We would like to thank the reviewers for their detailed comments and time that they have put into providing their feedback. The process of considering and adapting to all of these comments has considerably tightened and improved the paper. For this reason, we want to thank you both again. For the outline, *the reviewers' respective comments are given in italics and underlined* while the responses are numbered and given in regular text.

Reviewer #2:

-52: What about 2015?

1. 2015 was an El-Nino impacted year, starting in the latter period (August-October) in the Maritime Continent. The data as analyzed in this paper however ended in 2014. Given the strength of the El-Nino was even stronger than in 2006, it would make an interesting follow-up to see if this approach will be able to replicate the results.

   -Introduction: Given the focus of this paper, at some point early in the paper, a spatial map of the distribution of fire activity is needed to orient the reader.

2. An additional 2 supplementary figures have been added, showing the spatial distribution of the fires in January 2013 and September 2013. Maps of the fires in January 2013 and September 2013 respectively are given in **Appendices C1 and C2**.

   -91-92: Please revise this statement to include some newer citations--much work has been done on quantifying the distribution of fires and aerosol impacts in this region even if it's not exactly what the authors focus on here.

3. Agreed. Additional references have been added. **Lines 88-101**

   -112: What about FINN or the daily GFED datasets, for example?

4. The daily GFED datasets have been used in a previous study, while FINN on the other hand as not been attempted by this author, and will be considered for future. However, both are still based on similar satellite-based approaches, although the specific satellites used do differ. The results were found to not be dramatically different, since the underlying processes that were used to construct both of these datasets still suffer from the same basic issues of pixels obscured by cloud/smoke, events that are too small to be measured as Fire Hotspots, etc. will still be overlooked. For example, I have personally observed and photographed multiple sizable fires in 2014 that show up in the AOD measurements, but do not appear in any of these datasets nor in the hotspot measurements of any of the satellites used by FINN and GFED. **Lines 802-820**

   -112-115: I'm not sure I understand what you mean by this statement. Don't fire emissions inventories directly relate changes in land use/land cover to emissions? Please clarify.

5. Fire emissions inventories relate changes based on burned area and/or fire hotspots. They are not necessarily based on changes in LAI/NDVI/EVI. Yes, over regions where hotspots and burned area are correctly representing the whole of the fires, there is a relationship. However, given the errors involved with measurement of hotspots, especially due to their transient nature, in this region of the world, there is a significant mis-match between the spatial and temporal distribution of the land-use change. This is further exacerbated based on the underlying assumptions that go into the hotspot measurements, especially when anthropogenic land use change is such a prominent factor. In this region of the world, one of the major findings here is that this relationship is not the best way to proceed if one is interested to understand the fire emissions. **Lines 168-216**

   -115-120: If these are important findings of the paper, I don't think that they belong in the

introduction but should be described in the abstract and/or conclusions.

6. Agreed. This has been moved and re-worded. **Lines 29-34 & Lines 836-875**

-163: Why not the newer Collection 6?

7. The newer Collection 6 was not a well known product and was also not fully validated throughout the entire time period of data analyzed in this paper. It would be interesting to look into this in the future, to see what impact it has. It has been recommended not to mix the various different collection data types by NASA. Additionally, I have used the Collection 6 data in other ongoing piece, and the results are not significantly different, in terms of high confidence fires.

-170: Did you look into this?

8. Yes, I have looked into this, and actually have a paper currently under review that analyzes this issue in much greater depth, using a different approach and set of measurements. The conclusion is relatively robust that the fire hotspots are indeed biased due to clouds, even over otherwise “cloud free” regions of Southeast Asia in many cases. I do not know how I can reference my own under-review paper however.

-175: What about seasonality? Is there a range of LAI drops over the course of the year, but is this much smaller than after a fire?

9. There is definitive seasonality in LAI. The drops associated with the fires are statistically significant across the entire dataset in this region. There are other drops which are considerably smaller and which are not always statistically significant as well. It is an interesting topic for further investigation. Perhaps grouping individual biomes or geographic regions will allow this to be more closely investigated, so as to obtain a baseline of what the natural variation is, albeit small in comparison for the region under investigation.

-183: What is the range of fire counts?

10. The range of fire counts, on a monthly basis, is from as low as 0 to as high as 5000 at level 8 and 600 at level 9. The article has been correspondingly updated.

-219: Can you cite relevant papers that have used MISR vs. MODIS AOD data in the region?

11. Citations added.

-224: Were the variables lagged at all? Wouldn't you expect the drop in the vegetation signal to occur after burning? Unless you're looking at water-stressed vegetation before? Please clarify

12. There is a lag between the variables. However, the lag is not so obvious when weekly averaged, and is not-observable when monthly averaged. This is likely a reason why the weekly average product performs well. The fact of the matter is that the data is too sparse on a day-to-day basis to obtain reasonable statistics, while on a weekly basis, one can determine changes of magnitude and position. It is interesting to see if there are oddly-timed lags, such as around El-Nino and other large-scale phenomena, although these have been been observed over the regions specifically examined in this work.

-232: Include a map of emissions (even if from another source of data)? Or table of different sources of burning for each region?

13. The fire hot-spot map is similar enough to GFED and FINN, while Cohen (2014) and Cohen and Wang (2014) provide additional maps from additional perspectives. The author has access

to yet another map, but given that it is still under review, it cannot be cited.

-236: How variable is rubbish burning over the year?

14. Rubbish burning is an interesting question over this region. While I have not studied it specifically, there is a highly variable component in the data and from personal observations. It would be an interesting point to study in follow-up work. There are communities and regions where it is much more prevalent, such as in Cambodia, Thailand, and Vietnam, while there are others where it is completely taboo, such as Singapore.

-245: Was this check done? How?

15. Yes, these checks were done, as reflected in the text now.

-253: Citation for aerosol lifetime in the region?

16. A few references have been included.

-270: Please address whether a lag should be used to measure the vegetation signal decline.

17. Agreed, it is an interesting point, worthwhile of future study. Text has been added here to clarify this as an important issue, and what small step we have done to start to address this.

-277: Is there any indication of how quickly different vegetation types would respond after burning?

18. A very good question, which we can not easily answer here, since a lot of the burning is done by people and the regrowth is intentionally in a species different from what was there before. However, with a little additional work, this could make a great topic for future study.

-285: Recent work on relationships between drought and fire seems relevant here (Field et al. 2016, among others).

19. Additional references have been added here.

-310: Also different magnitude of fire emissions?

20. This has been added.

-324-425: Can this go into a new named subsection?

21. This section has been made into a new numbered section.

-371-400: Much of this material could go in the discussion section.

22. This section has been combined into the section above.

-402: Figure/table reference? Also please check that figures are in the appropriate order for when they are mentioned in the text.

23. A figure link has been placed here. Additionally, the figure order has been completely remapped.

414: Have any of these papers compared NDVI with land use/land cover mapping?

24. An additional reference relating to the use of NDVI in this manner has been added.

    438-440: This sentence is unclear as written for emphasizing what is new about these results. Including fires improves your determination of the characteristics of fire timing seems somewhat obvious, unless I'm missing something here.

25. This has been clarified in the text.

    -452: How was the timing taken into account with 8-day products? NDVI/LAI before a fire vs. immediately after.

26. This is explained in detail in the text. The timing is constant from year to year, and based on the calendar date, not the time of any fires that may exist in the surrounding area. This allows for proper comparisons across different years and times of the years to be made.

    -Table 2 seems more appropriate as supplementary material.

27. Agreed. This has been moved and the appropriate numbers updated.

    -Figures 5-6: Direct relationships between the two AOD would be helpful here. And why are figures 5+6 split into different figures? Either combine or describe in the caption why they are separated.

28. These figures have been combined and updated in the text where they appear.

    -514: It seems relevant to comment in the discussion on the model not capturing the intensity, similar to other studies mentioned in the paper.

29. This is a good point. A sentence and additional references have been added. While this model still underestimates the measurements, it actually performs much better than most models in heavily biomass burning regions, which require a scaling factor of much more than the magnitude of this error. This is now made clear.

    -I think that the text in Section 3.2 can be condensed a fair amount to focus on the new results only.

30. The text has been considerably condensed and additional points and emphasis have both been made, as recommended.

    -535: Figure 10 is confusing to me, can you revise or at least make the caption more clear and/or add a legend. Also why is Fig. 10 cited before previous figures?

31. This figure has been removed and the text re-written.

    -713: I thought the analysis ended in 2013?

32. This point has been acknowledged. It can already be seen in December 2013, so that is not made clear, and it is mentioned that this would have maximized in February 2014, although it is beyond the length of the data specifically used in this analysis.

    -736: Please compare with previous estimates that underestimated (which was given as a motivation of the work) emissions. Does this model offer improvements?

33. This point has been made very clearly. Thank you. This makes the overall flow and conclusion stronger and more precise.

Minor comments:
-88: Sentence fragment, please revise.

34. The sentence has been re-written.

-112: Typo

35. Corrected.

-123: Why is 'FireCount' without a space? Is this just the number of fires? Perhaps describe as such.

36. The term has been explained in more detail, and the marker has been changed accordingly.

-134: Why is Figure 3 referred to before Figure 1? This might be an appropriate point to show the distribution of regional fire emissions, perhaps averaged over the years of interest.

37. The figure order has been updated to reflect the text. Also, additional figures have been combined when they make sense to do so.

-142: Space before TERRA.

38. Corrected.

-241: "In specific?"

39. Changed.

-243: Absolute what?

40. Clarified.

-Fix all references with (??) in the paper.

41. Completed.

-Equations 1-5: Make sure to define all variables.

42. This has been appended and the terms have been clarified, including the indices.

-Table 1: Say in caption what bolded values mean.

43. Tables have been made more self-describing. Others have been re-numbered. While others have been combined or moved into the supplementary materials.

-765: Typo.

44. Corrected.

Reviewer #1:

Abstract: This is quite wordy. Perhaps the authors can trim things down a bit, which I think will help retain focus and help the reader quickly appreciate the main results. For example perhaps the sentence on lines 9-12 can be removed from the abstract.

1. Agreed. This has been done. Other reductions in the abstract have also been made.

   Section 2.2, general: I suggest the authors add some text here to discuss a bit more why both LAI and NDVI are used, and what is gained from using both rather than one or the other. These are distinct but related quantities, in that they depend on the vegetation cover, type, and health, but in different ways, and are derived from some of the same MODIS wavelengths (through different processing algorithms, so they are partly but not fully independent data). The authors demonstrate later that they act as predictors in their model differently, so it would be useful to expand on the reasons for using both, and if possible the reasons that they do behave differently, in more detail.

2. This is an excellent and well thought out suggestion. A significant amount of text has been added in these sections. While the LAI product is more robust overall, identifying its variance is harder over deforested regions due to the smaller magnitudes. On the other hand, while it is less robust in terms of number of channels, the ratio of the variance of the NDVI to the absolute NDVI can be indicative of rapid changes over both forested and non-forested regions, and seems to help provide an extension into regions which are partially but not fully disturbed.

   Page 5, line 143: Remer et al (2005) is the reference for MODIS Collection 4 aerosols. The reference for Collection 6 for this product is Levy et al (2013). This should be updated.

3. Thank you for this update. It has been done.

   Page 5, lines 148-149: This statement is not correct. Firstly, there is a 'noise floor' component to the error term as well as the AOD-proportional error which is listed here. So it is not only a relative uncertainty. Secondly, the magnitudes given are incorrect, and the error is known to be biased (differing systematic biases in different conditions) rather than unbiased. The Remer et al (2013) reference given is to the MODIS Collection 6 3 km aerosol product, not the 10 km product which is what the authors actually use in the analysis. The reference for the 10 km product is Levy et al (2013). Some validation of the 10 km product is given in that paper (mostly over land, some ocean), and some validation of the ocean component in clean conditions is in Sayer et al (2012). The Collection 6 uncertainty estimates are +/-(0.05+15*AOD) over land and from -0.02-0.1*AOD to +0.04+0.1*AOD over water (i.e. the water uncertainty is biased high and larger than thought previously). The authors' point that this is probably still ok given the uncertainties in the models is probably still fine, but the discussion of the uncertainties should be corrected.

4. Thank you very much for helping to clarify these important points about the measurements. All of these points have been added. Additionally, extra text describing the impacts and magnitude of these errors, as well as the model errors has been added. This has really helped the contents to improve.

   Page 7, lines 208: The Holben et al (1998) reference given for MISR is the main AERONET network reference, not a MISR reference. This should be corrected here. I am not sure what reference the MISR team prefer people to use for this data product, Mike Garay or Olga Kalashnikova (at JPL) would be the best people to check with.

5. Thank you for catching this important oversight. This reference has been corrected.

   Page 7, line 212: It looks like there is a missing reference here (the paper pdf has "??" where I *think a paper reference is supposed to be indicated). I think this reference is the basis of the authors' justification for MISR as an appropriate reference for evaluation of their predictive model. From the response to the previous reviewer comments, I think this is Cohen (2014), but after reading through I'm not sure how that paper supports this argument. The use of MISR for evaluation is one of the issues I still have with this manuscript (it was an issue I had with the original version as well). The MISR swath width is around 350 km, in contrast to that of MODIS which is 2,330 km, which means that MISR makes observations about once per week in the tropics. So this is about 4 or 5 times per month; assuming half of these are cloudy means that a MISR monthly mean probably has only 2 or 3 days of data contributing to it for a given grid cell. Even if the MISR retrievals were perfect (and they have uncertainties of order 0.2*AOD, see Kahn et al 2010, i.e. comparable to MODIS and other products), there is a huge question of how representative the 2 or 3 times MISR observes per month are of the monthly average. The MISR team even say at meetings that they don't like to compare monthly means with other sensors, and they prefer seasonal means, because only after a few months do these sampling errors become small. I am not sure how the 'smaller error' referred to by the authors is due in part to the narrow swath width (line 212), perhaps this can be clarified or reworded? Having a narrow swath doesn't help decrease AOD retrieval error. It limits sampling which causes the opposite problem of representivity issues. Reid et al (2013) discuss some aspects of observability issues in this region and how this relates to climatologies and aggregated data. Reid's perspective is that temporal variability of AOD remains an issue in this area, which is contradictory to what I think is the authors' assertion that 2 or 3 samples per month is enough. In short, I don't believe that MISR can be considered a useful tool to evaluate the authors' predictive model. By all means do a comparison, but it should be called a comparison, not an evaluation, and I would not read too much into the quantitative results of this comparison. So I would suggest that, as well as filling in the missing reference here, the authors should focus more on AERONET and less on MISR in the later discussions, and be more clear about the strength of conclusions that can be drawn from it.*

6. Thank you for talking more in depth about the spatial coverage issues of MISR. You are absolutely correct that in general this is the case. However, interestingly over this region of the world, MISR actually has a much better correlation with AERONET and a lower error. It is partially due to the fact that MODIS cannot clear the clouds as well, and also due to the fact that at very high AOD levels (AOD>2) that MODIS assumes that there is cloud, when in fact there is aerosol. I have added a reference to support this. I have also made careful observations with MODIS, AERONET, and MISR and found this to be the case, on monthly time scales. However, in order to obtain data at weekly or daily scales, the swath width is a huge issue, and in fact MODIS is far superior. Although I am sure that at the glob
al scale, you are right, based on my work in Southeast Asia and my reading and talking with experts on the ground there, I believe that in this case, MISR is an appropriate instrument to use. I have added in extensive cautions however, as you have mentioned. It will be interesting to see how this works in future studies to see if others can also confirm these findings.

   Section 2.4: I think the start of this section is another good place to go into more detail about the specific differences between LAI and NDVI which make both useful here. It sounds like the authors say (lines 275-277) that NDVI should recover more slowly than LAI? Is that right? What is the biological mechanism for this? Are there other studies that can be cited in support of the differing responses of these two retrieval products to burning?

7. This is also an interesting point. There is recovery in greenness, as reflected in the NDVI, which is related to the chlorophyll, and in the moisture content of the canopy and the soil, which is better represented in the LAI term. However, most of the fires are occurring in regions that are heavily managed or that are reasonable unmanaged but are being converted to managed regions. Furthermore, when the Monsoon Rains arrive, there will then be sufficient water to overcome the moisture issue. For these reasons, any lag between the variables would be expected to occur on time scales which are being managed, and with the complete destruction posed by the extensive fires, are not expected to be so obvious. Perhaps a different approach

to looking at the regrowth, for example in areas that are managed differently would yield an interesting solution, or in regions that use different types of crops to regrow after the fires, but that is beyond the scope of the study done here.

*Page 10, lines 325-326: Again, it is not clear to me why 5% of the total variability corresponds to the magnitude of variability to give confidence that something is "real" and not caused from the uncertainty in the measurements themselves. Can the authors provide more information here on how this measurement uncertainty is transformed to estimate the contribution to variance? Is it as simple as saying the MODIS AOD uncertainty is 5%, therefore we need at least 5% of the variance for a mode to represent something useful? Or is this something more complicated? If it is the former, then this should probably be updated to reflect the Collection 6 MODIS uncertainties (see prior comment), and what about the uncertainties in LAI, NDVI, rain, and fire count? In the response to reviewers the authors say "The value is not arbitrary, as it is based on the statistical robustness of the field of the PCA*EOF… More on this is to be included in the write-up as well." , however I did not find these details in the paper. Or does the 5% come because 5% (or p=0.05) is just a commonly-used metric of significance by many people doing statistical tests? I looked through the Björnsson and Venegas (1997) reference cited earlier and that seemed to be what they were going by (their Section 4.3); if that is the case, perhaps that paper should be cited again at this point.*

8. There are two ways in which to look at this issue. The first is, what fraction of the total variance is covered by the mode, and the second is what physical amount of change in the underlying variable is contained by this change in the variance. Clearly, and variance captured which is smaller than the error of the measurement itself is highly circumspect and to be confident, is ignored as not being meaningful in this analysis. Furthermore, if the absolute change in the magnitude of the associated pattern represented in the high variance region is sufficiently small, such as major changes in a mostly random manner, then it is likely to be more representative of noise than a signal. In both cases, the 5% value chosen here is related to the error in the measurements themselves. This is now more clearly made in the text and the reference is updated as well. In reality, such a strong constraint may not be required, especially since the bias in the measurement is low, but the point is to be more conservative scientifically. Thank you again for helping to clarify this.

General: in the authors' response, the authors confirmed that the term 'correlation' in the paper refers to R^2, the coefficient of determination (the square of the correlation coefficient). However I didn't see this stated explicitly in the revised version of the paper. I suggest the authors either change to the more standard terminology or else state that by correlation they mean coefficient of determination.

9. This has been clarified in the text.

Figures: The font size is somewhat small and hard to read when printed out, or viewed on screen without a lot of zooming. I suggest the authors increase this a lot (2-6 points perhaps, dependent on figure). Figure 3 is ok but the others are hard to read. I know that for the time series plots this will mean some axis labels will have to be deleted as larger font won't fit (e.g. Figure 5), but I think in these cases 1 tick per 2 years is probably sufficient to track time (as opposed to the current 1 per 3 months).

10. Considerable work has been put into re-arranging the figures, updating the fonts, etc. Additionally, some figures have been moved to supplementary data or have been added as new supplementary data.

Figure 10: I appreciate the intent of this figure, which shows that 4 AERONET sites follow more or less one pattern in terms of high-AOD months while the other 7 follow a different pattern. Perhaps there is another way it can be plotted? As it is the y-axis is numbered 1-11 without further legend, and the sites are referred to in the caption by symbol color and shape, which is

hard to make out as the points are small and colors are repeated. Maybe something more like a Hovmoller plot could be plotted, where each month is a binary shaded/unshaded. The y axis could give site names (or if numbers are still used, these can be put in the caption). If there is not enough space to fit names then the figure could be flipped so site is on the horizontal axis and time on the vertical.

11. This plot has been removed and the data has been presented in a different manner through other figures and tables, both in the main paper and the supplementary material. Thank you for the suggestion!

    Dennis et al (2005) reference: this appears to be typeset incorrectly as author names appear as a string of initials.

12. Corrected.

    Fuller and Murphy reference: appears as "fuller" rather than "Fuller".

13. Corrected.