Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-470-RC2, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Changing transport processes in the stratosphere by radiative heating of sulfate aerosols" *by* Ulrike Niemeier and Hauke Schmidt

Anonymous Referee #2

Received and published: 2 July 2017

Review of "Changing transport processes in the stratosphere by radiative heating of sulfate aerosols" by U. Niemeier and H. Schmidt

This paper focuses on addressing changes in sulfate aerosol transport in the stratosphere in the presence of different states of the quasi-biennial oscillation (QBO). The paper addresses a very important and timely topic, however the conclusions presented are not supported by the figures shown, and there is ambiguity in some of the presented analysis. Therefore, I recommend major revisions before this manuscript can be published.

Major Comments:

1. The methodology of compositing QBO phases, beginning of Section 5.1 is not clear. Transport varies with QBO phase as well as with the month of the year. The discussion in lines 260 – 270 implies that the QBO phase distinction is made for each month (correct), however it seems that in Figure 4, all months with QBOW and all months with QBOE are composited together. The caption does not say anything about which months are used in the composite, so I'm assuming that all months are used. If this is indeed the case, the plot does not show differences between QBOE and QBOW, but it shows those differences as well as differences between compositing different months of the year that coincide with QBOE and QBOE, and therefore does not answer the question that was posed. If the authors are indeed looking at the month of January or July in this Figure (the only way in which the differences between QBOE and QBOW phases can be separated clearly), then this needs to be clarified.

2. The authors in section 6 compare a simulation with 10T(s)/yr carried out with a 90-level (with QBO) and 39-level (without QBO) versions of the model in order to further demonstrate effects of the QBO. However, at no point in the manuscript do the authors show or discuss that properties of the models critical to aerosol transport are the same between the 39 and 90-level versions (except the QBO). For example, is the mean Brewer-Dobson circulation the same between these models? Is the dissipation from planetary waves (that directly impacts mixing) the same? What is the strength of stratospheric DJF and JJA jets? Are the temperature anomalies due to injections the same in both models? Without these aspects of the model demonstrated to be the same (or very similar), it is not clear here whether we're looking at differences due to the QBO or differences due to other model differences. I suggest that similarities between the aspects of the models mentioned above are shown in the appendix.

3. Interpretation of differences between simulation 10Tg60 and Geo10 I and 10Tg30 (Figures 7 and 8) is not clear and very confusing. There is no convincing explanation in the text to account for the differences in AOD between the 3 simulations shown in Figure 7. There is an overall increase in aerosol number density in the accumulation mode

in Geo10 as compared to 10Tg60 (Figure 8), however if the contour level is adjusted to show the maximum contour, it could be that the distribution with latitude of aerosols looks very similar to that as in 10Tg60, and the overall change is due to something other than QBO winds. Secondly, if the westerly QBO is inhibiting transport out of the tropics, why doesn't this apply to 10Tg30? There are plenty of aerosols in the accumulation mode in the extratropics and the QBO is in an even stronger westerly phase. Besides, if QBOW intensified vertical transport as mentioned earlier in the manuscript, why is the distribution of coarse mode particles in Figure 8 for 10Tg60 (bottom center panel) confined to a smaller vertical region as compared to that in Geo10 (left bottom panel). This plot alone implies higher vertical velocity at the equator in Geo10 as compared to 10Tg60. Again, this difference could be the consequence of different model dynamics, wave breaking and BD circulatoin due to differences in vertical resolution, and not to the QBO. Authors state is line 360 that the number density in the coarse mode is lower in Geo10 then in 10Tg60. I don't see that from Figure 8 – that maybe true right at the equator in a very small region, but overall the number density in the coarse mode if anything is bigger in Geo10.

4. Interpretation of differences in the text between 10Tg60 and 10Tg30 are also not consistent with the figures. For example: Lines 350-351: The difference between 10Tg60 and 10Tg30 is primarily the injection altitude and not the tropical wind system From Figure 10Tg60 shows that likely the aerosol transport out of the tropics occurs via the lower branch of the BDC, where in 10Tg30, the aerosols are transported via mixing and the upper branch of the BDC (see also minor comment 1).

Lines 366- 367: 'Figure 8 indicates low meridional transport resulting in low particle number densities in extratropics in 10Tg30.' This is not at all consistent with top rightmost panel in Figure 8: there are plenty of particles in the extratropics.

5. In order to explain the differences between AOD in the 3 simulations shown in Figure 7, it would be helpful to plot effective radius of particles and surface area density, instead of what is currently in Figure 8.

Minor Comments:

1. Line: 77-78: "This quasi-horizontal mixing is the main transport branch for sulfate aerosols'. This statement highly depends on the location of injection of the aerosols. There are three main ways the aerosols can be transported out of the tropics: a) The deep branch of the BDC, the shallow branch, and horizontal mixing. Aerosols injected right above the tropical tropopause are mostly going to be transported with the shallow branch of the BDC, those injected several kilometers above the tropopause will likely be primarily transported with the upper branch of the BDC. Some will be transported horizontally by mixing. I suggest that a discussion of the different branches of the BDC is added and how the location of injection (30 hPa and 60 hPa discussed here) affect which branch of the BDC is the primary transport mechanism. Figure 1 of Bonisch et al. 2011 has an excellent graphic (Atmos. Chem. Phys., 11, 3937–3948, 2011 www.atmos-chem-phys.net/11/3937/2011/ doi:10.5194/acp-11-3937-2011)

2. Line 92: An average period of the QBO is 28 (not 29) months.

3. Figure 1: Why isn't the QBO included here for the 4 Tg 30 hPa injection? Please include it.

4.Temperature anomalies in rightmost panel of Figure 2 clearly exceed the colorbar. Please change the colorbar so the maximum and minimum temperature anomalies are clear in all the panels. Please in the text also include the amplitude of maximum temperature anomalies for the simulations in Figure 2.

5. Line 203-204: 'Positive anomaly does not extend to the pole . . . because polar vortex blocks the transport' - it would be helpful to overplot the aerosol concentrations here to demonstrate this point clearly.

6. Why is there such a strong negative temperature anomaly near 5 hPa at the equator in the 8Tg30 hPa simulation? (Rightmost panel of Figure 2)

7. Figure 3: The color-scale is inappropriate. It is impossible to see what are the zonal

wind velocities in the top panels as well as anomalies in the bottom panels. Both clearly exceed the color scale. Please correct.

8. Figure 8: Again here, the colorbar needs to be adjusted that it is clear what the maximum contour is in the top leftmost panel.

9. Line 445-447: That is too strong of a conclusion! Injecting at 30 hPa and other location could be viable at other latitudes – only equatorial injections have been shown here, hence authors should not make such a sweeping conclusion.

10. Lines 457- 469: I'm not sure how this paragraph is relevant to the main point of the study. If the authors chose to keep it, please explain how you arrived at the injection estimates up to 2100 mentioned in line 460 and 462.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-470, 2017.