

Interactive comment on “CO₂ flux estimation errors associated with moist atmospheric processes” by N. C. Parazoo et al.

Anonymous Referee #1

Received and published: 6 June 2012

The paper describes the impact of different sub-grid parameterizations of vertical transport (moist convection, turbulent diffusion) on inversion results when using column dry air mole fractions from satellite based remote sensing instruments. By using the same offline transport model, but meteorological fields from slightly different data assimilation systems, attribution of differences in retrieved fluxes can be attributed to specific dynamical differences in the met fields. I regard this manuscript as a very useful contribution to the field of CO₂ inverse modelling, and recommend publication after a few minor revisions.

General comments:

1) There should be a discussion on the significance of the flux errors derived as differences from different inversions, given that the uncertainties are mostly larger than

C3319

the (statistical) errors for different regions. This is at least evident from the way it is presented in Figs. 5 a) and b). For example, the difference in “amplification” of the EU source due to only differences in vertical transport parameterizations (i.e. differences between Exp. 6 and Exp. 4) is about 0.55 – 0.2 GtC/year, while each has an uncertainty of about 0.25 GtC/year. For the N. America temperate region the situation is similar.

2) Although it is a commendable experiment in that only specific differences were allowed in the setup (same offline transport model, principally the same assimilation system to generate the met fields), there is also a downside: the differences retrieved fluxes from such an ensemble are likely to be smaller than those when allowing for other differences in the setups of the inversion system. Even then it can not be assumed that the true error (difference between one of the inversion results and the true “real” fluxes) is bracketed by the ensemble spread. May be the authors can include a comment on this in the conclusions.

3) It should be clearly stated if for the inversions the synthetic data were used at their given temporal and spatial resolution, or whether there has been any temporal or special averaging applied such as aggregation to monthly averages. This of course has a major impact on the interpretation of sampling biases such as in the cloud screening experiment (experiment 3).

Specific comments:

P 9990 L 24: It is a bit unclear why GEOS-5 does not require time-averaging: for offline transport simulations to have transport consistent with the parent model, time-integrated advective mass fluxes should be used (such that the mass balance is consistent).

P 9991 L 14: What is meant by “Wind vectors are also conserved”? This should be explained in a bit more detail. In order to have consistent advection between the two resolutions, dry-air mass fluxes (rather than winds) should be properly aggregated from

C3320

fine to coarse resolution.

P9993 L 15: what is the impact of using glint retrievals beyond the +/- 20° latitude band covered by GOSAT?

P 9998 L 4: Figure 8 does not show South America

P 9998 L 12: G4F05 does not exist; probably G5F05 is meant

Technical comments

P 9986 L 9: I would suggest replacing “conveyors” with “conveyor belts”

P 9987 L 26: insert “in” between “differences” and “grid”

P 9993 L 1: remove the word “based”

P 9993 L 5: replace O2 by CO2?

P9998 L3: replace “Figs. 3” by “Figs. 5”

P9998 L4: Figure 8 does not show South America

P 10008 Fig 2: in a) the profiles from G5F05 and G5F10 extend much closer to the surface than those of G4F10 and G4F20, while in b) this is the opposite. Is this a plotting mistake?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9985, 2012.