We thank the reviewer for his/her careful reviews and helpful comments. The manuscript has been revised accordingly and our point-by-point responses are provided below. (Reviewer's comments are in italic and the responses in standard font).

Reviewer #2

General comments

My only somewhat major comment is that the climate responses explored (i.e. temperature and precipitation) are based on atmosphere-only experiments, and therefore are somewhat incomplete. That does not mean that it is not worth showing the results, but it should be clearly stated that these results come from fixed-SST simulations, and therefore more work will be needed in the future in a coupled framework to understand the role of fire aerosols on climate in a more complete fashion. Adding a few sentences in the abstract, the corresponding section and in the conclusions would be sufficient for clarifying this better

Reply: Following the reviewer's comment, we have added the statement in the abstract, the corresponding section and in the conclusions that these results (i.e., climate responses) are based on atmosphere-only experiments with fixed-SSTs, and more work using a coupled atmosphere-ocean model will be needed in the future to understand the role of fire aerosols on climate in a more complete fashion.

Specific Comments:

Page 2, Line 32: Please remove "the".
Reply: Done.

Page 2, Line 38: Not sure whey a range is indicated both by two numbers and by the +/-
Reply: We changed to "-0.05 W m^{-2} and 0.04±0.01 W m^{-2}, respectively based on two calculation methods". The first number does not have an uncertainty range, since it is derived from a clean calculation.

Page 2, Line 39: South Africa -> southern Africa (here and elsewhere in the text).
Reply: Done here and elsewhere in the text.

Page 2, Line 48: Suggest stressing that this effect is small and insignificant.
Reply: Done. We added this information to the abstract.

Page 2, Lines 45-47: Need to clearly mention here that this is inferred from atmosphere-only simulations (and not from full coupled climate simulations).
Reply: We added this information in the abstract.

Page 3, Lines 55-56: Worth citing the review paper by Voulgarakis and Field (2015) here, as it is very relevant.
Reply: Done.

Page 3, Lines 59-60: Worth citing the paper of Bistinas et al. (2014) here.
Reply: Done.

Page 3, Lines 61-63: This reads as if this manuscript will fill the gap of knowledge of how fires will change in the future, which is not the case. Please rephrase to something that aligns better with the focus of the manuscript (or you could remove the second part of the sentence entirely).
Reply: Following the reviewer' comment, we removed the second part of the sentence.

Page 3, Line 72: Suggest changing "indirect effect" to "indirect effects"
Reply: Done.

Page 4, Line 76: Suggest removing "the" before "climate change".
Reply: Done.

Page 4, Lines 76-78: Well, it depends. RE is not always for both anthropogenic and natural. Sometimes we just study anthropogenic or natural RE individually. I suggest rephrasing to "RE represents the instantaneous radiative impact of atmospheric particles on the Earth's energy balance"

Reply: Thanks. We revised the sentence as the reviewer suggests.

Page 4, Lines 78-81: Similarly, RF does not have to always be pre-industrial to presentday. I suggest rephrasing to ". . .as the change of RE between two different periods, e.g. the pre-industrial and the present-day. . .", and then change the second half of the sentence accordingly.

Reply: Thanks. We have revised the sentence accordingly.

Page 5, Line 98: many -> some
Reply: Done.

Page 7, Lines 147-148: It is mentioned that two methods are presented – worth briefly mentioning them here.

Reply: Thanks for the suggestion. We added the following sentence to briefly mention the two methods: "One method estimates the DRE with different model simulations

[*Ghan*, 2013], and the other one calculates the DRE directly by multiple diagnostic radiation calls in a single simulation."

Section 2.1: Is the aerosol interactive with the model's chemistry?

Reply: The secondary aerosol, e.g., sulfate is produced from the model's gas and aqueous sulfur chemistry. The version of the model we are using in this study does not include a full-chemistry mechanism, and the oxidants (e.g., OH, HO_2 and O_3) are prescribed [*Liu et al.*, 2012].

Page 8, Lines 179-180: Any other performance features apart from the Arctic? What about over key biomass burning regions, and what about OC?

Reply: Compared to MAM3, MAM4 increases the concentrations of BC and POM in most global regions. The increase is the strongest over the remote regions (e.g., oceans and Arctic) and relatively small over the land source regions [*Liu et al.*, 2016]. We modified the sentence to include more discussion of performance features.

Page 9, Lines 194: Suggest changing "climate" to "atmospheric" or "short-term climate" as the SSTs/sea ice are prescribed.

Reply: Following the reviewer's comment, we changed "climate" to "atmospheric".

Page 10, Lines 227-228: I may be missing something here, but how can the difference between F in two simulations that do not involve any aerosols ("clean" and "clean, clear") tell you something about the aerosol-induced cloud radiative effect (CRE)?

Reply: Typically, the aerosol-induced CRE is estimated by the difference of the shortwave cloud forcings (Δ SWCF, or Δ (F- F_{clear})) between two simulations. With this method, however, the absorbing aerosols above clouds will produce a positive direct forcing and induce a bias in estimated CRE (Ghan, 2013). Ghan (2013) indicates that CRE be calculated under the clean conditions (i.e., no aerosol *direct* effects). The clean conditions are not meant to have no aerosols in the control simulation, but to have no aerosols in the diagnostic radiation call in the same control simulation. So the SWCF in clean conditions (Δ $SWCF_{clean}$, or Δ ($F_{clean} - F_{clean,clear}$)) is used to estimate the CRE.

As we define in the text, "F_{clean} is the radiative flux at TOA calculated from a *diagnostic radiation call* in the same control simulations, but neglecting the scattering and absorption of solar radiation by aerosols."

Page 11, Line 238: Could add "each time" before "neglecting the. . .".
Reply: Done.

Page 11, Lines 239: Suggest adding "more direct" between "This" and "method".
Reply: Done.

Page 12, Line 257: topics -> tropics
Reply: Done. Thanks.

Page 13, Line 284: activities -> activity
Reply: Done.

Page 13, Lines 286-288: If scaling is not applied here, mention it clearly (e.g. ". . .whereas here we do not apply any such scaling").

Reply: Done. We added the following words: ", whereas here we do not apply any such scaling."

Page 13, Line 294: trend -> seasonal cycle
Reply: Done.

Figure 3: Do the selected AERONET sites have data for exactly the same years as the simulation? Not entirely necessary, but needs to be mentioned. Also: Worth mentioning in the caption (also in Fig. 4) that the first row shows sites in southern Africa, the second row sites in South America, and the third row sites in the Arctic.

Reply: We downloaded the AERONET data at the selected sites for exactly the same years as the simulation. However, the selected AERONET sites have missing data for some periods of the model simulation, as shown in Figures 3 and 4. We mentioned this in the revision.
We now added the site information to the figure caption.

Page 14, Lines 303-304: There is also a notable early peak. Worth mentioning and perhaps commenting on.

Reply: Yes, the modeled AOD shows a notable early peak before the fire season, especially for Alta Floresta and Rio Branco, which could be due to the model overestimation of fire emission in this period. We mentioned this in the revision.

Page 14, Line 306: However, there is too strong a seasonality, it seems? Any explanation?

Reply: Yes, the modeled SSA is too low during the fire season and exhibits too strong a seasonality. It implies that the model underestimation of scattering aerosols (e.g., POM) may be more severe than that of BC during the fire season.

Page 15, Line 321: Sulfate and OC, right?

Reply: Yes. We changed to "e.g., sulfate and POM".

Page 15, Line 327: No need for "respectively" here.
Reply: Thanks. We removed "respectively" here.

Figure 5: There are too many significant figures in the global mean values shown on each panel (also in later figures). Also: With respect to what is statistical significance estimated for the right panels? Interannual variability or ensemble member diversity? Needs to be mentioned here and also in later figures. And why is significance not shown for the left hand panels?

Reply: Following the reviewer's comment, we reduced the number of figures in the global mean values shown on each panel (also in later figures) in the revised manuscript. The statistical significance test is applied to the results using the Ghan (2013) method, because DRE from this method is calculated as the radiative flux difference between two model simulations. Therefore, the difference is not only from the DRE of fire aerosols, but also from the model internal variability which includes both the interannual variability (2003-2011) and the ensemble member diversity (10 members). We mentioned this in the revised manuscript.

The statistical significance test is not applied to the BBFFBF method (shown in the left hand panels). The reason is that DRE using this method is calculated as the radiative flux difference between the control run and diagnostic radiation calls in each model time step, which ensures that the climate background (e.g., clouds) is exactly the same between the control run and diagnostic calls.

Page 15, Line 332: Why have you chosen to report only the global mean from the BBFFBF method in the text, and not from the one based on Ghan (2013)?

Reply: Actually here the global mean (0.155 ± 0.01 W m^{-2}) is from the Ghan (2013) method. With the Ghan (2013) method, the radiative effects including DRE, CRE and SAE of fire aerosols can be estimated, while the BBFFBF method only estimates DRE. We added a note in the revised manuscript that the two methods give very similar results for DRE of all fire aerosols, and thus we will report the DRE of all fire aerosols with the Ghan [2013] method.

Page 15, Line 336: "The" is not needed.
Reply: removed.

Page 16, Line 340: "of the tropical regions" -> "of the SH tropical regions"
Reply: Done.

Figure 6: Is the model panel (a) produced with all-sky values? In fact, was that the case for Figure 5 too?
Reply: It is DRE in the all-sky condition. This is also the case for Figure 5.

Figure 7: Which method was used for those maps to be made?
Reply: It is from the method of Ghan (2013). After a comparison with method BBFFBF, the DRE due to all fire aerosols estimated with Ghan (2013) is used in the rest of the paper. We added a note in the revised manuscript.

Page 16, Line 354: Define "high latitudes" here. Is it the same definition as the Arctic?
Reply: We changed the "high latitudes" to "Arctic regions".

Page 16, Lines 359-360: "there are much less noises from" -> "there is much less noise with"
Reply: Done.

Page 17, Lines 371-373: Why would it affect BC? Not clear. Explain better.

Reply: Because fire POM and fire BC are co-emitted and assumed to be internally mixed. The burden of fire POM is about a few times higher than that of fire BC, especially in Arctic. With the removal of fire POM emission and thus fire POM in the NOFIREPOM experiment, fire BC will be impacted due to changed properties (e.g., size and hygroscopicity) of aerosol particles within which fire BC and POM are internally mixed. Our results show that the fire BC burden in the Arctic is reduced in NOFIREPOM with the mechanism worthy a detailed budget analysis. We added an explanation in the revised manuscript.

Page 17, Line 373: it -> one
Reply: Done.

Page 17, Lines 375-376: global regions -> globe
Reply: Done.

Page 17, Lines 378-382: Could the authors provide a reference for this mechanism?

Reply: We added the following reference:
Zhang, Z., Meyer, K., Yu, H., Platnick, S., Colarco, P., Liu, Z., and Oreopoulos, L.: Shortwave direct radiative effects of above-cloud aerosols over global oceans derived from 8 years of CALIOP and MODIS observations, Atmos. Chem. Phys., 16, 2877-2900, 10.5194/acp-16-2877-2016, 2016.

Sect. 3.3: Can's some of the cloud changes that lead to indirect effects be a result of dynamical changes due to fire aerosols?

Reply: Yes, the cloud changes as a result of dynamical changes due to fire aerosols is also considered as a part of aerosol induced cloud radiative effect (CRE) with the

Ghan (2013) method. Since the same sea surface temperatures (SSTs) are used in these simulations, CRE as a result of dynamical changes due to fire aerosols should be small.

Page 19, Line 418: Please provide reference to support this statement ("Larger. . .").

Reply: We added the two following references:

Ghan, S. J., Liu, X., Easter, R. C., Zaveri, R., Rasch, P. J., Yoon, J.-H., and Eaton, B.: Toward a Minimal Representation of Aerosols in Climate Models: Comparative Decomposition of Aerosol Direct, Semidirect, and Indirect Radiative Forcing, Journal of Climate, 25, 6461-6476, doi:10.1175/JCLI-D-11-00650.1, 2012.

Jiang, Y., Yang, X.-Q., and Liu, X.: Seasonality in anthropogenic aerosol effects on East Asian climate simulated with CAM5, Journal of Geophysical Research: Atmospheres, 120, 2015JD023451, 10.1002/2015JD023451, 2015.

Page 19, Lines 420-421: What does "low-level" mean here?

Reply: The low-level clouds mean "vertically-integrated low clouds (from surface to 750 hPa)" as defined in CESM. We revised the sentence in the manuscript to make it clear.

Page 20, Lines 434-435: The higher OC/BC ratio does not seem like a good explanation, as it is mentioned a bit earlier that POM and BC are comparable in the NH and SH.

Reply: we agree with the reviewer, and removed "higher fire OC/BC ratios" in the revised manuscript.

Page 21, Line 449: I suggest adding "slightly" between "agree" and "better".

Reply: Done.

Page 21, Line 452: It reads as if you take values from Ghan (2013). Suggest rephrasing.

Reply: Thanks. We rephrased the words to "estimated with *Ghan* [2013]" in the revised manuscript.

Page 21, Lines 469-470: Even in tropical areas? Please discuss.

Reply: We re-wrote the sentence as:

"The negative SAE over land is a result of the surface albedo change (including snow depth change) caused by fire aerosols."

Page 22, Line 484: Instead of "The shortwave flux change in the atmosphere ", I suggest writing "The shortwave atmospheric absorption change", as it is more conventional.

Reply: Done. Thanks for the suggestions.

Figure 13a: Clarify to the reader why the values in Fig. 8a are somewhat different to those in Fig. 13a.

Reply: Figure 13a shows the net shortwave flux change at TOA due to fire aerosols, which is a sum of fire aerosol DRE, CRE and SAE. The CRE (-0.70$\pm$0.05) is larger than the DRE (0.155$\pm$0.01) and SAE (0.03$\pm$0.10). Thus, the TOA solar flux change is dominant by the CRE and similar to distribution of the CRE (Figure 8a). These values are also listed and compared in Table 2.

Page 23, Lines 505-506: There are also substantial differences with Tosca et al. (2013), especially over tropical oceans, therefore I would add "partly" before "consistent". Also the results over southern Africa are consistent with the recent findings of Hodnebrog et al. (2016), which the authors can mention.

Reply: Thanks for the suggestions. We added the word "partly" and also the sentence that "The precipitation reduction in southern Africa is consistent with the recent findings of *Hodnebrog et al.* [2016]" in the revised manuscript.

Page 23, Line 511: After this line, I suggest that you add a statement clearly stating that these results do not represent the complete impact of fire emitted aerosols on temperature and (especially) precipitation, since the climate system has not been allowed to fully respond (SSTs are fixed).

Reply: Thanks for the suggestion. We added a statement in the revised manuscript: "We note that the temperature and (especially) precipitation changes reported here do not represent the complete impact of fire aerosols, since the SSTs are fixed in our simulations. Fully-coupled atmosphere and ocean model will be used to further investigate the impact of fire aerosols."

Page 24, Line 519: effect -> effective
Reply: Done.

Page 26, Lines 575-579: Again, I suggest reminding the reader that these do not represent the full climate responses, given the atmosphere-only nature of the experiments.
Reply: following the reviewer's comment, we added a statement here in the revised manuscript "These results are based on the simulations with fixed SSTs and may not represent the full climate responses."

Page 27, Lines 596-597: Is the difference in emitted POM between the two studies equivalent (in size) to the difference in the CRE?

Reply: The CRE is strongest over southern Africa, South America and the Arctic. The emission scaling factors used in *Ward et al.* [2012] for these three regions are 3, 2 and 3, respectively. The CRE of their study is about 2.4 times of our study (-1.64 versus -0.70 W m^{-2}). So the difference in CRE between the two studies is approximately equivalent (in size) to the emission difference.