The manuscript by Wells et al. shows model simulations of the Raikoke volcanic eruption in 2019 and comparisons with space-borne observations. They found that volcanic sulfate particles alone are not sufficient in order to explain the high AOT values that were observed during summer, autumn and winter 2019-2020. Including ash into their UKESM1 simulations, however, enhances the agreement between model results and observations. The manuscript is an important contribution to the literature as it shows comparisons of Raikoke modelling results including ash in the simulations vs. observations. There are, however, some questions and comments to parts of the manuscript. Some of the concerns are listed as follows:

Question 1: Why should the Raikoke event regarding the produced stratospheric aerosol be so different compared to Pinatubo, Sarychev and many others.

After these eruptions, the ash was removed quite quickly (within a few weeks) and the sulfate was then left as the only volcanic aerosol type? And now, the ash was present even after months (September –November 2019)?

Question 2: If volcanic ash would be present in the northern hemispheric stratosphere in summer and autumn 2019 (authors write 0.4 – 1.8 Tg) in a comparable amount as sulfate (authors write 1.5Tg ± 0.2 Tg), one should find a lot of cases with enhanced particle depolarization ratios of the stratospheric aerosol layers, for example with CALIOP measurements. To our knowledge this was not the case.

Here are some examples that show low depolarization ratios only:

https://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_v4_detail.php?s=production&v=V4-10&browse_date=2019-07-18&orbit_time=06-56-08&page=4&granule_name=CAL_LID_L1-Standard-V4-10.2019-07-18T06-56-08ZD.hdf

https://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_v4_detail.php?s=production&v=V4-10&browse_date=2019-07-20&orbit_time=04-54-59&page=4&granule_name=CAL_LID_L1-Standard-V4-10.2019-07-20T04-54-59ZD.hdf

Question 3: Why is the potential impact of the record-breaking Siberian fires on the UTLS aerosol load totally ignored (for the period from August 2019 to December 2019)? The smoke certainly influenced the aerosol in the 8-15 km height range for latitudes from 65-

90°N. The Siberian smoke is discussed by Ohneiser et al. (ACP, 2021) and by Ansmann et al. (Frontiers, 2021).

We hypothesize that simulations with sulfate and smoke (instead of sulfate and ash) may even explain better all the OMPS-LP and CALIOP observations.

Question 4: How is the AOD computed in the case of the CALIOP observations. Is it computed from the backscatter profile multiplied by a sulfate lidar ratio (of 40-50sr)? …and then integrated from the tropopause to 20 km height?

Since we believe that the smoke fraction was more than 80-90% (and the sulfate fraction 10-20%, and the ash fraction 0%) at high northern latitudes in the UTLS height range then the appropriate lidar ratio is 80-90 sr according to Ohneiser et al. (2021) and then the CALIOP AOD would be even a factor of 2 larger then shown since September 2019.

Question 5: Are you sure that all the spaceborne passive remote sensing techniques work properly during the maximum of the stratospheric perturbation in August 2019 and September 2019? Maybe the channels were almost, partly or totally saturated or almost saturated, at least strongly biased?

Table 1 is not useful, AERONET data are biased by the tropospheric impact, measurements at 19-20°N were certainly influenced by the Ulawun eruption. It is at least impossible to state the Ulawun eruption had no effect. What about an MLO lidar? If there is a lidar, what depolarization ratio was measured, what about the observed AOD?

Is the Brewer-Dobson Circulation already so strong in August and September to explain such a strong transport of aerosol towards the North Pole?

Question 6: According to Fig. 9, the initial aerosol optical thickness in summer would be overestimated when assuming a sulfate + ash mixture. However, already from October onwards the high observed AOT values cannot be explained with sulfate + ash only; and of course, not at all with sulfate only. The decay times of sulfate and sulfate + ash seem to be way to short in order to describe the high AOTs in late 2019 and early 2020. The decay time of wildfire smoke is longer and the authors should mention that especially the Arctic aerosol situation was more influenced by wildfire smoke particles. A significant amount, especially north of 65-70°N the dominating amount of the stratospheric aerosol type must have been Siberian wildfire smoke particles, as Ohneiser et al. 2021, ACP and Ohneiser et al. 2022,

ACPD write.
Figure 9 is confusing. A mean tropopause for the latitudes from 30-90°N makes no sense…. A UTLS layer, one half in the upper troposphere and one half in the stratosphere, is not what is expected after a moderate eruption such as the Raikoke eruption. One expects that the layer is fully and clearly above the tropopause. And all the aerosol in the upper troposphere (at latitudes below about 55°N) should have been be efficiently removed by cirrus clouds. In addition, we speculate, nobody knows the exact SO_2 injection heights? Thus, all the simulations seem to be just playing around with possibilities.