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Interactive comment on "Climate System Response to Stratospheric Sulfate Aerosols: Sensitivity to Altitude of Aerosol Layer" *by* Krishnamohan Krishna-Pillai Sukumara-Pillai et al.

Anonymous Referee #3

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Review of

"Climate system response to stratospheric sulfate aerosols: sensitivity to altitude of aerosol layer" by Krishnamohan Krishna-Pillai Sukumara-Pillai, Govindasamy Bala, Long Cao, Lei Duan and Ken Caldeira.

General Comments

This is a well-structured paper which presents its results clearly, is well written with clear figures. The dependence of the amount of surface cooling on the altitude of the aerosol layer has been shown before (e.g. the work of Tilmes et al. [2017] referred to by the authors) so this work falls into the category of "confirmatory" rather than

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"groundbreaking" work. My main concern relates to the ability of their model to simulate stratospheric dynamics well enough to have confidence in their results - see Specific Comment 1.

Specific Comments

1. Page 4, Section 2.1, with implications throughout. With a top at 3 hPa (c. 40 km) and 26 layers in the vertical the model is both "low top" and "low vertical resolution". This leads to concerns about how well the model represents stratospheric dynamics and therefore how much confidence can be had in any results based on such dynamics, such as the amount of water vapor entering the stratosphere (page 7, lines 5-11; page 10, lines 5-10; page 12, lines 12-14) and changes to stratospheric circulation (the whole of Section 3.4). It is not surprising that, as the authors admit, their model does not produce an internally-generated QBO, but one is left wondering how well the model simulates the Brewer-Dobson circulation. Some validation of the model's Brewer-Dobson circulation against observations is required in order to justify confidence in the results.

2. Page 4, lines 26-27. The manuscript at present simply states "The zonal variations as well as interannual variations (for this study) in mixing ratio of the volcanic aerosols are ommitted". Although they do make this clearer later in the Discussion/Conclusion, it needs to be made much clearer here that this means that their model includes no aerosol transport, deposition, microphysics or chemistry - that the aerosol layers are simply represented by fixed, globally-uniform values.

3. Page 7, lines 18-19. What the authors call the "burn-off effect" with reference to Ackerman et al. (2000) is completely irrelevant as an explanation here. Ackerman et al. examined the impact on boundary-layer trade cumuli of low-level soot. This has no bearing on the reduction of upper-tropospheric cirrus cloud being discussed at this point.

4. Page 12, lines 8-9. The authors again use the term "burn-off effect" but this time with

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reference to Visioni et al. (2018). The term again seems inappropriate as Visioni et al. explain the thinning of high-altitude cirrus clouds in terms of an increase in atmospheric stability and thus a decrease in turbulence and updraft velocities - nothing about "burn-off".

Technical Corrections/Comments

1. Page 4, line 13: the number of model layers in the stratosphere should be given.

2. Page 7, line 22-23: the text currently reads "...leads to an increase in low cloud for the Volc_100hPa case relative to the Vol_70hPa and Volc_35hPa cases..." This is not incorrect, but I think it would be clearer to say "...leads to less of a decrease in low cloud for the Volc_100hPa case compared with the Volc_70hPa and Volc_35hPa cases..."

3. Page 11, lines 25-26: "autotrophic" is misspelled as "autotropic" three times.

4. Supplementary material, page 6: the caption to Figure S1 should explain what is shown in each of the panels (a) to (f).

5. Supplementary material, page 12: the term "1XCO2" is used in the caption to Figure S7 and has been used throughout the paper, but "CTL" is used in the titles of the individual panels; consistency would avoid any confusion.

6. Supplementary material, page 13: the values plotted in Figure S8 are presumably global-means?

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