We are very greatful to the Anonymous Referee # 2 for reading our manuscript the second time and providing constructive comments. We appreciate input and think that the final paper benefited from the suggested changes.

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

Dear authors,
thank you for the detailed answers. You did a very good job with revising the manuscript and clarifying open questions. In particular, I very much appreciate the high level of detail concerning technical aspects. I think this makes your study sound, comprehensible, and reproducible. Please find below 5 minor comments for consideration before publication.
Minor comments:
l478, l523 (revised manuscript): Tropopause
Indeed the tropopause definition is a crucial aspect. But, not only the tropopause definition, but also the algorithm to calculate the tropopause is important as different interpretations/implementations of the WMO lapse rate tropopause may lead to different results e.g. see Maddox and Mullendore (2018) WMO tropopause vs. simplified WMO. Please provide information on your implementation.

Thank you for pointing this out. We checked the study of Maddox and Mullendore (2018) where the two implementations of the WMO lapse rate tropopause are discussed. We follow the not-simplified WMO definition (Appendix A (1) of that paper), as we interpolate the temperature profile on a 100-m grid and compute the lapse rate for each consecutive level pairs within 2 km of the possible tropopause point. We added this information to the text.

l350-354 (revised manuscript): QBO pattern
I cannot identify any of the described QBO patterns between 25 and 30 km in Fig 3a and b. In Fig. 3b the most prominent feature I see is the annually (NH winter) reoccurring increase in extinction (Ext869). In Fig. 3a this pattern is also visible, but weaker and shifted to the middle of the year (NH summer). The dark blue bands mentioned in the reply I would identify in the years 2014, 2016, and 2019, which is different to the years listed in the reply and paper: "2013, 2015, 2017". Please clarify.

We agree, that mentioning of the blue stripe in the reply to the comment to l205-11 of the first round of the comments was misleading. What we tried to say there, is that in the 0 to -30° latitude band (panel (a) of Fig. (3) of the revised manuscript and panel (b) of Fig (2) of the first version), there are lighter stripes which are followed by darker stripes in the altitude range from 25 to 30 km. These stripes occur on a quasi-biannual basis, which we attribute to QBO, based on the cited literature. In the text of the revised manuscript, which the reviewer highlighted, we mention only the lighter colored stripes, or an increase in Ext_{869}, and name the years for them.

In this manuscript revision, to make the QBO effects more visible, we added

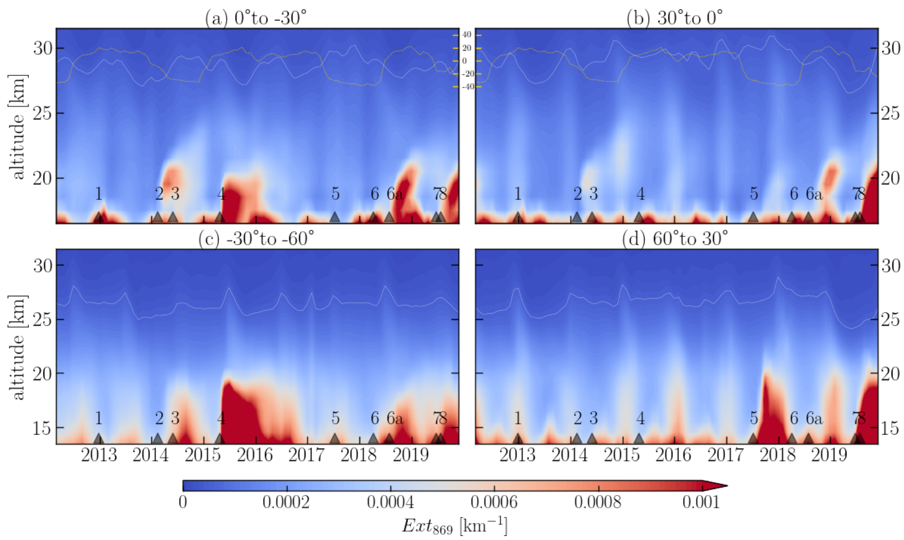

Figure 1: Monthly mean aerosol extinction coefficient (Ext_{869}) distribution as a function of time and altitude. The values were obtained by binning and zonally averaging OMPS-LP monthly level 3 Ext_{869}. The white lines show 0.00005 km^{-1} Ext_{869} level. The yellow dotted lines show monthly mean zonal wind components in Singapore at 15 hPa in m/s.

a white contour representing 0.00005 km−1 Ext_{869} level to each panel of Fig. 3 of the revised manuscript. This level is located on average between 27 and 31 km and is not as strongly affected by volcanic eruptions as higher Ext_{869} levels. In our opinion, the white line makes QBO pattern in particularly in panel (a) easily identifiable. This being said, in panel (b), those effects are not as prominent, most likely due to stronger annual oscillations in the Northern Hemisphere caused by stronger Brewer-Dobson circulation. As we also mention in the highlighted by the reviewer passage, there were two QBO disruptions, which surely masked the quasi-biannual pattern.

We also prepared a draft of the Fig. 3 of the revised manuscript but with monthly mean zonal winds at 15 hPa from Singapore, it is Fig. 1 of this document. (Data source: `https://www.geo.fu-berlin.de/met/ag/strat/produkte/qbo/singapore.dat`.) As QBO effects on stratospheric aerosols are not the topic of our paper, and QBO effects on the high-altitude extinctions were well described in the literature we cited in the paper's paragraph, we decided not to add this version of the figure to the final manuscript. In panel (a) of Fig. 1, one can see that white and yellow lines have quite similar shapes, but are out of phase. Hopefully, Fig. 1 strengthens our opinion on the QBO visibility. Here it's important to mention, as we state in the manuscript, QBO is not the only periodic signal, but it is still worth to mention it. Since we agree with the reviewer, that the annual signal can be considered to be dominant, we changed the order of the periodic signals in our description.

Additionally, we corrected the manuscript to state that the behavior of the high aerosol extinctions is affected by annual oscillations in addition to QBO, and we no longer mention specific years. We also added a citation to Vernier et al. (2011), where QBO effect on aerosols is discussed.

Fig. B1 (revised manuscript) and reply to comment l248-250: OMI/OMPS SO2

Thank you for also deriving the SO2 mass from OMI/OMPS for the second eruption. In my opinion this is an important figure, as it a) supports the finding that the second eruption injected more SO2 than the first one and subsequently this cannot be considered an artifact due to better sampling by TROPOMI, and b) makes the transition from OMI/OMPS to TROPOMI more transparent, as it gives an indication of the uncertainty of SO2 mass due to different instruments. It seems that OMI/OMPS data is more noisy, but averaging the 4 data points after the maximum (red circle) yields about 0.36 Tg, which is pretty close to 0.39Tg (0.26Tg) for TROPOMI with mass centered at 7km (15km) (Fig. B1). Please consider adding Fig. 3b from the reply to Fig. B1 in the manuscript.

We created a new figure, which incorporates Figure 3 from the previous review and Figure 1 of the revised manuscript, and its brief description to a separate Appendix.

Reply to comment l125/126:

Thanks a lot for the very detailed answer that explains the reasons why you have chosen this cloud filter threshold. Actually, I think, these thoughts are worth publishing. Please consider adding a condensed paragraph of your reply to the manuscript, e.g.:

"The highest retrieved extinction is 4.0978×10^{13} km^{-1}. ... This value occurs on the 28th of June 2017 at 08:40UTC at 26.8° N, 66.7° E at 10.5 km, which is most likely to be in a thick convective cloud.

The threshold to reject clouds is selected empirically to keep as much as possible of the aerosol extinction and reject as many clouds as possible. The trade-off is determined by the potential application of the data set. For applications, where it is more important to get rid of as many clouds as possible and single high aerosol peaks are not that important, a rather conservative value of 0.002 km^{-1} is used. This value is based on the results from Bourassa et al. (2010), where the Ext750 after the Kasatochi eruption were not exceeding 0.0015 km^{-1}. We used this threshold when we previously showed our OMPS-LP data (e.g., Malinina, 2019). For the investigation of an isolated volcanic eruption, as, for example, the Raikoke eruption 2019 (Mauser et al, 2020) or in this study, a higher threshold is necessary as we are interested in preserving all increased aerosol values. As we investigated the plume propagation at rather high altitudes we do not rate a possible contamination by clouds as a crucial issue. Thus, for the Raikoke case the threshold was set to avoid loosing any increased extinction

value above the tropopause, which resulted in the value of 0.1 km^{-1}. We also used this threshold for Ambae."

We added the suggested by reviewer passage in a revised form.

One comment on plume height derived from MLS SO2 data versus CALIOP and OMPS-LP. Yes, the latter two do not measure SO2, but since conversion to sulfate aerosol starts immediately (e.g. see von Glasow et al., 2009 and references therein) I consider measured sulfate aerosol heights in the first few days after the eruption as a good indicator of injection height.

Thank you for the clarification of the original comment. Indeed, though the conversion of SO_2 to H_2SO_4 starts immediately, determining the height of the sulphate cloud from CALIOP and/or OMPS-LP can still be problematic. For instance, as discussed in the previous revision round, in the paper and in the comment above, clouds can be a big limitation of limb-scatter extinction products. If a profile happened to be in a cloudy patch, in particular in the tropics, it might be impossible to determine the altitude of the sulphate cloud correctly. Nevertheless, in Muser et al. (2020), the manuscript mentioned above, our OMPS-LP product was used to identify the height of the Raikoke cloud. This was possible because of the favorable conditions on that day. During the 2019 Raikoke eruption, OMPS happened to pass directly over the Raikoke island and the mid-latitude clouds didn't influence the measured data, thus, we were able to identify the height of the sulphate injection, since it was accompanied by ash and the extinction coefficient was significantly increased. However, when we tried to identify how the cloud was propagating in the next couple of days, we couldn't uniquely attribute the increase in extinction at the altitudes of interest to the eruption. This is because the ash cloud wasn't as thick as right above the volcano during the eruption. In addition, extinction levels were already slightly elevated from before the eruption, so the formed H_2SO_4 right after the eruption aerosol was undetectable in that scene. Based on that experience, we preferred not to use OMPS-LP for the sulphate injection height. Although, under certain conditions, as the reviewer correctly mentions, it would be indeed possible.

At the same time, though clouds are not as prominent problem for CALIOP data products, the spatial coverage is. CALIOP registers the backscatter profiles in a very thin band, and for example, on the 27th of July 2018, during the second Ambae eruption, the closest point is about 5° longitude away from the volcano. Which is relatively far away, and later in time, in particular in the tropics, convection can lift the sulphate and/or ash higher and perturb the injection height value.

Based on those factors, we consider MLS SO_2 profiles to be the more accurate estimate of injection height in our and in a general case, however, in certain conditions, as reviewer states, CALIOP and OMPS-LP could be used to adjust that value.

Technical comments:

Please add comma before "which" in l28, 50, 53, 175, 405 and more

Added.

l 405 boarders − > borders

Corrected.

References:

Maddox, E. M. and G. L. Mullendore, 2018: Determination of Best Tropopause Definition for Convective Transport Studies. J. Atmos. Sci., 75, 3433–3446, https://doi.org/10.1175/JAS-D-18-0032.1

von Glasow R., Bobrowski N., Kern C. (2009) The effects of volcanic eruptions on atmospheric chemistry. Chem Geol 263:131–142.

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.