

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

Anonymous Referee #3

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In this paper, the authors have used numerous techniques to compare a nudged CMAM model run with long-lived tracer observations from ACE-FTS. Their sampling technique is excellent and allows for a like-to-like comparison, as much as possible when comparing model output and data. The use of the TLP model as well as the tracer-tracer JPDFs are appropriate. I think this paper is appropriate for publication in ACP, subject to addressing a significant number of concerns about the dynamical interpretation and discussion of the results. I therefore waver between minor and major revisions. Only one of my comments requires additional calculations.

Generally, the following need to be addressed: 1) Improved discussion of BDC 2) Improved discussion of pathways for mixing across the tropopause and if feasible, 3) More specific discussion of the implications of these results for model development

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P3L12: Hardiman et al. (2017) found a time of emergence for a trend of 30 years and showed that any trend less than 12 years could be the wrong way due to dynamical variability. The results of Mahieu and pretty much all of our observational records are too short. P3L13: I don't think there are fundamental questions about the mechanisms driving the stratospheric circulation. The mechanisms driving changes to the circulation are less clear, but the fact that data don't show the trend predicted to emerge from models over a much longer timescale is not surprising in light of the results of Hardiman et al. P3L16: Is it true that understanding how the structure of the BDC will change depends greatly on the ability to simulate its current behavior? The models examined by Butchart et al. 2011 have pretty different mean upward mass flux at 70 hPa, but the community still interprets their agreement on the strengthening of the BDC as robust. Getting the mean and present day right are important, but not necessarily for the trends. P3L16: "Typically"—please provide some citations demonstrating how typical.

P4L11: You haven't defined CMAM30 before. P4L13: I would appreciate some discussion either here or in 2.2.2 of the potential problems with CMAM30. In particular, the model is nudged to reanalysis. As far as I am aware, neither the nudging process nor the reanalysis itself conserves mass, energy etc. Are there any studies that show how that influences tracers? Are your tracers transported conservatively and do their budgets close? I'm not necessarily suggesting you calculate the tracer budgets, though such analysis might be interesting, but please address these concerns to whatever extent you can. P4L17: "morphologies" of CFC11, CFC12 and N2O.

P6L19: the model isn't being constrained to follow observations. It's being constrained to the reanalysis, which is a model-data product that is our best guess at a representation of reality.

Section 2.2.4: How does CMAM BDC compare to ERA-I BDC? Mean tropical w^* at a few levels would be sufficient. The speculation in this section is not necessary when you can do direct comparisons. Additionally, this section would benefit from considering the extratropical vs. tropical age difference (e.g. Neu and Plumb 1999, Linz et al. 2016)

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rather than just talking around the relationship between the age and the circulation. For example, the near-zero differences in 2a between 50S and 50N do not mean that the lower branch of the BDC is the same in the two simulations because the polar age on the same level is older in the free running model. This discussion would be aided by the conversion to isentropic coordinates. This section is one place to address general comment 1) above. E.g. “filtered out close to the tropopause” could be explained in terms of the physical mechanisms of wave propagation (Charney Drazin).

P12L11: “CHAM”→ “CMAM” P12L30: “Air in the polar vortex ... representative of older air ...” The terminology “representative of” is confusing to me. Isn’t air in the polar vortex composed of a larger fraction of older air transported from upper levels? P12L34-35: If the variability of the vortex edge is responsible, why is there so much difference in the middle of the vortex, and why is the Southern hemisphere, where the vortex variability is much weaker pretty comparable to the Northern hemisphere?

P13L17: No comma before between.

P14L5-9: I found this section strange. “readily observed” where? The other information seems redundant. Perhaps just remove all together? P14L16-17: Redundant—rephrase. P14L17-18: “Likely caused by significant differences in the conditions of the influence of downwelling...” This is confusing. Please rephrase or explain further. There is more downwelling in the SH vortex and you’ve mentioned later that N2O has a source higher up, so shouldn’t there be more N2O in the SH vortex than the NH vortex?

P15L11: Another place to address 1). The BDC is strongest in the NH winter because of the climatological westerlies and the enhanced wave driving from the troposphere both. If there were more waves in the NH summer, they wouldn’t do any good because they can’t propagate up into the stratosphere when there are climatological easterlies. P15L28: not sure what you mean by “robust”

P16L1: “significantly” is confusing. Significant with respect to what? P16L5:

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“particular”→“particularly” P16L5-6: Please explain more why these differences would be due to the polar vortex behavior. I agree with you, but a discussion of the mechanism would be helpful.

P17L4: no commas offsetting “with a stratospheric sink” P17L26: no “was” before “passed”

5.2: This discussion needs to be revised. Specifically, please review the literature that treats the tropopause as a “barrier”, review the recent literature on transport across the tropopause (Randel 2017 tropospheric dry layers or Randel 2016 asian monsoon transport, for example), discuss the difference between what has been defined here and the more typical treatment of stratospheric intrusions (tropopause folding events that cause deep stratospheric intrusions—see work by Meiyun Lin, for example). Compare to stratospheric intrusion climatology (Skerlak et al. 2014). Finally, please validate your method for defining intrusion events by looking at the colocated water vapor and ozone concentrations in the model. (Or other stratospheric tracers, you could use PV.)

P20L19-20: Which differences and how? I must have missed the discussion previously, so a brief repetition here couldn’t hurt. P20L25-6: Have you demonstrated this? If so, how?

P21L1: add “timescale” after transport. P21L7: This needs to be more specific. How were the turn around latitudes determined? Were they monthly mean or instantaneously calculated? P21L18-9: Wording is informal P21L23-5: You’ve already talked about Fig. 14, so why is this here?

Section 6: More discussion of the TLP would be useful. I am very familiar with it, but Eric’s paper is complicated, so a brief discussion of why it’s great and useful here would be helpful for the average reader who isn’t going to want to read through his whole paper. P22L14: “mixing levels” I thought it was mixing efficiency. P22L17: Be more specific—e.g. “As both the residual circulation and the mixing are driven by

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wave breaking, a weaker residual circulation likely correlates with less mixing and thus longer mixing timescales”

P22L25-6: The implication here and elsewhere in this section is that there is some knob to turn to “change” w^* or epsilon. There obviously isn't, and so while this paper has diagnosed that the mixing is too weak in JJA, for example, it hasn't come close to determining changes required in the CMAM30HR simulations to match the observations. Certainly changing the language here and P23L14, L29, P24L7 is necessary. If feasible, some discussion of what does set w^* (epsilon is probably harder) would be great. If anyone has looked at EP flux divergence esp. broken down by wavenumber, even in CMAM and not necessarily CMAM30, that would be a great thing to discuss here. The authors have done plenty to warrant publication and so they don't need to do it if it hasn't been done. Regardless, some discussion of what does drive the circulation and the mixing in the model would be appropriate here. P24L23: If you've proven that the BDC is too rapid, say so here. As far as I recall, you said that it might be too strong. P24L33-4: Nonsense. Insufficient mixing does not cause any changes to the BDC (at least to first order—second order effects on ozone and the corresponding heating due to ozone might be minor but that has definitely not been addressed in this paper).

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