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Comment on amt-2022-222

Anonymous Referee #2

Referee comment on "SAGE III/ISS aerosol/cloud categorization and its impact on GloSSAC" by Mahesh Kovilakam et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-222-RC2, 2022

Review of the paper "SAGE III/ISS aerosol/cloud categorization and its impact on GloSSAC"

Dear Editor, dear Authors,

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)

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

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.com/article/rs-1562573/v1 or https://egusphere.copernicus.org/preprints/2022/egusphere-2022-517/)

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

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

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

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

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)

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

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

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.

Please add labels a) and b) in Fig 1

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.

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?

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

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

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)

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

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

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

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.

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

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.

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

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

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?

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

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?

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

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.

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

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

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?

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.

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

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)?

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?

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

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?

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

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

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

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.