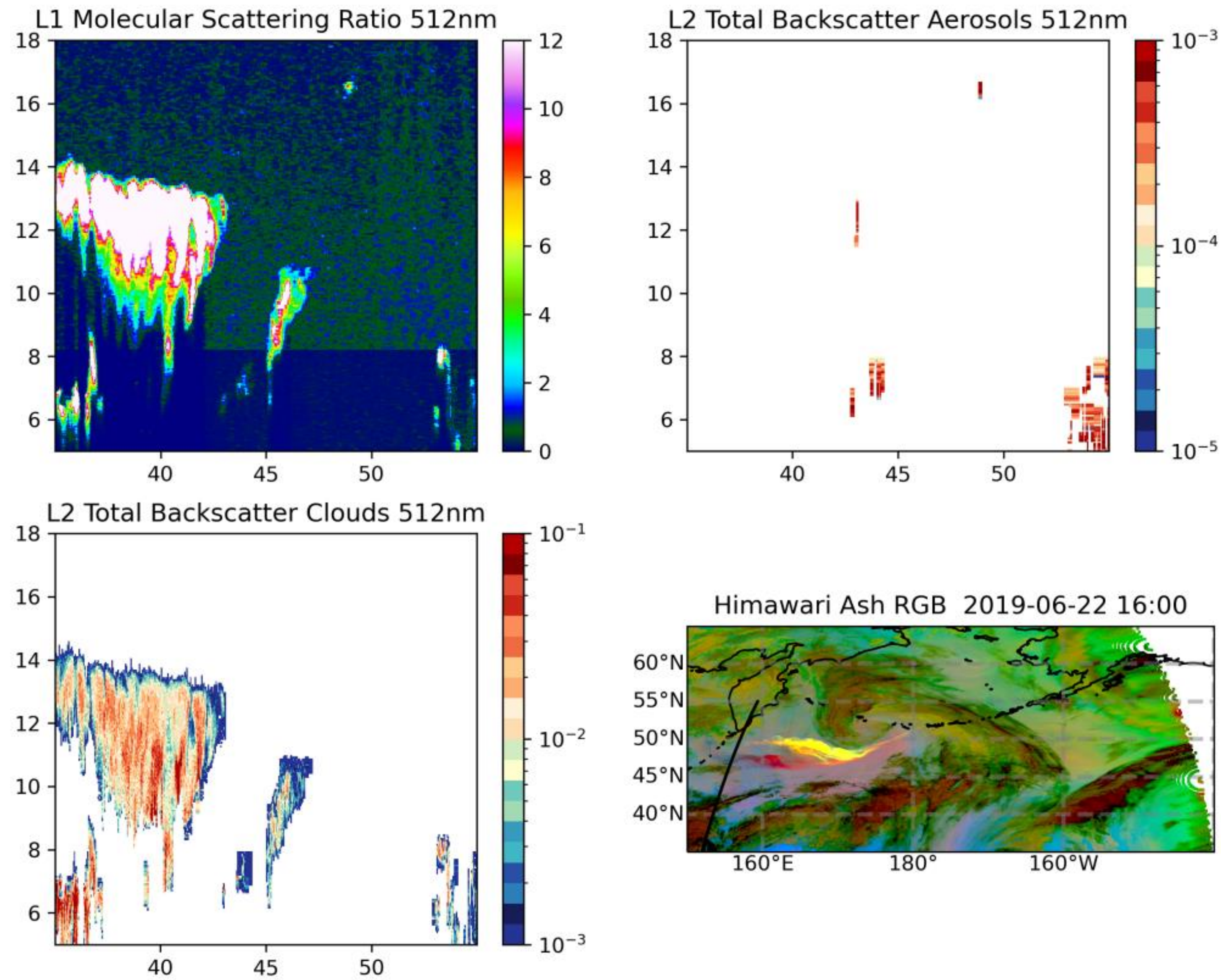
Referee #2

We would like to thank Reviewer 2 for the time he/she spent on the detailed and mostly positive comments and suggestions (including an independent, additional literature search), to improve our manuscript! In the following, we address each comment individually, including the changes we made to the manuscript accordingly.

1) Abstract: "Discrepancies between observations and models indicate that ash has played a role on evolution and sAOD values."
This is rather overstating what you conclude in your main text in your conclusions: "Discrepancies (in terms of aerosol concentration and lifetime) between observations and the global model WACCM point to the complexity of those events. In particular it may indicate that the initial injection of ash (which is not implemented in the WACCM set up) plays a role in the evolution of such plumes, in particular for Raikoke."
It is important therefore to change "ash has played" to "ash may have played"
The sentence was changed to: *'Discrepancies between observations and models indicate that ash may have influenced the extent and evolution of the sAOD'*

2) L223: "There are no CALIOP intersections of the core plume during the early stage". I would contest this. There is an overpass on the 22nd from what I can see. Have a look at the data here: https://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_v4_detail.php?s=production&v=V4-10&browse_date=2019-06-22&orbit_time=15-59-28&page=1&granule_name=CAL_LID_L1-Standard-V4-10.2019-06-22T15-59-28ZN.hdf
While not extensive (and it shouldn't be owing to the narrowness of the plume at that time), at ~ 50N, there is evidence of stratospheric aerosol at 16-17km. From the website, the potential temperature looks to be around 425K at 100hPa. A rough conversion to temperature gives me 220K. This is pretty close to your 225K. I would therefore suggest a slightly more rigorous assessment would be worthwhile using this CALIPSO data given that these values tend to support your assumptions. This will give the reader more confidence that your assumptions are robust.

Yes, we are aware of this intersection. With the following Figure it becomes clear that CALIOP did not intersect the bulk of the cloud seen from HIMAWARI, but rather a very thin tail. Therefore, we believe the CALIOP observations not to be of sufficient value for this study.



3) L233 and Caption Figure 1: There are some inconsistencies between the text and the figure caption: Text: “This plume is initially composed of ash (reddish colors, in Fig. 1), with also some evidence of SO2 (yellowish colors, in Fig. 1). The remaining greenish and pinkish colors indicate the presence of water clouds around the volcanic plume.” Caption “. Red: ash; Pink to violet: dust; Yellow: mixture of ash and SO2, Green: thick and thin mid-level clouds or cirrus clouds”. What is the difference between ash and dust in your caption? While I recognise that these are semi-quantitative estimates, the text should be better reconciled. The imagery is always semi-quantitative in the absence of in-situ observations of the ash owing to e.g. different refractive indices giving different ‘colors’ even for the same size distribution (e.g. Figure 6 of Millington et al., 2012 which uses the SEVIRI dust product; reference provided below). Some caveats surrounding this identification should be given and Millington et al. (2012) or similar should be referred to.

Millington, S. C., Saunders, R. W., Francis, P. N., & Webster, H. N. (2012). Simulated volcanic ash imagery: A method to compare NAME ash concentration forecasts with SEVIRI imagery for the Eyjafjallajökull eruption in 2010. Journal of Geophysical Research: Atmospheres, 117(D20).

We have changed the text accordingly (Figure 1 is now Figure 2):
"The Dust RGB product is used, instead of the Ash RGB product, because it is more sensitive for large satellite viewing angles, which is the case for the region of interest for Raikoke. This product is based on the stronger absorption of ashes at 12 µm than at 10.4 µm while it is the opposite for ice and liquid water and on the absorption by SO_2 at 8.7 µm. It depends a lot on the size distribution of aerosols and ice crystals and provides only qualitative information (Millington et al., 2012). This plume is initially composed of ash (reddish colors, in Fig. 2), with also some evidence of SO_2 (yellow and bright green colors, in Fig. 2). The remaining brownish and blueish colors indicate the presence of water and ice dominated clouds associated to the volcanic plume."

Furthermore, we changed the caption for Figure 2: "Himawari Dust RGB images from 21/06/2019 to 28/06/2019, over the region of Raikoke. Red: ash; Bright green: SO_2; Yellow: mixture of SO_2 and thin ash; Greenish: thick and thin mid-level clouds or cirrus clouds; Brown: thick and high ice clouds; Blue: humid low level air; Pink to violet: dry low level air. The contour lines are plotted …"
As well as for Figure A2: "Himawari ash RGB for the second Ulawun eruption. Bright green represent SO_2, while darker green shades show clouds"

4) The discussion starting around line 300: "For both Ulawun eruptions (June 26th and August 3rd), OMPS data show some AOD perturbations after the first eruption and more significantly elevated values after the second eruption. Like for the Raikoke eruption, WACCM shows immediate and stronger signals during the weeks following the eruptions, but decreasing faster. While for OMPS observations a significant impact (sAOD around 0.01) of the second Ulawun eruption is still apparent in the tropical stratosphere by the end of the year 2019, in the model comparable values are found in October and by the end of 2019 the sAOD has values down to 10 times smaller than for OMPS. The model shows a faster decrease…."
These statements would be aided by the addition of simple line plots of the global and hemispheric sAODs. Figure 7a does show OMPS sAODs integrated over some latitude bands in such a manner, over a longer time period. However, I think that it would be worthwhile indicating the global, 30-90N, 0-30N, 0-30S, etc for both the model and the observations as a comparison.
We believe the global mean sAOD adds valuable information. Therefore, we added another line to Figure 7 (now 8) a and b, the global sAOD (grey). By the way: The latitude bands are not randomly chosen. Raikoke is located at 48°N. We chose two bands in the NH, North and South of Raikoke, 1 in the tropics and one respectively in the SH.
The Figure label text has been changed accordingly: *'(a) 3- day mean sAOD from OMPS aerosol extinction values (from tropopause altitude up to 30 km) averaged over five latitude ranges (global, 50-90°N: North of Raikoke, 30-50°N: South of Raikoke, 20°S-20°N: tropics and 30-50°S: SH respectively).'*

Furthermore, we provide a respective Figure in the Appendix (A5) for the WACCM simulation.

‘Supporting material for Section 4.5:
Figure A5: WACCM mean sAOD values for the respective latitude bands, as shown with OMPS and SAGE III/ISS observations in Fig. 8 a and b. When comparing Fig. 8 a and b with Fig. A5 the higher and faster impact on the sAOD from the model simulations become evident (as also shown and explained in Section 4.2).’

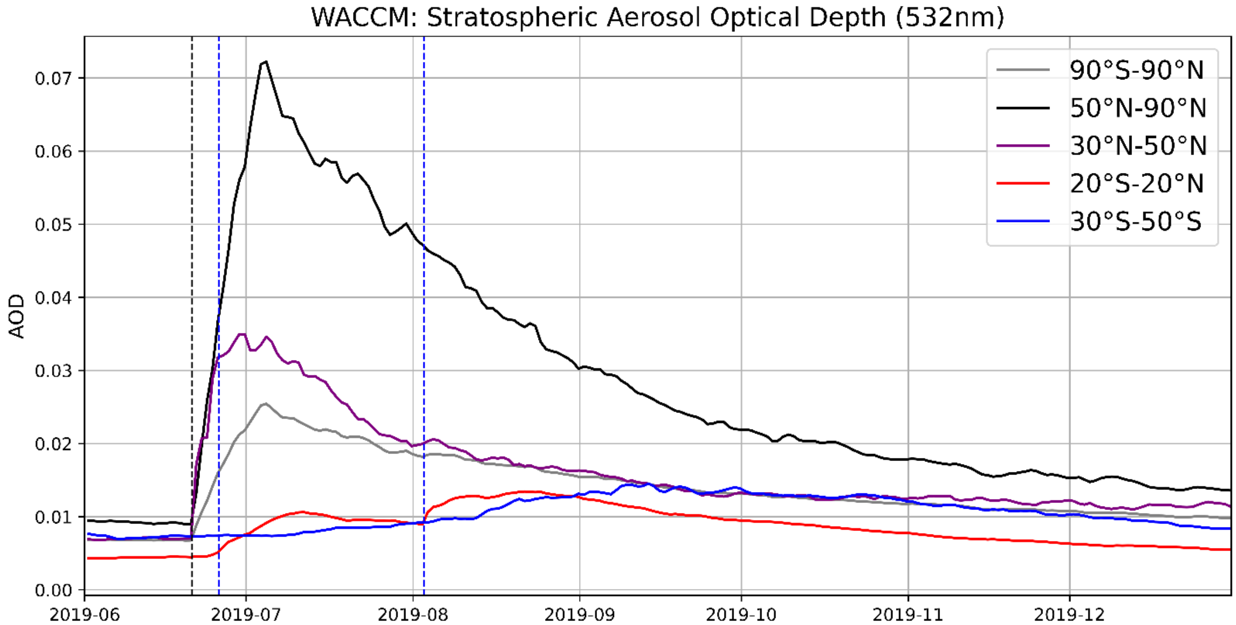


Figure 1: Respective to Fig. 8 (a) and (b), WACCM means sAOD values. The WACCM AOD is shown here for sulfate only, i.e. with no condensation of water, to eliminate the signature of PSCs in the winter SH, which would likely mask the plume signature closer to the pole.

We also added a respective sentence in the main text: *‘OMPS and SAGE III/ISS data suggest a comparable but smaller sAOD impact for the Australian fires than for the Raikoke eruption (30-50°S compared to 50-90°N).*
A similar representation of the sAOD as seen in Fig. 8a and b with the WACCM simulation is shown in the supporting material (Fig. A5). Discrepancies in terms of AOD extent and timing, compared to OMPS and SAGE III/ISS observations are also shown in Fig. 5 and explained in Section 4.2.’

Adding another Figure with slightly changed latitude bands (e.g. 30-90°N, 0-30°N, 0-30°S etc.), does not show new results or give new scientific conclusions. Therefore, we have not added another Figure.

5) CLAMS model: The initialisation of the model is pretty coarse (a box) which doesn’t have the details of the spatial distribution in the vertical or horizontal within the plume. More care is therefore needed in interpreting the results from the CLAMS model. For example, “Therefore, the sAOD enhancement above Europe observed by OMPS in Fig. 3b does not originate from Raikoke, but rather from forest fires in Alberta, Canada.” “Europe” is a large area: The OMPS data suggests that there is an enhancement of the AOD over northern Europe, Scandinavia, the Baltic countries, and western Russia (Fig 3). The CLAMS simulations suggest that the Raikoke plume impacts “southern” Europe. Areas such as the UK at the interface between northern and southern Europe experience both …… Some of this greater detail is worth stating more explicitly, plus the caveat that the CLAMS initialisation may not be that accurate.

We agree that the text can use more precise details on the location of the Alberta fire plume over Europe, compared to the areas partially impacted from the ClaMS trajectories (Raikoke box). This has been done accordingly: *'At the beginning of July the main bulk of the air mass tracer remains west of the Atlantic Ocean, with only a minimal impact above southern Europe (Fig. 6, second panel). Therefore, the sAOD enhancement above northern Europe observed by OMPS in Fig. 4b does likely originate from other sources than Raikoke (e.g., from forest fires in Alberta, Canada). ~~not originate from Raikoke, but rather from forest fires in Alberta, Canada.~~'*

We agree completely that the ClaMS simulation is very simplified in representing the injection location and does not include microphysics and chemistry. This is on the one hand a limitation, because trajectories are calculated which do not correspond to the actual plume (because of the box-shaped initialization area), on the other hand, the simplicity itself is of high value to the study, because we are not analysing quantities, but rather use ClaMS as a pure transport analysis. We try to state even clearer in the revised manuscript that the ClaMS tracer dispersal should be regarded as the effect of pure passive large-scale transport, and that comparison to OMPS allows assessing this effect – but not more. For this please also see answer to Review 1 point 5. Limitations and value of this simplified study are given in the text from line 362 to 370.

6) Section 4.4. Vertical distribution. While most of the graphical displays are reasonably logically chosen throughout, here I think that the choice of representation of the vertical distribution could be improved. Figure 6a-d are "around Raikoke and around Ulawan", while Figure 6e shows the OMPS data in a series of time stamps as a function of latitude and altitude. I would have preferred to see the model distributions plotted up in a similar way to the Fig 6e. One could then see if the modelled aerosol plumes interact or overlap (probably more likely) from using either the WACCM model or the CLAMS model. The approximate location of the stratosphere could be marked on Figure 6e and any of the new figures too.

As suggested by the Reviewer, we added a mean tropopause height in Fig. 6e (now Figure 7). Hence, the label description has changed : *'(e) OMPS aerosol extinction monthly averages over all longitudes from June to December 2019. White dashed lines represent the averaged tropopause altitude.'*

Furthermore, we prepared the mentioned model distribution as shown for OMPS in Fig 7e (see below). The main message from this plot is, that the initialization of the injection in the model results in an enhanced aerosol concentration much faster with a subsequent fast transport towards the tropics as well. This is also seen when comparing Figure 4 a and b (now Figure 5) and explained in the text.

Conclusions from the Figure below about a possible interaction/overlapping of the Raikoke and Ulawun plumes are therefore not representative and the Figure was not added to the main text.

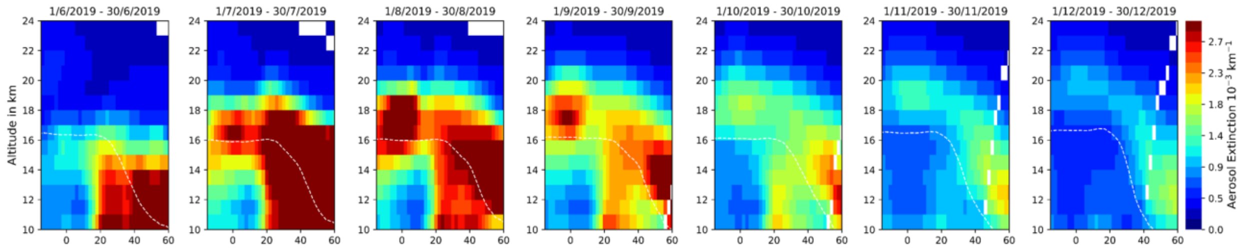


Figure 2: As Figure 7e in the text with WACCM data.

7) Section 5: Radiative effects: "calculated the shortwave RF of the Raikoke and Ulawun plumes using the UVSPEC radiative transfer model (see Sect. 2.7 for the setup of the model and calculations). As input parameters for the model, the SAGE III/ISS volcano-attributed aerosol extinction profiles discussed above are used."
Why isn't the radiative forcing (or the effective radiative forcing) given for the WACCM model? It can be used for these calculations can't it? The use of the SAGE extinction profiles and sensitivity perturbations of the single scattering albedo allow some assessment of the impacts on the clear-sky radiative forcing and the sensitivity to the assumptions. WACCM should be able to give both cloud-free and cloudy sky effective radiative forcings but these are absent from the paper.
How is the surface reflectance taken into account? I could not find details. Won't the co-location of the highest AODs over the highest surface reflectances need to be accounted for (weakening the TOA radiative forcing)?
A possible further study based on radiative forcing (RF) simulations with WACCM has been highly discussed among the authors. There are substantial differences between the model simulation output and satellite observations, especially in terms of aerosol timing and extent (e.g. comparison between Fig. 7 a,b, now Figure 8, and the new Appendix Figure A5). Possible reasons for those discrepancies are given in the main text (i.e. starting from line 330/ last paragraph in section 4.2). SAGE III/ISS and OMPS aerosol extinction values, however, agree very well (e.g. Fig. 7 a and b). RF estimations based on observations (as done in the manuscript in section 5) are therefore more meaningful and are consistent with those derived from a very similar method in Kloss et al. (ACP, 2019) for the 2017 Canadian wildfire plume and in Kloss et al. (JGR, 2020) for the Ambae volcano plume. This gives a similar setup for RF comparisons between these events.
Furthermore, the WACCM simulations in this work have been done by saving and storing chemical and microphysical variables only and not the ones dealing with radiative forcing. Deriving RF information with WACCM requires to calculate and store a large number of corresponding variables all along our WACCM simulations, if we refer to Schmidt et al., 2018 (JGR, 123, 12,491–12,508. https://doi.org/10.1029/2018JD028776). For each emission scenario (Raikoke only, Ulawun only, Raikoke+Ulawun, no volcano emission), we would need to run further independent simulations, i.e. taking all RF effects into account and ignoring aerosol RF effects. This would be a very dedicated work requiring both some new specific expertise with WACCM RF calculations for us and an adapted modelling infrastructure which goes beyond the scope of the presented work. These abilities are expected to be developed in the future. As a result, we have decided to not add any RF calculation with WACCM in the manuscript.

Shortwave surface reflectance was set to a fixed wavelength-independent value of 0.1, which is thought to be a representative value for main surfaces underneath the plume dispersion (ocean, bare and vegetated soil). Of course, the surface reflectance can be substantially higher for ice- and snow-covered surfaces and this adds uncertainties, as now mentioned in the text:

(from L154): '…*(as adopted from the SBDART code). We consider a fixed wavelength-independent value of 0.1 for the surface reflectivity. This is intended to represent an average value for main surfaces underneath the dispersed plume: ocean, bare and vegetated soil. It is important to mention that the surface reflectance can be significantly larger for ice- and snow-covered surfaces; RF estimations can be quite sensitive to the surface reflectance (Sellitto et al., 2016).'*

Sellitto, P., di Sarra, A., Corradini, S., Boichu, M., Herbin, H., Dubuisson, P., Sèze, G., Meloni, D., Monteleone, F., Merucci, L., Rusalem, J., Salerno, G., Briole, P., and Legras, B.: Synergistic use of Lagrangian dispersion and radiative transfer modelling with satellite and surface remote sensing measurements for the investigation of volcanic plumes: the Mount Etna eruption of 25–27 October 2013, Atmos. Chem. Phys., 16, 6841–6861, https://doi.org/10.5194/acp-16-6841-2016, 2016.

Typos/clarifications:

The level of English is generally acceptable, but there are a number of corrections noted below that will make the paper easier to read and digest. I would suggest that a native English speaker re-read the amended manuscript before re-submission as I won't have caught all of them.

Ok, thank you

l1: stratospheric moderate -> moderate explosive *we avoided the term 'moderate now'*

l15: Suggest Severe -> Explosive ok

l17: of sulfur dioxide (SO2) volcanic emissions -> volcanic emissions of sulphur dioxide (SO2) changed, but differently

l23: dominates -> strongly influences. You cannot say that it dominates as if it were an effusive eruption emitted at the surface it would have littleclimate effect (except perhaps through aerosol-cloud-interactions) ok

l28: Butchart, 2014 -> Butchart, 2014; Jones et al., 2017. I think that the study by Jones et al (2017) is worth including here. Their Figure 1, is perhaps one of the most relevant in terms of the injection latitude and altitude. ok

Jones, A.C., J.M. Haywood, N. Dunstone, M.K. Hawcroft, K. Hodges, A. Jones, and K. Emanuel, Impacts of hemispheric solar geoengineering on tropical cyclone frequency, Nature Communications, 8, 1382, doi:10.1038/s41467-017-01606-0, 2017.

l29: Point (3) does not have a suitable reference associated with it. I would suggest adding the Jones et al (2017) reference again here (see above): relative to the tropopause -> relative to the tropopause (e.g. Jones et al., 2017) ok

l 33: 20Tg SO2 is quite a large estimate for the amount of SO2 injected. I would suggest "Up to around 20Tg SO2" ok
l34: have been -> were ok
l37: climate occurred -> climate has occurred ok
l43: its good practice to be sequential in terms of the dates: Günther et al., 2018; Kristiansen et al., 2010; Krotkov et al., 2010 -> Kristiansen et al., 2010; Krotkov et al., 2010; Günther et al., 2018 true, thank you
l51: the complexity that -> the complexity and the uncertainty that ok
l53: time the -> time, the ok
l54: Canada, Alberta (June) and Siberia (July) -> Alberta, Canada (June) and Siberia, Russia (July) ok
l64: flies -> has flown sentence changed
l85 on multiple -> at multiple: Agreed: is it worth saying explicitly that the wavelength dependence provides information on the aerosol size distribution via the Angstrom exponent?
Yes, the sentence has been changed accordingly:
"However, the better vertical resolution and observations at multiple wavelengths compared to OMPS, bring an added-value when spatio-temporally averaged data are used for the radiative forcing calculations. The wavelength dependence, for example, can be used to extract information on the aerosol size distribution via the Angstrom exponent."
l93: to discriminate -> discrimination between *ok*
104: "volcanic effluents" is a strange phrase: I'd replace with "emitted in volcanic plumes".
The sentence has been changed accordingly*: 'While its primary target is the monitoring of meteorological parameters (surface temperature, temperature, humidity profiles and cloud information), IASI also provides high-quality information on trace gases parameters and particles, including gases and particles emitted by volcanoes (e.g., Clarisse et al., 2013; Carboni et al., 2016; Ventress et al., 2016; Guermazi et al., 2020).'*
L116: micronic -> micron *ok*
L147: With the UVSPEC the -> With UVSPEC, the *ok*
L150-l155: remove the "-"s for grammar purposes. *Replaced by numbers*
L191: for a pure -> from a pure *ok*
L212: possibly refer again to Jones et al. (2017) *ok*
L217: as in -> to *ok*
L239: mowing -> moving *ok*
L244 & 247: The use of possibly is questionable. It definitely is converted to sulfate aerosol owing mainly to gas phase oxidation. Remove possibly in both sentences. *ok*
L262: A reference to the smoke from the Alberta fires would be appropriate. There may be better ones appearing at present, but here is one I found: Jenner, L.: Alberta Canada Experiencing an Extreme Fire Season, NASA, May 30, https://www.nasa.gov/image-feature/goddard/2019/ 545 alberta-canada-experiencing-an-extreme-fire-season, 2019. *ok*
Fig 3: Caption – the wavelength for the AOD should be stated. Ok*: 'Global OMPS (at 675 nm) sAOD averaged..'*
L274. even one year -> even nearly one year ok

L279: The eastward transport dominates, which depends on the vertical distribution of the aerosol and the phase of the QBO (quasi-biennial oscillation). The sentence could do with a reference e.g. Lee and Smith, 2003:
Lee, H. and Smith, A.K., 2003. Simulation of the combined effects of solar cycle, quasi-biennial oscillation, and volcanic forcing on stratospheric ozone changes in recent decades. Journal of Geophysical Research: Atmospheres, 108(D2). Ok, thanks
L308: crossed-impact -> cross-impact ok
L325: interfered with the Raikoke evolution -> interfered with the evolution of the Raikoke plume ok
L334: mentioned limitations -> associated limitations ok
L340: which is a schematic estimate, but for sure causes discrepancies compared to observations and reality -> “which is a necessary simplification of reality where pulses in injection altitude and magnitude are inevitable”. Better, thanks
L347: potential cloud signatures are included -> cloud signatures are potentially included ok
L357 locationsof -> locations of ok
Section Heading: “Recent” is a subjective term: Kasatochi/Sarychev could be considered to be recent. I would simply add the range of recent to the title “In the context of other recent events (2017-2020)” ok
Fig 7. I like Figure 7. It is very informative. As a minor point, it would have been more logical for the LOAC points to have been plotted in purple so that the latitude of the observations correspond to the latitude band in Fig 7a-b. That has been changed
L451. The slight increase in the observed AOD in April 2019 -> The slight increase in the observed AOD in the southernmost latitude band in April 2019 ok
Fig 8. The 1e-2 scaling on the ordinate axis is tiny! This really needs to be more clear.

This has been done accordingly: