

Interactive comment on “Vertical distribution of aerosols over the Maritime Continent during El Nino” by Jason Blake Cohen et al.

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Response to Reviewer Number 1:

We thank reviewer number 1 for his/her comments. There are some constructive comments. Especially in terms of the uncertainties in the boundary layer heights being made more prominent and in terms of uncertainties in the lidar measurements being more carefully examined. Furthermore, there are multiple suggestions for improving wording, and for making clear when statistical significance is used, as compared to other comparisons. We appreciate these suggestions.

However, we want to emphasize that in fact, this work is a significant advance compared to other works in the literature. We hope to demonstrate that this work is up to date in terms of calibration, new in scope, and provides some major advances of

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interest to the larger community:

One of the most comprehensive papers with respect to CALIOP over Southeast Asia, is given by Lee et al. [2016] (plus an entire team associated with NASA). They published a paper using CALIOP data over Southeast Asia, using slightly less error quantification for high ice clouds found in the tropics [although they may have done this from the way they configure figure 3, but it is not explicitly mentioned], which we have employed. Additionally, they used products that we knew are less reliable, such as SSA, and assumed that they were extendable functionally as the backscatter ratio goes (which is not necessarily a good assumption, but is certainly a much weaker assumption than we use in equating heights to backscatter). We made sure to stick to only the most reliable product, backscatter, as is also demonstrated in more depth by Rogers et al. [2009] and Hostetler [2008]. Furthermore, we performed the exact thing that these papers identified as the major weakness of CALIOP data, a methodology of how to combine the spatially-disparate paths, into a useful contiguous product.

As can be seen in the literature, many other papers use CALIOP without any validation at all [Sugimoto et al., 2015], or actually use it to validate models, which are known to themselves be highly inaccurate. This can be seen by the many papers put put by Jeff Reid and his team, whereby NAAPS (A modeling system) is used to validate CALIOP [Campbell et al., 2013]. A quick look at their most important validation for the NAAPS model (supplemental figure 6) shows that it performs less well than the modeling system (Cohen, 2014; Cohen and Wang, 2014; Cohen et al., 2017) over this region during the biomass burning season, since the annual average values still perform only as well, and due to the biases included within. Furthermore, as already demonstrated in the paper's references, the results are very comparable with findings from Lin Neng-Hui's group in Taiwan [Lin et al, 2014, 2013, etc.], and the AD-Net [Sugimoto et al, 2014] who are making observations with on-the-ground lidar at multiple places within the Northern portion of Southeast Asia and Greater East Asia. Hence, it should be completely reasonable to assume that the validation against the results

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and findings from the underlying measurements, upon which those results are based, should be sufficient validation.

In an ideal world, there would be more validation on the ground in this region. However, this team, and many others are working to continue to improve this situation as best as possible. If there are any specific comparisons that you could recommend to continue to demonstrate the uniqueness of this result and its strength, compared to the other papers currently available, please let us know. From what we can see, this is the only paper that is comprehensive in its analysis on a day-by-day basis over a long-term such fire event, looking carefully over its total spatial extent over this region, not merely hunting for or tracing a few daily events, or randomly sampling over a very long spatial/temporal extent which is mixing both fire and non-fire type of atmospheric conditions.

Many of the other comments in terms of validating the errors of the models and measurements are mentioned in the other response and will not be repeated here. However, we take these suggestions very seriously and look forward to work with the reviewer to address their concern of the newness and significance of the results.

Finally, we agree that more information can be provided, and hope to do this by producing a probability distribution function of the results, on a day-by-day basis as well as over the entire month, for both the measurements and the models, and will include a summary as a new figure, with the remainder placed in appendices. We hope that this can address and demonstrate the significant importance, especially given the fact that the actual, stabilized boundary layer, may be lower than the 1000m as measured over Singapore.

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