates in the mean, but have significantly lower uncertainty.

The application of the joint ensemble-adjoint scheme should be an interesting development in the earth system modeling community. It is an example of a rigorous accounting of the uncertainty in ecosystem model parameters and the resulting uncertainty in carbon system diagnostics, such as the air-land carbon flux. As the authors note, inversion schemes can be troubled by parameters which are not resolved and they have addressed this in a clever way in this case.

Specific Comments

() pg 1514, ln 15 : Using the word likely in the phrase "it is more likely that we find the global minimum in the reduced parameter space" might be confusing for some readers, as likelihood has a specific definition in Bayesian statistics separate from their intended meaning. If the authors are making a specific statement about the probability of finding the global minimum in the reduced space, then we should see some quantity, otherwise they might consider terms such as "giving more confidence that we find a global minimum". This occurs several times in the manuscript and might warrant some attention.

() pg 1515, ln 25 : Characterizing the 4-D VAR methods as the 'most advanced' parameter estimation tool is not meaningful. It would be better to describe why it is a suitable method for this estimation problem. This is already set-up in terms of the computational cost of the TEM making pure monte-carlo methods not feasible several lines above.

() Section 2.0 : Provides a brief and well referenced description of the model and assimilation that deserves a positive comment.

() pg 1520, ln 15 : The acronym PFT is should be defined here. Plant Functional Type?

() pg 1521, eq 7 : The equation and surrounding description could be more clear. Does the superimposition of the posterior PDFs assume all are normal? Is it only done with the statistics? How exactly is it done? The last sentence in the paragraph is redundant and can probably be removed.

() pg 1521, ln 20 : The exclusion criteria for the 28 rejected runs might be better explained. What determines the physicality of a parameters estimation or a "small enough" value of the cost function minimum?

() Table 1 : What quantiles are being shown for the uncertainty range? The table is not easy to read because of the asymmetry of the distributions. It might be appropriate to plot the distributions instead of showing the table or include a column with some indication of distributional width.

() pg 1522, ln 13 : The degree to which the parameters are constrained by the data would be more clear if the authors included some metric like Shannon Information Content (Shannon and Weaver 1949) or the relative entropy of the prior and posterior distributions in the base case and the ensemble.

() pg 1522, ln 21 : The transition between two sentences here should be better. i.e. "The superimposed PDF is not necessarily Gaussian. However, the skewness and kurtosis for the 1990's indicate that a normal approximation might be valid." I would be interested in the author's interpretation of the difference in shape and relative uncertainty between the 1980s and 1990s distributions.

() pg 1523, ln 6 : The comparison to the results of Denman et al. (2007) is warranted but I am left wanting more interpretation or explanation. This method results in an uncertainty interval an order of magnitude smaller than Denman's. Does the presented estimate only reflect the uncertainty which stems from the TEM and not from the other fluxes (land use, oceans and emissions)?

() pg 1523, ln 13 : How much of a contribution to the small uncertainty estimate do the negative off-diagonal elements in C_d account for in this analysis? Additionally, are those entries an expression of some process, be it physical or biological, or are they simply a result of the atmospheric carbon budget?

() pg 1523, ln 16 : Would it be possible to include timeseries of the global mean NEP in another figure? This is the target variable and it should be shown as a result. Includ-

ing error bars and a prior estimate would do a lot to emphasize the strengths of this analysis.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1513, 2011.