

***Interactive comment on “Profiling tropospheric
CO₂ using the Aura TES and TCCON instruments”
by L. Kuai et al.***

Anonymous Referee #2

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See supplemental PDF

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/5/C2043/2012/amtd-5-C2043-2012-supplement.pdf>

Review of “Profiling Tropospheric CO₂ using the Aura TES and TCCON instruments”
by Kuai et al.

General Comments

This paper represents an honest and apparently successful attempt to measure lower tropospheric CO₂ by using information from both space-based thermal IR measurements of mid-tropospheric CO₂ as well as ground-based measurements of total column CO₂. By comparing the resulting lower tropospheric (surface to 600 mbar) CO₂ to aircraft, the authors show that their results demonstrate real skill in estimating this quantity. However, I have a number of (mostly minor) comments and questions that should be addressed before full publication.

My main general comment is that the authors should clarify upfront that they are NOT measuring CO₂ in the boundary layer. It would be better if they said right upfront, in the abstract as well as the introduction, that they are measuring lower tropospheric CO₂ over a specific pressure range (surface to 600 mbar) with their method, rather than specifically boundary layer CO₂. The boundary layer is typically 0.5 to 1.5 km above the surface, and is only rarely 2 km or more. This represents perhaps 50–200 mbar of pressure difference to the surface, and of course depends explicitly on the exact boundary layer height. For near-sea level locations (ie, much of the world), surface to 600 mbar will include less than a 50% contribution from

the boundary layer. Therefore, saying “boundary layer” CO₂ is somewhat misleading. I suggest “lower tropospheric CO₂” instead, and to be very explicit upfront that this is surface to 600 mbar, and state your reasons right away (rather than in section 5.2) for selecting this particular pressure range (as opposed to, for instance, surface to 1km).

The reviewer’s comment is quite fair. The study was initially set up by using 800 hPa to separate middle-upper troposphere and boundary layer. However, the results turn out to be much more improved by using 600 hPa instead of 800 hPa. We agree to change the word ‘boundary layer CO₂’ to ‘lower tropospheric CO₂’ in the paper.

Secondly, it would be worth mentioning somewhere how this approach is better than directly assimilating both TCCON and CO₂ measurements. It seems that if you are going to assimilate one (TES), to avoid problems of matching up columns, it might be even better to assimilate both TCCON & TES!

Actually, this work shows a basis of assimilating both TCCON & TES. We added following comments in section 6, page 4510 L8.

‘The approach describe in this paper shows that the problem of using independent total column and free tropospheric estimates to estimate lower tropospheric CO₂ is a linear problem. This linear problem is in contrast to using a similar approach for quantifying lower tropospheric ozone (Worden et al., 2007) or CO (Worden et al., 2010) by using reflected sunlight and thermal IR radiances and is likely because ozone and CO can have much larger variance in the lower troposphere than CO₂.

The study shown here indicates that assimilating total column and free tropospheric CO₂ will increase sensitivity to surface fluxes by placing improved constraints on lower tropospheric CO₂. Furthermore, quantifying lower tropospheric CO₂ using total column and free tropospheric estimates is useful for evaluating model estimates of lower troposphere.’

Third, there are numerous grammar errors throughout the paper. I have listed some of the errors, but I suggest that one of the authors go through the paper carefully to correct these.

We will do it.

It would be really nice to see what would happen if you used operational TCCON retrievals versus your own – would the results be worse or do you expect them to be the same. At a minimum, you should state if you expect the results to be equivalent, and if not, why not.

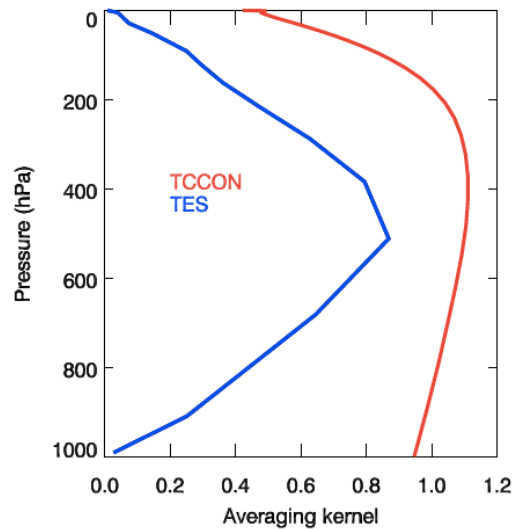
We've done the same study by using operational TCCON retrievals and the results are similar to what was show in this paper. The reason we used our own retrieval algorithm is that the optimal estimation allows us to do error characterization, which was discussed in the second part of this paper.

Finally, somewhere, perhaps in section 2, it is important to show the averaging kernels of TES and TCCON, to visually motivate how this approach is possible. I was surprised to not see such a figure.

We agree the plot combining both of their averaging kernels would visually show how our approach works. I have added a figure and sentences (below) in section 2, page 4498, L17.

'We expect this approach to provide estimates of lower tropospheric CO₂ because the TCCON and TES measurements have complementary sensitivities to the vertical distribution of CO₂. For example, Figure 1 shows that TCCON CO₂ estimates are sensitive to the total column CO₂ as described by its averaging kernel. The averaging kernel indicates the sensitivity of the retrieved estimate to the true distribution of CO₂. The TES averaging kernels for an estimate of CO₂, averaged from surface to top-of-atmosphere, is also shown in Fig. 1. Both averaging kernels show sensitivity of these estimates to the free tropospheric CO₂ whereas the TCCON total column estimate also has sensitivity to the lower tropospheric CO₂. Consequently we expect that combining these measurements will allow for estimation of lower tropospheric CO₂. This approach has been used to estimate lower tropospheric ozone and CO (Christi and Stephens, 2004; Worden et al., 2007; Worden et al., 2010; Fu, 2012).'

Figure 1. Averaging kernel for TES retrieval (blue) and TCCON retrieval (red).



Specific comments:

- Change “precision of 1.02 ppm” to “standard deviation of 1.02 ppm.” to be more precise (no pun intended). Note the missing period at the end of the sentence.

Fixed. Page 4496 L 19: ‘a precision (one standard deviation) of 1.02 ppm’

- The structure of the introduction is a bit confusing. You go through a long description on total column measurements, but give a single sentence on free-trop measurements from TES and AIRS. One approach you could follow would be to
 - End the first paragraph at “continental scale flux estimates (...).”
 - Move the paragraph starting with “Total column CO2 data” to the 2nd paragraph.
 - Let the exposition starting with “However, because CO2 is a long-lived greenhouse gas” be the 3rd paragraph.
 - In the sentence with TES & AIRS, say that they are passive thermal infrared measurements.

We start the introduction with the motivation to measure the boundary layer CO2 that although ground based in situ surface data and total column data has the potential to constrain the surface flux, the lower tropospheric data with a long-term and good global coverage is still desirable for local flux constraints and understanding of vertical transport. The second paragraph describes how the total column and free tropospheric CO2 are measured by different instruments. The third paragraph introduced our method and the structure of the rest part of paper.

If I move the part state with ‘However, because CO2 is a long-lived greenhouse gas’ to be the 3rd paragraph, I feel we are talking about the flux constraints back and forth. So I decide the keep the structure of introduction.

- Remove the Yokota & Yoshida references for GOSAT-2; these are references for GOSAT; there are no references (yet) for GOSAT-2.

Fixed.

- You need a reference for the TCCON profile retrieval you use – so far you just state that you do one (page 4499, lines 12-13). I believe there is one?

Fixed. Added a reference (Kuai et al., 2012).

- In Section 2.2, you need to move appendix A5 to here. This is not Nature!, and the content of the appendix A5 is not overly technical. Please move that short appendix directly into this section.

Fixed.

- Section 3, equations 1 and 6. These equations are just plain wrong (and they are formally negative, if you look at the integration limits). They don't even pass a units test!! I suggest looking in the Wunch 2010 paper on the calibration of TCCON and substituting her equation 5 for your equation 1, and just skip f_g , which is never used by anyone, and remove equation 2 altogether. You only need to talk about dry air mole fractions.

I have replaced the pressure coordinate to the altitude coordinate in equation 1 and 6. Now the units should be correct.

- When you introduce alpha in line 6 of page 4502, state that it is an *empirical* correction factor.

Fixed. Change to 'an empirical correction factor'.

- Section 4: "Jack---knifing" is not clear to everyone. Change to "vertical oscillations" perhaps?

Fixed.

- In section 4, are you using a standard optimal estimation retrieval? If so, I suggest changing "also depends on a constraint matrix to regularize choices for the retrieval solution (Bowman et al, 2006)" to "is a standard optimal estimation retrieval that employs an a priori constraint matrix to regularize the problem (see e.g. Rodgers, 2000)."

Yes, we are using the standard optimal estimation retrieval. We have rewritten the sentence in page 4505, L 7, as

'The non-linear least squares retrieval is a standard optimal estimation retrieval that employs an a priori constraint matrix to regularize the problem (e.g. Rodgers, 2000; Bowman et al., 2006).'

- It would be instructive to say a bit more about how strong the a priori CO_2 profile constraint is. Can you construct $h^T S_a h$, where h is the pressure weighting function, to yield the typical $1-\sigma$ uncertainty, and state what this is, as was done for example in O'Dell et al (2012)? Also, I suggest changing the 2% and 1% here to 8 ppm and 4 ppm, respectively; that is a bit more clear.

We showed the covariance matrix for a priori in Figure 3. We also added '~8 ppm' and '~4 ppm' after '2%' and '1%' respectively. The typical $1-\sigma$ uncertainty is about 1% (~4 ppm).

- After you state the cost function you are minimizing (eqn 13), could you say how the prior covariances of the other retrieved parameters are chosen, at least if you feel it is relevant?

All the other prior covariance is set to be 100% uncertainty. We've added this description in section 4, page 4505, L15:

'The elements of the a priori covariance corresponding to the other retrieved parameters (e.g. H₂O) are set to be equivalent to 100% uncertainty.'

- One of your key results, stated in section 5.2, is that the scatter of the surface--- to---600 mbar CO₂ versus aircraft is 1.02 ppm. It is important to state what it is for the prior (which is graphed in figure 6a), to see what is the real gain by using this method over what we already knew as expressed in the prior. Clearly it will be better, but you should quantify it.

Add in section 5.2, page 4507, L21:

'This uncertainty (1.02 ppm) in the surface-to-600 hPa estimates is improved compared to the uncertainty of the similar estimates by the TCCON prior (2.0 ppm).'

- In the summary (sect 6), you state that the precision is sufficient to capture the seasonal variability over "the TCCON sites" whereas you have really only shown it for Lamont. Please modify this statement to be more clear about what you have shown.

Page 4510, L 5, we change this sentence to

'We show that this precision is sufficient to characterize the seasonal lower tropospheric variability of CO₂ over the TCCON Lamont site.'

- I suggest to make the final couple of sentences a bit stronger, regarding applying this technique to GOSAT and OCO---2. If you plan to apply it to GOSAT in future work, say so! If you think it won't work from GOSAT, please say why not! This is key because applying this method to Satellite data would drastically expand the number of lower---trop CO₂ observations that you could get with this method.

Yes, we plan to apply it to GOSAT in future work and expect it will expand the number of lower tropospheric CO₂ data. We added the words below at the end of the section 6, page 4510, L17.

'Our future work will apply this method to combine GOSAT and TES data to expand

the spatial coverage of these lower tropospheric CO₂ estimates.’

- Appendices 1– 3 are kind of muddled and it is not really clear what you’re trying to do. A few introductory sentences right at the beginning, to prepare the reader for the outline of what you’re going to do in those appendices, would be really helpful.

- Page 4510, insert between L19 and L20:

‘One of the reasons we used our own retrieval algorithm to estimate the total column CO₂ instead of standard TCCON product is that the optimal estimation allows us to characterize the error budget. This appendix discusses the expected errors and shows how these terms represent the actual errors. Careful characterization of the errors is critical for evaluating the retrieval mechanics and for use of these data for science analysis.’

- (A1), Page 4511, insert before L1:

‘In addition to CO₂, we also retrieve a column amount of H₂O, HDO and four instrument parameters. This set of retrieval parameters is defined in following retrieval vector (see Section 4):’

- (A2), Page 4513, L9 change to:

‘The total error for a single retrieval is the difference between the estimated state vector (Eq. A10) to the true state vector (x):’

- (A3), Page 4514, insert between L1 and L2:

‘Considering different time scales, the uncertainties of the estimates would be attributed to different error terms. In this section, we will discuss the error terms one by one.’

- In the appendix A2, you briefly mention a covariance matrix S_L associated with spectroscopic uncertainty, but you say absolutely nothing about how you came up with it. Either get rid of this term altogether, or go into a bit of detail about how you came up with the error covariance matrix.

I added words below at the end of A2, Page 4513, L29.

‘It has been found that spectroscopic inadequacies are common to all retrievals from TCCON radiances (e.g. line widths, neglect of line-mixing, inconsistencies in the relative strengths of weak and strong lines) (Wunch et al., 2010).’

- Appendix A3.1 – do the aircraft really have an uncertainty of 0.02 ppm? I doubt this. Please state a reference for this number, or modify to be slightly less strong.

Table 2 in Wunch et al. 2010 shown that HIPPO CO₂ has a precision of 0.02 ppm and an accuracy of 0.1 ppm, and Learjet has a precision of 0.03 ppm and an accuracy of 0.16 ppm. I've added this reference. And rewrite this paragraph as below.

'Several aircraft only measure CO₂ up to approximately 6 km, but three of them go up to 10 km or higher, which are measured by Learjet on July 31, August 2 and 3 (Fig. 5). These three profiles show that the free troposphere is well mixed and the vertical gradient is small (Wofsy et al., 2011), less than 1 ppm/km on average between 600 hPa and tropopause. Therefore, the lower part of x_{FLT} is from the direct aircraft measurements. Above that, the TCCON a priori is scaled to the measured CO₂ values at the top of the aircraft measurement so that the profile is continuously extended up to 71 km.'

- A3.2 – where do you get S_m ? Please state what you assume for measurement noise, and if there are any important assumptions for the reader to be aware of here.

Page 4516, L20, we mentioned that ' S_m ' is calculated by Eq. (A14). It is dependent on the measurement noise covariance S_e . We have stated at the end of section 4 (Page 4505 L 25) that ' S_e is the measurement noise covariance, a diagonal matrix with values of noise squared. Noise is inverse of SNR.' SNR is defined in Page 4498, L21.

- I suggest a short appendix A3.5 to put all the error terms together and discuss which are the most important. At some point you state that temperature effects dominate the uncertainty – it would be good to talk about that here, and other important error terms, and which error terms are small enough to be neglected. So, a “putting it all together” subsection just to summarize the main results of the technical A3.1 to A3.4 subsections.

We have discussed each error term in subsection of A3. Further discussions of all the error terms together are in section 5.1 and 5.2. Due to the limitation of the manuscript length, we decide not to add another section after A3.4.

- Appendix A4 – please state what σ^2 TES and σ^2 TCCON are and that they come empirically from comparisons with aircraft (if so; I know that is true for TES).

Fixed. Page 4518, L11 change to:

'The estimate of this term is 0.90 ppm. The TES assimilated free tropospheric bias error and uncertainty (0.38 ± 0.71 ppm) is estimated by the comparison to the free tropospheric estimates from the aircraft-based profile (x_{FLT}). The TCCON total column mean bias error and uncertainty (-5.66 ± 0.55 ppm) has been discussed in previous section.'

Technical Comments:

- I recommend time ordering multiple references when used. For instead, the long set of references quoted in the intro starting with Baker et al, 2010 would make more sense if time ordered. I think this is a general approach that most people follow, rather than using alphabetical ordering.

Fixed

Page 4497, L2:

(Law and Rayner, 1999;Bousquet et al., 2000;Rayner and O'Brien, 2001;Gurney et al., 2002;Rayner et al., 2008;Baker et al., 2010a;Chevallier et al., 2010;Chevallier et al., 2011;Rayner et al., 2011;Keppel-Aleks et al., 2012)

Page 4497, L7:

(O'Brien and Rayner, 2002;Chevallier, 2007;Chevallier et al., 2011;Keppel-Aleks et al., 2012)

Page 4497, L14:

(Sarrat et al., 2007;Keppel-Aleks et al., 2011;Keppel-Aleks et al., 2012)

Page 4498, L 26:

(Yang et al., 2002;Washenfelder et al., 2006;Deutscher et al., 2010;Wunch et al., 2010;Messerschmidt et al., 2011)

Page 4503, L12:

Yang et al., (2002); Washenfelder et al., (2006); Deutscher et al., (2010); Geibel et al., (2010); Wunch et al., (2010;2011a)

- Carbonsat should be CarbonSat.

Fixed.

- P4497 line 15 overpredicts ---> overpredict

Fixed.

- P4497 line 28 “in near future” ---> “later this decade”. They are talking about 2017 for GOSAT---2 and 2020 for CarbonSat.

We have change it to

‘Similar space-borne instruments include OCO-2, which is expected to be launched in 2014 (Crisp et al., 2004), and GOSAT-2, which are also expected to be launched later this decade. In addition, CarbonSat (Bovensmann et al., 2010;Yoshihara et al., 2011) is a proposed instrument that could also be launched in the next decade.’

- P4499 line 1 “at 1.6” ---> “in the 1.6”

Fixed.

- Page 4505 line 6 “least square” ---> “least squares”

Fixed.

- Page 4507 line 26 “actually uncertainty” ---> “actual uncertainty”

Fixed.

- Appendix A3 “will be discuss in follow” please fix this sentence.

Fixed.