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Interactive comment on "North American CO_2 exchange: intercomparison of modeled estimates with results from a fine-scale atmospheric inversion" *by* S. M. Gourdji et al.

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We thank the reviewer for their constructive comments and suggestions, which will help to improve the quality of the manuscript.

Comment: "The paper compares flux estimates from a geostatistical inversion at a high spatial and temporal resolution with those from other inversions as well as with bottom up estimates. Instead of prior fluxes, in contrast to other inversions the geostatistical inversion uses auxiliary variables such as fossil fuel emission inventories and evapotranspiration and other variables from the North American Regional Analysis (NARR) at 3 hourly resolution.

One of the key advantage of Bayesian or geostatistical inversions is their ability to quantitatively propagate uncertainties from the observational data into the posterior flux estimates. Unfortunately, the results presented in the manuscript do not incude any posterior uncertainty estimates, as they where regarded unrealistic (P6787, L 18). This is specifically unfortunate, as the authors attempt to falsify bottom-up model estimated fluxes. The authors should reconsider if a posterior uncertainty estimate can be provided. Given that all uncertainties are optimized in the geostatistical inversion model (GIM), and that they have an impact on the outcome of the inversion, this should be possible."

We originally chose to not present uncertainties in this paper because we believed that they were unrealistically small, especially at temporally-aggregated scales (i.e. monthly or annual). This was evaluated using an inversion conducted in a synthetic data environment, and the under-estimation of the uncertainties at large scales was attributable to the lack of a priori temporal covariance assumptions, as implemented in this manuscript. At the time when this manuscript was written, we were constrained computationally in terms of being able to include temporal covariance assumptions and calculate full annual uncertainties. However, these computational limitations have since been resolved. Therefore, for the revised manuscript, we have chosen to include temporal covariance assumptions for both the flux estimates and uncertainties, and then present the uncertainties in the paper for spatially-aggregated flux estimates.

Comment: "A further shortcoming is related to the choice of linear coefficients for environmental variables, which uses the full year. Given that the growing season dominates the fluxes and their variance, may be a specific choice needs to be made to better capture dormant season fluxes. This choice of annual coefficients also has very likely an impact through aggregation on respiratory fluxes, e.g. as seen in the stronger sources seen in GIM inversions for the temperate grass/savannah/shrub biome. Together with the fact that no posterior uncertainties are available, the claim that process models need to better account for management should not be based on the comparisons to

BGD

8, C3980–C3987, 2011

Interactive Comment

GIM inversion results. This should be formulated more carefully."

We agree that the variability in the growing season does provide the strongest constraint on the results of the variable selection and the inferred betas for the NARR inversion, which could potentially negatively impact the spatial patterns in the dormant season. However, the components in the trend mostly impact grid-scale fluxes (as seen in the NARR inversion results), and have less impact on spatially-aggregated (e.g. biome-scale) fluxes in well-constrained areas of the continent. For example, one can see in Figures 4 and 6 the similarity between the Simple and NARR inversion results in the Temperate Grass/ Savannah/ Shrub and Boreal Forest biomes, whereas the fluxes from these two inversions are more distinct in less well-constrained areas like the Eastern Temperate Forests. Also, in the Simple inversion, the only component in the trend is the fossil fuel inventory, and therefore the grid-scale biospheric fluxes shown in Figure 3 are not impacted by the inferred coefficient on this variable. Together, this implies that breaking up the linear coefficients by season, as suggested here, would not change the conclusions of the study in well-constrained areas at spatially-aggregated scales, e.g. in the midwestern agricultural regions.

Comment: "In addition, the authors emphasize the unprecedented spatial scale of the inversion, but do not provide any comparison to eddy covariance data. Thus the claimed reduction in aggregation error remains largely speculative. It should be clearly stated why a comparison to flux observations is not possible, even though the spatiotemporal resolution is significantly higher compared to previous studies."

The grid-scale resolution of the inversion (1x1) is significantly coarser than the footprint of eddy covariance data (about 1 km2), making direct comparisons difficult due to the scale mismatch. In addition, we believe that the aggregation error argument that we use to motivate estimating fluxes at 1x1 and 3-hourly resolution may have been misunderstood by the reviewer. By solving at fine scales, we do not expect to be able to trust flux estimates at these scales (as shown by high uncertainties that imply few significant grid-scale sources or sinks, as will be presented in the revised supplementary material). Rather, we believe that by solving at fine scales, we are able to more accurately attribute fine-scale variability in the CO2 measurement data to fluxes in space and time, and will therefore recover more accurate fluxes at post-aggregated coarser scales. Conversely, directly scaling fluxes for large regions in an inversion, given CO2 concentration data influenced by finer-scale variability, can lead to biased flux estimates at these larger scales. This result has been shown in numerous papers, e.g. Kaminski et al. (2001), Engelen et al. (2002), and Schuh et al. (2009), Gourdji et al. (2010).

Comment: “I also agree with the referee 1 regarding the presentation of the major driving environmental variables: their impact on the flux estimates should be clearly visualized.”

These plots will be added to the supplementary material. Again, please keep in mind that these figures will give insight into the grid-scale spatial patterns of the flux estimates, but not necessarily into the estimates at coarser spatially-aggregated scales.

Comment: “Specific comments: Fig. 2a: The footprints should be calculated for the selected measurement times from each location as specified in supplement B, as the data density is different for the locations, impacting on the spatial pattern. This should be mentioned in the text.”

To create this figure, we summed the sensitivity across all measurements used in the inversion to each estimated flux location and time period, using the selected measurement times in supplement B as suggested by the reviewer. Then, for each flux location, we averaged this summed sensitivity across all 3-hourly estimated fluxes for the year. We will further clarify this procedure in the figure caption.

Comment: “P 6790, L 1: Not using remotely sensed vegetation indices just because they are reported on weekly rather than 3 hourly time steps seems arbitrary. Vegetation indices are not expected to change on such small timescales, so technically they could be included by using a simple temporal interpolation to the desired time step. Why has

BGD

8, C3980–C3987, 2011

Interactive Comment

this not been done? NARR evapotranspiration and canopy conductance are indirectly influenced by AVHRR green vegetation fraction, however a climatology is used rather than actual data in the Noah LSM; thus using NARR fields is not a real alternative to using weekly specific fields of vegetation indices."

We agree that using remotely sensed vegetation indices would give additional information to that provided by the evapotranspiration and canopy conductance variables from NARR. However, the concern about the temporal resolution of these variables is that the relationship between these vegetative indices and CO2 flux changes throughout the diurnal cycle, not that the indices themselves are changing substantially within an 8-day period. In order to appropriately take advantage of datasets such as MODIS LAI and fPAR within the model of the trend, we would have had to use diurnally-varying coefficients within the X matrix, adding substantial complexity to the model. This was evaluated in preliminary work, but was ultimately not included because it was not seen to help improve flux estimates. Also, as mentioned previously, the components in the trend have relatively little impact on spatially-aggregated flux estimates in well-constrained areas, reducing the motivation for introducing complexity into the trend for the purposes of this study. However, in future work focused on inference rather than flux estimation, experiments breaking up variables and inferred coefficients by portions of the diurnal cycle and year would be especially interesting for gaining process-based understanding into CO2 flux based on the atmospheric data constraint.

Comment: "P 6790, chapter Fossil fuel inventory: presubtracting, uncertainty in modelled fossil fuel signals? Ok, may be by optimizing R specific for each observation location and month: : : Was an impact seen, e.g. by plotting station specific uncertainties against the fossil fuel signal or its variance?"

In general, the uncertainty in the fossil fuel inventories is thought to be low in comparison to that in biospheric models, for North America. However, errors most certainly still exist. Also, a 1x1 grid-cell average in the fossil fuel inventory cannot perfectly explain the measured fossil fuel signal at the towers due to representation errors, and this

BGD

8, C3980–C3987, 2011

Interactive Comment

uncertainty should be appropriately accounted for within the inversion.

As suggested by the reviewer, we compared the variance in the fossil fuel signal at the towers (created by transporting forward the fossil fuel inventory to the tower locations and measurement times) to the optimized station-specific, monthly model-data mismatch variances. For most months and towers, the variance in the fossil fuel signal was less than that of the model-data mismatch. However, for a few isolated months, particularly at towers close to urban areas or downwind of industrial areas (e.g. HFM and SBL), the variance in the fossil fuel signal was higher than the model-data mismatch. Regardless, this type of analysis does not address representation errors due to fine-scale emissions variability around the towers, given that the fossil fuel inventory is defined at 1x1. An analysis of how well our estimated fluxes reproduce the data a posteriori has given us some confidence that our R matrix does capture all sources of model-data mismatch, including that due to the fossil fuel signal.

Comment: "P6794, L16: The fact that canopy conductance was not selected as a significant variable might be related to the fact that its information is largely contained evapotranspiration fields. Has this been assessed?"

The correlation between the evapotranspiration and canopy conductance variables from NARR in 2004 is 0.73. Therefore, the reviewer is most likely correct that these variables contain overlapping information, which is why only evapotranspiration (which appears to be a better correlate to GPP) is selected by the statistical variable selection procedure.

Comment: "P6795 L15: Similar to the fossil fuel inventory, which contains average diurnal cycles of specific activity factors such as emissions from transport, also fire emission inventories exist that have information on sub-diurnal variance. Those could have been incorporated."

We are not currently aware of fire emission inventories with diurnal variability. However, we did test the use of 8-day average fire emissions from the GFED database in the inversion, and this dataset was not selected as a significant covariate in the variable selection procedure.

Comment: “Fig. 4, right column: rather than highlighting the biospheric model with the closest agreement to the GIM fluxes, the median of the biospheric models should be shown as done in Fig. 3.”

We agree, and we will change this along with the corresponding discussion of individual biospheric models in Section 3.2.2. We will also add shading to this figure to indicate 2σ confidence intervals from the NARR inversion.

Comment: “P6802 L : The south eastern forest plantations are probably not well constrained in the inversions, either, when looking at the footprint (Fig. 2a).”

Yes, the reviewer is correct. The under-constrained southeastern forests are mentioned in the text on p. 6787, lines 4-5, and also in Section 3.2.2, p 6800, lines 4-6. This helps to explain why we see more of a spread between the Simple and NARR inversions in the Eastern Temperate Forest biome relative to the Boreal Forest and Temperate Grass/ Savannah/ Shrub, given that the auxiliary variables have a particularly strong impact on flux estimates in under-constrained areas.

Comment: “P6807 L16: Attributing the spread in inversion results for the North American carbon budget to differences in CO2 mixing ratios of air entering the country rather than to differences in prior somewhat contradicts the argument made on P6806 L29 regarding the temperate grass/savannah/shrub biome, which is responsible for a large fraction of the North American carbon budget.”

It is difficult to know why the GIM results show stronger sources in the Temperate Grass/ Savannah/ Shrub biome relative to the other inversions in the dormant season and at the annual timescale. However, at this sub-continental spatial scale, the observed differences across models can most likely be attributed to the choice of priors, covariance parameters, flux estimation scale, choice of observations, etc. At the net annual continental scale, it is true that the spread in the inversions is also wider than the spread in the GIM results due to the boundary conditions, particularly due to the Butler et al. results. Therefore, some aspect of inversion setup must be contributing to the relatively strong North American sinks seen in the Butler et al. results, rather than just the boundary conditions. The statement on P6807 L16 about the boundary conditions being the primary determinant of the continental budget was meant to compare the GIM results with CarbonTracker boundary conditions to that of CarbonTracker itself, which uses a substantially different inversion setup and prior assumptions. We will reword this statement in the manuscript to clarify.

Comment: "P6808, L22: The comparison of bottom-up with top-down estimates is not new."

We agree with the reviewer and will reword this statement.

Comment: "Technical comments, Fig. 5: which are the results for GLOBALVIEW, and which for CT boundary conditions? Should be included in the figure caption."

We will include an indication of the boundary conditions used in the figure caption, as well as in the figure itself.

Comment: "Supplement B, page 2, 2nd paragraph: replace "is subject other challenges" by "is subject to other challenges""

Yes, thank you. We will fix this.

Comment: "Supplement C, page 3, there is a reference (probably to a figure) missing regarding the model domain"

This should be a reference to Figure 1 in the main manuscript. We will fix this.

Interactive comment on Biogeosciences Discuss., 8, 6775, 2011.