Response to Referee 2 (S. Vattioni)

Manuscript egusphere-2026-2772: "Solid-particle stratospheric aerosol injection: a 2-D modeling exploration of the design space for amorphous silica and calcite"

Lederer, Wygoda, Halbertal, Kushnir, and Rose

Alongside this response we submit a revised manuscript, in tracked-changes and clean form, together with a revised Supplement. The referee's report is reproduced below in black italics; our reply to each part follows in blue. Line numbers refer to the clean (non-tracked-changes) revised manuscript.

The main changes of this revision: the Introduction is restructured (motivation, scope, the methodology applied and its suitability to that scope, and the assumptions and limitations); a cited comparison with previous work is added, stating where this study differs and comparing findings (lines 30–47 and Sect. 3.6); the DDA validation of the equivalent-sphere optics is extended (both materials, both monomer sizes, aggregates sampled up to $N = 32$; Appendix B); the Supplement now includes the comparison with sulfate on both forcing and heating (Figs. S1, S4 and S5); and a new subsection compares the forcing-level results with previous work, for calcite against Dykema et al. (2016) and Vattioni et al. (2024b) and for sulfate against Dai et al. (2018), Visioni et al. (2023), Duffey et al. (2025) and Bednarz et al. (2023), with the supporting fields added to the Supplement as the net-SARF row of Fig. S4 and as the forcing response functions of Fig. S8. Further changes are given in the line-by-line replies.

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.

The note concerns the authors and their organization and refers in part to manuscripts subject to their own review processes; we confine our reply to the manuscript under review and ask that the responses be judged on their content.

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?

The model was built for the scientific questions addressed in this manuscript. We consider models of this type well suited to forcing-level studies of this kind, and the model may prove useful for others who study SAI. The model and the data are fully public (github.com/stardust-initiative/solid-sai-2d-paper and dependencies within; Zenodo record DOI 10.5281/zenodo.20271742, concept DOI 10.5281/zenodo.20271741). The journal-policy question is for the editor.

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.

What this paper is (and is not). The paper is a study of two candidate solid materials, amorphous silica and calcite, at the level of radiative forcing (specifically, SARF), not climate response. This is an established first-stage methodology for candidate SAI materials (e.g., Pope et al., 2012; Ferraro et al., 2011; Weisenstein et al., 2015; Dykema et al., 2016; Dai et al., 2018). The study resolves how the injection latitude, altitude, and season, in interplay with particle-related parameters, shape the stratospheric aerosol layer, and maps the resulting forcing jointly with the stratospheric heating across that design space. The motivation for this approach, with the heating treated as a design constraint alongside the forcing, is set out in the quantitative safety and controllability requirements we propose in Waxman et al. (2026).

Scientific significance. (i) Engineered amorphous silica, to our knowledge not previously examined as an SAI candidate. (ii) The joint mapping of efficacy and heating versus injection strategy has not previously been carried out for solid-particle materials, and has been carried out for sulfate only in part (see the comparison table below); it shows that for a longwave-absorbing material the heating penalty can be reduced by injection strategy without severe loss of efficacy. (iii) A validated, open, ERA5-driven 2-D survey tool.

Why the approach is useful and valid. Heuristically, the forcing and the heating are the particle-dependent quantities. The climate response then follows through the climate sensitivity and feedbacks, which are properties of the climate system, not of the injected particle (Forster, Freckleton and Shine, 1997; Modak et al., 2016; Cao and Fang, 2026). Materials and injection

strategies are therefore properly compared at the forcing level, and the mapping of the forcing-heating trade-off is useful, as it can point to preferred strategies and guide subsequent, more detailed study with higher-complexity climate models.

The specific points behind the quality assessment (the discussion of limitations, the comparison to related work, and the literature) are each answered directly at the places the referee raises them, in the replies below. We think the points raised are matters for revision, undertaken as listed in the opening roll-up, not grounds for rejection. The subject, the stratospheric aerosol layer and its radiative effect, is in scope for ACP, and we ask that the revised manuscript be assessed on its scientific content.

Study	Material	Model	RF	ΔT_{str}	Detailed injection maps?	Coagulation	Heat→circ. fb.
Dykema et al. (2016)	calcite, diamond, alumina, SiC	1-D RRTM	yes	yes	no	no	no
Weisenstein et al. (2015)	alumina, diamond	AER 2-D	yes	no	no	yes	no
Dai et al. (2018)	sulfate	AER 2-D	yes	no	yes	no (small-rate limit)	no
Vattioni et al. (2024b)	calcite, alumina, diamond, SiC, TiO_2	SOCOL-AER, 3-D	yes	yes	no	yes	yes
Duffey et al. (2025)	sulfate	UKESM1, 3-D	yes	no	yes	yes	yes
Bednarz et al. (2023)	sulfate	CESM, 3-D	no	yes	4 cases	yes	yes
This work	**amorphous silica (new), calcite**	**2-D**	**yes**	**yes**	**yes**	**yes**	**no**

Representative solid-particle (and sulfate) SAI studies and the diagnostics they report. This content is incorporated in the revised Introduction as a concise prose comparison.

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 metrics we use for the analysis are not undeclared shortcuts; rather, they are defined clearly: SARF is defined with the troposphere and circulation held fixed (Forster, Freckleton and Shine, 1997), so the transport is prescribed and non-interactive, responding neither to the aerosol nor to the radiative perturbation, as now stated explicitly (lines 108–110, 129–131); the list of omitted processes at the start of Sect. 2 replaces the future-work framing (lines 108–118). The simplifications were never hidden: the submitted manuscript stated explicitly, in a dedicated paragraph

(its lines 73–81), what the model does and does not include: "Several feedbacks are deliberately omitted: aerosol radiative heating does not feed back into the transport circulation or eddy diffusion; stratospheric water vapor does not respond to tropopause warming; the lower-boundary loss is a prescribed-tropopause sink rather than a wet-deposition scheme; and ozone chemistry is not coupled to the aerosol surface area." The revision consolidates and extends that paragraph (Sect. 2, lines 108–118). On the missing microphysical details: the microphysics scheme we apply is not new to this work, and the manuscript says so explicitly. Sect. 2.2 states that the coagulation and sedimentation schemes follow "the formulations in Seinfeld and Pandis (2016), with the sectional solid-particle representation following Weisenstein et al. (2015); scheme details follow those works and are not reproduced here." The sectional bin structure and the aggregate conventions were already stated in the submitted manuscript (Sect. 2.4, its lines 160–169); the bin structure is now consolidated in Sect. 2.2, which also states explicitly the prognostic variable, the all-bin-pairs coagulation, and the settling radius; these likewise follow Weisenstein et al. (2015) and are stated for completeness (lines 154–161).

> *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/*

The paper draws no conclusions about climate impacts (see the reply above) and does not study the response of the QBO or the Brewer-Dobson circulation to the aerosol heating. On the implication that the missing feedback invalidates the forcing estimates: the heating acts on the circulation at the response level, and its effect on the forcing itself is second-order; in fully coupled sulfate simulations it reduces the forcing efficiency by 10 to 14 % even at 4 to 5 K of tropical heating (Niemeier and Schmidt, 2017), while, for reference, mid-latitude silica injection induces a low-stratosphere tropical heating of about 1.1 $\mathrm{K\,(W\,m^{-2})^{-1}}$ (Table 1). By comparison, the transport and coagulation treatments produce far larger differences at the forcing level. In the three-model intercomparison of Visioni et al. (2023, Part 1, their Fig. 4), which are fully coupled simulations that do include the aerosol heating and its circulation feedback, identical SO_2 injections give stratospheric sulfate residence times differing between CESM2 and UKESM1 by factors of 1.3 to 2.2 depending on injection latitude, global-mean column burdens differing by factors of 1.2 to 2.0, and zonal-mean burdens poleward of 60° differing by factors of 2 to 4. As a side note, we share the referee's view that the stratospheric heating should be minimized, and this is one of the motivations of this study; see Waxman et al. (2026, Sect. III.3), where we propose a quantitative safety requirement that the modification of the tropical lower-stratospheric temperature be limited to 1.5 K.

> *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.*

The Introduction is restructured and expanded. It now develops the motivation from the published literature: the limitations of sulfate-based SAI, and the case for a designed solid particle (reduced stratospheric heating, citing Wunderlin et al., 2024, and Stefanetti et al., 2024, and decoupling from stratospheric ozone chemistry), evaluated against the quantitative requirements

we propose in Waxman et al. (2026). It acknowledges the challenges and uncertainties of solid-particle SAI too (Vattioni et al., 2025). We do not attempt a weighing of advantages and disadvantages against sulfate: a designed solid particle is a broad family of solutions, and such a weighing involves many factors beyond the subject of this paper (see also the following reply). The Introduction further states the scope and working assumptions, explains why the forcing-level evaluation is appropriate, and closes with a cited comparison with previous work (lines 17–95).

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.

A full pros-and-cons assessment of solid particles against sulfate involves many factors beyond the subject of this paper, and, as the referee notes, a complete one-to-one comparison would require interactive sulfur chemistry and microphysics. We therefore add the limited comparison the referee suggests: a fixed-size sulfate case, without coagulation (following Dykema et al., 2016), compared against silica and calcite on both forcing and heating; the Supplement gives the sulfate SW/LW decomposition over the injection grid (Fig. S4), its latitudinal structure (Fig. S1) and the stratospheric-heating comparison (Fig. S5), and Fig. 9 (Fig. 10 of the revised manuscript) shows the same decomposition for silica and calcite (lines 372–379). In addition, the Discussion compares the forcing-level injection-map findings with the sulfate literature; this comparison is informative because amorphous silica is optically similar to sulfate (a comparable real refractive index and, like sulfate, a longwave absorber), so the sulfate injection-strategy results are a relevant benchmark at the forcing level (lines 472–480).

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 monomer-diameter dependence is presented as a parametric survey to guide particle design, and no attempt is made to assess a realistic post-plume size distribution; this is stated in Sect. 2 (lines 115–117) and in the Conclusions (lines 579–583). The model grid, time step, and ERA5 input resolutions are stated in Sect. 2 (lines 119–124).

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 cited and used, in the contexts appropriate to a forcing-level study: Vattioni et al. (2024a) in the Introduction (lines 51–54); Wunderlin et al. (2024) as part of the central motivation (lines 25–28). Of the injection-strategy examples: Visioni et al. (2019), which addresses the forcing level directly, is cited in the sulfate strategy-precedent sentence (lines 45–47); Kravitz et al. (2019), a climate-response study, is cited there as part of the strategy design axis, the response level itself being outside the stated scope; and Zhang et al. (2024) was already cited in the submitted manuscript and remains in the symmetric-injection discussion (Sect. 3.3).

> *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 11 $\mathrm{W\,m^{-2}}$ the referee reads is taken from the wrong figure: Fig. 9 (Fig. 10 of the revised manuscript) shows the no-coagulation case (as stated in its caption) and is not used for any normalization. The reported quantities are the with-coagulation ones: Table 1 gives $\epsilon = -0.22\ \mathrm{W\,m^{-2}\,(Tg\,yr^{-1})^{-1}}$ for tropical silica and $-0.13\ \mathrm{W\,m^{-2}\,(Tg\,yr^{-1})^{-1}}$ at mid-latitude, i.e., net SARFs of about -4.4 and $-2.6\ \mathrm{W\,m^{-2}}$ at 20 $\mathrm{Tg\,yr^{-1}}$, and the ϵ maps (Figs. 11 and 12; Figs. 12 and 14 of the revised manuscript) are computed with coagulation active at the full rate (see also the Table 1 reply below). On the suspected nonlinearity the referee is correct, and it is now quantified: for the cases of Table 1 we added ϵ versus injection rate (Sect. 3, lines 400–410); the efficacy weakens by roughly 30–40 % between 2.5 and 20 $\mathrm{Tg\,yr^{-1}}$ at the tropical point, less at mid-latitude, with the design conclusions preserved, and the aggregate bin populations versus rate are given in the Supplement (Fig. S2).

> *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.*

As stated above, the scheme is not new to this paper, and part of the requested detail was already in the submitted manuscript (its Sect. 2.4, lines 160–169: the bin structure, the aggregate-diameter convention, and D_f per Weisenstein et al., 2015, Eq. 1). The revision now states explicitly the prognostic variable, the all-bin-pairs coagulation, and the settling radius, which likewise follow Weisenstein et al. (2015) (Sect. 2.2, lines 154–161); the model grid, time step, and ERA5 input resolutions are stated in Sect. 2 (lines 119–124), with a resolution-sensitivity comment.

> *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.*

Addressed quantitatively in the Figure B2 and B6 replies below: the DDA validation of the equivalent-sphere optics is extended (both materials, both monomer sizes, aggregates sampled up to $N = 32$); the bin-population-weighted deviation is $-3\%/+1\%$ for silica and $+8$ to $+13\%$ for calcite (bin populations in Supplement Fig. S2).

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

Every line-by-line comment is answered below.

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.

The Introduction now defines the design space once (lines 63–64) and confines the evaluation to the forcing level, with the processes not coupled stated explicitly in Sect. 2 (lines 108–118); that injection strategy is equally a design axis of sulfate SAI is noted with the sulfate strategy precedents (lines 45–47).

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.

The text now states what is actually varied: material and monomer diameter, with the aggregate fractal dimension examined as a sensitivity, since it is set by post-injection aggregation rather than by design (abstract; lines 63–64 and 577–579).

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.

The abstract now presents the framework as "a tool to screen this design space at the forcing level and identify promising (material, injection strategy) combinations for more detailed study with higher-complexity models" (lines 6–7), and the Introduction motivates the 2-D choice in the same terms, acknowledging the feedbacks resolved only by 3-D models (lines 80–88).

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".

Replaced throughout with "simulations"/"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...

The abstract now names the reference against which each component is validated (lines 7–9).

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

Now "optical properties (refractive indices)" (line 624).

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.

ϵ is defined in Eq. 1 as SARF/R: per unit injected mass rate, not burden. Stated at first use in the abstract (line 2) and at the definition (lines 266–268); usage consistent throughout.

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

The abstract defines the efficacy at first use (line 2); "efficacy loss" refers to that quantity.

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?

See the revised abstract.

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.

Describing SAI as partially offsetting the greenhouse-gas radiative forcing with a negative aerosol forcing is standard usage: the net anthropogenic forcing is the sum of its components, and "offset" refers to that sum, not to a modification of the greenhouse-gas forcing itself. This is the framing of IPCC AR6 (Ch. 7), of Weisenstein et al. (2015), and of the referee's own publications (e.g., Vattioni et al., 2025), among many others. The sentence is nevertheless revised to remove the ambiguity: SAI imposes a negative radiative forcing that can partially offset the positive greenhouse-gas forcing in the net radiative balance (lines 17–20).

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.

Two of these are added where sulfate is introduced as the reference material: the natural stratospheric occurrence and the volcanic observational analogue (lines 21–24).

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.

The passage is reformulated. Sulfate is introduced as the standard reference material of the field, resting on its natural stratospheric occurrence and on the volcanic record, which provides valuable observational data (lines 20–25).

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

Reduced stratospheric heating is among the central limitations of sulfate in the revised motivation, citing Wunderlin et al. (2024) (lines 25–28); Stefanetti et al. (2024) is cited for the fully coupled climate response of solid-particle SAI (lines 38–39).

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

The sentence was replaced in the restructured Introduction; "developed" no longer appears in this context.

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.

The heterogeneous-chemistry and microphysics work is cited in the Introduction (Vattioni et al., 2023, 2024b; lines 34–36), and Vattioni et al. (2024a) at the calcite reduced-heating statement (lines 51–54).

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

"full chemical, environmental, and climatic impact" (line 37).

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

The challenges of solid-particle SAI (injection and dispersal at scale, substantial microphysical and chemical uncertainties) are acknowledged, citing Vattioni et al. (2025) (lines 36–38). We do not weigh solid particles against sulfate: a designed solid particle is a broad family of solutions (see the general reply).

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?

Chemical inertness is now a working assumption subject to verification, not an established fact: "The chemical inertness of both materials in the stratosphere is adopted here as a working assumption subject to verification" (lines 54–55). The absorption implied by the imaginary refractive index is in the desirable-properties list, negligible shortwave absorption and comparatively low longwave absorption (lines 49–50), and is computed from the complex refractive indices in Sect. 2.3 and Appendix B5. The paper the referee refers to as Spencer et al. (2026) is Spector et al. (2026); as it is not yet peer reviewed, its citation and reference are removed from the manuscript.

> *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., SIO2) 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*

Silicosis is indeed a disease of crystalline silica (quartz and its polymorphs), whereas this study concerns engineered amorphous silica, a distinct non-crystalline material of lower density (2200 against $\sim$2650 $\mathrm{kg\,m^{-3}}$ for quartz) with a different regulatory and toxicological classification. "Bio-compatibility" is dropped and the amorphous-silica specification retained. Deposition is downstream of the stratospheric problem treated here: where the prescribed-tropopause sink is stated (Sect. 2), the manuscript now notes that deposition has been studied for sulfate SAI (Visioni et al., 2020) and should be assessed per designed particle, outside the scope of this work.

> *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.*

The sentence now reads that calcite, while chemically reactive in its bare form, has attracted growing interest for its minimal longwave absorption, which results in greatly reduced stratospheric heating, citing Keith et al. (2016), Dykema et al. (2016), and Vattioni et al. (2024a, 2024b) (lines 51–54).

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

The model treats this: coagulation acts between all bin pairs on the $N \in \{1, \ldots, 128\}$ grid; stated accordingly (lines 70–74 and 158–160).

> *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.*

"residence time" is used throughout (defined at lines 241–242).

> *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.*

The size dependence of the coagulation rate is noted, and local plume concentrations are mentioned as unresolved by this framework (lines 72–74); the plume-scale limitation is stated at lines 115–117.

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.

The claim about three-dimensional models is withdrawn. The Introduction now motivates the 2-D choice positively: a zonally averaged framework is well suited to spanning the full grid used here, 168 injection locations crossed with seasonal schedules and the particle parameters, with each case integrated to steady state, and 2-D models have long served this role (lines 80–88, with the reference list extended; the 168-location grid is specified in Sect. 2, line 122). The corresponding sentence in the Conclusions is likewise reworded, and the limitations of the 2-D treatment are stated in Sect. 2 and in the forcing-level statement rather than as a comparison.

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.

Corrected: the Introduction now states explicitly what the prior studies did and what this survey does beyond them. Weisenstein et al. (2015) resolved coagulation, for a single injection scenario and without reporting the heating; Dykema et al. (2016) reported both forcing and heating, but without transport, coagulation or an injection-strategy dimension. Here the efficacy and the heating are mapped jointly across the full injection grid with coagulation resolved (lines 30–47).

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/

Changed to "latitude-pressure the natural coordinate system for describing the stratospheric aerosol layer under the prescribed Brewer-Dobson transport" (lines 82–84).

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.

Single definition at first use, referred to consistently (lines 63–64).

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

Reference added at first mention (lines 89–90); LAGRANTO is described in Sect. 2.1 (lines 141–142).

Line 57-59: These sentences need references.

References added (lines 89–93).

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).

See our reply above; stated explicitly at lines 108–110 and 129–131.

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.

The future-work framing is removed.

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.

This misreads the sentence: "the transport, microphysics, and direct-radiation response" lists the processes the framework retains, with the direct radiative response meaning exactly that, no other response; the sentence does not claim to isolate a transport response to SAI. The wording is nevertheless refined to preclude the misreading (lines 117–118).

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

Added; see the Fig. 1 caption.

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.

Answered in our reply to the general comments above; the conventions are stated in Sect. 2.2 (lines 154–161). On the mobility radius, see the Lines 161–162 reply.

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.

Reported in Sect. 2 (lines 119–124): the model grid, the ∼20-minute time step, the 168-point injection grid, and the resolution of the ERA5 input fields (3-hourly, 1° × 1°, 137 levels), with a comment on resolution sensitivity.

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/

The comparison to Weisenstein et al. (2015) is a verification of the implementation: our sectional scheme is adopted from that work, so agreement against it tests the code, not the physics; a comparison to a different model would not serve that purpose.

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

The refractive-index sources are cited (lines 188–189: Kitamura et al., 2007 for silica; Long et al., 1993 for calcite), and the Mie calculations are computed with `miepython` (Prahl), now cited at the calculation (lines 188–189) and in Appendix B.

Line 152-156: This statement/assumption is in complete contrast with what was found here: https://agupubs.onlinelibrar

See our replies to the Figure B2 and B6 comments below.

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

The 1° × 1°, 3-hourly, 137-level ERA5 fields are used to derive the monthly zonal-mean transport coefficients; the 2-D model is then integrated on its own latitude-pressure grid with ∼20-minute time steps using those coefficients. The two resolutions are separated explicitly (lines 119–124).

Line 161-162: This is not justified. Have a read of this study (also the SI): https://gmd.copernicus.org/articles/17/7767/ For sedimentation and coagulation the mobility radius is required which differs significantly from the equivalent sphere for agglomerates.

The settling radius follows Weisenstein et al. (2015, Eq. 1) and was stated in Sect. 2.4 of the submitted manuscript; it is now consolidated in Sect. 2.2 (lines 159–161), distinguished from the optical equivalent-sphere diameter.

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

No: shortwave absorption is computed from the complex refractive index and shown to be weak for silica and calcite (see Appendix B for details).

Line 173: Why "adsorbed shortwave radiation"? You talk about scattering here...

In energy balance, ASR and OLR are the common abbreviations for the TOA breakdown. The scattered sunlight increases the planetary albedo and reduces the top-of-atmosphere ASR, and the shortwave forcing is that ASR anomaly (lines 259–261).

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.

Rewritten: mass-weighted bin averaging is applied first, then spatial averaging (lines 249–251).

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?

The text and the figure captions state for each figure whether coagulation is active: it is not in Figs. 6a, 9, and 10a (7a, 10, and 11a of the revised manuscript) and Supplement Fig. S1; it is in all other results. As for D_f: the default is $D_f = 1.6$, as in Weisenstein et al. (2015), and the change to $D_f = 3.0$ is tested in Fig. 7 (Fig. 8 of the revised manuscript). The compact-sphere $d_{\mathrm{agg}} = d_0 N^{1/3}$ is only a reported diagnostic (lines 245–247).

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 $\eta_{T\mathrm{surf}}$ based on your model results.

$\eta_{T_{\mathrm{surf}}}$ is presented as exactly what it is: an approximate first-order design metric which uses a prescribed climate sensitivity: the manuscript takes $\lambda_{\mathrm{CO_2}}$ from IPCC AR6 (Ch. 7), weighted by the solar-versus-CO_2 efficacy factor of 0.8 to 0.9 assessed by Modak et al. (2016) and Cao and Fang (2026). Where it can be compared, our tropical calcite $\eta_{T_{\mathrm{surf}}}$ is close to the fully coupled Earth-system value of Stefanetti et al. (2024), which might support its use as a first-order indicator (see the Table 1 reply). Duffey et al. (2025) estimate cooling from forcing in a similar way, see their Sect. 2.4.

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

Fig. 2 is a verification of the implementation against the model the scheme is adopted from (Weisenstein et al., 2015); see the Line 110–116 reply.

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.

The 1° × 1°, 3-hourly, 137-level ERA5 fields are used to *derive* the zonal-mean transport coefficients; the 2-D model is then integrated on its own latitude-pressure grid with ∼20-minute time steps using those coefficients. The two resolutions are separated explicitly (lines 119–124).

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

The 100 LAGRANTO trajectory parcels released per injection event (every 3 days at the prescribed latitude and pressure, longitudes drawn at random) in the transport benchmark. Clarified (lines 309–311).

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.

The rate dependence is checked across 2.5–20 $\mathrm{Tg\,yr^{-1}}$ (Fig. 13 of the revised manuscript, lines 400–410; bin populations versus rate in Supplement Fig. S2).

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 deff? This needs more explanation.

Each grid point is an independent steady-state injection run on the 14×12 latitude-pressure injection grid (168 locations), with the diagnostics read from the steady state (final twelve monthly steps of each ten-year run). Stated in Sect. 2 (lines 119–122) and at the survey definition in Sect. 3.

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.

The colour-bar ticks now sit on the band boundaries and τ is introduced in the Fig. 6 panel headings (Fig. 7 of the revised manuscript). Panel (c) now shows the residence-time ratio $\tau_{\mathrm{coag}}/\tau_{\mathrm{no\,coag}}$ directly (lines 330–332), with the aggregate-diameter map moved to the Supplement (Fig. S3).

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 deff).

Renamed d_{agg} ("mass-equivalent aggregate diameter"), defined at first use and explicitly distinguished from the area-weighted effective diameter (lines 243–249); applied throughout the text, equations, and captions.

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.

Corrected throughout: the diagnostics are functions of injection location, e.g. "peaks for injection into the tropical pipe" (line 392 and elsewhere).

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

See our reply to the Line 172 comment above.

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 $\eta_{T\mathrm{surf}}$ 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.

The referee misreads Fig. 9 (Fig. 10 of the revised manuscript): it shows the no-coagulation case, as its caption states, and it is not used for any normalization. Table 1 and the ϵ maps are computed with coagulation, which roughly halves the per-mass backscatter; this factor is itself a central finding of the paper. The coagulation status is now stated in the panel titles of Figs. 9, 11, and 12 (Figs. 10, 12, and 14 of the revised manuscript) as well as in the captions, and the

Table 1 disclaimer no longer implies interactive transport. On the calcite $\eta_{T_{\mathrm{surf}}}$ agreement with Stefanetti et al. (2024), see the $\eta_{T_{\mathrm{surf}}}$ reply above.

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?

The same misreading: the 8–10 $\mathrm{W\,m^{-2}}$ values are from the no-coagulation Fig. 9 (Fig. 10 of the revised manuscript). The ϵ panels are with coagulation and self-consistent: dividing their SARF by the rate reproduces the 0.25–0.35 the referee reads. Panel titles now state the coagulation status.

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.

Fig. 12 (Fig. 14 of the revised manuscript) now shows the seasonal-minus-symmetric differences directly (lines 416–422).

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

Specified: the radiative-forcing efficiency of sulfate declines with injection rate (lines 477–480).

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?

The rate dependence is checked across 2.5–20 $\mathrm{Tg\,yr^{-1}}$ (Fig. 13 of the revised manuscript, lines 400–410; bin populations versus rate in Supplement Fig. S2).

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.

Yes, the injection is bilateral; the captions now state it (Fig. 13, now Fig. 15, caption).

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

The profile changes depending on injection latitude (Fig. 15 of the revised manuscript and lines 481–484).

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.

The sentence's point is that the meridional shape of the forcing is a design degree of freedom: how such regionally structured forcing translates into the climate response is studied in the literature (e.g., Roose et al., 2023, for the monsoon response to the interhemispheric forcing asymmetry; Zhang et al., 2024, for regional surface responses to injection strategy). The revised text states this and notes that the translation itself is not determined by this study (lines 484–488; 652–656).

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.

Stated specifically (lines 577–579; 93–95).

*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.*

The manuscript states what it does and its working assumptions once, where they belong (Introduction; Sect. 2, lines 108–118), and the future-work framing is removed. We do not agree that the text should enumerate everything it does not consider: the scope is defined positively, at the forcing level, and the relevant simplifications are stated where they act.

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.

The replies above address every specific concern raised in the report. We would welcome further specific comments on the Discussion.

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.

The Zenodo repository is populated and live, with citable DOIs (Data and Code Availability, lines 690–694).

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.

The row labels are clarified: "ERA5 (2008–2017 clim)", our derivation, and "GSFC 2-D (year 2000)". The suggested additional row cannot be made, as there is no ERA5 $D_{\varphi\varphi}$ field to show: the ERA5 inputs are 3-D wind and temperature fields, from which the 2-D diffusion coefficient is derived (Appendix A). The accuracy check is therefore the comparison shown, against the independent GSFC 2-D reference.

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

The appendix symbols are defined at first use; $S_{\lambda,0}$, previously undefined, is now defined (line 791).

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/10.1029/2024GL
Why is that the case? Can you compare your values and methods to what was shown in the linked publication?

The comparison is not valid, because the material is not the same. The aggregate curves of Vattioni et al. (2024a, their Figs. 1c and 1d) are for diamond, $n = 2.42$, whereas the materials considered here are amorphous silica, $n \approx 1.46$, and calcite, $n = 1.57$. The sensitivity of the backscatter efficiency to departures from the optimal radius grows with refractive index, so a steeper decline with aggregate size is expected for diamond and carries no implication for silica or calcite.

*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.*

The DDA validation now covers silica and calcite, both monomer sizes ($d_0 = 0.3$ and 0.5 μm), and compact and loose aggregates sampled up to $N = 32$ (60 configurations; Fig. B3 of the revised manuscript). The main takeaway is that the volume-equivalent-sphere optics is a reasonable working approximation: weighted by the steady-state bin populations at the full injection rate (Supplement Fig. S2), the expected accuracy is $-3\%/+1\%$ for $d_0 = 0.5$ μm silica (tropical/mid-latitude injection) and $+8$ to $+13\%$ for $d_0 = 0.3$ μm calcite, and Fig. 10 (Fig. 11 of the revised manuscript) carries this quantified uncertainty rather than being invalidated. The referee's MSTM map shows σ_{up} per unit mass at a single wavelength (520 nm) for an unspecified material, which is not enough data for a comparison against Fig. B3; we suspect that the differences in behavior relate to the absence of solar-spectrum weighting, which has a significant effect, and to a higher refractive index of material X. His failure case ($r_0 = 0.08$ μm) lies below the monomer sizes used here.

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

Stated in Sect. 2 (lines 119–124).

Line 667-668: "The key radiative quantity for SAI efficacy is the solar spectrum-weighted backscattering efficiency $\langle \bar{\beta} Q_{\mathrm{sca}} \rangle_{\mathrm{SW}}$, 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.

The zenith-angle dependence is handled in the radiative-transfer step: the daily-averaged cosine of the zenith angle is chosen to reproduce the diurnally integrated scattering response, validated against MODTRAN (Fig. 3; lines 926–928).

The revisions above address the specific scientific concerns raised in the report.

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