Replies to comments by reviewer 1

Comment: The authors present a brief study on the utility of solar occultation measurements under heavy stratospheric aerosol loading caused by continuous injection of 30 Tg/year sulfur (i.e., as part of geoengineering efforts). The authors employed a model to construct the theoretical atmosphere, from which they extracted aerosol loadings, which were then used to estimate the solar transmission through the atmosphere (i.e., simulated a solar occultation observation), followed by a retrieval of extinction profiles. The authors then evaluated the accuracy of these extinction profiles at select wavelengths and select latitudes. The concept of this study is very interesting, and I am excited to see the paper in its final form. This paper, when complete, will make a substantive contribution to AMT, and fits well within the scope of the journal. However, the paper is unfortunately incomplete in its current form. The evaluation is presented very quickly, often with only brief statements to figures, and lacks the presentation it deserves. Ultimately, this paper leaves the reader pondering many questions; questions that should have been answered in the paper. After reading the paper, I am left with the desire that the authors take more time to better present their work so I can better understand and appreciate it. Such changes should be relatively simple additions, so my recommendation is for publication after major revisions and another round of reviews.

Reply: We thank the reviewer for his/her constructive and helpful comments. We tried to answer every comment in an appropriate way.

Comment: I have 2 major criticisms of the current version of this paper. First, the authors must improve the clarity of their methodology. As written, I could not reproduce this work without making multiple key assumptions. Some of these will be highlighted in the next section. Second, the paper lacks a deeper discussion of what their results mean. In Results and Discus sion, there are a lot of results and little discussion. Most of what is given to us is along the lines of "we tested this wavelength at this latitude and got Fig. X"; it boils down to a presentation of a series of figures without meaningful discussion. I raise these concerns for 2 reasons. First, I see great value in this fundamental study. If the authors enhance their discussion then I believe this can be a great paper. Second, the authors undoubtedly put a lot of effort into this study as well as a great deal of thought. Don't deprive the reader of everything you learned through this exercise.

Reply: Thank you for pointing this out. In line with the comments of reviewer 3 we added the following to the results and discussion section:

l. 180 (after Fig. 2): "The lack of good agreement can be attributed to the high AOD (0.45 at 550 nm) near the latitude of the injection, resulting in high aerosol extinction and low transmission from the perspective of the satellite solar occultation instrument."
l. 190: "Consistent with expectations and the averaging kernels (panel (b) of Fig. 2) the minimum transmission values of $\approx 10^{-14}$ fall below the detection threshold where measurement noise dominates the signal, preventing the retrieval algorithm from extracting meaningful information about the vertical profile of aerosol extinction coefficients."

l. 198 (after Fig. 4): "The improved retrieval performance reflects the principle that aerosol extinction decreases with increasing wavelength, confirming the generally accepted idea that, in the case of very high emissions, the appropriate approach is to use longer wavelengths for aerosol measurements."
l. 207: "The transmission at the minimum still remains on the order of about 10^{-6} – 10^{-5}, while the transmission values at the other tangent heights stay within ranges that allow useful measurement information to be retrieved from these heights, leading to the improved retrieval performance."
l. 214: "The reduced aerosol loading at these latitudes allows shorter wavelengths to maintain sufficient transmission from the perspective of the solar occultation instrument for the aerosol extinction profile retrieval."
l. 244: "The results presented above reveal a relationship between latitude, AOD, vertical structure, and minimum retrieval wavelength. Thereby, three factors jointly control retrieval feasibility:
First, the AOD determines the overall attenuation of the signal. With high AOD, the extinction of the solar signal is so strong that no meaningful retrieval is possible at the corresponding altitudes. This is accompanied by correspondingly low transmissions from the satellite solar occultation instrument's perspective. The latter two points depend also on the wavelength and latitude.
Second, the vertical profile of the aerosol extinction coefficients, which modulates the retrieval sensitivity. For the latitudes near the Equator, aerosol extinction coefficients peak near 19 km (e.g, Fig. 4, for 5° N), creating a localized transmission minimum that can fall below detection limits at shorter wavelengths. At higher latitudes, aerosol extinction coefficients peak over a broader altitude range (e.g, Fig. 8 for 45° N and S), reducing peak extinction values and allowing information to be retrieved from a larger altitude range of the vertical profile.
Third, the steady-state nature of continuous injection differs fundamentally from volcanic eruptions, since continuous injections result in a much lower sulfate injection per time compared to a volcanic eruption with the same injected amount."
l. 276: "These findings, considering the assumptions made, can have direct implications for solar occultation instrument design. Instruments intended to detect and monitor SAI deployments above a certain size or major volcanic eruptions (or both in the hypothetical case of SAI deployments and a simultaneous volcanic eruption) should incorporate channels extending to at least 1900 nm to ensure coverage for the latitudes with high aerosol loading. The current SAGE III/ISS maximum aerosol wavelength of 1543 nm would be marginally insufficient for retrievals within $\approx \pm 15°$ of a 30 Tg S yr^{-1} continuous tropical injection, though adequate for mid-to-high latitudes and lower injection rates (as demonstrated by the 10 Tg S yr^{-1} results at 5° N)."

Furthermore, we added the following to the methodology subsection:

"In the following, monthly mean data from the quasi-steady-state phase, averaged over a three-year period, are analysed."

"For a latitude of 5° N and the visible spectral range (500 -– 550 nm $\rightarrow$ 520 nm), the calculated α were small and slightly negative throughout the altitude range (≈ -0.12 to -1.07), indicating nearly wavelength-independent extinction in this narrow spectral interval ($\Delta\lambda = 20$ nm). Consequently, the interpolation introduces a negligible change in extinction. In the near-infrared range, α increases to values between approximately 1.2 and 6 (higher values due to very low extinction coefficients ($\approx 10 - 14\,\mathrm{km}$)). These values confirm that the Ångström parameterisation provides a reasonable approximation for the spectral regions considered in this study.”

”The model reaches a quasi-steady state after approximately three years, and the spin-up time is ten years.”

”In the quasi-steady-state phase, the stratospheric sulphur burden stabilises at approximately 20.6 Tg S for the $30\,\mathrm{Tg\,S\,yr^{-1}}$ injection scenario, indicating that $\approx 10\,\mathrm{Tg\,S\,yr^{-1}}$ is continuously removed through sedimentation and deposition processes, balancing the injection rate.”

In response to the comments made by reviewer 3, we have made further additions to the manuscript. We kindly ask you to take this into account. A complete list of all changes would be too lengthy to be included here.

Comment: page 2, line 35: What is meant by “larger to large-scale”? Can this be quantified?

Reply: Thank you for the comment. We agree that the wording does not have a unambiguous meaning. There is no formal threshold definition, but the authors would define it based on the amount of sulphur injected, with large to large-scale injection defined as injection rates greater than 10 Tg S/y.
We have added this information to the manuscript as follows: ”Depending on the progression of climate change and increasing damage and costs, as well as future goals regarding the reduction of the global mean surface temperature of the Earth, large-scale SAI applications (here defined as injection rates $> 10\,\mathrm{Tg\,S\,yr^{-1}}$) might be considered.”.

Comment: page 2, lines 40-45: The discussion of SAGE II observations says the same thing 3 times. Please condense and reword for clarity.

Reply: We have reworded the section as follows: ‘The corresponding aerosol plume caused so much extinction of the solar signal that no retrievals were possible (e.g., Stenchikov et al., 1998). The SAGE II dataset shows gaps in aerosol measurements for the period June – August 1991 in the region 15° S to 20° N below 22 km (e.g., Antuña et al., 2003), and during the first year after the eruption, the instrument only provided measurements above $\approx$ 23 km altitude at wavelengths of 1020 nm and shorter (e.g., Arfeuille et al., 2013).’

Comment: page 3, line 67: “The prognostic modal aerosol microphysical Hamburg Aerosol Model...” This is hard to mentally digest. Can this be reworded for clarity.

Reply: We rephrased it as follows: ”The Hamburg Aerosol Model (HAM, Stier et al., 2005), a prognostic modal aerosol microphysics scheme, was interactively coupled to MAECHAM.”.

Comment: page 3, line 74: "...set to to climatological..." to "...set to climatological..."

Reply: Thank you, changed.

Comment: page 3, line 82: "wavelength" to "wavelengths"

Reply: Changed.

Comment: page 5, line 112: It is unclear what the authors mean by "in accordance with the objective of this study." As written, it sounds like the objective of the study was to not look at all wavelengths. If that is the case, then how can the authors make wavelength recommendations? Please clarify.

Reply: We agree that the sentence sounds confusing. We have rephrased it as follows: 'Although a larger wavelength range was analysed (compare Sect. 2.1), we focus in the following on selected wavelengths that best illustrate the key findings.'.

Comment: Section 3: I assume the authors allowed the model to achieve steady-state conditions. Is this correct? How many years did the model have to run before they started their retrievals?

Reply: Yes, the data is for the quasi-steady state phase. The model reaches a quasi-steady state after approximately three years, and the spin-up period is ten years. We added these facts to the MAECHAM5-HAM section.

Comment: Figure 1, panel B: While interesting, a line-of-sight (LOS) optical depth would be more informative to occultation observations. This would also show the reader why retrieval below the aerosol peak is problematic for certain wavelengths. Further, breaking this into a multi panel figure that has LOS OD for all wavelengths used in the current study (LOS OD as a function of altitude and latitude) would communicate how and why different wavelengths perform better at different altitudes and latitudes. This figure will strengthen the authors' arguments later in the paper so I ask the authors to please consider making this addition.

Reply: Thank you for this idea. We added this figure to the appendix and added more information about the corresponding LOS OD to the manuscript:

"The transmission at the minimum still remains on the order of about $10^{-6} - 10^{-5}$, while the transmission values at the other tangent heights stay within ranges that allow useful measurement information to be retrieved from these heights, leading to the improved retrieval performance. **This is also consistently shown in the corresponding line-of-sight (LOS) optical depth (panel (g) of Fig. A1**)."

"The pronounced increase in relative residuals at 12 km is primarily related to the very low transmission value at this tangent height (panel (a) of Fig. 7) **and the corresponding high LOS optical depth (panel (a) of Fig. A1)**."

We have made further additions in response to the following comments from the reviewer (see below).

Comment: page 8, line 161: "...is given below..." Figure positioning is relative, please provide a definite reference to the figure you want.

Reply: Thank you, changed.

Comment: Figure 3: What does this figure add? Why would you calculate transmission from your extinction coefficients? This seems unnecessary when the authors can use the extinction coefficients directly to calculate percent difference, which is more meaningful (if transmission is more meaningful, please explain how).

Reply: We believe that the illustrations of the corresponding transmissions are an essential part of the overall concept of the paper, as they show exactly how low or high the transmission is from the perspective of the satellite solar occultation instrument, depending on the tangent height, wavelength and latitude, thus supporting the conclusions. Furthermore, we consider the comparison of transmissions to be a logical step, as satellite solar occultation instruments do not measure aerosol extinction coefficients directly. We believe that the illustration of the aerosol extinction coefficient profiles and transmissions with the corresponding relative residuals are necessary for a complete understanding of the key points of the paper. Finally, we believe that presenting the corresponding transmission values when dealing with the 'zero transmission' problem makes sense.

Comment: Figure 4: Please correct the x-tick labels in panel (b) to match the other panels.

Reply: Corrected.

Comment: page 12, line 197: Why do the residuals blow up at 12 km and nowhere else? This is an example of the authors basically reading the figures to the reader without bringing additional insight to the discussion. Please convey to the reader why this happens and why it is significant (or not significant).

Reply: We added the following to the text:
"The pronounced increase in relative residuals at 12 km is primarily related to the very low transmission value at this tangent height (panel (a)). Since the residuals are expressed relative to the true transmission, small absolute differences between true and simulated transmission values translate into comparatively large percentage deviations when the transmission approaches its minimum. In addition, this altitude range corresponds to the lower flank of the aerosol extinction profile, where extinction values are still significant but decrease rapidly with decreasing altitude and deviations between retrieved and true aerosol extinction coefficients become more pronounced."

Comment: Figure 8: Did you try this with shorter wavelengths? What was the result?

Reply: Thank you for the comment. In order to determine a minimum wavelength for physically meaningful retrieval result at a specific latitude, we also performed calculations and retrievals at shorter wavelengths. For latitudes of 45° N and 45° S, the best agreement was at 1543 nm (as illustrated in Fig. 8).

Comment: Figure 11: Why is the residual so high? From Fig. 1 I expected this to be better than the southern hemisphere, but it is worse; why? Also, why the massive difference between Fig. 11 and Fig. A4 (roughly 2 orders of magnitude)? Please explain.

Reply: We thank the reviewer for pointing this out. The explanation lies in differences in the vertical structure of the aerosol extinction profiles rather than in the total aerosol burden. As shown in Fig. 10, the true aerosol extinction profile at 15° N (panel b, orange line) exhibits a sharper maximum around 19 — 20 km and a more pronounced change in curvature in the 20 — 21 km altitude range. In addition, the deviation between the retrieved and true aerosol extinction profiles is larger in the lower part compared to 15° S. In contrast, the profile at 15° S (panel a) is smoother and more monotonic throughout the altitude range and shows better agreement between retrieved and true aerosol extinction.
We added the following:
"The higher relative residual (TH = 20 km) at 15° N results from differences in the vertical structure of the aerosol extinction profiles. As evident in Fig. 10, the true profile at 15° N (panel b) exhibits a sharper maximum around 19 – 20 km and more pronounced curvature changes at 20 – 21 km compared to the smoother, more monotonic profile at 15° S (panel a)."

Comment: page 16, line 217: "...does not mean that solar occultation measurements are entirely useless." Please reword for clarity. As written, this is not correct. If there is zero transmission then there is zero signal, which makes the measurements useless.

Reply: Rephrased it here and throughout the manuscript: "However, the results demonstrate that encountering the zero transmission problem at a given wavelength does not render solar occultation measurements entirely impossible, rather it indicates that longer wavelengths must be used to obtain meaningful aerosol extinction profiles."

Comment: page 16: line 222: "...due to the low extinction coefficients at these altitudes..." Please reword for clarity. I doubt low extinction is the dominant problem. It is more likely the massive line-of-sight optical depth caused by the thick aerosol layer higher in the atmosphere, which obscures the signal at lower altitudes. I note that a line-of-sight optical depth plots (as suggested above) would easily communicate this.

Reply: Rephrased as follows: "Note that the large errors at low altitudes are due to the low aerosol extinction coefficients at these altitudes (compare, e.g., Fig. 10) and the corresponding low signal. In addition, the high LOS optical depths at low tangent heights (compare Fig. A1) also play a role, which likewise leads to a low signal at these altitudes."

Comment: page 16, lines 226-227: It is unclear what this is intended to communicate or why it is relevant to this study. Please clarify or remove.

Reply: Removed.

Comment: page 16, lines 229-230: "The present study examined which wavelengths, depending on the latitude, are necessary for the aerosol extinction profile retrievals in the case of continuous tropical injections of 30 Tg S yr-1." This is not correct. Most figures showed data from only 1 wavelength at 1 latitude without evaluating performance at other wavelengths/latitudes. What can we learn about other latitudes outside the 4 you present in the paper? What wavelengths can be used at, say, 30 N? Are there hard cutoffs, in latitude, where 520 nm suddenly fails to yield a viable product and we must then move to 1543 nm? What about 1020 nm, or 755 (the authors were focused on SAGE wavelengths)? Ultimately, I am left knowing very little more than I knew before reading the paper (i.e., longer wavelengths are required to see through denser aerosol layers). In short, we are forced to accept the authors' conclusions without seeing the evidence. Please reinforce your conclusions by "fleshing out" the analysis and giving us evidence for your conclusions.

Reply: Rephrased as follows: "The present study provides representative examples illustrating how the minimum wavelength required for a physically meaningful retrieval depends on latitude under a continuous tropical injection scenario of 30 Tg S yr^{-1}."

We added the following to the methodology section for clarification: "The results of the simulations with SCIATRAN are transmission values at 520, 1543 and 1900 nm for tangent heights of 10 to 60 km, in 2 km steps. Although a larger wavelength range was analysed (compare Sect. 2.1), we focus in the following on selected wavelengths that best illustrate the key findings."

As well as: "The selected latitudes (75° N/S, 45° N/S, 15° N/S, and 5° N) represent characteristic regions with different aerosol loading under the simulated injection scenario (compare Fig. 1). The transition between suitable wavelength ranges is likely gradual rather than characterised by sharp latitudinal cut-offs. The purpose of the present study is not to define strict thresholds but to demonstrate the general relationship between aerosol loading and wavelength requirements. In addition, since the study also deals with the 'zero transmission' problem, the latitudes were chosen accordingly."

Comment: page 17, lines 233-234: This is more of a side comment, but what the authors say here presents the opportunity for my comment. Yes, but it is also important to know if the atmosphere is in steady state (the authors deny us the benefit of these details!). If not, then the optical depth will continue to change, which may make the 1543 and 1900 nm channels insufficient for the task. Please provide better simulation details.

Reply: Thank you for the comment. As described in more detail in a reply above, the MAECHAM5-HAM simulations are for the quasi-steady state phase.

Comment: page 17, line 235: "...does not mean that solar occultation measurements are entirely worthless." I doubt anyone would conclude that, a priori. However, as stated above, if we are in a true zero transmission situation then there is not signal. Please clarify your intent.

Reply: Rephrased as follows: "The results also emphasize that encountering the zero transmission problem at shorter wavelengths does not render solar occultation measurements impossible, it requires appropriate wavelength selection based on aerosol loading. "

Comment: page 17, lines 236-237: Please be more precise in your language. You can state specifically what wavelengths are useful at each latitude and altitude range. Please provide this information.

Reply: Rephrased it as follows: "Depending on the latitude (see above), the already operating satellite solar occultation instrument SAGE III/ISS could probably also provide aerosol measurements in the relevant altitude range (10 – 27 km), assuming continuous emissions of 30 Tg S yr^{-1}. Specifically, the wavelength channel of 520 nm would be sufficient at high latitudes (75° N and 75° S), while its longest aerosol wavelength channel (1543 nm) would be necessary at mid-latitudes (45° N and 45° S). However, at low latitudes near the injection region (5° N, 15° N, and 15° S), wavelengths longer than SAGE III/ISS's maximum aerosol wavelength of 1543 nm (at least 1900 nm) would be required for physically meaningful aerosol extinction profile retrievals."

Comment: General statement: The authors conclude that a 1900 nm channel is required to get a viable retrieval through the most dense aerosol layers, but they fail to discuss why we wouldn't use solely longer wavelengths (i.e., $\geq$ 1900 nm) everywhere / all the time.

Reply: We added the following to the discussion:
"Although longer wavelengths prevent the zero transmission problem in regions of high aerosol loadings, they are not optimal under all conditions. Aerosol extinction decreases with increasing wavelength, reducing measurement sensitivity and information content in regions with moderate or low aerosol loading. At latitudes with lower aerosol loading, shorter wavelengths provide higher extinction signal and therefore improved SNRs and retrieval sensitivity. Consequently, an ideal satellite solar occultation instrument for detecting and monitoring large-scale stratospheric aerosol injections should include multiple wavelength channels spanning from the visible ($\approx$ 500 nm) to the near-infrared ($\geq$ 1900 nm), allowing for optimal wavelength selection based on aerosol loading and latitude."

In addition, particle size information would be difficult to retrieve with measurements at only 1900 nm. Furthermore, for radiative forcing estimates, the AOD in the visible spectral range has to be known and an extrapolation from 1900 nm would be highly problematic.

Comment: General statement: The authors refer to the 1991 Pinatubo eruption (20 Tg S) as a reference point for their assumed 30 Tg/year injection rate. A continuous injection is far different from an acute injection. I would assume that the continual injection would continue to build a reservoir of stratospheric sulfur that exceeds what we would get from a single, rapid, injection of 30 Tg. Can the authors comment on how a continual injection of 30 Tg corresponds to a single eruption (e.g., once steady state is achieved, what size of a volcanic eruption would be required to achieve a comparable stratospheric aerosol load)? This may be more than the authors can offer, so if they cannot provide this information that is fine.

Reply: Thank you for the comment. We added the following to the introduction:

"Continuous emissions lead to much lower injection amounts per time compared to a volcanic eruption with the same amount injected. The relationship between injected sulphur dioxide mass and the resulting stratospheric aerosol burden is non-linear due to aerosol microphysical processes (e.g., Niemeier and Timmreck, 2015)."

Comment: General statement (last one!): One aspect the authors neglected is how coincident volcanic eruptions may amplify the zero transmission issue. For example, how would a relatively modest (VEI 2/3) eruption impact wavelength selection as a function of altitude? If Raikoke, Calbuco, or Puyehue-Cordon Caulle erupted again, would the 520 nm channel still remain robust at higher latitudes? Would a larger tropical eruption provide too much load for 1900 nm to remain useful in the tropics? I see this as exciting future work and look forward to the authors' future publications.

Reply: Thank you for the comment. That is indeed something we want to look at in the near future. We added the following to the discussion:

"This study has focused on continuous stratospheric aerosol injections under quasi-steady-state conditions without considering natural volcanic perturbations. An important aspect is how coincident volcanic eruptions may amplify or modify the zero transmission problem and associated wavelength requirements. A relatively modest volcanic eruption, such as the 2019 Raikoke eruption, could temporarily increase stratospheric aerosol loading at mid-to-high latitudes. This additional aerosol burden would be superimposed on the background SAI aerosol layer, potentially requiring longer wavelengths than those identified here under pure SAI conditions. The temporal evolution of the combined aerosol load—from the initial volcanic injection through the decay phase—would likely require flexible or adaptive wavelength selection strategies. Even more challenging would be a large tropical volcanic eruption occurring simultaneously with large-scale SAI deployments, which could substantially increase aerosol optical depth in the tropics and potentially reduce transmission even at near-infrared wavelengths such as 1900 nm, particularly at lower tangent heights. A quantitative investigation of combined SAI–volcanic scenarios is beyond the scope of this study and represents an important direction for future work."

Replies to comments by reviewer 3

Comment: This manuscript presents a timely and fundamentally important study that connects the currently very active topic of stratospheric aerosol injection (SAI) to a concrete and practical problem in the satellite observation community: how heavy stratospheric aerosol loading affects solar occultation measurements and the retrievability of aerosol extinction profiles.
I find the overall research idea excellent. The study successfully links geoengineering scenarios to real observational limitations and demonstrates how SAI-induced aerosol loading can directly influence satellite measurements, retrieval sensitivity, and wavelength selection. In this sense, the manuscript represents a strong and valuable fundamental study that fits very well within the scope of Atmospheric Measurement Techniques. I also appreciate the use of established models (MAECHAM5-HAM and SCIATRAN) and the clear focus on the "zero transmission problem", which is highly relevant not only for hypothetical SAI deployments but also for major volcanic eruptions.
I largely agree with Referee 1 that, while the core idea and modelling framework are strong, the current version of the paper would benefit substantially from deeper analysis and more extensive discussion. Strengthening these aspects would significantly enhance the paper's impact, particularly for readers seeking a clearer and more quantitive understanding of the sensitivity and magnitude of SAI impacts on solar occultation measurements. Below I outline my two main suggestions, which I hope will help to improve the manuscript.

Reply: We thank the reviewer for his/her constructive and helpful comments. We tried to answer every comment in an appropriate way.

Comment: Major Comment:
1. The main analysis focuses on a continuous SO_2 injection rate of $30\,\mathrm{Tg\,S\,yr^{-1}}$, with only a brief mention of results for $10\,\mathrm{Tg\,S\,yr^{-1}}$. While I understand the motivation to examine an extreme, upper-end SAI scenario, the applicability of the conclusions would be strengthened by either expanding the discussion of intermediate or lower emission rates, or by more clearly justifying why 10 and $30\,\mathrm{Tg\,S\,yr^{-1}}$ were selected as representative cases.

Reply: Thank you for the comment. We chose $30\,\mathrm{Tg\,S\,yr^{-1}}$, as a deliberately high, upper-end SAI scenario to probe the zero-transmission problem under conditions where radiative forcing effects are most pronounced. This allows us to assess whether and how zero transmission would emerge in a hypothetical large-scale deployment. While ambitious, this injection rate has been discussed in SAI modelling studies [e.g, 1, 2]. We acknowledge that $30\,\mathrm{Tg\,S\,yr^{-1}}$ is at the high end of proposed scenarios, however, the upper limit in the context of possible SAI applications depends, for instance, on the the specific goal, such as specific radiative forcing effects.

The $10\,\mathrm{Tg\,S\,yr^{-1}}$ case was selected as a Pinatubo-like reference (the 1991 eruption injected approximately 20 Tg SO_2), while acknowledging that volcanic eruptions represent impulsive rather than continuous injections such as the SAI injections performed here. This lower rate allows us to examine whether and to what extent the zero-transmission problem occurs at more moderate injection levels and to determine the minimum wavelength required for the latitude range of the injection.

The results showed that a wavelength of at least 1543 nm would be necessary in the latitude range of the injection, which is not the case for the 30 Tg S yr^{-1} scenario. We believe that this is a valuable addition and, in line with the focus of the study, we decided to mention the latitude range of the injection because this is where aerosol loading is highest (for the month analysed here) and the zero-transmission effect is most pronounced, making it the region of greatest concern for this problem.

[1] Laakso, A., Niemeier, U., Visioni, D., Tilmes, S., and Kokkola, H.: Dependency of the impacts of geoengineering on the stratospheric sulfur injection strategy – Part 1: Intercomparison of modal and sectional aerosol modules, Atmos. Chem. Phys., 22, 93–118, https://doi.org/10.5194/acp-22-93-2022, 2022.

[2] Niemeier, U. and Timmreck, C.: What is the limit of climate engineering by stratospheric injection of SO2?, Atmos. Chem. Phys., 15, 9129–9141, https://doi.org/10.5194/acp-15-9129-2015, 2015.

We added the following to the introduction:
"This injection rate is analysed as a deliberately high, upper-end SAI scenario to probe the zero-transmission problem under conditions where radiative forcing effects are most pronounced. Although it appears to be a comparatively high emission rate, the upper limit in the context of possible SAI applications depends, for instance, on the specific goal, such as specific radiative forcing effects."

We added the following to the discussion:
"The injection rate of 30 Tg S yr^{-1} was selected as a deliberately high, upper-end SAI scenario to probe the zero-transmission problem under conditions where radiative forcing effects are most pronounced. This enables an assessment of whether and how zero transmission may emerge in a hypothetical large-scale deployment. While ambitious, injection rates of this magnitude have been discussed in previous SAI modelling studies (e.g., Niemeier and Timmreck, 2015; Laakso et al., 2022). The injection rate lies at the upper end of proposed scenarios, however, potential upper limits in SAI applications depend on the specific objectives, for instance the targeted radiative forcing."

As well as:
"The 10 Tg S yr^{-1} injection was selected as a Pinatubo-like reference (the 1991 eruption injected approximately 20 Tg SO_2), while emphasising that volcanic eruptions represent impulsive rather than continuous injections. This lower rate allows examining whether and to what extent the zero-transmission problem occurs at more moderate injection rates and to determine the minimum wavelength required for the latitude range of the injection, since this latitude range is where aerosol loading is highest (for the month analysed here) and the possible zero-transmission effect is most pronounced, making it the region of greatest concern for this problem."

Comment: In addition, I encourage the authors to clarify whether the relationship between required wavelength and emission strength is approx. linear, or whether it exhibits threshold behaviour (i.e. abrupt transitions where a given wavelength rapidly becomes unusable). Even a qualitative or semi-quantitive sensitivity discussion would greatly enhance the broader relevance of the results.

Reply: Thank you for pointing this out. Based on the injection scenarios of 10 and 30 $\mathrm{Tg\,S\,yr^{-1}}$, we can qualitatively evaluate the scaling behaviour. The results show that longer wavelengths are required at higher injection rates due to higher aerosol loading, resulting in higher aerosol optical depth and lower transmission from the perspective of the satellite solar occultation instrument (depending on the wavelength). Therefore, it is a monotonic relationship, where higher injection rates require longer wavelengths. Based on the data we assume a sub-linear relationship between required wavelength and the injection rate (factor 3 increase in injection rate yields $\approx 23\,\%$ increase in threshold wavelength for 5° N).

We added the following to the discussion:
"The relationship between injection rate and wavelength threshold appears monotonic: Higher injection rates increase aerosol loading and aerosol optical depth, requiring longer wavelengths to maintain measurable transmission for satellite solar occultation measurements. Comparing the two scenarios, a threefold increase in injection rate (10 to 30 $\mathrm{Tg\,S\,yr^{-1}}$) corresponds to approximately 23 % increase in the minimum wavelength threshold at 5° N (from 1543 to 1900 nm), suggesting sub-linear scaling. While a complete characterization would require additional intermediate injection scenarios, the results suggest that the zero-transmission problem intensifies with increasing injection rate."

Comment: 2. I concur with Referee 1 that the manuscript currently lacks sufficient depth in its analysis and discussion. The Results and Discussion section reads as a rapid presentation of figures, with limited interpretation of the underlying physical mechanisms or broader implications.
I encourage the authors to expand the discussion to address questions such as: Why do certain wavelengths fail or succeed at specific latitudes and altitudes? How do aerosol vertical structure and steady-state aerosol loading jointly control retrieval sensitivity? What do these results imply for instrument design, wavelength selection strategies, and the interpretation of real occultation data under extreme aerosol conditions?
Addressing these points would substantially strengthen the manuscript and allow readers to fully appreciate the insights gained from this study.

Reply: We followed the reviewer's suggestion and added additional statements and discussions to various parts of the manuscript:
l. 180 (after Fig. 2): "The lack of good agreement can be attributed to the high AOD (0.45 at 550 nm) near the latitude of the injection, resulting in high aerosol extinction and low transmission from the perspective of the satellite solar occultation instrument."
l. 190: "Consistent with expectations and the averaging kernels (panel (b) of Fig. 2) the minimum transmission values of $\approx 10^{-14}$ fall below the detection threshold where measurement noise dominates the signal, preventing the retrieval algorithm from extracting meaningful information about the vertical profile of aerosol extinction coefficients."
l. 198 (after Fig. 4): "The improved retrieval performance reflects the principle that aerosol extinction decreases with increasing wavelength, confirming the generally accepted idea that, in the case of very high emissions, the appropriate approach is to use longer wavelengths for aerosol measurements."

l. 207: "The transmission at the minimum still remains on the order of about 10^{-6} – 10^{-5}, while the transmission values at the other tangent heights stay within ranges that allow useful measurement information to be retrieved from these heights, leading to the improved retrieval performance."
l. 214: "The reduced aerosol loading at these latitudes allows shorter wavelengths to maintain sufficient transmission from the perspective of the solar occultation instrument for the aerosol extinction profile retrieval."
l. 244: "The results presented above reveal a relationship between latitude, AOD, vertical structure, and minimum retrieval wavelength. Thereby, three factors jointly control retrieval feasibility:
First, the AOD determines the overall attenuation of the signal. With high AOD, the extinction of the solar signal is so strong that no meaningful retrieval is possible at the corresponding altitudes. This is accompanied by correspondingly low transmissions from the satellite solar occultation instrument's perspective. The latter two points depend also on the wavelength and latitude.
Second, the vertical profile of the aerosol extinction coefficients, which modulates the retrieval sensitivity. For the latitudes near the Equator, aerosol extinction coefficients peak near 19 km (e.g, Fig. 4, for 5° N), creating a localized transmission minimum that can fall below detection limits at shorter wavelengths. At higher latitudes, aerosol extinction coefficients peak over a broader altitude range (e.g, Fig. 8 for 45° N and S), reducing peak extinction values and allowing information to be retrieved from a larger altitude range of the vertical profile.
Third, the steady-state nature of continuous injection differs fundamentally from volcanic eruptions, since continuous injections result in a much lower sulfate injection per time compared to a volcanic eruption with the same injected amount."
l. 276: "These findings, considering the assumptions made, can have direct implications for solar occultation instrument design. Instruments intended to detect and monitor SAI deployments above a certain size or major volcanic eruptions (or both in the hypothetical case of SAI deployments and a simultaneous volcanic eruption) should incorporate channels extending to at least 1900 nm to ensure coverage for the latitudes with high aerosol loading. The current SAGE III/ISS maximum aerosol wavelength of 1543 nm would be marginally insufficient for retrievals within $\approx \pm 15°$ of a 30 Tg S yr^{-1} continuous tropical injection, though adequate for mid-to-high latitudes and lower injection rates (as demonstrated by the 10 Tg S yr^{-1} results at 5° N)."
In response to the comments made by Reviewer 1, we have made further additions to the manuscript. We kindly ask you to take this into account. A complete list of all changes would be too lengthy to be included here.

Comment: Minor comments
1. While aerosol deposition and sedimentation are included in MAECHAM5-HAM, this is only mentioned briefly. A short statement clarifying how aerosol removal balances continuous injection in the quasi-steady-state regime would improve transparency.

Reply: We added the following to the MAECHAM5-HAM section: "In the quasi-steady-state phase, the stratospheric sulphur burden stabilises at approximately 20.6 Tg S for the 30 Tg S yr^{-1} injection scenario, indicating that $\approx$ 10 Tg S yr^{-1} is continuously removed through sedimentation and deposition processes, balancing the injection rate."

Comment: 2. The use of Ångström extrapolation to derive extinction at additional wavelengths is reasonable, but a brief comment on its validity under very high aerosol loadings (and large particle sizes) would be helpful.

Reply: We thank the reviewer for this important comment.
For the visible spectral range (500–550 nm → 520 nm) the calculated Ångström exponent α is very small and slightly negative throughout the 10–27 km altitude range (≈ -0.12 to -1.07). These values indicate that the aerosol extinction is nearly flat in this narrow spectral range ($\Delta\lambda = 20$ nm). Consequently, the interpolation to 520 nm results in a change in extinction that is negligible compared to the original 500 nm. In other words, the Ångström method introduces no significant error in this case. For longer wavelengths in the infrared, the calculated Ångström exponents are significantly larger, e.g.: 1050–1585 nm → 1543 nm: $\alpha \approx$ 1.2–1.97, 1585–1888 nm → 1800 nm: $\alpha \approx$ 2.1–5.4 (higher values due to very low extinction coefficients ($\approx 10 - 14\,\text{km}$)), 1888–2250nm → 1900 nm: $\alpha \approx$ 3–6 (higher values due to very low extinction coefficients ($\approx 10 - 14\,\text{km}$)). Therefore, even under conditions of elevated aerosol loading, the method provides a reasonable approximation for the wavelengths considered here. The explanations refer to a latitude of 5°N.
In summary, the Ångström method is valid for the current study.

We added the following to Sect. 2.2.1:
"For a latitude of 5° N and the visible spectral range (500 -– 550 nm → 520 nm), the calculated α were small and slightly negative throughout the altitude range (≈ -0.12 to -1.07), indicating nearly wavelength-independent extinction in this narrow spectral interval ($\Delta\lambda = 20\,\text{nm}$). Consequently, the interpolation introduces a negligible change in extinction. In the near-infrared range, α increases to values between approximately 1.2 and 6 (higher values due to very low extinction coefficients ($\approx 10 - 14\,\text{km}$)). These values confirm that the Ångström parameterisation provides a reasonable approximation for the spectral regions considered in this study."

Comment: 3. Although SAGE III/ISS provides an important reference, a brief clarification of how instrument-specific assumptions (e.g. field of view, tangent height spacing) influence the results would improve transferability to other occultation sensors.

Reply: Thank you for the comment. In our study, we did not assume a specific instrument configuration but adopted values representative of a typical satellite solar occultation system, such as SAGE III/ISS. Pointing uncertainty was explicitly included via a ± 100 m tangent height perturbation, as this represents one of the dominant error sources. Within the range of typical occultation sensor configurations, variations in FOV and tangent spacing primarily affect vertical smoothing characteristics rather than the magnitude of aerosol extinction uncertainty. The resulting error characteristics are therefore transferable to other occultation sensors with comparable vertical resolution and pointing performance.

We added the following to Sect. 2.2.1:

”Vertical field of view, and sampling assumptions represent a typical satellite solar occultation instrument and the resulting error characteristics are likely transferable to other occultation sensors with comparable vertical resolution and pointing performance, provided that a similar retrieval approach is applied.”