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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Changes in stratospheric aerosol extinction coefficient after the 2018 Ambae eruption as seen by OMPS-LP and ECHAM5-HAM" *by* Elizaveta Malinina et al.

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We are very grateful to the reviewers, Daniele Visioni and Anonymous Referee #2, as well as for Pasquale Sellitto for providing their insightful and constructive comments on the manuscript. We appreciate input and think that the paper benefited from the suggested changes.

Below, the answers to the reviewers' questions and comments are provided. In order to make the text more distinguishable, we highlighted the reviewers' and Dr. Sellitto's comments in **bold** and authors' answers in blue font.

Dear Authors,
thank you for your paper, which I found very interesting and an enjoyable reading.

Dear Pasquale, thank you very much for a careful reading of the paper and providing your comments.

I would strongly suggest to compare your aerosol extinction (and AOD) as well as your radiative forcing (RF) estimations with those previously published by Kloss et al., JGR,2020 (that you cite in your manuscript).

Please bear in mind that our paper is focused on a comparison of the measurement and modelling results and a comparison of the aerosol extinction with Kloss et al. (2020) would displace the focus and make the paper more lengthy and difficult to read. We would like to highlight that the only aerosol extinction result, which could be compared without making additional assumptions needed to convert to other wavelengths, is the averaged SAGE III/ISS profile at 869 nm, which is shown in Fig. 8, and the corresponding AOT. As our paper already presents a comparison with SAGE III/ISS data for the period of interest (Fig. 1 of our paper) and the agreement in tropics is found to be better than 25% we do not think a comparison of the averaged extinction profiles with those from Kloss et al. (2020) will bring any additional value to our research topic.

For the RF estimations, in particular, Kloss et al. have used a detailed radiative transfer modelling, which is expected to bring more precise estimations than the rough parameterisation of your Eq. 1.

One of the topics, discussed in our paper, is how well the simple approximation provided by Eq. 1 performs in comparison with the internal flux calculation of the ECHAM model. The latter is expected to be much more precise. The question, what is more precise, a radiative module of a GCM or a standard radiative transfer model, is open for now and cannot be answered in the framework of our study. Radiative transfer calculations in a GCM are based on strong approximations but the employed approach is optimized for broad-band flux calculations. A standard radiative transfer model is optimized for precise calculations of spectrally resolved radiances but is not necessarily the best choice if a small difference of two large values (fluxes) needs to be calculated as it is done in the case of the radiative forcing. Furthermore, the influence of the vertical and angular grids, Fourier harmonics and possibly atmospheric sphericity needs to be carefully investigated before making any conclusions.

Please also specify that the RF of Eq. 1 is the top of the atmosphere RF or otherwise clarify at which altitude it is estimated.

The RF is calculated for the top of the atmosphere. We will add this information to the revised manuscript.

Kloss et al. included also several hypotheses on the spectral variability of the extinction, the absorption properties of the particles and the angular distribution of the scattered radiation, and obtained quite different results if compared to yours. This should certainly be discussed in your manuscript. Please add such comparisons to your revised manuscript, that would be a great added value to your interesting work.

ECHAM features a built-in aerosol module which models aerosol micro-physical processes to calculate the aerosol composition and size distribution. This information is used then to calculate fluxes. It is not obvious how to compare an aerosol parameterization as represented by the model with assumptions made by Kloss et al. especially with respect to an outdated parameterization of the aerosol scattering with an asymmetry parameter. On the contrary, the values of the radiative forcings can be compared and the reasons for their disagreement will be discussed in the revised manuscript. In our opinion, the main reason for disagreement is the way the non-Ambae signal has been subtracted. By subtracting the radiative forcing just before the second eruption we ensure that only the Ambae contribution is accounted for. On the contrary, Kloss et al. use some "background aerosol" scenario, which is not clearly described in the paper, to remove other contributions than that of Ambae. We are quite sure that the state of the stratosphere just before the Ambae eruption was far from background (in the common sense of this term), e.g. because of a residual aerosol load from Canadian wildfires 2017. Another issue can be related to the definition of the tropopause. Although it is not stated clearly, from the remark "(full profiles, for the whole stratosphere)" in Kloss et al., we guess that only the stratospheric part of the aerosol profile is used to calculate the radiative forcing. This is the same approach as we used. If our guess is wrong and Kloss et al. use the tropospheric part of the profiles as well, this might be a reason for the disagreement. Otherwise, there might still be a difference in a definition of the tropopause. Unfortunately, Kloss et al. do not state how their tropopause was defined.

If the Editor thinks it useful, I could review the revised version of the manuscript, in particular for what concerns this comparison and the RF estimations themselves.

If you are interested in understanding possible reasons for the disagreement and evaluation of different methods, the only possible way to do it, would be a dedicated joint investigation which is far beyond the scope of our study. We see no possibility to draw any well justified conclusions based only on the information published by Kloss et al. (2020). If you still have an interest in a joint study, please contact the corresponding author (Alexei Rozanov) of the manuscript.

My best regards,
Pasquale Sellitto

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