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Comment on acp-2021-654

Anonymous Referee #3

Referee comment on "Reconstructing volcanic radiative forcing since 1990, using a comprehensive emission inventory and spatially resolved sulfur injections from satellite data in a chemistry-climate model" 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 SO2 proï¬les from different satellite instruments and chemistry climate simulations, this study characterizes the inï¬uence 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.

- 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.
- 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.
- 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.
- 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.

Specific comments

- Abstract, line 17: "significantly" is a big word. I did not find any significance tests in the paper.
- 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?
- 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.
- Page 12, lines 245-247 It would be nice to see a comparison with Carn et al (2017) and other recent emission data
- 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. Will the data set be published?
- Page 21, line 279 "strong" I wouldn't call Kasatochi or Raikoke a strong eruption
- 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.
- Section 6.3: Any reason why you look at the tropopause? What is the uncertainty range in your forcing estimates?
- Figure 11 I recommend a comparison with Schmidt et al (2018) here
- Page 409, 410: "This was demonstrated to be essential for correctly assessing the extinction coefi¬□cient in volcanically quiescent periods." By whom? Maybe I have

overseen it but I didn't find it in the paper.

- Page 445, Which studies?

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