

[Interactive
Comment](#)

Interactive comment on “Carbon balance of China constrained by CONTRAIL aircraft CO₂ measurements” by F. Jiang et al.

Anonymous Referee #1

Received and published: 30 April 2014

1 General

The authors describe two new atmospheric inversions with a focus over China: one that assimilates Globalview data only and one that includes aircraft measurements as well. The paper does not really innovate but could eventually be a useful piece of information in the estimation of the carbon budget of China. Maybe because it does not use a new method compared to previous papers, this one hardly describes and justifies its inversion set-up, so that the reader is left wondering what has actually been done and why. The analysis of the results relies on various resources but the discussion remains superficial at places. If the paper can be improved at these two ends with convincing arguments, it would be worth publishing, but as the paper stands, I find it

C1966

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



difficult to evaluate the study.

2 Specific comments

p. 7685, l. 17: “because the atmospheric inversion is highly depended on the atmospheric CO₂ measurements” is an evidence, by construction of the atmospheric inversion systems.

p. 7685, l. 19: the space-time scale to which this 10% applies should be given.

p. 7685, l. 21: Eastern Europe is hardly observed by atmospheric measurements.

p. 7686, l. 18: the authors should indicate the temporal resolution of their inversion increments (is it monthly?).

p. 7686, l. 23: TM5 exists in various flavours and the resolution of this one should be given (horizontal grid and number of vertical levels).

p. 7687, l. 2: The authors should explain how they use their prior hourly fluxes in their monthly(?) flux inversion.

Figure 2: a second image that would present the 13 regions of China only should be included. With the only one presented, the reader can hardly distinguish some of the tiling there and get a feeling of the size of each tile.

p. 7687, l. 6: The authors should define their uncertainty measure. If it is the standard deviation, I note that over land it is smaller than the actual bias of the prior fluxes (fossil fuel + biomass burning regrowth), which may damp the increments. In this case, the authors should justify their choice. Further the authors should describe their full prior error settings, not just the uncertainty of the global land-sea totals. How do they assign

error variances to each individual prior flux in their state vector, and error covariances between these individual prior fluxes? If a diagonal matrix is used, this should be justified.

p. 7687, l. 24: later, the authors explain that measurements are excluded below 2 km, so what is the role of the first two layers?

p. 7687, Section 2.2: this section does not give any clue about the observation errors that have been associated to the aircraft data. This is all the more surprising that the problem is particularly complex. These errors combine the errors in the measurements, the error of the binned measurements to represent the large boxes, the error of the smoothing, the aggregation error (caused by the very coarse tiling outside China) and the transport model error. The third term induces medium correlations. The last two terms induce strong correlations. All hypotheses should be made explicit and justified.

p. 7687, l. 27: it is not clear whether the 10° box also applies to the vertical profiles. If this is the case, the error of the measurements to represent the box may be large, so that the measurements should be given a very small weight in the inversion. The authors should comment on this.

Figure 4: the axis on the right shows numbers varying between 0.180 and 0.198. Spurious digits should be removed, i.e. the last two. Actually, the numbers do not vary enough to keep the bars on the plot.

p. 7689, l. 11: a change in posterior uncertainty of 4.3 TgC/yr over China is not meaningful at all. I actually conclude that the measurements reduce the uncertainty only marginally, in contrast to what is written elsewhere.

p. 7689, l. 13: ‘that’ should replace ‘those’.

p. 7689, l. 26-end: The term ‘correlation’ should not be used for just 5 points. The

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

apparent correlation could be spurious and is therefore not statistically significant.

p. 7690, l. 1: Here 'correlation' is applied to 3 points only (I do not count 2005 since the CT data only start late 2005), which is even less credible. Note that the 2002-2005 GV period cannot be concatenated with the 2006-2008 GVCT period in the statistics to study the impact of CT, because the CT time series only starts late 2005.

p. 7690, l. 15: the statement about the uncertainties is not correct.

Figure 6: the fluxes are normalized by the surface which is not convenient. A unit in TgC/yr would be more appropriate. For the uncertainty, the unit is very ambiguous since the inversion system estimates regional fluxes. The implicit conversion of the regional error budget from a std. X TgC/yr to a std. Y TgC/yr/m² requires a hypothesis on spatial error correlations that is not given. For instance, simply dividing X by the surface would clearly underestimate the uncertainty of a m²-scale flux.

p. 7691, l. 3-4: the authors should define the reference for the percent. It could be a % on the uncertainty reduction or a % on the prior fluxes, with very different implications.

p. 7693, l. 13-14: the CT actually do not not reduce uncertainties significantly.

p. 7693, l. 16: if tropical convection is so important in the results, these ones are likely flawed by the lack of robustness of convection schemes.

Fig. 9: why are the measurements reported in ppmv, ie by volume?

Fig. 9: The CT data do not change the concentrations in boreal autumn and winter at all, which is very suspicious given (i) that prior respiration fluxes are unlikely to be perfect, (ii) that there is a notable concentration offset in this period and (ii) that changing summer fluxes affects concentrations in the following autumn and winter. About (i), could it be that the prior flux errors are too small in autumn and winter? This

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

would be problematic when discussing annual budgets and seasonal amplitude, ie for all results shown. But this would still not explain (ii) and (iii).

p. 7694, l. 19-20: this statement is too strong.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7683, 2014.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

