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.

Answer: Many thanks for your accurate assessment of our manuscript.

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.

Answer: The current version of this manuscript has indeed benefitted greatly from your comments and suggestions during the review process of the previous versions. You have made a high-level intellectual input to this study that is valuable and highly appreciated. We realized that we should have acknowledged this in the Acknowledgement. We will do this in the revised manuscript.

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.

Answer: We completely agree with this view on the value of C13 measurements, which seems to be seriously confounded by the uncertainty of a large number of variables that influence the discrimination and disequilibrium of C13 exchanges in the Earth system.

Much of the uncertainty exists in biosphere-atmosphere exchanges, and therefore we attempt to reduce this uncertainty by developing a biospheric model that accounts for detailed processes in the C13 exchanges, including the separation of sunlit and shaded leaves (for the first time for C13 studies) and the use of Harley's mesophyll conductance equation in the C13 leaf model. This biospheric model is driven by remotely sensed vegetation structural parameters (LAI and clumping index), providing realistic spatial and temporal CO2 and C13 fluxes over land. The major advance that we made is in the relatively high-resolution of biospheric flux and atmospheric transport simulations that reduce the uncertainty in both the magnitude and spatial and temporal patterns of C13 concentration simulation. This in turn would improve the inversion results.

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).

Answer: We also agree that the uncertainty in estimating C13 fluxes has not been properly considered in our joint inversion system. Specifically, only the uncertainty in first the term of the right side of Eq. 10 (P7) enters into the inversion system through

the covariance matrix R, and the rest of the terms are not included in R. This makes the uncertainty for the transformed C13 concentration (ci) too small and has consequence in the inversion results. We will estimate the uncertainty of all terms in Eq. 10, and rerun the inversion for all scenarios.

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.

Answer: Thank you for your kind suggestion to just add discussion to our uncertainty estimation issues. We think that the issue is important enough, and we will rerun all our scenarios with new uncertainty to be estimated for all terms in Eq. 10. We will also add discussions on this issue where appropriate.

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?

Answer: Yes, we meant the dilution of the C13 flux to the atmosphere with time due to equilibration. According to the equation provided by Rayner et al. (2008), this is indeed not a small term (it reduces to about 25% in five years). We did not implement this equation because we felt that the terms in the transport Jacobian are the largest in the most recent months after a flux enters the atmosphere, and the influence of this correction would be small. However, since we are going to rerun the inversion scenarios, we will use your suggested equation in our inversion system to investigate its significance.

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.

Answer: We guess that this is related to the major issue of uncertainty treatment associated with modeled discrimination and disequilibrium fluxes. As replied above, we will rerun of inversion with new estimates of the total uncertainty in the transformed C13 concentration used in our inversion system.

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.

Answer: I see your point. In fact, the main purpose of the use of the various scenarios is to assess the importance of the discrimination and disequilibrium fluxes on the inverted CO2 flux at the regional and global scales. It is not a full blown sensitivity test. If the responses of the CO2 flux to these isotopic fluxes are linear, their sensitivities can be derived from the range of response to the range of isotopic fluxes. Since the responses are unlikely linear, only the bulk responses for the full ranges are simulated. We replace the word "sensitivity" to other words in revision under this context.

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

Answer: It was a mistake to include this reference for the statement on stomatal and messophyll conductance. This reference will be deleted there.

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?

Answer: The BEPS model used to create the CO2 prior flux according to remote sensing data does not include the carbon emission due to fire for any region, so there is no double-counting when GFED data are used to estimate the fire emission.

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.

Answer: The term "discrimination flux" means the flux due to discrimination. It is used to differentiate from "disequilibrium flux". It is convenient this way, although uncommon and odd. These terms first appear on page 8 associated with Eq. 10. For lack of better words, we will use these terms in the paper for convenience. ???

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.

Answer: This is indeed likely the case and was implied in our statement P17L34-P18L1. We will modify the statement to reflect this point.

P19 I would expect the growth rate 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?

Answer: Since the disequilibrium term is a large term in the C13 budget, it's influence on the carbon fluxes to land and ocean is also large. The CO2 growth rate is a large constraint on the overall fluxes to land and ocean but C13 observation also has almost an equal constraint on these fluxes in the joint inversion. This could be due to the fact that only C13 observation error is included in the error matrix, while model errors for C13 are not included. This issue may be addressed through using a comprehensive uncertainty for C13 in our inversion system.

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.

Answer: We agree with this point for global carbon cycle estimation. The reliability of the global average trends in CO2 and C13 concentration is most important is disentangling the fluxes between land and ocean as the global average, if the trends can be accurately obtained from existing data. However, in order to know the carbon source and sink distributions over ocean and land, much more data are needed. Our joint inversion system would be an effective way to gain this information using distributed observation sites.