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).

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!

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.

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.

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.

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.
- 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.
- Remove the Yokota & Yoshida references for GOSAT-2; these are references for GOSAT; there are no references (yet) for GOSAT-2.
- 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?
- 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.
- 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.
- When you introduce alpha in line 6 of page 4502, state that it is an *empirical* correction factor.
- Section 4: "Jack-knifing" is not clear to everyone. Change to "vertical oscillations" perhaps?
- 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)."
- It would be instructive to say a bit more about how strong the a priori co2 profile constraint is. Can you construct h Sa h^T, 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.

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

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.
- Carbonsat should be CarbonSat.
- P4497 line 15 overpredicts -> overpredict
- P4497 line 28 “in near future” -> “later this decade”. They are talking about 2017 for GOSAT-2 and 2020 for CarbonSat.
- P4499 line 1 “at 1.6” -> “in the 1.6”
- Page 4505 line 6 “least square” -> “least squares”
- Page 4507 line 26 “actually uncertainty” -> “actual uncertainty”
- Appendix A3 “will be discuss in follow” please fix this sentence.