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Interactive comment on "Atmospheric inversion of the surface CO_2 flux with $^{13}CO_2$ constraint" *by* J. M. Chen et al.

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1   General Comments

this paper presents a new attempt to use a combination of $\delta^{13}CO_2$ and CO_2 measurements in an inverse determination of surface fluxes over land and ocean. In some senses it is more ambitious than previous attempts such as (; ;) in its rather explicit use of a terrestrial model to calculate some of the terms in the $^{13}CO_2$ budget. It also makes use of a larger set of the GlobalView data product () and especially its $\delta^{13}CO_2$ counterpart.

In other respects it is a rather conservative advance. It does not use particularly high

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source resolution nor does it use the much richer datasets of continuous measurements now available. The paper is generally well presented. The methodology, however, is sufficiently unclear that my commentary may be based on a complete misunderstanding of what the authors did.

The results of the paper seem striking. Unlike most previous studies, the mean fluxes in the paper seem insensitive to details of the modelling of $\delta^{13}CO_2$. This is in strong contrast to the results of (;) who needed to include the product of disequilibrium and gross flux (sometimes called the isoflux) in their inversion explicitly.

I have struggled to understand the implications of the treatment of $\delta^{13}C$ in this paper. I'm going to try to summarize what I think the authors have done and comment on it. This will give the authors a chance to correct any misunderstandings but also perhaps give guidance to ways they can improve their explanation. I'm also not sure whether the equation numbering in the text and on the equations is consistent. I think the equations that describe the effective discrimination are in fact (6) and (7) but are referred to as (7) and (8)

Eqs. (6) and (7) define an effective discrimination of the net flux. This effective discrimination multiplies the CO_2 flux to produce a $^{13}CO_2$ flux. That suggests that if there were no CO_2 flux there would be no $^{13}CO_2$ flux. This is unphysical, the isoflux is the second largest term in the global atmospheric $^{13}CO_2$ budget ().

This apparent problem is resolved in Eq. (8) which shows that the $\delta^{13}CO_2$ values that enter the inversion are observations treated by "presubtracting" the contributions of fossil fuel, the ocean flux, terrestrial flux and biomass burning. Since the ocean and terrestrial terms are taken from BEPS and OPA-PISCES-T the effects of the isoflux term are implicitly included in the inversion, at least from the prior estimates. I presume the CO_2 observations are treated the same way although I didn't notice this mentioned in the text. A corollary of this presubtraction treatment is that the prior values of the unknowns f are zero, again I did not see this mentioned. If I have understood up to

this point then I agree with the treatment of the budget using the prior fluxes.

I don't agree with the calculation of the effective discrimination in Eqs (6) and (7). There will certainly be a $^{13}CO_2$ flux associated with the CO_2 flux and we can associate an effective discrimination with it. The ratio, however, includes the isoflux which doesn't seem right. I would recommend regressing the $^{13}CO_2$ flux against the CO_2 flux to calculate the discrimination.

It seems likely that the values of f produced by the inversion are small. Certainly the global mean value is small as indicated by the changes in the global mean flux. This is one potential reason why the different cases for calculating the fractionation produce relatively similar values since the different effective discriminations only enter the perturbation around the prior flux. If that perturbation is small then the differences among different formulations will likely be small.

This brings me to my last and deepest concern about the paper. The authors present a number of cases in which the value and spatial structure of the discrimination and disequilibrium are altered. They present these as changes in terms in Eqs. (6) and (7). In my view that is not likely to be the point of greatest sensitivity in the inversion to these values. that is likely to come from eq. (8). the authors don't mention whether any changes are made here. It seems unlikely to me since the disequilibrium and discrimination are emergent properties rather than inputs to BEPS and OPA-PISCES-T. If we changed these models in such a way that the isoflux was changed we would produce changes (probably large ones) in the $\delta^{13}C$ values used in the inversion.

In summary I believe that the low sensitivity to discrimination and disequilibrium found by the authors is an artifact of their setup. The formulation of the budget as a perturbation (with a good prior CO_2 flux) means the changes they make are unlikely to have a large effect. More importantly I believe they have neglected an aspect where a large impact might be expected. I stress again that I am having to make a lot of assumptions about what the authors have or have not done. If I understand correctly I believe they

do not have a general result. If I am misunderstanding they need to explain their setup more carefully.

2 Minor Points

Most of the following are suggestions for extra information needed in the paper.

- Although the paper uses a great deal of data, relatively little of it comes directly from measurements. It would be good to quote how many of the monthly values come from measurements made during that month.

- The χ^2 test is a good start but it is also interesting to ask whether the δ^{13}C measurements are being matched better or worse than the CO_2. the algorithm of () as used by () cann provide this.

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