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Atmospheric Measurement Techniques Discussions

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Interactive comment

Interactive comment on "Bias corrections of GOSAT SWIR XCO_2 and XCH_4 with TCCON data and their evaluation using aircraft measurement data" *by* M. Inoue et al.

Anonymous Referee #1

Received and published: 3 March 2016

This manuscript presents the derivation and application of an empirical bias correction for GOSAT XCO2 and XCH4 retrievals, and provides some evaluation of this bias correction through the use of independent aircraft measurements.

This paper was very clearly written and well structured, which made it quite pleasant to read and review. Nonetheless, I have some concerns that I would like to see the authors address.

Regarding the stratospheric completion of the methane profiles discussed at the top of page 7: I appreciate the effort to include the trend rather than just using a fixed climatology for the stratospheric extension of the methane profile above the tropopause, but I wonder if using a fixed growth rate is the best choice. The

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atmospheric growth rate as measured at the surface has been highly variable in the past years, ranging from 4.67 ppb/year in 2012 to 12.36 ppb/year in 2014 (see http://www.esrl.noaa.gov/gmd/ccgg/trends$_{c}h4/global_{g}rowth).Thisgrowthrateisofcourselaggedsomewhatinthestratospher$

Regarding the colocation criteria used here: Have you considered using a more sophisticated approach such as that used in the Guerlet et al. 2013 JGR paper, which takes into account the impact of both transport and flux variability on the colocation, rather than simply the geographic limits? This makes it more likely that you are really comparing similar air masses, while simultaneously expanding the potential match area in space. This could be quite useful for getting TCCON colocations with M-gain regions, for instance. At very least a more thorough discussion of the potential drawbacks of the 5 degree x 5 degree approach (and alternatives proposed in the literature, such as that of Nguyen et al., AMT, 2014) should be discussed.

As a reader, I questioned why the HIPPO profiles were used so sparingly. In Table 2f it shows that only 9 HIPPO profiles were considered for analysis, almost all in the southern hemisphere. What about the rest of the HIPPO campaigns and profiles? Presumably of these 9 campaigns, none of them were from July 2009, as these were not included in Figure 7. Perhaps it would be more useful to choose a month where there were HIPPO profiles available to show in a figure like Figure 7? Or is there a practical reason why these measurements could not be included?

When reading about the improvement in the correlation coefficient from the uncorrected to the corrected version of the data over land (page 11), I questioned the significance of the improvement. Given the error bars, I doubt that an improvement from 0.70 to 0.71 (or even 0.86 to 0.88) is really significant. Likewise, the decrease in the correlation over ocean is likely not a cause for concern (although it should be not simply be ignored in the text, as is the case now). Taking the uncertainties of the individual measurements into account makes it possible to estimate uncertainties on the correlation coefficients as well, so this should be easily resolved.

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I stumbled a bit in the interpretation of Figures 7 through 9. To begin with, it would be easier to interpret these figures if the logical colour scheme used previously (green for land, blue for ocean) had been maintained. Amending this is recommended.

In Figure 7, I am not entirely clear what the take-away message should be. I acknowledge that getting a feeling for the improvement globally is difficult, but I don't know why the satellite values are binned while the aircraft values are not. The only significant difference I see between the corrected and uncorrected values of XCO2 are that the land and the ocean seem to be further apart after correction, though still agreeing within uncertainty. The change in agreement with the aircraft measurements is difficult to discern.

In Figures 8 and 9 (panels c and d) it can be seen that the difference between the satellite soundings averaged over one latitudinal band and one TCCON site is reduced after bias correction, but, as is stated in the text, "the seasonality in the difference remains". And so it should! There is no reason to think that the measurements of Lamont should be identical to the mean total column values over a 15 degree latitudinal band. This also made the interpretation of panels a and b of the same figures rather difficult. At first I was wondering why the Tsukuba XCH4 values for, say, autumn 2013 so much higher were, but then realized that it was likely just the effect of regional emissions or synoptic transport during this period, and there was no reason to interpret any offset from the zonal mean GOSAT XCH4 value as a problem in the GOSAT data. Thus I found the interpretation of these figures rather difficult. I am not sure how this could best be improved, but at least the limitations of the comparison should be discussed. At present I am not convinced that the figures add a lot to the reader's understanding of the work.

And finally, and perhaps most importantly, I agree with Referee 2 that the results should be discussed in the context of other related studies using empirical multivariate approaches to correct the bias in retrieved GOSAT products. Are the same variables found to be significant? Are the spatial patterns in correction consistent? A full comparison might be too much to undertake in this study (though it would be an interesting topic for a follow-up study), but at least a superficial comparison would be appropriate.

If the authors can address these concerns, I think the manuscript is appropriate for publication in AMT.

Minor comments:

The previous commenter suggested (rightly) that the work be better put in the context of relevant literature. Might it be that the Guerlet et al. 2013 reference on page 4, line 5, is in fact referring to the paper this reviewer brought up?

Figure 2 and 3 have very difficult to read axes, and rather poorly chosen axis limits.

Figure 4 b is interesting in that for ocean data the correlation coefficient is quite high and rather improved, while the slope is considerably further from the 1:1 line. (I agree with the other reviewer that information about the slope would be useful here.) What does this shift look like in general (and not just for July 2009)? Does it seem reasonable? Granted the ocean colocations to TCCON sites are rather limited in geographic extent, so this might be difficult to interpret, but I think it warrants discussion.

P5 line 21: HeymanN spelled incorrectly in citation.

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