

# ***Interactive comment on “Tracking microphysical variations in emissions from Karymsky volcano using MISR multi-angle imagery, and implications for volcano geologic interpretation” by Verity J. B. Flower and Ralph A. Kahn***

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## **General comments**

This manuscript uses the MISR instrument, in combination with thermal alerts from MODIS, to investigate the variation of the composition of plumes emitted from Karymsky volcano over the lifetime of MISR (2000 – 2017). In spite of limitations in the MISR retrieval algorithm and the constraints at the upper end of the spectral range of the instrument, the authors are able to retrieve qualitative variations within a single plume over its horizontal extent as well as between plumes over time. From this, variations in

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the eruptive style and regime are deduced. The authors were able to demonstrate the ability of MISR to clearly distinguish between plumes dominated by sulfate particles and those dominated by ash particles.

Overall, this is an interesting article and the science within seems sound. I support the paper for publication once the concerns below are addressed.

### Specific comments

P9 Para3 (beginning L18): The authors discuss an eruption observed from ground-based instruments by Lopez et al. (2015) as a way to verify the processes inferred by the satellite-based observations. However, there doesn't appear to have been a MISR observation to compare the Lopez et al. measurements to and so this seems to be mostly speculation on the part of the authors as to what MISR *might* have been. I acknowledge that ground truthing observations from any satellite instrument is hard and made harder by the narrow swath of the MISR instrument but I'm not sure that this comparison can be drawn. If it is included, I think the authors need to be clearer about this being speculative.

Conclusions (P12-13): The authors ascribe the differences in the activity and plume composition from the volcano pre- and post-2010 as the end and beginning of a cycle. I am not convinced, by the data shown, that there is sufficient evidence that this is a long-term cycle rather than a change/evolution of the magmatic regime. I am not ruling out the possibility of this being a long-term cycle, simply that I don't feel the current data is sufficient to determine either way. There does appear to be cyclicity, as the authors say, especially post-2010. The support for this cyclicity I feel is quite strong, with the similar variations in composition shown in Fig. 6h when the measurements are normalized for eruption day. The lack of this shorter-term cyclicity and the change in the plume compositions retrieved pre- and post-2010 could equally suggest a change in the regime, rather than a different part of a longer-term cycle. Could the authors either present the data that led to their conclusion of a long-term cycle or rewrite this part of the conclusion.

### Technical corrections

P5 L3: “SSA” not defined

P5 L14: 10 km<sup>2</sup> grids (add square)

P5 L20-21: Table 2 has two small spherical absorbing flat profiles listed, highly moderately. Do the authors mean the combination of these? Could this be made a little clearer.

P7 L23: The figures displaying variations in the small, medium & large particles are Fig. 6 b, c & d rather than just Fig. 6c as written.

P12 L25-29: This is all one sentence. Please rewrite to reduce the length/split into more sentences.

P13 L4: The sentence reads as plural but only one Bardarbunga eruption plume was considered.

Table 2, title row: What do the “r”s refer to? I am assuming  $r_e$  is effective radius but  $r_1$  and  $r_2$ ? Could these be explained in the footnotes.

Table 2, column 1: s 2 and 9 missing from the table - is this intentional?

Fig. 1: This figure is not referenced in the main text.

Figs. 2, 3 & 4: Lat-lon markers or a length scale (preferably) should be included in these figures. The authors discuss the variable horizontal extent of the plumes on page 6 (L6-7) but the reader cannot distinguish this for themselves.

Figs. 2 and 3 b & c: There appears to be an abrupt increase in the plume height retrieved when the plume moves over the water. Is this real or an artifact of the RA? It seems to happen to all of the plumes shown in Fig. 2 and those in Fig. 3 b & c (though not Fig. 3 a). There is a significant variation in the plume shown in Fig. 3 d but there does not seem to be any coastline that may have caused it. The same sudden change is also seen in a number of the plumes in the supplementary material. Could the authors please elaborate on possible causes of these variations?

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Fig. 6 h: In the text, the authors include the  $R^2$  value for 2011 only as well as for all of the 2011-2015 plumes. It may be worth adding the  $R^2$  value for just 2011 to the plot as well as the full dataset  $R^2$ ?

Fig. 7 top: The 3D-effect used on the plot, while aesthetically pleasing, makes distinguishing the saturation of the point much more difficult, especially on the grey data set (LaSpNab).

Fig. 7 legend: In the text (Page 8, L14-15), the authors describe the 2007b plume (panel C) as being dominated by the sulfate proxy (MeSpNab, yellow) and medium grains (MeNspWab). The latter dataset is not denoted on the legend. I'm not certain if the data set is missing or mislabeled – it could be that the brown data set should be this data set – currently labeled MeNspNab. Could it either be included or labeled correctly.

Fig. 7 bottom: Could the wind direction be shown on the plots so that the variation over the plume described in the text can be more easily identified?

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