Responses to reviewe comments

[Review #1]

I sincerely appreciate the responses to reviewer comments and believe the expanded comparisons and discussions strengthen the paper. At this stage I only offer minor technical corrections:

[General response] We appreciate the reviewer's efforts for reviewing our manuscript and the valuable comments and suggestions, please find below the detailed responses.

-The responses to 'multi-day burning' and revisions are appropriate and appreciated. The explanation in the text, however, is somewhat confusing. It's not clear which fires from different days are grouped together, and what the term 'extra fires' means.

[Response] We have improved the descriptions in section 2.5.2 and hope it's more clear.

-[pg 24, lines 10-11] This is true (GFED observational dataset doesn't include fire duration), but not for the reasons listed. The burned area is based on the MCD64A1 MODIS product, which uses a vegetation index-based contextual algorithm to assign burning to 500m pixels. Although informed by active fires, this change index really isn't capable of giving fire duration information. That would require integration with active-fire based interpolations, which is feasible and an area for future research. The limitation is not really a function of data gaps, cloud cover, etc., as stated in the paper.

[Response] We think detailed discussion on the issue of satellite-derived fire persistence time goes beyond the scope of the current manuscript. So we simply removed the latter half sentence within the parenthesis to avoid any potential confusion. However, we provide some brief discussions here as a response to the reviewer's comments.

We agree with the reviewer that the data gap (for example, due to cloud cover) is not the only factor posing difficulties to retrieve fire persistence information from satellites, which is incorrectly suggested as in the original Line 10-11, Page 24. To extract fire patch information from pixel-based burned area data, a minimal temporal distance has to be imposed when grouping the connecting burned area pixels into a single fire patch. This limit was set as 8 days in Archibald and Roy (2009), 2 days in Archibald et al., (2013), and 14 days in Hantson et al., (2014). Thus the uncertainty of fire patch persistence time depends on this threshold and the uncertainty of detected pixel burning date. The date of detection for burned pixel is not exactly the date of burning, as pointed by the reviewer. Boschetti et al. (2010) showed that the date of detection is within 8 days of real burning date for the MCD45 burned area data. Combining active fire and burned area detection will allow more accurate location of the burning date, but both approaches are limited by the period of cloud-free observations. Because clouds indeed mask otherwise detected fires, (either in form of burned area or active fire), interrupt the temporal continuousness of the data, and increase the uncertainty of the resulting fire patches. To our knowledge, no targeted study has been reported to evaluate the reliability of satellite-derived fire patch persistence on the global scale, so this information was not used in our analysis.

[pg 24, lines 12 - 28] This added paragraph is important but not clearly written at present. Please consider some grammatical revisions. Examples include the run-on sentence in lines

25 - 28 and unnecessary comma in line 20. In general the explanation is hard to follow.
[Response] We tried to improve the descriptions for this paragraph (which become two paragraphs after revision) and hope it's more clear.

-[pg 23, line 11-13] Could consider including the 'response supplement material' into the paper's actual Supplementary material, as it may be interesting to readers who wouldn't look for the response supplemental material.
[Response] It's now included as section 2 in the Supplement Material and is referred to in the main text.

-[Figure 3] Please include y-axis labels.
[Response] y-axis label now included.

-[pg 2, line 14] Inappropriate use of semi-colon. Same in pg 12 line 25, pg 19 line 28, pg 22 lines 23 and 25, pg 27 line 24
[Response]
Page 2 line 14: changed into comma
Page 12 line 25: changed into comma
Page 19 line 28: changed "; followed" to ", which are"
Page 22 line 23: changed into comma
Page 22 line 25: changed into ". However,"
Page 27 line 24: changed the semi-colon to ".", followed by a new sentence.

-[pg 9, line 13] Change to 'Evidence shows…', same in pg 25 line 9
[Response] Change made.

[pg 19, lines 4-15] Here and throughout I would suggest using 'large fires' instead of 'big fires', which is a little ambiguous.
[Response] Change made.

-[pg 24, line 5] Tilda in 40~60 days should be dash: 40-60 days. Same in pg 25, lines 5 and 7, pg 26 line 10, pg 27 line 24 And I would suggest removing the statement, "if we trust the fire agency data…". This sounds overly-skeptical and I think generally, especially for the large fires, the estimates are pretty good.
[Response] The phrase was removed. We checked all dashes and hyphens throughout the texts to make sure they're properly used.

-[pg 24, line 14] The phrase "As our model development is similar to their work" doesn't make sense in this sentence, since you're pointing out how it's in fact different.
[Response] We removed this sentence and re-wrote the last two paragraphs of section 4.2.3, and hope it's more clear.

-[pg 24, line 18] Remove "essentially" here
[Response] Change made.

-[pg 25, line 11] Can include more recent references: Mu et al. 2011 (10.1029/2011JD016245), Zhang et al. 2012 (10.1029/2012D017459)
[Response] References included.

-[pg 28, line 14] "Make over-compensation" is not grammatically correct
[Response] The sentence is changed to, "ignitions seem to be excessive, leading to larger burned areas in spite of the small simulated fire sizes.".

[Review #2]
The authors improved and modified the manuscript and their analysis strongly. They answered and clarified all my questions and concerns. I have only a few miner points now.
[General response] We appreciate the reviewer's efforts for reviewing our manuscript and the valuable comments and suggestions, please find below the detailed responses.

On page 9 line 4 of the revised manuscript the authors state that land use change fires might influence the comparison. But acutally not only the fires related to land use change might be different using the static map, also the presence or absence of croplands influences the fire occurrence in reality (and I guess also in the model, either croplands burn or the cropland area is not available for fires). Pastures also burn frequently in reality as fire is a tool to avoid woody encroachment in many regions. In the model the pastures consist of grass fuel, which has a higher flammability, and therefore pasture areas would burn more frequent than forests. Therefore a static land cover map influences the modelled historical fire occurence.
[Response] We agree with the reviewer on the double effects of the land cover change on the burned area. Cropland fires were not included in the model (as explained in section 2.3); pasture fires (considered the same as grassland) fires were included in the model. The double effects of land cover change on the burned area are now included at the end of 3rd paragraph in section 2.3.

The authors modified and improved the manuscript strongly.
The description of the multi day fire approach is a little unprecise at some points:
the authors state that "some" fires persist. It is not clear which fires are these: is it all fires where fire conditions are good, e.g. always when it does not start to rain and there is still enough fuel? Later on they state that " the remaining patches were constructed similarly", what exactly does this mean?
[Response] We have improved the descriptions in section 2.5.2 and hope it's more clear.

p. 15, l.27: "compared that" change to "compared to that"
[Response] Change made.