Response to the Reviewer's comments version 2

We would like to thank the reviewers for their effort in helping us improve the manuscript. Below we respond point-by-point to the comments, with the reviewer comments in black, our responses in black, and the changes in the revised manuscript in blue. The line numbers are for the revised manuscript version.

Response to referee comments #1

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.

We thank the reviewer for their assessment of our manuscript. The review helped to improve the manuscript significantly.

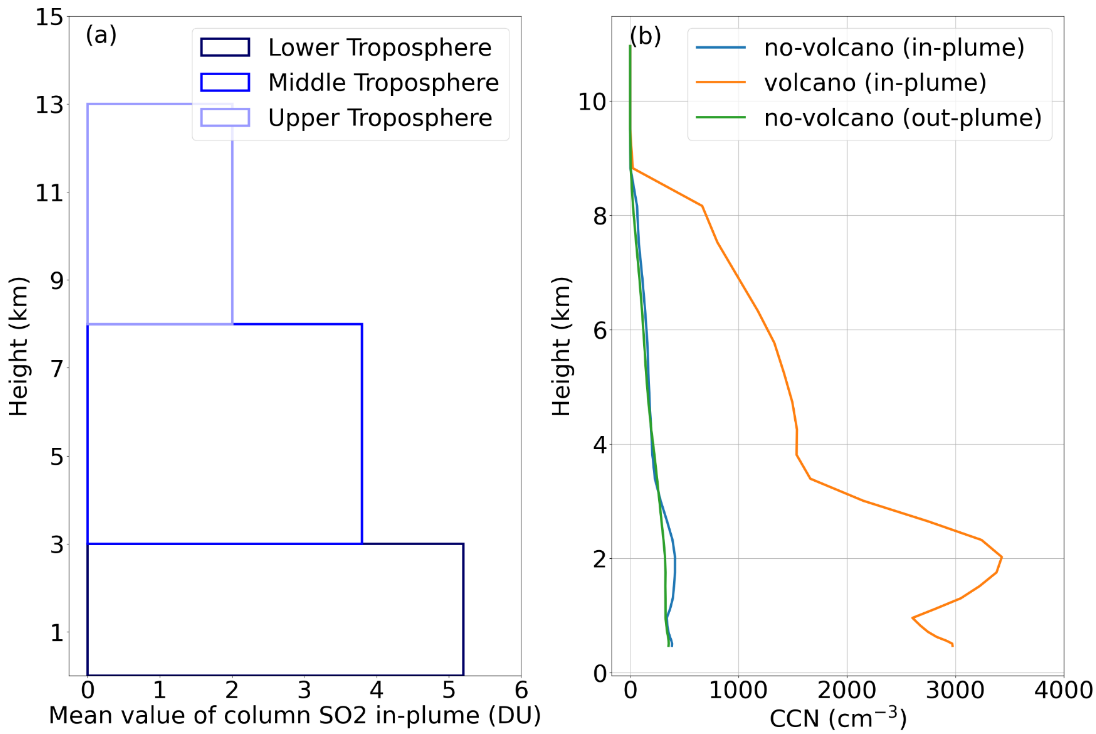
The number of lines, sections, and comments written in blue characters, mean my comments in previous round of review.

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 SO2 was originated from OMPS product. I think that the product is vertical column amount of SO2. Which layer did the authors add the SO2? 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 SO2 gas is as sulfate aerosol particle?

The authors added some descriptions about the treatment of the SO2 emitted from volcano (Line 185-193 of the revised manuscript). However, it is not clear for me about the treatment of SO2. Based on the revised manuscript, the authors added SO2 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 SO2 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.

We thank the reviewer for the suggestion. The scaling of CCN was done by computing the distribution of scaling based on the enhancement of SO2 inside the plume relative to the mean SO2 value outside of the plume in the lower troposphere (up to 3km). Then the sulfate concentrations in the CAMS reanalyses inside of the plume were scaled at each level by the computed ratio. So the sulfate aerosol concentration at each level was scaled with the same ratio but the concentration of the sulfate is not the same at each level because the background concentration is different at each level. In the next step, the box model was employed on the scaled sulfate aerosol concentration, and the scaled CCN profile was obtained. To clarify the vertical distribution of CCN more specifically, we add figure A1 in Appendix in the manuscript depicting the mean value of column SO2 concentration inside of plume in the lower, middle, and upper troposphere in OMPS retrievals along with the vertical profile of mean CCN concentration inside of the plume in the no-volcano and volcano run and outside of the plume in the no-volcano run for one specific vertical velocity (0.559 m/s) on 2 September 2014.



Line(362):

Vertical profile of activated CCN

The scaling of CCN was done by computing the distribution of scaling based on the enhancement of SO2 inside the plume relative to the mean SO2 value outside of the plume in the lower troposphere (up to 3km). Then the sulfate concentrations in the CAMS reanalyses inside of plume were scaled at each level by the computed ratio. So the sulfate aerosol concentration at each level was scaled with the same ratio but the concentration of the sulfate is not the same at each level because the background concentration is different at each level. In the next step, the box model was employed on the scaled sulfate aerosol

concentration, and the scaled CCN profile was obtained. To determine the vertical distribution of CCN more specifically, figure A1 shows the mean value of column SO2 concentration inside of the plume in the lower, middle, and upper troposphere in OPMS retrievals along with the vertical profile of mean CCN concentration inside of the plume in the no-volcano and volcano run and outside of the plume in the no-volcano run for one specific vertical velocity (0.559 m/s) on 2 September 2014.
Figure A1 in the manuscript.

In addition, I'm not sure about the treatment of aerosols in the model. In lines 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 SO2 as 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.

We added an additional sentence to now clarify how exactly this is done: "This is implemented by a simple prognostic equation for the CCN concentration that considers a sink for CCN at droplet activation and a source by relaxation to the prescribed CCN profile, advection is not computed."

Line(161-162):
This is implemented by a simple prognostic equation for the CCN concentration that considers a sink for CCN at droplet activation and a source by relaxation to the prescribed CCN profile, advection is not computed.

In addition, I cannot find the answer from the authors to my comment:
Specific comment for Section 2.2.: As well as the SO2, 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 SO2?

In the method used in this study, only emissions of SO2 were considered and the emission of water vapor from the volcano hasn't been taken into the account. We add this point to the manuscript to clarify this point.

Line (190):
It should be mentioned that in this study, the emission of water vapor from volcanic eruption hasn't been taken to account.

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

The phrases “(factual and counterfactual)” are removed.

Line (213)

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).

Thank you so much for pointing it out. The caption is revised to add the source of satellite images.

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.

The paper is cited in the manuscript as an example of ARG-parametrization.

Line (143)

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 the 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.

One needs to dig deeper into processes in the microphysics scheme to be able to explain in detail why this exaggeration in the enhancement of LWp has occurred, which this investigation has not been done in this study. But we can mention some hypotheses, for example, the finer horizontal resolution may help to improve the estimated enhancement in LWP. In addition, modifying the autoconversion rate in the microphysics scheme could lead to a better estimate of the suppression of light rain (drizzles).

Response to referee comments #2

The authors sufficiently addressed my previous review comments. I think the manuscript is acceptable once the following very minor points are revised. The authors do not need to make responses to the comments.

We thank the reviewer for their assessment of our manuscript. The review helped to improve the manuscript significantly.

Minor comments:

Ln. 81–83. The sentence is a little bit confusing. I suggest simply saying, "The Tiedtke (1989) shallow convection scheme contributes to the computation of specific cloud water and ice content.", if my understanding is correct.

The sentence is now revised consistent with what is suggested.

Lines(81-82): the Tiedtke (1989) shallow convection scheme contributes to the computation of specific cloud water and ice content.

Ln. 245: "and despite the simulated change in LWP" I think this phrase is unnecessary here in because the discussion on LWP starts from the next paragraph.

The phrase is removed.

Line (248)

Line

Ln. 262: ")" should be after "6"

Revised

Line (263)

Section 5 Conclusions: Some sentences are awkward.

We edit some sentences in the conclusion part in order to improve readability.

Line (313): Conclusion section.