Response to Reviewer #2

We thank the reviewer for helpful comments. Our responses to the reviewer's specific comments are listed below. The reviewer's concerns are in bold italicized font and our responses are in regular font. The page numbers and line numbers given in our responses below are in reference to the revised version of the manuscript.

The manuscript ?SAGE III/ISS aerosol/cloud categorization and its impact on GloSSAC? describes a new method of cloud screening for SAGE III/ISS spectral aerosol extinction observations and how this changes its time series, how the new data set compares with correlative observations (OSIRIS and CALIOP) and, finally, how this modification impacts on the GloSSAC merged time series ? one crucial component of which is the SAGE III/ISS data set. The topic of the manuscript clearly is of interest for the AMT readership. Nevertheless, I agree with the Anonymous Referee #1 that this manuscript is not clear, not well organised and this requires a very systematic work of synthesis, re-working of the text and clarification in many places before it can be considered for publication on AMT. During my review, I struggled to follow the flow of ideas that drive the manuscript and so I can only recommend a deep re-structuring and re-writing of the paper before I can actually review it. To help out with this, I add a number of suggestions which are not to be considered a full review but just examples of some modifications that are need throughout the text. A more general comment is that, in my opinion, there are in general too many figures and the overall paper is too wordy. I would suggest to distil the most informative information in terms of both methodology and results and transfer part of the reduntant information in a Supplementary Material section of the manuscript. Another general major comment is that, while the methodological aspects are described in very detailed way (in fact, overlenghty ? I suggest distilling most of this in a flow-chart figure or something similar), the results are discussed very superficially, with a large collection of figures with scarce or even no insights and discussions. Results must be analysed more in depth, to identify the differences of the new method SAGEIII versus the old one and, especially, the impact on GloSSAC and the differences of v2.0 versus v2.2. My best regards

Specific comments

L27: Why not citing more recent papers on stratospheric-aerosol-mediated impacts on the radiative balance? (Raikoke 2019: https://acp.copernicus.org/articles/21/535/2021/ Australian fires 2020: https://acp.copernicus.org/articles/22/9299/2022/, Hunga Tonga 2022: https://www.researchsquare.com/article/rs-1562573/v1)

We now include all of the above references.

L28-29: there is "stratosphere" twice, please correct.

Corrected.

L30: The Hunga Tonga eruption (the largest in terms of stratospheric aerosol perturbation since Pinatubo 1991) should also be cited here (e.g. https://www.researchsquare.co 1562573/v1 or https://egusphere.copernicus.org/preprints/2022/egusphere-2022-517/)

Done.

L30: "low aerosol loading" should maybe be "smaller aerosol perturbations"?

Changed to "smaller aerosol perturbations".

L33: why using the past tense ("...this study was...")?

Corrected.

L34: please add a reference to explain what a PyroCb is - there are many from the group pf Mike Fromm.

We revised line 28 to mention PyroCb events and included a reference of Peterson et al., 2018 paper.

L48: is v2 the newest version of GLOSSAC? In case, please specify

We revised lines 48 through 50 and now specified the latest version (version 2.2):

"Herein, we describe a cloud screening algorithm for SAGE III/ISS to study the challenges in identifying pure aerosol and aerosol-cloud mixture from SAGE III/ISS observations and their impact on the development of the latest version of GloSSAC (v2.2)."

L56: why there is such Section 1.1? I would not use subsections of Section 1 (Introduction) and in any case this looks like more a part of Section 2 (Data and Methods)

Done. Moved this section to Data (section 2) and Methods (section 3).

L57: "is recently released" should be "was recently released". Please check verb tense throughout the manuscript.

This has been rewritten. Thanks. (lines 56-57).

L62: "band of what"? Please correct to "Chappuis ozone absorption band"

Done. Thanks.

L66-68 : This is one example of very clumsy sentence. The sentence is not clear, as

many others throughout the text. Please try to improve text clarity and the general language throughout the text.

This has been rewritten (lines 68-72).

Please add labels a) and b) in Fig 1

Done. Thanks.

L77-79: and what about large aerosol perturbations at altitudes larger than 25 km (e.g. for the Hunga Tonga eruption 2022 or the ascending smoke vortices following the Australian fires 2020)? This choice looks like very arbitrary and btw not adapted to asuch events.

We do not filter out any data above 25 km in the screening. For altitudes above 25 km, negative values mostly occur due to noise and errors in the removal of ozone and molecular scattering and therefore all data above 25 km are retained. This section has been rewritten to make things clearer (lines 70-72).

L85: Overall, for this issue of negative values and their removal, it sounds like a better understanding for this behaviour is needed, i.e. wrt the inversion algorithm at the basis of SAGEIII/ISS product. Is it a matter of lack of vertical sensitivity? I personally feel that this should be better clarified and such empirical correction is not fully satisfactory as it might screen our a number of points that are actually informative. Why not e.g. resampling the vertical profile at lower vertical resolution? Is there something that can be done by smoothinf the profile with given averaging kernels functions?

For SAGE-like observations where horizontal homogeneity is a key assumption, where this assumption breaks down is always a data quality issue. Vertical smoothing would likely eliminate negative values but not improve the data quality since the paradigm failure is simply being masked. We have rewritten section to make the issue more clear (lines 76-89).

As far as vertical sensitivity is concerned, the above issue has nothing to do with vertical resolution and vertical averaging will not fix the underlying problem.

L91: what do you mean with "shape of the distribution"? "Size distribution"? Then, is it a repetition?

This section is now rewritten (lines 108-123).

L94-95: please define the "extinction efficiency kernels".

This section is now rewritten (lines 108-123).

The Figure caption now reads as: (a) shows Mie extinction efficiency kernel ($Q_{(\lambda,r)}$, where Q is extinction efficiency, λ is the wavelength, and r is the radii) as a function radius for all SAGE III/ISS wavelengths.

L98: Please add a refence for "...following large volcanic eruptions". In addition, it depends on the eruption: for Pinatubo and Hunga Tonga, e.g., quite larger average particle sizes were found (see https://egusphere.copernicus.org/preprints/2022/egusphere-2022-517/ for Hunga Tonga)

We have added a reference now. The sentence now reads as :

"The variations with particle radius in Figure 3b show that at larger particle sizes, the dependence on radius becomes invariant so that above a particle size of about 0.5 µm, all particles have essentially the same 525:1020 extinction ratio. Under most circumstances, particles of this size, or extinction ratios close to 1, are due to the presence of cloud. However, material from intense volcanic eruptions like Mt. Pinatubo or ash, can produce similar ratios (e.g. SPARC, 2006; Legras et al., 2022)."

Section 2.1: The whole section must be rewritten because it is very unclear. I just stooped reading because I don't understand.

We have rewritten this section and now use a flow chart to describe the steps used in the method (lines 132-155).

L103: what do you mean here with "primary aerosol and enhanced aerosol"? "Primary aerosol" is usually used as opposed to "secondary aerosol"

We change it to "standard aerosol". Thanks.

L147: "additional" wavelength with respect to what? (SAGEII?)

This section has been rewritten (lines 158-165).

L148-149: it sounds like SAGEIII data have been used because there is a negative bias in the 525 nm channel. I don't think that you meant that, thus please rephrase this.

This sentence has been removed from here. We mention about the negative bias in 525 nm channel in section 2 (line 62) and clarifies the usage of 525 nm extinction coefficient data in GloSSAC in section 4 (lines 427-434).

L175: "(Thomason and Vernier, 2013)" is always TV13? Why not using this abbreviation?

Corrected. Thanks.

Section 2.2.1: What I don't understand here is how the cloud of points for each event is chosen, in particular in terms of time intervals around the date of a specific event, which has an impact on the estimation of the centroid and thus is critical for the overall methodology described in this paper. This choice sounds quite arbitrary and it seems that there are other quite empiric choices through the method description.

We now use a flow chart to describe the new method. We now discuss the empirical model used for the new method in the supplementary section (S2).

Section 2.2.2: at this point this reader is lost in the details of the algorithm. The description of the algorithm should really be synthesized and decribed, in terms of the different choices, in a clear and compact manner. I strongly suggest to gather all the different steps of the algorithm in a scheme, a describing flaw-chart figure or something similar

We now use flow chart to describe the algorithm (section 3.3) and the section has been rewritten.

L211-213: this sentence is one exemple of the many repetitions throughout the text and thet you should get systematicaly get rid of

Done. Thanks.

L214-215: Again, is the choice of using a rigid monthly statistic a good choice for such method? If an event occurs at the beginning or at the end of a month, this is clearly different and the temporal window of such analyses should adapt to this. Why not making averages centered around the actual date of the event?

We have considered using averages centered around a date using a time window. We think it is interesting, but for this application, we don't see much of a difference in the identification process. Additionally, if we use this method, we do not get enough data points to do any sort of statistics and therefore we used the standard procedure for now.

L214: They are not exacly at "51 N and 15 S" so use another wording like "at about……"

Done. We use "$\sim$" before the latitudes. Thanks.

Section 2.3: I would say that this comparison is only useful if a number of the interesting points (e.g. what is kept with one method and rejected with the other) are studied in more detail. As it stands, Fig 7 is not very useful, it is just a group of 4 clouds of points without any insight about what are the reasons for one method

to screen out or keep one point or another. How can we be sure that the new method is better than the old one?

This section has been rewritten and the figures have been replaced with extinction profiles for the comparison between the two methods (Section 3.4).

Section 3: why only one case is shown here (Canadian fires 2017)? To be more convincing, I would suggest to show more cases, e.g. in the Supplementary Material of the manuscript

We are showing additional cases for the comparison using extinction profiles in section S3 of the supplementary.

Beginning of Section 3: would it be better to introduce GloSSAC in the Methods section? In addition, there are many repetitions here of what discussed in the Introduction, please get rid of these repetitions.

Done. We now introduce GloSSAC under Methods (section 3.5). Thanks.

L298-299: please explain why this is an improvement brought by the new method.

This section is rewritten and discussed in section 4.0.

Section 3.1: the history of the different versions and the difference amongst them would be much clearer if summarized with a table instead if the lengthy introduction of this section

This section has been moved to section 3.5 where we describe GloSSAC and the individual data sets used in GloSSAC.

Sections 3.2 and 3.3: it sounds strange that these sections' titles are almost the same. Why OSIRIS is mention at both? Why not just reorganising in one unique comparison section, merging the two?

For OSIRIS and CALIOP, we use different methods for the conformance process. We use different sections as the methods used in these sections are different with different data sets.

Section 3.3: why many methodological aspects of the correlative instruments CALIOP and OSIRIS is described here and not in gthe Methods section? There are many mixed methodological and results information throughout the whole Section 3. Please reorganise your manuscript with a clear structured separation of Methods and Results.

We have reorganized the sections and all introductory description of GloSSAC is now under Methods

section (section 3.5). The sections for comparison between SAGE III/ISS and OSIRIS/CALIPSO are under section 3.5 in the Methods section (lines 337-416).

L425-427: this is a clear exemple of sentences that can be easily be made shorter, e.g.: "Figure 13 shows extinction coefficient for September 2017, following Canadian wildfire, for GloSSAC v2.0 and v2.2, as well as their ratios" or something similar. Please be more synthetic throughout the whole text

Corrected (lines 447-449). Thanks.

Why "Stratospheric Aerosol Optical Depth" is a Section 4 and not a part of subsection of Section 3 (i.e. a part of the Results section)?

It is now a section of the results (section 4.2).

Fig 1: I cannot see any negative extinction value (the x-axis scale goes from 10-7 to 10-1, all ¿0). Is panel b useful at all?

All the extinction values are plotted as absolute extinction and then we color code negative extinctions as blue and red depending on the error bars on them. The caption is now revised to make it clearer. Thanks.

Fig 1 caption: "shows how...", is a part of this sentence missing?

Revised the caption now as it was just a typo. Thanks.

Tab.1: The pyroconvective cloud activity of Aw started well before 6 January 2020 (I would say 31 December 2019 - visible in OMPS and CALIOP time series since the very beginning of Januiary 2020). Also, what about Hunga Tonga 2022?

Yes the first Australian pyrocb occurred on 31 December 2019 and the next on 4 January 2020. So, we now use 31 December 2019. Since the focus of the paper was to study the implications of aerosol/cloud categorizations on GloSSAC version 2.2 which was extended until December 2021. That's why we did not include Tonga eruption in the analyses. For SAGE III/ISS data, we reran our code to include data till September 2022, which includes Tonga eruptions as well. However, to extend the GloSSAC data set, we are waiting on CALIPSO level 3 stratospheric aerosol data to be available for the year 2022. We provide several other cases of comparison between TV13 and new methods in the supplementary section (S3) that also includes a profile from Tonga eruption.

Fig 3, 5, 6 and potentially all figures: please increase size of all in-figure text and labels

Done. Thanks.

Fig 13: these panels are very small. Why not a vertical (one column, three lines) orientation?

This figure has been removed. We use Figure 16 to discuss the differences between the versions.

Fig 15: if the v2.0 and 2.2 are strictly identical before 2005, why the figure is not just displayed in the period 2005-2021? As it stands, there just is a lot of wasted space and the differences are not really visible as the informative part of the time series just takes a small space in the panels

Done. It now shows data from 2005 through 2021. Thanks.

Fig 16: the differences between v2.0 and 2.2 are very difficult to see. As for Fig 15, there is not much interest to see the time series before 2005 as these are identical. In addition, many statistical parameters of the comparisons can be computed (mean bias, RMSE, correlation coefficient, ...) that could help interpreting the differences between v2.0 and 2.2.

We now include percent differences in the text and point the differences in the figure (lines 473-481) .

References

Legras, B., Duchamp, C., Sellitto, P., Podglajen, A., Carboni, E., Siddans, R., Grooß, J.-U., Khaykin, S., and Ploeger, F.: The evolution and dynamics of the Hunga Tonga–Hunga Ha'apai sulfate aerosol plume in the stratosphere, Atmospheric Chemistry and Physics, 22, 14 957–14 970, https://doi.org/10.5194/acp-22-14957-2022, 2022.

SPARC: Assessment of Stratospheric Aerosol Properties (ASAP), Tech. rep., SPARC Report,WCRP-124,WMO/TD-No. 1295,SPARC Report No. 4, 348 pp., 2006.