

## ***Interactive comment on “A regional high-resolution carbon flux inversion of North America for 2004” by A. E. Schuh et al.***

**Anonymous Referee #2**

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This paper presents results from an inverse modeling study aimed at quantifying the North American carbon balance for 2004. Several aspects of the paper are very appealing, including the use of multiple sets of boundary conditions, fossil fuel inventories, and sets of a priori covariance parameters. The authors also have a very thorough section interpreting the results of their inversion. There are, however, some fundamental aspects of both the approach and the results that are problematic, and in some cases not discussed in sufficient detail. As a result, it is difficult to objectively interpret the quality of the presented results, and therefore it is difficult to gauge the scope of the revisions that will be required.

### GENERAL COMMENTS

First, the timescale on which the bias parameters are allowed to vary is unclear. Based  
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on p. 10206, the bias parameters change on a weekly basis. If the parameters are indeed allowed to change during each 7 day period, is there any temporal correlation assumed between weeks? If so, it is not described. If not, that would not be consistent with the authors' statement regarding the fact that these parameters are assumed to vary slowly in time. I would also suspect that this yields quite noisy bias parameters unless some reasonable temporal regularization is included, especially in the early parts of the year.

Contrary to the paragraph above, in other portions of the paper the authors seem to use the estimates of bias parameters from a previous week as a “prior” for bias parameters for the subsequent week. This leads to my second major methodological concern. The authors correctly state that if a posteriori estimates from one week are used as a priori estimates for the following week, then two problems arise. First, by definition, through the sequential Bayesian updating procedure, the uncertainties appear to decrease as time evolves. This would be reasonable if in fact the bias parameters were considered constant throughout time, but leads to unreasonably low uncertainty estimates otherwise. Second, the estimates of the bias parameters converge to constant values, because the incremental impact of new observations on the best estimates is low. The authors claim to solve both problem in equation 7, by adding a “grand” prior term to pull the bias estimates back their original priors (with high uncertainties). However, in an objective function such as the one in equation 7, the principle of Bayesian updating is still such that even if one adds a second set of “priors” with high uncertainty  $\sigma^2_{\text{grand}}$ , the a posteriori uncertainties will still by definition be lower than that in each of the individual terms in the objective function, and therefore be lower than  $\sigma^2_0$ , which is the uncertainty that the authors had described as getting unreasonably low. In fact, by adding a third term (i.e. a third piece of information) to the objective function, the new a posteriori uncertainties will now become even lower. The values of the best estimates themselves will be pulled slightly back to the a priori values (only slightly because these “grand” priors are given high uncertainties), but there is no guarantee that these priors are any better than the a posteriori estimates from the pre-

vious step. This approach does not seem to make sense from a statistical perspective, or from a scientific one.

Perhaps even more critically, it appears (lines 10-13 p. 10206) that only 7 days of data are used to constrain each set of bias parameters. This is an unreasonably short window, and much shorter than any time lag that has been shown to be appropriate in the application of Kalman filtering approaches in the past (e.g. Peters et al. 2005 JGR, Bruhwiler et al. 2005 ACP, Peters et al. 2007 PNAS etc.). Also, in a classical Kalman filter, the state space remains unchanged, and one only steps through the observations. In this way, the same state space is sequentially updated, while stepping through the observations. In fixed-lag filters such as the ones references above, each element of the state space is instead updated a fixed number of times, until the newest observations are no longer informative of past variability. If the authors are instead solving for a new weekly set of biases with each set of observations, then they are essentially solving a series of “batch” inversions, with the very unreasonable assumption that each week of observations only contains information about that same week’s bias parameters. This is not a Kalman filter, and also is not justified from a scientific perspective. If I have misinterpreted the numerical implementation of the approach, then this needs to be clarified in the manuscript.

Furthermore, I find it somewhat frustrating that the parameters that are actually being estimated through the inversion are the bias parameters, and yet the authors never present these results. Instead, they jump directly to time series and maps of GPP, ER, and NEE. Although I understand that these are the quantities that we are ultimately interested in, one is left with the suspicion that perhaps the estimated biases were not reasonable, and that this is why they were not presented. These are, however, critical in order to evaluate the effectiveness of the approach. Do these bias parameters vary in a reasonable way both spatially and temporally? How does the apparent uncertainty of these parameters evolve over the course of the year? Are their absolute values reasonable within the context of what the authors understand about the underlying

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biospheric model? etc.

Related to this point, all the figures that involve maps have aspect ratios, color schemes, and other plotting choices that hinder the interpretation of the presented results. For example, whereas figures 1 and 2 are presented at the native resolution of the datasets used in the analysis with a high contrast color scheme, and figure 5 (which presents uncertainties) is still presented at the native resolution of the inversion, all figures showing inversion results (Figure 7, 8, 10) use a washed out color scheme with smooth contouring that make it difficult to interpret the spatial variability and overall magnitude of the estimated fluxes. Many figures also have very small fonts, inconsistent color schemes, etc., which further complicates the interpretation of results.

#### OTHER SPECIFIC COMMENTS

The use of a single uncertainty for all towers does not seem justified (p. 10204-5). At the very least, I would expect that the degree of mismatch will differ between tall and short towers.

Related to this issue, do the a posteriori fluxes from the inversion reproduce available observations to within the assumed mismatch error (figure 3)?

Overall, how are the results of the inversion evaluated? Do the a posteriori bias estimates remain close to their prior values? How are other model assumptions evaluated? Are results equally reliable throughout the continent? Have any OSSE studies been conducted to evaluate in an idealized setting whether the presented approach can accurately constrain biases in a setup analogous to the one presented here? In other words, how do the authors evaluate whether they can trust their estimates?

The papers lacks references to several papers that have taken related approaches, either through the implementation of a Kalman filter approach to the solution of the inverse problem, the use of statistical methods for estimating error covariances, the use of correlation length scales to describe the covariance of fluxes or errors, specific

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choices for observations error variances, etc. The authors need to put their approach and their modeling choices more clearly within the context of other recent inversion papers, both to give credit to this earlier work, and to better justify the changes from previous approaches that they have chosen to implement.

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