We thank the reviewer for making very useful suggestions to improve the paper. Our point-by-point responses to the reviewers' comments and corresponding changes are detailed below in blue text, and the changes are shown in the version of the manuscript with track changes.

Comment on acp-2021-654

Anonymous Referee #3

Referee comment on "Radiative forcing by volcanic eruptions since 1990, calculated with a chemistry-climate model and a new emission inventory based on vertically resolved satellite measurements" by Jennifer Schallock et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-654-RC3, 2021

Review of "Radiative forcing by volcanic eruptions since 1990, calculated with a chemistry-climate model and a new emission inventory based on vertically resolved satellite measurements by Jennifer Schallock et al.

Using various (occultation and limb based) satellite instruments, with vertical SO2profiles from different satellite instruments and chemistry climate simulations, this study characterizes the influence of stratospheric volcanic aerosols for the period between 1990 and 2019. The results show that small but relatively frequently eruptions contribute to the stratospheric aerosol layer and could cause a global radiative forcing in the order of−0.1 Wm−2 at the tropopause. In specific, the objective of this study was to generate a detailed volcanic sulfur emission inventory, to improve the EMAC model simulations of the global stratospheric aerosol and sulfate burden, and to compute the volcano-induced radiative forcing through validation with satellite data.

Honestly, the paper keeps me a bit loss, as I am not sure if it is a more scientifically or more technically oriented paper. The scientific objective is not clear to me in particular the added value to the recent literature. I am wondering if the paper would not better fit in Earth System Science data (ESSD, https://www.earth-system-science-data.net/) or in Geoscientific Model Development (GMD, https://www.geoscientific-model-development.net/). The topic of the paper is in general very suitable for ACP but the paper needs major substantial revisions before publishing in ACP, see my major comments below.

Major comments:

The introduction needs a complete rewriting, less text book more scientific background with respect to the questions to be addressed. The paper is a successor of Brühl et al. (2015; 2018) and Bingen et al. (2017) but I miss a clear separation and explanation about the added values of this paper compared to its predecessors. The better horizontal resolution has already been discussed in Brühl et al. (2018), so the new aspect, as far as I understood it, is the increased amount of volcanic eruptions and the extend time period by using new satellite data.
-> Response: Abstract and introduction are expended to address this. The resolution is now only mentioned in the model description section

I completely miss references to recent literature in the introduction with respect to radiative forcing estimates of recent eruptions. There are several publications e.g. Andersson et al., (2015); Friberg et al., (2018), Schmidt et al., (2018); Kloss et al;(2021) just to name a few which have addressed the radiative forcing of small to moderate volcanic eruptions in the recent years. These papers have to be cited and differences/added values to their work have to be addressed in the introduction.
-> Response: Most included in introduction now, see replies to other referees and "Friberg et al. (2018) included the whole time series of CALIOP data from 2006 to 2015 and derived stratospheric AOD using reanalysis data for the tropopause, but mentions only medium size eruptions explicitly. Radiative forcing is estimated there from multiplying AOD with -25, an approach which is valid only for purely scattering aerosol". Kloss et al. (2021) cited in sections 6.2 ("Our northern hemisphere results for AOD of about 0.025 for Raikoke (550 nm) agree within uncertainties with Kloss et al. (2021) who use different satellites and different modelling

approaches") and 6.3 ("The value for Raikoke/Ulawun is within the range discussed in Kloss et al. (2021)") now. This paper was not available when we wrote the first version, thank you.

The discussion needs also to be rewritten. As mentioned above the lack of references of recent literature is astonishing. The results of the study need to be discussed in the context of recent literature, e.g. what do we learn from this paper, what we didn't know before from previous studies.
-> Response: Done, see comments to other referees.

I am also wondering about the importance of the small eruptions for the global radiative forcing. It would be interesting if you neglect all small eruptions below a certain threshold values e.g. 10 kT SO2, how this would really change the global radiative forcing. What is range of uncertainties, the range of interannual variability in background periods? Estimates about the uncertainty range are completely missing in the paper.
-> Response: See comparison with results of Schmidt et al. (2018) in Fig 11. They neglect also larger eruptions than your threshold, however. See also remark to referee 1 on background.

Last but not least, differences between the model simulations and satellite measurement need not to be the only cause of missing SO2 sources. There could be several other reasons for the discrepancies (transport, microphysics), neither model simulations nor satellite measurements are perfect. This has to be discussed here as well.
-> Response: Mentioned at several places.

Specific comments

Abstract, line 17: "significantly" is a big word. I did not find any significance tests in the paper.
-> Removed: "significantly"

Page 3, which SSTs do you use? I suppose you run only one ensemble members did you check for the influence of internal variability at least in short sensitivity studies?
-> Response: The CCM is nudged to ERAI which includes SSTs (see section 3).

Description of the EMAC module could be reduced, to only the parts which are really relevant for the paper,. e.g. the calculation of the radiative forcing. This part could be more elaborated. More detailed model descriptions can be put in the appendix.
-> Response: Parts of the model description are moved to the appendix:
"As EMAC is a very complex chemistry climate model it contains many submodels and functions which are essential for running the simulations but are not directly related to the sulfur cycle, these are mentioned in Appendix A. In this section we focus on the sulfur cycle."

Page 12, lines 245-247 It would be nice to see a comparison with Carn et al (2017) and other recent emission data
-> Response: You compare then apples and oranges, a hint is given in Appendix C.

Table 2: It would be nice to see (e.g. with different color) which entries are new or changed with respect to the previous data set.
-> Response: Eruptions from in Bingen et al. (2017) and Brühl et al. (2018) are marked in italics in the table: "Based on a previous study from Brühl et al. (2018) with scaling factors for T63 and already published in an earlier version in Bingen et al. (2017) *(in italics)."*

Will the data set be published?
-> Response: Yes, the data set is published on WDCC: https://doi.org/10.26050/WDCC/SSIRC_3.

Page 21, line 279 "strong" I wouldn't call Kasatochi or Raikoke a strong eruption
-> Corrected: "medium strong"

Figures 9, 10, 11: A comparison with Brühl et al. (2015) for the Pinatubo period and with Brühl et al (2018) for 2002 to 2012 would be nice, to better asses the improvements of this study. Also a validation with GloSSAC (Thomason et al., 2018; Kovilakam et al., 2020) would more than beneficial.
-> Response: Pink lines for comparison with Brühl et al. (2015) are added in fig. 9+11. GloSSAC gives no additional information here since it is derived from data shown in the figure but we can include it in the upper panel since it covers a longer time period than the blue line. Nevertheless, we include a black line in Fig 9 since it is interesting for Pinatubo and the period 2012 to 2018 (based on V2 of Kovilakam).

Section 6.3: Any reason why you look at the tropopause? What is the uncertainty range in your forcing estimates?
-> Response: This is because of the comparison with Solomon et al. (2011). For Pinatubo it differs not much from the value at TOA.

Figure 11 I recommend a comparison with Schmidt et al (2018) here
-> Response: Schmidt et al. (2018) is available for global AOD at 550 nm and added as black line in Fig. 9, AOD at 750 nm (Fig. 10) is not shown by Schmidt et al. (2018). In Fig. 11 the data for volcanic effective radiative forcing from Schmidt et al. (2018) is added as black line.

Page 409, 410: "This was demonstrated to be essential for correctly assessing the extinction coefficient in volcanically quiescent periods." By whom? Maybe I have overseen it but I didn't find it in the paper.
-> Reformulated: "This was demonstrated to be important for correctly modelling the AOD in volcanically quiescent periods". Convection was mentioned in earlier sections.

Page 445, Which studies?
-> Check given reference and Brühl et al. (2018).

Added References:

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