

## ***Interactive comment on “A pervasive and persistent Asian dust event over North America during spring 2010: lidar and sunphotometer observations” by P. Cottle et al.***

### **Anonymous Referee #2**

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The authors present lidar data, sunphotometer data and model simulations for spring 2010, when two persistent dust events occurred, related to transport of Asian dust to North America and Canada. The paper presents indeed in many details the evolution of these two events concerning the meteorological processes and the description of the measured quantities. The authors claim that these events are extreme but on the other hand they also show from the literature that such transport of Asian dust over North America is not unusual. So apart from its duration and spatial extent, which is of course of great interest and importance, it is not evident from the paper, the way it is structured, where this study improves our knowledge concerning the properties of Asian dust after its transport. My main concern is how the authors use the depolariza-

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tion ratio as an indicator for mixing of dust with local aerosol sources. To my opinion their analysis provides only qualitative information concerning the mixing processes rather than quantitative, which could be very useful to check model estimates of these processes. There are many unclear points in the paper concerning the measurements and analysis of the depolarization lidar data, which do not allow to compare the results with previous studies related to desert dust.

1. In page 30593 the authors briefly present their lidar measurements and products. They use backscatter ratios without providing any information on uncertainty. They just mention in line 21-23 that extinction and overlap can introduce large changes. No information if these (extinction and overlap) are finally considered and what is the uncertainty. What is the overlap of their system and how this has been estimated?

2. More important for the discussion of mixing of dust are the measurements of the depolarization ratio. The authors don't make clear what exactly they show. Is this a simple ratio of the two signals? Is this the linear particle depolarization ratio or volume depolarization ratio? How do they calibrate these ratios? If these ratios are not calibrated they can only provide qualitative information concerning the potential different aerosol type. Freudenthaler et al (2009 in Tellus) provide a detailed discussion on that and propose solutions and relevant literature.

3. In order to be able to make a quantitative discussion about mixing of dust with other aerosol types using depolarization ratios one should provide what are the representative ratios of "pure" types at their source. What is the typical depolarization ratio over the Asian desert? What is the typical depolarization ratio of continental aerosol over Canada etc. If these values are not available or known then the whole discussion is purely qualitatively and as such the whole study should focus more to the dynamics and its extreme behavior rather than the mixing, since the latter is just demonstrated but not quantified.

Therefore I suggest that the authors should clarify the issues related with their lidar data

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and modify the focus of their paper accordingly, to avoid any misleading conclusions.

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