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Interactive comment

Interactive comment on "Upper tropospheric ice sensitivity to sulfate geoengineering" *by* Daniele Visioni et al.

Anonymous Referee #1

Received and published: 15 March 2018

This is an interesting and timely study of how geoengineering in the form of stratospheric aerosol injection (SAI) would impact ice clouds in the upper troposphere (UT). Few papers have been published on this poorly understood aspect of SAI, so in that sense the paper is a welcome contribution to the geoengineering discussion. However, the paper is in its current form very unclear when it comes to the representation of central processes in the ULAQ model, poorly structured, and full of incorrect/poor grammar. It need a serious overhaul in all these respects. I further question the validity of the results presented, in light of the coarse resolution of the model, as well as its overly simplistic treatment of UT ice nucleation. I challenge the authors to justify why their results can be trusted despite these shortcomings. Below I've listed some additional major concerns I had about the paper, and thereafter some minor

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comments (typos, questions for clarification, etc.). I would like to see all of these concerns addressed before I will consider the paper suitable for publication in ACP.

Major comments:
1) With respect to the paper structure, I found it strange that in Section 2 ("ULAQ-CCM and setup of numerical experiments") some model results in response to SG are presented (in Fig. 3-6), but not until sections 2.1 and 2.1. are descriptions of the model treatment of stratospheric aerosols and ice clouds described. I suggest moving the presentation and discussion of model results until AFTER you've described the model, and to only put content in the various sections that is consistent with the section titles.
2) There is no discussion of the effect of additional SO4 available for homogeneous nucleation in the SG cases, as evident in Fig. 7a. Why is the effect of what appears to be a tripling of SO4 particles that can nucleate ice seemingly negligible? Please explain?

Minor comments:
Abstract: "Goal of. . ." should be changed to "The goal of.."
Abstract: Don't understand what "coupled to" means in this context.
Page 2, line 4, End sentence after "documented.
Page 2, line 9: Add "optimal" before "magnitude and location".
Page 2, line 23: Add reference for the claim that homogeneous nucleation normally dominates cirrus.
formation. "Supersaturation ratio" should be "saturation ratio".
Page 2, line 31: "anyway" is not suitable here. "However" could be an alternative.
Page 2, line 32: cloud optical properties are also important here.
Page 4, line 1: This statement is confusing: sulphuric acid droplets are not ice nuclei. Please clarify.
Furthermore, this statement is not very interesting unless you explain WHY there was no effect on RF. Page 4:Vertical velocity is important for cirrus formation not primarily

because it transport water vapour to the UT, but because it controls the adiabatic cooling rate and thus supersaturation, for a given water vapour content.

Page 4, line 14-17: Catastrophic grammar.

Figure 1: This figure is confusing and not well explained. I don't find it particularly helpful at this stage of the paper, but it could be good as a final figure summarizing the findings in the paper.

Page 5, line 5: Ash dust is not the same.

Page 6, line 1: Sassen et al. (2008) is a paper on cirrus coverage seen by CALIPSO, so I don't see how that could possibly address UT ice changes.

Page 6/Table 1: The horizontal resolution is extremely coarse - how can we have confidence in changes driven by dynamics in this context?

Page 7, line 4: Clumsy and confusing statement. Suggest writing: . . .a negative anomaly in the Arctic region that is approximately 1 K larger than that of high southern latitudes.

Page 7, line 7: What do you mean by "increasing atmospheric stabilisation"? Do you mean "increasing atmospheric stability?

Page 7, line 16-18: The Antarctic warming is not statistically significant, so I don't see the point in discussing it.

Page 8, line 2: Is the vertical velocity change mainly caused by changes to TKE, or also due to large-scale (resolved) velocity changes. If TKE is very important here, I would like to see vertical profiles of TKE for both simulations.

Page 9, lines 19-20: Discuss here the uncertainty associated with cloud ice in MERRA, which uses highly uncertain cloud parameterisations and incorporates very few ice cloud observations in its reanalysis. It would be better to use CALIPSO/CloudSat retrievals of ice cloud properties.

Page 10, line 3: Given how central UT vertical velocities are to this paper, you need to be clearer about how the calculation of vertical velocity is done, i.e. include equation for vertical velocity as a function of TKE, and clearly state if you put any upper/lower bounds in it.

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Page 10, line 4: What justifies the assumption that cirrus clouds form only via homogeneous nucleation? That seems to be in stark contrast to papers that report that cirrus clouds appear to form mainly through heterogeneous nucleation (e.g., Cziczo et al., 2013).

Page 13, line 15: Remove “from”.

Page 16, line 13-14: This is inaccurate - homogeneous nucleation sets in at approximately 238K, but NOT through "water vapour freezing", but rather through the spontaneous freezing of small solution droplets.

Page 16, line 18-19: This is an outdated view (and references that back this claim are not provided) - the current understanding is that a majority of cirrus clouds form via heterogeneous nucleation.

Fig. 8: Again, I do not think of MERRA as the most appropriate data set for validation of the simulated UT ice.

Page 16, line 21 (and throughout the manuscript): The standard terminology is “ice mass mixing ratio”, not “ice mass fraction” which can be misleading.

Page 16, line 8 - 15: The description of how UT ice clouds form is extremely unclear. How is cloud cover determined? What probability distribution for supersaturation is used, and how does it relate to TKE. A lot of essential information is left out here.

Page 18, line 4: “each thick” is not correct English.

Page 18, line 8: What do you mean by “we are only considering sub-visible clouds”?

Fig. 10: Is the ice crystal number density calculated only when there is a cloud (i.e. an in-cloud average), or is this an average over both cloudy and cloud-free grid-boxes? The former quantity is certainly of most interest and more directly comparable to field measurements.

Page 18, line 10-11: Neglecting heterogeneous ice nucleation would lead to an overestimate of ice crystal number, because you are not able to represent the competition between heterogeneous and homogeneous nucleation that will in some cases lead to a suppression of homogeneous nucleation and therefore a reduction in ice crystal number density. In other words, that cannot explain the disagreement with

MERRA+MODIS seen in Fig. 9.
Page 26, line 7-9: How can you be confident about the radiative effect when the model consistently produces ice clouds that are optically too thin? This could bias especially the LW cooling effect of cloud thinning.
General comment: Friberg et al. (2015) seem to qualitatively support your findings based on analysis of cirrus cloud reflectance changes after volcanic eruptions, so that would be a good paper to cite.

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