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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lidar ratios of stratospheric volcanic ash and sulfate aerosols retrieved from CALIOP measurements" *by* Andrew T. Prata et al.

Anonymous Referee #3

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I find this paper to be an interesting contribution with a few minor issues. My main concern is with section 6.2 which I think needs some more care in how they infer things from the data. A common issue throughout the paper is that the meanings of things like color ratio and depolarization ratio are given much context (what does a value of X really mean).

Page 7, line 26. Does mean there was effectively no change in the values during measurement period?

Page 7, line 28. You commonly refer to layers as either sulfate or ash. While sometimes these layers separate themselves, other times they can be mixed in a complex fashion. You may wish to define your layers as 'layers optically dominated by ash or

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sulfate aerosol' rather than imply that they are distinctly one or the other. Is it possible that complex mixing is responsible for the rather large variations in the backscatter to extinction ratio? Alternatively, is it consistent with noise or variability in the sulfate and/or ash itself?

Page 8, Some of these figures are much too small to see much detail in. I know I can blow them up to see them but my experience is that ACP makes them into JPGs for the final figures and they are always 'infinite' resolution like some bad TV show.

Page 9, Here I will be a curmudgeon, I hate VEI. People use it like it is a quantitative assessment of volcanic explosivity and I think it is disappointingly far short of that and often is not relevant to stratospheric impact. Check this out (a commercial site but the definition is correct) http://geology.com/stories/13/volcanic-explosivity-index/ . The definition is a mess.

Page 9, How do you avoid ice-rich layers? (line 20) Also, since there is a composition change from sulfate to ash, how sure are you that the changes in the color ratio are due solely to size rather than simply that they are a different color?

Page 13, line 11. I think it would be more proper to say 'unambiguously identifying this layer as containing non-spherical particles. It is not necessarily an either/or situation. . .

Section 6.2. I find much of this discussion to be speculative and perhaps the authors are over analyzing their results. Certainly, changes over time that are small compared to the measurement uncertainty is not terribly convincing. They authors are seem to forget that they never measure the same aerosol and that for an inhomogeneous cloud they cannot really be sure that some of the differences are not just variability in the cloud. The authors also do not mention that the aerosol is mixing with ambient aerosol throughout this period and so some changes are may be a result to that process. I would not bother with the humidity explanation and the comparisons with the Icelandic eruption are not likely to be particularly relevant. It is extremely common for sulfate aerosol to contain volcanic material (and meteoritic, etc.) while optically suggesting

spherical particles. Given the high number densities after an eruption some coagulation between ash and sulfate is bound to occur in mixed layers. Perhaps some of these arguments would hold together if we had any idea of how big the ash particles are (i.e., what does the color ratio mean?). (For that matter how good do the authors believe the color ratios are? My impression of the 1064 nm channel on CALIOP is that it is not very robust though differences are real even if not correct).

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