**Comment on “Solar geoengineering using solid aerosol in the stratosphere“**

by D. K. Weisenstein and D. W. Keith

The manuscript describes a novel approach to manipulate the stratospheric albedo to counteract global warming (geoengineering; solar radiation management, SRM) by releasing solid alumina particles into the lower stratosphere. The authors investigate the response of a two-dimensional coupled aerosol microphysics-stratospheric chemistry model to estimate the desired aerosol radiative forcing and attempt to quantify associated risks concerning the potential increase in diffuse light and impacts on the stratospheric ozone budget.

This study is a necessary step towards a better understanding of this particular research topic. The manuscript is generally well written. Although investigating a new type of aerosol in the (higher) atmosphere, the author’s succeed in introducing the necessary background information without overstraining the reader. Methods are explained fairly good, the quality of figures are matching standards of a scientific journal, and the results seem plausible.

I do have, however, a few concerns which should be addressed before the manuscript is published in ACP.

**General comments:**

The current structure of the paper does not allow to get a quick overview about the experiments which have been conducted and are described, before one has read the entire Sect. 3. Even then it remains unclear, because on several places the author’s phrases like „..we perform additional model calculations…“, or „…model…for a number of parametric…scenarios…“, and so on. The paper would substantially gain in quality if the experiments are briefly described en bloc before the results (even the test cases) are addressed. I suggest to introduce such a (sub-)section before the section „Model results“, potentially also including an additional table presenting an overview of what has been tested and which parameter have been adjusted (in my understanding a common practice in manuscripts presenting numerical studies).

In this respect I also found it difficult to understand when the model was interactively coupled to a chemistry scheme and when not. Since chemistry is such an important issue for the study, and the author’s relatively often emphasize that they assess potential risks associated with the method, I suggest to rephrase respective parts of the manuscript and make chemistry coupling more transparent.

Concerning simulated ozone changes, I wonder why the experiments are not compared in more detail to recent studies investigating ozone changes due to proposed geoengineering scenarios using SO2 or sulfate (e.g. Tilmes et al. 2008,2009; GeoMIP)? This is a clear deficit of the manuscript. I understand that the model applied here is technically different from other models which have been used to study the geoengineered sulfate-ozone relationship. But I in my understanding of the research topic and chemistry/climate/transport/aerosol models, several aspects could be elaborated here.

I also would like to see improvements towards are better discussion of aspects related to atmospheric dynamics. For instance, seasonal dependence of results, temperature dependent effects. Both are associated with the underlying meteorology (which is also unclear, see specific comments below). The author’s are mentioning phenomena like QBO only briefly when the methodology is described, but never refer to them later when the results are analysed and discussed. An important part of the manuscript deals with stratospheric chemistry, which is known to be temperature dependent, but respective investigations or discussions about e.g. additional errors induced by associated impacts are not considered so far. I think it is recommended to consider such aspects in the revised manuscript.

With respect to diamond aerosols proposed as a forcing agent, I do not really understood the message of respective parts of the study. Does the manuscript really profits from the diamond aspects? In opinion not - instead the additionally considered diamond investigations lead to some confusion because the subject is discussed only here and there in the manuscript, only a few figures, for instance, contain data from the diamond simulations and so on. I suggest to critically examine whether it is really helpful to investigate this additional compound here.

**Specific comments:**

Please include a list of abbreviations (e.g. MSA, QBO, …).

p11801 lines 14-23:
Is it a chemistry coupled model ? should be mentioned here.

p11802 lines 2ff:
The author's attest “other“ models a good job in quantifying the response to idealized SRM methods - which in my opinion is too speculative since such idealized scenarios cannot be proved in reality, so that a less subjective valuation seems appropriate here.

p11805 lines 1-4:
Since also in a 2-D model the dispersion of aerosols, hence the desired aerosol effect, depend on the transport characteristics of the model, please describe in more detail the underlying methodology referring to Fleming et al (1999).

p11805 line 11:
In my understanding it was not the goal of the SPARC Assessment of Stratospheric Aerosol Properties (ASAP, 2006) to rate participating models or to introduce any kind of metric for them, I strongly suggest to chose a less qualitative statement instead of „best“.

p11805 line 12:
The understanding of mechanisms forming new aerosols in the stratosphere is far from being clear so that I suggest to rephrase the sentence, e.g. using a formulation like „is thought to form by“ or „may nucleate“.

p11805 lines 18-20:
Logarithmically spaced bins?

p11805 line 20:
You are referring to sedimentation and wet deposition here, but only the former is described below. Please make a note that a description of sedimentation follows and add (at least) references describing how wet deposition is handled in the model.

p11806 lines 5-10:
Please add references to the two equations.

p11803/4 and Sect. 3.1 :
The description of the use of diamond with respect to “the primary test case“ is confusing. When I interpret the figures correctly, the authors show results from a “diamond simulation“ only in the 6th Figure (and later), but describe the compound and related experiments at the beginning of the results section. Again, an experiment table would help to clarify this issue.

Sect.3.2 and Sect. 3.3:
Until the first results are described about the Figures 1 & 2, I do not find any information about the integration period of the model. When did you start the model, how long was it integrated and so on. This is a clear deficit of the manuscript and should be corrected. Please describe in detail the settings but also mention which averaging period has been used for the figures, otherwise the results are meaningless (some figures do contain this information, but some do not).

p11810 referring to Fig.1:
As above, for which period of integration? Shown for steady state, annual mean? Is the latitude band the same as for the upwelling?

p11811 lines 9-11:
In terms of mass or mixing ratio?

p11812 lines 6-7:
Does it mean you performed a spinup over 10 years and then started to analyse the results? I assume with emissions you refer to surface and/or volcanic emissions (of S compounds).

p11812 lines 21ff & Fig.2 a,b: The top panel show mixing ratios but in the paragraph the authors infer and compare the burden from the figures. In my understanding the burden is a vertically integrated quantity which is not easy to infer (visually) from a concentration altitude-latitude plot. In particular not, when the legends of the plots are different. Please adjust and/or calculate/plot the burden.

p11813 lines 22ff & Fig. 4:
When I understand it correctly, the green curves should resemble the values of the bars of Fig. 3 b-d? Mentioning that in the paragraph would help to better understand the result.

p11814 lines 22ff & Fig. 5b:
Do you also show here sulfate from Pierce et al (2010) as in panel a? I think not - the caption is a little confusing for me. Also the beginning of the paragraph is a bit misleading, as it indicates that you are showing sulfate here (only). I suggest to introduce the burden issue with a statement about alumina in order to avoid confusion. I am also not sure why the sulfate burden increases - I thought you emit sulfur continuously (assuming it continuously reaches the stratosphere) so that the sulfur content of the stratosphere is an equilibrium state (you model quasi a background Junge layer). Then I would assume to find a constant sulfate burden in the model (assuming this is an annual mean). Or is it because I cannot distinguish thick lines from thin lines?

p11815 line 16/17:
Please add references in order to make clear how you calculated the upscatter cross section. Same for the other parameter panel in Fig. 7 b,c.

p11816 line 7ff & Fig. 7:
Again, what is the time period you are showing? Shortwave, cloud-free forcing only?

p11817 lines 21-24:
Is there any possibility to estimate the uncertainty you mention? Wouldn't it be possible to estimate upper and lower bounds of associated errors, at least for the most „promising“ scenario only?

p11818 lines 2-7:
Two things are not clear:

a) I am a little confused about "per megaton". In Fig 7a it refers to "per megaton burden" - which is different to Fig 7b where it refers to "per megaton injection"?

b) About sulfate, lines 4-7: I though that is what you show. What is the methodology of $H_2SO_4$ (and $SO_2$) in Fig. 7b if not direct injection? And, if different from Fig. 5, please make it clear.

p11818 lines 8-25:
LW effects are estimated offline from your results with the AER model? do you show data for one year only in Fig. 8?

p11819 line 15:
What context for "preliminary"?

p11819 lines 16-18:
I do not understand the whole sentence „To enable comparison of the ozone impact of sulfate geoengineering we use the same model to compute change in ozone abundance arising from injections of both solid particles and of sulfate aerosols.“. What do you mean with „the same model“?

p11819 lines 18ff:
Are there any references available for the coupled aerosol-chemistry AER model? E.g. studies showing the capability of the coupled model to simulate certain characteristics of the stratospheric chemistry system - characteristics/features which are well known and explored by a range of other studies? Please add. If such references does not exist, I suggest to include a complete table of reactions, potentially as supplementary material, in order make the respective information available for the reader.
In the same paragraph, please add also references for the methodology concerning the treatment of PSCs and the underlying meteorological climatology as well (see comments made earlier).

p11820 line 4:
Please add „primarily“ before „Cl“.

p11820 line 4ff:
Please make clear that you refer to future conditions. In the context of the ozone Section 3.5, you refer to „present day conditions“ - how does the time period(s) match with your emission scenarios of S compounds?

p11821 line 5-7, Fig. 9 b, c:
The response of the 80 nm alumina injection is very different from that of 240 nm. To me it suggests that in the 80 nm case sulfate is more rapidly dispersed towards the poles as in the 240 nm case. Do you have any idea why the behavior is so much different, e.g. from a dynamical perspective?

p11821 line 13-14:
I do not understand the sentence „We would expect similar chemical ozone loss from similar changes in sulfate SAD whether due to geoengineering by $SO_2$, $H_2SO_4$, or alumina injection.“ In a general context?

p11821 line 25:
Please add „uncoated“ before „alumina SAD“.

p11823 line 14:
You are mentioning the „transport timescale“. For me it is even unclear what the timescale at all is, concerning simulation period, meteorology, analysis.

p11828 lines 9-15:
Does it mean the aerosol scheme is not interactively coupled to the transport model’s radiation scheme? If so, please make it clear here and in Sect. 2 and describe briefly how heating rates are handled in the model (prescribed?) and how this relates to the transport of aerosols.

p11829 paragraph 2 (also affecting other sub-sections of Sect. 4):
It would be helpful to insert cross references to the figures here in order to make it easier to understand what you mean, and to make it possible for the reader to find the features you discuss in the figures. In particular, I have difficulties to approve the last sentence of paragraph 2, which sounds contradictory at a first glance.

p11829 line 28-29:
Please remind the reader that the ozone changes depend on specific model assumptions, e.g. add „under the assumptions made“ or so.

p11830 lines 8ff:
Difficult to interpret. When you switch off reaction R1 or switch off the coating, to which injection scenario or alumina sizes do you refer with the calculated changes in %?

**Technical comments about figures:**

Please increase the readability of numbers and indices in the figures 1,2, 4, 5, 7, 8 - 13.

I would like to see the line plots Fig. 1, 4, 5, 7, 10, 12 (!!), and 13 enlarged.

Figure 6: Please use a different color for the magenta curves (on poor resolving printer they may be indistinguishable from reddish colors).

Figure 9: If appropriate, please use decimal numbers for the legend in panel a. To better differentiate the contour plot in panel a from the deviations/differences shown in panels b-d, I suggest to use a different colorbar for the the latter, e.g. a gradient turning from blueish (for negative values) to reddish (for positive values) colors, with a transition around zero marked in white or very bright colors (alternatively use a contour line to mark zeros).

Figure 11: I see a similar same colorbar issue as for Fig. 9.