EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-1060-RC2, 2022

Comment on egusphere-2022-1060

Anonymous Referee #2

Referee comment on "Including ash in UKESM1 model simulations of the Raikoke volcanic eruption reveals improved agreement with observations" by Alice F. Wells et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1060-RC2, 2022

General comments

The paper analyses simulations with a state of the art earth system model including chemistry and satellite observations concerning the effects of a medium size volcanic eruption. It considers 2 satellite instruments providing information on the vertical distribution of aerosol but still focuses too much on the horizontal distribution of vertical integrals in the initial phase where the uncertainty is largest due to lack of observations. Possible interactions with smoke from forest fires are mentioned but not included in the simulations.

The model description is too short. Concerning radiative effects, the standard quantities and notations (e.g. radiative forcing) should be used and not only subsets.

The parts on MLO-AERONET might be skipped because of large uncertainties.

The quality of the figures should be improved. The selected color scheme is almost as bad as a grey-scale. The paper should be published after revision.

Specific comments

Line 38ff: Here again only estimates for total injections of SO_2 are cited. Satellite observations (e.g. Glantz et al., 2014, Höpfner et al., 2015) show that stratospheric aerosol optical depth and SO_2 concentration of Sarychev are larger than that of Kasatochi, because Kasatochi has a larger fraction staying in the troposphere (despite of the opposite behavior of the total).

Line 90: Here already 'pumice' should be mentioned, section 5 is too late.

Line 102: Heating by soot from forest fires might be mentioned here, especially if a sensitivity study is included.

Line 173: At Mauna Loa can be effects of local volcanoes. Also a possible signal might be dominated by Ulawun.

Line 188: Here more details on the aerosol module (modal or sectional, microphysics) should be provided. The sentence in the abstract (2 moments for what?) is not enough.

Line 192: Is CNTL without explosive volcanoes only or without any volcano including the ones outgassing into the troposphere?

Line 194ff: Here is a big source of uncertainties. Are the injections equally distributed in a slab like assumed by Mills et al. (2016)? Or is there a vertical distribution derived from observations? If yes, mention instrument. How thick is the slab in the upper troposphere? More details please, for SO_2 as well as for ash.

Line 214ff: Section 3.3 should be merged with section 3.1, also with respect to the

assumed size distribution.
Line 229f: Please provide a figure for the OMPS-NM background and the background SO_2-burden in CNTL in an electronic supplement.
Line 293ff and Figure 3: Don't expect agreement for the initial peak in burden. The e-folding time for OMPS-NM looks smaller than the number in the legend because of the secondary peak. Is OMPS-LP used for the vertical separation? Averaging over half of the northern hemisphere might introduce a lot of artifacts from data gaps (and maybe a mismatch in convection patterns when subtracting results of 2 simulations, depending on the calculation method). What causes the spike at day 60? It might be useful also to show averaged vertically integrated values from OMPS-LP. Please provide more information on the "background" (extra lines here or a figure in a supplement).
Line 368 and Figure 5: Provide a number or a curve for the longtime background to allow for quantitative comparisons. Unfortunately that is often a problem in the literature.
Table 1: Is there something wrong with the presentation of the CALIOP-data here? You may remove the column since it is said in the text that CALIOP sees no signal.

Line 421: This is not valid for the latitude of MLO, see Table 1.
Figure 6: Can the bias between the left and right columns be related to background removal?
Line 430ff: This is difficult to understand. How are limits applied? By scaling or truncating of a model quantity, e.g. sAOD? Or by sampling only the regions with data? The jumps in Figure 7 at the time of switching between the instruments look odd. Linear interpolation of what? More details and clarification please.
Line 506: This points to the presence of pumice or soot particles.
Line 525: 'and longitude' or 'half of the northern hemisphere'.
Line 564ff: This is something like clearsky radiative forcing. However, by subtracting results of 2 GCM-simulations you have always some kind of cloud feedback. Allsky forcing is smaller but difficult to derive by such an approach. Also infrared is not negligible. Please expand on that and use the proper notation. For the global results for 2019 the number represents the combined effect of Raikoke and Ulawun. Concerning Sarychev and Raikoke/Ulawun you may compare with the instanteneous allsky forcing of Schallock et al. (2021).
Line 597: and OMPS-LP.
Line 602f: It might not be worth, to repeat this uncertain difference here. Please shorten.
Line 630ff: There should be also important contributions from Canadian forest fires as indicated in Osborne et al. (2022). A sensitivity study with volcanic ash and forest fire soot would be of interest, also in the light of the CALIOP observations in the contribution to the discussion by Ohneiser, but maybe in a separate paper if that causes a too long delay. At least mention findings of Osborne et al., (2022) on the effect of soot.
Line 647: Caution with these statements. That might be in contradiction to other shown results.

Technical corrections
Line 49: What 'REFS'? Insert citations.
Line 69: Typo in citation.
Line 205: Typo.
Line 318: zonally averaged or averaged over longitude. Include a-f in Figure.

References
Glantz, P., Bourassa, A., Herber, A., Iversen, T., Karlsson, J., and Kirkevåg, A.: Remote

sensing of aerosols in the Arctic for an evaluation of global climate model simulations, Journal of Geophysical Research: Atmospheres, 119, 8169–818, 2014.
Höpfner, M., Boone, C. D., Funke, B., Glatthor, N., Grabowski, U., Günther, A., Kellmann, S., Kiefer, M., Linden, A., Lossow, S., Pumphrey, H. C., Read, W. G., Roiger, A., Stiller, G. P., Schlager, H., Von Clarmann, T., and Wissmüller, K.: Sulfur dioxide (SO_2) from MIPAS in the upper troposphere and lower stratosphere 2002–2012, Atmospheric Chemistry and Physics, 15, 7017–7037, 2015.
Mills, M. J., Schmidt, A., Easter, R., Solomon, S., Kinnison, D. E., Ghan, S. J., Neely, R. R., Marsh, D. R., Conley, A., Bardeen, C. G., and Gettelman, A.: Global volcanic aerosol properties derived from emissions, 1990–2014, using CESM1(WACCM), Journal of Geophysical Research, 121, 2332–2348, 2016.
Schallock, J., Brühl, C., Bingen, C., Höpfner, M., Rieger, L., and Lelieveld, J.: Radiative forcing by volcanic eruptions since 1990, calculated with a
chemistry-climate model and a new emission inventory based onvertically resolved satellite measurements, Atmospheric Chemistry and Physics Discussions, Preprint, doi:10.5194/acp-2021-654, 2021.