

Replies to comments by reviewer 1 - Filip Vanhellemont

Comment: Review: Detectability of solid particle injections into the stratosphere with satellite solar occultation instruments

By Anna Lange et al.

In this paper, the authors investigate the possibility to detect the presence of artificially injected solid particles (5 Tg per year) into the stratosphere by using the standard satellite solar occultation measurement technique. The injection of these particles is at present debated in the (still controversial) framework of geoengineering. Usually, SO₂-induced sulphate aerosols are considered; here, solid alumina and calcite particles are discussed, due to their increased scattering efficiency and reduced infrared absorption. Using the SOCOL-AERv2 circulation/chemistry/microphysics model and the SCIATRAN Radiative Transfer model, synthetic retrievals of 565 nm aerosol extinction were obtained, taking into account a number of error sources. The results clearly show that for injections of 5 Tg per year, the presence of these particles is clearly observed in solar occultation retrievals, and can be distinguished from the mean background stratospheric aerosol loading, even when natural variability is considered.

General comments

This paper forms a substantial contribution to the scientific research related to the mitigation of global warming by geoengineering, since it clearly shows that the effects of stratospheric injections (here, in particular, alumina and calcite) can be measured by standard occultation techniques. Since geoengineering is quite a hot topic, I consider ACP to be an appropriate journal. Overall, the assumptions, methods and conclusions are valid, although one aspect of the retrieval mathematics should be clarified (see below). I've suggested some additional references, but in general, ample credit is given to other authors in the field. The structure of the paper is well organized. Use of English is also good, but I did include a list of suggestions to improve the clarity of the text. Furthermore, mathematical notation should be improved. If the comments below are properly addressed, I see no objection to the publication of the paper. I therefore advise to publish with minor revisions.

Reply: Dear Filip Vanhellemont, we thank you for your constructive and helpful comments. We hope we answered them in an appropriate way.

Comment: Specific comments

Is there a specific reason why the paper deals with solar occultation instruments only? It is fair to assume that similar conclusions can be drawn for ground-based observations, or limb scatter and nadir instruments. If there is a reason, a short explanation would be good.

Reply: We thank the reviewer for pointing this out. Solar occultation is based on the attenuation of the direct solar beam and therefore do not require additional assumptions on, e.g., the aerosol phase function, multiple-scattering contributions, or surface albedo, which are needed for limb-scatter and nadir geometries, respectively. This keeps the retrieval and error analysis comparatively well-constrained. Furthermore, this work connects directly to the retrieval framework established in our previous study (Lange et al., 2025), which was also developed for solar occultation geometry. The adoption of the same instrument concept ensures methodological consistency and comparability be-

tween the two studies.

We added the following to the introduction:

”This study focuses on satellite solar occultation instruments because they measure the attenuation of the direct solar beam and the associated retrievals do not require additional assumptions on, e.g., the aerosol phase function, multiple-scattering contribution, or surface albedo, which are needed for the interpretation of limb-scatter measurements. This keeps the retrieval error analysis comparatively well constrained. Furthermore, this work connects directly to the retrieval framework established in the previous study (Lange et al., 2025) and the adaption of the same instrument concept ensures methodological consistency and comparability between the two studies.”

Lange, A., Niemeier, U., Rozanov, A., and von Savigny, C.: Investigating the ability of satellite occultation instruments to monitor possible geoengineering experiments, *Atmos. Chem. Phys.*, 25, 11673–11688, <https://doi.org/10.5194/acp-25-11673-2025>, 2025.

Comment: Line 14: please also add the more recent paper by Kremser et al, Stratospheric aerosols - Observations, processes, and impact on climate, *Reviews of Geophysics* 54, 278-335.

Reply: Added.

Comment: Line 26: please cite the ALTIUS paper: Fussen et al., The ALTIUS atmospheric limb sounder, *JQSRT* vol 238, 2019.

Reply: Added.

Comment: Line 38: Please explain from where the number of 5 Tg per year originates. If it comes from Vattioni et al (2025), it would be a good idea to briefly repeat the reason for this number here. The entire conclusion of the paper (detectability or not) depends on it.

Reply: The injection rate of 5 Tg per year was chosen because, as shown in Vattioni et al. (2025), it results in an all-sky radiative forcing of about -1 W m^{-2} , an appropriate order of magnitude for an SAI scenario aiming to offset some of the effects of anthropogenic climate change. We have added the following to the manuscript:

”The injection rate of 5 Tg yr^{-1} was chosen because it results in an all-sky radiative forcing of about -1 W m^{-2} (Vattioni et al., 2025), a reasonable magnitude for an SAI scenario aiming to offset some effects of anthropogenic climate change.”

Comment: Line 78: Briefly explain here why the tropical latitudes from 30°N to 30°S are chosen as injection site. It may seem obvious to the authors, but it would improve the text for readers that are non-specialists.

Reply: Thank you for pointing this out. We added the following to the manuscript:

”These tropical and subtropical latitudes were chosen because material injected into this region is efficiently distributed globally by the Brewer–Dobson circulation, leading to a more even spread across both hemispheres.”

Comment: Line 85: What is the spin-up period? Is it the period necessary to reach steady-state?

Reply: Thank you for the question. We rephrased it at follows:

”The extinction coefficients represent yearly means averaged over a 15-year period in the quasi steady-state phase, following a 5-year spin-up period required for the simulation to transition from the initial reference conditions to quasi steady-state, during which the stratospheric aerosol burden builds up under continuous injection.”

Comment: Section 2.2.2 (Retrieval): it is not clear from the text if random simulated noise was added to the transmittance, before converting to the logarithm (measurement vector y) for every individual retrieval. Please specify.

Reply: We rephrased it at follows:

”...with y as measurement vector, containing the logarithms of the transmission values at 565 nm for all tangent heights (no random noise realisations were added to y), ...

Comment: Again, Section 2.2.2: the measurement vector y contains the logarithm of the transmittance. Therefore, the Beer-Lambert law says that y is linearly related to the aerosol extinction coefficients: $y = Kx$, with K the matrix containing the path segment lengths in the atmosphere. In Eqn. 1, the terms $F(x_a)$ and $-K^*x_a$ disappear. The merit function (Eqn. 2) is then simply $\|Kx - y\|^2 + \|x - x_a\|^2$, which is the standard expression for optimal estimation. Can you clarify why the equations are unnecessarily complicated? (perhaps I’m missing something).

Reply: Thank you for the comment. We agree that, for the retrieval of extinction coefficients from the logarithm of the transmission, the Beer–Lambert law results in a linear expression. We presented the equations in the general (linearised, iterative) optimal estimation form because this is the formulation implemented in the SCIATRAN retrieval algorithm (Rozanov et al., 2011), where the inverse problem is formulated in logarithmic form and solved with a Gauss-Newton iterative approach.

Comment: Eqn. 5: is it valid to add the variances due to T and P (constant air density) to the variances due to T only and again P only? Isn’t this overestimating the error?

Reply: The ”temperature and pressure (constant air density)” perturbation varies T and p jointly such that the air density is held fixed, thereby isolating only the effect of the temperature dependence of the ozone absorption cross section. In contrast, the individual ”temperature” and ”pressure” perturbations are applied, so that they isolate only the effect of the resulting change in air density.

We clarified this in Table 3 as follows: ”Temperature and pressure (constant air density, effect on

ozone absorption cross section)". Furthermore, we added: "Note that temperature and pressure are treated as statistically independent error sources, whereas in reality temperature and pressure are physically coupled. This may lead to a slight overestimation of the total error."

Comment: Figs. 2 and 5: I think the figure would be clearer to interpret if you would plot the altitude of the tropopause on every plot as a horizontal line. It really is the stratosphere that is of interest in this paper.

Reply: Thank you, added. The tropopause heights shown now in Figs. 2 and 5 are taken from the SOCOL-AERv2 reference simulation. For the SAOD calculations (Figs. 3, 6) and the natural variability analysis (Sect. 3.3), SAGE II tropopause heights (2000–2004) were used, to ensure consistency with the SAGE II-based natural variability estimate.

Comment: Figs. 3 and 8: how was the stratospheric optical depth obtained? Integration from the tropopause up to the maximum available altitude in the data? Please specify.

Reply: Yes, correct. The stratospheric optical depth is obtained by integrating the extinction coefficient from the tropopause up to the maximum available altitude in the respective dataset. The latitude-dependent tropopause heights are taken from SAGE II data (2000 – 2004), consistent with the natural variability analysis. For the SOCOL-AERv2 data, the upper integration limit is 27 km, as described in the Methodology section. For the SAGE II data, the same upper limit of 27 km is applied, to ensure consistency with the SOCOL-AERv2-based results. We have clarified this in the manuscript as follows:

"...The stratospheric optical depth is obtained by integrating the extinction coefficient from the tropopause up to 27 km, consistent with the upper limit of the SOCOL-AERv2 data (see Sect. 2.1)."

Comment: Line 202: It is stated that at 75°S, detectable SAI signals are present above about 20 km. In Fig. 5, I observe detection (background outside the retrieval error) from 10 km to 24 km, which does not correspond with the text. Please correct.

Reply: Corrected, thank you. This referred to the comparison with the alumina injection, where detection above 20 km was not possible. But we have now adjusted this (change is printed in bold black):

"Compared with the extinction coefficient profiles based on the injection of alumina (compare Fig. 2), the extinction coefficients for calcite are slightly higher, but still of the same order of magnitude. Consequently, the corresponding profiles deviate more distinctly from the background profiles. In particular, at 75° S (panel (b)), this leads to detectable signals above ≈ 20 km (**here up to 24 km**), where the background profile lies outside the error range of the retrieved profile."

Comment: Technical comments, corrections and suggestions

It would be good to specify in the abstract that conclusions were obtained for stratospheric aerosol extinction at the specific wavelength of 565 nm. For example, Line 7: "Retrievals of 565 nm aerosol

extinction demonstrate that, under the assumptions made, ...”

Reply: Thank you for this suggestion. We have revised the corresponding sentence in the abstract accordingly: ”Vertically resolved retrievals of the stratospheric aerosol extinction coefficient at 565 nm demonstrate that...”.

Comment: Line 26: change ‘probably’ – > ‘potentially’, and ‘...above a certain size ...’ – > ‘...above a certain particle size ...’

Reply: Thank you for your comment. We have amended the first part. In the second part, ‘size’ refers to the injection rate. To make this clearer, we have reworded it as follows: ”...can potentially be used to detect possible future SAI deployments above a certain injection rate.”

Comment: Line 36: change ‘...start-ups may see an option to earn money with SAI (e. g. “Make sunsets” company)’ – > ‘...start-ups may find SAI to be profitable (e. g. the “Make sunsets” company)’.

Reply: Changed, thank you.

Comment: The entire paragraph from line 76 to 81 contains some redundancy (alumina or calcite, radius of 240 nm are mentioned several times). Could you remove this redundancy?

Reply: Rephrased as follows: ”Following Vattioni et al. (2025), simulations were conducted with a continuous injection of 5 Tg yr^{-1} of mono-disperse alumina or calcite particles with a radius of 240 nm, close to the optimal radius for backscattering solar radiation (Dykema et al., 2016), injected at 50 hPa ($\approx 20 \text{ km}$) and distributed homogeneously across all model grid boxes ($\approx 325 \text{ km} \times 325 \text{ km} \times 1.5 \text{ km}$) between 30°N and 30°S . These tropical and subtropical latitudes were chosen because material injected into this region is efficiently distributed globally by the Brewer–Dobson circulation, leading to a more even spread across both hemispheres. The particles were assumed to be fractal aggregates composed of 240 nm spherical monomers, further details on the model simulations are given in Vattioni et al. (2025).”.

Comment: It is important to make a distinction in mathematical notation between scalars and vectors/matrices/multidimensional objects in general. One possibility is to write multidimensional objects in boldface, and scalars in plain roman. Eqn. 1 then is: $\mathbf{y} = \mathbf{F}(x_a) + \mathbf{K}(\mathbf{x}-x_a)$. Please do this everywhere in the text.

Reply: In accordance with the ACP guidelines, we have decided to make the following changes:

- Matrices boldface
- Vectors boldface italic

Comment: Line 109: change text to: “... the logarithms of the transmission values at 565 nm for

all tangent heights, ...”

Reply: Changed, thank you.

Comment: Line 122: change text to “... and $T(\text{TH})$ as the transmission value ...”, in order for the notation to be conform with Eqn. 4.

Reply: Changed.

Comment: Line 128: change text to “... the error estimation on the extinction was ...”

Reply: Changed to “..the error estimation of the aerosol extinction coefficient was performed as follows:...”.

Comment: Eqn. 6: This is a scalar equation; it can be written in a better way as $r_i = (x_i - x_{\text{ref},i})/x_{\text{ref},i}$, with i the index indicating the tangent heigh index.

Reply: We have rewritten Eq. (6) as suggested. With i denoting the altitude index of the retrieved extinction coefficient profile, and x_i , $x_{\text{ref},i}$ the retrieved extinction coefficients at altitude i for the modified and reference settings.

Comment: Table 4, second row, first column: change to “A priori”.

Reply: Changed.

Comment: Table 4, 4th row, second column: change to “Extinction coefficient profile with SAI (alumina or calcite), retrieved with SCIATRAN.”

Reply: Changed, thank you.

Comment: Fig. 3, Fig.6, caption: change to “... optical depth (565 nm) as function of latitude ...”. Similar for Fig. 8.

Reply: Changed in the captions and in the corresponding text.

Comment: Line 201: change to “... the corresponding profiles deviate more distinctly ...”

Reply: Changed.

Comment: Line 205: change “pannel” to “panel” (twice).

Reply: Corrected, thank you.

Comment: Fig. 5, caption: change “apriori” to “a priori”

Reply: Changed in all relevant captions.

Comment: Line 247: change to “For both injection scenarios (compare Figs. 3, 6), this is the case.”

Reply: Changed, thank you.