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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantification of the depletion of ozone in the plume of Mount Etna" *by* L. Surl et al.

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We would like to thank the reviewer for their helpful comments, several of which we have used to make revisions to the manuscript. The numbers in these responses relate to the numbering of the comments by the reviewer.

(1) Regarding the potential for dry deposition of gases at the mountain top

We do not believe that dry deposition of either SO2 or O3 is likely to cause significant perturbations to the mixing ratios within the plume, this is based on a rough numerical assessment of the likely deposition that occurs within transport from crater rim to the measurement site.

As there is no vegetation on the mountain top the only relevant deposition pathway is to ground, characterised by the rg values of Wesely (1989). Even if aerodynamic and quasilaminar sublayer resistance were zero, the deposition velocity would be 0.25 cm/s for O3. Assuming the plume exists in a 5m layer above ground, and this layer is in contact with the ground for 100 s before measurement, this yields an upper estimate of 5% loss of O3 to the surface. SO2 losses, due to higher rg, would be about half of this.

The following text has been added to the manuscript discussing this and relevant literature is cited:

"The plume was observed to be grounded at all downwind sites, enabling measurement. As both O3 and SO2 are known to exhibit deposition to the ground, there is the potential for the mixing ratios of these species at the elevation of the instruments to be perturbed by this physical process. However the ground at the peak of Etna is rocky and devoid of vegetation and could be classed in the scheme of Wesely (1989) as "barren land". The bulk surface resistance of this category sufficiently high that, even if the aerodynamic and sublayer resistances are low, the deposition velocity will be too low to cause a significant perturbation during the transport from crater rim to the measurement sites."

Wesely, M.: Parameterization of surface resistances to gasous dry deposition in regional numerical models, Atmospheric Environment (1969), 23, 1293-1304, doi:10.1016/004-6981(89)90153-4, 1989.

(2) As per the reviewer's suggestion the co-ordinates on Figure 1 have been changed to Cartesian co-ordinates, relative to the location of the centre of the North East crater. This should make the figure more informative for readers.

(3) Following the reviewer's comment, the explanation of the uncertainty/error-bar calculation has been rewritten in a manner that should hopefully be more clear to the reader.

(4) In regards to the reviewer's comments about the ongoing plume-edge discussion:

While the question of BrO/SO2 ratios at plume edges is interesting, we do not believe any of the data we collected in this campaign, included within this manuscript or not, are suited for evaluating this.

he reviewer states that "All cases when the instrumentation was at the verge of plume are excluded from the consideration in this manuscript". While some measurement sites were excluded from the manuscript, this was done either because there was no background site measurement on that day preventing depletions from being assessed (plume measurements on the 26th and 28th July), or because the plume failed to intercept the downwind measurement site (1st August). We do not believe we have excluded any measurements which would have helped to address the plume-edge issue.

Based on visual assessment of the situation at site d2-30, and the SO2 ratio measured, this site was not in the core of the plume. However we do not believe the quantity/quality of data we have here is sufficient to attempt any answer to the plume-edge question based on this site alone.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23639, 2014.