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Comment on acp-2021-526

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Referee comment on "Dependency of the impacts of geoengineering on the stratospheric sulfur injection strategy – Part 1: Intercomparison of modal and sectional aerosol modules" by Anton Laakso et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-526-RC2, 2021

The authors use a state-of-the-art climate model coupled to two different aerosol modules to simulate a wide range of stratospheric aerosol injection (SAI) deployment strategies and deployment scales. The authors find that the sectional model produces a substantially larger negative shortwave forcing and smaller positive longwave forcing, and hence an even greater net radiative forcing, than the modal model. The main driver of this difference is the competition for gaseous sulfate between the nucleation of new particles and condensation onto existing large particles, with more of the sulfate captured by large particles in the modal model. The authors note that due to limitations in the microphysical representation in the modal model, a "modal gap" (my term) opens up between efficiently-scattering accumulation mode particles and coarse mode particles that falls within the range where particles most effectively scatter light, producing a drop in SW forcing. The authors find that in both models the net forcing from SAI increases sub-linearly with deployment scale, with a greater decline in forcing efficiency in the modal model. Almost all alternative deployment scenarios produce a greater forcing than the baseline scenario (10N-10S, all longitudes). Scenarios which inject the aerosols over a wider range of latitudes or at different latitudes in different seasons avoid an over-concentration of particles in the tropics which promotes the growth of larger, shorter-lived particles in the baseline scenario. Both the high and low altitude deployment increase forcing, for the high scenario this is a result of increased lifetime, and for the low scenario this is due to increased polewards dispersion. Pulsed scenarios also increase the total forcing by increasing the concentration of gaseous sulfate to existing particles, promoting nucleation and the growth of smaller particles.

This paper makes a novel and important contribution to the literature on SAI, making the most thorough evaluation of SAI deployment scenario in a state-of-the-art model and doing so with two different aerosol modules. It is generally well-written, presented and argued, and I learned a lot reading it. However, I feel the authors need to do more to communicate the implications of these findings for the field.

For the scenario choice, this could be addressed straightforwardly with a more pointed discussion of the usefulness of focusing on equatorial injections as the baseline case for evaluating SAI. This paper piles yet more evidence on to the case that they are inefficient and produce an uneven, tropics-heavy sulphate burden, shortcomings that can be readily avoided with alternative deployment choices. GeoMIP G6sulfur was an equatorial strategy, what do these results suggest about the wisdom of the next generation of GeoMIP focusing on equatorial injection?

While I'm not an expert on aerosol microphysics, it seems to me that the authors present evidence (discussed around line 360) that suggests modal models can, or at least the modal model they employ did, produce an unrealistic simulation of SAI for large deployments. Later, the authors speak loosely of uncertainties when describing the differences between these two aerosol modules but what I'd like to see instead is much more expert judgment and discussion on the realism of the simulated aerosol distributions. It sounds to me that the "modal gap" that opens up, right around where aerosols most effectively scatter light, between accumulation-mode and coarse-mode aerosols is unphysical and represents a serious shortcoming in this representation of the aerosol microphysics of SAI.

The question of whether the simulated aerosol distribution is realistic in this modal model is crucial and the introduction and discussion do not do enough to set it up and answer it, with the main discussion of this issue buried in the midst of the results section. I'd like to see a paragraph or two devoted to the differences between modal and sectional representations of aerosol microphysics in the introduction that raises common shortcomings, challenges (presumably the possibility of a modal gap opening up has been discussed previously?) and the relative performance or realism of the different options. Similarly, I'd like to see a much more in-depth discussion of these issues in the discussion and conclusion. Would this issue have occurred in a modal model with more modes? Similarly, the discussion should address the question: "are modal representations appropriate for SAI given our findings?" and provide some guidance on their limits or warnings about particular setups to other modelers working on this topic so that they can represent SAI more realistically.

In terms of the analysis related to this issue, I found the crucial aerosol number size distribution plots which show this issue in the main text very busy and difficult to follow. Figures 2 and 5 had many overlapping features and were not at all intuitive, whereas the design choice for figure S5 was much, much clearer. Would it be possible to show light scattered and LW absorbed by aerosols as a function of aerosol size? If not, is there a way to plot the scattering like the absorption? In either case, a much clearer way to show these results and the implications of the modal gap is needed, i.e. something more like figure S5.

Relatedly, the authors devote a page to describing the onegb results as an outlier, indicative of some numerical issue, where it seems to me that the disproportionate efficiency of onegb is due to the fact that no modal gap opens up in this scenario (figure S5). Am I wrong to think that it is the other scenarios that show the shortcoming and not onegb?

Another general issue with this paper is that the discussion section is a little thin, with much of the detailed discussion of the results occurring within the results section. As most readers won't get into the depths of the results text, the discussion and conclusion should really bring out all the most important discussion points. Some expansion of the discussion, drawing existing text from the results section, would address this.

I also had a few other general comments that I think need addressing and a long list of minor suggestions that will follow.

A longer discussion of Kokkola et al. 2018, which is mentioned briefly in the methods, seems warranted in the introduction as they apply both aerosol modules to simulate Pinatubo, a highly relevant case.

A wide range of different terms are used for SAI, I'd suggest sticking with ONE and using consistently throughout all text and figures.

The text is generally well-written and clear but there are definitely some English grammar and phrasing issues that could be picked up with a careful proof-read.

L33 – "changes the structure" – in what sense?

L34-36 – make clear that this is relative to no change, i.e. it is not still warming relative to a case without geoengineering.

L42 – which have

L47 – is the lack of measurements the leading driver? Are there others?

L49 – most rely on models: which don't and are they worth taking seriously?

L58 – I'd suggest rephrasing this and the previous claim so they are compatible, i.e. if tilmes simulated aerosol microphysics but found a linear effect then the previous statement is misleading.

L60 – I'd suggest adopting consistent terminology throughout, there is no need to use SAI, stratospheric geoengineering and stratospheric sulphur injections.

L71 – "both LW and SW radiative forcing, which have opposite impact on net radiation" this isn't universally true but true of those forcing terms for SAI.

L91 – larges = larger

L96 – at a certain

L109 – this sets limitations

L140 – suggestion: ‘the term “radiative forcing” refers to the instantaneous radiative forcing’

L146 – are these slight changes included here?

L149-166 – This description of SALSA could be clearer on which aspects described are part of the standard SALSA configuration and which are novel to this paper, and how they differ from this standard configuration.

L150 – one change made but what was it? Do the next sentences describe the change or the standard configuration?

L159 – “new particle formation scheme” new as in new to this paper or new in some other sense? Is this the change?

L163-165 – changed from what to 3 nm?

L177 – “Our model setup does not include all modifications done in (Niemeier and Timmreck, 2015) e.g. the simple stratospheric sulfur scheme.” Have the authors described all the changes, and Niemeier and Timmreck did more or are there additional, unspecified changes that take us some of the way towards what Niemeier and Timmreck did? Not clear that the “simple stratospheric sulphur scheme” refers to, is this described by Niemeier and Timmreck?

L184 – does "band" here mean injection occurs across all longitudes, either way make this clear and explain it in table 1.

L187 – narrow, 1.9N – 1.9S – is this +1 / -1 gridbox from equator, if so make that clear and make clear the similarity to onegb.

L192 – Perhaps clarify, does pulse2m inject 6 months out of the year, i.e. every other month?

L192-194 – Are pulse2m / pulse6m the most intuitive name for these scenarios? Pulse alt months, pulse twice yearly?

L195 – seasonal – specify how it varies through the year, then explain that the authors followed laakso et al. 2017's approach. Readers don't want to have to look up the methods of another paper to capture a detail that can be explained in a few words.

L197 – why not call it two grid box or something else? Seems odd to inject into 2 grid boxes and then call the scenario 1 grid box.

Table 1 – Might be good to specify here or in text the vertical distribution in terms of

gridboxes. I presume 21km is the centre of a single gridbox, which has a bottom and top that spans some altitude, and 20-22 km captures 3 or so gridboxes.

Figure 1 – This is unclear, do the faint results correspond to the right-hand faint axis. This needs to be made clear in the figure caption!

L221 – in other hand the = on the other hand?

L250 – does this (sulfur burden) indicate that (size, number) alone?

L284 – 1) Does that imply: shorter residence times meaning fewer particles make it to high latitudes? If so spell that out.

L285 – This sentence is a bit of a mess, how about: "as it has been shown (visioni…)," sulfate aerosols à tropical warming à strengthened polar vortex à reducing (not preventing) transport of particles to polar stratosphere

Figure 3 – position of a / b not consistent. Choice of colorscale a bit odd, going from green to cyan to green again. Why not have a simple perceptually uniform, colorblind-friendly sequential colormap like viridis

(https://matplotlib.org/stable/tutorials/colors/colormaps.html). Y-axis label: normalized SW forcing, would be snappier and could be explained in caption.

Figure 2 and 5, legend to top-right? Presumably the magenta line is unitless but linear rather than logarithmic like the Y-axis, but this should be specified.

Figure 5 – Is this the best way to show this? I find Figure 5 very difficult to follow as there are so many elements and I'm comparing left-right panels. Perhaps reconfigure so that the relevant difference (results at one latitude vs the other) can be seen legibly in individual panels (more than 2? Or for only 5 or 50 not both?)

L306-307 – Is the deposition process slower, implying for the same mass it falls out more slowly, or was there less to deposit so the total rate is lower?

L308-311 – perhaps rephrase to remove some of these caveats and details, these sentences are a little tangled.

L324 – can this 10nm particle effect be explained briefly here?

L334-334 – not clear what is meant by nucleation over-running (out-competing?)

condensation, or how figure 4 supports that view.

L339-345 – Describing these supplementary results quantitatively would help to make clear the significance of this effect relative to other differences.

L346-L364 – Proof-reading for English grammar and phrasing is needed here.

L369 – SRM? Stick to one term

L361-364 – Should this be stated more strongly? The modal representation seems to have a substantial shortcoming, a gap just where the aerosols are most effectively scattering, which means that it surely under-estimates the radiative forcing from SAI.

L365-368 – Seems to change topic here, why not open in a way that makes clear where this new paragraph is heading, i.e. simulations of volcanic eruptions do not expose this shortcoming of M7.

L383 – This clear-sky comment could perhaps be better made in the methods, as the clear-sky analysis is the focus across much of the paper rather than a novelty of this section.

Figures 6 and 7 – I wonder if there's a simpler / clearer way to plot this. Perhaps, rather than scattering the scenarios as coloured lines along an injection amount X-axis, flip this with the scenarios laid out along the X-axis (like a bar chart?) and the injection amount as coloured points scattered up the y-dimension. Just an idea.

Figure 8 – the seasonal overlay obscures more than it reveals, I'd suggest relegating it to a supplementary figure and referencing it in the methods (where it is unclear how the seasonal strategy is implemented). Having a conventional legend would be more legible, and the blue arrows are unnecessary. There's a lot of lines here, narrowing the y-range would make them more distinct

L410-412 – so the heating effect is more concentrated?

L423 – "less aerosols over the equator are under continuous injection" – isn't clear what this means.

L427 – Best to explain how they are different or simply remove this sentence and give the results.

L437 – This last sentence isn't clear.

L442 – accumulation mode, grow.

L445 – would be clearer to state what Niemeier and Timmreck found.

3.3.2 – it seems like the issue is more with the normal scenarios than with onegb as a gap opens up between the accumulation and coarse modes in the normal scenarios and not in onegb.

L468 – This isn’t informative unless the difference in the untuned version is made clear, and its significance explained.

L532-533 – this nice, clear description of the pulse scenarios should appear in the methods section.

L540 5% larger?

L543 – should that be 6 times greater – 12 months of emissions in 2 months?

L545-546 – rephrase this sentence.

L560 – explain the strategy in more depth, then cite laakso.

L568 – compared to baseline or is it a different equatorial scenario?

L572 – distributed evenly would be a fairer description, concentrated in mid-latitudes suggests the forcing is much greater there than elsewhere.

L573-585 – This paragraph needs some rewriting, I’d suggest opening in a way that makes it clear what the paragraph is driving at.

L580 – the previous paragraph reports increases of at least 30% which is inconsistent, unless I misunderstood.

Figure 11 – plotting control last would make it easier to see some of the changes.

L599-611 – might be worth extending the plot to include more (all?) of the troposphere and mentioning the substantial decrease in vertical velocity in the upper troposphere seen in the simulations with large warmings, looks like it's down by a third in the 100TG M7 case.

L620-622 – is there a consistent temperature threshold between the 2 cases?

L666-667 – "2) limiting the evolution and the shape of the particle size distribution due to mode setup in M7." This isn't clear, perhaps it should be expanded a little here.

L669-671 – The phrasing here suggests this effect is large, could it be quantified? What fraction nucleates vs. coagulates in these 2 models?

L673-675 – This "modal gap" is critical and should be elaborated in a full paragraph. One of the take-aways for the community from this paper ought to be to watch out for this phenomena in their own modal model setups. Some more discussion of whether this "feature" of the modal setup is realistic, presumably not, and what it implies about the applicability of these models in future is needed. Does this imply that the modal model is significantly underestimating the effectiveness of SAI? Does this imply that modal models need a minimum of 5 modes to represent SAI? Are there some other design choices that could be made to avoid this issue or is it a fundamental weakness of modal models, and an argument for the application of sectional models?

L690 – should make clear that this is sufficient to return forcing to pre-industrial levels, as readers may assume that the authors mean change relative to today.

L688-699 – Is it fair to describe these differences as uncertainties when the authors have revealed a serious shortcoming in one of the models?

L704-705 – Mention which were the most and least efficient scenarios, and perhaps briefly why.

L707-709 – As I suggested before, it seems like Onegb was only spurious in that it didn’t cause M7 to exhibit the modal gap issue. I’d argue that this suggests M7 is only capable of reasonably simulating small deployments, before modal gap opens up, and onegb where it doesn’t open up.

L710-720 – It seems to me that a reasonable take-away from this paper is that equatorial deployments are inefficient and result in an over-concentration of aerosols in the tropics, and that wide, seasonally-shifting or extra-tropical (other papers) deployments produce more efficient, more even forcing and hence are likely preferable. The implication then would be that future SAI studies should consider abandoning the equatorial strategy (which was chosen for simplicity, and features in G6sulfur) in favour of one of these alternatives (in a future round of GeoMIP?).

L725-728 – Isn't the key the relative concentration of large particles vs. gaseous SO2? Onegb and narrow both deploy into an existing plume but differ in that the concentration of gaseous SO2 is ~100 times higher in onegb promoting nucleation.

L729 – explain the baseline distribution, with its disproportionate forcing in the tropics, before the differences.

L735-736 – There is no warming at high latitudes due to SAI, it offsets ~80-90% of the warming from GHGs rather than 100%

L739-741 – This is the only mention of the dynamical changes in the discussion. The authors gave a whole sub-section to the topic, seems odd to not give it at least a paragraph in the discussion and conclusions.