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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of aerosols and thin cirrus clouds on the GOSAT-observed CO_2: a case study over Tsukuba" *by* O. Uchino et al.

Anonymous Referee #1

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General Comments

Overall, I found this paper well-written and a useful contribution to the field of satellite remote sensing of CO2. It postulates a much-needed explanation for the relatively poor performance of the NIES v01.xx operational CO2 retrieval algorithm, and motivates modifying that algorithm to help resolve some of its issues. Also, this is the first paper that reports using ground-based lidar observations to validate space-based CO2 observations, and is useful for that reason as well. That said, I have several questions and some suggestions for the authors that will hopefully enhance the paper.

Specific Comments

1. The main upshot of the paper is that retrieving AOD confined to 0-2 km altitude,

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which is what the operational v01.xx NIES algorithm does, is not sufficient. The authors argue that using more realistic vertical profiles (and types) of aerosol enables better XCO2 retrievals, but they show little evidence that this is precisely what is going on. The v01.xx algorithm seems to retrieve anomalously high values of aerosol in some cases. I think it would strengthen the paper if the authors talked a little more about what the algorithm specifically retrieves in terms of aerosol optical thickness(es) (AOT), for the different versions of the algorithm (v01.xx, "revised", and "new"). For instance, please add (and discuss) the retrieved AOT values in the v01.xx algorithm, and how these values change (presumably they decrease) for the "revised" and "new" versions of the algorithm. In the case where aerosol are cirrus are retrieved separately, it would be nice to see how much of each the algorithm is retrieving.

Discussing physical mechanisms would be even better, because we as the reader are left with the impression that this is just retrieval "black magic"; things get better, but we really don't know why. For instance, for the "revised" algorithm with improved aerosol treatment, does the improvement come mostly from improving the aerosol vertical distribution rather than the aerosol type, or vice versa, or are both equally important? Also, do the spectral residuals improve by either of the two algorithm changes, so the changes allow the inversion model to fit the observed spectra better? If so, by how much and in which spectral bands?

2. The metric for a good improvement is stated in the paper as "the differences between GOSAT XCO2 data and TCCON become much less." This seems too simplistic and does not identify well-defined metrics other than the overall mean bias amongst the 9 soundings considered. I would recommend stating both the mean and standard deviation of the XCO2 differences between GOSAT and TCCON. This will allow the reader to see what helps in terms of the overall bias, and what helps to reduce the scatter in the retrievals. Based on Figures 1, 4, and 5, it appears that the new solar model only helps with the bias, but not the scatter, whereas the improved aerosol treatment helps with both the bias and scatter. Therefore, please add these statistics

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to the discussions on pages 29888, 29893, and 29894.

3. Figures 1, 5, and 6 seem a bit redundant, and don't allow for direct comparison of the different retrievals at the single sounding level. I suggest combining these into a single figure that shows TCCON, v01.xx, revised, and new all on the same plot. If it looks "too busy", you could still have figure 1 as-is, but remove figures 5 and 6, and add a figure that shows retrieved XCO2 minus TCCON vs. date, for v01.xx, revised, and new.

4. The XCO2 statistics are generally given in percent, even though much of our field thinks in terms of parts-per-million (ppm). Would it be possible to change all the difference statistics to be in units of ppm? I realize this is a personal choice, so it is entirely up to the authors to make this change or not.

5. It would be useful to see how the retrieved surface pressure changes due to each of the two retrieval modifications, "revised" and "new". Figure 8 currently shows the retrieved surface pressure for "new" only. Could the authors add the Ver 01.XX and the "revised" surface pressures to this figure? Then we could see how much of the improvement in XCO2 comes from improvement in the retrieved surface pressure.

6. Could the authors briefly explain their colocation (distance) requirements? As far as I can tell, they've used special-target observations so the distance from the center of the GOSAT field-of-view to the TCCON station is very small (less than 2 km), and this causes there to be only 9 soundings for comparison. This would not be obvious to most readers. It directly leads to the "low-number statistics" that this work partially suffers from.

Technical Comments

- P29886, top paragraph. This paragraph would read better if the present tense were used. E.g, "In this study, we investigate. . ."; "Next, we show. . .", etc.
- P29887, L1. "chi-square" → "chi-squared"

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- P29891, L15-17. I suggest you remove the two sentences on the definition of the single scattering albedo; single scattering albedo is a common parameter and should be well-known to the readers.
- P29894, L1. Please explain what the “low-frequency baseline correction” is.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29883, 2011.

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