

Interactive comment on “Latitude-time variations of atmospheric column-average dry air mole fractions of CO₂, CH₄ and N₂O” by R. Saito et al.

Anonymous Referee #1

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The paper describes comparisons between TCCON measurements and corresponding tracer transport simulations for CO₂, CH₄ and N₂O using the ACTM model. The combination of multiple species is suitable for attribution of model-data mismatches to uncertainty in transport vs. surface-atmosphere fluxes. I regard this manuscript as a useful contribution, and recommend publication after a few minor revisions.

General comments:

The basic question when assessing a model's performance is “how good is good enough?”, so I think a more quantitative assessment needs to be based on what inverse models are required to provide based on the atmospheric observations. So one

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should start with the requirements for inverse modelling of CO₂ and CH₄ (may be also N₂O) to provide surface-atmosphere fluxes at a specific spatiotemporal resolution with a targeted uncertainty, and relate these to the required model-data mismatch.

Regarding the N₂O results: The fact that N₂O column variability is dominated by the variability in the stratosphere should have stimulated the authors to take a closer look not only at effects from tropopause height variations, but also to effects from photochemistry and stratospheric circulation. As shown in Figure 6, partial columns of N₂O in the stratosphere indeed show a large spatial variability. Thus errors in stratospheric circulations or in photochemistry will result in significant model-data mismatches. To differentiate these from tropopause height effects, the authors should have a look at comparisons between simulated tropopause heights in ACTM with those derived from radiosondes.

Specific comments:

P 5683 L19: What was the spin-up period for the simulated tracer fields? This should be specified.

P 5683 L25: Using a single year for fluxes at monthly resolution for a four-year transport simulation is likely to introduce errors (diurnal cycle, interannual variations), those should be discussed.

P5685 L2: It should be mentioned whether the dry air column abundance is taken from the model or from the observations.

P5685 L5: some explanation on the potential origin of the offsets for N₂O and CH₄ would be appropriate

P5685 L15: Why does the integral over dp not include the water vapour pressure? It is unclear to me how this can result in dry air partial columns.

P5685 L17-L19, table 1: According to the definition of the bias b and the RMS difference d , d should always be larger than the absolute value of b . However, table 1 shows

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several sites where this is not the case. Is it possible that the authors used the standard deviation of the difference rather than the RMS difference?

P5690 L13: As the CASA model is used for a priori fluxes of the inversions, it is not sufficient to say that the coarseness of the flux regions is to blame. In addition the CASA model does not seem to capture those variations at the regional scale. The same also applies to the discussion of the seasonal cycle phase at BIK and ORL. This should be discussed a bit more in detail.

Technical comments:

P5684 L24: replace "Rogers" by "Rodgers"

P5684 L28: ad subscript j to x_a

P5685 L13: the "x" in the integral should be capitalized

Figure 3, caption: symbol colours in figure do not agree with those in the caption

Supplement, Figure S4: The caption reads "Figure 4" instead of "Figure S4"

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5679, 2012.