

Atmos. Chem. Phys. Discuss., referee comment RC2  
<https://doi.org/10.5194/acp-2022-372-RC2>, 2022  
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## Comment on acp-2022-372

Anonymous Referee #2

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Referee comment on "Climate response to off-equatorial stratospheric sulfur injections in three Earth System Models - Part 2: stratospheric and free-tropospheric response" by Ewa M. Bednarz et al., Atmos. Chem. Phys. Discuss.,  
<https://doi.org/10.5194/acp-2022-372-RC2>, 2022

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This manuscript details differences between simulations of stratospheric aerosol injection (SAI) among three models, here focusing on SAI responses in layers of the atmosphere well above the surface, with a large focus on circulation and chemistry. The in-depth exploration of model differences and their causes herein is commendable, and is very on topic for the special issue. Attempts to evaluate geoengineering strategies with simulations have been plagued by poorly understood disagreements between models. Hence, this assessment could be useful for future attempts to quantify SAI uncertainties, as well as to inform model developers and users intending to explore SAI and related scenarios. However, the manuscript explores the different responses among models without much attempt to communicate why these responses matter and why the intermodel disagreements are worth assessing. The manuscript thus in its current form presents itself more like an academic exercise than the scientific contribution it clearly could and should be. The reviewer requests major revisions, most critically textual changes to clarify the context and significance of findings.

General comments:

The text should be augmented to explain the significance of its findings. There's no mention of the how the stratospheric and free-tropospheric responses detailed herein could matter to humans, ecosystems, to what extent SAI is a viable strategy, or to how SAI might be designed to minimize risks and uncertainties (given the focus on varied-injection-latitude SAI experiments). Contrary to the final line of the abstract (line 48), this study does not really explore "climate impacts from SAI" in the typical sense – temperature and precipitation changes at Earth's surface, which is instead discussed only in PART1. The significance of this study, PART2, would be far clearer if the text links the stratospheric and free-tropospheric changes it analyzes to their possible ramifications for surface climate, citing relevant figures in PART1 as it does so. As is the current study only makes two passing references to the surface temperatures in PART1 (lines 224 & 230),

leaving the reader with an uncommonly large amount of detective work to appreciate the significance of the results. This study should be amended to spell out the significance of the stratospheric and free-tropospheric responses it focuses on, including a handful of comments on how the circulation and chemistry features discussed here might impact the surface climate responses presented in PART1.

Related to the first point, the study should clarify the links between model results and real world implications, as this is central to its purpose. Line 64-5: “[M]odel intercomparisons are useful in understanding uncertainties in climate responses to SAI”. There is truth in this statement but it should be explained to the reader, as it is central to the significance of this study yet is not trivial. Under the assumptions that models differ because of poorly constrained parameters / process rates and the collection of models sufficiently samples these uncertain inputs, the spread of results across models can be used as a proxy for uncertainties in the real world (here being uncertain outcomes of SAI). But if intermodel differences are instead caused by bugs or identifiable biases, their use as a proxy for real world uncertainties is diminished – nevertheless, identification (and ultimately correction) of these issues is an important step toward the original purpose (hence this work has a technical purpose that supports the main scientific one). Running experiments in three/four models is a commendable effort though too few to truly cover the uncertainties, while the presence of model bugs discovered here complicates the applicability of simulation results to the real world. Fortunately, as is demonstrated, the number of models is sufficient to identify major differences and facilitate comments on their causes, giving this work value. Some discussion along these lines should be added as context for readers who are not climate modelers, to help them understand the results of this study.

The paper should indicate which model results are more and less reliable estimates of SAI responses that would occur in the real world. Out of the four models used, some are clearly less useful than others for some purposes. Most glaring is the use of the GISS OMA scheme, which should be explained upfront. Due to the lack of interactive aerosol size this model cannot be expected to be as realistic as the others for SAI experiments, wherein aerosol size is expected to grow to far greater size than as emitted. As a reader might naively treat it initially as on equal footing with the other, more fully interactive aerosol models. Perhaps GISS OMA’s use here is meant to represent early models used in interactive aerosol geoengineering experiments that similarly had fixed aerosol size? Or more generally as a benchmark to demonstrate the necessity of the more interactive two-moment schemes? The reason for its use should be presented upfront, as its divergence from other model results is more of an expectation than a novel finding as presented here. Similarly, if “GISSmodal” (which is an inappropriate name for this model, as explained below) has a large ozone bias, the ozone biases from CESM2 and UKESM should be treated as the best approximation of the real world ozone response. The manuscript would be best if it made statements based on the collection of models deemed trustworthy for each response, rather than predominantly explaining separately what the response is in model 1, then model 2, then model 3.

GISS ModelE with MATRIX is not a “modal” scheme despite having similarities, so “GISSmodal” should be replaced with “GISSmatrix” (in both PART 1 and 2, for consistency). As explained in Bauer et al 2008 (<https://doi.org/10.5194/acp-8-6003-2008>), MATRIX is based on the Quadrature Method of Moments, which “provides a computationally efficient statistically-based alternative to modal and sectional methods for aerosol simulation that does not make a priori

assumptions about the shape of the size distribution". What causes confusion is that outside the core equations of this scheme, MATRIX then treats each of its aerosol populations as lognormal size distributions, making its output look a lot like that from a modal method (but with a larger number of aerosol populations). For PART1 and PART2 to refer to "GISSmodal" would be quite bad in that this would make the distinction even more confusing for anyone who comes across these studies. Similarly, text in the manuscript that discusses "modal models" with MATRIX in this category should rename the category to "two-moment models", as what these models share is more accurately that they enable both aerosol mass and size to change (two-moment), as compared to mass only (one-moment, as with OMA). "GISSmatrix" is the best renaming option, as it clearly connects to other uses of the MATRIX scheme in the literature, eg for AEROCOM and CMIP. Another potential renaming option might have been "GISStwo-moment", but this would create confusion with the separate TOMAS two-moment scheme option in GISS ModelE. While not as incorrect as "GISSmodal", "GISSbulk" would be better replaced by "GISSoma" in order to also connect more clearly to other literature using the OMA scheme.

The relevance of the off-equatorial experiment setup should be explained more, as it currently seems to be an afterthought despite its central placement in the title. One would anticipate that, given the title, the results of this study are meant to guide SAI injection strategy, but there's no statement of what the results imply for what injection latitudes are less problematic or have less uncertainties than others. Please make a statement on this. Presumably such a statement would take into the consideration the findings of both PART1 and PART2.

As written the abstract is more of a summary than an abstract and includes substantially more technical details than necessary, obscuring its usefulness. It likewise gives almost no indication why these findings matter. The abstract should be rewritten to clearly emphasize what the main intentions of the study are within its first few lines, and should more succinctly summarize the scientific results and their merit (eg key areas where models agree vs disagree and why these matter for potential plans to use SAI).

Specific comments:

Lines 21, 91, 119, 333: The injection latitudes are unnecessarily listed four times. Please remove at least two of these instances to be less repetitive.

Lines 46-8: "[O]ur results contribute to an increased understanding of [...] the sources of uncertainty in model projections of climate impacts from SAI". This is a critical statement of purpose yet as written encloses itself in 'model land', neglecting the greater goal to further understanding of SAI uncertainties as they would matter in the real world. Please rework this to convey this study's significance in understanding how the real world might be under the hypothetical SAI scenarios.

Lines 55-6: Please briefly state why these "side-effects" are important and worth study.

Lines 64-5: Yes, "models are themselves imperfect", but this does not in itself make them useful for understanding uncertainties, as stated (the expectation would be that model imperfections make them not useful). Please rework this in consideration of the general comments (second paragraph).

Line 107: Three points here. First, "the key findings", not "they key findings". Second, what "key findings"? Third, the introduction would be stronger if its last sentence were a more general segue to the next section.

Lines 110-123: The methods section is awkwardly quite short for the obvious reason that nearly everything is in PART1. Perhaps some of the material in the "Results" section actually belongs here and should be moved? I'm thinking particularly of the SAD diagnostics (Lines 130-139).

Line 115: Please explain here why GISS OMA is used. Its lack of dynamic aerosol size would seem inappropriate for a study where so much sulfur is emitted that the aerosol would be nowhere like their fixed emission size (see discussion of this in general comments, paragraph three).

Line 115: GISS ModelE's MATRIX is not a "modal" scheme so the name and description of "GISSmodal" should be altered to clarify this (see general comments, paragraph four).

Lines 115-6: Please alter or add to this sentence to instead trace out how the methods (models + custom output) are used to achieve this manuscript's main aims (see general comments, first three paragraphs). To "test the importance of detailed representation of aerosol processes for the simulated response" doesn't seem like a major goal, unless one of the main goals is to argue the 3

Lines 137-8: Where are the "2" in Eqn. 1 and the "4.5" in Eqn. 2 from? Lognormal statistics? Please specify.

Line 138. The "r" in Eqn. 2 looks like it should instead be an "ri".

Lines 176, 193, 215, 217, 235, 242, 296, 310, 314, 358, 370, 377, 389, 422: All these lines generalize the 3 models other than GISS OMA as "modal", despite "GISSmodal" being a misnomer for what is not properly a modal model. More appropriately the

commonality is that these are “two-moment” models, representing both mass and number as changing rather than just mass.

Line 188: Cite that the QBO response is focused on later in the manuscript (within Section 5.3.5).

Line 202: It would be surprising if the LW effect is indeed substantially aerosol-size sensitive, as is more established with the SW effect. Does the Laakso et al 2022 really show this? Their Section 3.1.1 raises other reasons (differences in optical properties and radiative transfer schemes).

Lines 235, 272, and 301: The sectional breakdown between 3.3.1, 3.3.2, and 3.3.3 seems clunky. 3.3.1 sounds like it contains nearly all the info (“tropics” and “mid-latitudes?”). Perhaps 3.3.1 and 3.3.2 should be combined into a single “Stratospheric ozone” section in contrast to what’s now 3.3.3 (“Tropospheric ozone changes”).

Line 237: Please briefly explain why stratospheric ozone response to SAI is important. Is it that this matters to human health via impacts on UV radiation? Might ozone’s role as a greenhouse gas impact the surface cooling effectiveness as shown in PART1?

Line 258: Missing a period.

Line 272: Please briefly state why one should care about the SAI ozone response specifically in the Antarctic stratosphere.

Line 301: Please clarify why tropospheric ozone response to SAI is important, and why it should be discussed separately from stratospheric ozone.

Line 322: Why does stratospheric water vapor response to SAI matter? Is this through chemistry influence that itself matters for health via ozone/UV or feedbacks on surface climate? Or water vapor acting as a greenhouse gas in a way that itself alters the radiative forcing, and hence SAI efficiency? This should briefly be explained.

Line 342: This section (3.5) details features of “zonal winds” under SAI, yet makes no reference to the Northern and Southern Annular Modes, the most frequently discussed zonal wind structures in the stratosphere. Changes to these structures are apparent in Fig. 10, so may deserve explanation in the section. If the simulations are too short for interpretation to be worthwhile (mentioned vaguely in lines 442-4), explain here this

decision rather than ignoring NAM and SAM entirely.

Line 356: Replace "a lot of" with less casual wording (eg "extensive").

Line 356-7: "due to the short length", not "due to short length".

Line 357: "prevents", not "prevent"

Lines 409-44: It would be well worth going over these paragraphs to ensure they advertise the manuscript's best scientific and technical strengths, since there's room for improvement here (see general comments).

Line 409-11: Is this statement an accurate representation of the study? Given this study is wholly assessing models of hypothetical SAI scenarios (no observations), it doesn't thoroughly comment on what is "realistic". As expressed in the general comments, the goals of this study should be presented more clearly.

Lines 434-6: This is a restatement of lines 46-8, so please see the comment for those lines. This would be an appropriate place for discussion of just how applicable the intermodel spread is to understanding SAI uncertainties, as they would relate to actual deployment in the real world (see general comments, paragraph 2).

Line 439: Does this locking of the QBO matter? Should this inform what latitudes SAI should be injected at? Please make a concluding statement on (or at least discussion of) which latitudes should or should not be used, considering the relative importance of this QBO locking to other injection latitude sensitivities from both PART1 and PART2. This would seem to be the natural wrap up for the "off-equatorial" injection focus in the study's title.

Line 440: Missing comma, "[...] of the simulations, detailed [...]" .

Lines 442-4: Please clarify what dynamical responses are being skipped due to the short simulation length. Please also state the rough number of years or decades that would be needed.

Figs. 5-7,9,10: Please revise each figure to have only one large colorbar to avoid

overcrowding.

Fig. 6: What does the black contour shading represent? This is not on the colorbar.

Fig. 8: Please include only one legend on the figure, which should be larger.