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Atmospheric Measurement Techniques Discussions

Interactive comment on "The ACOS CO_2 retrieval algorithm – Part 1: Description and validation against synthetic observations" *by* C. W. O'Dell et al.

Anonymous Referee #2

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This paper represents an ambitious effort to describe the retrieval algorithm developed by NASA for the GOSAT satellite and its theoretical performance. It is a mine of information that will be of use to a wide community of scientists working on this type of measurements. There are a few weak points though that should be addressed before the paper is published in AMT. I list them hereafter.

- p.6098, l. 14: the last words of the paragraph about surface reflectance and radiative transfer assumptions are not so well explored in the paper and may not deserve a place in the abstract.

- p. 6099, l. 15. I was curious to read the reference given about 'inversions that ingest these data' and actually only found theoretical simulations in it. To my knowledge, the only published studies that have ingested real data are Chevallier et al. (2005, 2009) and Nassar et al. (2011)
- p. 6104, l. 21: 'prior errors' may replace 'profile' here.
- p. 6104, l. 27: I guess that the 12 ppm refers to a standard deviation. If it does, it is about 4 times the variability of XCO2. The quality of the prior profile should be much better than that. I understand that the authors want to minimize the weight of the prior XCO2 information in their retrieval system, but doing that, the balance between the various prior errors of the state vectors cannot be correct.
- p. 6105, l. 14: do the authors mean 'precision' rather than accuracy?
- p. 6105, l. 15: I guess that the 4hPa figure refers to a standard deviation. This should be clarified.
- p. 6105, l. 13-16: the text here is ambiguous in that it refers to a publication that actually contains much better figures than the one used here.
- p. 6110, l. 2 and 5: the acronym RT is defined after it is used. It may actually be removed since it is not used elsewhere.
- p. 6112, l. 4-6: it is not clear whether this approximation is used in the results presented. If it is not, it would be interesting to show its impact on the results, in particular those presented in Section 3.4. Again, the errors are assigned in a very ad hoc manner while a more rigorous setting could be used.
- p. 6113: the symbol ΔP_{cld} is not very intuitive and could be replaced by something like ΔP_s to help the reader.
- p. 6116, l.1: missing 's' in particles.

- p. 6125, l. 19: A standard deviation of 4.4 hPa is unrealistically large. There seems to be more here than just interpolation problems. In other words, the authors should not say that such differences can be caused by interpolation.
- p. 6126, l. 3: With an RMS error of 1.7hPa, the inverted surface pressure does not look better than NWP analyses. Does this variable deserve to be in the state vector?
- p. 6128, l. 26: the numbers given here (1-2 ppm) are important and it would be appropriate to explain how they are computed from the results presented. Also, do the authors mean precision rather than accuracy?

References

Chevallier, F., et al. 2005: Inferring CO2 sources and sinks from satellite observations: method and application to TOVS data. J. Geophys. Res., 110, D24309, doi:10.1029/2005JD006390.

Chevallier, F., et al. 2009: AIRS-based vs. surface-based estimation of carbon surface fluxes. J. Geophys. Res., 114, D20303, doi:10.1029/2009JD012311.

Nassar, R., et al. 2011: Inverse modeling of CO2 sources and sinks using satellite observations of CO2 from TES and surface flask measurements, Atmospheric Chemistry and Physics, 11, 6029-6047.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 6097, 2011.