

Interactive comment on “Opportunistic validation of sulfur dioxide in the Sarychev Peak volcanic eruption cloud” by S. A. Carn and T. M. Lopez

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We thank the referee for a constructive review of the manuscript. Please find below our response to the referee’s specific comments.

* Abstract: the 5-6% is misleading, and I suggest formulating this in a more qualitative way; perhaps mentioning the many challenges involved in such a comparison.

Instead of specifying a 5-6% difference, we now state that the FLYSPEC and OMI measurements agree ‘within measurement uncertainty’. We also stress the challenging nature of the comparison in the abstract.

* Fig 1. This is a really nice figure, but hard to read. It would be good to make this

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figure also available as kmz (with the 3 different images included) in supplementary material, so readers can have a better look at it with Google Earth.

This is an excellent idea and we will provide the following KMZ files as supplementary material (zip archive) for viewing in Google Earth:

omso2_20090618_comp.kmz: OMI SO2 data

2009-06-18_21-00-00_Exp_V2.02_4_6.kmz: CALIPSO lidar data

ARCTAS_Spring_Alaska.2009169.aqua.500m.kmz: MODIS visible imagery

SouthwestAlaska.2009169.aqua.1km.kmz: MODIS visible imagery

AleutianIslands.2009169.aqua.1km.kmz: MODIS visible imagery

Note that we have also made a few changes (annotations) to this figure based on the comments of referee #2, to improve its interpretation.

* I like the way spatial averaging is dealt with, the idea of extrapolating is very nice. Is it original or has this been done before?

We believe this approach is original and we have inserted some text on page 6 of the ms to stress this fact.

* The paper makes mention of the wind speed and the issue of different timing. Would it be possible to shift the OMI measurements, to match the timing of the ground based measurement? It would make more sense than making an average of the northbound and southbound traverses. In any case, I think this should be discussed in more detail.

The problem here is that the ground-based measurements were not collected at a unique time, so shifting either dataset to match the other is not straightforward. Referee #2 provided some detailed comments and suggestions regarding how to reconcile the different timing of the two datasets, and we refer this referee to our responses to those comments.

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* Related to this, it seems comparison/interpretation would be much easier using stationary ground based measurement. Do you agree? I would be interested to read a recommendation (stationary vs mobile) in the concluding section, for the benefit of future validation campaigns.

We believe that stationary ground-based measurements would be preferable except in the case of very slow-moving volcanic clouds (where there is a significant difference in velocity between the cloud and the ground-based sensor). We have added a paragraph to the conclusion that provides some recommendations.

* page 3866, line 14 mentions "unweighted" for the first time, without explaining why or how. I suggest removing the word "unweighted" in this place, as it confusing. Alternatively, it could be kept, but then you should explain what is meant.

We have deleted the word 'unweighted' from this sentence as suggested.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/4/C1403/2011/amtd-4-C1403-2011-supplement.zip>

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