We would like to thank the anonymous referee 1# for his/her comprehensive review and detailed suggestions. These suggestions help us to present our results more clearly.

Referee: 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 difficult to evaluate the study.

Response: We agree with the assessment that the description of methodology and the discussion of results can be considerably improved. Some of the improvements can be made by addressing your specific comments below, and further improvement can also be made by careful revision of the text throughout.

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.

Response: Thanks for this comment. We will remove this redundant sentence and rewrite that sentence in the revised paper.

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

Response: Thanks for this comment. We will give the space-time scale in the revised paper.

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

Response: Thanks for this comment. Yes, there are also very few CO_2 measurements in Eastern Europe. However, in our study, Europe is treated as one region, and as a whole, there are much more observations in Europe and North America than in China.

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

Response: Thanks for this suggestion. Yes, it is monthly. We will add this information in the revised paper.

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).

Response: Thanks for this suggestion. We will give more details for the inversion setup in the revised paper, including the grid setup of the TM5 model.

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

Response: Thanks for this suggestion. The hourly prior fluxes are averaged to monthly before being used in the inversion. This information will be added in the revised paper.

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.

Response: Thanks for this suggestion. We will plot a second image that focuses on China.

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.

Response: Thanks for these suggestions. The uncertainties of global terrestrial ecosystem carbon fluxes used in previous inversion studies are ranged from 0.6 ~ 6.0 PgC yr^{-1}, while those of global ocean fluxes are ranged from 0.26~2.5 PgC yr^{-1} (Gurney, et al., 2002;

Houweling et al., 2004; Baker et al., 2006; Piao et al., 2009; Nassar et al., 2011; Deng and Chen, 2011). The uncertainties used in this study are in the range of those used in previous studies, and the same as Deng and Chen (2011). That is because we use the same land and ocean prior fluxes with Deng and Chen (2011). Deng and Chen (2011) did χ^2 test for their selections, and results indicated that these uncertainties are reasonable. It should be noted that we made a mistake for the global ocean uncertainty in the paper. We use an uncertainty of 0.67 PgC yr^{-1}, not 0.88 PgC yr^{-1}. The uncertainty on the land is spatially distributed based on the annual NPP distribution simulated by BEPS, while the one on the ocean is distributed according to the area of each ocean region. We do not consider the relationship among different regions. Hence, a diagonal matrix for error variances was used. That is because the global land was separated into a series of regions mainly according to land cover types, and we assume that the relationship of the fluxes of different land cover types could be negligible.

We will give a full prior error setting in the revised paper.

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?

Response: Thanks for this comment. The data of the first two layers are not used in this study. After we got the CONTRAIL data, firstly, we divided the vertical measurements into five layers, which was similar with Niwa et al. (2012). Then, we checked the combined data of each layer, and found that the measurements of the first two layers were highly affected by local emissions, mainly from frequent aircraft ascending and descending near the airports. Hence, we did not use these data during the inversion.

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.

Response: Thanks for this comment. Yes, this is very important, since the observation errors affect the final results. And we should give the observation errors and the details about how we calculated these errors. We will add details about these error calculations in the revised paper.

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.

Response: Thanks for this comment. The 10^ox10^o boxes are only applied to the level flight. We will make it clear in the revised paper.

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.

Response: Thanks for this comment. The prior uncertainty of terrestrial ecosystem carbon fluxes in China is 0.257 PgC yr^{-1}, and the posterior uncertainty of Case GV is about 0.195 PgC yr^{-1}, with 24% reduction; and when the CONTRAIL data is added (Case GVCT), the posterior uncertainty is 0.183 PgC yr^{-1} in 2007 and 2008, with 29% reduction. In our another study (Jiang et al., 2013), we added flask measurements (measured during 2006 to 2009) of three additional surface observation sites (LFS, Northeast China; SDZ, North China; LAN, East China) in the same inversion system, and showed that the posterior uncertainty was 0.173 PgC yr^{-1} in 2007 and 2008, with 33% reduction. These indicate that the CONTRAIL data have contributed to the reduction of posterior uncertainty, though the impact is limited. We think that this uncertainty reduction by adding the CONTRAIL data is reasonable in comparison with the uncertainty reduction caused by adding the three Chinese sites and in consideration of the fact that there are no CONTRAIL data in China: all data were measured in the downwind or upwind of China and only data above the boundary layer is used in the inversion.

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.

Response: Thanks for this comment. The change in posterior uncertainty of 4.3 TgC/yr over China is averaged from 2002 to 2008. Since the CONTRAIL data are from 2006 to 2009, the posterior uncertainty reductions caused by CONTRAIL data mainly occur from 2006 to 2009 (see Figure 4, the situation in 2009 is not shown in this study). In 2007 and 2008, the reduction of posterior uncertainties is about 12 TgC yr^{-1}, which account 6 % of the posterior uncertainty of Case GV. As shown in the response to the previous comment, we think that this uncertainty reduction is reasonable and meaningful. However, this impact is indeed limited, and we will revise the manuscript to make appropriate comments on the role of CONTRAIL data on the posterior error reductions.

p. 7689, l. 13: 'that' should replace 'those'.

Response: Thanks for this correction. We will modify 'those' to 'that' in the revised paper.

p. 7689, l. 26-end: The term 'correlation' should not be used for just 5 points. The apparent correlation could be spurious and is therefore not statistically significant.

Response: Thanks for this comment. We agree with the reviewer's standpoint, the number of points is too few to obtain a reliable correlation. We will rewrite this paragraph to weaken the 'correlation' statement and strengthen the discussion.

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.

Response: Thanks for this comment. We will rewrite this paragraph to weaken the 'correlation' statement and strengthen the discussion.

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.

Response: Thanks for these suggestions. The unit of the uncertainty is percent (%). We forgot to give this unit on the image. And we did not convert the regional error budget from a std. X TgC/yr to a std. Y TgC/yr/m2, we only simply use the regional posterior error reduction. In addition, for the flux images, we will replot those and use the unit of TgC/yr.

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.

Response: Thanks for this suggestion. The uncertainty reduction is calculated as follow:

a $\%=(error_{post}-e_{prior})*100/e_{prior}$

We will add this definition in the revise paper.

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

Response: Thanks for this comment. The uncertainty reduction is more than 10% in Southeast Asia.

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.

Response: Thanks for this comment. We will add the description of convection schemes in the inversion setup section in the revised paper.

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

Response: Thanks for this comment. We will change the unit of 'ppmv' to 'ppm' in the revised paper.

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

Response: Thanks for these comments.

(i) Because the monthly prior uncertainties are assigned according to the variations of monthly NPP, the prior flux errors are very small in autumn and winter in this study. Hence, the change of carbon flux caused by CONTAIL data mainly occur in summertime (see Figure 7a), leading to large changes of simulated concentrations in summer and small changes in autumn and winter.

(ii) The notable gaps between simulated and observed concentrations in autumn and winter are explainable. The first reason is that we do not assimilate the data of these three sites in the inversion system, and the second is that the prior flux errors are set too small during this period.

(iii) The changes of summer fluxes do not affect the simulated concentrations in the following autumn and winter. That is because the air masses are in movement , and the increases of land sinks in China in summer could not directly affect the local CO_2 concentrations in the following seasons.

Overall, we agree with the referee's viewpoint, the small prior flux errors in autumn and winter would be problematic when we discuss the annual budget and seasonal amplitude. In the manuscript revision, we will investigate the sensitivity of the influence of prior flux uncertainty in autumn and winter on the inverted flux over China.

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

Response: Thanks for this comment. We will rewrite that sentence and weaken that statement in the revised paper.