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.

Response to Paragraph 2:
This will been made more explicit by mentioning a combination of missing emissions sources (fires), more complex in-situ aerosol processing in the atmosphere, and other dynamical events important in this region.

Response to Paragraph 3:
This is now tightened. The goal is to find a way to quantify what is driving emissions. But, there are currently no measured emissions. What we know is that when given emissions datasets are used, that models are not capable of reproducing the magnitude or timing of the measured AOD (and other aerosol measurements too) even over short time-scales, never the less over the annual and decadal scales.

Response to Paragraph 4:
Thank you. Your suggestion has been taken and adopted.

Response to Paragraph 5:
This sentence will be re-written, emphasizing the need to find ways to fill the gaps in the driving forces of fire emissions, and finding new ways to try to parameterize them, so that we can reproduce the AOD measurements.

Response to Paragraph 6:
The definition of AOD has been re-written, as also suggested by reviewer 1, and now follows what was previously used in Cohen and Wang, 2014 (the vertical sum of solar radiative extinction).

Response to Paragraph 7:
The other reviewer also questioned one-to-one. And while this is mathematically accurate, it is clearly a communication issue. Thanks to both of your recommendations, this will be re-written to be clearer. The point to be made is that there is a fundamental relationship that can be mapped and computed between the different species involved. The point is that physically, it is the emissions due to the fires that first and foremost, drive the variability in the AOD over these regions.

Response to Paragraph 8:
While there are many studies that have looked at this, I agree with you that they can be improved with new approaches, new data sets, and new ways of looking at the data. This is now included here. However, to date, besides Cohen, 2014, and possibly Cohen and Wang, 2014, there are no other papers that I have found, which look at this region of the world in a comprehensive manner for the spatial/temporal profiles over the decadal scale from this time period, and are thus able to match both inter-annual and intra-annual relationships.

This set of sentences will be improved following your recommendations. Thank you.

Response to Paragraph 9:
More details on the errors in the measurements will be written. Also, how cloud clearing has been done will be written. Furthermore, one of the reasons why the time-averaged data may match better, just as the MISR data matched better, is due to the cloud-interference issue being less of an issue in the dry-season over longer-time averages. In addition, how these may impact the results will also be brought up.

Response to Paragraph 10 & 11:
Reviewer 1 has raised these issues, and they will be responded to as described in that response. Thank you for pointing this out and helping to improve this part of the text.

Response to Paragraph 12:
Another sentence will be added to address the issue of cloud contamination. Yes, the pixels are removed from the results for the AOD, EVI, and NDVI. Only the quality assured data is used. This is one of the reasons why there are some regions that have missing data, even over the entire nearly 14 years worth of data. In the case of the fire counts, only those with a quality assurance of 7 or higher are retained.

Response to Paragraph 13:
All AERONET data was initially gathered. Only quality assured data were then combined into daily average values. These daily average values were then averaged to 8-day or monthly, as required, for those specific comparisons.

Response to Paragraph 14:
I could not find any standard value used in the literature. However, physically the angstrom exponent should be positive. In this case, 0.2 was used, because the results were relatively statistically robust. I have tried a few different values and there is little difference in the end result, with the data used, from a cutoff of 0.2 to 0.4. If the reviewer happens to have a better value to use, I can re-compute the results accordingly.

Response to Paragraph 15:
As also asked for by reviewer 1, a few sentences have been added to point out some of the differences between MODIS and MISR, including the relative advantages and disadvantages of each with respect to sampling width, ability to detect sphericity of particles, spatial resolution, and temporal resolution. It is also clearly mentioned that over this area, the results from MISR match the AERONET readings better, as found when compared against the results from Cohen 2014.

Response to Paragraph 16:
The 8-day product was formed as an interpolated value from the 16-day NDVI product. Both were tried, but the 16-day end result did not have the precise match in terms of timing as the 8-day product did, and did not have the precise match in terms of magnitude like the monthly product. This is likely because the lifetime of the plume varies on the order of a month, while the lifetime of the individual smoke emissions varies on the order of a week.

Response to Paragraph 17:
This is an excellent point, and has been added into the analysis. However, given that many of the fires are already burning in areas that have a significant amount of peat (and hence water) or are already in soil which is quite wet, but still burns, we did not want to prejudice the results. It is a good idea to explore more deeply, especially since there is a significant anthropogenic forcing to the fires in this region.

Response to Paragraph 18:
The goal is to build a predictive model that can still work under cloud-covered

conditions. It also is to build a predictive model that can work under conditions where low-temperature fires contribute a significant amount to the changes in the land and emissions, but are not necessarily observable directly as fires. Furthermore, the goal is to try to find a model that at least can be physically explained and yet capable of running in a GCM context, so as to provide for prediction of emissions. It is clear that the current fire hot spot method cannot reproduce the timing or magnitude in these regions of the world, for many known reasons.

Response to Paragraph 19:
They were chosen since the results are statistically significantly different from each other. A couple of sentences are now added to explain this.

Response to Paragraph 20:
A reference will be updated.

Response to Paragraph 21:
There has been no definitive work, that I have found, that has been able to answer this question. It could be an interesting problem for future consideration.

Response to Paragraph 22:
Good suggestion. This will be done as a list of the percentage of retrievals.

Response to Paragraph 23:
5%. This is now re-written, and the grammar is edited.

Response to Paragraph 24:
This work is now cited.

Response to Paragraph 25:
Yes. This connection is made clearer.

Response to Paragraph 26:
I have looked into the FRP values and am currently using them for a different study. The values in the FRP, while they are hypothesized to contribute in a functional manner to emissions of CO_2 and CO, actually have little functional relationship to aerosol emissions of BC and OC. Some of the lowest FRP fires can pour out more emissions, if they are burned under the right ambient conditions, while in other cases, very high FRP fires also can pour out more emissions. I think this is the right way to go, but it is also clear that current approaches relying on FRP cannot match the atmospheric measurements. So this work was to try to cover the more basic drivers first, and then move up the chain as our ability to understand the connections improve.

Response to Paragraph 27:
That is one of the beauties of this issue. Because the large-scale fire plume is roughly consistent in space and time on the 8-day to monthly-scales, the distance between the station and the fire sources only matters if they are significantly outside of the emissions region, and even then, only in terms of secondary production on AOD, in-situ removal on AOD, and/or as a time-lag. This is discussed in depth in Cohen, 2014. A couple of sentences have been added to this work to reinforce these points.

Response to Paragraph 28:
These results are quite good with respect to other work in the published literature, although there certainly are needed improvements. This work has also been done without the need for scaling, which is a fundamental issue that most of the models are doing today to get around the issue (as pointed out in Cohen and Wang, 2014, and Cohen 2014). This is now included in this paragraph.

Response to Paragraph 29:
The fraction of measurements that have been removed due to cloudiness can be estimated from the amount of missing values in the plots, as well as a look deeper into the statistical data. We did not retain directly the number of points removed from the QA flags. A sentence will be added to address this.

Response to Paragraph 31:
This is to be re-written, based on the recommendations of the first reviewer.

Response to Paragraph 32:
The spatial resolution issues will be commented upon, as also recommended by reviewer 1.

Response to Paragraph 33:
This is a mistake. The bolding will be removed.

Response to Paragraph 34:
This recommendation will be applied. The locations will be updated on the map..

Response to Paragraph 35:
Tables 4 and 9 are now re-done as a single new figure, using time-series plots.

Response to Paragraph 37:
Some tables have been eliminated, others have been combined into figures, and others have been combined into tables.

Response to Paragraph 38:
The reference in the text has been added, as addressed in the response to reviewer 1.

Response to Paragraph 42:
All of the results are given for the regions in which there is available data. No data has been interpolated or filled into these regions. Likely some form of interpolation would change the data and hence the result. However, this is likely one of the problems that has lead to the current inventories not being representative when used in real models. This is an interesting point to delve more deeply into for future work.

Response to Paragraph 43:
Sections 3.2 and 3.3 are now consolidated, with additional portions moved to supplemental material.