Atmos. Chem. Phys. Discuss., 11, C12165–C12168, 2011
www.atmos-chem-phys-discuss.net/11/C12165/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional radiative impact of volcanic aerosol from the 2009 eruption of Redoubt volcano" *by* C. L. Young et al.

Anonymous Referee #1

Received and published: 21 November 2011

General Comments

This manuscript examines the impact of a range of volcanic aerosols – varying thicknesses and ratios of ash and sulfate – on the radiative balance of the Arctic. Aerosol properties are highly variable globally, regionally, and even locally through time so improved knowledge of the properties of these varying aerosols is of value. The work adds to the literature that shows the sensitivity of radiative flux to variability in aerosol composition and other relevant parameters such as surface albedo and solar zenith angle, which are of increased interest in the Arctic. While there is much in the literature on this topic, the unique aspect of this work is that it provides a rigorous sensitivity study of the impacts of real volcanic aerosol, loosely constrained by satellite data, in a region where surface cover and reflectivity can change appreciably and with quite sig-

nificant impacts on aerosol radiative forcing. Specifically, this work provides longwave and total radiative fluxes as well as heating rate profiles, which are often ignored for shortwave fluxes alone.

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-2τ-1). This definition reflects what is typically termed a radiative forcing efficiency (RFE). Radiative forcing (ΔF, 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 τ 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).

In Section3.4, comparisons are made among their results, those from the two Stone et al. papers above, and a Quinn et al. study. The quantity drawn from the Quinn et al. study was originally calculated as a ΔF, 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 ΔF 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.

In order to make comparisons to other studies that report direct radiative forcing the authors multiply their DARF by AOD (τ), which returns the units of ΔF. So why not define and present the quantity ΔF (as the difference in the fluxes calculated here from fluxes in the absence of aerosol) throughout the manuscript then multiply the Stone values by τ550 to make a comparison, stating that the Stone et al. value is actually an RFE and not equivalent to ΔF? 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 τ550, labeled ΔF.

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.

Specific Comments

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.

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?

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)

How is the radiative transfer model modified? This may be of interest to readers, especially if is has some bearing on the results.

Section 3.3.1 – Last paragraph is a nice conclusion – little work has been presented on total 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).

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.

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'

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26691, 2011.