Biogeosciences Discuss., 11, C3049–C3051, 2014
www.biogeosciences-discuss.net/11/C3049/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Atmospheric inversion of the surface carbon flux with consideration of the spatial distributions of US crop production and consumption" *by* J. M. Chen et al.

Anonymous Referee #2

Received and published: 30 June 2014

The paper appears to be heavily dependent, and similar, to Deng and Chen 2011. The only real difference seems to be that the authors adjusted the prior flux estimation in the Midwest U.S. using agricultural data, and at a resolution much higher than the transport operator, or inversion framework, could hope to resolve. Much of the basic "lateral C transport" content has already been written about somewhat extensively over the past five or six years. The paper doesn't appear to bring much new to the table, utilizing old techniques, extremely coarse transport operators, and interpolated/smoothed CO2 data.

The interesting piece of the work was the sensitivity of the inversion results to the MidWest prior flux adjustment. Having a global inversion framework allows for the

atmospheric circulation patterns necessary to "connect" a number of these regions. Additionally, TM5 has been used for a lot of global inversion work so there might have been some application if they had.

We applaud the authors on including a covariance matrix in the paper (Table 4) although an exploration of this matrix might have been very interesting. Other "regions" which we know influence the MidWest, such as the TX/OK/LA area, due to monsoonal like summer time flow, appear to be more strongly related to flux uncertainty in the MidWest than the SE U.S. but are not commented on as far as I can recall. The strong correlations between these regions even comes up in regional inversions like Schuh et al 2010 and Miller et al 2013. Could the authors provide more specific commentary/support on Table 4?

I believe the authors would have been better to focus more on these topics than on complex high resolution biosphere modeling which doesn't appear to be useable at nearly the resolution that it is constructed at.

One of the main messages of the paper appears to be that we should use the "best" a priori flux information available and I'm sure nobody would ever have an argument with that, although the effect of using this information is somewhat fuzzy. The inability to connect the posterior flux estimates to any other metric that might provide evidence of improvement makes conclusions somewhat limited.

It will also most certainly be a point of contention in the inversion community whether one can justify that interpolated/smoothed CO2 from a limited network on a 3x2 degree grid can provide much information on regional fluxes, despite the fact that Deng and Chen 2011 appears to have done the same thing.

The authors find a 50% deeper sink over the Midwest as well as the United States. One would have thought that cropland production statistics would be reasonably constrained at the national level. Therefore, reasons for similar estimates ($\sim$ +50%) at both levels should probably have been explored more, i.e. do you think these #'s are right? In other words, is there any ancillary data to support these new sink estimates as being correct?

One particular note is that by adding only 10% uncertainty on the sink that is added to the MidWest, it would appear that one is simply adding a certain sink to the flux estimate. There must be a large uncertainty on the non-crop portion of the sink? in order to generate a large posterior sink. Could the authors comment on this? it seems a bit confusing.

Additionally, patterning the crop flux seasonal signal after NEP from a forest biosphere model appears a bit of a stretch as the ag seasonal cycle is of a much shorter and intense cycle. Do the authors have any notion of the error that is possibly induced or sensitivity to attributing the fluxes in this way?

Figure 1 gives the impression of relatively dense coverage of data over the Midwest U.S. but (I believe) the tower/flask data is essentially nil between 2000 and 2007 (unless I'm mistaken). Will the authors provide the data that is used to justify the observation points in Fig 1?

Lastly, the authors should be warned that, although stated in the manuscript, that GlobalView is not a data product and therefore the use of an interpolated data product should be taken with a grain of salt. Further studies should be conducted on such "higher level" products to ensure their consistency with the general inversion framework typically used.

There are a few grammatical mistakes and errors in some equations although not to the point of distracting from the paper.

In summary, we don't find a lot of specific errors in the paper (and hence are recommending minor revisions) but do not necessarily feel the paper adds a lot of new information to the literature.

Interactive comment on Biogeosciences Discuss., 11, 6069, 2014.