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Biogeosciences Discussions

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

Comment: "The results are looking better than their previous version, but cannot agree with their claim that the flux patterns are derived without the use of a priori. We suspect the evapotranspiration and/or other drivers of CO2 fluxes used in the inversion provides strong 'a priori' for GIM. No wonder the results of CO2 flux distributions look so similar to those simulated by the terrestrial ecosystem models (TBMs). The authors needs to show the maps of major drivers and revise the manuscript accordingly."

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Interactive Discussion

Discussion Paper

While the grid-scale maps from the NARR inversion do partially reflect a combination of the patterns of the NARR variables (especially evapotranspiration, which explains a large portion of the small-scale variability in the fluxes), two sets of results are presented here: those with and without NARR variables, or the Simple and NARR inversions. The grid-scale maps from the Simple Inversion are completely independent of the biospheric model median, as shown in Figure 3 in the manuscript. At aggregated spatial scales, both inversions look quite similar, particularly in the well-constrained biomes (i.e. the Boreal Forest and Temperate Grass/ Savannah/ Shrub), implying that fluxes at these scales are more reflective of the atmospheric data constraint than the underlying auxiliary variables used in the model of the trend (or the geostatistical "prior"). However, to address this concern, we will add maps to the Supplementary material of the underlying auxiliary variables in the NARR inversion, showing the contribution of each component to the final flux estimates.

Comment: "page 6778, line 20ff : it is unfortunate that the cite NOAA/ESRL network only, while the Gurney and Baker papers used Globalview data products for sites managed by many other organisations."

We will reword this sentence to acknowledge other data providers around the globe.

Comment: "page 6778, line 25ff : I think the transport models are also not that sofisticated to ingest continental site data. We find the selection of continental data as done here or elsewhere by imagining that the transport model performs better or worse at certain time of the day or certain sites vague. For synoptic variations, it does not really matter, as seen in TransCom continuous studies, whether day, night or daily averages are selected."

We disagree that the transport model used here (WRF-STILT) is not sophisticated enough to ingest continental site data. This Lagrangian model has been specifically designed to represent the near-field around highly variable measurement locations (Lin et al., 2003), and the high-resolution meteorology in WRF (i.e. 10-km around the majority of the towers) implemented here also helps towards this end. A number of recent papers have compared the use of high-resolution mesoscale transport models to coarser-scale global models and concluded that the mesoscale models perform significantly better in areas with complex topography and high CO2 flux variability (Ahmadov et al., 2009; Nehrkorn et al., 2010; Pillai et al., 2011). We do know, of course, that there still exist transport model biases that can be difficult to assess. One approach is to transport forward modeled fluxes to measurement locations and then compare simulated concentrations to actual measurements. A weakness of this approach is an inability to decompose errors in the fluxes from errors in the transport. However, using multiple flux models can partially help to separate these two sources of errors. This exercise was performed for all measurement locations and times of the day used in this study. Our results supported the commonly-held notion that the transport model used (WRF-STILT), in general, is most robust during the well-mixed afternoon hours. Also, the decision to remove data collected on the Pacific Coast (e.g. La Jolla or the flask and aircraft sites at Trinidad Head) was also made due to the observed mismatch between modeled and actual concentration data at these sites. A statement clarifying the choice of data by time of day will be added in the revised version of the manuscript.

Comment: "Similarly, one can select the background conditions for the coastal sites and use in inversion. For example, La Jolla and other sites in California may provide useful constraints for the western side (page 6784, last para)."

The GlobalView, or "empirical", boundary conditions used in the manuscript represent a 3-dimensional curtain (varying by latitude, altitude and time) created by interpolating concentration data including those collected on the West Coast, e.g. at La Jolla or Trinidad Head. Therefore, while West Coast sites were used to generate the background conditions for the empirical boundary conditions (some of which were also included in the CarbonTracker data assimilation system for the CarbonTracker boundary conditions), we did not include these data directly in the inversion due to potential concerns with transport error in WRF-STILT.

Comment: “page 6786, line 4ff : I am not sure that everything is that good in backward trajectory models compared to the Eulerian models. How well is convection represented in the trajectory models. How different are the plumes when WRF is run at higher resolution, say 20 km or less grid size, and the same meteorogy is used for runing trajectories.”

The representation of convection in WRF-STILT is evaluated in Nehrkorn et al. (2010). To build on this work, we performed sensitivity tests by creating trajectories with and without the 10km resolution WRF domain in the eastern United States (as shown in Figure 1). Results were generally consistent, although there were instances of the higher model resolution leading to more sharp differences in direction and coverage in the footprints. Ultimately, we chose to work with the higher resolution (i.e. 10km) in the vicinity of most of the towers, and used the 40km resolution elsewhere. This was motivated by the goal of maximizing the representation of near-field transport, while also making computational costs manageable. A complete analysis of the impact of the spatial resolution of the meteorology on the computed footprints is an interesting topic, but outside the scope of this study.

Comment: “Much of such discussions on methodology (Section 2) can be reduced significantly, just by focussing on what is done here. Unless comparisons are done for various aspects of the methods, the better or worse statements makes less sense in the main paper (may move such details to supplements).”

We will try to reduce the discussion of methodology in the final paper, moving less relevant details to the supplementary material.

Comment: “page 6789, line 11 : how is that the ’...atmospheric observations provide the only data constraint on biospheric fluxes’, while infact the atmospheric CO2 is a product of "all" types of fluxes and transport. It is only that we traditionally assumed emissions from fossil fuel burning as a known type.”

The original intent of this sentence was to emphasize that the Simple inversion does

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not use any ancillary information about biospheric fluxes, unlike the NARR inversion. It is true that the inferred biospheric fluxes from both the Simple and NARR inversion may be affected by biases in the fossil fuel inventory used to separate the two signals. We will rephrase the sentence to try to make this clearer.

Comment: “page 6790, line 1 ff : I am wondering whether some sort of LAI and PAR parameters are already used in NARR model running?

As mentioned in the text, the NARR data rely on NDVI from the AVHRR instrument (which is used in the Noah Land Surface Model), but not LAI or fPAR as far as we are aware.

Comment: "page 6791, line 1 ff : have you tried any sensitivity run by changing the fossil emission pattern or strength and check whether the recoverred reggression coeff was significantly different from ’one’? Also this discussion is repeated in Section 3.1.1."

While we did not try any sensitivity tests changing the fossil fuel emission pattern or strength, we performed numerous sensitivity tests varying the atmospheric data used (by time of day and/or included sites). We did find the recovered regression coefficient on the fossil fuel inventory to be somewhat sensitive to the data choices for the inversion. Therefore, the regression coefficient of 1.0 should not be taken as a validation of the exact spatial patterns in the inventory. We will clarify this in the revised manuscript. We will also reduce the length of the repetitive discussion in Section 3.1.1.

Comment: "Section 3.2 : I am curious to see diurnal cycles you derive by GIM, in comparison with SiB-hourly or CASA-3hr fluxes."

A comparison of the monthly average diurnal cycle for July 2004 is shown in Figures 1 and 2 below for 2 grid-cells: one in a forested area in Ontario, and the other in an agricultural region in Iowa. Fluxes are compared for these grid-cells from the Simple and NARR inversions, as well as from CASA-GFED and SiB3. While both inversions have a weaker diurnal cycle than the bottom-up models (CASA-GFED or SiB3), the

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NARR variables help to increase the amplitude of the diurnal cycle. Given that we use mostly day-time (afternoon) data in the inversion, it is remarkable that the shape of the inferred diurnal cycle looks so realistic. Note also that, while the differences between the diurnal cycles as inferred by the inversions and the biospheric models are substantial, so is the difference between the CASA-GFED and SiB3 diurnal cycles themselves. Finally, please note that we estimate fluxes directly at the 3-hourly, grid-scale in the inversions in order to avoid spatial and temporal aggregation errors in post-aggregated fluxes. However, we do not expect to be able to interpret fluxes directly at the fine spatial and temporal scale shown in the figures below. (Specifically, we expect the a posteriori uncertainties from the inversion at the grid-scale to be relatively high at this scale, although we do not currently have them to show in Figures 1 and 2 below.)

Comment: "page 6797, line 20ff : is this true if you look at California & Nevada in terms of say MODIS LAI?"

The reviewer is right that there should be short longitudinal correlation lengths in U.S. states like California and Nevada, which may be shorter than the latitudinal correlation lengths at the same locations. This line in the text was meant to make a more general point that the covariance structure of biospheric CO2 fluxes can be quite complex, and that isotropic assumptions are likely an over-simplification in most areas. This is particularly true for the Simple inversion, where the covariance structure in Q is used to describe the total biospheric flux. In contrast, in the NARR inversion, the NARR variables explain a substantial portion of the variability in the fluxes, and therefore we expect the residual covariance structure (modeled in Q) to be more Gaussian, stationary and isotropic. We will reword the sentence in the text to emphasize these points.

Comment: "Section 3.2.1, para 3 : please show figures of evapotranspiration and discuss you flux results."

We will add maps with the contributions of the auxiliary variables to final flux estimates to the supplementary material. We will also include a short discussion in Section 3.2.1

as to how the spatial patterns of the NARR variables influence the final flux estimates.

Comment: "page 6800 : one can argue that GIM and CT are having weaker summer uptake in the boreal region. Schuh/Butler might have extrapolated signal from high productive region. How good is the data coverage for GIM & CT inversions over the boreal region?"

The reviewer is right that it is difficult to know which set of inversions is more likely to be accurate over the Boreal Forest region without any validation data. However, as shown in Figures 2a) and b) in the manuscript, the Boreal Forests are relatively well-constrained by the atmospheric data, especially in the central portion. Therefore, a well-designed inversion (barring substantial transport errors) should be able to accurately resolve the fluxes for this region. The main difference in flux estimates in the Boreal Forest (shown in Figure 4 in the manuscript) is between the GIM, CT and Butler et al. results (given that the Schuh et al. flux domain did not include this region). The coarse resolution of the estimation regions and concentration averaging in the Butler et al. study lends support to the idea that the Butler et al. fluxes may be biased due to aggregation errors, relative to the GIM and CT results, in both the Boreal and Eastern Temperate Forests. An over-sampling of highly-productive areas, combined with an inability to correctly spatially attribute this uptake, could explain the overall stronger apparent uptake for the region as a whole in the Butler et al. study.

Comment: "You must show the measurement locations on Fig. 2a for better clarification on this. We also think, all information shown in Fig. 1 can be merged on to Fig. 2."

The measurement locations are generally in the center of the burgundy areas in Figure 2a), but we can add small stars for the exact location. We can also try to add outlines to Figure 2a) to represent the boundaries of the biomes currently shown in Figure 2b).

Comment: "page 6800-6801 : a lot of speculations in this text."

We will rework this section to eliminate speculative argument in regards to particular models. For example, the discussion comparing individual biospheric models to GIM results will be substantially shortened, and replaced with a comparison to the biospheric model median at the monthly biome-scale.

Comment: "page 6801, line 11ff : In the early part of the paper you have argued for high resolution inversion. From this discussion here, We cannot see any sign of aggregation error playing any role in hindering CO2 flux estimation. Please clarify."

The line the reviewer is referring to is not a discussion of aggregation error, but rather a statement that prior flux assumptions appear to have little impact on flux estimates at post-aggregated spatial scales, e.g. the biomes shown in Figure 4 in the manuscript. The aggregation error argument is something different. This argument is that, an inability to attribute fine-scale variability in the measurements to grid-scale spatial patterns in the estimated fluxes at fine temporal scales, will most likely bias flux estimates at larger post-aggregated scales. Following from this, we make the argument that the coarse estimation resolution in the Butler et al. study may explain the anomalously strong uptake in the Boreal and Eastern Temperate Forests relative to other studies, including GIM.

Comment: "page 6803, para 2 : do you really need this?"

Summary paragraphs help to integrate the message presented in previous sections. We will re-examine these in the revised manuscript, and shorten or eliminate them where possible without sacrificing clarity.

Comment: "Section 3.3 : I do not follow your argument. Why the model to model to differences pops up when aggregating to annual mean (from monthly means) in a negative way (if We get your point right!), but spatial and temporal aggregatiion worked alright from 1x1 deg and 3-hourly to continental and monthly? Or, is it just that the scale of plottings are different?"

The main point here was that all models can be expected to represent the monthly seasonal cycle fairly well. However, after removing the seasonal cycle, differences in the models at the net annual timescale may become more apparent. We will reword the text in this section to make this point more clear.

Comment: "Section 3.3.2 and Fig. 6 : so far you have seperated the biospheric fluxes from fossil fuel emission. Adding fossil fuel emissions to biosphere in Fig. 6 is certaintly not desirable. We strongly recommend you to show the biosphere fluxes separately to be consistent with Fig. 3 - 5."

Our original intention was to show that net annual biospheric sinks from all models are small in comparison to the fossil fuel signal at the annual timescale. But we can also see how this is a secondary point that confuses the main results highlighted within the discussion. We will therefore change this plot to show biospheric fluxes only, as suggested by the reviewer.

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

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Interactive Comment

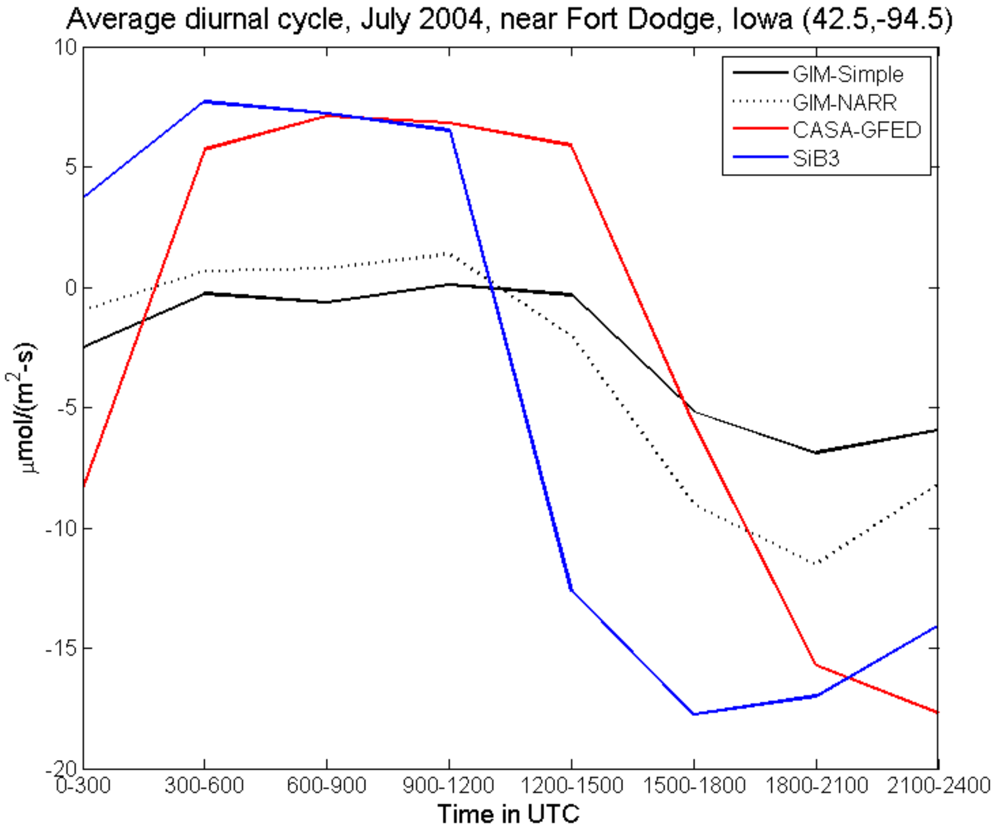

Fig. 1.

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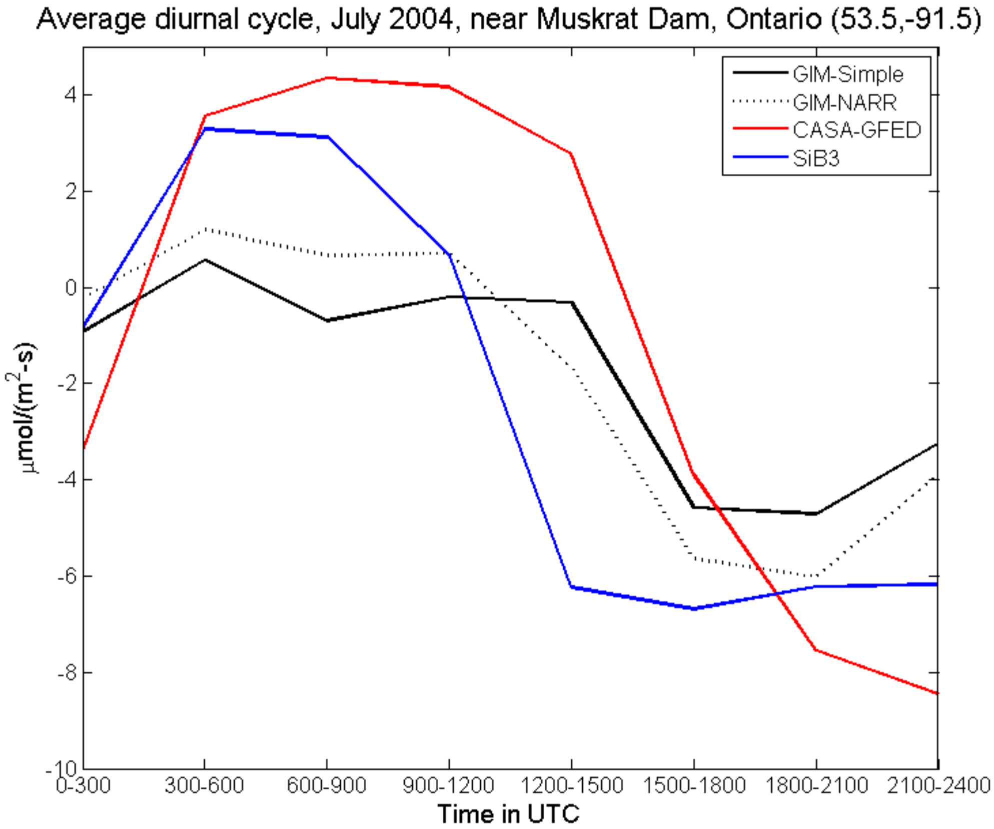

Average diurnal cycle, July 2004, near Muskrat Dam, Ontario (53.5,-91.5)
GIM-Simple
GIM-NARR
CASA-GFED
SiB3
μmol/(m2-s)
4
2
0
-2
-4
-6
-8
-10
0-300
300-600
600-900
900-1200
1200-1500
1500-1800
1800-2100
2100-2400
Time in UTC

Fig. 2.