

Interactive
Comment

Interactive comment on “A framework for comparing remotely sensed and in-situ CO₂ concentrations” by R. Macatangay et al.

R. Macatangay et al.

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We thank Peter Rayner for an evaluation of important points on our paper that we are pleased to respond to.

(Peter Rayner) It might help to know how well TM3 reproduced the aircraft measurements themselves.

(authors) TM3 is only used for the part of the column between the ceiling of the aircraft and the tropopause. We argue that the variability in the column is dominated by the contributions from the lowest 3 km, where highly variable surface fluxes cause variations in the CO₂.

This was done in two publications:

1. Yogesh Kumar Tiwari, MPI-BGC Technical Report No. 7

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Short summary: The comparison shows that model predictions are in good agreement with the long term upper troposphere CO₂ airplane observations. Even short-term transport events as well as longer-term anomalies are well captured.

Quoting the paper: The comparison of upper troposphere TM3 simulations and observations showed a RMS of approximately 0.4 ppm.

2. TransCom Upper Air exercise http://www.purdue.edu/transcom/T4_upperAir.php
status: upcoming

(Peter Rayner) Similarly there were significant differences between concentrations simulated with WRF and STILT. Should we regard this as a measure of model uncertainty?

(authors) One could regard this as an effect from uncertainty in transport and in biospheric fluxes. STILT uses ECMWF wind fields, while WRF is run in forecast mode for 30 hour periods. Further, STILT runs used the simpler diagnostic biosphere GSB, while WRF was coupled with VPRM. However, overall model uncertainty can be derived better from the comparison between modeled and measured CO₂ at the Biscarosse station. Additional statistics for afternoon values are now included in the revised manuscript (see Table 3).

(Peter Rayner) Does one of them fit the aircraft observations better than the other?

(authors) The reason we used WRF is to assess if there is a difference between slant and vertical columns of CO₂ (Figure 17). WRF was also used to assess if there are differences in using GSB or VPRM.

(Peter Rayner) Also, why was not the WRF vertical integral performed with the correct averaging kernel? It would be best to do this consistently but failing that, the authors should test the importance of this omission using STILT where, presumably, analysis is computationally cheaper.

(authors) This is now included in the revised manuscript.

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