Response to Paragraph 2:

This approach was done to provide a first unified approach to understanding the drivers behind the large-scale smoke plumes found in Southeast Asia. These plumes extend for thousands of kilometers and are a recurring issue every year, although to a different extent in terms of space, time and length of occurrence, and magnitude. As of today, I have found no studies, other than Cohen, 2014, that have been able to successfully been able to reproduce the spatial and temporal extent of the fire plume over the past decade. Furthermore, the studies found consistently underestimate the atmospheric aerosol loading, and hence impacts on people, the atmosphere, and the larger climate and earth systems.

This work explains in detail what new approaches are required to improve our ability to reproduce the spatial, temporal, length of occurrence, and magnitude of the large-scale aerosol plumes found in Southeast Asia. This work addresses the issues of the usability and reliability of the remotely sensed measurements that underlay the analyses that others use. This work quantifies which regions, which measurements, and under which combinations are most effective.

This work is the first that has been robustly able to quantify the relationships between land-use change variables, AOD, and fire over this region on both inter-annual and intra-annual scales. While Cohen 2014 introduced the mathematical formulation, and also reproduced the AOD measurements, it used a different set of input data that was limited to monthly AOD, without inclusion of the higher temporal frequency and spatial scaling of the data used here. Cohen 2014 also went a step further to go through the entire modeling process to prove that the model could reproduce the results, which is beyond the scope of what is done here, but which is a next logical step for this work. This work, however, looks into various land-use measurements and measurements of the fires themselves, which is also new.

No other work, to my knowledge, as successfully done this. I have quantified the amount of variance in the onset, duration, and intensity of the burning events, from year to year, and over different geographical regions of Southeast Asia. I have found additional variability not previously talked about in other works, as well as reproduced other better known inter-annually and intra-annually occurring influences such as the Monsoon and El Nino events.

This work has also shown that the use of 8-day (better for onset and duration) or monthly (better for magnitude) measurements is much better than the daily measurements, which are commonly used in other works. This is an important finding that takes a significantly different approach from others' work. Also some insights are explained into why this is the case. This will clearly benefit future work in this field.

Response to Paragraph 3:

I disagree that there are technical errors relating to the satellite data and aerosol and definition of aerosols. I will address these issues as they are raised point-by-point. It appears that some of these misunderstandings are due to wording, and these will be clarified as appropriate, so as to make the point more precise and easy for the reader to follow. However, I do agree that including discussion of the observational errors is an added value and are modifying the manuscript appropriately, for example, as presented in Cohen and Wang, 2014.

Response to Paragraph 4:

First of all, at any given point on the Earth's surface, the vertical integral of the atmospheric extinction (scattering and absorption) of solar radiation is in fact the

reduction in the direct incoming solar radiation as observed at that point. The statement made is not technically incorrect. However, if you feel the other wording will make the paper easier to understand, I am happy to change it. It will now be similar to another paper of mine, Cohen and Wang, 2014.

Second of all, while I appreciate the reviewer's assistance with making sure that the science is precise, I do believe that some of the statements are unfairly picky and in the process the reviewer also makes mistakes. Case in point: the definition of "proportion" nowhere implies a fraction, which is the definition of "directly proportional", merely a one-to-one relationship. This clearly exists, since there is an exponential relationship between AOD and the extinction, which is itself a functional relationship of the aerosol's mass, number, shape, size, optical, and mixing state properties.

This same update is also applied in response to Paragraphs 12 & 13.

Response to Paragraph 5:
I agree with you on this point, and will make sure to include something similar directly in the text, in addition to what is already mentioned in the references.

Response to Paragraph 6:
This usage of one-to-one is completely appropriate. First of all, the paper goes into great lengths to explain some of the non-linearities, such as the vertical distribution, secondary processing, and impacts of meteorology. I am aware that many of these occur in the parts of the paper that the reviewer did not review, so the reviewer may have missed them. Secondly, the characteristics are such that there is very limited precipitation/removal near to the land regions, during these events. Thirdly, one-to-one implies that an increase in one variable will always lead to an increase in the other variable, which is completely true, under the conditions during which the fires occur in Southeast Asia. The fire's emissions dominate the atmospheric loading during these periods of time, with a direct relationship between an increase in emissions and an increase in the atmospheric loading, and hence AOD.

Response to Paragraph 7:
Thank you. This change is completely agreed to and made throughout the document. The actual term is the fire-count of all fires that have a confidence level of 7 or higher.

Response to Paragraph 8:
Agreed. The figures will be re-numbered so that they appear in the correct order.

Response to Paragraph 9:
Agreed. These changes about MODIS as an instrument on the TERRA and AQUA satellites will be made throughout.

Response to Paragraph 10:
The level 2 AOD, 10km swath product, collection 6. The version number will be added and the reference will be updated. Thank you.

Response to Paragraph 11:
Yes, the AOD product is 10kmx10km at nadir. The data is first interpolated to the 0.1x0.1 grid on a day-by-day basis, using only the quality assured data. It is then

averaged over an 8-day period, with missing values ignored during the averaging.

Response to Paragraph 14:
Actually, the AOD depends very heavily on the vertical distribution of the aerosols in a column. This is because, as you rightly point out, the radiative properties of the aerosols, with respect to water, size, and other effects, is a function of the vertical positioning. Furthermore, this is important, because there is additional multiple scattering and secondary absorption that occurs based on the thickness and height of the aerosol layer.

Response to Paragraphs 15, 16, & 17:
The specific versions of the products used will be made clear, along with more up-to-date references.

Response to Paragraph 18:
All reported AERONET data is used. Yes, this is important, and the variability, especially due to cloud cover changes, is high. Only data which is quality assured is retained, and then daily averages are computed.

Response to Paragraph 19:
The references for MISR have been updated. Actually, it was demonstrated in Cohen 2014 that MISR has a better correlation and a smaller absolutely error, with respect to AERONET, for monthly averaged values of AOD, over this region of the world. This is also helped out by the fact that MISR has a spherical AOD product that is relatively robust and reliable. However, for this work, in order to test the 8-day and 1-day products, you are correct in that MISR could not be used. This forms a significant basis for the findings and implications from this work.

Response to Paragraph 20:
Some discussion of the errors is included. Thank you for emphasizing the fact that the error is a function of the AOD. This is one of the beautiful things about using the EOF idea: it is searching for patterns in the variance of the data, and looking for the maximum. Therefore, it is making comparisons with the highest and lowest values and gradients in space and time. An unbiased error will not impact the result. However, this is taken into consideration with respect when being compared with the magnitude of the AERONET measurements. Furthermore, it is an interesting idea to explore for a follow-up work: looking at how propagating the errors forward between various different measurements can lead to quantifying the overall error of the modeled or measured end result.

In terms of the undetected thin cirrus, perhaps this is why MISR works better over this region? I am not sure, but this has also been added. The fact is that this argument cannot be used to preclude this work, since it has not been even raised by many other works that have used this data, over this region of the world.

Furthermore, by using the entire dataset from 2000 through 2014, we guarantee that any patterns that are consistently found are less likely due to some extreme event or mis-representation on a given day or a given season in a given year. This is an additional strength to the approach applied here, in that it is more statistically robust.

Response to Paragraph 21:

AOD does not have a lifetime; it is the aerosols that have a lifetime. First of all, the aerosol lifetime, during the burning season, is frequently more than 8 days over this region. The amount of rain is very low, a large amount of the aerosols are lofted into the free troposphere, and they can advect for a long period of time.

However, this misses the point. This paper is not interested in reproducing the travel and ultimate fate of a given aerosol from when it is emitted until when it is removed. The fires are so extensive in space and time, that it is the properties of the plume that are being modeled. In fact, it can be demonstrated that the behavior of the plume itself is what is important. Also, note that the lifetime of the plume itself is reasonably certain and well captured on this space and time scale. Furthermore, the fact that measurements have been made of these smoke plumes, frequently thousands of kilometers away, adds value to this conclusion.

Actually, the AOD is reasonable stable over these periods of space and time over this region of the world. This has all been explained in Cohen 2014 and in Cohen and Wang 2014, as well as other papers. References have been added and more background material has been considered.

Response to Paragraph 22:
This is the way in which the sensitivity study of the EOF analysis has been conducted. These are cutoff values corresponding to the most extreme values of the EOF, or the times during which the pattern is most significant.

Response to Paragraph 23:
The value is not arbitrary, as it is based on the statistical robustness of the field of the PCA*EOF. However, a sensitivity analysis has been performed, as explained above, and there is no significant difference if this number is varied a bit. Yes, it is important to make sure that the size of the errors in retrieval/sampling is accounted for, however this is not the same as the contribution to the variance, since the errors are likely unbiased. More on this is to be included in the write-up as well.

Response to Paragraph 24:
The variability of these two regions to the total AOD's variability over Southeast Asia, including from all sources, such as El-Nino, planetary dynamical events, regional dynamical events, small-scale perturbations, short-term anthropogenic events, cloud-cover, any bias in the data, urbanization from a few important but variable cities, development of new urban areas, etc., are 51%. These are the two largest contributions to the variance, and the only two that are more than 5%.

Response to Paragraph 25:
The correlation values are the R^2 values given between the respective Ti and the time series of the measured AOD averaged over the EOFi ranges.

Response to Paragraph 26:
This is not new information, but the value of existing measurements, taken on the decadal scale, has not been done before in the literature, based on what I could find. I have searched more and not found any other references for this.

Response to Paragraph 27:
The model result is the best one found in the literature for predicting the timing and the range of variability of the AERONET AOD. It is the only one that can

match this well without random scaling being applied. Yes, it is still far from perfect, but it just shows how much more we, as a community, need to do to understand what is happening in this region of the world.

This is more clearly explained and more references are added.

Response to Paragraph 28:

It is disappointing that the reviewer has not continued to go through the remainder of the paper. It is in here that most of the important scientific conclusions have been made.