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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 #3

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1 Overall Recommendation

The article describes a comparison of GOSAT XCO2 retrievals with co-located ground based TCCON FTS measurements in Tsubaka, Japan. The authors use ground based lidar and sky radiometer data to derive aerosol and cirrus properties. This data and Toon's solar spectrum are used for three case studies with modified satellite retrievals showing a distinct improvement of agreement with TCCON.

The paper covers an important and interesting scientific topic, is well written and has an overall clear structure and figures. Nevertheless, the paper gives little new insight into geophysical processes of the carbon cycle. Its focus lies more on the improvement of a satellite remote sensing technique. Therefore, one could argue that the paper fits

better to the aims and scopes of a journal like Atmospheric Measurement Techniques – which does not reduce its scientific relevance. If the editor finds that the topic is fitting well into ACP, I would recommend publishing the paper after some major and some minor revisions.

2 Major Comments

Regional Biases: The authors motivate the need for retrieval improvements with systematic biases found by Morino et al. 2011 (P29885L2). However, systematic global biases are not really an issue when thinking about the application of inverse modeling. In contrast, regional varying bias patterns of only a few tenth of a ppm have the potential to hamper inverse modeling (e.g. Miller et al. 2007). This should be discussed within the paper because clouds and aerosols can produce distinct regional patterns (see e.g. L3 statistics of CALIPSO COT and AOT). This means, an improved NIES algorithm which can better handle clouds and aerosols would have the great potential to reduce regional (or temporal) bias patterns rather than a global offset.

Case3: From the first paragraph at P29895 I understand that in case3 a new retrieval has been set up which was applied to the same GOSAT data as before. The results of this retrieval are shown in Fig.10. Within the paper the case3 retrieval is referred to as "simulated XCO2". If I misunderstood something I would recommend making the paragraph clearer. If I understood the paragraph correctly, I have the following recommendations/questions:
a) "Simulated" is extremely misleading please find a better name e.g. replace "new", "revised", and "simulated" simply by "case1", "case2", "case3".
b) The results shown in Fig.10 agree extremely well with the TCCON FTS measurements. I would estimate from Fig.10 that the standard deviation (TCCON-Simulated XCO2) is about 0.5ppm. From earlier studies (e.g. Boesch et al. 2011) one can esti-

mate that the theoretically optimal retrieval precision due to SNR over land surfaces can amount already 1ppm under typical viewing geometries. As the TCCON data (even if averaged over 30min) also have a small random error, one would expect that the standard deviation of the difference should be even larger. Studies with real GOSAT data (e.g. Butz et al. 2011) found 2.8ppm. This leads me to the hypotheses that the retrieval of case3 is maybe over-constrained by the a priori. For this reason, I would propose to show XCO2 a priori within Fig.10 and also the error bars of the a priori and the retrieval results. Additionally, it would be very interesting to discuss within the text how large the influence of the a priori is, i.e. by giving the error reduction.
c) Why are these very promising results not mentioned within the abstract and in the conclusions?

Fig.1, 5, 6, and 10: a) It would be much easier to see how the individual retrieval modifications improve the results if these figures where merged into only one figure. b) Please provide error bars for the retrieved XCO2. This information is available either from the optimal estimation output or from earlier validation studies. c) Please show also the used a priori values.

3 Minor Comments

Co-location Criterion: The analyzed period spans over six months but the comparison includes only nine GOSAT measurements. I expect that an extremely strict co-location criterion has been used. Please discuss the used co-location and why it is so strict.

Spectroscopic Line Parameter Database: Other XCO2 retrieval teams found systematic biases of the surface pressure and/or XCO2. In this context they speculate about in-accuracies of the spectral line parameters of the HITRAN database (e.g. Boesch et al. 2006, Reuter et al. 2011). Some tackle this issue with a bias cor-

rection in the post processing and others by modifying the HITRAN line parameters. Fig.6, 8, and 10 show only minor or no biases. a) Which spectroscopic line parameter database is used? b) Are there indications for biases introduced by the spectroscopic line parameter database?

Representativeness: How representative is a lidar (point) measurement for a GOSAT pixel with 10km in diameter? One could use this argument also to discuss remaining discrepancies in Case 1 and 2.

Fig.1: Is there a reason that XCO2 retrieval results are sometimes shown in green (Fig.1) and sometimes in blue (Fig.5, 6, 10)?

Fig.2: Too small.

P29885L9: Add something like “if accurate and precise enough”.

P29885L21: Morino et al. 2011 did a side by side comparison. They found for Tsukuba a bias of -6.38ppm. Wouldn’t it be better to cite this value, because the results of Morino et al. show that the bias can strongly change from station to station?

P29885L29: Several other publications could also be cited in this context, showing that many scientists see great potential in an explicit consideration of aerosol and/or cloud properties: E.g. Bril et al., 2007, Connor et al. 2008, Reuter et al., 2010, Boesch et al., 2011.

P29887L3: Is the SNR criterion really applied as post processing filter? Why not using it as pre-processing filter?

P29887L27: 7751-8000cm-1 referrers only to the O2 band.

P29888L12: I think 0.8ppm is the 2sigma uncertainty.

P29888L16: Is “demonstrated” the correct word in this context?

P29888L19: Caption 2.3 should differ from Caption 2. “Comparison” would be suffi-

cient.

P29888L23: "About half of the. . ." This sentence is misleading, as also the "rejected" data is used for the analysis. (?)

P29888L27: Don't use sometimes % and sometimes ppm for XCO2 differences. I would suggest using always ppm.

P29889L4: Morino et al. 2011 did a side by side comparison. They found for Tsukuba a bias of -6.38ppm. Wouldn't it be better to compare with this value?

P29889L4: ". . .adequately rejects outlying. . ." I find this not so obvious when looking into Tab. 2 and sorting from large to small bias I get (0=reject, 1=quality OK): 001011011.

P29890L7: I would suggest to consistently using Angstrom exponent alpha (as in Tab4) instead of wavelength exponent Alp.

P29891L1: ". . .indicate that the retrieval . . . is greatly influenced. . ." sounds somehow contradictory. Correlating the lidar optical thickness (Tab.4) with the CO2 error (Tab2) gives 0.24 which indicates that the relation is not so obvious.

P29893L7: I assume that Hess' cirrus model is used to define all microphysical parameters of the cirrus particles which are not available from the lidar measurements (e.g. phase function). Do the results of case1 critically depend on the used cirrus microphysics (i.e. the used cirrus model) or are the result rather stable?

P29894L19-25: The focus of this discussion lies in the reduction of the overall bias. However, it should be pointed out that the case study retrievals are capable to follow the seasonal cycle better. I would think that this is the main benefit.

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