Review of the manuscript numbered acp-2022-38, revised version 1

Title: "Impact of Holuhraun volcano aerosols on clouds in cloud-system resolving simulations" written by Haghighatnasab et al.
Manuscript number: "acp-2022-38".
Decision: "Minor revision"

The authors have done a good job responding to most of my concerns and comments. I thankful for the authors' efforts to revise the manuscript. However, a few comments were not addressed. So, my decision is minor revisions.
The number of lines, sections, and comments written in blue characters, mean my comments in previous round of review.

The comments that have not been addressed:

Specific comment for Section 2.2.: In this section, the authors describe the method for implementing aerosol effects on the ICON-NWP, and the authors shows distribution of column-mean CCN as shown in Fig. 3. I think the distribution of CCN is reasonable. However, there are no information about the vertical distribution of CCN. Based on the body of the manuscript, the data for SO_2 was originated from OMPS product. I think that the product is vertical column amount of SO_2. Which layer did the authors add the SO_2? Based on my experiences, the layer that aerosols are input is really sensitive to the simulated impact of aerosol on cloud microphysical properties. In addition, did the authors assume SO_2 gas is as sulfate aerosol particle?
The authors added some descriptions about the treatment of the SO_2 emitted from volcano (Line 185-193 of the revised manuscript). However, it is not clear for me about the treatment of SO_2.
Based on the revised manuscript, the authors added SO_2 retrieved from OMPS data product with a "scaling" to lower troposphere (i.e., up to 3 km height). However, how did the authors "scale" the data? Was the SO_2 added uniformly up to 3 km height? or added some vertical distribution (i.e., decreased exponentially with height)? The author should add more detailed information. The figure of the vertical profile of the activated CCN in supplemental material will be helpful for the readers.
In addition, I'm not sure about the treatment of aerosols in the model. In the line 164-165, the authors indicate that the consumption (or depletion) of CCN can be considered in the method used in this study. However, the method in this study used observation of OMPS for volcanic SO_2 as an external data, and the consumption and depletion process of aerosol cannot be considered in this

method. In addition, the authors refer a literature of Costa-Suros et al. (2020), but in my understanding, Costa-suros et al. (2020) used offline aerosol transport model. If the authors used offline aerosol model, the consumption and depletion process can be calculated explicitly as a wet deposition process. However, I'm wondering the consumption process can be included by the method in this study that is described in Section 2.2.
If I misunderstand the method used in this study, please explain the method more clearly.

In addition, I cannot find the answer from the authors to my comment:
Specific comment for Section 2.2.: As well as the SO_2, water vapor is also emitted by the eruption, and the emitted water vapor can affect the meteorological field and cloud properties. Did the author only consider the emission of SO_2?

Line 157: I think that "(factual and counterfactual)" is not necessary.: The word, "factual" and "counterfactual" are remained in conclusion in revised manuscript.

Technical comments:

Figure 4: Information of data source of these satellite image should be included in the caption (I know the information is included in acknowledgement, but I think the information should be added in the caption).

Line 145: If the authors add the literature of Sato et al. (2018), which was used NICAM, Goto et al. (2020, GMD, doi:10.5194/gmd-13-3731-2020) can be added as an example of the model (NICAM) using ARG-parameterization.

Additional comment

Line 334-335: In this part, the authors suggest that the model exaggerates the increase in large LWP values. Based on my experience, such exaggeration commonly occurs in the model, in which effects of clouds are calculated by cloud microphysical model. Why does such exaggeration occur? If the authors have any answer or some speculation, some comments about this exaggeration will helpful for scientific community.