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***Interactive comment on* “Lidar observations of Nabro volcano aerosol layers in the stratosphere over Gwangju, Korea” by D. Shin et al.**

Anonymous Referee #1

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While there is merit in presenting lidar measurements of stratospheric aerosol of volcanic origin over South Korea, this paper lacks some of the required elements to be published in ACP (From ACP Aims and Scope: “The journal scope is focused on studies with general implications for atmospheric science rather than investigations that are primarily of local or technical interest”). In my opinion, this work fits into the latter category: primarily of local or technical interest.

I believe that after a few modifications this manuscript would be suited to be published elsewhere. Asia-Pacific Journal of Atmospheric Sciences or Geoscience Letters, for instance, seem more appropriate choices.

Specific comments:

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1. In the abstract the authors claim to report the first Raman lidar measurements of stratospheric aerosol layers obtained over South Korea. I have two problems with this statement. First, disregarding the "Raman" term in that sentence for a second, are these really the first lidar measurements of stratospheric aerosol over South Korea? I found a paper by Park et al. (1997) reporting measurements of stratospheric aerosol (from Pinatubo) obtained with a lidar system.

Second, it seems misleading to state that these are Raman lidar measurements. From my understanding, only the elastic lidar measurements were used in the study. Therefore it would be more appropriate to say that these were lidar measurements of stratospheric aerosol obtained with a lidar system that happens to have Raman capabilities, instead of stating that these were the first (?) "Raman lidar measurements" of stratospheric aerosol over South Korea.

Reference: Park et al. (1997), Lidar observation of ozone and aerosol in the stratosphere and troposphere over Suwon, Korea. *Advances in Atmospheric Remote Sensing with lidar*, pp 545-548.

2. Page 1176, line 1 – 3: Correction: the 38 sr lidar ratio value was used by one group that participated in the study from Sawamura et al. (2012). Most groups used 50 sr at 532 nm.
3. What exactly is PRCF? I found many papers on PSCF (Potential Source Contribution Function) but not on PRCF. I am assuming they are both the same thing, except that you start running a forward dispersion model from the source instead of a backward trajectory from the measurement location? Please clarify. How did you select 2, 6 and 12 for the number of endpoints in your weight function? How and why is that important?
4. Typo in title of subsection 3.1: plum → plume.
5. Page 1179, lines 1 – 2: You should be careful when stating that the aerosol plume

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- was injected into the UTLS by the Asian anticyclone while citing both Fairlie et al. (2014) and Bourassa et al. (2012). Those papers have different views on how the aerosols reached the stratosphere following Nabro's eruption. Fairlie et al. (2014) demonstrates that the injection occurred directly while Bourassa et al. (2012) claimed the aerosols were lifted through deep convection.
6. Page 1179, lines 18 – 25: Although it is true that the depolarization ratio values observed in the layer are higher than the molecular component, a depolarization ratio value of less than 2% is still rather small and it is very doubtful that it would indicate the presence of non- spherical particles. Mineral particles would have higher values of depolarization ratio, with at least something larger than 3%.
 7. Page 1180, lines 12 – 21: Same as last comment.
 8. Figure 1: It would be helpful to have the lidar location plotted in this map. Y-axis and X-axis labels are missing.
 9. Figures 2 and 3: Typo in wind direction.

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