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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solar geoengineering using solid aerosol in the stratosphere" *by* D. K. Weisenstein and D. W. Keith

Anonymous Referee #1

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First I should say I have not evaluated the ozone depletion part of the paper as this falls out of my expertise, and have focused instead on the physical part of the study. I confess I am not very knowledgeable in fractal aerosols and have not checked the details of the authors' treatment in this study.

Independently of what one may think of the idea of injecting alumina particles in the stratosphere, I think this is, scientifically speaking, a good study. The limitations of the model used (simplified injection mechanism and lack of a plume model, 2D modelling, geometry of aggregates, ozone chemistry and missing feedbacks on the stratospheric circulation and ozone distribution) are highlighted and well discussed in Section 4. The conclusions are clearly explained and the results make physically sense.

I have a few major comments:

The number of monomers in a fractal particle is always a power of 2 as a consequence of the sectional representation that doubles the number of monomers in successive bins. Yet fractal particles of different sizes can coagulate and produce particles with any number of monomers. How is this treated in the model? How much of an assumption is this? Actually fairly little is said on the coagulation scheme for coated and uncoated particles. As this is a new development, it would be useful to describe it in an Appendix. Likewise a better description of the other aspects of the scheme like condensation of sulphuric acid on the particles is needed.

It seems that all "radiative forcing" estimates are for SW effects only (at least this is what I understand from page 11816, lines 10-12). If this is the case, this should be made explicit and justified. This said neglecting the LW (positive) radiative forcing is hardly justifiable given that it can vary significantly between the different particle types (as the authors explain themselves).

The paragraphs on heating rates are also unclear and possibly incorrect. Heating is caused by both absorption of SW and LW radiation. It seems odd that the authors only consider the latter (at least this is the impression they give). Also the LW heating rates can be positive (heating) or negative (cooling) depending on the aerosol and temperature vertical profiles whereas the authors seem to associate the interactions of aerosols with LW radiation to a systematic heating of the stratosphere. Please clarify.

There are a number of notations and units that need to be clarified (as discussed below).

Specific comments

Page 11800, line 12: sentence is a little unclear (maybe "yet" should read "although")

Page 11801, line 5: The study of Ferraro et al (GRL, 2011) could be cited here.

Page 11802, line 7: and also “cirrus formation” if the dynamical effects propagate in the upper troposphere as some models suggest.

Page 11802, line 11: a citation to Mercado et al (Nature, 2009) or an earlier paper would seem more appropriate here.

Page 11806, line 3: I am sure the explanation is somewhere in the cited literature, but could you explain why mass is proportional to $R_g^{D_f}$. A diagram might help to understand.

Page 11807, line 9: what is R? it has not be defined previously. Or do you mean R_g?

Page 11807, line 10: is N the same as N_i defined previously?

Page 11807, line 11: are you talking about area or surface area projection (as on line 3) here? What is area relevant here?

Page 11807: I do not pretend I understand the details of fractal aerosols very well, so it would be useful if the authors point to limitations in their model.

Page 11810, lines 20-30: does this depend on N_i?

Page 11813: I am not sure what the authors mean when they say “fractals never contain more than X monomers’‘. Surely there must be but in (very) low concentrations?

Page 11816, line 5: the word “significant” is used in a very subjective way here. What is a significant or insignificant amount of diffuse radiation for terrestrial ecosystems?

Page 11816, line 19: scattering becomes negligible per unit mass particle, but not per unit particle. Eventually it depends how much of the mass is in this range of monomers.

Page 11816, lines 27-29: you should say this earlier.

Page 11818, lines 8-9: note that IR radiative effects result in both heating / cooling

depending on the altitude and aerosol vertical profile considered.

Pages 11816, 11817 and 11818, line 24: are these Wm^{-2} of net (SW+LW) forcing or SW forcing only?

Page 11840: change “mixing ratio” to “mass mixing ratio” for clarity on panels a) and b). A mixing ratio is not the same as a concentration, so the caption should say “Mass mixing ratio” and not a “Concentration in ppbm” !

Page 11844, figure 6: I do not understand what is plotted here as the terms used as different from what I am used to. A cross-section is not dimensionless. Is panel (a) showing an upscatter fraction (but the values appear too large)? Or an upscatter cross-section (define) per unit geometrical cross-section? Likewise I am not sure what an upscatter cross-section per unit volume is. This time, it seems the cross-section is not dimensionless as the unit is μm^{-1}.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11799, 2015.