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 30:
A supplementary figure of the climatological-scale wind fields will be added as supplemental information.

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 and the remaining details moved to a supplement.

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

Response to Paragraph 36:
A very good idea. A new figure is now made from tables 6 and 7: the data is plotted on a single map plot, using dots of different colors and sizes to represent the information.

Response to Paragraph 37:
This will be done. Hopefully this will make the tables more readable and usable.

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

Response to Paragraph 39:
The caption will be updated and the plot's vertical structure will be made more precise.

Response to Paragraph 40:
Overlaying the fire data set on the make makes it very hard to read. However, the information will be provided in a separate additional map in the supplementary material that is made at the same spatial resolution.

Response to Paragraph 41:
The plots will be re-drawn to be easier to read.

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.