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Geoscientific Model Development Discussions

# *Interactive comment on* "A Joint Global Carbon Inversion System Using Both $CO_2$ and $CO_2$ Atmospheric Concentration Data" *by* Jing M. Chen et al.

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This paper describes a new method for using C13O2 observations in atmospheric inversions for land and ocean carbon fluxes. The novelty in the method relies on the very direct use of underlying biogeochemical models to estimate some of the terms required, namely the discrimination and disequilibrium fluxes. The paper is generally clearly written and in scope for GMD. I believe it should be published with minor revisions.

For the sake of transparency I mention that I reviewed a previous version of this paper for another journal. I have tried to review the new version in its own right, without reference to the older version. That is obviously difficult and the authors and editor



may wish to take this into account when considering my review.

I believe the paper makes a useful though not dramatic contribution to a field which has been shrinking. The field is the use of C13 observations in atmospheric inversions of carbon fluxes. Atmospheric C13 started out as a panacea for the big scientific question of the 70s and 80s in global carbon cycle studies, what was the relative contribution of land and ocean to the global carbon sink. Once formal inversion methods appeared it became apparent that the extraneous or nuisance variables needed to close the C13 budget carried such uncertainty that the original purpose of the observations was out of reach. Later papers made more limited use of the observations, e.g. to determine interannual variability but these were also challenged as uncertainties arose over assumptions they required. Couple this with difficulties of generating and propagating measurement standards and one can understand declining interest in using atmospheric C13.

A solution to the problem of nuisance variables is to calculate them more rigorously and with consequently lower uncertainty. The use of biospheric models which include treatment of isotopes is one way to do this and this paper is one attempt at this.

The task has two stages: Model the relevant isotopic effects then include the resulting information in an atmospheric inversion. The authors have done a good job with the first part of this (as have many before) but a poor job with the second.

The failure comes in the way uncertainty is transferred from the modelling exercise to the inversion. It can't be said often enough that the inputs to inversion studies are probability distributions not mean values. This means that the uncertainty is comparably important to the mean (or whatever other location parameter is used). The formulation of the inversion here does not allow uncertainties like those of the disequilibrium flux to enter the inversion since these fluxes are not included as unknowns. The authors compensate for this with some sensitivity experiments which is only a partial solution. We can't tell without doing it how much the atmospheric C13 information would have fed back on knowledge of the isoflux itself. There is a yet harder problem of uncertainties in the net flux discrimination which raises a number of technical problems (not to mention being difficult to calculate).

I still recommend publication since the task of feeding some information from a C13 model to an inversion is still worthwhile. The authors, though, need to be clearer about what they have not done and its consequences. I recommend text either in the introduction or discussion pointing out that the work is part of a larger project among the community and how far along the road the current work takes us.

Specific comments

P5L20-25 I'm not sure what the authors mean by the difference from Rayner et al. (2008), probably they mean the dilution of net fluxes by equilibration. This is not that small a term and is easy to add, is there a reason why the authors didn't do it?

P7L20 It's worth noting that by assuming the isoflux terms are perfectly known the authors are somewhat begging the question. The isoflux is the 2nd largest term in the atmospheric C13 budget so this should have a big effect on the calculated uncertainty.

P8 Note that "ignoring" the isofluxes isn't a good description of the authors' sensitivity cases. They're in fact setting those fluxes to zero. This isn't quite a classic sensitivity experiment since it doesn't consider the derivative of the desired quantity (inverted fluxes) with respect to the considered quantity (isoflux) but rather that sensitivity multiplied by a given change. Thus if, for example, one isoflux is 40 and the other is 80 the second will appear twice as sensitive even though the response of inverted fluxes to a unit change might be the same. In more detail, sensitivity calculations should really consider the uncertainty of the varied quantity, the calculation here makes an implicit assumption of 100% uncertainty.

P12 Rayner et al., 2008 didn't calculate a mesophyll conductance.

P14 are disturbance effects for the U.S. and Canada considered during the inversion



period as well as before it? If so is there a risk of double-counting with GFED over these regions?

P16 The term "discrimination flux" seems to be new here, is it anything other than the net flux multiplied by the relevant discrimination? If so perhaps use a different term.

P18 Regarding improvement in Amazon uncertainty: It could also be just that the posterior uncertainty from the CO2-only case is large here so new information contributes more

P19 I would expect the growthrate constraint of CO2 to be very strong so that land and ocean changes should compensate when one changes disequilibrium, this isn't happening, can the authors explain why not?

P19 The original spatial inversion using C13 (Enting et al., 1993,1995) showed that the main impact of the C13 data on the land-ocean partition of uptake was via its global trend so the insensitivity to exactly what data is used is unsurprising, provided a global trend can be established.

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