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Interactive comment on "Quantifying CanESM5 and EAMv1 sensitivities to volcanic forcing for the CMIP6 historical experiment" *by* Landon A. Rieger et al.

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Radiative forcing from stratospheric volcanic sulfate aerosol is a major cause of climate variability and a key forcing in CMIP6 historical experiment. For the satellite era, the GloSSAC dataset version 1.0 was used by all CMIP6 models to prescribe stratospheric aerosol data. However, new versions of this dataset have recently been released. The aim of the manuscript is to test whether using version 1.1 of the GloSSAC dataset instead of version 1.0 as in CMIP6 significantly affects the radiative forcing and climate response to the Mount Pinatubo 1991 eruption. Using two different Earth System Models, the authors show that differences in radiative forcing, tropospheric tempera-

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ture, precipitation, ocean heat content and ENSO responses caused by the GloSSAC update are generally negligible compared to natural variability, although there are some important differences in heating rate and stratospheric temperature responses.

The manuscript is generally solid, clear, rigorous and very pleasant to read. It adresses an important question for the CMIP6 communauty and will be a very valuable contribution. I thus recomment the manuscript for publication after moderate or minor revisions. My main comment, further detailed below, is that the paper never mention the version 2.0 of the GloSSAC dataset, which I believe has stronger differences with version 1.0 than the version 1.1 tested by the authors. I think the authors should at least discuss differences between version 1.1 and 2.0, and ideally complement their results with some experiments using version 2.0.

Major comment:

The GloSSAC dataset version 2.0 was advertised at the 2019 American Geophysical Union Fall Meeting (Thomason et al. 2019, see reference list at the end) which I guess some of the authors are aware about, but this newer version is never mentioned in the manuscript. Given the main objective of the manuscript, I think the fact that a newer version exists should at the very least be discussed? I attach a plot briefly comparing global mean SAOD in v1.0, v1.1 and v2.0, which is a modified version of Figure S1 in Aubry et al. (2020) (see reference list at the end). For the Pinatubo period, v2.0 has larger SAOD than v1.0, whereas v1.1 has smaller SAOD than v1.0. Differences between v2.0 and v1.0 also tend to be more important than those between v1.1 and v1.0. Using v2.0 instead of v1.1 would thus likely change many of the results presented in this study, even though I would expect the conclusions that using any of these GloSSAC versions cause changes in the Pinatubo climate response that are small compared to the natural variability.

I recommend that the authors at least make the reader aware that a version 2.0 of the GloSSAC dataset exists and discuss differences between version 1.1 and 2.0. Adding

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a SI figure similar to the nice figure 1 but showing GloSSAC v1.0, v1.1 and v2.0 would be useful. In support of this discussion, I think that the authors should reproduce figure 3 and 4 using GloSSAC v2.0. I believe this should have a relatively low computational cost given that these are 5-year AMIP simulation(s)? It would provide a first test of the differences caused by using the newest GloSSAC version in terms of radiative forcing. I believe it would additionally be a very useful contribution to further quantifying how uncertainties in stratospheric aerosol datasets - which are very challenging to build - translate in terms of radiative forcing uncertainty. Repeating the other simulations (e.g. the fully coupled CanESM simulations) with GloSSAC 2.0 would be fantastic if computational cost allows it.

I understand that the time of creation of the GloSSAC v2.0 version likely was very close to the time at which simulations for this study were conducted, and I am also not entirely sure whether the v2.0 version has been officially released (the dataset webpage still seems to mention v1.1: https://eosweb.larc.nasa.gov/project/glossac/glossac). However, given that this newer update has been advertised to the scientific community and is available (at least upon request), I believe that the authors should at the very least make the reader aware of v2.0 and discuss the differences with v1.1.

Other comments:

1) The authors mention that there is no apparent difference in El Nino Southern Oscillation (ENSO) states in the 2 years following the Pinatubo eruption, but I don't think the North Atlantic Oscillation (NAO) response is mentioned anywhere? Given that changes in stratospheric temperature response are significant and much larger in the tropics, I think it would be very valuable to show/mention whether using the new aerosol dataset affects the response of : i) the meridional temperature gradient in the stratosphere; ii) the polar vortex strength (during winter) and iii) the winter NAO phase.

2) I find the manuscript very clear, concise and pleasant to read except for the presentation of the experimental design (and to a lesser extent, for the models). The reader

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discovers along the way which simulation set-up was used for which parts, with often a lack of details. For example, section 3.1 suggests that CanESM5 was used in fully-coupled mode, but then in section 4.1 it is used in AMIP mode and it is not clear how many simulations were conducted. In section 4.2, the coupled version is used and the number of ensemble member is specified, but it is not clear how initial conditions were sampled. Overall, I would prefer to see all details of experimental design in a section 3.3 clearly presenting the model setup used (for both CanESM and EAMv1) for different diagnostic, the number of ensemble members, and how initial conditions were sampled. (a table could be useful here). It would also be nice to harmonize a bit the model description, e.g. describe the model resolution in similar units (degree vs km) so that the reader can easily compare them, and give information about ability to simulate QBO for both model in their respective sections.

Specific comments:

Page 1, line 14 and 18: I believe the abstract would read a bit better if you directly mentioned that you used two different models, and then the main results from the two models.

Page 1, line 22: replace "leading to a cooling effect" by "leading to a surface cooling effect"

Page 2, line 1: I find the formulation "multiplying the impact on climate" a bit confusing; maybe replace by something like "which in turn strenghtens this surface cooling"

Page 2, line 2: as you give a range of radiative forcing you could give a range on the injected sulfur mass, which is still a major source of uncertainty.

Page 2, line 5: I would avoid expressions like "equally impressive"; maybe replace by "There was also a significant impact on oceans, "?

Page 2, line 8: More recent references you may consider to add are Stocker et al. (2019) (they use GloSSAC) and Schmidt et al. (2018)

Page 2, section 2 title: "The Stratospheric Aerosol Dataset" reads a bit funny; maybe say "The CMIP6 Stratospheric Aerosol Dataset" ?

Page 3, line 7: I think it would be neat to briefly describe what kind of error it was, in one or two sentences?

Page 3, Figure 1: this relates to my main comment, but I think having a similar figure for GloSSAC v2.0 would be nice, and I really think you have to mention and discuss this newer update.

Page 4, line 4: do you mean optical thickness? If so please clarify

Page 5, sections 3.1 and 3.2: could you give the rough vertical resolution at the altitude of the Pinatubo plume for both models?

Page 5, lines 8 and 17: to ease the model comparison, could you give the horizontal resolution either in degree or km or both?

Page 5, line 20: could you clarify whether the version you use includes this modified parameterization?

Page 5, section 3.1: could you provide in this section some information on the model capability to simulate the QBO, like you do for EAMv1 in section 3.2? You could then remove it from Page 9 line 15.

Section 4.1: it is very hard to understand which model(s) was used to conduct simulation to diagnose radiative forcing (I understand it's CanESM5 from the caption of Figure 3?). Please clarify. I think having a section 3.3 with summary of experimental design would greatly help as highlighted in one of my main comments.

Page 6, line 1: Section 3.1 gives the impression CanESM5 is used in fully-coupled mode; I'd prefer if you clarified earlier that you use it in AMIP mode to quantify changes in radiative forcing. Similarly, you say "simulations": how many? How were initial conditions sampled?

Page 6, line 5: Would shorwave/longwave be a more standard terminology than solar/thermal?

Page 6, Figure 3: even though the point of the paper is not to compare the model with observations, I think it would be neat to show some on this figure (e.g. from ERBE)

Page 7, line 3-5: just a personal preference but I think this should be in the figure caption only, not in the main text.

Page 7, line 5-9: given the reduction in heating rate is mostly in the tropic, an immediate question coming to mind whether it affects the meridional temperature gradient, winter polar vortex strength, and winter NAO response?

Page 7, Figure 4: clarify in the legend that these results are from CanESM?

Page 8, line 1-9: I wish it was clear before that coupled simulation were used to diagnose climate response and AMIP simulations for radiative forcing. Here you specify ensemble size, and you are clear about which model you use (in contrast with section 4.1), but I think you should briefly mentioned how initial conditions were sampled. (and again instead of mentioning it here I would rather have a section 3.3 devoted to the experimental design)

Page 9, line 1-2: It's nice that you show comparison with observations here.

Page 9, lines 11-13: Although the changes highlighted are small, they slightly improve consistency with observations? I think it's worth highlighting explicitely? That being said I would expect a stronger temperature response if you used the version 2.0 of GloSSAC... I really think this should be discussed.

Page 10, line 5-6: You may consider including a more recent citation for ENSO response to volcanism such as Khodri et al. (2017)

Page 10, line 6: changes in volcanic forcing before the eruption are only from January 1991 onwards, and are of magnitude smaller than 0.01 in terms of SAOD, is that cor-

rect? I find this clear shift to a La-Nina like state quite impressive given the really small changes applied for just 5 months.

Page 11, line 1-4: It's nice to comment on ENSO but given the changes in tropical stratospheric temperature you find, I am really curious to know if the winter NAO response is affected, or at least the winter polar vortex.

Page 12: Conclusions are clear and concise.

References

Thomason et al. (2019), The Global Space-based Stratospheric Aerosol Climatology: Features of v2.0, https://agu.confex.com/agu/fm19/meetingapp.cgi/Paper/502030

Aubry, T. J., Toohey, M., Marshall, L., Schmidt, A., & Jellinek, A. M. (2020). A new volcanic stratospheric sulfate aerosol forcing emulator (EVA_H): Comparison with interactive stratospheric aerosol models. Journal of Geophysical Research: Atmospheres, 125, e2019JD031303. https://doi.org/10.1029/2019JD031303

Stocker, M., Ladstädter, F., Wilhelmsen, H., & Steiner, A. K. (2019). Quantifying stratospheric temperature signals and climate imprints from postâĂŘ2000 volcanic eruptions. Geophysical Research Letters, 46, 12486– 12494. https://doi.org/10.1029/2019GL084396

Schmidt, A., Mills, M. J., Ghan, S., Gregory, J. M., Allan, R. P., Andrews, T., et al. (2018). Volcanic radiative forcing from 1979 to 2015. Journal of Geophysical Research: Atmospheres, 123, 12,491– 12,508. https://doi.org/10.1029/2018JD028776

Khodri, M., Izumo, T., Vialard, J. et al. Tropical explosive volcanic eruptions can trigger El Niño by cooling tropical Africa. Nat Commun 8, 778 (2017). https://doi.org/10.1038/s41467-017-00755-6

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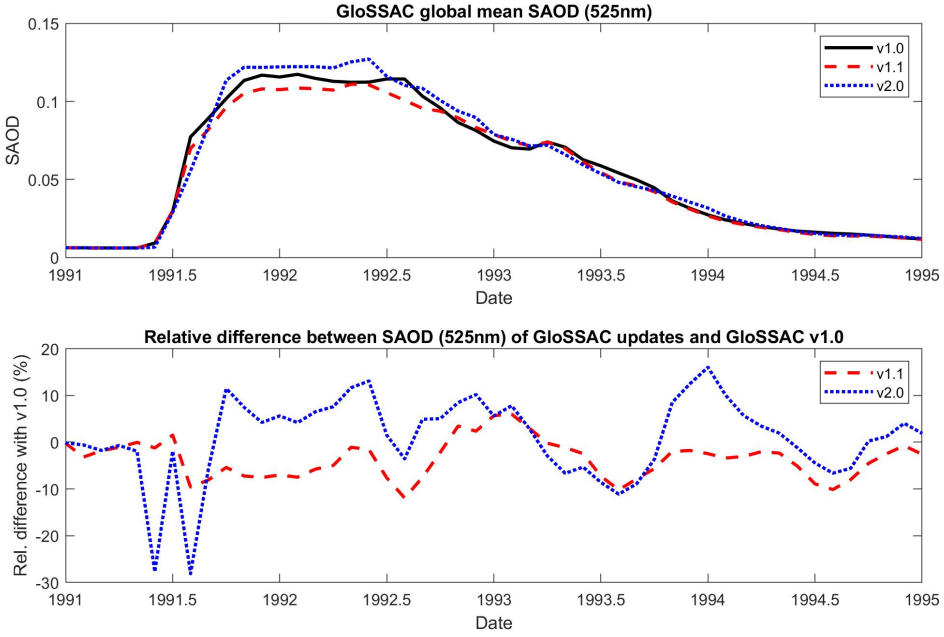

Fig. 1. Post-Pinatubo SAOD in GloSSAC 1.0, 1.1 and 2.0

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