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 quantitative 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.

Major Comment:

1. The main analysis focuses on a continuous SO_2 injection rate of 30 Tg S yr^{-1}, with only a brief mention of results for 10 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 Tg S yr^{-1} were selected as representative cases.

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-quantitative sensitivity discussion would greatly enhance the broader relevance of the results.

2. I concur with Referee 1 that the manuscript currently lacks sufficient depth in its analysis and discussion. The Results and Discussion section often 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.

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.

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.

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.