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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The effect of atmospheric nudging on the stratospheric residual circulation in chemistry-climate models" *by* Andreas Chrysanthou et al.

Anonymous Referee #2

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In this paper, the authors compare the free running and specified dynamics simulations of the CCMI models. They find that although the nudging involved in the SD does make the models short term variability line up, the residual circulation in the nudged runs matches neither the original models nor the reanalyses to which the models are nudged.

This paper is important. It is interesting and provides a cautionary tale against using these SD runs to examine transport of long-lived trace gases, which one would expect would be significantly impacted by these inconsistencies. It is well reasoned and well written. I recommend publication following some minor revisions. An expanded

introduction would be extremely valuable to the reader, and I have included recommendations for that below. The primary scientific points that would add significantly to the paper are:

1. Discuss the choice of metric for the BDC in more detail. (Line 191)

2. Reconsider the results of the multiple linear regression in light of the importance of the relative phasing of the annual cycle and the QBO.

3. Consider the differences between the models and the reanalyses they are being nudged towards, especially vertical profiles, to try to better understand why the results are what they are.

Details below: 1) Recent work (Abalos et al. 2015, Linz et al. 2019) has raised the issue that the differences due to different calculations of the residual circulation are as significant as (or more than) the differences between the reanalyses themselves (c.f. Seviour et al. 2012, also). Therefore, I would appreciate more discussion of the choice made here to use the TUMF and downward control (which is used to distinguish the different wave forcing components). How do these choices impact the results, if at all? A thermodynamic calculation might be very different, since the temperature is nudged, which will introduce spurious diabatic terms. How is TUMF different from w* averaged between turnaround latitudes?

2) Although the method used here to do the multiple linear regression is somewhat standard, it does not actually remove all of the variability that is related to these signals. In particular, the relative phasing of the QBO and the annual cycle will cause an enhanced wintertime upwelling in some years, so the result that the leftover variability is highly correlated could be related to this. Randel et al. 1999 have a nice treatment. I would consider using the method in that paper (described on p. 458, see also Randel and Wu 1996) to determine if the relative timing of the annual cycle and QBO is the cause of the covariation of the nudged time series. This phenomenon is also mentioned in the cited Baldwin et al. 2001 QBO review. 3) The authors find that the reanalyses

differ from the nudged runs which in turn differ from the free running models. I would like to see some presentation of the difference between the mean states of the reanalyses and the mean states of the models without nudging. The result that the models tend to have stronger upwelling in their SD runs could be caused by a systematic low bias in the model temperature in the tropics compared to the reanalysis temperature. The repeated nudging with a primarily positive value would then cause an apparent increase in upwelling, without changing the underlying model physics that are the cause of any systematic mean state bias. To understand why the nudging does what it does, I think the difference between the free running models and the reanalyses needs to be explored in much more detail–perhaps for just one model. A continuous spurious forcing because of a mismatch in the mean state (and likely also in the seasonal cycle) should impact the mean while leaving the interannual variability alone (line 612).

More minor comments below: I find the second paragraph of the introduction to be lacking. Although Abalos et al. (2015) did conclude that there was likely a 2-5%/decade trend in the circulation, they found substantial differences with different calculations for the circulation strength. Ploeger et al. (2019) have a new paper out on the inconsistency of the BDC trends for the different reanalyses, looking at the age spectra. Age of Air is mentioned somewhat abruptly and with no explanation. The results are also misrepresented. Engel et al. 2009 did not find a statistically significant increase in the mean age of air in the midlatitudes. They found no statistically significant change. There are limitations both on the availability of measurements and on the interpretation of age of air as a direct proxy for the residual circulation, and much more recent papers discuss this, rather than Waugh and Hall 2002. There are new satellite based measurements–ACE-FTS (e.g. Ray et al. 2016), MIPAS (Haenel et al. 2015), and MLS (Linz et al. 2017)–and new measurements from Air Core (Engel et al. 2017). The interpretation of the data is also difficult because of the separation of the residual circulation from the mixing (e.g. Garny et al. 2014, Ploeger et al 2015, as mentioned in the first paragraph–perhaps just move this comment to this paragraph?). The only attempt to calculate the residual circulation from tracer data did not calculate the resid-

ual circulation directly, but instead the diabatic circulation (Linz et al. 2017), and they did not calculate trends.

A more detailed explanation of what nudging is, how it is implemented, why it is used, and the motivation for this work would help the introduction. The current paragraph in the introduction (line 85) is good, and I think that including more specific examples would be very helpful. Similarly, in the conclusion, commentary on what these nudged runs could be useful for would be appreciated in addition to the entirely appropriate cautionary words.

Do any of the “free running” models have a nudged QBO, and if so, which ones?

Since the focus of this paper is really on the difference between SD and free running models, it might make sense to omit the models that do not include both. I respect the authors choice to have those comparisons in the supplementary information. However, I think I would find the plots more manageable without the additional lines.

Line 106: aren’t they necessarily inconsistent? Lines 100-129: excellent discussion. Line 153: Do you mean Table 2? Line 155 etc.: More details here would be good. I don’t understand what is meant by T (with wave-0), and I assume that “temperature” is also T (for CMAM), but if that’s true then they all nudge temperature so maybe mention that? Including the sentence “Nudging timescales range from 6 - 50 hours." (or some equivalent) would also emphasize how different these treatments are. Line 159-160: Table 1? Line 168: “primary variable”–not sure what is meant by this Line 442: I have trouble seeing this from the plot. Could you provide the numbers? Is the difference in the peak-to-peak amplitude significant? Line 476...: How do these results relate to Abalos et al. 2014, who found such a strong dependence of the upwelling on the location of the EPFD? Line 488-90: I found this sentence confusing. Line 498: remove “due to” Section 3.6: Nice. Line 606...: This paragraph is very long and contains a lot of important information. Turn into two or even three shorter paragraphs?

Figures: Consider stippling the insignificant part of the plot. I read this on a number of different devices, and on some it looked fine, but on one of them the plots were impossible to see because the colors weren't visible behind the stippling. This wasn't an issue for the last two figures for some reason.

It could be useful to have models that are nudged to the same reanalysis plotted in the same color family to aid interpretation of the complicated line plots.

References: Linz et al. 2019: https://www.atmos-chem-phys.net/19/5069/2019/acp-19-5069-2019.html Randel et al. 1999 https://acomstaff.acom.ucar.edu/randel/JAS%201999.pdf Randel and Wu 1996 https://journals.ametsoc.org/doi/10.1175/1520-0469%281996%29053%3C2546%3AIOTOQI%3E2.0.CO%3B2 Ploeger et al. 2019 https://www.atmos-chem-phys.net/19/6085/2019/ Ray et al. 2016 https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD024447 Haenel et al. 2015 https://www.atmos-chem-phys.net/15/13161/2015/acp-15-13161-2015.html Linz et al. 2017 https://www.nature.com/articles/ngeo3013 Abalos et al. 2014 https://journals.ametsoc.org/doi/10.1175/JAS-D-13-0366.1

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