Reply to Anonymous Referee #3

Thank you for your thorough examination of our manuscript. We have individually addressed each of your comments below. We also improved the quality of our figures and have made every effort to produce better plots.

Reviewer's comment: "*I would therefore propose the following structure of the paper. 1. As already done the results of the satellite retrievals during the Redoubt volcano eruption should be presented to constrain the input parameters used later on for the sensitivity runs. 2. A sufficient explanation of the radiation scheme should be given. A detailed explanation of the input data that was used for the radiative transfer should follow. 3. Then a well-structured presentation of the results of the sensitivity runs should be given*."

Reply: We have re-structured the manuscript and have added more detail about the radiative transfer model (see section 2.3). The satellite results are consolidated in new section 3.2 of the revised manuscript. We have also removed repeated text in section 3.3 and deleted old section 2.2.

Reviewer's comment: "*I do not see the necessity to include the results of the HYSPLIT model especially as long as they show only poor agreement with the observations.*
This would also help to shorten the paper to make it easier to read."

Reply: The HYSPLIT image erroneously used in the satellite data comparison was an image from another day. We have replaced this image with the correct one, which shows good agreement with satellite data. We also tried to address the reviewer's suggestion for shortening and enhancing the readability of the paper. We have restructured section 2 by deleting section 2.2 and moving it to the beginning of section 2. Additionally, we decided that the inclusion of figure 3 is not absolutely vital and have removed it to condense the number of figures and shorten the paper.

Reviewer's comment: "*The abstract is too long; the revised one should concentrate on the major methods and findings of the study*."

Reply: We have shortened the abstract and modified it to contain less detail and more on the major points and scientific novelty of the paper.

Reviewer's comment: *"What is the meaning of 'ash reduction level'?"*

Reply: "Ash reduction level" is an option in the HYSPLIT model for volcanic ash, which reduces the size of the volcanic ash cloud. This is sometimes necessary when satellite data shows a smaller ash cloud than what is forecast by the model. Because this is not important for our study, we elected not to use ash reduction. Therefore, we feel that touching on "ash reduction level" in the text is not really necessary and will cause confusion for the reader. We have deleted our mention of "ash reduction level" in the text.

Reviewer's comment: "*What is the spectral resolution of the model?"*

Reply: The resolution we used was 5 nm for SW and 20 cm^{-1} for LW. This information has been added to the revised section 2.3.

Reviewer's comment: "*I am sure that this is information is given in the original papers but it is necessary to a have a short explanation of the methods, the required input data, and the spectral resolution of the model.*"
Reply: We have added the spectral resolution to the text and provided additional information about the capabilities of the model (see section 2.3).

Reviewer's comment: "*What means 'we considered conditions representative of the Arctic environment' and 'a subarctic winter atmospheric profile'?*"
Reply: This means we took care to consider the specific environmental conditions, such as surface albedo, solar zenith angle and temperature profile, which unique to the Arctic environment for the time of year under study. The atmospheric profile (temperature, pressure and gases concentration) comes from the standard atmospheric models of McClatchey et al., 1972, which are commonly used in radiative transfer modeling. We use the subarctic winter standard atmospheric model since the eruptions occur closer to the winter solstice than the summer solstice. This has been clarified in the revised manuscript.

Reviewer's comment: "*It would be helpful to have a figure with the profiles of the prescribed variables.*"
Reply: We have added this reference to the text. We feel that we already have too many figures.

Reviewer's comment: "*The authors have to include a table or a figure that gives the wavelength dependent refractive indices that were used for the study. These are basic input data and have to be documented in detail to give other groups the possibility to compare their own results with those of the present study*."
Reply: We have added this figure, see revised figure 6a.

Reviewer's comment: "*Is the surface albedo prescribed wavelength dependent? Please give numbers.*"
Reply: Yes. The spectral surface albedo for water and snow are from Viollier (1980) and Wiscombe and Warren (1980), respectively. We provided this information in the revised manuscript. In addition, Table 1 gives values of albedo at 550 nm.

Reviewer's comment: "*The assumptions about the sulphate and the ash aerosol have to be explained more precisely. Are sulphate and ash treated as external mixtures? What about condensation of sulphuric acid on pre-existing ash particles?*"
Reply: Yes, they are external mixtures. Condensation of sulfuric acid onto pre-existing particles has not been accounted for.

Reviewer's comment: "*What means sulphate solutions around 70%?*"
Reply: Volcanic sulfate particles are actually spherical droplets of sulfuric acid solution, and the concentration of this solution is dependent upon the temperature and humidity of the surrounding air (e.g., Stenchikov et al., 1998). This point has been clarified in the revised manuscript. We have chosen a sulfuric acid solution of 70% for reasons already discussed in the text.

Reviewer's comment: "*What means ratios of fine and coarse mode? Are those ratios based on mass or on number?"*
Reply: The ratio is in terms of particle number concentration. This was clarified in the revised manuscript.

Reviewer's comment: "*Page 26701, line 5: What means 'The change in Fnet is the flux divergence, or the change in the net flux between layers of thickness z'? Is that a mixture of a mathematical formulation and its approximation by numerical methods?"*
Reply: There is no numerical approximation used here. This is a common approach to calculating the radiative heating rate of a layer. The change in Fnet (dFnet) is computed as the difference between F at altitude $z + \Delta z$ and F at altitude z. We also refer to this difference as the flux divergence for layer Δz. However, we do not think the discussion of this "flux divergence" is actually necessary and, as the reviewer pointed out, may cause more confusion to the reader than it is worth. We have taken this bit out of the text.

Reviewer's comment: "*In equation 7 the variables DARFT OAand DARFsurf ace are defined. Please use these variables instead of 'DARF at TOA' or 'SW DARF at the surface' later on in the text. This makes the paper easier to read."*
Reply: We have changed this.

Reviewer's comment: "*Maybe it is due to the different scaling of the sat pictures and the model results but I have the impression that the HYSPLIT model is not able to describe the ash dispersion in the right way. E.g. figure 4d shows ash transported into northerly directions where the figure 4c shows observed ash transport to the south. Please prove that I am wrong."*
Reply: We mistakenly included a HYSPLIT image from a different day. We have replaced this image with the correct one, and now there is good agreement.

Reviewer's comment: "A comparison of model results and sat data requires that the figures show at least approximately the same area and have the same scaling. Otherwise the figures are misleading."
Reply: Different figures show different type of data and therefore the domains shown differ somewhat. However, all figures are centered at the region of study. Since the modeled HYSPLIT fields are examined in the qualitative sense, we feel that differences in the domain are not important for our analyses.

Reviewer's comment: "*Page 2607 line 9: The figure indicates flight levels not heights."*
Reply: We have changed all text and figures to report heights in kilometers instead of flight levels to be consistent.

Reviewer's comment: "*Maybe one should add that the extinction coefficient is normalized by number density."*
Reply: This has been added to the text.

Reviewer's comment: "*Wouldn't it be better to give the extinction coefficient normalized by mass?"*
Reply: We are not sure why the reviewer would prefer an extinction coefficient normalized by mass instead of number density.

Reviewer's comment: "*I think sections 3.3.1 and 3.3.2 would be easier to follow if a figure would be added that gives the vertical distribution of the ash or of the optical properties. Both sections are written in a narrative style and need a better structure. Maybe a sketch that summarizes the different effects as vertical plume structure etc. could be added.*"
Reply: We already explain that aerosol is uniformly distributed in each of the layers we consider. We also give the vertical thickness, plume top altitudes, and vertical placements in the atmosphere of both plumes. Additionally, the CALIPSO cross-section of the plume from April 2 supplies a visual of profile of this particular plume. We think there are already too many figures and that this point is well explained and illustrated in the text.

Reviewer's comment: "*Page 26713, lines 14-29: The whole section is misplaced. It might be shifted to the introduction.*"
Reply: We think these few sentences provide an interesting, non-repetitive end to our paper, highlighting the importance of volcanic aerosol in the Arctic and accentuating the scientific merit of our results. We like them and believe their placement in the text to be appropriate.

Reviewer's comment: "Table 1: *Please check the table; I think the model parameter for the number 0.18, 0.38 and 0.58 is missing.*"
Reply: No, but it is shifted to a lower line in the table, which is difficult to read. We have corrected this.

Reviewer's comment: "'*Realistic sun angles' as source of input makes no sense*."
Reply: This means we used solar zenith angles which were realistic to the time of year of the study in the Arctic environment. Our choice of angles is explained in the text.

Reviewer's comment: "Table 2: *Please check 'between 0.16 and 0.58' and '~2.5 -7 km' in the table heading. Different numbers are given elsewhere in the text.*"
Reply: We made sure these are consistent and correct in the text.

Reviewer's comment: "Table 4: *I think it makes no sense to compare the results of this study with the one of Ritter (2005) as long as the latter study does not contain basic information as the vertical placement of the ash plume amongst other missing information.*"
Reply: There are not many papers which report LW forcings. Our paper for the first time provides IR values for volcanic aerosol. Ritter is one of the other few papers that include IR calculations for Arctic haze, but they do not provide sufficient information. However, we still believe there is merit in comparing forcings for a thin volcanic layer to the haze layer reported in Ritter. We also feel that in doing the comparison we further stress one of the novel aspects of this paper, which is that we provide IR values along with the information necessary to do our calculations.

Reviewer's comment: "*Figure 1: This figure should be rescaled to show only the area that is indicated by the red circles. Otherwise the figure is hard to read.*"
Reply: We intentionally chose the entire five minute granule to show the direction of transport of ash across a larger area and illustrate Arctic conditions at the time of the eruptions.

Reviewer's comment: "*Figure 2: the labels of the axis are hard to read*."
Reply: We have made the figure more discernable by enlarging longitudes/latitudes and numbers on the color bar, as well as by adding a title to the colorbar.

Reviewer's comment: "*Figure 3: the figure has to be rescaled. Please depict the same area that is shown in Figure 2."*
Reply: We have actually removed this figure in efforts to shorten the paper.

Reviewer's comment: "*The heading of the figure is too long. Please reduce it to the facts and move explanations to the corresponding sections."*
Reply: This figure was removed entirely.

Reviewer's comment: "*What means 'a duration of 1 h'. What means 'ash column height to flight level 650'?*"
Reply: The "duration" refers to the duration of eruption. Although we have removed figure 3, we have clarified this in the HYSPLIT part of figure 4. The "ash column height" refers to the plume top height. We have reported this in kilometers in figure 4 to reduce confusion with flight levels.

Reviewer's comment: "*Figure 4: figure caption is quite long. Information is partly doubled. Figure 4b, 4c, and 4d must show the same area."*
Reply: All figures were generated from the different sources, so achieving exactly the same domain and region shown is not possible. We have tried to shorten the caption, but do not want to clutter the text with information that is better reported in a caption.

Reviewer's comment: "The number on the axis and the colour codes are hard to read."
Reply: We have enhanced the readability of the plot by increasing the size of the text on the axis and color codes.

Reviewer's comment: "*Figure 7: The figure caption is too long. Wavelength axes have to be rescaled."*
Reply: We have rescaled the axes, but we had to length the caption in order to incorporate information on the refractive index figure, which was added at the request of the reviewer.

Reviewer's comment: "*Figure 8: the figure caption."*
Reply: We feel that this caption is already rather short. We have tried to provide sufficient information that figures may be understood by the reader without reading the entire text. However, we have tried to shorten captions where we feel we can do so without removing essential information.

Reviewer's comment: *"An R2 value for three data points is meaningless (same holds for figures 12 and 14)."*
Reply: As the text explains, the net F versus AOD curve is not a linear function. For narrow ranges of AOD, this function can be approximated as linear. The R2 value is shown here to indicate the region of curve where the linear dependence holds.

Reviewer's comment: "*Figure 9: Figure caption is too long, reduce it to the facts. Skip 'in the plot'*."

Reply: We have shortened the caption. We have also taken time to delete unnecessary phrases like this in other captions as well.

Reviewer's comment: "*Figure 15: what is the difference of opacity and AOD*?"
Reply: They are synonymous, but for consistency, we have decided to only use "AOD" in the figures.

Reviewer's comment: "*In general the number of figures could be reduced. Why not combining Figures 8 and 12, respectively, 10 and 14 into one figure?"*
Reply: While we agree that this would lessen the number of figures, it would also change the order in which they are presented and therefore the flow of our results sections. We experimented with several different variations on the order to present our results and believe the current arrangement works bests. In efforts to reduce the number of figures, we have removed figure 3 and combined figures 16 and 17 into one figure. We decided that figure 3 was not essential to this paper. The combination of figures 16 and 17 allows the reader to see both the SW and LW forcings and heating rates at once, enabling him/her to more easily observe the big picture of the aerosol comparison section in one figure.