Dear Editor,

According to the reviewers' comments and suggestions, we have made major revision to our manuscript. The main changes are as follows:

1) We give more details about the inversion system, including the set-up of transport model, the settings of prior fluxes uncertainties, the settings of model-data mismatch errors, and so on.
2) We give more details about the use of CONTRAIL data.
3) We rewrite the paragraph in section 3.1, which is about the relationship between land sinks and climate factors in China.
4) According to the suggestion by reviewer #1 i.e. the monthly prior error setting may have impact on the inverted carbon budget in China, we added a new section (section 3.4) and a new figure (Figure 10) to present the sensitivity study on this issue in the revised paper.

The point-by-point response to the reviews and the detailed changes are listed in the attachments.

Great thanks to you and the referees for the time and effort you expend on this paper.

Best Regards,

Sincerely yours,

Fei Jiang

## Referee #1:

**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. We have carefully revised our manuscript according to these comments.

**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 have removed that redundant sentence and rewrite that sentence in the revised paper, as follow:

"…One of the main reasons may be attributed to the lack of enough $CO_2$ concentration observations. Jiang et al. (2013) pointed out that due to lack of sufficient observations…"

For details, please refer to lines 48-49, page 2.

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

**Response:** Thanks for this comment. The uncertainty reduction of 10% is in annual and regional scale.

That sentence has been revised as follow:

"…Jiang et al. (2013) pointed out that due to lack of sufficient observations, most regions of the country have a low regional and annual uncertainty reduction percentage (< 10%) by way of atmospheric inversion, especially for South and Southwest China…"

For details, please refer to lines 49-50, page 2.

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 have add this information in the revised paper, as follow:

"In this study, a nested inversion system (Jiang et al., 2013) based on the Bayesian synthesis inversion method (Rayner et al., 1999; Enting et al., 1989) is used to improve the estimations of monthly CO2 sources and sinks as well as their uncertainties…"

For details, please refer to lines 77-79, page 3.

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 have the grid setup of the TM5 model in the revised paper, as follow:

"… In this study, TM5 model is driven by the ECMWF outputs, and is run at a horizontal resolution of 3° × 2° around the world without nesting a high-resolution domain and a vertical structure of 25 layers, with the model top at about 1 hPa."

For details, please refer to lines 96-99, page 4.

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. We have added this information in the revised paper, as follow:

"…Hourly terrestrial ecosystem carbon exchanges simulated by the BEPS model (Chen et al., 1999; Ju et al., 2006) and daily carbon fluxes across the air-water interface calculated by the OPA-PISCES-T model (Buitenhuis et al., 2006) are considered as prior fluxes. Before being used in the inversion, both these two type of fluxes are averaged to monthly. In addition, the monthly terrestrial carbon fluxes of each grid are neutralized annually…"

For details, please refer to lines 102-104, page 4.

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 have plotted a second image as follow.

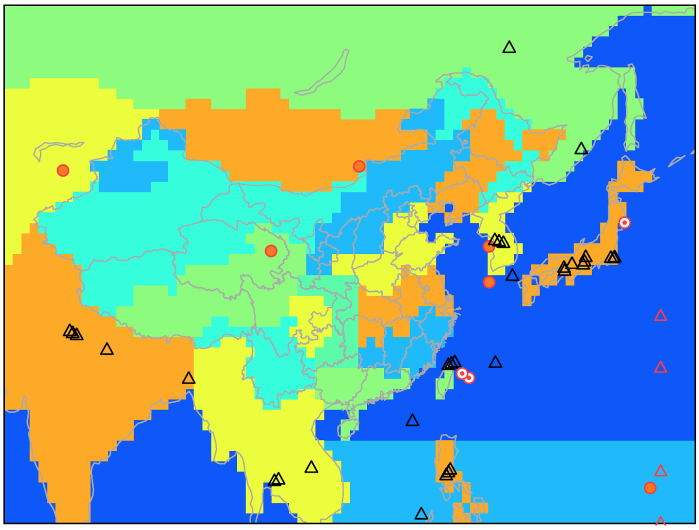


Figure R1

For details, please refer to page 23.

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 $\chi^2$ 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 have revised the manuscript as follow:

"… The 1σ uncertainties for the prior fluxes over the global land and ocean surfaces are assumed to be 2.0 and 0.67 PgC $yr^{-1}$, respectively, which are the same as those used in 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 $\chi^2$ test for their selections, and results indicated that these uncertainties are reasonable. 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. The monthly uncertainties for the terrestrial regions are assigned according to the variations of monthly NPP, while the ones for oceanic regions are assumed to be even. We do not consider the relationship among different regions. Hence, a diagonal matrix for error variances is used. That is because the global land is 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."

For details, please refer to lines 104-116, page 4.

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. We have revised the manuscript as follow:

"… 3) the variations of the monthly $CO_2$ concentrations for each site are carefully checked, and we find that in the boundary layer (below 2000 m), the concentrations at most sites are highly affected by local emissions, probably emitted by frequent aircraft ascending and descending, hence, only data measured above 2000 m are used. Niwa et al. (2012) also only used the free tropospheric (above 625 hpa) data."

For details, please refer to lines 151-156, page 5.

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. We has revised the manuscript as follows:

"…The model-data mismatch error in ppm is defined using the following function, which is similar to those used by Peters et al. (2005) and Deng and Chen (2011).

$$R = \sigma_{const}^2 + GVsd^2$$

where $GVsd$ reflects the observation error, for the GV data, it is the standard deviation of the residual distribution in the average monthly variability (var) file of GLOBALVIEW-$CO_2$ 2010. The constant portion $\sigma_{const}$ reflects the simulation error, which varies with station type that is because the transport models generally have different performances at different observation stations. Except for some difficult stations, the observation sites are divided into 5 categories. The categories (respective value in ppm) are: Antarctic sites/oceanic flask and continuous sites (0.30), ship and tower sites (1.0), mountain sites (1.5), aircraft samples (0.5), and land flask/continuous sites (0.75). The value of 3.5 is used for the difficult sites (e.g., abp_01D0, bkt_01D0). "

"… The model-data mismatch errors of the CONTRAIL data are calculated using the same method as those of the GV data. The observation error ($GV_{sd}$) is the standard deviation of the residual after smoothing, and for the item of $\sigma_{const}$, considering the large range (10° × 10°) of the level flight sites and the thick layer of the vertical sites, we use a constant of 0.75 ppm, which is larger than that of GV aircraft samples (0.5 ppm)."

For details, please refer to lines 120-132, page 5; lines 162-167, page 6.

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 $10^o$x$10^o$ boxes are only applied to the level flight. We have made it clear in the manuscript, and we also added the setting of model-data mismatch error in the revised paper, as follow:

"…It should be noted that the 10° × 10° boxes are only applied to the level flight."

"…The model-data mismatch errors of the CONTRAIL data are calculated using the same method as those of the GV data. The observation error ($GVsd$) is the standard deviation of the residual after smoothing, and for the item of $\sigma_{const}$, considering the large range (10° × 10°) of the level flight sites and the thick layer of the vertical sites, we use a constant of 0.75 ppm, which is larger than that of GV aircraft samples (0.5 ppm)."

For details, please refer to lines 148-149, lines 162-167, page 6.

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. We have revised the manuscript as follow:

"… The CONTRAIL CO2 also has an impact on the posterior uncertainties from 2005 to 2008, especially in 2007 and 2008. The posterior uncertainties in 2007 and 2008 are reduced from around 0.195 to 0.183 PgC $yr^{-1}$, with a reduction rate of 6%. This reduction rate is defined as $(Uncertainty_{posterior,GVCT} - Uncertainty_{posterior,GV})\times 100/Uncertainty_{posterior,GV}$ (same thereafter). In our another study (Jiang et al., 2013), we added $CO_2$ measurements of the three Chinese sites in the same inversion system, and results showed that the posterior uncertainties were reduced from around 0.195 to 0.173 PgC $yr^{-1}$ (~11%) in 2007 and 2008. Hence, We think that this uncertainty reduction caused 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 are used in the inversion. Overall, when…"

For details, please refer to lines 193-204, page 7.

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 have revised the manuscript to make appropriate comments on the role of CONTRAIL data on the posterior error reductions.

1) Line 193, page 7: we have changed the "has an obvious impact on the posterior uncertainties" to " has an impact on the posterior uncertainties".
2) Lines 243 -244, page 9: we have changed the "has large impacts on the inverted carbon fluxes and uncertainties during the 2006 – 2008 periods" to "has a large impact on the inverted carbon fluxes and a certain impact on the uncertainties during the 2006 – 2008 period".

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

**Response:** Thanks for this correction. We have rewritten that paragraph, and the word of 'those' do not appear in the new sentence.

For details, please refer to line 207, page 7.

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 have rewritten this paragraph to weaken the 'correlation' statement and strengthen the discussion, as follow:

"Climate factors such as temperature, precipitation and radiation could affect plant growth (Zhu et al., 2007; Myoung et al., 2013), thereby the IAVs of land sinks (Ciais et al., 2005). Generally, a warmer condition advances vegetation growth for most regions in middle and high latitudes of the Northern Hemisphere (NH), including the crops in Europe and US, and the forests in central Siberia, west Canada and northeast China (Myong et al., 2013). Zhu et al. (2007) also showed that in northern China, the plant growth was temperature-limited. Usually, more vigorous vegetation growth corresponds to more carbon uptake. Studies on the relationships between the net ecosystem exchange (NEE) measured by eddy covariance equipment and the environmental factors confirm that the IAVs of annual mean air temperature is significantly related to the ones of NEE in the forest regions in middle and high latitudes of the NH: Yuan et al. (2009) pointed that air temperature was the primary environmental factor that determined the IAV of NEE in deciduous broadleaf forest across the North American sites, and NEE was positively correlated with the mean annual air temperature; Dunn et al. (2007) found that in central Manitoba, Canada, warmer annual temperatures were associated with increased net uptake, while annual precipitation did not explain any of the variability in NEE. Nevertheless, in low latitudes of the NH, for example, southern China, the NEE may be related to solar radiation and precipitation. Zhu et al. (2007) reported that in southern China, the plant growth is radiation-limited. Usually, more radiation corresponds to less cloud cover, so as to less precipitation (Figure 5a). Based on eddy covariance measurements in two forest sites in southern China, Yan et al. (2013) found that the greater annual NEE (more uptake) usually occurred in the dry years and smaller annual NEE in the rainy years for the both forests. Hence, in order to study whether the impact of CONTRAIL data is reasonable, it should be useful to check the relationship between inter-annual variations of climate factors and land sinks in different regions of China. Because the changes of posterior fluxes mainly occur in southern China and northern China (see section 3.2), only these two regions are investigated. Monthly climate data of 484 stations obtained from CMA during the study period are used for the analysis, in which 269 stations are located in southern China, and the others are located in northern China (Figure 3). Figure 5 shows the IAVs of annual mean climate factors anomalies and land sink anomalies in southern and northern China. It could be found that in southern China, the changes caused by CONTRAIL data lead to a better correlation between the fluxes and radiation as well as precipitation, and in northern China, the changes also make the fluxes better correlated with the temperatures. These relationships are consistent with the previous findings, indicating that the IAVs of posterior fluxes in China by additional constraint of CONTRAIL $CO_2$ data could be more reasonable."

For details, please refer to lines 207-241, pages 8-9.

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 have rewritten that paragraph to weaken the 'correlation' statement and strengthen the discussion. Details could be found in the previous response.

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.

1） We have changed the sentence of " CONTRAIL $CO_2$ has large impacts on the inverted carbon fluxes and uncertainties during the 2006 – 2008 periods" to "CONTRAIL $CO_2$ has a large impact on the inverted carbon fluxes and a certain impact on the uncertainties during the 2006 – 2008 period".

   For details, please refer to lines 243-244, page 9.

2） 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. We have added the definition of the error reduction and the unit in Figure 6.

For details, please refer to lines 684-687, page 27.

3) We agree with the reviewer that the unit of TgC $yr^{-1}$ would be more convenient to know the carbon sink of each region. We have tried to plot the distributions of carbon fluxes in the unit of TgC/yr, but we find that it is still not convenient. The main reason is that when the carbon sinks of two or more adjacent areas are in the same range (e.g., -200 ~ -300 TgC $yr^{-1}$), they will have the same color in the image. In this condition, the readers can't distinguish the relative size of carbon sequestration in different regions of the world. As shown in Figure R2, for example, the carbon sinks of different regions in China, Mongolia, West Asia, North Africa, and Middle Africa are in the same range (i.e., 0 ~ -100 TgC $yr^{-1}$), these regions have the same color in the image, thus we still don't know the land sink of one region (e.g., China). We also check the fluxes maps of previous studies (e.g., Niwa et al., 2013; Deng and Chen, 2012; Nassar et al., 2011; Peters et al., 2007; Houweling et al., 2010; Miyazaki et al., 2011), and find that most of these studies use the unit of gC $m^{-2}$ $yr^{-1}$. Therefore, we still use the unit of gC $m^{-2}$ $yr^{-1}$ to plot the surface fluxes distribution.

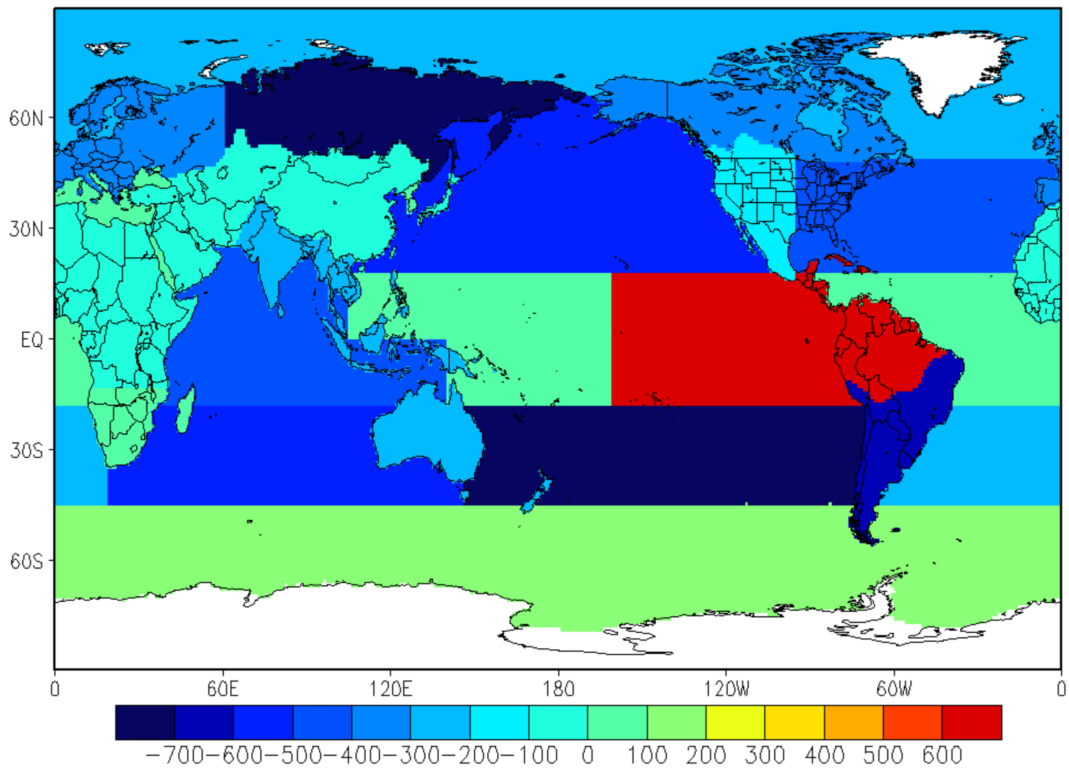



Figure R2 Inverted global carbon flux for Case GV (TgC $yr^{-1}$)

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 rate is calculated as follow:

$(Uncertainty_{posterior,GV} - Uncertainty_{posterior,GVCT})*100/ Uncertainty_{posterior,GV}$

We have added this definition in the revise paper, as follow:

"…The posterior uncertainties in 2007 and 2008 are reduced from around 0.195 to 0.183 PgC $yr^{-1}$, with reduction rate of 6%. This reduction rate is defined as $(Uncertainty_{posterior,GVCT} - Uncertainty_{posterior,GV})*100/ Uncertainty_{posterior,GV}$ (same thereafter). …"

For details, please refer to lines 194-197, page 7.

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

**Response:** Thanks for this comment. We have checked that sentence, and find that there is no description for the significant reduction of uncertainty. The sentence is as follow:

"The main reason of the significant changes in South and Southeast Asia is that there are very few $CO_2$ measurements in the GV dataset in these regions (Fig. 2), so there is an insufficient observational constraint, leading to large uncertainties in the inverted carbon fluxes. The addition of CONTRAIL data reduces uncertainty by markedly increasing observations in these regions."

For details, please refer to lines 318-321, page 11.

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 have added the horizontal and vertical transports and diffusions schemes in the revised paper, as follow:

"… Except for sources and sinks, the key processes of $CO_2$ in the atmosphere are horizontal and vertical transport and diffusion. In the TM5 model, the horizontal transport is based on the slopes advection scheme (Russel and Lerner, 1981), the convection is parameterized according to Tiedtke (1989), and the vertical diffusion near surface layer and in the free troposphere are parameterized using the schemes of Holtslag and Moeng (1991) and Louis (1979), respectively. An evaluation showed that TM5 has very well performs on vertical and horizontal transport (Stephens et al., 2007)."

For details, please refer to lines 90-96, page 4.

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

**Response:** Thanks for this comment. We have changed the unit of 'ppmv' to 'ppm' in the revised paper.

For details, please refer to line 341, line 348, page 12, Figure 8, page 29, and Figure 9, page 30.

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, the second one is that the prior flux errors are set too small during this period, and the third one is the model resolution, since LAN is close to Hangzhou City and SDZ is close to Beijing City.

(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. We have investigated the sensitivity of the influence of prior flux uncertainty in autumn and winter on the inverted flux over China, and added a new section (Section 3.4) in the revised paper. As follow:

"3.4 Sensitivity analysis

It could be found that the changes caused by CONTRAIL data over China, including the inverted monthly carbon sinks (Figure 7a) and the concentrations simulated using the inverted fluxes (Figure 9), mainly occur in the warm seasons. This phenomenon may be attributed to the inversion setup for the monthly prior fluxes uncertainties, i.e., the monthly prior errors for the terrestrial regions are assigned according to the variations of monthly NPP (Section 2.1), which lead to the prior fluxes errors in the cold seasons are very small, especially in the high latitudes areas. Therefore, we conduct two sensitivity experiments, i.e., Case GV_s and Case GVCT_s, in which both variations of monthly NPP and soil respiration (RESP) are considered when we assign the month prior errors.

As shown in Figure 10a, in China, after both considered the NPP and RESP, the monthly prior uncertainties in summer are reduced, while those in cold seasons are enhanced, especially in northern China. These changes cause the land sinks decrease in Northeast China and West China and increase in the other regions of China (not shown). As an aggregate, in southern China, the land sink increases in most years, but the IAVs are the same as before; in northern China, basically, there is no change. Seasonally, in southern China, the land sinks increase in all months, and in northern China, they increase in spring and decrease in autumn. As a whole in China, though the land sink decreases in 2004 and increases in 2007 at a certain level, and little changes occur in the other years, but the IAVs are basically the same as before. Seasonally, more carbon uptake occurs in spring and less happens in autumn (Figure 10b, c), leading to the simulated concentrations decrease in spring and more closer to the observations at all three sites (Figure 10d). However, the gaps between the simulated and the observed concentrations during the cold seasons are still large, especially at LAN and SDZ. Since LAN is close to Hangzhou City, and SDZ is close to Beijing City, these large gaps may be related to the model resolution as well. Overall, the mean land sinks during 2002-2008 in China are 0.187 ± 0.20 and 0.255 ± 0.20 PgC yr$^{-1}$ for Case GV_s and Case GVCT_s, respectively, which are almost the same as the values of -0.194 ± 0.19 and -0.253 ± 0.19 PgC yr$^{-1}$ for Case GV and Case GVCT, indicating that the different settings for monthly prior errors have little impact on the inverted carbon budget in China."

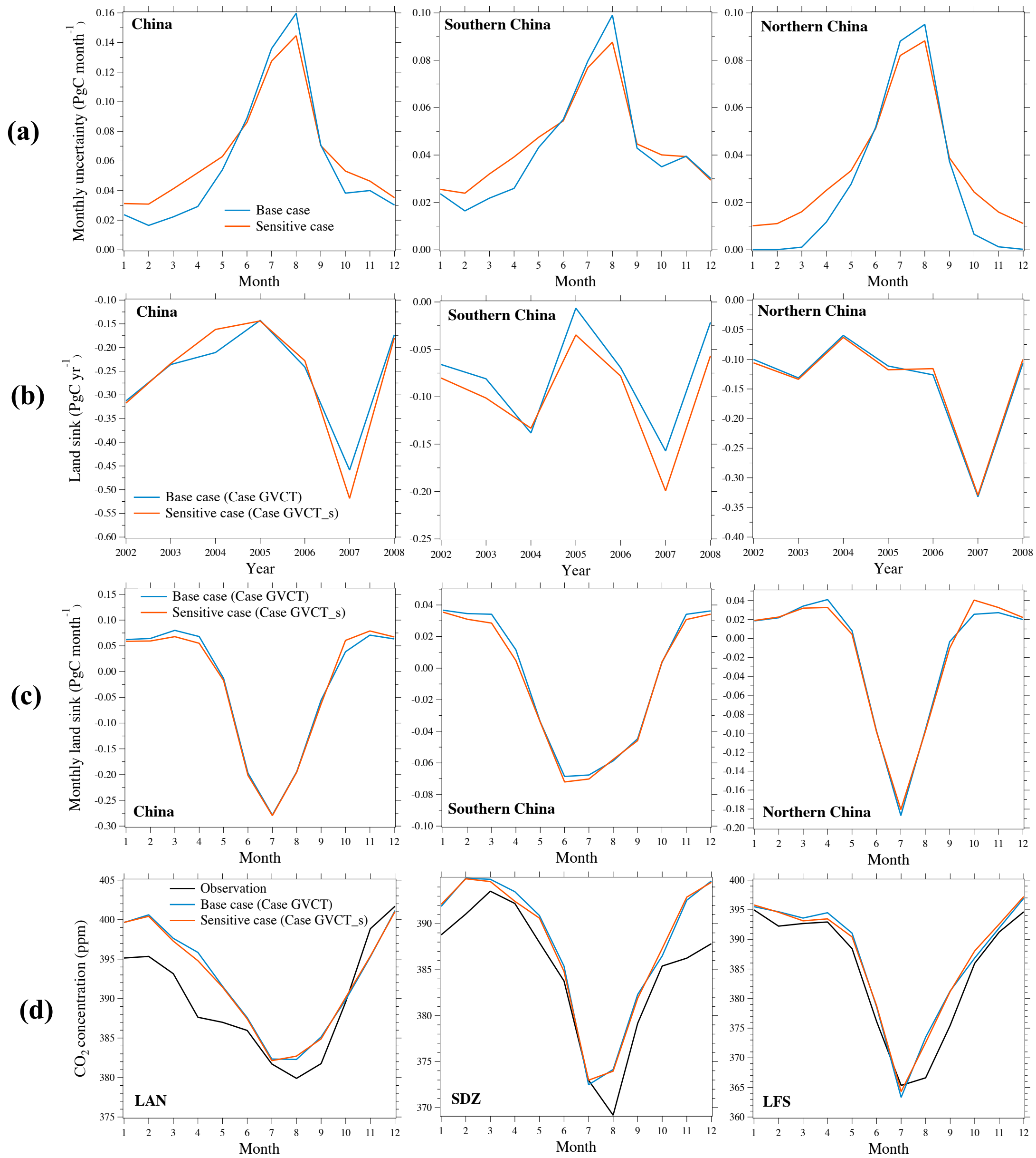



**Figure 10.** The sensitivity of the influence of (a) monthly prior flux uncertainty on the inverted carbon sinks over China, including (b) the inter-annual variations and (c) the

monthly variations, as well as on (d) the simulated $CO_2$ concentrations in 2007 at the three Chinese sites using the inverted carbon fluxes.

For details, please refer to lines 355-383, page 13, lines 710-714, page 31.

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

**Response:** Thanks for this comment. We have rewritten that sentence and weaken that statement in the revised paper, as follow:

"…Therefore, CONTRAIL data may have helped improve the inversion results for China at a certain extent."

For details, please refer to lines 353 – 354, page 12.

## Referee 2#:

**We would like to thank the anonymous referee 2# for his/her comprehensive review and detailed suggestions. These suggestions help us to present our results more clearly.**

**General comments:**

This paper studies the terrestrial carbon fluxes in China using a previously established model setup and different data constraints, which lead to two different inversion products, i.e. GLOBALVIEV-CO2 data based (GV) vs. GLOBALVIEV-CO2 and CONTRAIL data based (GVCT). In addition, CO2 measurements at three observational sites in China are used to evaluate the posterior carbon fluxes in a forward model framework. The authors also try to correlate integrated carbon fluxes in large regions with climate factors such as radiation and temperature in an effort to validate their inversion results. It appears that the inversion results are qualitatively correlated with the climate factors, but is less convincing without a thorough quantitative analysis. On the other hand, the inversion method and the data used in this paper are sound in general, however, critical screening of the data and discussions of their limitations are clearly missing. Furthermore, the reduction in the posterior uncertainty with the added CONTRAIL data is only 2.2%, which does not support the authors' conclusion that "CONTRAIL data have helped improve the inversion results for China". The manuscript needs a major revision before it can be considered publication.

**Response**: Thank for these very valuable and helpful general comments. We have carefully studied these comments and made corrections for the manuscript. The questions listed in these general comments are responded as follows:

1) It appears that the inversion results are qualitatively correlated with the climate factors, but is less convincing without a thorough quantitative analysis.

**Re**: Because our inversion only span 7 years, and the impact of COTNRAIL data on the inverted land sinks only occurs in the last 3 years, it is difficult to give a quantitative analysis. According to the comment of another reviewer, we have rewritten that paragraph to weaken the 'correlation' statement and strengthen the discussion.

For details, please refer to lines 207-241, pages 8-9.

2) the inversion method and the data used in this paper are sound in general, however, critical screening of the data and discussions of their limitations are clearly missing.

**Re**: The criteria of screening for CONTRAIL data have been given in the subsequent response of specific comments. The limitations mainly reflect in the small error reductions, we have given some discussions about this limitation in the revised paper.

For details, please refer to lines 194 – 204, page 7.

3) the reduction in the posterior uncertainty with the added CONTRAIL data is only 2.2%, which does not support the authors' conclusion that "CONTRAIL data have helped improve the inversion results for China"

**Re:** Yes, the impact of CONTRAIL data on the error reduction is very limited. We have modified the conclusion as follow:

"…CONTRAIL data may have helped improve the inversion results for China at a certain extent."

For details, please refer to lines 353-354, page 12.

1) Details on the use of the CONTRAIL data are needed. For example, what are the criteria for screening out polluted profiles near airports? The authors' statement on page 7688, "In view of high pollution near the ground around airports by aircraft emissions, only data measured above 2000m are used" does not give sufficient information for a judgment. Furthermore, how did the authors deal with potential stratospheric influences? Note that stratospheric influences were characterized and filtered out in Sawa et al. 2008 and Niwa et al. 2012.

**Response:** Thank you for these suggestions. We have added some details on the use of the CONTRAIL data.

1) For the criteria of screening out the polluted profiles near airports, we have revised manuscript as follow:

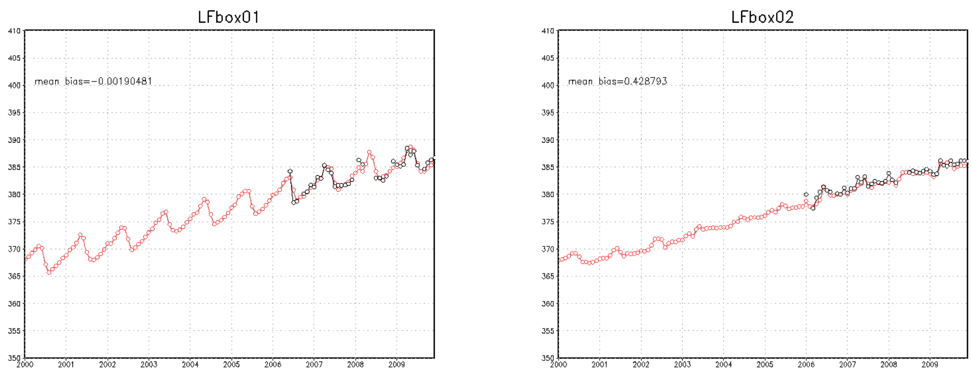
"… 3) the variations of the monthly $CO_2$ concentrations for each site are carefully checked, and we find that in the boundary layer (below 2000 m), the concentrations at most sites are highly affected by local emissions, probably emitted by frequent aircraft ascending and descending, hence, only data measured above 2000 m are used. Niwa et al. (2012) also only used the free tropospheric (above 625 hpa) data.”

For details, please refer to lines 151 – 156, page 6.

2) For the potential stratospheric influences, i.e., the seasonal variation of $CO_2$ in stratosphere is quite different from that in troposphere, we have considered it and found that it is not necessary to distinguish whether the $CO_2$ recodes are in stratosphere or in troposphere. In our study, level flight is considered as flights at heights greater than 8 km. For level flight sites, first, we define 19 regions with ranges of 10 deg x 10 deg according to the airlines (Figure 1a). Then, all the observation records with heights greater than 8 km located in one region are clustered, so that for one site, the concentration is the average of all observation records located in its region, and the 3-D location is the mean of all observation records as well. We have checked the monthly concentrations of level flight sites processed using this method, and found that there are still obvious seasonal variations. Fig. R1 shows the comparisons of observations and simulations for 4 selected level flight sites. It can be seen that the observations could fit well with the simulations. We have revised the manuscript as follow:

“…In addition, in previous studies (Sawa et al., 2008; Niwa et al. 2012), the $CO_2$ recodes in the stratosphere were filtered out, because the seasonal variation of $CO_2$ in stratosphere is quite different from that in troposphere. However, in this study, we don’t distinguish whether the $CO_2$ recodes are in stratosphere or in troposphere. We have checked the monthly observed and forward simulated $CO_2$ concentrations at the level flight sites and thought that the influences of stratospheric $CO_2$ could be neglectful in our inversion system.”

For details, please refer to lines 156-161, page 6.



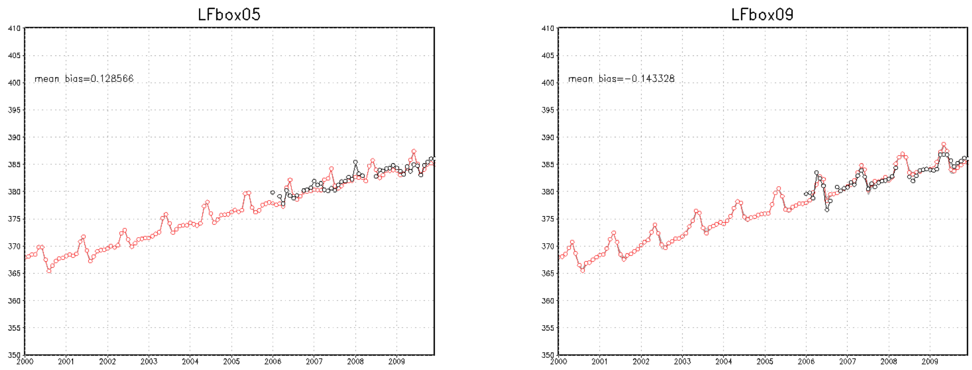



Fig R1. Comparison of observations and simulations for CONTRAIL sites

2) Regarding the CO2 measurements at three stations in China, why were they not assimilated directly in the inversions? The authors discussed the seasonal and spatial variations of the priors and posterior fluxes, and compared them with Niwa et al. 2012. However, there are clear mismatch between the simulations and the observations for all three stations, which seem to point to late ecosystem carbon sinks.

**Response:** Thank you for these comments. Yes, we don't assimilate the three Chinese sites in these inversions. There are two reasons: the first is that these sites have been assimilated in the inversion in one of our previous studies (Jiang et al., 2013), and the second is that we need some independent sites in China to evaluate the changes caused by the use of CONTRAIL data. There are still indeed differences between simulations and observations at these sites because the observations at these sites were not used in the inversion and there are uncertainties in the inverted flux. However, the comparison with the $CO_2$ data at these three sites indicates that the use of CONTRAIL data probably improves the carbon flux inversion over China.

Reference:

Jiang, F., Wang, H. W., Chen, J. M., Zhou, L. X., Ju, W. M., Ding, A. J., Liu, L. X., and Peters, W.: Nested atmospheric inversion for the terrestrial carbon sources and sinks in China, Biogeosciences, 10, 5311-5324, doi:10.5194/bg-10-5311-2013, 2013.

3) How were the data uncertainties prescribed? Especially in the context of surface (GLOBALVIEW) vs. free troposphere (CONTRAIL).

**Response:** The data uncertainties are defined using the following function, which is similar to Peters et al. (2005) and Deng and Chen (2011).

$$R = \sigma_{const}^2 + GVsd^2 \quad (4)$$

where *GVsd* reflects the observation error, for GLOBALVIEW data, it is the standard deviation of the residual distribution in the average monthly variability (var) file of GLOBALVIEW-$CO_2$ 2010, and for CONTRAIL data, it is the standard deviation of the residual item which is output during the smoothing of daily $CO_2$ concentrations using the ccgcrv processing package (Masarie and Tans, 1995). The constant portion $\sigma_{const}$ reflects the simulation error. For GLOBALVIEW data, this portion varies with station type because transport models generally have different performances at different observation stations. Except for some difficult stations, the observation sites are divided into 5 categories. The categories and respective error values are: Antarctic sites/oceanic flask and continuous sites (0.30), ship and tower sites (1.0), mountain sites (1.5), aircraft samples (0.5), and land flask/continuous sites (0.75). The value of 3.5 is used for the difficult sites (e.g., abp_01D0, bkt_01D0). For CONTRAIL data, we use a constant of 0.75.

We have added this information in the revised paper.

For details, please refer to lines 120-132, 162-167, pages 5-6.

A few details:

1) The unit of CO2 concentrations should be ppm, not ppmv

**Response:** Thank you for this correction. We have changed the unit of "ppmv" to "ppm" in the revised paper.

For details, please refer to line 341, line 348, page 12, Figure 8, page 29, and Figure 9, page 30.

2) Page 7684, line 17: the magnitude of the uncertainty reductions needs to be clarified

**Response:** Thank you for this suggestion. We have added the magnitude of the uncertainty reductions in the revised paper.

For details, please refer to line 26, page 1.

3) Page 7688, what periods are the CO2 measurements at the three sites available

**Response:** The $CO_2$ measurements at the three sites are from Jul 2006 – Dec 2009. We have added this information in the revised paper.

For details, please refer to line 170, page 6.