Response to Reviewer #1

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.

Radiative forcing from stratospheric aerosols is a major driver of climate variability. Building robust and consensual observational records of stratospheric aerosol observations is thus critical to understand past climate changes and to predicting future climate. For the satellite era, this task is made particularly challenging by the fact that different periods are covered by different instruments with different measurement technique, so that producing an homogeneous and consistent continuous record is difficult. To address this challenges, the Global Space-based Stratospheric Aerosol Climatology (GloSSAC) dataset v1.0 was produced (Thomason et al., ESSD 2018) and has since become the reference dataset of stratospheric aerosol observations. In particular, it has been used as input to historical experiments of Phase 6 of the Climate Model Intercomparison Project. This manuscript presents the latest version v2.0 of the GloSSAC dataset which follows the original version v1.0 and its update v1.1. Many improvements have been done since version v1.0 and in particular: i) the dataset is now extended to year 2018; ii) an error in the processing of data from the CLAES instrument has been corrected; iii) the processing of data from the OSIRIS and CALIPSO instrument has been improved, in particular thanks to data from SAGE III having become recently available and overlapping with the OSIRIS/CALIPSO observations. Given these major improvements and the importance of the GloSSAC dataset, this manuscript will be an extremely valuable contribution and I recommend its publication following minor revisions. The manuscript describes and justifies in great details the updates to GloSSAC, and generally reads well. There are a few places where I think the figures or text could be improved (cf minor comments below). My main comment is that even though technical changes since v1.0 are well described, the paper lacks figure(s) and discussion extensively comparing v2.0 to v1.1 and v1.0 (cf specific comment below), which seems important given this paper provides an update to an existing dataset. I think this would represent an important improvement to the paper, and that it wouldn?t require much work from the authors which is why I recommend minor revisions.

Specific comments

1) If I?m not mistaken Figure 15 is the only figure showing the final differences between two versions of the GloSSAC (v2.0 and v1.1) and only do so for the 2002-2016 period. I don?t think GloSSAC updates have been documented in a peer-reviewed literature since GloSSAC 1.0 release (Thomason et al. 2018)? It thus seems very

important to show the differences between all three versions (1.0, 1.1 and 2.0) and for the full period in common (1979-2016). From my own analyzes of GloSSAC version it seems that each version is different in the post-Pinatubo period, which is one of the period with the most research on stratospheric aerosol forcing. I thus really think that figure 15 should be extended to include all versions/the full common period. In addition to showing contour plot of SAOD as a function of latitude and time, I also think it would be very valuable to compare global mean SAOD time series between the three versions as this is the canonical metric for stratospheric aerosol impact on climate. Section 5 of the manuscript should then be extended to discuss these differences in greater details. Getting an idea of SAOD differences among GloSSAC versions will likely be a major expectation of GloSSAC users from this paper, so I strongly encourage the authors to address this comment.

Figure 15 is now extended to the entire record of GloSSAC and now shows AOD plots of v 1.1 and v 2.0. We also included another figure (Figure 16) that show v 1.0 and v 2.0 and their differences. Additional plots include a global SAOD plot (Figure 18) with labels of volcanic eruptions on Figures, and we discuss these changes in section 5.0.

2) In line with comment 1 above, I think that the abstract should end with a few sentences summarizing the main changes between versions in terms of SAOD. The abstract is very focused on the technical changes in GloSSAC 2.0 which is of course appropriate for an ESSD paper, but it is currently hard for a scientist with little expertise in remote sensing to get a sense of the impacts of these changes on the GloSSAC product from the current abstract.

Thanks for this suggestion. While we appreciate the reviewer for this suggestion, due to the word limit constraints for the abstract, we are not able to include this into the abstract. We now address this in the conclusion section of the paper (lines 3:8, page 17).

3) This comment is very much a suggestion. Figure 1 is an excellent introduction figure to the manuscript. I was wondering if it could be complemented (or if you could add a new figure) showing a timeline of some of the main features/limitations/challeng in the GloSSAC record, such as what type of instrument is used (e.g. solar occultation or other), the resolution/frequency of measurements (e.g. global daily coverage with OSIRIS/CALIOP vs global monthly coverage with SAGE instruments), assumptions required (e.g. periods in which an assumption on size distribution is required), etc... Such a figure would enable people with limited expertise in remote sensing to understand in one glance some of the main features of the GloSSAC dataset before using it, which I believe would be very valuable.

We have added a paragraph (line 25:30, page 2 and line 1:5, page 3) about the main features, limitations and challenges. The new paragraph now reads as:

”Figure 1 depicts the measurements that are currently used for constructing GloSSAC data. While Thomason et al. (2018) discusses about the measurements that have been used in GloSSAC v 1.0 dataset in detail, some of the main features of entire GloSSAC v2.0 dataset including various space based measurements, their limitations and some challenges are worth mentioning here. We divide the entire dataset into three periods based on the measurements used. The first period being the pre-SAGE II period (January 1979- September 1984), followed by SAGE II period (October 1984 - August 2005) , post-SAGE II period (September 2005- May 2017), and SAGE III/ISS period (June 2017-present). Pre-SAGE II period data mostly consists of data from solar occultation measurements such as SAM II , SAGE and some surface based Lidar measurements (Thomason et al., 2018). For SAGE II period, the measurements are dominated by solar occultation measurements that provide multi-wavelength measurements for size information. For the post-SAGE II era, we are limited to single wavelength measurements from OSIRIS and/or CALIPSO. While OSIRIS and CALIPSO continue to make daily global measurements with a less direct measurement of aerosol extinction coefficient that requires further assumption of particle size, additional direct measurements of aerosol extinction coefficient from SAGE III/ISS are now available that provides a roughly monthly coverage of multi-wavelength measurements since June 2017. ”

page 1 line 1: I stumble a bit on the first sentence of the abstract. In addition to being a bit cumbersome, it introduces what a stratospheric aerosol dataset should do, but the second sentence does not follow on the dataset so it?s confusing.

We rewrote the sentence to have the continuity.

page 1 line 7: I don?t think Zanchettin et al. (2016) is the adequate reference for CMIP6 unless you are talking specifically about VolMIP.

Replaced with Eyring et al. (2016).

Page 2 line 4-5: ”can impact climate on scales from the subtle [...] to the more profound [...]”: I find this wording vague and confusing: are you talking about the timescales of the impacts? Their magnitude?

We changed it to ”can impact climate on magnitudes from ....

I would be more specific and clear about the difference in these modelling approaches, e.g. ”Some of these modelling studies directly use observations of stratospheric aerosol optical properties as input, whereas other use observations of SO2 as input and interactively simulate stratospheric aerosol life cycle”

We specifically state that in the following paragraph (lines 10-15, page 2).

page 2, line 6-14: You focus on GCMs study as a motivation but I feel like you could

include other examples that have used the GloSSAC dataset to make important contributions. In a purely observational study, Stocker et al (2019, https://agupubs.onlinelibrary quantify the temperature footprint of 21st century eruptions using GloSSAC which in turns enable to better quantify temperature trends related to anthropogenic forcing. Aubry et al. (2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JD03130 used GloSSAC to calibrate a box model of volcanic forcing. Such box model is the typical tool used to derive SAOD/forcing time series from ice-core records so that?s an important application of dataset like GloSSAC. These are just suggestions and application to GCM study is of course a major motivation, but I think it?s nice to highlight that applications of the GloSSAC dataset go beyond that.

Thanks for the suggestion. We now added a sentence on Stocker et al., 2019 (line 15, page 2).

page 2, line 14: I am not too sure where to put this comment, but it feels like the paper under review by Rieger and co-authors (https://gmd.copernicus.org/preprints/gmd-2019-381/) is very relevant to your paper and should be mentioned if its publication status allows it. Maybe instead of in the introduction it would fit better in your discussion of an extended figure 15 (cf specific comments) showing the differences in SAOD between v1.0, v1.1 and v2.0 for 1979-2016.

We have added this reference in SAOD discussion section.

page 2, line 15: it?s a bit confusing as you say "mostly unchanged" followed by "significant improvements" and "major version change". These statements sound contradicting and you may want to reformulate.

The data sources are unchanged. However the usage of the data (with some updated versions) and the analysis approach resulted in significant improvements. We have revised the sentence now and it reads as:

"While data sources are mostly unchanged from earlier versions, there are significant improvements in the use of OSIRIS and CALIPSO data with inclusion of SAGE III/ISS data for the first time."

page 3, line 13-33: this feels like a very detailed and technical discussion of the changes you made for an introduction. I feel like this content should be in section 2 instead?

We have now moved those lines to section 2.

page 4, line 7: there and hereafter, I suggest you provide date in parenthesis when you refer to XXX instrument period. It will be very helpful for readers not perfectly familiar with the period spanned by different instrument.

Done.

page 4, lines 10-18: if I?m not mistaken no figure illustrate these results? The Pinatubo period is of course of utmost interest to climate modelers so it feels like there should be a figure accompanying this paragraph? (although if you extend Figure 15 according to my suggestions that would illustrate this paragraph well)

We now discuss this in section 5.0.

page 5, line 14-16: this is an important comment. Again it would be nice to specify dates in parenthesis for user who are not familiar with SAGE missions dates. I know that the reader could just look at Figure 1 but it would facilitate the reading if you also provide such dates directly.

Done.

General comment on section 2: I enjoyed this section and although I don?t have the expertise to understand all details, you clearly highlight the differences in methods/ limitation/challenges of different periods of the GloSSAC record. A figure with a timeline showing this features would be a very neat addition (see specific comment 3 for a more detailed suggestion)

We have now added a paragraph in the beginning of section 2 and discuss Figure 1 and the instruments used in detail.

page 6 line 7: maybe you could give an idea of the uncertainty on the 50 sr value?

This number comes from OPC and lidar measurements study Jäger and Deshler (2002, 2003) that is based on specific size distributions. It could also be tested theoretically using Mie theory with an assumption of size distributions. Again, please note that lidar ratio strongly depends on size distribution. So, specifically using a constant number for extinction-to-backscatter ratio has limitations. For this study, we however used a pseudo-extinction to backscatter ratios (defined as "Scale Factor" in the manuscript) and its related uncertainties in Figure 9. Using a value of 53 sr seems reasonable between 30S and 30N from 18 km and above based on Figure 9a and the relative standard deviation based on our method is mostly within $\pm$ 20% (Figure 9b).

Also, Vernier et al. (2011) reported the variability in lidar ratios across various latitude bands, showing lidar ratios vary with latitude and altitude.

page 6, lines 25-34: Is this cloud-clearing method more challenging to apply when there is a very large volcanic eruption (e.g. Pinatubo like or larger)? I?m just wondering whether the IQR would be larger following a large eruption.

There are limitations on this method as well, especially when we use this in the vicinity of tropopause with large eruptions. It is particularly challenging to differentiate between clouds and aerosols near tropopause during and following volcanic eruptions. We are currently working on developing a cloud screening algorithm for SAGE III/ISS in particular which could be incorporated in a future version of GloSSAC.

Page 8 line 4-5: does the Raikoke 2019 eruption provide a good test for this hypothesis?

The technique we use in here has limitations when it comes to periods of volcanic activity particularly due to change in size distributions and for the period 2005-2017, we are limited to using single wavelength measurement that lacks information about aerosol sizes. Yes, multiwavelengths measurements have been available from SAGE III/ISS since June 2017, which help us understand better as to how aerosol size changes during and following a volcanic eruption. For the Raikoke eruption, a detailed study using SAGE III/ISS measurements is in progress.

Page 9 line 2-3: why do you say "though probably not at 756nm"? Doesn?t figure 5b show a strong high bias in the lower stratosphere and low bias in the tropical midstratosphere?

We are aware of a low bias at 521 nm channel of SAGE III/ISS. We, however do not observe any such changes in other aerosol measurement wavelengths such as 756 nm. In addition, for Figure 5a, there is an additional complexity that the OSIRIS 525 nm extinction coefficient is computed from a constant Angstrom exponent of 2.33 while 750 nm extinction coefficient is the primary reported wavelength for OSIRIS and Figure 5b is a straightforward comparison with SAGE III/ISS. Therefore, comparing the differences between Figure 5a and 5b is not a direct one.

Page 9 line 14: I can?t find the definition of lambda

Lambda represents wavelength. The sentence now reads as : "$(\frac{\lambda_{525}}{\lambda_{750}})$ represents ratio of wavelengths at 525 and 750 nm."

section 3-4: these sections were generally clear and provide a good overview of differences between instruments and data processing/conforming procedures employed by the authors.

Thanks.

page 13 line 33: So I guess the tropopause height is a climatology as in Thomason et al. (2018)? I think it would be useful to remind here the period used to derive this climatology, as well as the reanalysis used (MERRA if I remember correctly). Additionally, the tropopause height is quite variable at highlatitude and is increasing in the tropics as a consequence of anthropogenic forcing. Given these two points, I am wondering why you are using a climatology instead

of the reanalysis data directly? Differences would likely be small but it would be a bit more rigorous approach? As an example figure S1 in Aubry et al (2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JD031303) show GloS-SAC SAOD at v1.0 and v1.1 using the MERRA climatology and the NCEP-NCAR reanalysis for tropopause height with some interesting differences. I haven?t analyzed these differences further, but the SAOD I get are smaller for the 21st century which could be in part due to tropopause height increase?

The tropopause climatology is derived from MERRA for the SAGE II lifetime as stated in Thomason et al. (2018) paper. We continue to use that in here as well. Yes, we do agree that the tropopause is variable with latitude and time of the year and using a climatology may not be an accurate representation. We will definitely keep this suggestion in mind and make use of a variable tropopause in a future version of GloSSAC.

Section 5: please see my specific comment 1, but I really believe that this section would be more complete if you: -show differences between all 3 versions of GloSSAC -show differences for the full time period shared between the 3 version (1979-2016) -show global mean SAOD time series in addition to SAOD contour plot -extend text in section 5 to include discussion of the above

We have now revised this section to include the entire record of SAOD.

page 14 lines 22-23: it feels like you could add a few references to support this statement? There have been multiple modelling studies showing that post- 2005 SAOD enhancement can be largely explained by SO2 emissions from explosive volcanic eruptions as well as wildfire for some of the recent years. See e.g. Schmidt et al. (2018, https://doi.org/10.1029/2018JD028776), Peterson et al. (2018, https://www.nature.com/a 018-0039-3) or Aubry et al. (2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.102

Done. The sentence now reads as:

"As with v1.0, we cannot exclude the possibility that on-going volcanic activity plays a dominate role in the apparent enhancement after 2005. This possibility is bolstered by noting that optical depths shown in Figures 15b and 15e approach those observed in 2004, 2013, early 2014 during a lull in a decade of repeated minor volcanic stratospheric enhancements. We also note that several recent modeling studies (e.g. Schmidt et al., 2018; Aubry et al., 2020) using sulfur dioxide emissions in aerosol-climate models, have reported an enhancement in SAOD for the post-2005 time period."

page 16 line 9: avoid repetition of "inferred 1020nm extinction" twice in the same sentence. Otherwise, I think this is a nice paragraph to close the conclusion section!

Done.

Section 6: the conclusions are ok but overall I feel like you could be a bit more succinct on some of the technical details, and that you should add a few sentence commenting on major differences in SAOD in GloSSAC 2.0 compared to 1.0/1.1. This really seems critical as the aim of the paper is to present the newest version of the GloSSAC dataset, so in general it really feels like you should do more to compare SAOD in the different versions. This would likely be the most expected results from this paper for users of the GloSSAC product.

We now briefly discuss changes in SAOD occurred in version 2.0 compared to previous versions.

Table 1 caption: replace "since 2002" by "over 2002-2018" as this table doesn?t include 2019/2020 eruptions (e.g. Raikoke and Ulawun in 2019, Taal in 2020)

We have now revised the table to include all events since 1979.

Figure 7: I like this figure a lot. Maybe you could have a SI table specifying the dates of "after"/"before" (or add these dates in the caption directly)

We have added dates to each event in the figure. There is another paper in review in ACP (https://doi.org/10.5194/acp-2020-480) that discusses this method in detail.

Figures 10/12/17: given the differences between datasets are relatively small, I?m wondering if you should not use lines instead of markers? The marker sometime overlaps a lot making it harder to distinguish any systematic difference.

Done. Replaced makers with lines.

Figures 13-16: on most of these figures, the density of contour labels is too high and prevent the reader to see clearly the data on the figure (this is made worse by the white rectangles in which each label is inserted). Consider removing the labels altogether or at least reducing their density/removing white rectangles.

Done. Removed labels and contours from all these plots and a new color scale has been used.

Figure 16: it looks like this figure has been stretched horizontally?

This has been fixed. It was in fact related to the adjustment of the figure size that was made in the latex version.

References

Aubry, T. J., Toohey, M., Marshall, L., Schmidt, A., and Jellinek, A. M.: A New Volcanic Stratospheric Sulfate Aerosol Forcing Emulator (EVA_H): Comparison With Interactive Stratospheric Aerosol Models, Journal of Geophysical Research: Atmospheres, 125, e2019JD031 303, https://doi.org/10.1029/2019JD031303, e2019JD031303 10.1029/2019JD031303, 2020.

Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, Geoscientific Model Development, 9, 1937–1958, https://doi.org/10.5194/gmd-9-1937-2016, URL `https://gmd.copernicus.org/articles/9/1937/2016/`, 2016.

Jäger, H. and Deshler, T.: Lidar backscatter to extinction, mass and area conversions for stratospheric aerosols based on midlatitude balloonborne size distribution measurements, Geophys. Res. Lett., 29, 35–1–35–4, https://doi.org/10.1029/2002GL015609, 1929, 2002.

Jäger, H. and Deshler, T.: Correction to "Lidar backscatter to extinction, mass and area conversions for stratospheric aerosols based on midlatitude balloonborne size distribution measurements", Geophys. Res. Lett., 30, n/a–n/a, https://doi.org/10.1029/2003GL017189, 1382, 2003.

Schmidt, A., Mills, M. J., Ghan, S., Gregory, J. M., Allan, R. P., Andrews, T., Bardeen, C. G., Conley, A., Forster, P. M., Gettelman, A., Portmann, R. W., Solomon, S., and Toon, O. B.: Volcanic Radiative Forcing From 1979 to 2015, Journal of Geophysical Research: Atmospheres, 123, 12 491–12 508, https://doi.org/10.1029/2018JD028776, 2018.

Thomason, L. W., Ernest, N., Millán, L., Rieger, L., Bourassa, A., Vernier, J.-P., Manney, G., Luo, B., Arfeuille, F., and Peter, T.: A global space-based stratospheric aerosol climatology: 1979–2016, Earth System Science Data, 10, 469–492, https://doi.org/10.5194/essd-10-469-2018, 2018.

Vernier, J.-P., Thomason, L., Pommereau, J.-P., Bourassa, A., Pelon, J., Garnier, A., Hauchecorne, A., Blanot, L., Trepte, C., Degenstein, D., et al.: Major influence of tropical volcanic eruptions on the stratospheric aerosol layer during the last decade, Geophys. Res. Lett., 38, 2011.