Reply to the reviewer #3

Thank you very much for your helpful comments. Based on your comments, we have improved the manuscript.

General comments: This study examines global warming impacts on fire activities, focusing on the burned area and fire emissions of CO2 and PM2.5 over equatorial Asia. Considering June-November 2015 when a strong El-Nino induced a large decrease in precipitation over the area, the authors examine changes in the probabilities of droughts and fire activities due to anthropogenic influences using the MIROC5 AGCM large ensemble (100 members) simulations. They find increased probabilities of the droughts and fire activities as global warming become stronger. In particular, they show that 3.0 degree warming that represents the current mitigation policies would bring severe droughts and increased fire activities due to the intensified El Nino at near 100% chance. I find this paper overall well written, providing interesting and policy-relevant results. However, there are a few issues, mostly related to uncertainty factors, which need to be improved through revision.

Thank you. Please see our replies to the following comments.

Major points: 1. Model dependency: It would be useful to discuss limitations of the atmospheric model experiments and its possible impacts on the results. Particularly, precipitation changes in the future warming simulations are shown to be critical for determining changes in fire burned area and CO2 and PM2.5 emissions (Fig. 8), but atmospheric models tend to have large biases in precipitation over the Tropics partly related to the omission of air-sea coupling. It seems that normalized precipitation is used to overcome this problem but some justification would be needed with showing precipitation bias of the model. In addition, future projections of precipitation look highly dependent on the SST change patterns (Fig. 4). Uncertainty in these SST change patterns needs to be discussed as well.

Unfortunately, MIROC5 is only one model that produced all the Nat, Hist, 1.5°C, 2°C and 3°C ensembles. Therefore, we cannot use the other HAPPI models for this study. With a simple bias correction (i.e., dividing precipitation anomalies by their standard deviation values), the MIROC5 model has very good hindcast skill regarding interannual variability in the EA-averaged $\triangle P$ and $\triangle\omega 500$ (correlation values between the model and observations are ~0.9) [lines 134-145]. We also suggest that our future projections are consistent with previous studies that have analyzed the CMIP5 ensemble: Lestari et al. (2014) and Yin et al. (2016) also showed that the coupled model ensembles of CMIP5 projected future drying trends and enhanced fire carbon emissions [lines 263-266]. We also add a caveat in lines 280-282.

2. New findings: New results compared to previous studies are not clearly explained, in particular, in view of Lestari et al. (2014). What advances have been achieved by increasing ensemble size? Adding more information on this would be helpful, such as how to construct ensembles and how uncertainty is assessed with the large ensemble simulations.

Although Lestari et al. (2014) showed anthropogenic effects on the historical trends of droughts, it is not clear how historical climate changes affected the *particular drought event of 2015*. Based on probabilistic event attribution, we investigated whether historical climate changes affected the 2015 event. Because the 10 member ensembles of Lestari et al. (2014) are too small to estimate the probabilities of extreme events, we use 100 member ensembles of Shiogama et al. (2014). The computing cost of AGCM is lower than AOGCM, which enables us to perform such large ensembles that are necessary for PEA. [lines 59-78 and 204]

Although Lestari et al. (2014) and Yin et al. (2016) showed increases in droughts and fires in the future projection ensembles of AOGCMs, it is not clear how future anthropogenic warming affects droughts and fire when 2015-like El Niño events occur in a future warmer climate. It is important to investigate changes in extreme events at 1.5ºC and 2.0ºC warming levels to inform stakeholders, as the Paris Agreement set the 2°C long-term climate stabilization goal and is pursuing 1.5 °C to reach stabilization (United Nations Framework Convention on Climate Change 2015), but Lestari et al. (2014) and Yin et al. (2016) did not perform such analyses. In this study, we examine how the probabilities of drought, fire and fire emissions of CO_2 and $PM_{2.5}$ would change when major events like the 2015 El Niño occur under 1.5ºC, 2.0ºC and 3.0ºC warmed climates. [lines 79-95 and 263-266].

We added details of the experimental designs in section 3. We also explain how uncertainty is assessed with the large ensemble simulations below.

Also, the empirical relation between precipitation and fire activities is used to estimate future changes in fire activities and the authors consider its uncertainty somehow in their analysis. I think this part is important and more details needs be provided on its uncertainty ranges and associated impacts on main results. See my specific points below.

L141-144: This way of sampling looks important to capture uncertainty arising from internal variability, and showing resulting spreads in P and omega responses in Fig. 7 would be interesting. Also, it would be useful to explain here how to construct CDF using 1000 samples and estimate probabilities exceeding the observed value and its 10-90% confidence intervals. L161: “1000 random samples of the regression factors in Eq. 1”. Please provide details given its importance. Also see my major comment.

We explain how to construct the CDFs and estimate the uncertainty ranges by using the large ensembles and the resampling techniques as follows.

“We also estimate the 10%-90% confidence intervals of the fitting curves by applying a 1000-time random sampling of the observed data: we randomly resample 20-year samples from the original 20-year (1997-2016) data and compute a and b; we repeat the random resampling process 1000-times; we consider that the 10%-tile and 90%-tile values of the 1000 regression lines indicate the 10%-90% confidence intervals. ” [123-126]

“Here, we use the cumulative histograms of 100×10=1000 samples of △P to estimate the probabilities of △P. The values in parentheses indicate the 10-90% confidence interval estimated by applying the 1000-time resampling: we randomly resample 100×10 data from the original 100×10 samples of △P and compute the probabilities of drought exceeding the 2015 observed value; we repeat the random resampling process 1000-times and consider the 10%-tile and 90%-tile values of the 1000 estimates of probability as the 10-90% bounds.” [lines 206-210]

“ We consider uncertainties by combining randomly resampled △P and resampled regression factors of Eq. 1: (i) we compute the regression factors of Eq. 1 using randomly resampled data (the same as the process used to estimate the uncertainty ranges of the regression lines); (ii) we randomly resample 100×10 data from the original 100×10 samples of △P; (iii) we use the regression factors of (i) and the 100×10 △P samples of (ii) to compute the 1000 estimates of fire or emissions and estimate the probability of exceeding the observed values; (iv) the processes of (i)-(iii) are repeated 1000-times; and (v) the 10%-tile and 90%-tile values of the 1000 estimates of the probabilities of exceeding the observed values are considered to be the 10-90% bounds.” [lines 215-221]

3. Implications: The last part on implications is rather confusing and hard to follow. I would suggest rephrasing it for better understanding. For example, it is unclear what are exactly

compared between MIROC5-based estimations and diverse SSP scenarios: fire CO2 emissions due to climate change versus land use CO2 emissions? From this comparison, the authors seem to suggest that additional fire CO2 emissions due to climate change should be considered in SSP scenarios, but this interpretation is not that clear at the present form. I am wondering if it can be made more specific by suggesting how much increase in CO2 emissions should be added, for example.

We improved this paragraph [lines 228-248]. Currently, it is not easy to compute fire CO2 emissions due to future drying in AIM/CGE because we have to develop a new fire module considering climate change effects on fire for AIM/CGE. The development of such a new module is an issue for subsequent CMIP7 activity.

Specific points: Title: “year 2015” sounds a bit strange to be connected with “future” warming effects. How about saying “2015-like” or similar instead.

We changed the title to “Historical and future anthropogenic warming effects on droughts, fires and fire emissions of CO2 and PM2.5 in equatorial Asia when 2015-like El Niño events occur”

L87-88: How is the EA box selected? I think it can be adjusted (e.g., narrower in zonal direction) to better capture the P decrease area. Or it doesn’t matter since only land is considered? Please clarify this.

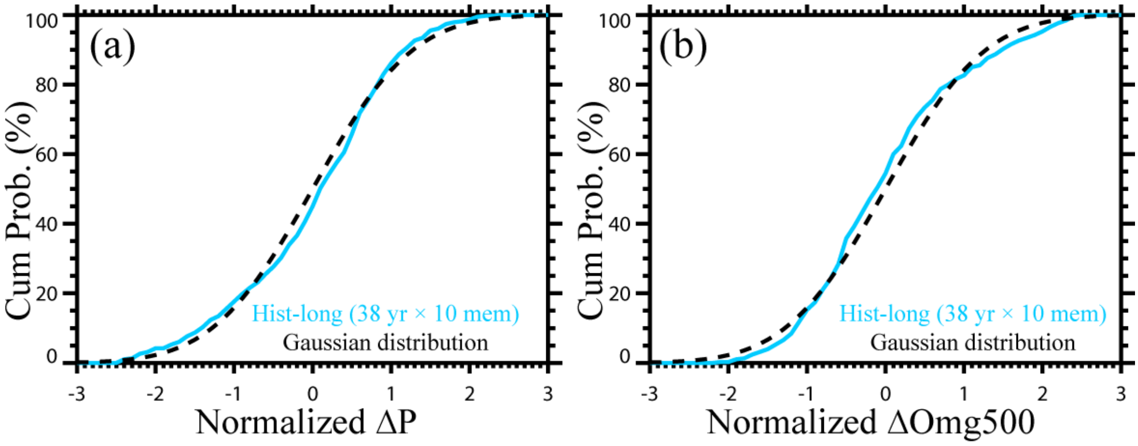
We apologize that the EA region shown in the original Fig. 1 was incorrect. Actually, we use the definition of the EA region of GFED4s. We show the EA region in Figure 1g and explain it in the caption of Figure 1 and lines 36-37.

L91-92: In line with “precipitation anomalies and accumulated water deficits”, wouldn’t it be better to use accumulated precipitation like SPI?

We use the June-November mean precipitation, which is the accumulated precipitation during the dry season divided by the period length. Therefore, we substantially use the accumulated precipitation anomalies.

L93, L108: “divided by standard deviation”. Can we assume normality for precipitation and omega anomalies? Area averaged 6-month mean values might be okay but a quick check would be useful.

The following figure shows the cumulative distribution functions of normalized precipitation and omega anomalies and those of Gaussian distribution. It is suggested that the area-averaged 6-month mean values have a Gaussian distribution.



L115: I would suggest providing more details on how "long-term anthropogenic signals were removed" as SST patterns are important for determining precipitation responses to El Nino.

Anthropogenic SST changes were estimated by taking the ensemble mean differences between the all-forcing historical runs and the natural-forcing historical runs of the CMIP5 ensembles. The multi-model averaged anthropogenic signals were subtracted from the HadISST data, and the Nat sea ice was estimated by using empirical functions between observed sea ice concentrations and surface temperature. [lines 153-159]

L122: "100 member ensembles during 2006-2015 with 1.5 degree and 2.0 degree warming". Do it mean that ensemble runs are performed only for Plus15 and Plus20 or there are 100-member HIST runs for 2006-2015 as well?

We performed 100 member runs of 2006-2016 for each of Hist, Nat, Plus15 and Plus20 [lines 158-159, 160-161 and 166-167]. The Plus30 are 100 member runs of 2006-2015 [line 176].

L150-153: Why stronger El Nino (and P responses) are simulated in 1.5 degree warming simulations than 2.0 degree ones? Some discussion needs to be provided. Does it occur in all

1000 samples? Do other HAPPI models share this or is this a characteristic of MIROC5?

All the HAPPI models share the SST anomalies that were taken from the CMIP5 model ensembles [lines 160-175]. It is not clear why the ensemble average of the CMIP5 RCP2.6 runs (i.e., the prescribed SST anomalies of the 1.5 ºC runs) has a larger SST difference between the Niño 3.4 region and the tropical ocean mean than that of the weighted sum of RCP2.6 and RCP4.5 (the 2.0 ºC runs) [lines 201-203]. The differences in the prescribed SST warming contrasts between the 1.5°C and 2°C runs cause the difference between the blue and green CDFs in Fig. 6a [195-212].

L163-164: Please explain how to assess significance of this change.

By comparing the uncertainty bounds of future changes with the uncertainty bounds of Hist and Nat, we assess the significance of changes.

L169: Why is the emission of Japan used as reference here?

We included this comparison because it was highlighted that the fire emissions of EA exceeded the fossil fuel emissions from Japan when the 2015 massive fire event occurred (e.g., Field et al. 2016). However, readers other than Japanese readers may be not interested in this comparison. Therefore, we omitted this paragraph.

L172: First sentence. This needs to be mentioned clearly above and also in figure captions to avoid confusing.
L172-188: This paragraph and Fig. 9 are hard to follow with many skips and limited explanations. Please consider rephrasing it. See my major comment above.

We have improved our explanations in lines 228-248.

L196: “82%, 68%, and 93%”. Please add uncertainty ranges or indicate these are ensemble means or medians. Same for L204.

We add the uncertainty ranges in lines 254, 256 and 261-263.

L199-202: Model dependency issue is here. How representative is MIROC5 projected precipitation in the future? Any comparison with other models would be useful. See my major comment above.

Please see our responses to your major comments.

L205: “additional changes”. Are these significant?

Although the differences between 2.0°C and 3.0°C are not statistically significant for the burned area and the CO_2 and $PM_{2.5}$ emissions, the 50th percentile values of probabilities exceeding the 2015 observations first reach approximately 100% in the 3.0°C runs. [lines 270-272]

L209: “modifying fire CO2 emissions scenarios”. Can authors suggest how much modification is needed? See my major comment above.

Currently, it is not easy to compute fire CO2 emissions due to future drying in AIM/CGE because we have to develop a new fire module considering climate change effects on fire for AIM/CGE. The development of such a new module is an issue for subsequent CMIP7 activity.

Fig. 1: Line 342: “left panels” should be “right panels”.

We corrected this issue in the caption of Fig. 1.

Fig. 2: There seems to be a stronger case than 2015, perhaps 1998? Where is 1982 that has also a stronger P decrease in Fig. 3? It may affect fitted curves.

We apologize that the explanations of the original manuscript were not corrected. Although we used 1979-2016 GPCP data, GFED4s covered only 1997-2016. Thus, Fig. 2 shows the scatter plots between precipitation and GFED4s during 1997-2016, not 1979-2016. We corrected this mistake in lines 116-119 and the caption of Fig. 2. Therefore, 1982 is not included in this figure. The 1997 case (the 1997-1998 El Nino) is stronger than 2015 case. We indicate 1997 in Fig. 2.

Fig. 3: Indicating 2015 case in time series and scatter plots would be useful. Is there any underestimation or overestimation by models in P and omega responses?

We indicate the 2015 case in Fig. 3. The model estimates P and omega responses well [lines 136-145].

Fig. 6: It’s not clear why difference from NAT is shown even for future changes. Is this for 2015 or using all years?

We show differences from NAT because the mixing of differences from Hist for future changes and that from NAT for Hist may confuse readers. These figures are for 2015 (caption of Fig. 4).

Fig. 7: Is this also for 2015? Related to my major comment on model dependency issue, are these are supported by other coupled models?

This figure is for 2015. Please see our responses to your major comments.

Fig. 9: Difficult to understand. How is the CDF of CO2 emissions (red curves) estimated? Are these CO2 emissions only due to increased fire over equatorial Asia?

We improved the descriptions in lines 228-248.