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Comment on acp-2021-569

Anonymous Referee #2

Referee comment on "An interactive stratospheric aerosol model intercomparison of solar geoengineering by stratospheric injection of SO_2 or accumulation-mode sulfuric acid aerosols" by Debra K. Weisenstein et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-569-RC2, 2021

Review of manuscript "A Model Intercomparison of Stratospheric Solar Geoengineering by Accumulation-Mode Sulfate Aerosols" by Debra Weisenstein et al.

This manuscript presents results from a model intercomparison comparing interactive stratospheric aerosol simulations within co-ordinated multi-model experiments to explore the
global dispersion and radiative forcing that would result from a continuous source of sulphur dioxide
or accumulation mode sulphate aerosol particles with two different emissions scenarios: one emitting only at 30N and 30S, the other as a constant source between 30S and 30N.

The intercomparison compares results from 3 different interactive stratospheric aerosol models
(WACCM-MAM3, MAECHAM5-HAM and SOCOL-AER), and represents a potentially very interesting
contribution to understand the predictions from the models, each having differing sophistication
in their aerosol modules, and in the vertical and horizontal resolution of the GCM's advection.

Whilst the results are interesting, and certainly will be publishable in a revised form, the aim
and design of the model experiments are surprisingly poorly described, and the Introduction and
interpretation need to include some discussion also of the tropical stratospheric reservoir, in relation to the differences between the two scenarios.

In several places the manuscript has unscientific language and vague statements that need to be
changed to terms more appropriate to a journal article. For example "may produce overly large aerosols"
(page 1, line 12) and "unfavorable aerosol size distributions" (page 2, line 20) and
"Our aerosol size distribution" (page 3, line 14) are clearly subjective terms that need to be
better phrased to communicate the issues involved.

There are also a few places where the wording is poor, for example "These limits might be addressed"
(page 2, line 25), "some of these limits may be addressed by altering the size distribution of
sulfate aerosol" (page 2, lines 26-27). The authors are clearly aware that these issues are at the heart of the science to understand the efficacy and risk of a hypothesised large-scale
injection of precursor gas of idealised particle for solar radiation management. Referring to "altering the size distribution of the sulfate aerosol" grossly simplifies the complex interplay
of processes involved -- and the wording needs to communicate consistently with an awareness
of these issues.

I also find it very surprising that, in this initial version of the manuscript, the authors have
not adequately explained the rationale for the very interesting model experiments they are
presenting results from.

The two "injection scenarios" presented in the paper: 1) emitting continuously at two sites
at 30N and 30S, and 2) emitting continuously throughout all latitudes between 30N and 30S,
not surprisingly cause very different enhancements to the stratospheric aerosol layer (as is clearly seen in Figure 2). The 30N and 30S two-site scenario causes the stratospheric
aerosol layer enhancement to be almost exclusively in mid- and high-latitudes, with only a very minor elevation in stratospheric aerosol optical depth in the tropics.

The main reason for this is of course that the two-site injection scenario emits SO2/particles
entirely outside the tropical stratospheric reservoir (between 20S and 20N). It is well established
(e.g. Dyer, 1974) that the residence time for volcanic aerosol clouds formed in the tropical stratosphere are much longer (around 2 years) than for eruptions forming stratospheric aerosol clouds
in the mid-latitudes. The reason is the continuing tropical upwelling and the transport barrier
at the edge of the tropical pipe, and analysis of satellite measurements in 1991-1992 show
the effect for example on the dispersion of the Pinatubo aerosol cloud (see Grant et al., 1996
for example). There needs to at least be sentence briefly mentioning the tropical stratospheric
reservoir in the Introduction, and some discussion of the seasonal cycle of the Brewer-Dobson
circulation (e.g. as set out originally by Dyer et al., 1968 for the Agung aerosol cloud).

Just to be clear, my review is not saying these results are not interesting, the results are indeed very interesting --- and this is a laudible effort to have a set of experiments to better understand any differences in predictions with the models -- but there needs to be a much clearer explanation of the rationale for why these scenarios were chosen.

It's implicit in the text that the 30N and 30S case might represent a limited 2-site injection

strategy, but it needs to be made clear that the two scenarios are not really comparable, and that our understanding of stratospheric circulation would clearly mean that the 2-site 30N and 30S injection scenario would give a mid-latitude focussed stratospheric aerosol forcing,
whereas the 30S-to-30N area-source scenario is presumably designed to give a more evenly-spread
stratospheric aerosol layer enhancement, with then substantial radiative forcing also in the tropics.

I'm also recommending the authors consider changing the title, because the term "Solar Geoengineering by Accumulation-Mode Sulfate Aerosols" is not consistent with what the authors
state that particular model experiment is representing.

Firstly, the models ran separate simulations with continuous emission of SO2, in addition to
the experiment with the particle source, so the experiments are to explore also the injection
of SO2, in addition to direct injection of particles. So the title should either state that both are carried out, or else just give a more general summary-term there.

Secondly, the manuscript states (page 3, lines 14-17) that the particle-injection experiment
is designed to represent particle sizes that would occur at the grid-scale of the large scale models following injection of SO3 or H2SO4 from high altitude aircraft, a localised plume subsequently generating a source of accumulation mode particles at the grid-scale of the GCMs.

The text states "Our aerosol size distribution is consistent with Pierce et al. (2010) and Benduhn et al. (2016) who modelled plume microphysics and found that injection rate could be
adjusted to produce sulfate aerosol size distrihbutions in the 0.1-0.15 micron radius size range."

I'd say first that I'm not sure either of those two first authors would argue that one can simply
adjust the injection rate to produce the desired size distribution. I would expect that both would explain that there would be a substantial variability in the size distribution generated
as the plume subsequently entrains into, and becomes mixed with the surrouding ambient air.

So I'd argue that the text "could be adjusted to produce" is not really adequately representing
the eventual variability in sizes that would result there.

That said, I accept that a large diversity range for particle size is given (0.1 to 0.15 microns).
It's again a case of the wording not adequately communicating the issues involved.

In my specific comments below, I'm recommending the authors consider using the terminology
"sub-grid-scale sulphate emission" rather than "accumulation-mode particle emission", or as an
alterntive they could actually explain the rationale of the experiment is to represent a proxy

for an idealised particle source, deliberately designed to produce particles at a particular desired size.

In light of the likely large variability in particle sizes that continued gaseous emission of SO3 or H2SO4 would cause, to me it is actually this engineered particle-emission scenario that
these controlled size-distribution experiments are representing.

The other similar terminology issue I identify in my specific comments, is that the authors seem to use both the acronym "SRM" for solar radiation management, and also use the acronym
"SSG" for stratospheric solar geoengineering.

In my view, the paper needs to be consistent in either using SRM or SSG, but not both.

My recommendation would be to use the acronym SRM, since the acronym SSG is often used
for "scientific steering group", and SRM is also (in my mind) the more established term.

I'm suggesting the title should also be clear these are interactive stratospheric aerosol simulations being intercompared, with my suggestion being to change the title from

"A model intercomparison of stratospheric solar geoengineering by accumulation-mode sulfate aerosols"

instead to something like:

"A co-ordinated intercomparison of interactive stratospheric aerosol model experiments for hypothesised scenarios of solar radiation management by sulfate aerosols"

I provide below a list of specific comments I am asking the authors to address, and with these
comments requesting a change in the tone of the narrative of the manuscript, my review then
finds major revisions are needed.

The authors may find it relatively easy however to make these changes -- with the Figures, and
much of the results section is in good shape, requiring only minor revisions.

Specific comments:
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1) Page 1 -- lines 1-2 -- Further to the comments above, I strongly recommend the authors
consider using a different term than "solar geoengineering by accumulation-mode sulfate aerosols".
The optical depth from the stratospheric aerosol layer mainly comes from sulfate aerosols in the accumulation mode size range, and the forcing from any geoengineered enhancement
to the stratospheric aerosol layer would be caused by particles in the accumulation mode part of of the size spectrum. So using the precursor term "accumulation-mode" prior to the "sulfate aerosols" is not really a useful descriptor of the effect.

I realise that one of the co-ordinated multi-model experiments involves each model adding

a continued source of sulfate aerosol particles at a particular constant size distribution (in the accumulation mode size range) but that term is then referring to some specifics of the design of the model experiment.

Remember that the residence time of particles in the stratosphere is months to years and the resulting size distribution from a continued emissions is rather a response to that source of particles, and the microphysical and dynamical processes likely mean the resulting size distribution may differ substantially from that within a localised primary emission. That does not necessarily rule out devising an source of particles engineered
to achieve a particular resulting size distribution. But a terminology referring simply to "solar geoengineering by accumulation-mode sulfate aerosols" could lead to some readers
inferring too simplified a relationship between the size distribution at particle emission and the evolving size distribution of the resulting dispersed aerosol cloud.

The authors refer to Pierce et al. (2010) and whilst the 2D-AER interactive stratospheric aerosol simulations give a reasonable assessment for the progression of the geoengineered
aerosol cloud, the dilution of the initial plume and its subsequent evolution of the size distribution of the dispersed aerosol within the stratospheric dynamics of a higher resolution 3D GCM may well have given differing result.

As I say, I am not at all underplaying the value of these model experiments, which could well help to shed light on some of these issues, but I strongly advise the authors use a different terminology for the mechanism the model experiments are investigating.

Within the article, the authors need to be clearer whether these experiments really are representing a scenario of injected H2SO4 vapour. With the resulting plume rapidly nucleating
particles to form a source of new particles that progress to be large enough to scatter incoming solar radiation.

The current model experiments do not really represent that situation, because there would certainly be greater variability in the size distribution in that case of H2SO4 vapour emission.

Rather I would argue these experiments mimic a situation where particles are emitted with
a controlled size distribution, the particles deliberately engineered to achieve a certain subsequent response within the stratospheric aerosol layer.

My recommendation in this first comment is to change to a more general title something like:

"A co-ordinated intercomparison of interactive stratospheric aerosol model experiments for hypothesised scenarios of solar radiation management by sulfate aerosols"

That's partly because the experiments are not restricted to only assess an emitted source of particles, they also assess the models' response to emitted SO2. In light also of the potentially large variations in particle size distribution that would result, to simply tag the approach as "Accumulation mode particle geoengineering" is not appropriate (in my opinion).

As per the subsequent specific comments, within the article, I can understand there is a benefit to referring to the effect from the strategy (in that it is specifically

introducing accumulation mode particles into the models), but still I'm recommending the authors use a different terminology than "AM-H2SO4 geoengineering".

Global aerosol microphysics modellers may tend to use the term "sub-grid scale particle formation" or "primary sulphate emission" for this approach, with the former being much preferred to the latter by experts. And in my comments then I advise to use the term "sub-grid scale particle formation model experiments" or similar as the alternative term.

2) Page 1 -- Abstract, line 11 -- Suggest to insert "tended to focus on" rather than simply "focussed on", and rather than the somewhat vague term "Analyses", be clear you're referring to interactive stratospheric aerosol model analyses". In fact probably better to use "studies" rather than "analyses".

3) Page 1 -- Abstract, lines 11-12 -- the 2nd half of this 1st sentence then refers to climate models (whereas I think the first half refers to interactive stratospheric aerosol models). I think I understand what the authors mean when they say the climate model experiments "have assumed injection of SO2", but that could confuse some readers,
because the majority of climate models do not tend to use their interactive aerosol modules for stratospheric aerosol, and therefore do not tend to represent injection of SO2 at all.

I think what the authors mean is that the model experiments tend to be designed to represent a scenario of imposing radiative effects consistent with best estimates of what could be expected from continued injection of SO2.

I suggest to change "have assumed injection of SO2" to "are based on scenarios aimed to represent the effects from continued SO2 injection". Or similar.

4) Page 1 -- Abstract, line 12 -- As per my general comments above, "may produce overly large
aerosols" is obviously unscientific language. Also, that particles grow larger with increased SO2 is a scientific fact, with then use of the word "Yet" not good grammar.

It's an important point the authors are making, but this should be stated in an objective way,
whereas the precursor word "Yet" suggests the authors consider it somehow unfortunate or
undesirable.

Suggest "It is well established (e.g. Pinto et al., 1989) that greater emission of SO2 leads to
larger sulphate aerosol particles, with shorter residence time in the stratosphere."

5) Page 1 -- Abstract, line 13 -- I think changing "new" to "additional" changes to a more accurate representation, to ensure authors do not mistakenly infer that particles form immediately
at accumulation mode sizes (but rather grow from an initially smaller Aitken mode sizes) in this scenario of aircraft injection of SO3 or H2SO4.

This is an example of where I think the simplified term "geoengineering by accumulation mode
sulphate" might only increase the probability of an incorrect inference in that respect.

I therefore strongly suggest the authors delete "AM-H2SO4", as the acronym similarly will tend to embed an increased likelihood of that over-simplified perception of the progression

of the microphysical and dynamical processes involved.

The term "nudged" is also not appropriate in this context, tending to over-simplify the response
of the stratospheric aerosol layer.

I'd suggest to re-word to "Some studies have explored whether a stream of very small particles
can be generated by injecting H2SO4 vapour rather than SO2, potentially then leading to longer-lived aerosol paricles for a given sulphur injection rate."

Introducing a specific delivery mechanism seems un-necessary, and my suggested re-wording
then also keeps the point more general than that specific situation of aircraft injection.

6) Page 1 -- Abstract, line 15 -- For the reasons given earlier, please change the terminology
"AM-H2SO4 injection" to refer to the specifics of the model experiments rather than an apparently
more general "type of geoengineering".

As explained in my comments above, I'm suggesting to use the term "sub-grid scale source of particles"
as the descriptor, referring then to the specifics of the model experimetns, with also an acronym
then not required in this case.

I suggest then to change this sentence to instead say "Whereas GeoMIP has included experiments
to intercompare SO2 injection scenarios, the results here are the first multi-model intercomparison
of the effects from a sub-grid scale source of sulphate aerosol. Or something like this.

With the subsequent sentence referring to GeoMIP, suggest to reserve the statement of "first" for
after the sentence referring to GeoMIP. The scope of that sentence can be made more general
by changing "We compare three models" to "A co-ordinated multi-model experiment designed to
represent this SO3- or H2SO4-driven geoengineering scenario was carried out with 3 interactive
stratospheric aerosol models:". The word "coordinated" can then be deleted later in the sentence.

7) Page 1 -- Abstract, line 24 -- Further to my general comments above, the term "sensitivity to injection pattern" is not an adequate description of the two experiments, the two-site experiment resulting in a midlatitude-focused forcing with little enhancement to the tropical stratospheric reservoir. The word "sensitivity" suggests a slight change whereas these two alternative representations of the geoengineering enhancement are much more
substantially different. Better to actually crystallise in the reader's mind what the two alternative scenarios represent -- a mid-latitude-focussed forcing (presumably designed
to avoid perturbing climate-sensitive regions in the tropics?) and an evenly distributed injection rate across the tropics and mid-latitudes.

Suggest then to change the sentence beginning "We explore the sensitivity to injection pattern" to
"Simulations with two scenarios were designed to compare a two-site injection focussed to force only the mid-latitudes, with a more evenly distributed geoengineering forcing, each run with both SO2 and sub-grid particle experiments." or something like this.

The "and find opposite impacts" is explaining the results, and should be explained in a separate
sentence, changing "and find opposite" to "We find opposite" or similar.

8) Page 2 -- Introduction, line 2 -- insert "long-wave" before "radiative forcing" and change "from the rise in CO2" to "from increased CO2 concentrations". The reason here is to keep in the reader's mind that emissions are not necessarily the same as concentrations.

9) Page 2 -- Introduction, line 3 -- "Despite the complexity" -- it's not corect to say "Despite"
here and I'd argue it's more "because of the complexity" that these models are needing to be used
to try to predict how the overall system responds given the complex interactions and feedbacks.

Suggest to change "Despite the" to "In light of the".

10) Page 2 -- Introduction, line 4 -- "solar radiation management (SRM) is being studied". It's not really the solar radiation management itself that is being studied -- it's the effects from hypothesized solar radiation management (whether that be the responses of the stratospheric
aerosol and ozone layers or the surface response to climate and the hydrological cycle).

Suggest to insert "the effects from hypothesized" after "carried risks".

11) Page 2 -- line 5 -- since the word "climate" is used later in the sentence (and with the change above also earlier in the sentence) change "climate models" to "earth system models".
And add citations to 2 or 3 of the key papers here.

12) Page 2 -- line 11 -- change "the climate response to stratospheric aerosol injection" to "the climate response to a geoengineering-enhanced stratospheric aerosol layer" or similar.
It's the eventual enhancement to the stratospheric aerosol layer that causes the forcing, not the injection. With a residence time of months to years, there is quite some difference between the nature of any injection and the resulting forcing that the climate then responds to.

13) Page 2 -- line 13 -- change "alter the climate" to "cool the surface climate and warm the stratosphere".

14) Page 2 -- line 19 -- change "Studies of SSG" to "Studies of SRM" -- and change other instances
of "SSG" within the paper instead to "SRM"

15) Page 2 -- line 20 -- change "unfavorable size distributions" to "shorter residence time in
the stratosphere (larger particles)".

16) Page 2 -- line 25 -- As I explained in my general comments, "These limitations might be addressed.."
is not scientific language, and should be focused on which of the 5 limitations stated, emitted
"various solid particles" the suggested solid particles is intended to address.

17) Page 2 -- line 25 -- The phrase "altering the size distribution"
does not adequately communicate the complexity of the microphysical and dynamical processes that
combine to effect the stratospheric aerosol layer's adjustment to a geoengineering source of
aerosol particles. Whilst I understand that a strategy can be designed for engineered particles
to aim to achieve a given desired size within the subsequent months to years of their circulation
within the stratosphere, it is over-simplifying this to refer to "altering the size distribution".

It is of course certainly possible to alter the size distribution of the emitted particles, but any control within the response of the stratospheric aerosol layer in the subsequent months is
too uncertain to be referred to simply as "altering the size distribution".

Please change "alternatively some of the limits may be addressed by altering the size distribution" to
"with engineering strategies potentially able to achieve a more prolonged aerosol particle residence time
in the stratosphere". Or if the authors mean the radiative efficacy, please phrase this more explicitly
to be clear of the size effect intended.

18) Page 2 -- line 28 -- The sentence beginning "Efficacy is decreased" needs to be re-written
for at least two reasons. Firstly, this is the first time the word "Efficacy" has been introduced,
and it's not clear where this is scattering efficiency or efficacy in terms of residence time.
The remainder of the sentence suggests it is mainly the latter -- so rather than "Efficacy is decreased...",
suggest instead to say "Aerosol particle residence time in the stratosphere reduces...".

19) Page 3 -- line 15 -- "Our aerosol size distribution" is unscientific language.
Please change to "The constant size distribution used by the models in the co-ordinated experiment..."

References

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Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-569/acp-2021-569-RC2-supplement.pdf