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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.

Anonymous Referee #2

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In the manuscript "Changes in stratospheric aerosol extinction coefficient after the 2018 Ambae eruption as seen by OMPS-LP and ECHAM5-HAM" by Malinina et al. the Ambae eruptions in 2018 are investigated using multiple (∼6) satellite measurement data sets and a model simulation. The first 10 pages (Sections 2-5) focus on the OMPS-LP extinction retrieval, data quality, and result in an OMPS-LP extinction climatology and a section focusing on Ambae. The second part (section 6) on the model simulation introduces four more satellite data sets, derives SO_2 mass injection time series for Ambae, presents the model setup and the results. At this point I started wondering what this study was about and what would be the key result(s). The discussion section compared the OMPS-LP and ECHAM model results and additionally introduced an

estimate of the radiative forcing. Comparisons to previous studies on Ambae and references to studies observing similar effects when comparing measurements and satellite observations are missing. In my opinion this study contains a lot of material that merits publication, but the material need to be sorted, the results should be put into reference of existing knowledge from previous studies and the key message(s) and conclusion(s) should be worked out and stated explicitly. Hence, I'd recommend for publication after a major revision. Please find major and minor comments below.

Major comments:

Scientific objective: To me the scientific objective of the study is not clear. It seemed that the Ambae eruption was studied as an end in itself and certain aspects, such as assessing the quality of the OMPS-LP extinction retrieval algorithm, estimation of mass injection time series, and the radiative forcing estimation were just some by-catch. Please make clear why did you perform the ECHAM simulations? Which new aspects did you learn from the ECHAM simulation that the measurements did not provide? Did you learn anything from the differences between observation and model? Do you have recommendations for improved volcanic plume simulations?

Paper structure: In section 6 four new satellite data sets (MLS, OMI, OMPS-NM and TROPOMI) are introduced to derive the mass SO_2 injection time series and injection altitude. These data set description are scattered over all sub- and sub-subsections and distract from following the line of arguments that should lead to the model setup. I'd recommend to introduce all instruments and data sets at the beginning in an own section on instruments and data sets. I assume section 6.2 should be section 6.1.2. Please also consider a methods section. E.g. in section 6.1.1 and section 6.2 the method to grid OMI/OMPS-NP and TROPOMI SO_2 data seem identical. The subsection on the radiative forcing in the discussion section 7 belongs into the main part of

the paper.

Discussion: A presentation of the key finding(s) and a discussion with respect to existing knowledge from previous studies on e.g. Ambae, volcanic eruptions into the UTLS in the tropics, and simulations of volcanic plumes is missing. Please see suggestions in detailed comments.

Detailed comments:

l31-33: Please provide a reference.

l35: The reference is from 2011 and does not cover the bush fires in 2019 mentioned in the text.

l37: Please add a reference for pyrocumulonimbus, e.g. Fromm et al., 2010

l46-52: Here, multiple simulation studies investigating volcanic plumes are listed. However, the scientific questions that are addressed and the findings are not mentioned. Investigating volcanic plumes is not an end in itself.

l81: Please add a separate section for all the instruments and data sets used throughout the paper: OMPS-LP, OMPS-NP, SAGE III, MLS, OMI, TROPOMI.

l100: "... from all altitudes from 290 to 1000 nm ..." Please provide the altitude range here.

l125/126: What was the highest retrieved extinction? Please add this information. Why did you chose this thresholds? Please justify. Did you take further measures to filter out ice clouds? What about volcanic ash? Does volcanic ash affect the sulfate aerosol extinction retrieval?

l131-145: Please shift to instrument and data set description section.

l139: ..is a near infrared photodiode...

l145: At which wavelengths are the SAGE III extinctions provided?

l155-157: Why would the OMPS instrumental uncertainties not impact a comparison with SAGE III? I'd rather think that instrument uncertainties would contribute to differences between both instruments. Please provide an explanation.

l163: Which Ångström exponent did you use? What is the larges difference between 830-900 nm?

l166: Since you used SAGE III solar occultation and OMPS-LP solar scattering measurements, what was the minimum time difference between the profiles that were compared?

l177-180: It would be more interesting to know the reasons for the differences and not only who studied it. Please add a short summary of the reasons.

l180-181: Is this a finding by Rieger et al. (2018)?

Figure 1 and 170-174: Can you comment on why OMPS compared to SAGE-III systematically overestimates the extinction in extra-tropics? Did you filter out polar stratospheric clouds (PSCs) at high latitudes in winter time? Did both data sets include PSCs?

l186-187: If you have to average the extinction data rather depends on what you want to study. I.e. if you are interested in maximum plume height or detailed transport and conversion processes you'd rather not average the data. To create Fig. 2 actually no pre-gridding as described here is necessary. It probably even may introduce artefacts. Which vertical bin size did you use?

Figure 2: It is not clear if here an average of your level3-product is shown, or if these are real monthly zonal 30° averages. There are reoccurring vertical white stripe patters in winter time. What is causing these stripes? Why is the data cut at 16 km? In the

extra-tropics this is well above the tropopause and missing the lower stratosphere.

l201: Please add reference to Vernier et al., 2011 for the aerosol tape recorder effect.

l205-11: I find this explanation confusing and to some extent misleading. Do you mean the annually reoccurring white stripes in Fig. 2 here? This pattern is visible at all altitudes at the same time in winter. This does not look like QBO to me. Please mark or zoom into the mentioned QBO pattern, because I cannot see it in Fig. 2. What causes the yearly changes in stratospheric aerosol loading? Or is this annual pattern rather an instrument artefact? Please explain.

l219: "...averaged over longitudes.." Do you mean zonal means here? Or did you restrict these averages to a certain longitude range?

l222-224: How large was the increase in extinction after the April eruption? Is this increase significant? In Fig. 3 I cannot see any increase after the April eruption. At 18.5 km a slight increase to the north of Ambae is already visible before the eruption. Between 30-40S the extinction remains constant until June. Please make clear which increase you mean and provide numbers/factors for/of the extinction increase that match with what is shown in Fig. 3.

l232: Does the plume really vanish by mid-October? Fig. 2 shows that the plume is still there, but at higher altitudes. Please rephrase.

l234-235: I don't understand what is meant here. South of 30S the extinction is constant over the whole period shown. Please clarify.

l242,251: Why did you use MLS data to obtain vertical information of the plume? MLS has a significantly coarser vertical resolution than e.g. CALIOP and OMPS-LP. Why didn't you use CALIOP and OMPS-LP to derive information on the injection altitude?

l248-250, Fig. 4b: Showing the OMI/OMPS-NP SO_2mass injection time series for the July eruption too would provide valuable information. Comparing both approaches would allow for an estimate of the uncertainty of this approach for the April eruption

due to the use of different instruments. Are there also spatial gaps in OMI/OMPS-NP data after the April eruption?

l253: How deep was the SO_2 injected into the stratosphere? How does the injection altitude profile look like?

Section 6: I'd recommend to separate instrument descriptions from the method description, the results, and their discussion. In particular the descriptions of the grid and method (l262-272 and 284-291) seem redundant.

l258: Please describe briefly what the OMI row anomaly is.

l262: When introducing a threshold to distinguish background from volcanic signal, please provide information on the OMI sensitivity towards SO_2. Please state why you selected 0.05 g/m2 as a threshold.

l263: Why do you convert g/m^2 to g/m^2? Please clarify.

l258,266,277-278,286: I think a little bit of explanation of the assumptions made for the centre of the SO_2 mass altitude would be helpful. This information is distributed over the text it take some text forensics efforts to understand that these differences introduce some uncertainty. How sensitive is the result on the assumed mass altitude? How much would the result change if 7 km were assumed for TROPOMI? Please add to Fig. 4.

l275: Please provide information on much of the self-defined grid was covered by TROPOMI before applying the threshold to provide a reference value.

l287: Why did you only consider column densities less than 1000mol/m^2?

l308: Please explain, what is TM5 model?

l311: What does MECHAM stand for? What is the difference to ECHAM?

l320: ... from Sect 6.1 and 6.2, right? From which data set were the altitudes derived?

l324: Where does the OH field come from, some climatology?

l345ff, Fig. 3: I'd suggest to add a difference plot to show the agreement and regions of largest difference, probably due to the wild fires.

l358-359: I doubt that the plume remains at the same geographical location. There is zonal transport. It rather remains at the same latitude band.

l364: Please describe your internal studies on ECHAM SO_2 sensitivity in more detail. What did you investigate? Could these studies provide some kind of uncertainty estimate?

l368: I wouldn't consider ERA5 a pure model product. It has a substantial amount of measurements assimilated.

Fig. 5: Here I'd also recommend to add a difference panel.

l374: Actually I think the April plume is nearly invisible. What is the OMPS-LP extinction detection limit? Which changes can be considered significant?

l376: At 19.5 km in November the OMPS-LP maximum is about $1.5\times10^{-3}\,\mathrm{km}^{-1}$ and $1.0\times10^{-3}\,\mathrm{km}^{-1}$ in the ECHAM simulation. Isn't this a significant difference compared to the increase from about $1.0\times10^{-4}\,\mathrm{km}^{-1}$ to $2.0\times10^{-4}\,\mathrm{km}^{-1}$ at the same altitude for the April plume?

Section 7.1: A discussion of the results is completely missing. How well do your results agree with e.g. Kloss et al., 2020; other estimates of Ambae/Aoba SO_2 mass injections (https://so2.gsfc.nasa.gov/omi_2004_now.html)? Did you expect that the ECHAM simulation reproduces the plume correctly? Why did you expect that? How well did previous model studies simulate tropical UTLS injections? What are the error sources? In how far does your aerosol uplift rate agree with the expected uplift rate from the water vapour tape recorder? You mention that it is probably faster due to additional heating, but no evidence is shown.

Section 7.2: The radiative forcing is presented as method description and results. In my opinion this part rather belongs into the main part of the paper. Furthermore the analysis is not well thought through. The result is presented in a descriptive manner. I'd recommend to condense and get to the point.

l401-408: Here, merely the figure and the line colors are described. This rather belongs into the figure caption. Please explain the results and which conclusions can be drawn from the data shown in Fig. 6.

l418, 420: "between dashed and solid blue lines" and "panel a of Fig. 5": Please name the parameters represented by the colored lines or panels.

l434: Why does your result differ significantly from Kloss et al., 2020? Please discuss.

Technical:

Please write out all abbreviations on first use.

l14: injection estimates

l18: Which ECMWF reanalysis, ERA-interim or ERA5?

l24: ... climate system -is- now ...

l40: comma before which

l41: (UTLS); closing bracket missing

l59: used

l109: This results in the situation that the usual stratospheric aerosol extinction wavelength 750 nm, used by e.g. SCIAMACHY and OSIRIS (Rieger et al., 2018), is not suitable, as OMPS-LP measurements around this wavelength are affected by the O2-A absorption band.

l111,115,123: used

l117: for all the tangent altitudes -> for all tangent altitudes

l122: is -> was

l200: comma before because

l226: "...below 20°S..." Do you mean south of 20°S?

l242: used

l246: do -> did

l261: -1E30 -> -1×10^{30}

l279: I assume this should be 6.1.2.

l274: ejected -> injected

l306: "g/m^{2}2" remove 2

l317: 2x is -> was

References

Fromm, M., D. T. Lindsey, R. Servranckx, G. Yue, T. Trickl, R. Sica, P. Doucet, and S. Godin-Beekmann (2010), The untold story of pyrocumulonimbus, Bull. Am. Meteorol. Soc., 91, 1193– 1209, doi:10.1175/2010BAMS3004.1.

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Vernier, J.-P., et al. (2011), Major influence of tropical volcanic eruptions on the stratospheric aerosol layer during the last decade, Geophys. Res. Lett., 38, L12807, doi:10.1029/2011GL047563.

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