We would like to thank both reviewers for their careful assessment of our manuscript and for their constructive comments. We have addressed these in our responses below and have revised the manuscript according.

The reviewers' comments are repeated below in black font, while our responses are in blue. The line numbers in the responses refer to the revised manuscript. Revised text from the manuscript is added in italics.

In addition to the revisions in response to the referees' comments, we have applied a number of minor changes to the text for streamlining, clarification and stylistic improvement. These are all highlighted in the manuscript version with tracked changes.

Referee #1

This manuscript investigates the cloud response to volcanic aerosol in the ICON-ART model. The investigation includes simulations of the initial stages of two volcanic eruptions, the 2014-2015 Holuhraun eruption and the 2021 La Soufrière eruption. The two eruptions are different in nature as Holuhraun emitted almost exclusively SO2 and the aerosol was injected mainly into the lower trophosome, while the La Soufrière eruption consisted of both SO2 and ash particles and reached the upper troposphere and stratosphere. The simulations of the Holuhraun eruptions are mainly used to investigate changes in liquid cloud and rain properties in response to the increased number of CCN from the eruption. The results indicate a strong increase in the cloud droplet number concentration and a decrease in warm rain processes in the clouds impacted by the volcanic aerosol. In the simulation of the La Soufrière eruption the changes in ice cloud properties in response to both the ash and the sulphate aerosol is investigated. The model simulations show that the ash particles can act at INPs and reduce the ice crystal number concentrations by supressing homogeneous nucleation.

The manuscripts address relevant scientific questions that are in the scope of ACP. The investigation, in particular the parts investigating ice cloud responses to volcanic aerosol in ICON-ART are novel. The scientific methods are valid and mostly described in sufficient detail. The findings of the investigation are interesting there is good interpretation and analysis of the results. The figures are clear, and the structure and language of the manuscript is of good quality. I recommend the manuscript publications after the comments below has been addressed.

Major comments:

The description of the model simulations is generally good but some descriptions of the volcanic emissions are vague and not explained properly. On page 4 line 124 it says "ash particles are given predefined small values". And again on line 125 it says "source strength of both ash and SO2 is zero, and small predefined background values are considered". Small and background is also mentioned a few times in the results section. The term "small" is not very quantitative. I would like the authors to explain these predefined and background values in more detail. What levels are these values at, and why were these not set to zero?

These background values are used per default in ICON-ART for numerical reasons. Specifically, the log-normal aerosol size distribution and nucleation parameterisations implemented in ICON-ART cannot be evaluated at a number concentration of zero. Therefore, ICON-ART considers a fixed number concentration of 100 kg^{-1} per mode. While these values have practically no big effect on CCN activation or INP nucleation in comparison with the high source of volcanic aerosols, they must be included in the simulations to prevent the occurrence of infinite values.

In the revised manuscript, we have inserted this information in line 128: "*However, the source strength of ash is assumed to be zero, though ash and sulfate particles are given predefined background values of a number concentration of 100* kg^{-1} *per mode for numerical reasons.*"

From the General Setup section I understand that there is only sea salt aerosol, sulphate and ash considered in the model. This would make the conditions in the model very clean if no anthropogenic or land-based aerosol sources are included. Is there a particular reason to inly include sea-salt aerosol? Why was this compound chosen? There is some mention in the paper that only including this aerosol does impact the results of the study but I think perhaps this could be a bit extended.

We acknowledge that sulfate, originating from marine precursor emissions such as SO2 and DMS, is always present in the marine atmosphere. There are also some marine organics. Continental sources include anthropogenic emissions, organic matter from vegetation, Icelandic dust and long-range transport. However, we do not expect these to be as significant as sea salt, the dominant natural aerosol in remote oceanic regions. Consequently, we only considered sea salt to be the background aerosol.

We have inserted a sentence in section 3.1 (line 167) for clarification: *"Further primary and secondary sources, such as continental aerosol emissions or gaseous precursors from the oceans, are neglected in order to investigate the effects of volcanic emissions compared to the dominant natural marine aerosol."*

That no dust is included should means that there are no other INPs available. Nevertheless, on page 12, line 274 you write that "along with the heterogeneous freezing driven by background INPs". What are these background INPs?

The predefined background values of 100 # kg^{-1} per mode are considered for all the aerosols in the simulations. In simulations involving the emission of ash particles, the number of INPs is calculated from the number of volcanic particles. However, in simulations where volcanic ash is not considered, such as the Holuhraun simulations and the La Soufrière NO-VOLCANO and VOLCANO-NO-ASH simulations, these predefined values are used to calculate INPs. Therefore, the term 'background INP' refers to these values. Although these numbers are very small compared to the number of volcanic ash particles, they may still affect heterogeneous ice nucleation. Figures 10a and 10b in the paper show the numbers of ash particles and heterogeneously formed ice particles. However, plotting these figures on a logarithmic scale (see Fig. R1 below) shows that the numbers for the NO-VOLCANO and VOLCANO-NO-ASH simulations are not zero, but are much lower than the results for the VOLCANO simulation.

The Holuhraun volcano did not emit ash particles; however, we used the same configuration model setup for both case studies. Therefore, the predefined ash values in the code were also considered in the Holuhraun simulations. Nevertheless, our results showed that these values had little effect on ice nucleation in the Holuhraun case. Figure R2 shows the number of ash particles and heterogeneously formed ice particles in the VOLCANO and NO-VOLCANO simulations of the Holuhraun eruption. The number of ice particles reaches 1e4 m^{-3} (not shown), while the number of heterogeneously formed ice particles is at maximum 1e2 m^{-3}.

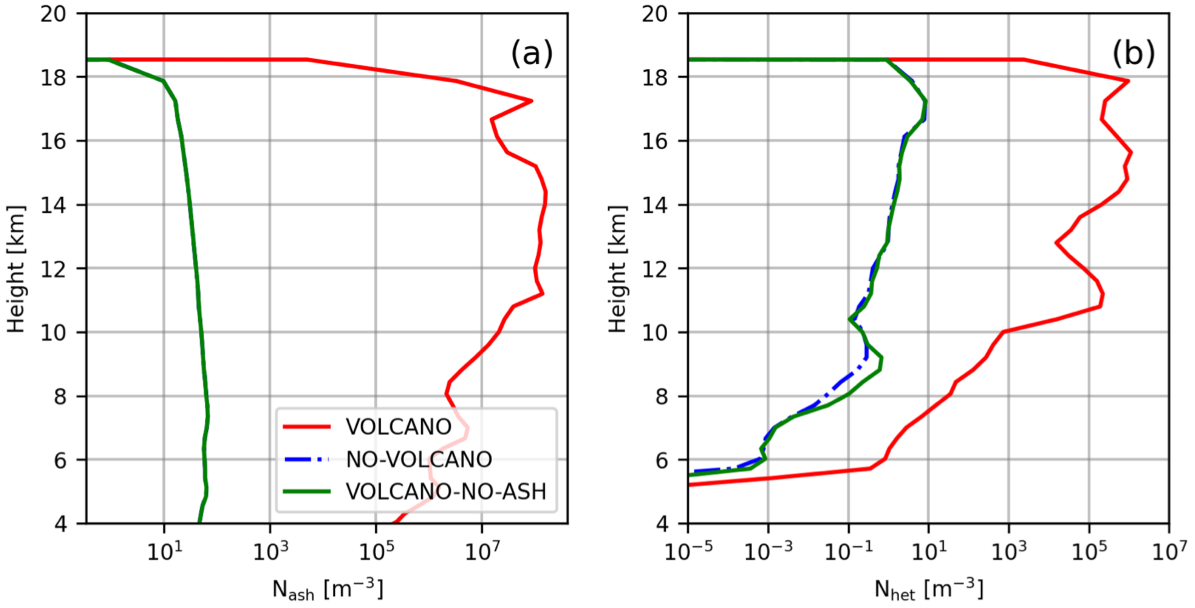

Figure R1: Number concentration of ash particles (a) and heterogeneously formed ice particles (b) in VOLCANO (red), NO-VOLCANO (blue), and VOLCANO-NO-ASH (green) for La Soufrière. Same as Fig. 10a, b of the manuscript, but with a log scale of the x-axis.

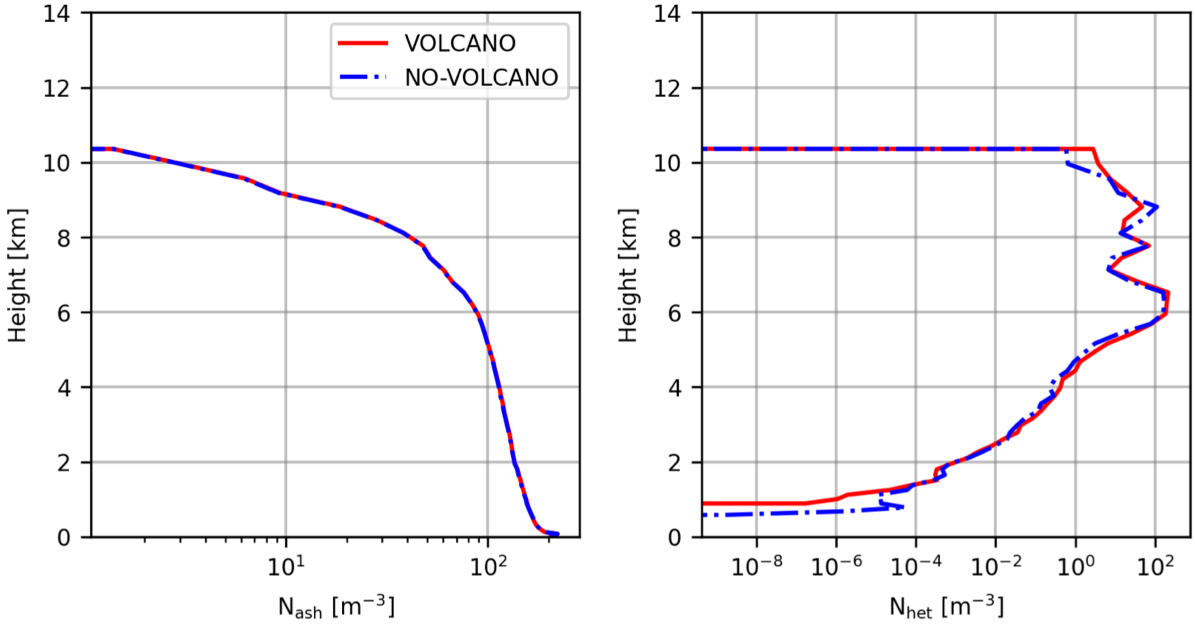

Figure R2: Number concentration of ash particles (left) and heterogeneously formed ice particles (right) in VOLCANO (red) and NO-VOLCANO (blue) of the Holuhraun eruption.

Minor comments:

Page 4, line 115: It would be good to mention the altitudes that the volcanic plume reached in the case description, in particular for the La Soufrière eruption but also for the Holuhraun eruption.

We have added this information for Holuhraun in line 98: "*Arason et al. (2015) stated that the maximum plume height close to the eruption site has mainly been in the range 1-3 km above ground and the middle of the plume few kilometers from the eruption site often about 1 km above ground.*"

And in line 118 for La Soufrière: *"Most plumes spread near the tropopause at 16–17 km or penetrated the stratosphere at 18–20 km, with overshooting tops reaching up to 23 km (Horváth et al., 2022)."*

Page 4, line 116: Please add the length of the La Soufrière simulations here in the case descriptions.

Added in line 119: *"This study simulated the first four days of the eruption because most of the eruptive pulses occurred during that time."*

Page 5, line 123: What does "Second" refer to here?

We have removed "Second,".

Page 6, line 150: Since Figure 7 is the second figure to be referenced in the manuscript, should it be called Figure 2?

Since Figures 1-6 refer to Holuhraun and Figures 7-13 to La Soufrière, we prefer to keep the order as is because this allows a better overview. Note that we have revised all captions by clearly stating "Holuhraun" and "La Soufrière" to enhance readability.

Page 9, line 230: "we" should be changed to "We".

Done.

How far from the domain boundaries did you exclude data?

We have added this information to the paper in line 250: *"We excluded data near the domain boundaries (2.5° in each direction) from our analysis."*

In detail, this results in the following boundary zones: The Holuhraun domain extends from 50–80°N and 40°W–20°E. In the simulations, the results were remapped onto a regular latitude–longitude grid (instead of the native triangular grid) with a resolution of 0.1°. This corresponds to a grid of 301 × 601 points. For the analysis, 25 grid points were excluded from each boundary. Since 1° of latitude corresponds to approximately 111 km, the excluded distance in the north–south direction is: 25 × 0.1 × 111 ≈ 278 km. In the zonal (east–west) direction, the distance per degree depends on latitude and is given by 111 × cos(φ). Using 65°N as the midpoint of the domain (50–80°N), this yields: 25 × 0.1 × 111 × cos(65°) ≈ 117 km. Thus, approximately 278 km were excluded from the northern and southern boundaries, and about 117 km from the western and eastern boundaries (on average across the domain).

The same approach was applied to the La Soufrière domain (0–26°N). The meridional exclusion remains approximately 278 km. Using 13°N as the midpoint latitude, the zonal exclusion is: 25 × 0.1 × 111 × cos(13°) ≈ 270 km.

Page 11, figure caption for figure 3: It seems a bit strange to explain the sub figures in the order a, b, d, e, c, f. Please consider changing this.

We have modified the text so that panels c and f are discussed together. However, the order of mention in the text is still not alphabetical; as we think that it is beneficial to show the number and mass concentrations next to each other, respectively, we prefer to keep the arrangement of the panels as is.

Page 15, line 339: I agree that the ice distributions are rather symmetric but for the snow data (Fig 8b and d) the mean values and median values are rather far away from each other, in particular in fig 8d.

Agreed. We have changed the text accordingly.

Page 16, figure caption for figure 8: Should there be a space after the comma in "(a,b)"? This is missing in several figure captions.

Thanks for pointing this out. We have corrected the captions.

Referee #2

This study examines the response of cloud microphysical processes to volcanic aerosols by simulating two volcanic eruptions: the 2014–2015 Holuhraun eruption, in which warm-phase and mixed-phase clouds dominate, and the 2021 La Soufrière eruption, where ice clouds are more prevalent. By conducting sensitivity experiments with and without volcanic aerosols, the authors quantify how the number concentrations of cloud droplets and ice crystals, along with associated precipitation processes, respond to enhanced volcanic aerosol loading. Their results show that in warm clouds, cloud droplet numbers increase while rain formation processes are suppressed. In contrast, for cold-phase clouds, the total number of ice crystals decreases due to the activation of ash particles, which deplete water vapor and suppress homogeneous ice nucleation. The study also explores the potential role of mixed-mode aerosols acting as CCN. Overall, the paper is well written and organized, and easy to follow. The study provides valuable insights into the process-level understanding of aerosol effects on both warm- and cold-phase clouds through simulations of natural volcanic events. However, some clarification is needed regarding the model configuration and the potential co-influence of meteorological factors when interpreting aerosol effects. If these concerns, along with the issues outlined below, are addressed, I believe the paper would be suitable for publication in ACP.

Thanks for the constructive comments. We believe that the added clarifications have significantly improved the manuscript.

Major comments:

1. A major concern is the very large initial bias in cloud droplet number concentration in the Holuhraun simulation. The authors state that the original simulation produced a relative enhancement (RE) of 6167%, compared with 42% from MODIS observations, and that tuning reduced the modeled RE to 1219%. Although this represents an improvement, the remaining discrepancy is still substantial. This suggests that the aerosol perturbation associated with the volcanic eruption may still be overly strong in the simulation.

We agree that the fact that the simulated relative enhancement (RE) of cloud droplet number concentration remains higher than observed, even after tuning, is unsatisfactory. One factor contributing to the high RE is probably the simplified background aerosol configuration. In our setup, sea salt is the only background aerosol species, which likely leads to an underestimation of CCN concentrations outside the plume. As a result, the contrast between plume and non-plume regions and therefore RE is overestimated. The table below shows the mean total column N_c for the simulations before and after adjustment, as well as for MODIS.

	VOLCANO_In-Plume_Before	VOLCANO_In-Plume_After	VOLCANO_Out-Plume_Before	VOLCANO_Out-Plume_After	MODIS_In-Plume	MODIS_Out-Plume
Mean	2.25e8	3.51e7	3.59e6	2.66e6	8.78e6	6.16e6

As can be seen, the adjustment reduced the mean by an order of magnitude within the plume. This removes very extreme values and makes the distribution less skewed. The causes for the remaining uncertainty are discussed at the end of section 3.2, and have been extended by the following sentences (lines 226-229): "*However, as shown in Fig. A1 of the manuscript, this discrepancy can be attributed mostly to an overestimation of N_c inside the plume, while the mean N_c outside the plume only deviates about a factor of 2 from the observations. The concentrations inside the plume are driven by the strength of the prescribed volcanic emissions, which are associated with a large uncertainty (Schmidt et al., 2015). To remain consistent with previous studies, we have decided not to tune the source strength.*"

This raises the question of whether the simulated reduction in warm-rain processes (or the increase in cloud water shown in Figure 3d) could partly be an artifact of an excessively strong activation response. In particular, previous observational studies reported no detectable or only minor changes in liquid water path during the Holuhraun eruption (Malavelle et al., 2017; Haghighatnasab et al., 2022). Please discuss how the remaining bias in droplet number concentration may influence the interpretation of the results and the robustness of the conclusions.

Figure 3d shows the vertical profile of cloud water in cloudy pixels, which cannot be directly compared to the LWP changes discussed in previous studies. To assess the impact of the bias in N_c on our conclusions, we performed an additional analysis of liquid water path (LWP). Figure R3 shows the relative frequency of LWP inside and outside the plume before and after adjustment for VOLCANO and NO-VOLCANO simulations.

For each case, the mean and median are shown in the figure legend. It can be seen that the means and medians for both simulations inside the plume converge through the tuning of the CCN activation scheme. The histograms show that the LWP for NO-VOLCANO (green) is closer to that for VOLCANO (cyan) after adjustment, with a relative enhancement of only 118% compared to the 1219% for N_c. These results are consistent with previous studies indicating that the volcanic plume has a minor impact on LWP. In addition, Haghighatnasab et al. (2022) state that “the almost unchanged LWP on average is a result of some LWP enhancement for thick clouds and a decrease for thin clouds”. Examining this figure confirms that the LWP in our simulations does not differ significantly between the VOLCANO and NO-VOLCANO simulations at lower values. However, for the rare occurrences of larger LWP values (>500 g m^{-2}), the plume's effect on LWP is considerable.

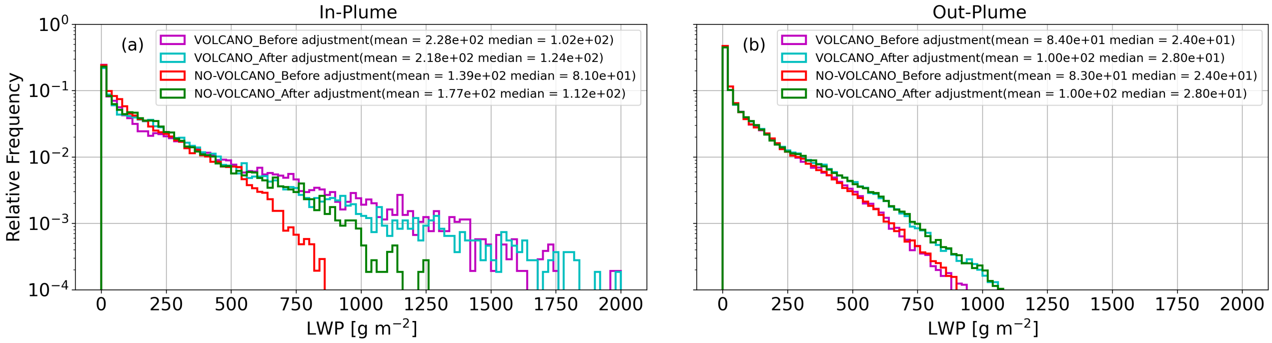

Figure R3. Relative frequency of LWP for VOLCANO and NO-VOLCANO simulations for Holuhraun inside (a) and outside (b) of the plume before and after adjustment.

Figure R3 has been added as a new Fig. A2 into the appendix of the manuscript, and the corresponding text in lines 230-235.

In addition, was a similar tuning applied to the simulations for the La Soufrière case?

The same tuning of the CCN activation was used in the case of La Soufrière, but played a smaller role as liquid clouds are not strongly affected by the plume. This was added in line 217: *“The same modifications of the CCN activation parameterization were also applied to the La Soufrière case, although liquid clouds were less prevalent there.”*

Moreover, for the background aerosol setup, are the assumed aerosol concentrations based on observational constraints? If not, it is possible that the large bias in RE partly originates from the background aerosol configuration. This aspect should also be discussed.

The background sea salt aerosol concentrations are calculated prognostically based on a wind-driven emission parameterization. This information was added in line 165: *“this study focuses on sea salt*

(Na$^+$ and Cl$^-$ as the background aerosol (calculated prognostically from parameterized emission functions)". Unfortunately, no in-situ observations of the sea salt aerosol concentrations are available to constrain them. For a discussion of the possible contributions of other, not considered aerosols outside the plume, please see above.

2. Since this study aims to understand aerosol effects, it is important to clarify how the influence of meteorology is controlled across the simulations. Did the authors apply any nudging strategy (e.g., toward reanalysis data) to constrain the large-scale wind fields and ensure comparable meteorological conditions among the simulations? If not, please compare the wind fields across the simulations, particularly the vertical velocity within the plume region, as differences in dynamical forcing could strongly influence cloud development and precipitation processes.

All simulations were performed with identical initial and boundary conditions, and no nudging was applied. Through the boundary conditions, large-scale meteorological conditions are consistent across simulations, and the only difference is the aerosol configuration. Figure R4 shows the PDF of upward vertical velocity inside the plume region in the VOLCANO and NO-VOLCANO simulations, which are almost identical.

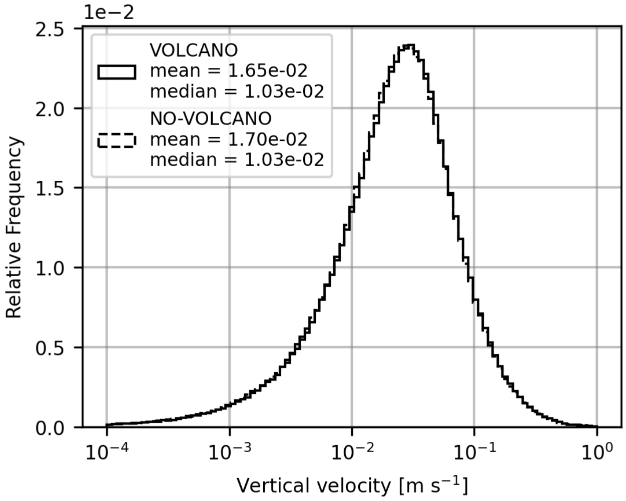

Figure R4. Relative frequency of upward vertical velocity inside the plume for VOLCANO and NO-VOLCANO simulation.

Also, in the Holuhraun simulations the authors seem to attribute the differences in the behavior of cloud and rain inside and outside the plume primarily to volcanic aerosol effects, while ignoring the regional variations in meteorological factors. To more convincingly isolate aerosol impacts, it would be helpful to examine whether key meteorological factors (e.g., free-tropospheric moisture, lower-tropospheric stability, large-scale subsidence) differ between the plume and non-plume regions.

We agree that it is likely that meteorological factors contribute to the differences between in-plume and non-plume regions. To assess this potential influence, we compared in Fig. R5 key variables inside and outside of the plume region, including free-tropospheric moisture- between 500 and 700 hPa- (a), lower-tropospheric stability (LTS)- potential temperature at 700 hPa and 1000 hPa- (b), and vertical velocity between 500 and 700 hPa (c). Free-tropospheric moisture is higher outside the plume, which may contribute to larger cloud droplets and therefore stronger autoconversion and accretion processes and enhanced rain formation in that region. Meanwhile, LTS is shifted to larger values outside the plume region. Vertical velocity distributions are very similar inside and outside of the plume.

To reflect these findings, we have modified the text in line 270: *"Nevertheless, even without volcanic emissions, N_c is higher inside the plume area, possibly due to the enhanced presence of sea salt aerosols in those regions or to meteorological factors like differences in humidity and stability (not shown)."*

We decided not to include further details into the manuscript because this would distract from the more relevant discussion of the differences between the VOLCANO and NO-VOLCANO simulations.

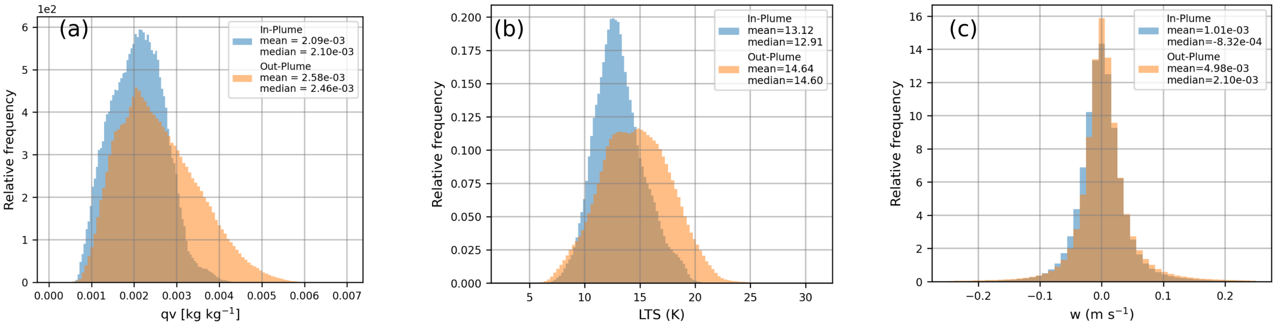

Figure R5. Relative frequency of qv between 500 and 700 hPa (a), lower tropospheric stability (b), and upward and downward vertical velocity between 500-700 hPa (c).

3. The authors conducted the VOLCANO-MIXED simulation for the La Soufrière case to examine the activation of mixed-mode aerosols as CCN. However, the results indicate that ice clouds dominate in this case, with relatively limited involvement of warm-phase cloud processes. Since CCN activation mainly affects warm-cloud microphysics, it may be more appropriate to conduct the VOLCANO-MIXED experiment for the Holuhraun case, where warm-phase clouds are more prevalent. The authors should discuss whether testing it in the Holuhraun simulations would be more meaningful.

We agree that the effect of mixed-mode aerosols is most relevant in warm-cloud regimes. However, the Holuhraun eruption emitted negligible ash, resulting in very low concentrations of mixed-mode aerosols. Therefore, a VOLCANO-MIXED experiment would not produce meaningful results in that case. The La Soufrière eruption, which emitted both ash and sulfate, provides a more suitable framework for testing this process. However, as discussed in the manuscript, ice clouds dominate in this case, limiting the impact of CCN activation. It can be suggested that future studies investigate eruptions that combine strong ash emissions with a significant presence of warm clouds.

4. Please provide more details on how the Mann-Whitney U test was applied to compare the two simulations. For example, the authors should clarify the statistical basis of the test, the temporal and spatial scales of the analyzed data, and the effective sample size used in the analysis. It would be helpful to include a brief description of these aspects in the Method section.

Thanks for this suggestion. Instead of placing it in the Methods section, we have decided to include the description into section 4.1.3 (Statistical Test) and have modified the text to read as follows:

"Our results show differences in cloud microphysical processes and hydrometeors in warm and mixed-phase clouds between the VOLCANO and NO-VOLCANO simulations. To assess the statistical significance of these differences, we applied the non-parametric Mann–Whitney U test (Mann and Whitney, 1947), which determines whether two independent samples originate from the same distribution without making any assumptions about normality. Differences are considered statistically significant when the p-value is below 0.05. Due to the large number of grid points, the

data were first averaged spatially over the vertical and horizontal dimensions, with the analysis restricted to cloudy grid points within the plume region. This produced time series of domain-averaged values, with one sample per time step. The Mann–Whitney U test was then applied to these time series. Figure 6 shows the resulting p-values for the mass mixing ratios and number concentrations of all hydrometeors over six days of simulation. On the first day, significant differences were only observed for cloud droplet number concentration (Nc), indicating an early aerosol impact on this variable. As the volcanic plume evolves, statistically significant differences emerge for cloud water (Nc and qc), rain (Nr and qr), and graupel (Ng and qg) between the VOLCANO and NO-VOLCANO simulations. Conversely, no statistically significant changes were found for cloud ice (Ni and qi) and snow (Ns and qs), suggesting these hydrometeors are less sensitive to volcanic aerosol perturbations under the simulated conditions."

Technical suggestions:

- Figures 6 and 13: To make these figures more informative, I suggest adding the magnitude of the changes, or at least the sign of the changes, for each bin so that readers can more easily interpret the relevant processes.

We have replaced the previous figures by the following new versions, in which the color represents the relative change instead of p. Black boxes denote p-values <0.05. The figure captions have been modified accordingly.

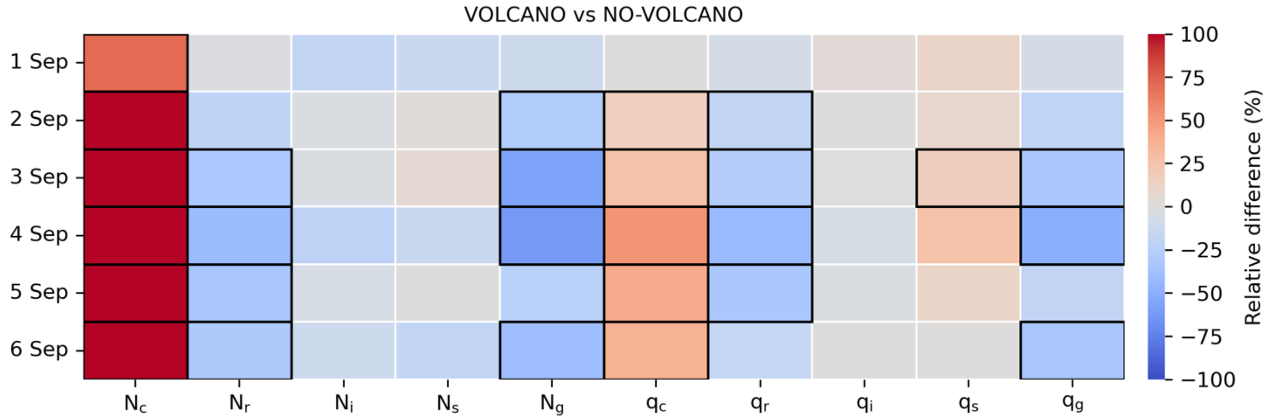

Fig. R6: Relative differences (VOLCANO minus NO-VOLCANO divided by NO-VOLCANO) and statistical significance of daily changes between the VOLCANO and NO-VOLCANO simulations for Holuhraun inside the plume. Colors indicate the relative difference, while black boxes denote statistically significant differences $p < 0.05$ in cloud hydrometeors for each day (y-axis).

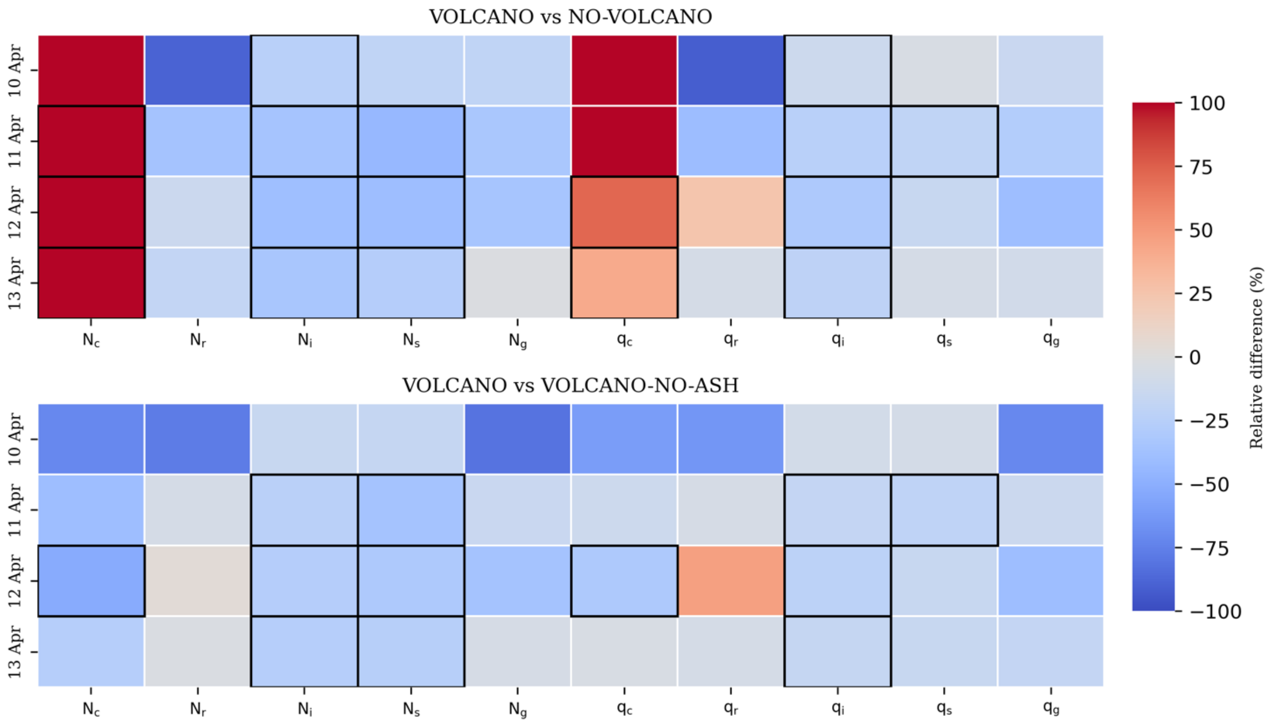

Fig. R7: Relative difference and statistical significance of daily changes in VOLCANO and NO-VOLCANO simulations (top) and VOLCANO and VOLCANO-NO-ASH simulations for La Soufrière inside the plume. Colors indicate the relative difference, while black boxes denote statistically significant differences $p < 0.05$ in cloud hydrometeors for each day (y-axis).

- Some case descriptions and modeling setup details could be slightly tightened to reduce repetition between the Case Studies, Results, and Conclusions sections.

Thanks for pointing this out. We have shortened the case description by removing references to the expected impacts, and the conclusions by shortening statements which can be considered as background information. Please see the version with tracked changes.

The manuscript may benefit from a summary figure for each case that illustrates the proposed causal chain. For example, for Holuhraun, a schematic linking sulfate, increased cloud droplets, suppressed warm-rain processes, and reduced graupel would be helpful. But whether to include this figure is up to the authors.

Thanks for this suggestion. We've added the following schematic as Fig. 14 of the manuscript:

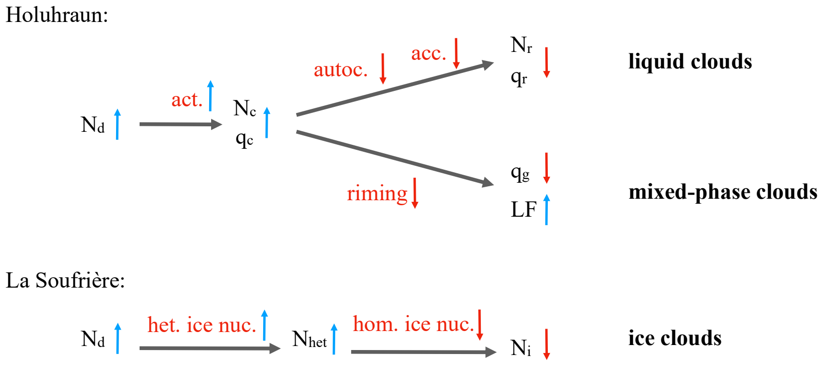

Fig. R8: Schematic of the causal chains identified for aerosol-cloud interactions in the Holuhraun and La Soufrière simulations.

The correspondence line seems to contain a typo: “kit.deu”.

Thanks. Corrected.

Minor comments:

L245: Change “(d, f)” to “(e, f)”

We actually meant Figures 2d and f which show the enhancement in cloud droplets. Panel 2e shows sulphate. But to avoid confusion, we only refer to Fig. 2f (the difference in cloud droplet number) now.

L229: Just curious how sensitive the conclusions are to the choices of the threshold. If it does not require substantial effort, it would be helpful to test the robustness of the results to such choices.

To assess the sensitivity of our results to the chosen cloud threshold, we conducted an additional test using a lower criterion of qi+qc>1e-6 kg/kg. The resulting vertical profile of Nc for Holuhraun (corresponding to Fig. 3a) is shown in Figure R9. As illustrated, reducing the threshold does not modify the overall structure or pattern of the profile. However, there is a modest quantitative impact, with maximum values decreasing slightly from approximately 600 to 520 for the VOLCANO simulation in-plume.

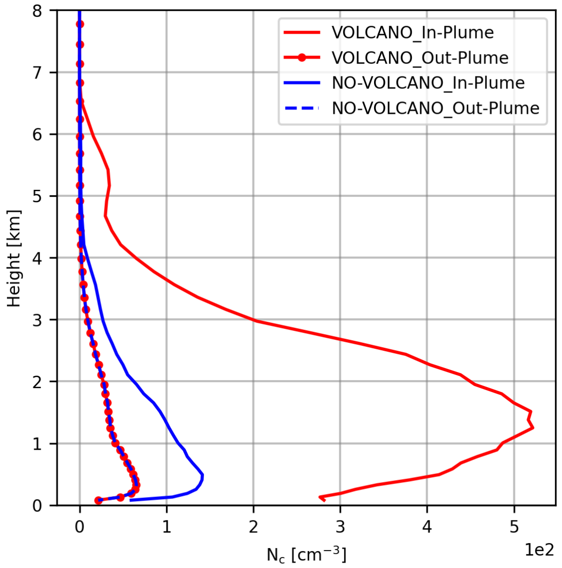

Figure R9. Spatiotemporally averaged profile of Nc, similar to Figure 3a in the manuscript, but with threshold qi+qc>1e-6 kg/kg

We have therefore inserted the following sentence in line 245: *“Sensitivity tests (not shown) with a lower threshold of 10^{-6} kg kg^{-1} have given very similar results.”*

L339: But it is not close for snow. Please soft the tone.

Agreed. We have changed the statement for snow.

L411: The larger number concentration of ice crystals between 10 and 12 km in the VOLCANO run compared to other runs seems not significant.

This is correct, but as this signature is consistent with ice crystals formed via immersion freezing, we decided to keep this statement. However, the wording was adjusted as follows: *"At altitudes between 10 and 12 km, there is a local maximum in the profile of the VOLCANO simulation that was previously observed in Figure 10b. This small peak indicates immersion freezing. As even the reduction of* $Smax_{ice}$ *cannot diminish its effect, at these altitudes, the VOLCANO simulation has a slightly larger number concentration of ice crystals than the other experiments."*

L415: Please elaborate how water mass conservation is applied to Figure 10f.

The principle of mass conservation is applied to update the number of ice crystals by linking the number of newly formed ice crystals calculated from the Barahona and Nenes (2009b) scheme to the available water vapor mass and a prescribed minimum mass per crystal. First, the number of nucleated particles is determined by calculating the difference between the number of newly formed ice crystals and the existing ice number concentrations. This number is then converted into a corresponding mass requirement using the minimum crystal mass. Since ice crystal formation cannot exceed the available water vapor, the actual ice mass increment is limited by the available vapor using a minimum condition. This ensures that vapor depletion does not exceed the physically available water. The ice mass mixing ratio is then increased by this constrained amount, and the maximum allowable number of crystals consistent with the updated mass is determined. Finally, the ice number concentration is limited by this mass-based bound to ensure consistency between mass and number. Thus, the scheme maintains a physically consistent relationship between ice mass and number concentration while conserving total water mass during nucleation.

In Line 442, we have summarized the above explanation as follows: *"After a check for a water mass conservation to ensure that the newly nucleated particles predicted by the ice nucleation scheme after Barahona and Nenes (2009b) are consistent with the actually available water vapor, the number of ice crystals formed in the upper troposphere above 14 km is reduced to the values shown in Figure 10h."*

L428: How is Figure 11a different from Figure 10f?

Figure 10f shows the number concentration of ice crystals produced directly after the ice nucleation scheme. In contrast, Figure 11a represents the resulting ice crystal number concentration after all relevant microphysical and transport processes have been applied. Following their formation, ice crystals are modified by additional processes, including ice production through the freezing of cloud droplets, removal through conversion into other hydrometeors such as snow and graupel, and transport by sedimentation, advection and diffusion. Therefore, Figure 11a reflects the net ice crystal number concentration after accounting for all sources and sinks, whereas Figure 10f isolates the contribution from ice nucleation alone.

We have modified the explanation in line 453 and have removed the distinction between ART and ICON, which may have been confusing: *"After ice crystals form both through homogeneous and heterogeneous nucleation (see Fig. 10h) and through rain or cloud droplet freezing, they grow (or shrink) through the deposition of water vapor and subsequently undergo transport by sedimentation, advection and diffusion."*