We thank Dr. Barsotti for the constructive comments and suggestions, which helped us a lot to improve the manuscript.

We decided to re-run our simulations due to different comments in all three reviews. This leads to slightly different results, but does not change the main arguments of the paper. Please find our answers to your comments below (Reviewer's comments in **bold**, our replies in standard font, and modifications of the text in blue).

- **please clarify somewhere in the text if the plume height is intended above sea level or above ground.**

  We clarified plume heights above sea level and above vent where necessary. Additionally, we added in the Methods section (L. 181-182):

  As FPlume requires the plume height above the vent, we converted the GOES-17 above-ellipsoid heights by subtracting a vent height of 550 m.

- **the multiple reference to Muser et al. 2020 paper, makes the reading sometime difficult and incomplete**

  We changed to the abbreviation M20 instead of Muser et al. 2020.

- **I'm worried there is some confusion about the use of the word „plume" which often is also used instead of „cloud". Please clarify throughout the text so that the readability and the understanding of the results will be improved.**

  When we refer to 'plume', we always mean the part originating from the volcanic emission. Thus, it is also the ash and SO2 that is spreading in the atmosphere. We only want to use the word 'cloud' in a meteorological context. However, we double checked our manuscript and had to corrected some sentences in which we used the word 'cloud' instead 'plume', which was indeed inconsistent in the original manuscript.

- **Please be consistent throughout the text if you refer to ash or fine ash.**

  Done.

- **Please in the abstract specify how long lasted the eruption.**

  Done (L. 3):

  The eruption was characterized by several eruption phases of different duration and height summing up to a total eruption length of about 5.5 h.

- **Line 7: what do you mean with „the simulated effect... is in the order of 6 km??“**

  We clarified and rearranged the sentence as follows (L. 70-71):

  „..the simulated lofting effect for the Raikoke eruption resulted in a 6 km rise of the plume top after the first 4 day.

- **Line 103: 7 in word**

  Done (L. 104).

- **Line 126: ... by Marti et al. (2017)**

  Done (L. 127).

- **Line 135: please provide references for this statement.**

  We added L. 139 to the main text and L. 523-524 to the reference section:

  … (e.g., Thomas and Prata, 2011). (L. 139)

  Thomas, H. E. and Prata, A. J.: Sulphur dioxide as a volcanic ash proxy during the April–May 2010 eruption of Eyjafjallajökull Volcano, Iceland, Atmospheric Chemistry and Physics, 11, 6871–6880, https://doi.org/10.5194/acp-11-6871-2011, 2011. (L. 523-524)

- **Line 142: what do you mean with: „... to ensure an uninterrupted simulation?“**

  We decided to re-run the simulation based on different arguments of the reviewers. One argument was to initialize in the FPlume experiments also the eruption phases <10km with FPlume, but with a higher exit velocity (instead of Mastin equation for the MER calculation). Thus, we now could remove the sentence that you commented as unclear.

- **Paragraph 2.3: the Radius of the vent is also part of the equation, so I‘m wondering which value or range of values you adopted here?**

  In FPlume, the vent radius is not an input parameter. The following equation relates the MER M_0, the vent radius r_0, the plume density at the vent ρ_0, and the exit velocity u_0 (Folch et al., 2016 and FPlume code):

  $$M_0 = \pi r_0^2 \rho_0 u_0$$

  M_0 is first calculated with the method of bisection in case the plume height is given: depending on whether in the loop step before the fixed (given) height is

overestimated or underestimated M_0 is either lowered or highered. Afterwards, r_0 is calculated with the equation above and further used for the calculations of the plume radius, the entrainment coefficients, and the umbrella height. Thus, we did not adopt values for the vent radius here.

- **Line 166: what Sc layer means?**

  Changed to stratocumulus layer in L. 177.

- **Line 176: please refer to Table 1**

  Done (L. 190).

- **Table 1: I'd suggest to make this table more complete. I'd add in the first column the day the phases refer to. I'd then also add a column specifying the Fine fraction flux (kg/s) or specify in the caption of the table that the flux for fine ash is shown in Figure 2. Please specify in the caption how the SO2 flux is estimated. Please explain the caption the source of all data showed in the table.**

  This table summarizes our input values for the simulation, which are fixed during each individual phase. Therefore, we only added a column with the day, specified in the caption that the flux for the very fine ash is shown in Fig. 2, and included how the SO2 flux is estimated. We cannot provide the very fine ash fraction flux, because this quantity is calculated online and varies between the time steps and not only between phases. We added to the caption of table 1:

  The definition of the phases and plume heights above sea level (a.s.l.) are based on GOES-17 satellite observation as described in sect. 2.3.2. The exit conditions are based on typical values of basaltic eruptions as described in Sect. 2.3.1. The SO2 mass emission rate is based on an observational estimate of the total SO2 mass following the 2019 Raikoke eruption from M20, which was distributed over the individual phases with Eq. 3 . This table only shows the values that are predefined and fixed for the individual phases. The temporally varying MER of the very fine ash, which is derived with FPlume and the relationship by (Gouhier et al., 2019), and which is released into ICON-ART, is shown in Fig. 2.

- **Line 186: Figure 2 shows only the MER and not the height**

  We corrected it (L. 201):

Fig. 2 shows the MER of very fine ash ...

- **Figure 2: Fine ash is <30 or <32 micron? Please correct the title of the plot. What means E on the y-axis? Please clarify. In the caption: ...calculated with Fplume MER times.....and calculated with Mastin MER times…**

  Done.

- **Line 200: H_T is not the plume height averaged over the entire eruption duration? Please clarify.**

  H_T is a quantity averaged over the entire eruption duration, however longer phases are weighted more strongly. We extended the description in the manuscript and added the underlying equation (L. 222-225):

  E_{ph} is the phase-dependent MER of SO_2, E_{SO2} is the mean MER based on the observed amount of SO_2 and the sum of the duration of all phases, E_{ph} is the phase plume height (above the vent), and H_T=11571.2 m is the phase duration-weighted mean plume height derived as:

$$H_T = \frac{\sum_{i=1}^{10} H_{ph,i} \cdot t_i}{\sum_{i=1}^{10} t_i}$$

- **Line 237: Please check the dates, aren‘t they 21 June and 22 June, instead?**

  No, the dates are correct. However, we added the UTC-times for clarification (L. 269):

  June 22, 0-23 UTC and June 23, 0-23 UTC

- **Line 239: (compare Fig. 3 top and bottom)**

  No, we here compare the horizontal spreading of ash and SO2.

- **Line 242: is the temporal evolution of ass loading on an hourly basis?**

  We only wrote output for the 3D field every full hour for the original version, although our time step is 60 s. More details are given in one of the comments

below.

- **Line 243: plateau? Is not a peak??**

  We rephrased the sentence as (L. 275-277):

  The Himawari-8 data reveals a steep increase of ash mass at 22 UTC on June 21 until a peak of 1.0 Tg is reached at 5 UTC on June 22 and the curve remains above 1.0 Tg for 5 hours. The maximum at 7 UTC (June 22) of 1.1 Tg is followed by a descent to 0.3–0.5 Tg.

- **Line 244: please start a new line when „Muser et al.…“**

  Done (L. 278).

- **Figure 3: Please double check the dates**

  The dates are correct here.

- **Line 246: please double check the timing, I guess it is 18 UTC of 21 June as the plot shows the hours since the June 21, 12UTC**

  Yes, we corrected the date (L. 280).

- **Line 251: in the bracket add: in Figure 4**

  Done (L. 284).

- **Line 254: here you refer to the effect of meteorological conditions. I guess it would be useful to add come vertical profiles of wind speed in correspondence of the vent.**

  We found your comment very helpful to further explain plume dynamics and to highlight the advantage of FPlume instead of Mastin for dispersion forecasts. Therefore, we placed Fig. A2 to the appendix and we added the following description (L. 207-213):

  The vertical profiles of the meteorological variables in Fig. A2a indicate increasing temperatures in most levels below 10 km during the long eruption phase between 9 and 14 h after simulation start. Additionally, the specific humidity increases by up to 1 g kg−1 in the lower 2 km (Fig. A2c). When warmer and moist air is entrained into the plume, the plume density reduces faster due to the lower ambient air density and the release of latent heat. This effect results in a higher positive buoyancy and a lower MER to reach a fixed height. In addition, the wind speed decreases in the lower 4 km between 9 and 14 h after simulation start, which reduces the plume bending and subsequently the MER needed to reach a fixed height.

- **Figure 4: Please be consistent with terminology: is this ash or fine ash? Are these hourly values? I'd much like to see the occurrence of pulses in these plots, to understand when the eruption dynamics affected the detection.**

  We corrected the title. Although our simulation time step is 60 s, the output of the 3-D fields (meteorological variables and tracers) of the simulations were written only every full hour and therefore the plot shows hourly values. We added the following sentence to the manuscript (L. 275):

  The temporal resolution of the data is 1 h.

- **Line 276: here you are suggesting that the fact that you are not describing the gravitational spreading occurring nearby the source is affecting your capability in reconstructing the SO2 forecast in its initial phase. Then please explain why this is not also impacting the quality of the ash forecast?**

  We realized that this sentence caused confusion. We removed it completely, as it is outside the scope of this paper.

- **Line 281: so from Figure 4 it turns out that the total amount of ash compares well with the satellite retrieval. However figure A2 shows a wider extent than what is retrieved. In this sense I'd have expected that this meant forecasted concentration lower than the observed and in this sense an Amplitude < 0 (as at Line 223-225 you say that when the model overestimates the parameter A is positive, and negative otherwise). But Figure 5 shows positive Amplitude for ash at all time intervals. Please explain.**

  The Amplitude indicates that the mean column loading in the whole domain considered. On the one hand, the higher amplitude values can be explained by the higher mean column loading within the modeled ash plume. On the other hand, we can see the impact of the zero values in the domain which cover more grid cells when the plume is more dispersed. We added to the manuscript (L. 317-320):

  The high amplitude value for ash between 12 and 36 h, despite the almost perfect agreement in the total mass in Fig. 4, also stems from the larger spread of the ash plume in the beginning. The reason is that the background values are considered zero and the Amplitude in the SAL analysis, unlike object-based structure and location values, is a domain-averaged quantity.

- **Line 285: here you say that S for ash in the beginning is negative. You justify the same results for SO2 saying that this is most likely due to the**

ash thick presence that obscure the gas component in the retrieval. What might be affecting the negative S for ash?

We removed this sentence because it is not visible in the data anymore after the re-run of the simulation.

- **Paragraph 3.3: please double check the dates you refer to…**

  We double checked the dates and confirm that these are correct.

- **Figure 6: please clarify how the different heights are obtained. Here I see grey heights, are they coming from Fplume or satellite? Please specify. How are you getting the different heights for SO2 and ash? Are you maybe referring to the cloud height?? Please clarify.**

  We added explanations of our methodology in the caption of Figure 3 as:

  The gray bars indicate the duration and height of the 10 individual eruption phases. They are based on the analysis of the GOES-17 data, which serve as inputs for FPlume.

  However, as stated above, we refer to the tracers originating from the volcanic emissions as the plume.

- **I guess the caption in Figure 7 and A3 partly clarifies it as it mention this „horizontally averaged vertical distribution of mass". Please spend more words in explaining the methodology, referring to Muser et al. 2020, is not enough. And use the words „plume" and „cloud" in a distinct manner.**

  We added the description of our methodology (L. 329-332):

  The plume top height in a) is defined as the maximum height of all grid cells in the plume that was separated from background mixing ratios as explained in Sect. 2.2.1. The average plume height in a) is the mean height weighted by the mass of all grid cells considered as inside the plume. The values in b) and c) were horizontally averaged over the whole detected plume,again excluding grid cells outside the plume. In b) and c), we picked June 23, 12 UTC, because it allows a direct comparison to Fig. 8 in M20, which only shows the ash plume top height.

  Concerning the usage of 'plume' and 'cloud', please see the comments above.

- **Line 358: I think the statement about the „dense ash plume" hampering the validity of the comparison needs to be demonstrated. Please rephrase the conclusion.**

We agree with the reviewer, that the conclusion is too strong here, as we only speculated in the result. Thus, we rephrased it (L. 398):

However, we hypothesize that the validation of the simulated ash and SO2 dispersion was partially hampered by a dense ash plume in the beginning of the eruption and by overlapping water and ice clouds later on.