The paper presents an analysis of the UKESM1 model simulations of the Raikoke eruption and compares it to satellite observations. The authors argue that including ash in the model emission scheme, which is usually neglected, provides a more accurate simulation of the evolution of the volcanic plume and a better agreement with observations.
The paper is well written and contributes significantly to modelling the impact of eruptions on the stratosphere and climate.
I recommend it for publication, subject to some improvement. My main concern is the lack of a proper description of the methods to construct the observation dataset used for comparison, especially CALIPSO. I also find the Radiative impact analysis incomplete and suggest the authors expand the analysis to other months when the aerosol loading peaks.

Specific comments:

L49: "(e.g. Cai et al., 2022)". The reference is missing.

L116:" This study uses quality-assured (QA) daily averaged vertical profiles of aerosol extinction (km-1) at 532nm from the Version CALIOP Level 2 data product."
The authors' description of the CALIPSO data used in the study is lacking. The paper also lacks a data availability section, making it challenging to identify the version used. Based on the description given above, one can guess that the authors are using "CAL_LID_L2_05kmAPro-Standard-V4-20", which is then averaged and gridded into a daily file. If that is the case, I find using CALIPSO's L2 data to derive the sAOD zonal mean time series flawed. L2 files only report aerosol extinction for detected layers and fill values for the rest of the profile. Averaging these profiles will bias the mean sAOD toward large values during the first few months when the aerosol signal is strong and toward very low values later, which appears to be the case in Figure 4. Kar et al. (2019) reported that to retrieve profiles of stratospheric aerosol extinction and backscatter coefficients reliably, they relied on substantial spatial and temporal averaging because of CALIPSO's poor signal-to-noise ratio in the stratosphere. Applied vertical averaging was 900 m, 5° latitude, and 20° longitude for a whole month. They averaged Level 1b backscattered profiles, not level 2 extinction profiles. Others used similar averaging to retrieve their stratospheric aerosol profiles (Thomason et al., 2007; Vernier et al., 2009; Vernier et al., 2011). The authors need to describe better the CALIPSO data and methods used to derive the sAOD. If indeed they used L2 data, they need to acknowledge its limitation when comparing it to OMPS and the model's sAOD.

L119: "Campbell et al. (2012)". The reference is missing.

L152: "80km", change to 40.5km.

L164: "The retrieval issues described here have a significant impact on our study since it was estimated that the initial plume reached altitudes of between 11 and 20km (Vaughan et al., 2021; Osborne et al., 2021)." This sentence contradicts the previous one, which states that the loss of sensitivity below 17 km is only for the short wavelength and SH. Please explain why this is an issue in the NH where this eruption took place.

L293: “The notable difference between the observations and the model is unexpected given that the magnitude of SO2 injected was based on observations (Muser et al., 2020; Kloss et al., 2021; De Leeuw et al., 2021).” Have you considered that the modeled SO_2 might compare better with other instruments or datasets, given that cited SO_2 volcanic mass was derived using different instruments?

Sections 4.2 and 4.3, Figures 4 and 5s: If the authors used L2 CALIPSO data (see my previous comment), they need to revise this section. OMPS LP and the model provide complete aerosol profiles, unlike CALIPSO L2, which only provides the elevated layers. I also suggest they compare their sAOD to CALIPSO’s official L3 data for reference.

Section 4.6 and Figure 9 and L520: “After this, the observations show much of the aerosol plume below the average tropopause height, and by December, the magnitude of aerosol extinction coefficient is negligible.”
Simplifying the volcanic aerosol distribution and tropopause altitudes in NH into one profile/one altitude per month can lead to the wrong conclusions. The volcanic plume is transported poleward to lower altitudes but still in the stratosphere, which is not apparent in the figure. The tropopause altitudes can vary significantly between 30-90N (~7 and 15km). Therefore, the conclusion that the plume is moving to the troposphere is inaccurate. The authors should modify the figure by removing the tropospheric part of each profile before calculating the monthly mean profile, thus ensuring that it only represents the stratospheric profile. Similarly, they can also calculate the tropospheric profile if they believe that a significant part of the plume was transported to the troposphere, although I doubt it. They should also modify the text accordingly.

L533: “This could be due to aerosol microphysical processes such as the rate of coagulation and/or condensation or how the model represents new particle formation.” I don’t understand why the authors are speculating on particle growth. They should be able to investigate the particle size in the model and provide a definitive explanation.

L549: “….Due to more removal processes in the troposphere this results in a much faster decay rate and shorter lifetime.” See my previous comment. Please revise this paragraph and any discussion regarding the average tropopause altitude.

Section 4.7 Radiative impact: This section is a significant result of this work; however, the way it is presented is lacking. The RI calculation is only shown for July when Raikoke’s sAOD peaked in the later months. The authors need to expand their analysis and table 2 to provide the RI for other months. The analysis did not show when Sarychev's sAOD peaked. Was it in July or later?

L580: “We also note that we cannot make exact global comparisons between our analysis and the Sarychev Peak eruption due to the impacts of the first Ulawun eruption (26th June) in UKESM1” I don’t understand why this is not possible. The authors should be able to run the model simulation without Ulawun and derive the Raikoke-only RI. Please revise the text accordingly.

L584: “The cooling effect is greatest over North America, similar to the distribution of sAOD. This is in contrast to the Sarychev Peak eruption, where the modelled radiative impact indicated a stronger cooling over Russia in comparison to North America.” Can you please comment on the cause of this difference?

Section 5. “Discussion and conclusion”: the section should be renamed “Summary and conclusion” as it mainly summarizes the analysis and results presented in the previous sections.

L642:” lower in altitude resulting in a much faster decay rate due to transfer to the troposphere through tropospheric folds.” See my previous comment regarding the average tropopause and revise the text accordingly.

L655: “Some of these differences may stem from the large dependence on meteorology that has been noted for low-altitude eruptions (Jones et al., 2016).” I don’t understand this explanation. The main difference between the two eruptions is the presence of a large amount of ash in Raikoke, not the meteorology. The authors should investigate their model for the exact differences in the aerosol optical properties that result in such differences and provide a better explanation. They could also compare the SO_2only RI to the SO_2+ash and Sarychev, which is mostly SO_2.

L663: “Future work might consider …” The authors should include the addition of smoke aerosol to future work needed to improve their results.

The paper is missing the code and data availability section.

Kar, J., Lee, K.-P., Vaughan, M. A., Tackett, J. L., Trepte, C. R., Winker, D. M., Lucker, P. L., and Getzewich, B. J.: CALIPSO level 3 stratospheric aerosol profile product: version 1.00 algorithm description and initial assessment, Atmos. Meas. Tech., 12, 6173–6191, https://doi.org/10.5194/amt-12-6173-2019, 2019.
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