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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Characterisation of a stratospheric sulphate plume from the Nabro volcano using a combination of passive satellite measurements in nadir and limb geometry" *by* M. J. M. Penning de Vries et al.

S. Carn (Referee)

scarn@mtu.edu

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In this paper the authors present nadir and limb measurements of aerosol and SO2 made by UV satellite sensors in the Nabro volcanic plume in June 2011. The June 2011 eruption of Nabro was one of the largest in recent years and has generated some controversy and debate in the scientific literature, mostly concerning the mechanism by which the volcanic SO2 entered the stratosphere (direct injection vs. Asian monsoon transport). The main goal of this paper appears to be characterization of the aerosol

plume (altitude and composition) in the first few days of the eruption, and some new limb aerosol retrievals from SCIAMACHY are presented. Overall I think the paper is suitable for publication in ACP after some significant revisions. The paper documents an unusual feature of the UV Aerosol Index (UVAI) in high altitude plumes (dependence on viewing direction), and the SCIAMACHY limb retrievals are also novel, although interpretation of the limb measurements appears complicated and some of this is rather hard to follow.

My main criticism of the paper is that the precise composition of the aerosol in the volcanic plume remains ambiguous – the authors appear unable to distinguish between sulphate and ice using the available data. Of course, I would expect both species to be present but the challenge is identifying the dominant component. The fact that the aerosols were at high altitude and strongly scattering (high SSA) seems robust, but either ice or sulphate (or a combination) could produce a negative UVAI. This is clearly apparent in Fig. 1, where the area of meteorological cloud north of the volcanic plume exhibits strongly negative UVAI. The authors acknowledge this ambiguity and even avoid mentioning sulphate explicitly in the conclusions. Given this uncertainty, referring to a 'stratospheric sulphate plume' in the title and 'sulphate aerosols' in the abstract seems a little disingenuous; 'stratospheric aerosol plume' would be more appropriate. Overall I would like the authors to be more candid regarding what can be robustly concluded from their data.

Regarding the negative UVAI, this is a fairly common feature of fresh volcanic plumes that are not dominated by ash. In the latter case aerosol absorption dominates and a positive UVAI results. Exploring the origin of negative UVAI further is certainly worthwhile. However, it is important that the authors explicitly state which OMI UVAI dataset they are using, since there are (confusingly) several different UVAI data products available. Also, for very high SO2 loadings the SO2 absorption at longer UV wavelengths can also affect the UVAI. I have looked at OMI UVAI data from both the operational OMI OMAERUV product (which uses wavelengths of 354 and 388 nm) and the UVAI from the operational OMI SO2 and O3 products (which uses wavelengths of 331 and 360 nm to be consistent with the old TOMS UVAI), and the appearance of the Nabro volcanic plume is not the same in both datasets. Unlike the OMAERUV UVAI (which I presume the authors used in the paper), the 'TOMS-like' UVAI does not show the positive values in the western part of the aerosol plume on June 14. Hence I suggest that the authors conduct more radiative transfer calculations to assess the wavelength dependence of the UVAI effects – perhaps this would provide more information on the nature of the aerosol.

In addition to these major concerns, I list numerous specific points by page and line number below:

7740, L5-7: 'Formation of sulphate aerosols in the stratosphere takes about a month. . .' – this statement could be misleading since, whereas the average SO2 lifetime in the stratosphere may be $\sim$1 month, aerosol formation starts immediately.

7740, L24-25: the very first lines here suggest that sulphate aerosols are a primary product of volcanic eruptions, whereas it is mostly SO2 that is emitted and subsequently converted to sulphate. This should be clarified. Furthermore, the long stratospheric lifetime of SO2 is also due to slow gas-phase oxidation rates.

7741, L3: I think at least one reference should be given for the direct radiative effect of sulphate aerosol.

7741, L12: whether the Nabro eruption was 'exceptional' in terms of SO2 release depends on context (e.g., it was much smaller than Pinatubo or El Chichon), so please clarify.

7741, L16: the altitude of the tropopause at Nabro should be specified.

7741, L23: SO2 removal mechanisms depend on altitude, and dry/wet deposition may be very significant in the 'atmosphere' as a whole (since most SO2 is emitted at low altitudes).

7741, 28 and 7742, 2: the vast majority of volcanic eruption plumes are dominated by water vapor, both entrained atmospheric water and volcanogenic water, and I find it highly unlikely that Nabro was exceptional in this regard. I would not use the Smithsonian reports as evidence for a water-rich plume as there can be no actual measurements to confirm this. It may have been motivated by the 'bright' (i.e., high albedo) appearance of the Nabro plume in visible satellite imagery (e.g., MODIS), but many fresh volcanic plumes have a similar appearance.

7742, 5: note that the Sawamura et al. (2012) study is ambiguous about the nature of the aerosol detected at Sede Boker – they speculate that 'ash and sulfate' may be present but I would not say that they are 'in agreement' with the results presented here. I don't think they even used depolarization information to distinguish solid vs. liquid phase aerosol.

7742, 10: the full names of the satellite sensors should be given here, rather than later on.

7743, 6: 'horizontally narrow' – does this refer to the horizontal extent of the plume, and/or is vertical thickness also important?

7744, 13: OMI pixels are only 13x24 km in size close to nadir; they increase in size gradually towards the swath edge.

7745, 12: the actual definition of UVAI (i.e., an equation) is not given here; given that UVAI is one of the main foci of the paper, I think it should be explicitly stated.

7745, 15: the authors should clarify exactly which operational OMI aerosol product was used – OMAERUV? Also, the OMI UVAI in OMAERUV does not use exactly the same wavelengths as SCIAMACHY (354 and 388 nm are used).

7746, 13: some parameters require definition here. Is 'g' the asymmetry parameter?

7747, 4: this sentence needs rewriting – it starts 'It can be very well measured from space..' but then continues '..both from the ground and from space'.

7748, 6: again, the SO2 emission was large but not ‘exceptional’. It was likely the largest volcanic SO2 emission since Pinatubo, if the entire Nabro eruption ($\sim$1 month) is considered.

7750: in the discussion of the variable OMI UVAI signal here, it is critical to explicitly state which definition of the UVAI the authors are using (see major comment above) as the change in sign of the UVAI is not observed in all versions.

7750, 15: in addition to the clouds clearly visible in the MODIS image, I think the presence of thin ‘subvisible’ clouds (e.g., thin cirrus) collocated with the volcanic plume cannot be ruled out. This could also include small ice particles in the volcanic plume itself.

7751, 3-5: were the optical properties of the sulphate aerosols obtained from the literature? If so, a reference should be provided. Otherwise, some further justification for the choice of these parameters (and also the AOT values used in RT calculations) is needed. This also applies to P7746, L12-15.

7751, 13-14: the authors should also note that the region of meteorological clouds to the north of the volcanic plume also shows a change in UVAI with viewing angle (Fig. 3). Presumably the UVAI remains negative or close to zero in this case due to the lower cloud altitude?

7756, 15: but if small ash particles had acted as ice nuclei and become coated by ice then they would likely not produce a positive UVAI either.

7756, 20: I think it is highly improbable that the positive UVAI outside the SO2 plume is due to volcanic ash – there will be significant amounts of desert dust in the region.

7757, 9: I don’t think the fact that the ice cloud disappeared from MODIS visible imagery can be used as proof that there was no longer any ice present in the volcanic plume. Small subvisible ice particles could still be present.

7757, 17: exploiting the viewing angle dependence would seem to be highly dependent

on the assumed phase function (and hence also on particle size and scattering regime).

7758, 9: the Sawamura et al. (2012) paper should be referenced here – I presume this is where the AOT value of 0.17 comes from?

7758, 10: the Sede Boker lidar data were collected on June 14, so it would seem more consistent to compare the lidar AOT with the SCIAMACHY limb AOT measured on the same day (i.e., the profile in Fig. 5)?

Fig. 3: left and right panels are confused in the caption.

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