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 SO_2 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.

Specific Remarks

- The authors dedicate a lot of text space to highlighting the sensitivity of this method (e.g., it can detect extremely small injections of SO_2 over very short time scales (i.e., 3.5 tons SO_2 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.

- 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.

- **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.

- **line 77:** "...and the needless physically..." It is unclear what was intended. Please revise.

- **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.

- **line 93:** "...exploratory SAI injection dynamics (small rate at high altitude) are unlikely what is observed..." It is unclear what was intended. Please clarify.

- **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.

- **line 154:** What is meant by "relatively optimistic"? Relative to what? Please clarify and/or quantify.

- **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?

- **Section 3.1:** Including information on injection amounts (rates) and longitudes would help orient the reader to interpret upcoming figures. The authors provide this information later in the paper, but it should be here. Please include that information in this section.

- **Figure 1, caption:** "...SO_2 concentration..." These are mole fractions or mixing ratios, not concentrations. Please correct here and throughout the paper.

- **Figure 1, panel d:** Per lines 177-178 and 246-247 the vast majority of SO_2 was injected in the northern hemisphere. Why is there virtually no SA in the NH of this figure, but massive amounts in the SH?

- **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).

- **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.

- **lines 216-217:** "These uncertainties are generally smaller than background unperturbed stratospheric concentrations of SO_2..." A few points regarding this:

  - Saying these are "generally" smaller implies they are not always smaller. Please quantify this. What are the background SO_2 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.

- **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.

- **line 235:** Per a previous comment, here the authors state the injections were at 15° N/S, no mention of 30°. Please clarify.

- **line 237:** "...are also seen by CAIRT POs" Where?

- **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.

- **Figure 4, panel a:** Coming back to a previous comment, it would greatly help the reader to know where, and how much, the injections were made.

- **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.

- **line 247:** Please move the injection amounts to earlier in the paper.
- **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.
- **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 $\approx$ 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.
- **line 256:** Please cite a specific law or remove "illegal" throughout.
- **Figure 5:** Please include latitude in the subplot titles for easier reading.
- **Figure 5:** Please confirm correct panel orientation. Per the caption, (a) is for SH and (b) is NH, and the detected SO_2 mass in (b) is $\approx$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.
- **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 SO_2 was present in Fig. 4. Please clarify.
- **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.
- **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?
- **line 345:** "...scarce revisit times..." Per Fig. 4(a), the CAIRT revisit time doesn't look very good either. Please clarify.

- **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 SO_2 injections are large enough to make a climatic impact they would be well above the detection limit of modern satellite instruments.

- **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.

- **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.

- **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.

- **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.