We would like to thank this reviewer for reviewing this manuscript and providing valuable feedback. Our responses are provided below (blue) to the reviewer's comments (black).

There are no questions regarding the results because the conclusions are predicated on assumptions established during computation.

This is not a correct interpretation. While we do not interrogate every possible assumption regarding the inference of aerosol size distributions using SAGE III data, we do, and in fact it is the main point of the paper, examine in depth the impact of key assumptions in this process. So in this regard, we are not certain exactly what this reviewer is objecting to.

I believe the title should be revised, adding "Characterization of Stratospheric. . . "

Stratospheric was added to the title.

The affiliation list for the third author, 2,1, seems odd. Switching it to 1,2, if it is associated with both institutes, appears more appropriate.

The affiliation list was updated.

Is this approach not a revisiting or improvement of Wrana et al.'s Stratospheric Aerosol Size Distribution Retrieval from SAGEIII/ISS?

To some degree yes, though these types of estimates have been done for decades (as cited in the paper). The Wrana et al. paper was discussed within this manuscript and we included discussion on improvements we sought to make (including an extensive analysis of the impact of the assumptions).

Section 2.1 seems like a less significant subsection. Yes, the computational strategy remains a fundamental aspect of data loading, processing, and modeling. In open science research, limitations in computational resources should not serve as excuses for achieving suboptimal results. Nevertheless, it is ultimately up to the authors to decide what they choose to present.

It is unclear to us what portion of our work the reviewer is referring to as "suboptimal" but we hopefully address this misunderstanding here. First, the purpose of this statement, within the context of this manuscript, it to tell the reader that reproducing the single-mode analysis does *not* require expensive hardware such as the A100. However, the bimodal analysis does require the bigger A100. There were no trade offs made here and the hardware selection in no way limited the accuracy of the results.

Line 44: The challenge of performing "traditional" ????

The text was updated to include examples of traditional validation work.

"65%, 70%, 75%, and 80% sulfuric acid by weight." At what conditions are all these propositions of sulfuric acids present in the stratospheric aerosols? 75% is conventional acceptance for

background aerosols. May be going up to 90% just after significantly impactful volcanic eruptions makes more sense, especially when SGAE signals are not penetrating below tropopause or so.

This is possible. However, the purpose of this section is to evaluate the impact of incorrect composition assumptions. To that end, we believe the section fulfilled its purpose. The reviewer makes a good point about the variability of sulfuric acid content especially in the wake of large volcanic injections. As another reviewer pointed out, the sulfuric acid content can be estimated based on collocated temperature and water vapor measurements. Currently, the SAGE mission produces a water vapor product, but the temperature product is still in development. When the temperature product becomes available we can possibly modify our algorithm to estimate sulfuric acid weight percent. However, such an endeavor will be a separate study.

Do the black carbon and brown carbon are uniformly present in the stratospheric aerosols? If so, is the Mie theory still suitable for this situation? Yes, the possibility of the existence of absorbing aerosols may be considered, but it is not guaranteed solely based on sporadic occultation extinction.

As discussed in the paper, stratospheric smoke is a huge unknown. The composition, etc. is largely undetermined. Again we note that the intent of this section is to determine the impact of an incorrect composition assumption. To answer the reviewer's question: yes, Mie theory is an appropriate model for smoke particles.

Also, "65%, 70%, 75%, and 80% sulfuric acid by weight." So, what constitutes the remaining fraction of water vapor, black or brown carbon, or unknowns?

Yes, the rest of the particle is composed of water. The text was updated to reflect this.

Also in line 250: "H2SO4 is typically assumed to be 75%."" Correct. We see no required change in regard to this comment.

It seems appealing to use black or brown carbon, but it cannot ignore the complex chemistry in the stratosphere at higher temperatures and shorter wavelengths of radiation, as well as the ozone at its maximum. So, we need to be mindful while making such arguments with limited resources.

We apologize, but it is unclear to us what the reviewer means by this comment. The SAGE retrieval algorithm accounts for diverse components in the stratosphere (including ozone) and the aerosol extinction coefficients were derived in light of these components.

Line 148: (0.3±15%,1.2±10%) It should be explained: the basis of 0.3 and 1.2 to be selected as representative and any reasoning for selecting these two ratios. Is there any information provided by these ratios (just an arbitrary random number or something else)?

Correct, these ratios were not taken from the SAGE record. Rather, they are referred to in the text as being "nominal" values to inform the reader that these are reasonable ratios but not representative of a specific measurement. As described in the text, this section is an illustration of the general solution process. Since the text already describes these values as "nominal" we see no necessary change.

Shorter wavelengths (448 nm) are susceptible to the other molecules. So, the ratio obtained by using them is reliable and consistent?

This depends on where the observations occur within the atmosphere. As discussed in the text, the shorter wavelengths (including 448 nm) attenuate higher in the atmosphere than the longer wavelengths. Conversely, the signal in the longer-wavelength channels drops below the noise floor at higher altitudes. This was one of the factors in our decision to limit the altitude ranges and in our selection of the hybrid "condition" selection (specifically section 4.1). Since the paper already addresses this issue we do not see any changes to be made.

In Figure 5 (a), The ratios on the x-axis place the larger wavelength's extinction in the numerator, while on the y-axis, the smaller wavelength occupies the numerator position. If there's no specific reason for this arrangement, it might be more consistent to use the same approach for both ratios and incorporate their respective values unless there is an otherwise to do otherwise.

The reviewer is correct that there is no scientific basis for putting one wavelength above the other. However, this presentation is consistent with that presented in Wrana et al. 2021 (indeed, this specific section addresses the Wrana et al. method, including limitations) so we keep this the same for consistency.

Same also in line 240 (#5, #6, and #15) from table 3 applied further.

It is unclear what the suggested change is. The manuscript was not changed in regard to this comment.

Lines 459 and 499: The cloud is filtered. The aerosol product is already cloud (opaque) filtered. Is it not? (SAGE III/ISS documents suggest that.) Don't we miss out on the fresh, larger particles after the eruption during cloud filtering? Did your results show a significant difference between cloud-filtered and non-cloud-filtered?

This is not correct. The SAGE data are not already cloud filtered and it is unclear what documentation the reviewer is referring to. If there is an ambiguity or incorrect statement in the retrieval documentation then we would kindly ask the reader to bring this to the attention of someone on the SAGE team.

The filtering algorithm is discussed in a publication by Mahesh Kovilakam (cited in the paper). Indeed, distinguishing between thin clouds and thick aerosol can be challenging. Undoubtedly removing clouds will change the PSD estimation on individual points within individual profiles (i.e., cloud filtering removes data). However, we did not notice any substantive change in the aggregate products after cloud filtering. This is now explicitly stated in the paper.

Figure 15 caption: last line: "(i.e.,k1020/k1020$\geq$1.4)" Now corrected