

***Interactive comment on* “Spatial resolution of tropical terrestrial CO₂ fluxes inferred using space-borne column CO₂ sampled in different earth orbits: the role of spatial error correlations” by P. I. Palmer et al.**

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We thank two anonymous reviewers (paraphrased in italics) for providing comments.

Anonymous Referee #1

Section 2, page 3255, line 6 and following: I wonder if the statement that glint observations are better than nadir observations... I highly recommend to rephrase the bold statement that glint only observations are more effective.

We have revised this statement to reflect this comment (see below) and have added a statement about sub-visible cirrus in response to another comment by this reviewer.

Previous work has showed that the glint measurements of tropical continental outflow over the oceans, if retrieved with sufficient accuracy, should be more effective at inferring tropical CO₂ fluxes than nadir measurements over land which are subject to seasonal clouds and biomass burning aerosol (Feng et al., 2009, Baker et al., 2010).

Section 3, page 3257, line 4 and following (and Appendix A). Critical for this study is the identification of cloud free scenes. To what extent have (very) thin cirrus (including sub-visible cirrus) been considered? Please add information on this important aspect.

It is true we have not explicitly considered these (very) thin cirrus clouds in our calculations – MODIS data that we use to filter for clouds does not have the sensitivity to identify them. However, if these clouds are thin enough XCO₂ values can be retrieved from the observations. Thin, high clouds are expected to introduce a clear spectral signature (i.e., attributable to the clouds) which can be used to fit for such clouds in the retrieval. If we assume for the OSSE that this is the case, the only weakness is that we have potentially underestimates the XCO₂ errors associated with the spectral fitting.

Caption, Fig 2: Typo fixed.

Anonymous Referee #2

p 3152, 1 14-19. The first sentence (“For a specified spatial resolution...”) tells of a “disproportionately small decrease in flux uncertainty.

The statement is correct as read. As the number of observations increases beyond a critical value the resulting posterior flux uncertainty decreases but with ever smaller decrements.

Same page and lines...’Confirm’ would be appropriate...

We now “show” rather than “confirm” and have added “as expected” to convey the idea

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that this result is not new.

P 3154, I 12-14.

Changed as described above.

P 3258, I 8.

We use a temporal window of 1 day and make that clear in the revised text.

P 3258, I 10-11

This is a mistake. What we meant to say was that given that a large number of measurements (i.e., $1/n$ tends to 0) model error provides the dominant contribution to the overall error budget the role of alpha is inconsequential. We have clarified this point in the text.

P 3258, I 12-13

Assigning a standard deviation of 2.0 and 1.5 ppm to characterize the ability of a model to simulate the CO2 column contradicts actual model-data comparisons. See Reuter et al.

Reuter et al compare SCIAMACHY XCO2 retrievals with TCCON FTS measurements and the Carbontracker transport model. Table 1 from this paper shows that SCIAMACHY vs the model has a typical mean bias between +/- 0.5 ppm but with a standard deviation of close to 3.0 ppm which is larger than the continental value we assume in our paper. However, I also notice from this Table that that the model is typically within 0.1 +/- 1.0 ppm of the FTS data. Assuming that the TCCON data is less noisy and closer to the truth than SCIAMACHY (also true for any other space-borne instrument) our value of 2.0 ppm for the model error is a sensible value.

P 3258 I 17-18

Model error should not dominate the observation error budget, at least for single sound-

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ing statistics. The situation may be less favourable for the model when looking at temporal or spatial averages of the observations.

The reviewer is corrected that our statement about model error dominating the error budget refers to the situation where we have averaged the measurements over a day. We have clarified this point in the revised paper.

P 3238 | 21-22

The temporal window at which the spatial correlation applies should be given..

The temporal window is 1 day. This point has been clarified in the main text.

P 3259, | 28

The fact that random errors are well below 1 ppm means that OCO-2 measurements will be better than current TCCON data....This is not realistic.

We agree that this is not realistic for single soundings. The random errors we present represent daily values and over subcontinental spatial scales.

P 3260, | 12-14

The explanation given in the sentence starting with “we find” is obscure to me.

Including spatial error correlations penalises high-spatial-density measurement strategies. GOSAT has a sparser measurement strategy than OCO so the error correlations penalises OCO more than GOSAT over some regions.

P 3261, | 8

“Less” would be more appropriate than “no significantly”. At the 0.5 level, the prior still plays a large role.

Agreed. Revised text changed accordingly.

P 3261, | 21-22

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The authors acknowledge a pending issue about spatial error correlations without addressing it. This is surprising given the topic of the paper.

The reviewer is referring to the correlations between instruments in different orbits. This is non-trivial and would involve including temporal and spatial error correlations which is beyond the scope of this paper but is the subject of ongoing studies.

Atmospheric mixing will likely prevent inferring some diurnal cycle of the surface fluxes from that of the column [sic].

An atmospheric column is already a quantity that has been integrated over time and source type/origin so that a column measured at 1pm local time, for instance, does not necessarily reflect fluxes from that time and location. With respect we believe there is scope to extract some diurnal information out of these data, even if it is night and day over a month. The statement already includes the caveat “in theory” so that a reader will not automatically assume it is possible. However, we have added “subject to dilution due to atmospheric mixing processes”.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 3251, 2011.

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