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Comment on acp-2021-654

Anonymous Referee #1

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-RC1, 2021

This paper focuses on the injection of SO2 to the stratosphere by volcanic eruptions, and the resulting variability of the stratospheric aerosol layer. It presents a new volcanic SO2 emission database, derived from a collection of satellite instruments, covering the period 1990-2019. It also presents results from a chemistry climate model which uses the updated injection database, and compares the results of the model to various satellite data sets, focusing on the multi-wavelength aerosol optical depth and instantaneous radiative forcing produced by the aerosols.

The construction of such detailed SO2 injection estimates covering the 1990-2019 period is an impressive accomplishment. It is also to my knowledge quite novel, as I believe it is the first attempt to produce SO2 injection values from sulfate aerosol extinction measurements. Unfortunately, the description of the methods used to produce these estimates is lacking. Furthermore, assumptions and choices made in the methods are not given justification. More detailed comments are included in “Major Comments” below.

Chemistry climate model simulations using the new SO2 injection data set are performed and some results shown. Good agreement with observations is achieved, but there is insufficient analysis to provide any improved understanding of the physical or chemical processes that control stratospheric aerosol evolution.

Major comments:

- The description of how SO2 amounts were calculated lacks sufficient detail. I am not aware of any other study that has estimated SO2 injection amounts based on aerosol extinction measurements. This is thus a novel technique, but the method used is not described beyond a few statements along the lines of “The SO2 mixing ratio perturbation is derived from the extinction perturbation observed in a 10-day period beginning about a week after the eruption by dividing by air density, multiplying by a constant and subtracting a typical background.” This explains extremely little: what constant is used, and why? How is the typical background determined? How well can the volume of the aerosol cloud be estimated a week after eruption from the satellite measurements? SAGE in particular has a very sparse sampling density, how does this impact the estimates? Can the method be validated? It would seem that the method could be applied to SAGE and OSIRIS during periods of overlap with MIPAS and the values from the new method compared to the "direct" MIPAS measurements. This would help increase confidence in the method, and provide some idea of the uncertainties in the estimates.

- I highly recommend that the emission database be provided as an electronic supplement (e.g., csv or xls), to allow it to be readily used by other researchers. The table, as text, presently takes up almost 8 pages of the manuscript: it would be more efficient to visulatize the data somehow and include the values as supplemental information. Also, I strongly suggest that the format of the table be modified so that each individual eruption be listed per row, even if there are multiple eruptions on a given date. This will greatly improve the ease in which the data can be read within a computer program and thus used in other studies.
- The model resuls show good agreement with observations, but it's impossible to know whether the improved agreement (compared to prior works from the same group) is a result of the updated SO2 injection data, or to model improvements or changes in model resolution. Given the theme of the ACP journal, the reader expects that this work should improve our understanding of the chemical and/or physical processes that control stratospheric aerosol evolution, but it remains unclear if there is any improvement in understanding being extracted from the study. Nor is there any real motivation or objectives stated in the introduction for the model simulations.

Specific comments:

L11: "Reproduce" is too strong

L12: Here it is said that "slight deviations … were found only for the large volcanic eruption of Pinatubo in 1991", but later in the document deviations in other time periods, e.g., 2010 are discussed, so this is inconsistent.

L19: precise language is needed here, is this the peak radiative forcing produced by a typical "small" eruption, or the time average forcing from these eruptions? And what is a small or medium eruption? Also, it's not clear how this number is estimated, a value of 0.10 W/m^2 is not mentioned in the results or conclusions, and if it comes from Fig 11, how is the effect of small eruptions separated from that of "background" sulfur (e.g., DMS, OCS) transported into the stratosphere via atmospheric circulation?

L22-24: references needed for these statements.

L25: I believe Bruehl et al., 2015 were not making the actual measurements of the size distribution of stratospheric aerosols. Better reference needed.

L31: part of the aims stated here is apparently related to the interaction of aerosols with ozone, but this is not shown in the manuscript.

L34ff: Reference(s) needed.

L37: I am skeptical of a 3-year upper limit on the impact from volcanic eruptions: if ocean temperatures are a part of "climate", then there is good evidence that volcanic impacts on climate can last much longer than 3 years (e.g., McGregor et al., 2015). Obviously the period of impact depends on many factors, but we should be careful to not overly simplify statements which might be misleading to some readers.

L44: Reference(s)?

L65: Some information should be given on how the SO2 column data was used, especially in regards to how a stratospheric component was estimated from the full column.

L130: The gaps in spatial coverage of the OSIRIS data at 17 km extend significantly beyond the polar night: they seem to extend even in best cases to 20-30deg. Some rephrasing needed.

L136: It's not apparent how the sensitivity to clouds can be seen in Figure 4.

L140: How is the correction factor determined? This sounds suspiciously like numbers have been chosen only to produce best agreement.

L157: The study of Grainger et al. (1995) does not seem to provide a relationship between SAD and SO2 mixing ratio. More explanation needed.

L190: It is not clear how differences in the "vertical transport of tracers, like dust and water vapor or ozone" between model resolutions has any importance to the present study.

L216: What parameters?

L218ff: The double radiation call most likely calculates the "instantaneous radiative forcing". It is important to be clear about this and consistent with the terminology.

L219: There is a double radiation call, but how exactly is the radiative forcing calculated?

L220: Not understanding this, are you diagnosing the impact of volcanic aerosol on upper stratospheric UV absorption? Nothing like this is shown in the results.

L241: What is the justification for the lower limits to the vertical integration given? You use 12 km as the lower limit in high latitudes, but the climatological tropopause height in high latitudes is 9-10 km. Conversely, you use 14 km in low latitudes, but the tropopause there is around 17 km. A thorough explanation for these counterintuitive thresholds will need to be given.

L251: An "integration time" has not been introduced, it is not clear what this means in terms of the method.

Table 2: There are a number of cases where the number of values do not match between the different columns in a particular row, e.g., 11 Feb 1990, 19 Aug 1992, 18 Sep 1996. Expanding the table so each eruption is listed in a single row would help this issue, as well as improve the machine readability of the table more generally. There is also a case (14 Jan 2002) where values are listed within brackets, and I did not find an explanation for what this means.

Table 2: The methods used produce an estimate of about 17 Tg for Pinatubo, which is in line with direct measurements of SO2 (e.g., Guo et al., 2004), but in contrast to recent model studies which suggest the effective injection for Pinatubo was much less (e.g., Mills et al., 2016; Dhomse et al., 2014). Some discussion of this issue would fit well into the paper.

L269: Mixing ratios appear quite variable, what is meant here by "typical"?

L271: What upper limit is referred to here?

L281: References should be included to support this statement on the transport of aerosols from Nabro.

L290: “The comparison of the simulated and observed SO2 values” is really hard to do since Figures 1 and 6 use different units and color schemes. It would be helpful to extract the MIPAS years from the simulations and show them with the same units and color scheme in comparison to the observations.

L293: Is the statement on SO2 lifetimes made here a result of this study, or are the lifetimes equivalent to those given by Hoepfner et al. (2015)? If the result is the same as Hoepfner et al., (2015), that should be explicitly stated. If estimated lifetime are different from Hopefner et al. (2015), how and why?

L300: This sentence seems to say that stratospheric aerosol optical properties were calculated using a range of different aerosol types (sulfate, dust etc.). Is this correct, or is the sentence just misleading?

L330: The OSIRIS data is converted from 750 nm to 550 nm, which is fine, but this contradicts the statement just a couple sentences earlier that “Unlike most other studies, the stratospheric AOD is compared at the original wavelengths derived from different optical channels of the satellite instrument measurements.”

L333: The statement that “differences after the large Pinatubo eruption in 1991 between the model simulations and the SAGE II observations are related to the “saturation” effects of the satellite instrument” seems much too confidently worded. It seems quite possible that “saturation” effects explain some of the difference, but how certain can you be sure that it is the only, or even the primary reason? In the tropics, the simulated AOD appears to be ~3 times larger than the SAGE II measurements—is it likely that the SAGE II measurement is so strong an underestimate of the true total AOD?

Fig. 11: The ERBE measurements are not described at all in the text. Are they anomalies? What is the global coverage of the measurements? Likewise, the data from Solomon is only mentioned in passing in the text, and a little more detail should be included on how those radiative forcing estimates were calculated.

L352: “The new model simulations with the additional volcanic eruptions (red line) are closer to the calculated estimates from satellite extinction measurements of SAGE, GOMOS and CALIOP (Cloud-Aerosol Lidar with Orthogonal Polarization) by Solomon et al. (2011) (green crosses) than in previous studies (e.g., Brühl et al. (2015)).” This statement, a concrete conclusion of the study, is impossible for the reader to verify without accessing the prior study, finding the relevant figure, and trying to visually compare the two. This is asking too much of the reader. Please include the result of Bruehl et al. (2015) directly on Fig 11 here so we can directly assess the validity of this statement.

L361: Are the results of Minnis et al. (1993) equivalent to the ERBE data shown in Fig 11? Please clarify.

L362: clarify that the *simulated* AOD drops too quickly compared to the observations.

L374: “2019” is not an eruption.

L375ff: This paragraph is quoting results from other papers, not showing work from this study. If these statements are important, they should be moved out of the Results section or linked directly with results of the study.

L385: The fact that this study uses a higher resolution model than previous studies should have been mentioned earlier, in the model description and/or introduction.

L386: This appears to be a result of the study by Bruehl et al. (2018), which would be important in describing the experiment earlier in the manuscript but not here in the conclusions.

L388: The SAGE II and OSIRIS extinction measurements are not really "newly available", some version of this data has been available for many years. The estimation of SO2 from these data sets is quite new—it's what this paper is presenting!

L402ff: This conclusion is not supported by the results: there is no quantification of the impact the increased number of eruptions included in the database has on the radiative forcing, or its level of agreement with observations.

L408ff: This is an interesting conclusion, but it is not supported by the results. There is no demonstration that including the injections below the tropical tropopause improves the agreement. Even a comparison with prior studies will not prove necessarily support the statement since those prior studies used a different resolution model.

L418: This is not a new result, as it has been shown by prior studies.

L422: The impact of volcanic aerosol on tropical upwelling is not diagnosed in this study. Prior studies have explored this, but statements like this can not be included in the conclusions of this work if there are no new results shown to support it and build upon prior work.

L437ff: This paragraph talks about meteoritic dust, which was not investigated in the study. Perhaps simply adding a sentence or two on the agreement between the model and observed aerosol extinction in the upper stratosphere to motivate the discussion of meteoritic dust would help the reader follow the logic here.

448: Confirming the findings of the IPCC report is, firstly, incorrectly phrased, since the IPCC report only summarizes and reports findings gathered from the published literature. It would be more important to compare the results here with the primary sources, including studies that have been published since the IPCC AR5 (e.g., Schmidt et al., 2018). Second, confirming some general results from prior studies does not make an overwhelming case for publication. What does this study add to the understanding of volcanic radiative forcing that wasn't known before?

L450: Radiative forcing is stated to be that at the surface here, where Fig 11 is said to be RF at the tropopause. Also the numbers quoted here don't seem to agree with Fig 11. It would be best to only refer to calculations for which the results are shown in the paper.

Editorial comments:

Line 9: Volcanic SO2 is not "pollution" in the usual sense of the word, suggest it be cut here.

L49: "Distribution"?

L53: "constitute a source of background…"

L55: Awkwardly phrased: the processes aren't structured, the paper is, and not strictly according to processes.

L80: I've never seen pptv written with v as a subscript, is this a new standard?

L111: confusingly phrased.

References

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