Reply to reviewers of the manuscript "The enhanced capabilities of mid-infrared limb emission sounding to observe stratospheric aerosol injection geoengineering interventions", Sellitto et al.

Dear Editor, dear anonymous Reviewers #1-2,

Many thanks for your very kind words, your constructive criticism and the useful comments. Based on your comments, we have produced a revised version of our manuscript, with a thoroughly revised text in several parts and added new information.

Please find more details and a point-by-point reply to the Reviewers' specific comments in the following (Reviewers' comments are in black and our replies in blue). We refer to specific text sections pointing at specific lines intervals (Lxxx-yyy, with xxx and yyy the starting and ending lines) of the revised manuscript.

We think that, thanks to your comments and suggestions, the present version of the manuscript is further improved with respect to the previous version.

Thank you very much,

Pasquale Sellitto on behalf of all co-authors

Reviewer #1

The authors analyse the capabilities of a potential future high-spectral-resolution mid-infrared limb emission sounding instrument for detecting and monitoring potential SAI deployments. Overall, I find this an interesting study, however, in its current form, the manuscript requires revisions before it can be considered for publication. I recommend another round of reviews. Below are some general and specific comments intended to improve the manuscript.
Thank you for your kind words and your comments and suggestions for improvement. Please find in the following a point-by-point reply to your general and specific comments (identified by "GC" and "SC", respectively, followed by a progressive number).

General comments:
GC1) Throughout the manuscript, the authors repeatedly refer to 'weak' or 'very weak' SAI scenarios without specifying the corresponding injection rates or aerosol loadings. I strongly recommend that all SAI scenarios be clearly defined in terms of injection rate. The manuscript gains greater depth and clearer statements when it refers to more quantitative measures rather than qualitative and ambiguous expressions.
The magnitude of the SAI, in terms of the SO2 injection rates during the time period addressed by this study, is already mentioned in the discussion within the Results sections, i.e. the following sentences, slightly modified in the revised manuscript version (L273-280): "It is important to stress that what is shown in Figs. 1a-c and Figs. 4-5 are very weak initial SAI interventions, in the simulated SAI protocol of Geo SSP5-34-OS 2.0 PR scenario. The total SO2 mass values intercepted by this specific CAIRT orbit segment, in the area impacted by *(note the modification)* the two injection points, is 0.5 t (injection at 15°S, Fig. 5a) and 3.4 t (injection at 15°N, Fig. 5b), respectively. More in general, the total SO2 injected mass in the PR scenario, cumulated until February 2034 (SO2 PR of Fig. 1c) does not exceed 200 tonnes. As mentioned in the Introduction, these injections can be seen as analogues of "near-term" to "mid-term" experiments, i.e. scenarios considered

feasible even with presently existing technology and relatively low cost. As shown in Figs. 4-5, CAIRT would be capable of detecting such deliberate releases, even those as small as those described in this work. *(note the modification*" The "very weak" SO2 injections targeted by this study are also defined in the Conclusions (L405-408): "Our results suggest that CAIRT would be able to detect SO2 injections and quantify their mass along the observational orbits segments, and to locate their horizontal and vertical injection position, even for these very weak initial injections in our PR scenario. These injections are of the order of magnitude of a few to a few hundreds of *(note the modification)* tonnes of SO2." This quantitative statement is also mentioned in the Abstract. These amounts are also put in the context of what is defined a "near-term" or "mid-term" SAI experiment in the Introduction: "First, unilateral tests of SAI might be realised with extremely small amounts of SO2 injections, producing very small SA burdens. These are what can be defined as "near-term" or "mid-term SAI experiments", i.e. stratospheric injections of only a few kilograms to a few hundreds of *(note the modification)* tonnes of SO2, for example using stratospheric balloons. These experiments are considered feasible even with presently existing technology and relatively low cost (between 10 and 100 M$, following studies under the EU-funded Co-Create project, https://co-create-project.eu/)." By the way, to be clearer, we repeated and expanded this information in Sect. 3.1, when describing the PR SAI scenario (L188-L195): "This is associated with a slow start of the SO2 injection rate, with SO2 releases starting in the year 2034. Global amounts of the injected SO2 rises from less than a tenth Tg of SO2 per year to a few Tg of SO2 per year in the 2040s. Individual SO2 injections are of only a few to a few hundreds of *(note the modification)* tonnes of SO2 each, with e.g. the cumulated SO2 injected from start of the SAI experiment to end February 2034 not exceeding 200 tonnes. Even when considering the overall annual injection in this PR scenario in the late 2030s and 2040s, this corresponds to the order of magnitude of stratospheric injections of quite moderate, if not small, stratospheric volcanic eruptions (e.g., Schmidt et al., 2018). For example, the recent eruption of Hunga volcano (Kingdom of Tonga) injected around 1.0 Tg of SO2 (Sellitto et al., 2024), while climate-relevant eruptions, like the one in 1991 from Mount Pinatubo (Philippines), can reach values as large as 15-20 Tg of SO2 (e.g. Aubry et al., 2021)." We also repeated the order of magnitude of interest for this paper, for SO2 injections, in the discussion of Sect. 5. We hope that with this existing and added information, the quantitative amount of what is called here a "weak" SAI experiment, which is targeted by this study, is clearar.

GC2) The manuscript uses the terms 'geoengineering', 'SAI', and 'SAI geoengineering' interchangeably and without a clear definitional framework. Since the study focuses specifically on stratospheric aerosol injection, I recommend adopting SAI as the primary term. Consistent terminology is not merely a stylistic concern, it is essential for clarity, precision, and to avoid ambiguity, particularly given the broad and sometimes contested use of the term 'geoengineering' in the literature.
We now use the "geoengineering" term only when defining SAI in its broader context (title, abstract and introduction), and use just "SAI" consistently in the following text.

Specific comments:
SC1) Title: "... stratospheric aerosol injection geoengineering interventions". Unfortunately, the list of terms overlaps and is redundant. Stratospheric aerosol

injections (SAI) are a form of (solar) geoengineering and already involve deliberate intervention. I recommend using only the term ‘stratospheric aerosol injections’ in the title. If there is a valid reason for this combination of terms, please explain.
We agree, this is quite redundant. We would like to keep this just in the title (we corrected throughout the main text) both the “SAI” and the “geoengineering” reference, for the sake of indexing and referencing, but we suppressed “interventions”.

SC2) l. 19: ”…of a high-spectral-resolution limb-emission sounding technique…” or ”…of high-spectral resolution limb-emission sounding techniques…”.
Yes, right, corrected

SC3) l. 19: ”very weak”, please provide quantitative information rather than ambiguous qualitative descriptions. Please make this change here and throughout the manuscript.
A few line later in the Abstract (please note the modification): “This concept would quantify the SO2 injected mass, even for injections of the order of magnitude of a few to a few hundreds of *(note the modification)* tonnes of SO2, characteristic of “near-term” to “mid-term” experiments feasible even unilaterally *(note the modification wrt Reviewer #2 comments)* …”. On the one hand, the target orders of magnitude of the SAI deployment are explicitly mentioned and, on the other hand, we think that this is the best way to convey all the needed information in a compact way fit for an Abstract (which is already 250 words, i.e. the maximum allowed). We implemented the above-mentioned modification throughout the paper. See also our reply to your GC1.

SC4) l. 20: ”stratospheric aerosol injections geoengineering interventions”, please see my comments above. Please make this change here and throughout the manuscript.
We systematically avoided the “SAI” + “intervention” combination throughout the text and sometimes used rather the “SAI deployment” wording.

SC5) l. 24: ”unilaterally/illegally”: But only with regard to the injection rates examined here. Illegal injections are very unlikely to occur evenly between 30° N/S and 15° N/S. Please rephrase this here and throughout the manuscript.
We totally agree that the specific locations of injection for this existing PR scenario (oceanic regions in the tropics) are probably not what would be expected for a deliberate uncoordinated SAI deployment, but our results don’t loose generality because they are based on magnitude analyses of the PR/PO fields only, not on the specific location. We feel that this is quite clear specifically in this Abstract sentence. By the way, we added a sentence in Sect. 3.1 (description of the geoengineering scenario) to clarify this point further (L181-187): “As mentioned in the Introduction, in this paper we test the capability of CAIRT-like instruments to detect and quantitatively characterise small SO2 injections and their effects, to represent the possible case of a near-term SAI experiment, a unilateral weak SAI deployment or a field experiment. Our PR scenario is meant to be a representative case in terms of the magnitude of the SO2 injections, but it is important to stress that it would be quite unlikely that such possible deliberate injections take place in the injection points of this specific scenario (over oceanic regions in the tropics). Despite this, our study does not loose generality, being associated with analyses of the observable magnitude of the resulting SO2 and SA fields.”.

SC6) l. 106: ”We assess the capabilities of CAIRT to detect extremely small amounts of SO2”. Please be more precise (here and throughout the manuscript).
See our reply to your GC1

SC7) l. 141: ”The solar geoengineering scenario” or ”The stratospheric aerosol injection scenario”.
We modified the title of Sect. 3.1 to “The solar geoengineering scenario”

SC8) l. 157: ”In this geoengineering simulation…”- see my comments above. Please make this change here and throughout the manuscript.
“Solar” added here and in other places in the text.

SC9) l. 160: Please add the injection height.
We added the information

SC10) l. 168: The difference between volcanic eruptions and potential SAI is not clearly pointed out here. Continuously injected sulphur amounts lead to much lower injection amounts per time compared to a volcanic eruption with the same amount injected. Please rephrase it here and throughout the manuscript.
It is actually stated in the Introduction (L92-95 and L100-106) where we added a sentence to clarify the inherent different of injection rates between volcanic eruptions and SAI deployments: “In addition, volcanic eruptions generally produce SO2 injections over short time interval while SAI deployments are based on continuous injections with consequently smaller SO2 injection rates.”.

SC11) l. 205: through?
Corrected.

SC12) Fig. 3: Maybe a different colour combination instead of orange and green? Or use different line styles.
We did not get this comment: why are orange and green not a satisfactory colour combination, from your point of view? Please clarify this point and we will be happy to modify the figure in case.

SC13) l. 235: ”The weaker SO2 injection…”, please be more precise. Change it here and throughout the manuscript.
We added here: “…(cumulated injected SO2 mass less than 10 tonnes overall until end February 2034)”

SC14) l. 248: What is the corresponding radiative forcing?
We did not calculate this but, we suppose, with 200 tonnes SO2 injection overall the radiative forcing of the formed SA would be largely negligible.

SC15) Fig. 8: Some panels have y-ticks and some don’t?
Corrected.

SC16) l. 308: ”… CAIRT’s extensive spatiotemporal coverage…”. Please be more specific and provide us with the quantitative information here. So far, this is purely a qualitative and ambiguous statement.
This was corrected to “Finally, these simulations demonstrate that CAIRT’s dense spatial sampling and relatively good revisit time, …”, which are quantitatively described in Sect. 2.

SC17) l. 323: Where is the 5- 10% error range coming from?
(I suppose you are referring to rather L325 and the “lower than 5%” statement.) That’s how we read the data in Kovilakam et al., 2024 (Page 1, Sect. 2) but please correct us if you think that we are wrong.

SC18) l. 326: Unfortunately, that statement is not correct. The statement in Lange et al. is not that SAI deployments of less than 1 Tg S/y cannot be detected, but rather that 1 Tg S/y can definitely be detected. Furthermore, the study also examines the first month of the initial phase– at that point, only 1/12 of the injected amount had been released, which could not be distinguished from natural variability. Please rephrase.
Yes, that’s true, our phrasing was unfortunate. We slightly modified the sentence to account for these two points.

SC19) l. 328: OK, but you can’t compare continuous injections to volcanic eruptions, even if they involve the same amount of injection. Continuous injections result in much smaller injected amounts per time compared to a volcanic eruptions with the same amount injected.
Also in reference to SC10, to clarify this aspect, we modified the sentence as follows: “…and thus proportionally smaller resulting burdens of the formed SA. While the SA formed by a full-blown SAI deployment (i.e., with cumulated injected SO2 of the order of magnitude of a moderate volcanic eruption or more) would rather be observable by solar occultation instruments like SAGE III/ISS, initial or intermediate deployments, or limited-scale tests, will be harder to characterise with these instruments. As an example, Lange et al. (2025) demonstrate that it is possible to detect stratospheric aerosol perturbations resulting from SAI with SAGE III/ISS-like solar occultation instruments in case of injections of 1-2 Tg of sulfur per year, which is typical of (or even larger than) recent moderate volcanic eruptions and far larger than the magnitudes of the injections analysed in the present study.”

SC20) l. 378: ”… our results stress the importance of increasing our global observational capabilities…”. As the manuscript currently is, I do not agree with this statement. Please elaborate on this point and revise the manuscript accordingly.
This is meant to be a summary statement associated with the larger discussion of Sect. 5. and the results presented in Sect. 4. As none of the existing and planned satellite instruments can presently characterise SAI based on injections of a few hundred tonnes of SO2, in terms of SO2 itself and the resulting SA simultaneously, while high-spectral-resolution mid-infrared limb-emission instruments, like to one concept described in this paper and discussed in Sect. 5, can do, we think that this statement is justified. We don’t think, for the sake of concision, that these different arguments, already expressed in detail in Sects. 4 and 5, should be repeated here.

Reviewer #2

The authors present a theoretical estimation for the performance of a potential future instrument. The intent of this instrument, as described within the manuscript, is to detect and monitor extremely low levels of SO2 and sulfuric acid stratospheric aerosol. Every aspect of the analysis is theoretical in nature from the emissions, to the transport, the instrument measurement, and retrieval. Though the authors do not describe it as such, this is ultimately a theoretical sensitivity study. In my view, this paper can be published after major revisions.
First, we would like to thank you for the careful evaluation of our manuscript and for providing constructive comments that helped improve the quality and clarity of the paper. The nature itself of this study makes it intrinsically "theoretical", in the sense you hinted here. We study the capability of a concept instrument to detect and characterise SAI from a simulated PR scenario – both the instrument and the scenario must be simulated as they do not exist in the real, observable, reality. We don't see how this can be realised if not with *simulation* ("…from the emissions, to the transport, the instrument measurement, and retrieval."). In fact, this might be possibly called a "reduced chemical observing system simulation experiment" (reduced in the sense that any data assimilation in a control run isn't realised here, see framework by Edwards et al., 2009 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JD011375, in particular their Fig. 1). This work is fully described as such, even considering the inherent PO/PR terminology, so there isn't any ambiguity here, in our opinion.

Please find in the following a point-by-point reply to your specific remarks (identified by "SR" followed by a progressive number).

Specific Remarks:

SR1) The authors dedicate a lot of text space to highlighting the sensitivity of this method (e.g., it can detect extremely small injections of SO2 over very short time scales (i.e., 3.5 tons SO2 injected over 2 months)). What is not clear is whether this is important or not. Why should we care about detecting such small-scale injections that have no impact on radiative forcing? Providing this context will greatly help orient the reader. Please add this information. On the other hand, if the authors have no justification for needing this sensitivity and their only intent is to demonstrate the sensitivity of the method that is fine too. Knowing this will help me, as a reader, orient myself to the intended importance of this paper.
This kind of detection and quantitative characterisation of inherently weak field SAI experiments or near-term, possibly unilateral, SAI deployments is at the very core of the scientific and science-policy debate for the general theme of SAI MRV (monitoring, reporting and verification). This is, in turns, strictly linked to the ongoing debate on the SAI governance, which defines what is "unilateral" and, ultimately, "legal". A few examples of this present debate, carried out at different national and international levels, are in the following, for institutional entities going from the global scale (see the United Nations Environment Programme - UNEP - report "One Atmosphere: An Independent Expert Review on Solar Radiation Modification Research and Deployment", https://www.unep.org/resources/report/Solar-Radiation-Modification-research-

deployment - already cited in the manuscript), to the continental scale (European Parliament briefing document "Options for the responsible governance of research on solar radiation modification", https://www.europarl.europa.eu/thinktank/en/document/EPRS_BRI(2026)789310) to the national scale (the following report endorsed by the German government: https://metis.unibw.de/en/nike/). An example of this kind of identified SAI deployments risks and a definition of "unilateral deployment" can be found in the mentioned UNEP report: "SRM could create societal risks including the potential for international conflicts (because of transboundary effects), unilateral SRM ('rogue' parties may opt for SRM deployment) and counter and countervailing SRM deployments. SRM deployment would therefore raise ethical, moral, legal, equity and justice questions." On the other hand, the same document also states that "The European Parliament calls for an EU-wide moratorium on the use of SRM and advocates a global SRM governance framework based on a firm stance of non-deployment." So, the governance of these deployments is felt as a pressing but unresolved issue ("Existing treaties regulate certain pollutants, but none cover the deliberate and non-hostile release of aerosols in the stratosphere.", in the following report from EU think tank CFG: https://cfg.eu/climate-interventions-faq-spotlight/) and underlines the pressing need for the definition of a legal framework where actions and, possibly, test should be framed. In this context, in spite of their small potential immediate impacts on the radiative balance, field or near-term SAI experiments, like the ones studied in this paper, are a clear entry point to SAI deployment and are matter of intense debate (formal ban by some governments was called in recent years, e.g. in France, https://www.academie-sciences.fr/rapport-geo-ingenierie-climatique-appel-la-prudence-et-un-encadrement-rigoureux, or Germany, see previous report mentioned above), because already feasible with today's technology and clearly less monitorable with exiting observing systems, as our paper tries to demonstrate. A call for a formal governance of field SAI experiments is suggested in the previously mentioned UNEP document. To reply collectively also to other related SRs, we added more details on our definitions of "SAI governance" and "unilateral SAI deployments", see text added at L72-78: "The deployment of SAI techniques outside a shared global governance framework has been identified as a potential source of societal risks, including of international conflicts, due to their intrinsic transboundary effects (United Nations Environment Programme, 2023). Unfortunately, such a global SAI governance framework still does not exist to date, even though the urgent need for such a framework was emphasised by different international (e.g. United Nations Environment Programme, 2023; European Parliament, 2026) and national institutions (e.g. Consortium National Interdisciplinary Climate Risk Assessment, 2025; Academie des Sciences, 2025). In the following, we will refer to hypothetical SAI deployments carried out outside global international agreements as "unilateral SAI deployments."
Based on these considerations, we think that the (presently unavailable) capability to monitor such small-scale SAI experiment is an important issue and, thus, our present work is important (and unique), to reply to your question.

SR2) Throughout the paper the authors refer to "illegal" SAI activity. If the authors can reference a law that defines such activity as illegal then please reference that. If not, then please remove all declarations of such activity as being illegal.

Using the framework defined in our reply to SR1, we don't define anymore these potentially unregulated deployments (i.e. outside a global international SAI governance framework), at all spatiotemporal scales, as "illegal" anymore. In fact, this is not needed anymore as, in effect, there is not yet an international governance framework, even if, as discussed above, it is a pressing need raised by part of the international community. We corrected the text throughout the paper accordingly.

SR3) lines 29-30: "Our results stress the importance of increasing our global observations capabilities…" I apologize, but I failed to see how your results support this conclusion. This is, ultimately, a sensitivity study and the authors fail to provide a compelling need for this specific instrument.
This need is discussed in detail in Sect. 5. Our answer to the question "Do we have the capability to detect early SAI deployments with the present satellite instruments?" (which is the title of Sect. 5) is "No, we don't". This evidence, coupled with the points discussed in the Introduction and in our reply to SR1, suggests the need of this specific satellite observing system technology. Nevertheless, we decided to smooth down a bit this claim and some of the statements in Sect. 5.

SR4) line 77: "…and the needless physically…" It is unclear what was intended. Please revise.
This means that satellites do not need to physically access the areas of interest, opposed e.g. to in situ techniques. We have reformulated the sentence to clarify this point.

SR5) line 81: Please consider referencing Vernier et al. 2011 (doi:10.1029/2011GL047563) because they show the impact of small eruptions on stratospheric aerosol load and hence climate.
Agreed and added.

SR6) line 93: "…exploratory SAI injection dynamics (small rate at high altitude) are unlikely what is observed…" It is unclear what was intended. Please clarify.
We mean that, as clearly expressed by Carn et al., 2016, volcanic eruptions inject SO_2 with burdens that are correlated with the magnitude (VEI) and then altitude: large amounts are injected at higher altitudes and vice-versa. This is not the case for SAI, where this correlation is not in place and small amounts of SO_2 can be injected at high altitudes. We slightly modified the sentence to clarify it.

SR7) lines 98-101: Here, the authors indicate the CAIRT instrument will be flown (i.e., it is "planned") in the 2030s. Later (lines 138-139) they state the instrument was not selected for flight. Please clarify.
We modified the sentence to "The purpose of the case study presented here is to assess the capabilities of the high-spectral-resolution mid-infrared limb emission sounding satellite technique to detect the deployment of exploratory SAI interventions and to monitor their impact at a short-term time scale. This technique is modelled around the Changing-Atmosphere Infra-Red Tomography explorer (CAIRT) mission concept, one of the candidates to the European Space Agency (ESA) Earth Explorer 11 mission definition and feasibility studies (Phase 0/A).", to explicitly mention that it was one candidate through the preliminary phase of mission definition and feasibility studies. We also

mention in Sect. 2: “The CAIRT mission was designed for a duration (lifetime) of at least five years, and this study is set around the 2030s, the programmatic timeframe of Earth Explorer 11 operations. Unfortunately, CAIRT was not finally selected for implementation within the Earth Explorer 11 programme.”, to clarify that it was not finally selected for implementation.

SR8) line 154: What is meant by “relatively optimistic”? Relative to what? Please clarify and/or quantify.
This is “with respect to other SSP scenarios”, and this was added to the text.

SR9) line 161: Here, the authors state injection latitudes were 15◦ and 30◦ N and S. There is no discussion of the 30◦ injections and I see no indication of injections at 30◦ in the figures. Would the authors please clarify whether injections were made at these latitude or explain why they are not part of the broader discussion and seen in the figures?
See SR12 and SR16.

SR10) Figure 1, caption: “...SO2 concentration...” These are mole fractions or mixing ratios, not concentrations. Please correct here and throughout the paper.
You are right, thank you. That’s corrected here and throughout the text.

SR11) Figure 1, panel d: Per lines 177-178 and 246-247 the vast majority of SO2 was injected in the northern hemisphere. Why is there virtually no SA in the NH of this figure, but massive amounts in the SH?
Because the SAI injection rates vary over time in this specific PR scenario, with initially weak injections at NH and weaker at SH, then switching to larger injection rates in the SH – all is described in detail by Tilmes et al. (2020), that we cite in our paper to reference the scenarios used here. In January-February 2034, in particular, 15°N is largely dominating as an injection point. We slightly modify the sentence at L177-178 to clarify that that specific NH/SH breakout is “for that specific time frame”.

SR12) Figure 1, panel d: The thrust of this study is focused on January/February 2034, which is obscured by the scale of the colorbar. Since this figure is referenced throughout the paper, is this really the best visualization? Please consider revising or please help me better appreciate the utility of this figure (i.e., tell me why I am wrong).
We just wanted to show the general context of the PR scenario, at larger time scale, which is of interest, we think, and well placed in the section where we describe the PR scenarios (Sect. 3.1). More specific elements showing the SA conditions during the year 2034 are in Figs. 6-8.

SR13) line 216: “values of 0.15-0.25 10−9 mol/mol, for SO2, and 1.0-1.5 kg/kg, for SA.” First, the 2 in SO2 needs to be a subscript. Second, why the shift in units (mol/mol to kg/kg)? The differing units make it harder to interpret the upcoming figures.
SA cannot be quantified through moles, as it is a liquid solution comprising different species. On the contrary, SO2 is better expressed in molar mixing ratio. Also, and for these reasons, this is how the pseudo-inversion scheme works in the official CAIRT FL2S used in this work. We corrected SO2 to SO_2.

SR14) lines 216-217: "These uncertainties are generally smaller than background unperturbed stratospheric concentrations of SO2..." A few points regarding this:– Saying these are "generally" smaller implies they are not always smaller. Please quantify this. What are the background SO2 mole fractions?– These are such small values (and the authors focus on extremely small injections), how does this compare to the natural variability in the context of background conditions and persistent small eruptions?– Please provide references to support these claims.
Please find quantitative background SO2 and SA with a representative reference (MIPAS observations) added to this part of the text.

SR15) line 230: "monthly average" Is Fig. 4(a) really an average over February 2034? To me this looks like a month's worth of satellite observation tracks. Please clarify.
Yes, you're right, this is over one representative daily track group of 15 orbits (discussed in Sect. 2). We clarified the text accordingly.

SR16) line 235: Per a previous comment, here the authors state the injections were at 15∘ N/S, no mention of 30∘. Please clarify.
As said before, the SAI rates varied over time. The very first injections in January-February 2034 are indeed at 15°N and 15°S.

SR17) line 237: "...are also seen by CAIRT POs" Where?
As we start with point inject locations, the zonal dispersion can be easily seen in Fig. 4a (SO2 plumes covering a large longitude range and not just two "point"). Similarly, Figs. 4b-c show that the vertical extent of the plume is not limited to two "points".

SR18) line 238: "...closer to the detection limit." True, this is closer to the limit of detection than in the NH. However, this is still ≈5x the limit of detection. When I first read this I thought the authors were trying to caution the reader against putting too much trust in the SH measurement (e.g., warning, this is close to the LOD!). Please clarify whether this is still considered reliable or not.
We think so and modified the sentence accordingly.

SR19) Figure 4, panel c: There is clearly signal in this panel (the PO) that was not present in the PR. Further, it appears these "false positives" are well above the stated uncertainty of Fig. 3. Further, the magnitude of these sporadic signals look to be roughly the same magnitude of the SH signal where SAI occurred. This demonstrates an unaccounted for bias in the methodology, which raises serious questions regarding the feasibility of this method in accurately detecting small injections. Please address.
There is probably a small pixel-wise occurrence of random errors giving small positive values (a few tenths of 10^{-9} mol/mol) where PR points at zero-values, that can generate what is seen by the Reviewer as false positives. We mention now this in the discussion of the figure: "Limited and isolated pixel-wise small positive values are also noticeable in areas where the PR points at zero-values (e.g. around 40°N and 30 km altitude). These can be attributed to random PO errors and are still easily morphologically distinguishable from the identified plume-shaped and multi-pixels detection areas." It must be stressed again that these are extremely small SO2 injections (overall less than 200 tonnes), producing the global effect shown in Fig. 4a, and corresponding to specifically 0.5 tonnes

intercepted by the satellite track around the overall plume injected at SH. On the one hand, a one pixel isolated “false positive” of this kind (e.g. around 40°N) would be associated with even smaller SO2 injections, so of the order of magnitude of background stratospheric SO2 content, and on the other hand practical detection implementations can be easily imagined using techniques based on positive detections over different contiguous pixels.

SR20) line 247: Please move the injection amounts to earlier in the paper.
This is part of the discussion of Fig. 5 and we don’t think it would be a good idea to move this earlier. For the global injection amounts, this is now discussed earlier in the text, see Reviewer #1’s GC1 and related SCs.

SR21) line 249: The authors reference Fig. 1(c). Given the scale in Fig. 1(c), it has no relevance to the current discussion. Please revise Fig. 1(c) or clarify.
See SR12.

SR22) line 252: What is meant by “a few tonnes”? On line 249 you stated the integrated injection did not exceed 200 tons, but on lines 247 and 253 you reference 0.4 and 3.x tons. I am very confused as to what is actually happening in the model and what you are claiming. Are you suggesting you can detect injections of “only” 200 tons, or are you claiming to detect injections of only 3.4 tons? I think the latter is not defensible because, if I understand the preceding text correctly, you are stating the mass detected in Fig. 4 was ≈ 3 tons in the NH, which is not the same as identifying an SAI of 3.4 tons. Please clarify the model parameters (especially injection location and injection rate, not just total amount) and please clarify the surrounding text.
As now clarified in the text (see our replies to different comments of Reviewer #1), for the first SO2 injection of the PR scenario shown here the total SO2 injection is about 200 tonnes, with the amount intercepted by the identified orbit in the figure is about 3 and 0.5 tonnes in the NH and SH injection points.

SR23) line 256: Please cite a specific law or remove “illegal” throughout.
Done, see also SR1 and 2.

SR24) Figure 5: Please include latitude in the subplot titles for easier reading.
Done

SR25) Figure 5: Please confirm correct panel orientation. Per the caption, (a) is for SH and (b) is NH, and the detected SO2 mass in (b) is ≈7 times higher than (a)…but the x-axis in (a) is an order of magnitude lower. This cannot be true; the panels were probably swapped.
You are totally right, thank you for spotting this. We corrected the caption accordingly.

SR26) Figure 7: There seems to be significant discrepancy between this figure (specifically panel (a, b)) and the data shown in Fig. 4 (b, c). Granted, the figures contain different parameters, but Fig. 7 seems to show SA formation where no SO2 was present in Fig. 4. Please clarify.

Those in Fig. 7 are monthly averages SA mixing ratios (background and SAI enhancements), and in particular Fig. 7a-b are for March. First, stratospheric (SA) aerosols exist at all latitudes naturally (the Junge layer), even in the absence of SAI (or volcanic) perturbations. This is basically what is visible at high latitudes in March. Also, by that time (March and then later for the other panels), the SAI enhancements of the stratospheric aerosol layer dispersed meridionally, consistently with typical time scales of the meridional dispersion of secondary SA aerosols formed by SO2 injection in the stratosphere (see the case of recent moderate volcanic eruptions, e.g. Hunga: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL105565 or others). This figure looks like perfectly reasonable for us. We added the adjective "SAI-enhanced" to SA in the text linked to this figure to clarify this.

SR27) lines 294-296: It is unclear why the authors only show the nucleation mode in the corresponding figure (Fig. 8), whereas they showed all 3 modes in the preceding figures/discussion. This impairs comparison between figures. Please either justify this choice or make the figures consistent.
This is already justified in the text: "This mode is selected here because its response to SO_2 injections is quicker and more sensitive to their deployment than the other two larger-size modes in the model PR simulations and PO".

SR28) Figure 8: Several points:– Adding grid lines to the plots greatly helps the reader determine changes in magnitude and altitude. Please consider adding to help the reader to quickly interpret and contrast these panels.– The caption states these are "global" averages. Is that true, or NH only?– It is difficult to determine whether the March panel matches 7(b). This looks like a gross underestimation. This is just an example of where having Fig 7/8 depict the same thing would aid interpretation.– Can the authors comment on why April has the highest SA content of all panels?
For the grid lines, in busy figures like this one, adding further grids (12 here!) might get the figure even busier. By the way, if the Reviewer really thinks this useful, we will add. These are global averages, not only at NH. It is not an underestimation as it is just one of the three modes composing Fig. 7a-b. Please also note that April month has larger peak values but a sharper peak than later months.

SR29) line 345: "...scarce revisit times..." Per Fig. 4(a), the CAIRT revisit time doesn't look very good either. Please clarify.
The repeat time proposed for the CAIRT concept was 29 days for a full global coverage (see Sect. 2), which is not possible with solar occultation or space LiDAR instruments. Figure 4a is just for a representative daily orbit group. We also now mention in the text that solar occultation provide "sparse observations", and space LiDAR instruments have "no across track capabilities", which gives maybe a clearer idea of their geometrical limitations.

SR30) Conclusions section: The authors still have not explained why we would want to identify such small injections. Granted, current satellite instruments cannot make this observation. However, by the time the SO2 injections are large enough to make a climatic impact they would be well above the detection limit of modern satellite instruments.
See SR1 and 2.

SR31) line 377: "…clandestine nature." This is a provocative statement and presents an unnecessary distraction from the text. I truly wonder what is meant here. Can the authors provide examples of where and why somebody would conduct clandestine SAI and tell us whether this is a real threat or not? If so, then this information would better fit the introduction. If not, then this claim is an unnecessary distraction and I ask the authors to consider removal.
To make a link with the definitions given in the Introduction, we changed "clandestine" to "of uncoordinated nature with respect to the international community or any SAI international governance".

SR31) line 380: "…well characterized by CAIRT PO" This is not true. The model you used provided the self-lofting, CAIRT provided no characterization, it just saw what the model provided. Please correct.
We changed "well characterised" to "well reproduced".

SR32) lines 381-386: This seems to be predominantly speculation about what a theoretical instrument may provide and does not belong in the conclusions. This may be more appropriate in the Introduction. Please consider revision or removal.
For us, this is a perspective point, pointing at what else is observable with such an observation technique and then which possible analyses might be imagined beyond what is described in this manuscript. Such technique is not studied only for the capability to detect and characterise weak SAI and we think this statement belongs to perspectives and, we think, is more meaningful after we present our results (thus in the Conclusions).

SR33) lines 391-393: "This issue is expected to become more urgent…" How? In the preceding sentence you stated that current satellite instruments are incapable of providing these types of measurement. If current satellite instruments are not capable of detecting early SAI then there is no difference between having these instrument and not having them (from an SAI-detection perspective), which makes the last sentence of your paper false. Please revise.
We slightly revised the sentence to state that even the potential capability of present-generation instruments to get information for larger SAI deployments will possibly decrease in the near-future due to the projected dearth of observations of stratospheric composition