

## Response to Reviewers' Comments

Jing Chen, Gang Mo and Deng Feng

Reviewer 1:

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The revision has addressed all my comments, and I recommend it be accepted for publication, after minor corrections:

**Answer: Many thanks. Your very careful review is much appreciated.**

1. more explanations on why in table 3, the only CO<sub>2</sub> inversion has smaller uncertainty than CASE 1 to 5.

**Answer: It was indeed not clearly discussed. The first part of the third in Discussion is modified as (Lines 785-794):** “After adding <sup>13</sup>CO<sub>2</sub> data to the inversion system, the uncertainty in the inverted CO<sub>2</sub> flux increased from 0.84 to 0.93 PgC y<sup>-1</sup> for land and from 0.40 to 0.49 PgC y<sup>-1</sup> for ocean (Table 3, difference between the CO<sub>2</sub>-only case and Case I), i.e. 11% and 23% increases in uncertainty for land and ocean, respectively. The relative error in preprocessed <sup>13</sup>CO<sub>2</sub> measurements used in the joint inversion is considerably larger than that in CO<sub>2</sub> measurements, causing these increases in the uncertainty of jointly inverted CO<sub>2</sub> fluxes from the CO<sub>2</sub>-only case. The <sup>13</sup>CO<sub>2</sub> measurements were preprocessed before the inversion as the remaining concentration after removing the contributions of fossil fuel emission and prior land and ocean discrimination and disequilibrium fluxes (Eq. 10), and therefore they contain uncertainties from these contributions in addition to measurement uncertainties.”

2. Line 1201, Page 51: change '13C' to '\$^{13}\$C'

**Answer: corrected.**

3. Line 1295, Page 65 (Figure 13 caption): Change 'green' to 'blue'

**Answer: corrected.**

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I believe the authors have done a satisfactory job of addressing the reviewers' comments and that the paper should now be published.

**Answer: Many thanks for high-level intellectual input during the various stages of the review process. Your constructive comments have led to several important refinements of our joint inversion system.**

I agree that the question of using the dilution effect in the Jacobian for C13 observations remains a matter of debate. Since this forum is archived and accessible it seems a good place to continue the debate so I will respond to the authors' response:

First I will say that we debated long and hard about using this term in the Rayner et al. (1999) study. I am still cautious rather than confident about our conclusion. The choice depends on what is included in the target variables and the prior estimates. For Rayner et al. (1999) we only included long-term means and trends in the isoflux. Thus there was no scope for the isoflux to respond to short-term variations. This is not physically correct; if a net flux changes the disequilibrium of the atmosphere then the isoflux will respond. We needed some way to include this effect and the best way we could think of (given the target variables at our disposal) was to modulate the C13 response to a net flux.

The case for this paper is different but, I think, not completely different. Short-term variations in the isoflux for the prior are captured by the process models, provided that these models are forced with actual C13 atmospheric records. This effect is accounted for by the modification of the observations used in the inversion. However as soon as one varies a flux from the prior this response is no longer correct since the calculated isoflux pertains only to the prior estimate. Thus I still believe the dilution should be applied to variations from the prior fluxes. I'm also quite prepared to be wrong and don't think this question should hold up this paper further. I believe the authors have done a satisfactory job of addressing the reviewers' comments and that the paper should now be published.

**Answer: Thanks for these additional comments, which provide further insight on the need to do this adjustment to the Jacobian matrix. This is indeed a technical issue that deserves more attention in inversion systems. No method is perfect, but we are all seeking the best method. In our case, we used the global mean 13C observation record for the prior 13C flux calculation which should have captured its first order spatio-temporal variability. The short-term variations in the isoflux are therefore considered to the first order. We agree that the responses of 13C concentration at various stations from a flux pulse from a region would decay faster with consideration of the uptake of the flux in downwind regions than the case without this consideration. The adjustment proposed by Rayner et al. (1999) addresses this time-decay issue effectively. However, in our view, the exchange of 13C with downwind surfaces may be considered as additional pulses that are automatically considered in the inversion system. These additional pulses are all included in the time sequence contained the Jacobian matrix so that the observed 13C concentration at a given station is considered to be the realization of the responses to the various individual pulses treated as independent of each other. This independence could be an assumption that causes errors, but the errors are unlikely to be unidirectional to cause a faster decay. We are not entirely sure about this neither, and this issue indeed deserves some serious attention as the same argument can be applied to CO2-only inversions by various groups, who have so far not included this type of adjustments.**