

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

Jason Blake Cohen et al.

jasonbc@alum.mit.edu

Received and published: 27 March 2017

We thank reviewer number 2 for his/her comments. There are some constructive comments. Additionally, some of the comments show indirectly that the wording should be improved for clarity, as part of the structure of the way the research was organized was possibly misunderstood.

However, we strongly urge the reviewer to be more careful, and to actually read the paper to the end. The reason being that a few of the points made, including stating that the data was “inadequate” to “support the scientific conclusions”, were actually already addressed in the paper, through figures and tables.

These details will be given below in the point-by-point response. Reviewer comments are preceded by RC, while the author responses are preceded by AC.

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RC: "Authors didn't mention about the aerosol-retrieval uncertainties over the land, especially during large-scale fire events" AC: This has been mentioned in other papers we have cited and already performed over this region (Cohen, 2014; Cohen et al., 2017). However, for clarity the values will be added into this paper. There are two issues, the first with cloud cover, in which a bias may exist because extremely high AOD conditions ($AOD > 2$) are frequently flagged as clouds. The second is the error itself over land, which can go as high as 15%. However, as you can see from the figures and the tables, the difference between the "fire region" and the "non-fire region" is significant at values where the errors are much larger than 15%. This has already been factored in, but will be made clear. The terms of the issues of the bias by cloud screening reducing the measured AOD is if anything a stronger supporting issue for the results given here, since it would make the difference between the "fire region" and the "non-fire region" even larger. It is also the reason why MISR was used for the initial definition of the two regions, since its ability to cloud clear is better than MODIS over this region. It is furthermore a point of interest when analyzing the CALIOP data, which has some ability to distinguish between aerosol and cloud. There is discussion of this in Section 2.2 and it can be expanded.

RC: "How good are MODIS and MISR retrievals over Southeast Asia?" AC: This has been mentioned in other papers we have cited and already preformed over this region (Cohen, 2014; Cohen et al., 2017). In general, these products need to be used carefully, but the relationships found, when applying the techniques outlined here and in previous work, match extremely well with ground measurements from AERONET, within the errors of the 15% stated above. Additionally, model results forced with these measurements, have been shown to match very well with NOAA, Chinese, and other networks downwind throughout Southeast and East Asia. Additional comparisons about this can be included. And these matches have been demonstrated over not only the time period of this paper, but for the past decade. There are many errors using the satellite data, but the errors are sufficiently small as to not impact the analysis when looking at the events outlined here, due to their intense magnitude and spatial extent.



There is discussion of this in Section 2.2 and it can be expanded.

RC: "What is the mean AOD observed during the fire events?" AC: This was clearly something that the reviewer missed. I urge the reviewer to carefully look at Figure 3 and the text on lines 132-138. I am attaching them for reference:

RC: "Whether authors validated satellite and plume-rise model prior to this study?"

AC: Again, it seems that the reviewer did not carefully read the previous works of the author, in which careful validation of the measurements and models have already been performed. Perhaps, for clarity, additional paragraphs can be added to this work to further enhance this, without repeating verbatim. Specifically, a quick summary of the performance under the local conditions in Southeast Asia, which seems to be the reviewer's primary concern.

RC: "Why did authors use AOD and FRP from two different satellites instead a single satellite?" AC: We did not. All of the AOD and FRP measurements used in the statistics and figures (except for figure 1) are from MODIS. We used MISR only to constrain in space and time the domain that was influenced by the fires "fire region" and no influenced by the fires "non-fire region". This is explained in detail in sections 2.1 and 2.2. The explanation includes the major rationale, since MISR cloud clears better over this region, but has a lower frequency of measurement.

RC: "How does the atmospheric stabilization due to direct effect of aerosols affect the vertical transport of aerosols?" and "This may be attributed to several other reasons"

AC: As mentioned in the article, there is an atmospheric stabilization due to both the DIRECT EFFECT and the SEMI-DIRECT EFFECT. There is literature to support this, some of which has been cited in the draft. It is an excellent question, and a question we are currently tackling. Additionally, we talk about the issue of localized convection also not being properly resolved. The fact of the matter is that, at the present time, there are hundreds of papers being published with regional models (i.e.: WRF-CHEM) and global models (i.e. GEOS-CHEM), which do not also address these issues at all.



In fact, they are not even designed to allow for communication between the chemistry and the meteorology. The rationale of this work was to follow their approach, which is a currently accepted approach in the community, and to do an extremely comprehensive study. There is currently no other paper that has analyzed more than 10,000 daily data points of CALIOP measurements, and run a model jointly with more than 3000 MODIS daily fire hotspots and meteorological measurements, over this region of the world, that we can find. In fact, the papers that are generally cited over this region of the world do not even mention what methodology they use to analyze the measured data, nor details of which versions of the data they are using. Frequently, they make mere comparisons with CALIOP or AOD image files, without carefully looking at the data and making sure it is of high quality. I have even seen such articles published in this prestigious journal.

RC: "This has nothing specific to about El Nino, general to all large-scale fire events."

AC: This is not true, and the reviewer knows this. While this was a large-scale fire event, it was very special. The Monsoon has generally arrived in October, and hence October is usually the transition from the burning season to the rain season. As such, the meteorology was completely different during this period of time. It was heavily influenced by El Nino. The length of the burning season, the wetness of the soil, the large-scale meteorological flow, and such, were all not typical in this year. It is an interesting idea, however, to expand upon such and to compare the differences between El Nino and non El Nino years, however, the amount of data analysis to be required would be huge. In fact, this is a common mistake made by other authors in the past in this region of the world, and one of the major reasons why their results do not necessarily compare as well against measurements from AERONET, NOAA, Chinese measurement networks, etc.

RC: "Line 16: Authors mentioned that "our results are significantly different from what others are using". However, it is hard to find any discussion on this topic in later sections. Provide references to justify the statement." AC: Thank you. While there are

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already some references, more will be added, and the discussion will be extended as well.

ACPD

RC: "Line 39-46: Poor clarity and readability" AC: This will be addressed. Thank you.

RC: "Line 58: hydroscopicity or hygroscopicity?" AC: Hygroscopicity, thank you.

RC: "Line 101-107: Rewrite the sentence. Message is not clear." AC: Thank you. This will be addressed.

RC: "Line 110: What is the "reasonable approximation" mentioned here?" AC: References and some more detailed explanations of the physics will be added here. It is based on the physical relationship between backscatter and the aerosol optical properties, which are reasonably uniform when considered over the thousands of kilometers extent of the plume as a whole.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-1179, 2017.

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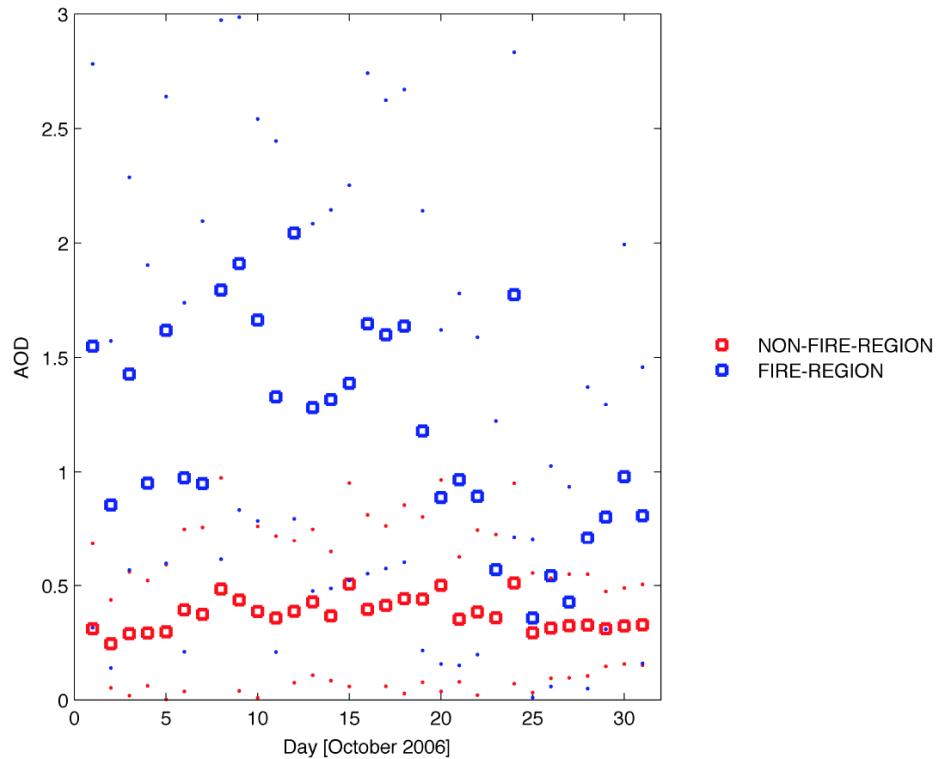
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132 *et al.*, 2007]. On the other hand, while some grids are contaminated, the sheer spatial distance of the plume
133 and the fact that the overwhelming majority of atmospheric aerosols during this time of the year are due to
134 fires, means that there is no significant bias in the overall statistics of the measured AOD [Cohen, 2014], as
135 observed by looking at the spatially averaged MODIS AOD and statistics over the fire-constrained and non
136 fire-constrained regions (**Figure 3**). The AOD is significantly higher ($p < 0.01$) over the fire-constrained
137 region, making the findings consistent with the approach employing the 12 years worth of MISR
138 measurements.

Fig. 1.[Printer-friendly version](#)[Discussion paper](#)

546 **Figure 3:** Time series of daily averaged measured AOD over the fire-constrained regions of the Maritime
547 Continent [blue], and the non fire-constrained regions of the Maritime Continent [red], as given in **Figure**
548 1. Circles are computed daily mean values, while dots are computed daily standard deviation bands.



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Fig. 2.

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