Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1104-RC1, 2020


Atmospheric Chemistry and Physics Discussions
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# *Interactive comment on* "Differences in the QBO response to stratospheric aerosol modification depending on injection strategy and species" *by* Henning Franke et al.

**Anonymous Referee #1**

Received and published: 7 December 2020

This paper documented changes in QBO in responses to stratospheric aerosol modification simulated in ECHAM and WACCM. The authors compared three injection locations, two injection rates and two injection species. It is found that the QBO is strongly disturbed when aerosols are injected at the equator or evenly over the tropics, but not when aerosols are injected at two subtropical points, and H2SO4 has a stronger effect on QBO than SO2. The two models simulated different responses in QBO for the region injection case, which is attributed to the ozone changes. The authors explained these difference by the thermal wind relationship and linked the wind shear to the temperature curvature. This paper provides a thorough discussion on how geo-engineering affects QBO. It is logically organized and clearly written. I do have one major concern



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Discussion paper

regarding the interpretation of the thermal wind relation. I recommend the publication of the paper after the authors address this comment.

Major comment:

The authors attributed the equatorial winds to the temperature curvature, and argued that aerosols drives temperature changes which then drives wind changes. There is no doubts that the thermal wind relationship is valid, but it is important to realize that the thermal wind relationship itself does not guarantee a causal relationship. The agreement between temperature curvature and wind shear shown in the paper is not sufficient to prove that temperature changes drive the winds. This is particularly the case for levels above ∼20 hPa, which are well above the aerosol layers so that the direct radiative effect from the aerosol should be quite weak. Yet, these upper level changes are key to the QBO changes in many cases. An example would be the ECHAM region-so4-5 vs region-so4-25 (Fig. 12 f vs h). These two simulations only differ in the magnitudes of the aerosol injection, but the changes in temperature gradient are opposite above 20 hPa, which suggested to me that the temperature changes there are not directly related to the aerosol's radiative effect. It may be helpful to plot the changes in the radiative heating rate from the aerosols (and ozone).

Minor comments:

Line 96-97: The radiative heating of the sulfate aerosols consists of long wave and near-infrared radiation, but aerosol properties are only passed to shortwave and near infrared radiation. Is the aerosols' long wave radiative effect ignored?

Line 118: These AMIP simulations cannot simulate the surface cooling by the sulfate layer as most of the surface temperature is constrained. But they can be used to estimate the forcing.

Line 207-210: T/Lˆ2 basically provides a scaling for the temperature curvature. So the equation (3) may still hold if choosing a correct "L".

Line 251: Fig. 2 does not show the comparison between the two injection rates.

Line 397-401: The authors attributed the ozone increase above the aerosol layers to the changes in NOx. I am not sure how NOx changes at 10 hPa or higher where there is almost no sulfate aerosols. It is also interesting to compare the CESM-region-so4-5 vs CESM-region-so4-25 (Fig. 14 b vs d). The aerosol layer is much deeper in the latter case, but the vertical structure of the ozone changes does not seem to vary much between the two. Also, does the temperature dependence of the reaction rates play any role here?

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