Review of: Stratospheric aerosol layer perturbation caused by the 2019 Raikoke and Ulawun eruptions and climate impact by Corinna Kloss et al

There are some very nice aspects of this paper – particularly the OMPS analysis and the WACCM model simulations.

Generally, I was less impressed with the CLAMS modelling which has to be coupled to an off-line RT code (UVSPEC) and SAGEIII aerosol profiles in order to provide a rough estimate of the radiative effect. The radiative effect will only ever be rough because i) there are no clouds considered in the RT calculations, ii) how variations in the surface albedo were taken into account is not documented, iii) it makes a simple equinox assumption in order to simplify impacts from daylength, solar zenith angle variations etc. It would have been a better approach to examine the effective radiative forcing in the WACCM model and adjust some of the optical parameters within that model as this would have allowed for precise co-location between e.g. aerosol, cloud, surface reflectance, atmospheric profiles etc.

It does seem as though it has been rather rushed – particularly the latter sections lack the methodological rigor to provide a credible estimate of the climate impact via the radiative forcing. There are some areas where the text and Figure captions needs clarifying/improving. The choice of Figure presentation is not always optimal.

On balance, I feel that the paper does provide enough new results to be of interest to the scientific community, but I would recommend that the authors consider both the more major comments and typos/corrections before the paper is acceptable for publication.

More major Comments:

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"

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.

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).

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.

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.

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.

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)?

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.

l1: stratospheric moderate -> moderate explosive

l15: Suggest Severe -> Explosive

l17: of sulfur dioxide (SO2) volcanic emissions -> volcanic emissions of sulphur dioxide (SO2)

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)

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.

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)

l 33: 20Tg SO2 is quite a large estimate for the amount of SO2 injected. I would suggest "Up to around 20Tg SO2"

l34: have been -> were

l37: climate occurred -> climate has occurred

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

l51: the complexity that -> the complexity and the uncertainty that

l53: time the -> time, the

l54: Canada, Alberta (June) and Siberia (July) -> Alberta, Canada (June) and Siberia, Russia (July)

l64: flies -> has flown

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?

l93: to discriminate -> discrimination between

104: “volcanic effluents” is a strange phrase: I’d replace with “emitted in volcanic plumes”.

L116: micronic -> micron

L147: With the UVSPEC the -> With UVSPEC, the

L150-l155: remove the “-“s for grammar purposes.

L191: for a pure -> from a pure

L212: possibly refer again to Jones et al. (2017)

L217: as in -> to

L239: mowing -> moving

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.

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.

Fig 3: Caption – the wavelength for the AOD should be stated.

L274. even one year -> even nearly one year

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).

L308: crossed-impact -> cross-impact

L325: interfered with the Raikoke evolution -> interfered with the evolution of the Raikoke plume

L334: mentioned limitations -> associated limitations

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”.

L347: potential cloud signatures are included -> cloud signatures are potentially included

L357 locationsof -> locations of

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)”

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.

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

Fig 8. The 1e-2 scaling on the ordinate axis is tiny! This really needs to be more clear.