Review of *"Solid-particle stratospheric aerosol injection: a 2-D modeling exploration of the design space"* by Lederer et al.

General note on the various manuscripts published by Stardust:
The authors sometimes demonstrate a lack of understanding of basic scientific concepts relevant to SAI and often they demonstrate a shocking degree of unawareness of the research literature conducted on SAI to date (not only in this manuscript). In my point of view, this is because none of the co-authors has relevant expertise in stratospheric research or expertise with SAI research in particular. Maybe this is because most people with relevant expertise might decline collaboration due to Stardust's questionable practices, which for example are not in alignment with AGU's ethical framework principles for climate intervention (e.g., inclusive public participation, transparency).
I appreciate the idea of separating aerosol surface from bulk properties, but I see many technical and practical challenges as well as substantial associated uncertainties, which are often not declared and not addressed. The research presented in these papers might have been conducted in a fine way, but the interpretation of the results and the implications derived from the experiments are often incorrect and misleading.
I think Stardust might be best advised by first (1) trying to understand basic concepts of climate and stratospheric chemistry, then (2) reading all the relevant literature on SAI (there is already a lot out there!) and (3) think about whether deploying SAI is a justified option at all. Finally, if the conclusion is yes, (4) maybe think about alternative particle types, if there is need. I have the impression Stardust is going exactly the opposite way (from 4 to 1). They try to come up with a solution for a problem, which they do not understand.

General comment to authors and editor (or editorial board):
I first asked myself why the authors put in the effort to build their own 2D model instead of using a pre-existing one, until I realized that most of the pre-existing models are for non-commercial use only. Having this in mind, I am asking myself (1) what the author's intention is to go into peer-review with this paper and (2) what the journals rules are in terms of the conflict-of-interest declared by the authors. Could any for-profit company submit their research to ACP and get free expert reviews of their business case?

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My scientific review in brief:

My review follows the journals review criteria outlined here: https://www.atmospheric-chemistry-and-physics.net/peer_review/review_criteria.html

Scientific Significance: Low; the authors present a new model approach; however, it is not novel at all. No substantially new concepts, ideas, methods, or data are presented.

Scientific Quality: Low; The scientific approach and applied methods have only very limited validity. Most importantly, the results and their substantial limitations are not appropriately discussed and are not compared to related work. A substantial amount of relevant literature is not cited and discussed.

Presentation Quality: good; structure, language, number and quality of figures and tables is okay, even though many of the presented results are not clear at all.

The prerequisite for research to be published in ACP is that *"articles should have important and clearly argued implications for our understanding of the state and behaviour of the atmosphere and climate or present substantial new insights into the atmosphere's role in other parts of the Earth system."* This does not apply to the manuscript presented here.

Therefore, I suggest declining publication of the presented manuscript in ACP. Many of the criticized points could be addressed with very substantial major revisions (basically equal to writing a new manuscript). After that, the manuscript could be resubmitted to a different journal with a more suitable scope. Since the biggest part of the manuscript is the validation of the 2D transport model, a more suitable journal might be Geoscientific Model Development.

Detailed scientific review:

What the authors are essentially doing is prescribing transport to ERA5 observations. In the model, transport is not interactive with anything (not only stratospheric heating as pointed out during in the manuscript). This should be clearly stated. Based on this prescribed transport fields the authors calculate the equilibrium aerosol burden resulting from different injection locations while accounting for sedimentation and agglomeration. Microphysical details on how sedimentation and agglomeration of the particles are represented are missing. Subsequently, the authors use the resulting zonal mean particle number concentrations offline in a radiative transfer code to calculate stratospheric heating and LW and SW RF. This simplistic approach is generally fine but must be clearly acknowledged and not be embellished. The limitations of this approach must be appropriately discussed (instead of promising future work) and the analysis conducted with this model setup should remain within its limited valid scope (currently not the case).

The authors cannot make conclusion about climate impacts with a 2D models by any means. There are countless aspects of climate which cannot be resolved with a 2D model. A 2D model might be appropriate for some applications in the stratosphere, but definitely not for tropospheric climate aspects. But also in the stratosphere, the use of 2D models is limited by many factors. For example, an important aspect for stratospheric transport, which is not resolved is how the Brewer-Dobson Circulation or the quasi-biennial oscillation respond to the injected aerosols: e.g. https://acp.copernicus.org/articles/21/8615/2021/

A common paper structure starts with discussion of published literature on a specific problem including a clear motivation on why the presented research is performed. This is missing. In the introduction the authors try to highlight the advantages of solid particles over sulfur-based injections, but the authors should also highlight and acknowledge the disadvantages, challenges and uncertainties associated with SAI of solid particles as well as the advantages of sulfur-based SAI. Some of these aspects are for example discussed in the discussion section of this publication: "Injecting solid particles into the stratosphere could mitigate global warming but currently entails great uncertainties" or in this perspective: Alternative Particles Could Reduce the Side Effects of SAI.

Subsequently, the presented results should also be compared to the existing literature of SO2 and solid particle SAI in the discussion section. Therefore, the authors should also compare their results to SAI via injection of SO2 to clearly highlight the differences, advantages and disadvantages of injecting silica and calcite. Since it would be difficult to implement interactive sulphur chemistry and microphysics the authors could just assume 70wt% sulfuric acid aerosols of a fixed size as done e.g., in Dyekma et al. 2016. But comparison to sulfuric acid is important.

Injecting large amounts of solid particles into the stratosphere via aircraft would include aircraft wake plume processes to disperse the particles. The presented model does not capture this process (there is also no information provided on the model's resolution). How relevant are the presented coagulation results when plume scale processes are not accounted for? This is not even mentioned or discussed.

The authors do not cite and neither discuss a shocking amount of key-literature relevant to this paper. Just a few examples:

1) Since you analyse agglomeration of solid particles, this publication would be important, please have a read! https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL110575
2) The key advantage of solid particles compared to sulfuric acid aerosols is that many materials could potentially reduce stratospheric warming. A substantial portion of the ozone anomalies in the stratosphere from SAI via sulfuric acid aerosols is due to dynamical effects from stratospheric heating, and not heterogeneous chemistry (especially towards the end of the century with decreasing concentrations of ODS). https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL107285

3) Sensitivity of SAI to injection strategies such as injection altitude, latitude and season have been investigated extensively in the past. The authors should refer to these studies and discuss the results compared to these findings. Just a few examples, there are many! more: https://doi.org/10.1029/2019JD030329, https://doi.org/10.5194/esd-15-191-2024, https://doi.org/10.1029/2019GL083680

All the results are based on injection scenarios injecting 20 Mt/yr. This results in up to -11 W/m2 RF for tropical injections (this is about 3x (!) the magnitude of the RF resulting from current climate change). These results are then normalized linearly to the resulting W/m2 radiative forcing and also to the injection rate. However, there is no evidence that this is valid. I think this includes non-linearities. For example, the degree of agglomeration will likely be substantially smaller for smaller more realistic injection rates. The injection rate would thus be an important variable of the “parameter space”.

The authors evaluate and describe 2D transport model and the validation in very much detail. However, the authors do not provide any details on how aerosol microphysics (sedimentation and coagulation) is represented and how the aerosols are represented (which radius for agglomerates how many bins or modes). An important factor for transport and microphysics is also the spatial and temporal resolution of the model. The authors do not provide any information on this neither and the authors neither discuss the impacts of a too coarse resolution on transport.

The equivalent effective sphere approximation you applied to calculate RF of agglomerates is not valid for agglomerates of monomers other than the optical radius of silica (i.e., 0.5 um). See my comment on section B6 below.

I could summarize many more major flaws here… Just have a look at the line-by-line comments.

Line by line comments:

Line 1 and 2: You define a “parameter space” for solid particle SAI here. However, this parameter space is incomplete. What about heterogeneous chemistry, aerosol-cloud interactions (or governance/ethical aspects if you want to stretch it beyond physical science)? You should be more specific here. Maybe write that you focus on the listed aspects of an otherwise very broad physical parameter space. Also, you compare the solid particle SAI parameter space to SAI via sulfuric acid aerosols. However, injection strategies in altitude, latitude and season are also important parts of the parameter space of sulfur-based SAI.

Line 2: Are you analysing these parameters in the paper at all (e.g., composition and morphology)? You rather look at two different materials, but not composition. And you don't show a "parameter space" for morphology in this paper, right? So, I would leaf this away.

Line 3: "practically prohibitive"? I suggest being more specific here. I guess you refer to the "computational costs", right? There are many three-dimensional chemistry–climate models which can be run in different degrees of complexity in terms of temporal and spatial resolution as well as in respect to the resolution of the various physical processes accounted for. This should be rather easy and affordable nowadays, especially for research laboratories focusing on the Earth's climate.

Line 3: I have never seen the use of "sweeps" in this context. Maybe it's better to be specific and call it "simulations" or "calculations".

Line 5/6: "… with each component extensively validated." This is a very general statement. Is it needed here? I suggest either be more specific and mention for which (user-) case it was validated or leave it away. Otherwise, the statement, as it is written now, could give a reader the impression that the authors feel the need to highlight the validity of their methods…

Line 7: "Material properties" is a very broad term. Be more specific here. Heterogeneous chemistry, optical properties … ?

Line 8: "radiative forcing efficacy" this term must be defined. Radiative forcing efficacy in respect to what? Injected material or aerosol burden? Also throughout the paper, not only here.

Line 12: "loss of efficacy" which efficacy? In respect to what?

Line 14: This is a rather abrupt end of the abstract. After listing your results, can you maybe add a sentence about the implications of your work? Or maybe some limitations? What is the novelty of your results?

Line 16-17: *"… aiming to partially offset the radiative forcing of accumulating greenhouse gasses by deliberately increasing the stratospheric aerosol loading".* This statement is wrong and makes me doubt the authors understanding of basic physical principles of Solar Radiation Modification. A stratospheric aerosol loading is not doing anything to the radiative forcing of greenhouse gases. Green house gases absorb and re-emit terrestrial long wave radiation. Stratospheric Aerosols primarily scatter incoming solar radiation in the short-wave lengths, thereby creating a negative radiative forcing themselves. But they do not affect (or "offset") the radiative forcing of greenhouse gases. The radiative forcing from the greenhouse gasses will still be there. SRM might potentially reduce some impacts of climate change (or of the warming created by the greenhouse gases) by cooling the climate, but it does not offset the radiative forcing of greenhouse gasses.

Line 17-20: Can you list some reasons why sulfuric acid aerosols have evolved as the standard material? The sentence reads as if sulfuric acid aerosols were just arbitrary determined as the standard by the research field. Some reasons are for example: 1) sulfuric acid aerosols occur in the stratosphere naturally, 2) they play an important role in stratospheric ozone chemistry and thus, have well constrained physio-chemical properties, 3) the natural analogue of explosive volcanic eruptions which emit large amounts of SO2 to the stratosphere provides observational evidence of their effects in the Earth System, 4) due to their natural relevance in the Earth system, sulfuric acid aerosol and their physical and chemical properties are key components in many aerosol chemistry climate models which makes it easy to simulate SAI scenarios with sulfuric acid aerosols.

Limit 23-24: This is true. However, this reads as if we do not know much about sulphate aerosols: Maybe have a read of this paper:
https://acp.copernicus.org/articles/23/5149/2023/
Also, this sentence is even more true for solid particle SAI. There, the uncertainties are even larger as compared to sulfuric acid aerosols (see also my comment on line 17-20 above and have a read of this paper: https://www.nature.com/articles/s43247-025-02038-1). Generally, this sentence does not motivate your case at all… quite the opposite. I suggest reformulating.

Line 25: *"These limitations have motivated exploration of solid aerosol alternatives."* Reading this sentence right after the previous one gives the impression as if the limitations were the result of the "central modelling challenge", which is not true. The modelling challenge for solid particles is even larger (see previous comment on line 23-24).
In my opinion the central motivation for solid particle SAI is reduced stratospheric heating (main limitation of SAI via sulfuric acid aerosols). Stratospheric heating creates a dynamic response which perturbs global atmospheric circulation patterns and precipitation patterns (major side effect of SRM). I think this should be your key motivation to do this research. This is a key paper in this respect:
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL107285
Also important is this one: https://iopscience.iop.org/article/10.1088/2752-5295/ad9f93

Line 27-28: Change "developed solid-particle alternatives" to "assessed solid-particle alternatives"

Line 27-30: Another important aspect these studies have assessed (e.g. Vattioni et al. 2023 and Vattioni 2025) is heterogeneous chemistry taking place on these particles.

Line 27-30: Furthermore, a key publication for this manuscript which is currently not referred to in the entire manuscript is this one:
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL110575
It highlights the effects of solid particle microphysics on optical properties… You should have a read.

Line 29-30: add: "… full **chemical, environmental** and climatic impact."

Line 30: Please also add 2-3 sentence about the limitations of solid particle SAI? E.g. technical challenges with injection and dispersion (see for aspects discussed in Vattioni et al, 2024 and 2025), substantially larger uncertainties and cost compared to sulphate SAI. And motivate your research by addressing these limitations. Maybe also have a read here: https://srm360.org/perspective/alternative-particles-could-reduce-side-effects-sai

Line 32-34: This sentence needs detailed references. Spencer et al. 2026 is not a peer reviewed paper yet. I doubt that silica is "chemically inert". This is a strong statement, which either needs robust references or further explanation and a clear declaration that this is not proven, but an assumption subject to significant uncertainty. Another aspect, or THE key aspect of a solid particle SAI candidate, which is not listed here, is a low imaginary refractive index. What about that?

Line 32-34: While I agree with reviewer 1 (Ben Kravitz) on his point 3, that your statement about "bio-compatibility" should be formulated more carefully, I do not share his concern about "silicosis". Silica (i.e., SIO_2) is the major component of desert sand (e.g., in the Sahara). Annually more than 50 Mt of sand (orders of magnitude more tan in SAI scenarios) is lofted into the air naturally alone from the Sahara desert and transported over thousands of km. However, deposition patterns from SAI are likely different from natural deposition patterns. Thus, this needs to be looked at carefully. Have a read here: https://iopscience.iop.org/article/10.1088/1748-9326/ab94eb

Line 36: Keith et al. 2016 is on heterogeneous chemistry on calcite, they do not report optical properties or stratospheric warming calculations. Provide a different reference for your statement here.

Line 39: It's not only monomers which coagulate. Fractals can coagulate with other fractals too.

Line 40: "lifetime" generally refers to chemical species. It's not the correct term here. For aerosols, the term "residence time" is more appropriate here. Also, throughout the paper.

Line 41-42: The coagulation is also a strong function of particle size. Also worth mentioning. Furthermore, I would be more concerned about local elevated concentrations in injection plumes, rather than elevated concentrations in the tropical pipe… Definitely worth mentioning too here. Or at least somewhere in the paper.

Line 45-50: Why are you only listing 2D models here? *"Spanning this space with three-dimensional chemistry–climate models is practically prohibitive."* Why are three dimensional models practically prohibitive? I don't think this is true. Quite the opposite. Three-dimensional models are an important tool to capture many effects such as dynamical climate feedbacks on circulation and stratospheric composition. These are important aspects for assessing the parameter space mentioned by you, which 2D-models cannot capture. You should discuss this limitation and proper motivate the use of a 2D model.

Line 50: *"Neither has been used for an SAI parameter survey in the scope undertaken here."* This is not true. What about this for example: https://acp.copernicus.org/articles/15/11835/2015/acp-15-11835-2015.html

There is also a good amount of follow-up papers using 3D models which focus on the parameter space mentioned by you. It's fine to use 2D models, if the purpose is appropriate, but this should be properly motivated.

Line 53-54: *"The 2-D framework is justified by the rapid zonal mixing of the stratosphere on seasonal and longer timescales, which makes latitude pressure the natural coordinate system for studying the Brewer–Dobson circulation and its interaction with injected aerosols."* You cannot study the interaction of the BD-circulation with aerosols by any means with your model setup! Transport is not interactive with radiation. What about the QBO? It is also important for global distribution of aerosols in the stratosphere: https://acp.copernicus.org/articles/21/8615/2021/

Line 55-56: This is at least the third definition of your parameter space here (see lines 45-46, and lines 1-2 and again in lines 60-61 and many more throughout the paper). The definitions are all slightly different. Make this consistent and indicate which exact parameter you want to analyse and why and how.

Line 57 and 94: Readers might not know what "Lagranto" is. Can you briefly explain and provide a reference?

Line 57-59: These sentences need references.

Line 74-78: These are just the highlights of omitted processes. If I see that correctly, you prescribe transport to overreactions using parameterisations derived from observations. Thus, in your simulations, transport does not react to anything. It is not interactive with any relevant process for SAI. You should make this clear here. Your transport not only omits feedback from stratospheric heating but also from the general perturbance of the radiative balance (e.g., reduced SW irradiance, which cools the climate and affects sensible and latent heat fluxes, among many more).

Line 78-80: I don't think this is the place to talk about future work. Promising future work does not add arguments to the presented methods. Thus, this is irrelevant here.

Line 80-81: *"These choices isolate the transport, microphysics, and direct-radiation response that is the focus of the parameter survey reported here."*
This is not true. Your simulations do not isolate the transport response (to SAI?). As you correctly explain above you do not account for any radiative feedback on transport and eddy diffusion… Thus, your transport is prescribed to observed fields using parameterisations. How exactly do you "isolate the transport response to SAI"? See also comment above. Also, you do not isolate the microphysics response. You only look at agglomeration and sedimentation which are two aspects of microphysics. As you correctly state you neglect interaction with sulfuric acid aerosols, which is also part of microphysics. Thus, be more specific here. Maybe reformulate to: "This approach allows estimating the global distribution of aerosol burden as well as the degree of particle agglomeration, which then serves as input to offline radiative transfer modelling to calculate the radiation response." Or similar.

Figure 1: You specify in the caption what orange and dashed cyan means. What about the other legend items and aberrations?

Section 2.2 Aerosol microphysics: This section needs much more details. How were solid particles represented? With modes or sectional bins? How many bins? How large are the bins? Do they represent Monomers, Dimers, Trimers, 4-mers, 5-mers ect.? What information is tracked with prognostic variables? What are your assumptions for the representation of the radius of the agglomerates? For sedimentation you need to account for the mobility radius. How do you derive the mobility radius? How is sedimentation represented (with what scheme)? This section needs much more details.

2 Model description: It is common to indicate the applied time step and the spatial (horizontal and vertical) resolution of models in the model description. What are the resolutions you applied? Please report and justify your selection.

Line 110-116: Why are you only comparing to Weisenstein et al. 2015? Have a look at this study: https://gmd.copernicus.org/articles/17/7767/2024/

Line 125-127: These two sentences need references? Which code did you use? Where did you take refractive indicates from?

Line 152-156: This statement/assumption is in complete contrast with what was found here:
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL110575

Line 159: Does this mean you applied a time step of one month in your simulations?

Line 161-162: This is not justified. Have a read of this study (also the SI):
https://gmd.copernicus.org/articles/17/7767/2024/

For sedimentation and coagulation the mobility radius is required which differs significantly from the equivalent sphere for agglomerates.

Line 172: So, absorption of SW radiation by the aerosols is neglected?

Line 173: Why “adsorbed shortwave radiation”? You talk about scattering here…

Line 165-167: *“Global values quoted in the text are obtained by averaging Neff over the simulated stratospheric distribution with each grid cell weighted by its aerosol mass, before applying the same compact-sphere formula.”*
I don’t understand this sentence… Very confusing.

Line 168-169: So coagulation is not always active? When it is active and when not? Before you write you calculate with the equivalent sphere radius (i.e., Df=3), now you write Df=1.6? Why? This is confusing?

Lines 195-201: The hydrological cycle is only one out of many aspects, which your approach does not address (e.g., tropospheric circulation response, precipitation patterns, land-sea differences … to only name a few). In fact, your approach is a back of the envelope approximation, not more.
And again, omit writing about future research here. This is not relevant here. You cannot get meaningful results of the climate response with any 2D model. Thus avoid calculating and showing ηTsurf based on your model results.

Figure 2: How do your Figure 2 compare to results shown here:
https://gmd.copernicus.org/articles/17/7767/2024/

Line 213: How do you drive a 2-D model with data with 1° x 1° horizontally resolved grid? Also, I thought you have a monthly time step? I am confused about the 3 hours here.

Line 218: What do you mean by “100 tracers”? Which tracers are you talking about? What do these tracers represent?

Line 233-237: Why are you assuming and injection of 20 Tg/yr? Isn’t this unreasonably large? Wouldn’t it be more meaningful to look at a 1 or 5 Mt/yr scenario? 5 Mt/yr would result in ~1 W/m2. Arguably a more reasonable scenario.

Figure 6: How was this figure derived? Did you run an injection scenario for all model latitudes and all model altitudes (what is the resolution? how many realisations?)? And then you plot the resulting residence time and d_{eff}? This needs more explanation.

Figure 6: The numbers and colour boundaries of your label bars do not align with each other. You could also introduce the τ for the residence time already for the figure headings here, not only in figure 7.

General: In atmospheric research the effective radius (or effective diameter) usually refers to the third mode of an aerosol distribution divided by the second mode of the aerosol size distribution, which I think is not what you are refereeing to here. Thus, I suggest avoiding the term "effective diameter" and use a different term instead (instead of d_{eff}).

Line 284: Be careful here with language. RF does not peak "in the tropical pipe", but for "injection into the tropical pipe". Be precise about this, not only here, but throughout the manuscript.

Line 266: In Section 2.3, line 173 you write the in the shortwave you only account for scattering, not absorption.

Table 1: You report an Epsilon for tropical injections of silica (d=0.5 um) of −0.22 W/m2/(Tg/yr). However, looking at your figure 9 at 0◦, 65 hPa your SW SARF is about -10 W/m2 and your LW SARF is about -2.2 W/m2, which would result in total SARF of -7.8 W/m2 for 20 Mt/yr injection. This would translate to an Epsilon of about 0.39 W/m2 / (Tg/yr), almost double the value you report.... How exactly did you calculate your Epsilon values? Same confusion for the other epsilons listed here and panels a and b of Figure 11.

Nevertheless, your tropical ηTsurf value for calcite is very close to the one shown for calcite in this paper, also assuming tropical injections, simulated with a fully-coupled Earth-System model: Stratospheric injection of solid particles reduces side effects on circulation and climate compared to SO2 injections - IOPscience
Have a read!

Furthermore, the disclaimer (a) below the table is misleading. It gives the impression that your transport is somehow interactive and only does not account for the heating response. However, your transport is effectively prescribed and not interactive with anything. You should be clear here.

Figure 11: Panel a and b show Epislon (W/m2 per Tg/yr), which is derived from RF values of the 20 Mt/yr injection case with 0.5 um silica particles, shown in Figure 9. Subtracting the LW SARF from the SW SARF in Figure 9 yields values of about 8-9 W/m2 for silica and 10 W/m2 for calcite for tropical injections around 50 hPa. However, in this region figure 11 show peak values of 0.25 and 0.35 W/m2 per Tg/yr (i.e., about 5 and 7 W/m2 for 20 Tg/yr injections). What am I doing wrong?

Figure 12: This figure looks almost identical as Figure 11. Differences are barley visible by eye. Why don't you show differences relative to Figure 11 here? That would immediately highlight the differences.

Line 347: "Saturation" of what? Can you be more specific here?

Line 347-348: Yes, you might resolve coagulation with your model, but you don't show that this effect does not apply to solid particles... You only show 20 Mt/yr injection cases. Why aren’t you showing different injection rates to demonstrate that this effect does not (or also does) apply to solid particles?

Figure 13: Your figure titles say that injection is only at X° N, but you mean X° N and S, right (i.e., two symmetric point sources)? Please correct this.

Line 351: How can it broaden and narrow at the same time? Be more specific here.

Lines 353-355: *“The pattern effect consequences of this RF shape on surface temperature and the hydrological cycle, which determine whether this latitudinal flexibility translates into a comparable degree of freedom in the climate response, will be studied in future work”*

This whole sentence is very cryptic, and I don’t understand what exactly you want to say. I would avoid talking about and promising future work but instead highlight the limitations of the analysis presented here.

Line 360-361: Please be more specific here. Which “particle properties” did you test? Only coagulation and size. You did not test “SAI performance” by any significant means.

Line 361-365: Better highlight the limitations of your work, instead of promising future work. Since the number of things that you do *not* consider is much larger than the few things you do, it might make more sense being explicit about the few things you consider, rather than being explicit about the things you do not consider.

4 Discussion and outlook: I am stopping giving detailed comments here, since there is no point doing that, based on the substantial flaws highlighted so far.

Line 465-469: The authors are doing an excellent and exemplar job on the data and code availability. The scripts and code are extremely well documented and accessible on the github-link. My only question is how permanent this link is. But as the authors promise, this will be turned into a repository with permanent doi, which would be fine in case of publication.

Figure A1 and lines 510-512: The y axis label of the upper row in Figure A1 suggest that what is shown is ERA5, but it is your calculation of ERA5. Please change the label and be clearer. I also suggest adding another row with the actual ERA5 climatological field. And then show your results as difference to the ERA5 data. This would make more sense to see whether what you did is accurate.

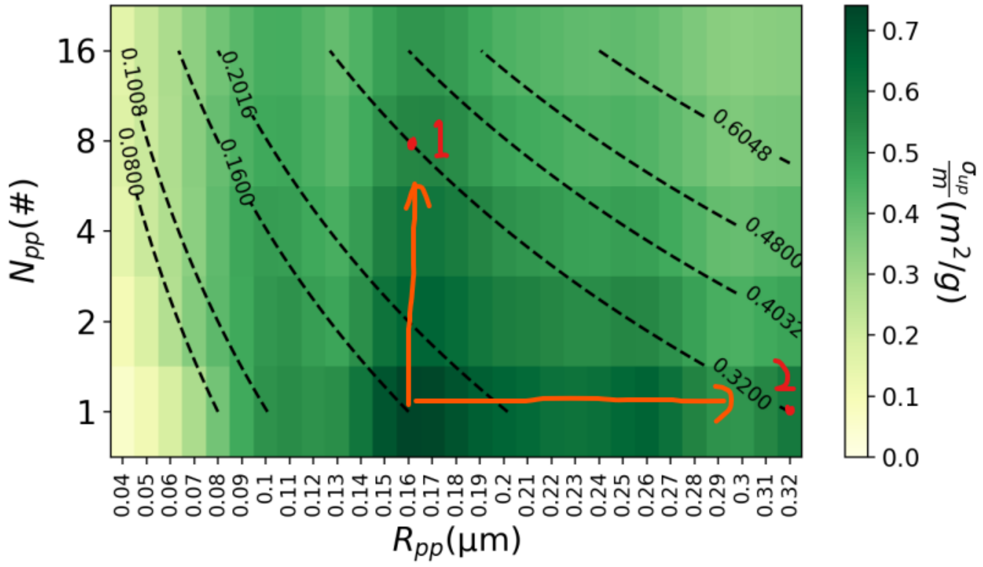
B1: Radiative forcing of a stratospheric aerosol layer.
This entire section contains many undefined variables. E.g. $S\lambda$. Same applies to B2.

Figure B2: Your decrease in radiative forcing efficiency per volume with increasing agglomerate size is much weaker than what was shown in this publication: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL110575

Why is that the case? Can you compare your values and methods to what was shown in the linked publication?

B6 Optical properties of agglomerates, Figure B2. The effective equivalent sphere assumption (deff = d0*N1/3) only holds for your particular optimal scattering particle size (i.e., d0=0.5 um for silica, and maybe d0=0.3 for calcite, which you don't show). From the optimal scattering particle size, the scattering cross sections decreases both with (1) increasing agglomerate size and (2) if you go to larger monomer diameter (see also figure below). Thus, the agglomerates have similar scattering cross sections as spheres of a larger diameter (as shown in your figure B2). But it doesn't hold d0 smaller or larger than the optimal scattering size. If you for example have a silica monomer of d=0.3 (as shown in your figure 10), the scattering cross section (1) decreases with increasing agglomeration, but (2) increases if you go to larger particle size. Thus, the assumption does not hold anymore. These effects are not linear. I suggest you validate the equivalent sphere assumption for calcite too, and show the same figure as for B2 for calcite as well to proof that the assumption also holds for calcite at the optimal scattering size. But it definitely doesn't hold for particles smaller than the optimal scattering particle size. Thus, your figure 10 is invalid.

The figure below shows mass normalized scattering cross section of material X derived with a quick MSTM calculation at wavelength 520nm. The figure shows in the x axis d0 (Rpp) and in the y axis number of monomers per agglomerate (Npp). 8 monomers equal a doubling in radius under the equivalent effective sphere assumption. If you have a monomer of r=0.16 um (i.e., your d0/2), the resulting values for 8-mers (point 1) and monomers of 0.32 um (point 2) are about equal. But if you for example go from r=0.08 um monomers and compare 8-mer values with 0.16 um values, substantial differences occur. Thus, you should clearly state that your assumption only holds for your optimal particle size. And you should prove the validity for calcite as well with a figure similar as B2.



Line 507-508: Can you please be explicit about the time step and the spatial resolution applied?

Line 667-668: *"The key radiative quantity for SAI efficacy is the solar spectrum-weighted backscattering efficiency* $\langle \bar{\beta} Qsca \rangle SW$*, which determines the fraction of incident solar radiation scattered back to space."*
How did you account for the zenith angle here? The zenith angle affects the *spectrum-weighted backscattering efficiency*.