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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.

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Reply to Anonymous Referee #2

We would like to thank the referee for the review of our manuscript and the constructive comments. Below, each comment (in quotation marks) is followed by an individual response.

"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."

We agree with the reviewers comments and we will rephrase the above mentioned statement in the revised manuscript accordingly.

"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."

We agree and we will remove this statement in the revised manuscript.

"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?"

This is correct. We will provide the definition for PFT (plant functional type) in the revised manuscript.

"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."

The posterior parameter PDFs and the PDFs for the target output quantity are all normal. The superposition of the PDFs is not done by using only the statistics of the individiual PDFs (this would result into a final PDF which is also normal). We discretize the PDFs using a step length of 1x10-4 PgC and then calculate the sum of all discrete points divided by the total number of PDFs for each step in order to obtain the final PDF. In this way the final PDF can also be non-Gaussian.

We will revise the whole paragraph and explain the superposition of the posterior parameter PDFs and the PDFs of the target output quantity in more detail as outlined above.

“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?”

We require for all parameters that they are positive in order to be physically meaningful. Some parameters are even more constrained, for example the fraction fs (decomposition flux going from the fast to the long-lived soil carbon pool) has to be between 0 and 1. However, the optimal set of parameters may contain values outside those ranges and we therefore have to exclude the corresponding runs. We also exclude runs where the gradient in the cost function minimum is greater than 10-3 to ensure that we really are in a minimum.

We will include this explanation in the revised manuscript.

“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.”

The table shows the upper and lower percentiles equivalent to one standard deviation. This is mentioned in the caption for Table 1. This means that mu-sigma is equivalent to the 15.9th percentile (0.159 quantile) and mu+sigma is equivalent to the 84.1th percentile (0.841 quantile).

We will clarify this in the caption for Table 1 in the revised version.

We will also explain in the revised manuscript that we distinguish between model parameters (physical domain) and parameters as used by CCDAS (normalized domain). For most of the parameters we use a log-normal distribution in the physical domain,

which results in the asymmetry as shown in Table 1. However, we use a transformation so that parameters follow a normal distribution in the normalized domain. Results are only discussed here in the physical domain.

“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.”

We will provide the relative reduction in uncertainty for all parameters in Table 1. The relative reduction in uncertainty indicates how well parameters are constrained by the atmospheric CO2 data.

“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.”

We will reword the two sentences in the revised version as suggested. The reason why the final PDF for global NEP for the 1980s deviates more from a Gaussian than the final PDF for global NEP for the 1990s is that we have an “outlier” PDF amongst the ensemble runs for the 1980s. Here, the optimal parameter values for Q_10,f (temperature sensitivity of the fast carbon pool respiration) and tau_f (turnover time for the fast carbon pool) are much higher than the base case with Q_10,f=5.01 and tau_f=8.79, which results in a much higher net carbon flux for the 1980s.

“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)?”

The presented estimates for global NEP only take into account the parameter uncertainties in the TEM. These are the prior uncertainties of the NPP-related parameters (parameters related to photosynthesis and autotrophic respiration) which are include via the ensemble runs and the posterior uncertainties for the soil carbon parameters (parameters controlling the heterotrophic respiration) which are obtained from CCDAS. Uncertainties in the background fluxes (i.e. land use and ocean) are not considered here.

"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?"

The negative off-diagonal elements (individual years) in the covariance matrix C_d have a large influence on the overall uncertainty over the 10yr period. As stated in the manuscript, the uncertainty for global mean NEP for a single year (for example the year 1990) is by at least a factor of two larger than global mean NEP for the decade of the 1990s.

The negative off-diagonal elements are a result of the atmospheric carbon budget (atmospheric constrained). This is also discussed in Scholze et al. (2007)

Scholze M, Kaminski T, Rayner P, Knorr W, Giering R (2007) Propagating uncertainty through prognostic carbon cycle data assimilation system simulations, Journal of Geophysical Research, 112, D17305, doi:10.1029/2007JD008642.

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

We will include a time series plot of global mean NEP from 1980 until 1999 including

error bars in the revised version of this manuscript. The prior estimate for global mean NEP is zero due to the fact that we assume a balance between NPP and soil respiration as a first estimate. This is discussed in Rayner et al. (2005).

Figure 1 (NEP time series) caption: Time series of the global mean net ecosystem productivity (NEP). Mean values and error bars are based on the final NEP PDF (third stage of the ensemble-adjoint optimization approach) for each year. Error bars represent one standard deviation.

Rayner PJ, Scholze M, Knorr W, Kaminski T, Giering R, Widmann H (2005) Two decades of terrestrial carbon fluxes from a carbon cycle data assimilation system (CCDAS). it Global Biogeochemical Cycles, bf 19, GB2026, doi:10.1029/2004GB002254.

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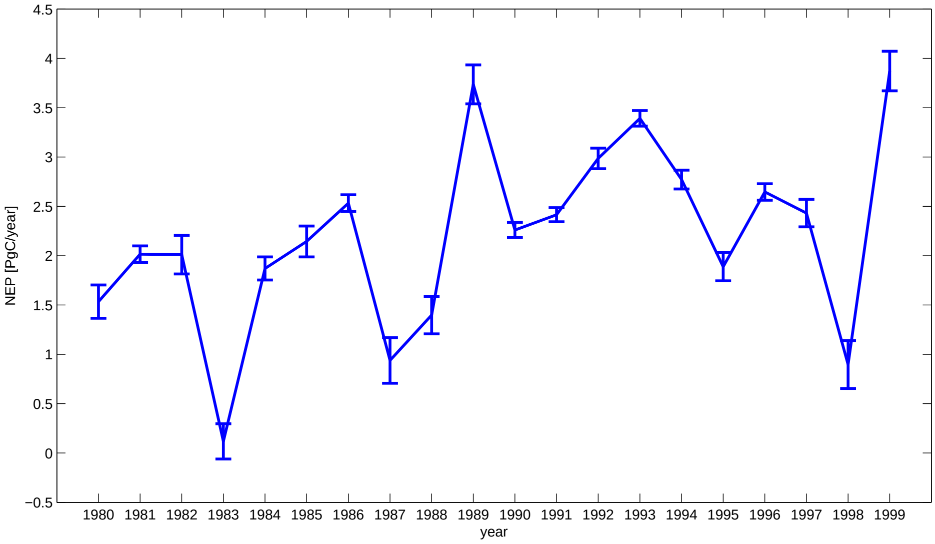

Fig. 1. Time series of the global mean net ecosystem productivity (NEP) including error bars.