Answer to Reviewers' Questions

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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.

Action: We have acknowledged your contribution in Acknowledgement of the revised manuscript, with the sentence "Dr. Peter Rayner was a critical and constructive reviewer of several versions of this paper and provided high-level intellectual inputs to this study which are greatly appreciated"

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.

Action: not needed.

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 (c_i) too small and has consequence in the inversion results.

Action: We have estimated the uncertainty of all terms in Eq. 10 (L269-276), and have rerun the inversion for all scenarios. Some results changed significantly but most results changed only slightly. Therefore, Tables 3 and 6 and Figures 7, 9, 10, 11, 12, 13, 14, and 15 are changed.

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 to deserve our full attention.

Action: We have rerun all our scenarios with new uncertainty for all terms in Eq. 10. We have also added 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 were not sure if it is necessary to do so. In both Joos et al. (1996) and Thompson and Randerson (1999), based on which you derived your equation, the pulse response function was developed to describe the decrease of the remaining fraction of a gas emitted to the atmosphere with time due to exchanges of the gas with land and ocean. This response function is useful for diagnosing the influence of a carbon emission from a particular source on the atmospheric carbon concentration at different times. However, from the mass balance point of view, we are interested in influence of C13 emission from a region to the various stations assuming there is no exchange of the emitted C13 with other surfaces because the influences of the exchanges on the atmospheric C13 concentration are already considered in the inversion system. For this reason, we did not use the equilibration adjustment. This could be a matter of debate, and we will make it clear through revising the text. This is similar to CO2 transport Jacobian calculation where a pulse of CO2 enters the atmosphere and distributes around the globe by wind without considering the exchange of atmospheric CO2 with the surface.

Action: We added a clarification on this issue (L181-186): "This simple treatment of the transport matrix differs from Rayner et al. (2008) who considered the reduced response of observed $^{13}CO_2$ concentrations to surface fluxes with time due to its accumulated exchange with the surface. As we are interested in the net CO_2 flux, the exchanges of both $^{13}CO_2$ and CO_2 with the surface are consistently not included in the M matrix calculation, although this simplification would induce errors in the inverted CO_2 flux when the accumulated exchanges are spatially highly heterogeneous."

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.

Action: As replied above, we have rerun the 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.

Action: We replaced the word "sensitivity" to other words in revision under this context, see L697 ("importance") and L735 ("difference").

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.

Action: This reference is now not cited for this point.

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 carbon emission due to fire for any region, so there is no double-counting when GFED data are used to estimate the fire emission.

Action: To make this clear, we added the following sentence in section 2.2.1: "The output of BEPS used as the prior flux in the inversions is NEP, which does not include carbon emission due to disturbance." (L345-346).

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, albeit uncommon. These terms first appear on page 8 associated with Eq. 10.

Action: For lack of better words, we continue to 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.

Action: To make it explicit, we added the following line "…… where CO2 observations are sparse, causing large uncertainties in the inverted flux in this region in the CO2-only inversion." (L641-642).

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 terms are large terms in the C13 budget, their influences on the carbon fluxes over land and ocean are 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. Our initial response to your question was that this could be partly due to the fact that only C13 observation error is included in the error matrix, while model errors for C13 are not included. However, after a new set of inversions with the model errors included for the C13 observation, we found similar results, although the magnitudes of changes from the CO2-only inversion are slightly smaller because the C13 constraint is relaxed.

Action: This issue is investigated through using a comprehensive uncertainty for C13 in our inversion system, and the results are discussed in L712-715: "The influence of $^{13}CO_2$ on the joint inversion depends only weakly on the estimated uncertainty in the $^{13}CO_2$ data. We found that if the uncertainty is reduced by half, the sum of the land and ocean sink deviates from the CO_2-only case by 2-6% for all scenarios, suggesting that the mean disequilibrium fluxes play the dominant roles in the joint inversion."

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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 in 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.

Action: We added the following statement at L732-734: "Our finding on the usefulness of the small $^{13}CO_2$ dataset somewhat confirms the claim of Enting et al. (1993 and 1995) that the temporal trend in $^{13}CO_2$ concentration is the major signal constraining the partition between ocean and land sinks.".

Anonymous Referee #2

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This manuscript presents an interesting approach for using $^{13}CO_2$ data as extra constraints for top-down flux inversions based on in-situ surface CO2 data. This approach has taken into account spatial variation of isotropic discrimination and disequilibrium by using a terrestrial

biosphere model and an ocean model to simulate the discrimination rates. The manuscript is well written, and their results are interesting. It should be published after minor revision.
Answer: Thank you for your positive comments and the penetrating comments below which indicate that you have understood our methodology and analyzed our results carefully. Your expert views are much appreciated.

Major comments:
My major concern is that the uncertainties in model simulation of D_j and C_i (Eqs. 6 and 8) have been properly taken into account in the flux inversions.

1. Ideally D_j should be part of the state vector, with prior estimates taken from the biospheric or oceanic model simulation. Treating D_j as a single fixed value could result in artificially enlarging the impacts of $^{13}CO_2$ data, as well as distorting the spatial distribution of the posterior fluxes.

Answer: We agree that Dj would ideally be part of the state vector if its uncertainty is to be fully considered for its influence on CO2 inversion. The same can be said to other isotopic parameters, such as disequilibrium coefficients over land and ocean. To include all these as state variables, our inversion system would have to be much too big to be manageable for usual computational facility. In order to keep our system simple and practically useful, we have followed the strategy to estimate these isotopic parameters as accurately as possible both spatially and temporally and used them as input and not to be adjusted in the inversion system. We also felt that the amount of C13 data is still limited, and we cannot be overly ambitious to do too much with the data. If we are not limited by computing resources, we may even opt for higher spatial and temporal resolutions rather than adding more state variables.

Action: we added a clarification (L246-249): "In the joint inversion system, we treat S_l and S_o as the state variables and D_j as predetermined parameters that vary in space (region) and time (monthly). It is therefore prerequisite to estimate accurately these parameters as well as other isotopic parameters on the right hand side of Eq. 8.".

2. The observation errors for $^{13}CO_2$ should also be enlarged to account for possible modelling errors (Eq.10). Actually it is a bit surprising that the uncertainties for both land and oceanic fluxes inferred from $^{13}CO_2$ data only (Table 6) are smaller than those directly based on surface CO2 data (Table 3), considering that they have fewer sites.

Answer: We completely agree and this is also implied by reviewer Peter Rayner.

Action: We have estimated the uncertainties of all terms in Eq. 10 (the total uncertainty of ci is nearly doubled), and have rerun our inversion for all scenarios. The resulting uncertainties in the CO2 flux as shown in Tables 3 and 6 have changed in the right direction.

3. In the joint inversion, the observation error correlation between CO2 and $^{13}CO_2$ data, (for example, due to the common model transport errors) has not been taken into account.

Answer: In our joint inversion system, the transport error is not included in the co-variance matrix, similar to many other systems. To do this properly, we need to run multiple transport models, and this would be the next step.

Action: We now mentioned this limitation (L451-453): "Since the main purpose of this study is to develop a joint inversion system, only one transport model is used, the transport matrix M is assumed to be free of errors.".

Minor comments:
1. Line 17, Page 4 ': : :60 months' The time period 2000-2004 could be mentioned here.

Answer: Thanks for this suggestion.
Action: it's done as suggested.

2. Eq. 9, Page 7: It would be helpful if the authors can add some discussions on temporal variation of D_j in the following sections.

Answer: Thanks for this suggestion.
Action: L246-249 are added to this effect.

3. Line 1, Page 13: 'A transport-only : : :' What is the spatial resolution of TM5 ?

Answer: The spatial resolution of TM5 is 6x4 degrees for the globe and 3x2 degrees for North America. The atmosphere is divided vertically into 25 layers with 5 layers in the planetary boundary layer.

Action: This information is now added at L444-446.

4. Line 34: 'equal the sum of : : :' Uncertainty of (ab) usually is not equal to such a simple linear sum.

Answer: We agree that in this case (P18L34), the total uncertainty should equal to the square root of the sum of the two variances.

Action: We recalculate the total uncertainty of the terms involved and modified the associated text in Section 3.2.4.

5. Figure 13. I only see blue solid line (instead of the green one in the caption). Also, it seems that over Northern hemisphere, the posterior model CO2 concentrations have a larger seasonal cycle than the GV data. What is the reason?

Answer: The solid line was indeed blue rather than black (a mistake in the caption). Over some stations in the Northern hemisphere, the posterior CO2 concentrations have a larger seasonal cycle that the observation. This is likely due the remainder influence of the prior CO2 flux which might have respiratory fluxes too large in the winter. However, we cannot rule out the influences of transport errors and the site representativeness.

Action: We have added some discussion to this effect (L761-767): "The posterior CO_2 concentrations for either CO_2-only or joint inversion show larger seasonal amplitudes than observations at northern hemisphere stations, although the means are about the same as observations. This suggests that both carbon uptake during the growing season and ecosystem respiration in the non-growing season might have been overestimated, even though the annual net carbon flux may be unbiased. Further work is needed to constrain the large photosynthetic and respiratory fluxes separately rather than the net flux only.".