Reply to Anonymous Referee #1

Thank you for your thoughtful review of our manuscript. We address each of your comments below, as well as outline the changes we have made in our manuscript in response to each comment. First, we would like to take a moment to very briefly reiterate the scientific merit and novel aspects of our paper here:

- We performed a detailed, day by day satellite data analysis for the entire duration of eruptive activity in order to obtain the most complete satellite data set available, which we used to constrain the modeling of radiative forcing.
- This study is the first to include longwave fluxes and to calculate total radiative fluxes accounting for solar and longwave components.
- Our results show that the radiative perturbations due to volcanic aerosol are comparable to those caused by other aerosol common to the region.
- The findings of this study necessitate the inclusion of volcanic aerosol, along with other natural aerosols, in Arctic warming mitigation considerations.
- These results could also be instrumental in improving the treatment of volcanic aerosols in climate models.

Reviewer's Comment: "*The study makes a fairly good contribution with one general exception with respect to the terminology used for radiative forcing quantities and comparisons of these quantities to others in the literature that are not equivalent. These issues are detailed below and should be addressed for consideration of publication. The manuscript provides values for the direct aerosol radiative forcing (DARF), defined as the change in net flux with respect to the change in aerosol optical depth (units of Wm-2⌒ -1). This definition reflects what is typically termed a radiative forcing efficiency (RFE). Radiative forcing (F, units Wm-2) is accurately defined as a perturbation from an initial state (e.g., Charslon et al. 1991, IPCC, CCSP). For aerosol in global climate change studies the initial state is typically a value for pre-industrial aerosol whereas for local aerosol studies or sensitivity studies it is often the absence of aerosol. The utility of radiative forcing is that the impact of different forcing mechanisms can be compared, which is an important aspect of this work. The authors chose this definition following Stone et al. 2007, 2008 (erroneously defined there) in order to facilitate comparisons, but these comparisons should not be made as they are here because of the nature of the quantity itself (some of which are detailed by the authors in Sec 3.3.2, para 3). Most notably the RFE, or DARF as it is defined here, is dependent on the range of optical depth used in the calculation. The rate of change can be non-linear as the aerosol becomes optically thick, which occurs at values of seen here in the thicker plume. Thus the thinner and thicker plumes shouldn't be directly compared in this way (also discussed by the authors).*"

Reply: While we agree that the IPCC defines radiative forcing as a perturbation to an initial, pre-industrial state, we must note that 1D stand-alone radiative transfer models use an aerosol free atmosphere as the initial state. In these 1D studies, authors may report either delta F or radiative forcing efficiency. The concept of forcing efficiency makes sense, because it provides insight into how the forcing changes with optical depth. Forcing efficiency can allow for useful comparisons between plumes, but only if forcings for those plumes are calculated over a similar range of optical depths. This concept is not clearly addressed in previous studies which report forcing efficiency. We do acknowledge that the quantity we (as do the two works by Stone et al.) define as DARF is actually a direct aerosol radiative forcing efficiency. We have clarified this in the revised manuscript in efforts to assure there are no miscommunications made about the quantities reported our paper. To make this clarification, we have introduced the quantity DARFE (direct aerosol radiative forcing efficiency). Both quantities DARF and DARFE are discussed in the revised manuscript.

Reviewer's Comment: *"In Section3.4, comparisons are made among their results, those from the two Stoneet al. papers above, and a Quinn et al. study. The quantity drawn from the Quinn et al. study was originally calculated as a F, however, it is a diurnally average forcing whereas the quantities from this study and the Stone et al. studies are instantaneous. A quick calculation in SBDART for the parameters reported in Quinn et al. provide an instantaneous TOA F of ⁄10 Wm-2 which would*

change its relative placement in Fig 16. (note that this calculation is not intended to represent what Quinn et al. may have found with their own model as some input parameters may vary) Additionally, this value is for a wavelength range of 300-1100nm – making the same rough calculation for 250-4000nm (closer to the range used by the authors) results in ⁄13 Wm-2. This is just a rough example to

show the potential, substantial discrepancies that may result for comparing unequal radiative quantities."

Reply: We thank the reviewer for pointing this out. Because the forcings in Quinn et al., 2007 were reported with a noon zenith angle, we mistakenly thought they were instantaneous. In a personal communication with Dr. Quinn, we confirmed that these forcings were daily averages. Dr. Quinn was kind enough to report to us the instantaneous TOA DARF at solar noon for the parameters used in this study. We have used this value in our comparison – shown in new figure 15.

Reviewer's Comment: "*In order to make comparisons to other studies that report direct radiative forcing the authors multiply their DARF by AOD (⌒), which returns the units of F. So why not define and present the quantity F (as the difference in the fluxes calculated here from fluxes in the absence of aerosol) throughout the manuscript then multiply the Stone values by ⌒550 to make a comparison, stating that the Stone et al. value is actually an RFE and not equivalent to F? The heating rate profiles include a clear sky curve (assumed to be for no aerosol) so the flux calculations have already been done. Alternatively, the quantity that is currently presented throughout the manuscript could just be labeled RFE then, when multiplied by ⌒550, labeled F. For these reasons, I would resist placing all of these quantities together as in Fig 16 and 17 and drawing conclusions about the relative impact of the different aerosol types on the Arctic radiation budget in this way because the magnitude of these values could change relative to each other with somewhat minor changes in the circumstances of their calculations. Instead, all of these studies could be presented and discussed in the text, including their caveats, in Section 3.4. It would not change the conclusions of the paper considerably and could be quite interesting to see where the differences are in how the values are derived and how future studies might reconcile them. Many studies do not state explicitly how forcing calculations are made which essentially precludes them from being directly compared to others representing a missed opportunity."*

Reply: As was explained above, we computed both DARF and DARFE so the radiative quantities are now directly comparable and can be presented in the figures. We do this to visually help the reader understand the scientific contributions of our work in the broad context of previous research on other types of Arctic aerosols. Although we use the most complete data sets from previous works that we could find, we acknowledge that there are some missing bits of information about the calculations that were performed, for instance the paper by Ritter et al., 2005. 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. We 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 showcase 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: "*Abstract - is too long with too many specifics. Alternatively, within a particular aerosol plume provide the differences between surface albedos and SZA to make the point that environmental conditions are important rather than giving the long list of values."*

Reply: We have shortened the abstract.

Reviewer's Comment: "*Section 2.1 – what is the reliability/uncertainty of the MODIS fine mode fraction product under the difficult conditions of snow cover that dominate the region in this time period?"*

Reply: MODIS products officially report expected error bars for AOD are: $\Delta\tau = \pm 0.03 \pm 0.05\ \tau$ over ocean and $\Delta\tau = \pm 0.05 \pm 0.15\ \tau$ over land. These error bars were taken from the ATBD for MODIS tropospheric aerosol products and are designated for the 0.55 μm channel, but are assumed to apply to other channels as well. However, uncertainties for specific conditions are not reported. The purpose of this study was to constrain a range of the forcing caused by volcanic aerosol. Therefore, MODIS products do not have to be exact.

Reviewer's Comment: "Section 2.3 – you could omit this section and move the material to the beginning of Section 2.5 (there's no need for two model sections)"

Reply: We have done this.

Reviewer's Comment: "*How is the radiative transfer model modified? This may be of interest to readers, especially if it has some bearing on the results."*

Reply: The model was modified to incorporate a user defined aerosol layer, which introducing more capability when compared to the public version. This point has been clarified in the revised manuscript.

Reviewer's Comment: "*Section 3.3.1 – Last paragraph is a nice conclusion – little work has been presented ontotal forcings and the effect of LW. The single scattering albedos are important in this discussion – it would be nice to have a reference to their values here (could add those values to Table 1 and reference that)."*

Reply: To address this concern, we discuss SSA at 550nm in the revised manuscript.

Reviewer's Comment: "*Figures – these are all way too small. The text is too small in most of them to read and details of the images/plots that are important to the results are not discernible."*

Reply: We have enhanced the readability of the plots.

Reviewer's Comment:
Technical Comments
P 26698, L 4 – 'is' should be 'was'
P 26699, L 23 – 'course' should be 'coarse'
P 26700, L 27 – Eq 4 quantity should be Fnet total (rather than Ftotal net) for consistency
P 26707, L 19 – 'overall radiative forcing' should be 'total radiative forcing'

Reply: All technical comments have been changed.