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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.

Anonymous Referee #1

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The mansuscript discusses the CO2 fluxes at 1x1 degreee latitude-longitude grid scale to continental scale, i.e., North America (coverring the latitudes of Central Americal to Canada) using the Geostatistical Inversion Model (GIM). 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. I 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. Major revision is needed before publication in Biogeosciences.

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Specific comments:

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.

page 6778, line 25ff : I think the transport models are also not that sofisticated to ingest continental site data.

I 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.

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).

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.

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).

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.

page 6790, line 1 ff : I am wondering whether some sort of LAI and PAR parameters

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are already used in NARR model running?

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.

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

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

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

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?

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

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

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

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

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 I get your point right!), but spatial and temporal aggregatiion worked alright from 1x1 deg and 3-hourly to continental and monthly?

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Or, is it just that the scale of plottings are different?

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. I strongly recommend you to show the biosphere fluxes separately to be consistent with Fig. 3 - 5.

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