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Earth System Science Data Discussions

Interactive comment on "A Global Space-based Stratospheric Aerosol Climatology (Version 2.0): 1979–2018" *by* Mahesh Kovilakam et al.

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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.

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.

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/challenges 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.

Minor comments

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.

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.

page 1 line 14: reformulate "noted in v1.0 as noted in"

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?

page 2 line 7-9: 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"

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.wiley.com/doi/full/10.1029/2019GL084396) 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/10.1029/2019JD031303) 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.

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.

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.

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?

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.

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)

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.

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)

page 6 line 5-6: "another potential source of bias is repeated twice in the same sen-

tence; improve wording

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

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.

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

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?

Page 9 line 14: I can't find the definition of lambda

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.

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 high-latitude 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 GloSSAC 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?

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

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/articles/s41612-018-0039-3) or Aubry et al. (2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JD031303).

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!

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.

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)

Figure 2: I initially got confused by how noisy these pdfs are. Even though the figure looks fine after consideration, it may be clearer if you use larger bins?

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)

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.

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.

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

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