Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-352-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing stratospheric transport in the CMAM30 simulations using ACE-FTS measurements" *by* Felicia Kolonjari et al.

Anonymous Referee #2

Received and published: 20 June 2017

This manuscript describes a detailed set of comparisons between specified dynamics simulations with the CMAM chemistry climate model and satellite-based observations of stratospheric long-lived tracers, with the goal of identifying discrepancies and therefore errors in the simulated stratospheric dynamics. The topic is suitable for ACP, and the conclusions reached by the authors are generally reasonably well supported by the analysis presented.

General comments:

1. I have no doubt that the "advanced" comparison technique–which samples the model data along the actual line of sight of the satellite instrument–is the best way of minimizing sampling errors in the CMAM vs. ACE-FTS comparison. But, the explanations given in Sec. 3.3 don't quite make sense to me. If the difference between the

advanced and intermediate methods is so small, as shown in Fig. 4c, this implies that the "line of sight" sampling is actually making very little difference to the sample means. The difference between advanced and basic (fig 4b) is much larger, which means that the most important source of sampling error has to do with a bias in the distribution of samples within each 5deg latitude bin (which is fixed by performing the 2D horizontal interpolation rather than using the closest neighbor gridpoint). It's possible these issues would be easier to sort out if Fig 4 showed differences between the 3 methods and the full model sampling, rather than differences between the advanced sampling and the other sampling methods. Or perhaps BASIC-FULL, INTERMEDIATE-BASIC, and ADVANCED-INTERMEDIATE. In any case, I believe the logic of the explanations here could be sharpened.

2. No doubt there is much more information given in the Ray et al. (2016) paper, but Sec. 6 requires a little more guidance on the set up of the TLP simulations. At some point, in passing, we learn that there were 480 TPL simulations, but it is not said how these simulations differ; presumably certain input assumptions are varied in the different simulations, but not, it seems, w* and epsilon directly. Also, the terms w* and epsilon should be better defined. At some point, w* is introduced as a mean tropical upwelling, but it is later used to quantify vertical motion in the extratropics. It's also not really clear if epsilon is a prognostic or diagnostic variable, and how it depends on height, latitude, time, etc.

3. I have trouble following the logic from Figure 15 to 16. Figure 15 seems to show that best agreement with the ACE-FTS measurements is achieved running the TLP model with parameter settings which produce the smallest w* and largest epsilon values (at least in Fig 15a,b,c). In fact, it seems that the range of TLP simulations is not large enough to find the actual best agreement with ACE-FTS–a point which could be discussed. But then, in Figure 16, it is implied that e.g., best agreement with ACE-FTS is produced with no significant change in w* values in the tropics. Something seems inconsistent here.

4. For many of the difference contour plots, it would be helpful if the colorbars were chosen such that positive differences could be more easily differentiated from negative differences. An example is Fig 6, where it is very difficult to know whether the CMAM-ACE differences in the UTLS are positive (like the middle stratosphere differences) or negative.

Specific comments

P1, l11: “The model consistently. . .” could be taken out of context–this conclusion is specific to the trace gases examined in this study (and probably wouldn’t apply to ozone, for example).

P1, l14: the “too little isentropic mixing” should probably be connected if possible to a height or range of heights.

P2, l1: I’m not sure this is the only reason for the increase in interest in stratospheric transport–and it’s a bit of a chicken and egg problem.

P2, l4: *Accurate* projections rely on good models.

P2, l5: Definitely the distribution of long-lived trace gases depends on the BDC... for short-lived species it may not have that much influence.

P3, l14: “has” or “have”? The word choice depends on whether the models project changes in the BDC (plural) or evidence of those changes (singular).

P4, l31: ACE-FTS measurements have high vertical resolution, not the instrument itself.

P8, l2: For comparisons of model to measurements, it’s probably more intuitive to treat the measurements as the truth, and show relative differences of the model with respect to the measurements, rather than the mean of the measurements and model. It’s of course not a big deal as long as it is clear how the calculation is being done, but I feel the simpler the calculation, the easier it is to interpret.

P8, l8: this statement of significance applies to the 1 sigma confidence level. For 2 sigma, I guess all differences would be not significantly different from zero.

P9, l24-26: Apparent contradiction between “increased isentropic mixing” and “slower shallow branch”.

P12, l19: the end of this sentence could be misconstrued, as of course there has been vertical interpolation applied in the translation to the vertical levels of the ACE-FTS retrievals. I would suggest to remove this last part, and write “. . . location of the tangent points with altitude”.

P13, l25: But the INTERMEDIATE sampling technique is also limited to the 30 km tangent altitude, and the differences to the advanced method are much smaller. Indeed, by construction, differences between a method using the variable tangent heights and one using only the 30 km tangent height should be zero at 30 km (which appears to be the case in the advanced-intermediate comparison). Therefore, the differences between basic and advanced, which are strongest around 30 km, cannot be due to the line of sight sampling.

P15, l26: how are large disagreements in the north polar region so confidently connected to problems with tropical upwelling? Could this not be an issue with mixing across the polar vortex?

P22, l17: I would avoid the term “mixing levels” as it could be taken as meaning isentropic surfaces.

P22, l19: Is this result based on looking at the best agreement between CMAM and ACE-FTS under the natural variability of the CMAM simulations? Located here within the discussion of the TPL, it comes across a little as one has tuned CMAM, which I think is not the case. Also, “a reduction from the fitting estimate” is unclear to me, is this the best fit of the TPL parameters to the CMAM climatology?

P22, l21,22: First sentence implies best agreement when epsilon is increased–which, based on previous description I take to mean a mixing rate should be increased. But the following sentence says mixing “times” should be increased, which actually means rates should decrease. Some clarification here would be useful.

P23, l5: Figure 15 is quite dense, and really could use better description in the main text and figure caption. The “level of agreement” between CMAM and ACE-FTS needs to be explained fully, what kind of quantity is this? It took me some time to determine that the white-to-black shading and the white isolines were describing the same quantity. Also, there are bluish boxes which are barely detectable in the plots, are these the “agreement matrices”? As mentioned in the general comments, these agreement boxes don’t seem consistent with the ACE-FTS “agreement” scale.

P23, 19: How are these thresholds chosen? 0.65 seems like a rather lenient agreement threshold, since the agreement values shown in Fig 15 go as low as 0.1.

P23, l20: what chemical species is Fig 16 based on?

P24, l32: If mixing and the meridional residual circulation are both driven by Rossby wave breaking, it is hard to see how insufficient mixing could be the cause of a too-rapid BDC.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-352, 2017.