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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of Global Wildfire Aerosols on Direct Radiative, Cloud and Surface-Albedo Forcings Simulated with CAM5" *by* Yiquan Jiang et al.

Anonymous Referee #2

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The manuscript of Jiang et al. investigates the impacts of fire-emitted aerosols on the Earth's radiative balance, through direct, indirect, semi-direct, and surface albedo effects. This is pursued via the analysis of some carefully designed global model experiments. The global aerosol direct radiative effect simulated is in agreement with the few previous studies that have focused on this topic, while the indirect effect is found to be somewhat weaker, though still strong. Additionally, the authors discuss the geographical distribution of the fire aerosol effects. The study also briefly describes some implications for climate response, i.e. temperature and precipitation, though that is only done via atmosphere-only simulations.

The manuscript is certainly within the scope of ACP. The topic is important and rela-

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tively under-explored, with large uncertainty remaining in how fire emissions affect the Earth's radiative balance and climate. It will be a useful addition to the emerging discussion of the role of fire in the Earth system. The language and structure is ok. I do not have any major concerns, but I have a number of (mostly minor) suggestions that I list below which I believe will improve the manuscript. Following those, I expect that it will be ready for publication.

GENERAL COMMENT:

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.

SPECIFIC COMMENTS:

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

Page 2, Line 38: Not sure whey a range is indicated both by two numbers and by the +/-

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

Page 2, Line 48: Suggest stressing that this effect is small and insignificant.

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).

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

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

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).

Page 3, Line 72: Suggest changing “indirect effect” to “indirect effects”.

Page 4, Line 76: Suggest removing “the” before “climate change”.

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”.

Page 4, Lines 78-81: Similarly, RF does not have to always be pre-industrial to present-day. 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.

Page 5, Line 98: many -> some

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

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

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

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

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)?

Page 11, Line 238: Could add “each time” before “neglecting the. . .”.

Page 11, Lines 239: Suggest adding “more direct” between “This” and “method”.

Page 12, Line 257: topics -> tropics

Page 13, Line 284: activities -> activity

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”).

Page 13, Line 294: trend -> seasonal cycle

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.

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

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

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

Page 15, Line 327: No need for “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?

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)?

Page 15, Line 336: “The” is not needed.

Page 16, Line 340: “of the tropical regions” -> “of the SH tropical regions”

Figure 6: Is the model panel (a) produced with all-sky values? In fact, was that the case for Figure 5 too?

Figure 7: Which method was used for those maps to be made?

Page 16, Line 354: Define “high latitudes” here. Is it the same definition as the Arctic?

Page 16, Lines 359-360: “there are much less noises from” -> “there is much less noise with”

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

Page 17, Line 373: it -> one

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

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

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?

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

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

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.

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

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

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

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.

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

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.

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).

Page 24, Line 519: effect -> effective

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.

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

REFERENCES:

Bistinas, I., Harrison, S. P., Prentice, I. C., and Pereira, J. M. C. (2014), Causal relationships versus emergent patterns in the global controls of fire frequency, Biogeosciences, 11, 5087-5101, doi:10.5194/bg-11-5087-2014.

Hodnebrog et al. (2016), Local biomass burning is a dominant cause of the observed precipitation reduction in southern Africa, Nature Communications, doi:10.1038/ncomms11236.

Voulgarakis, A., and R.D. Field, 2015: Fire influences on atmospheric composition, air quality, and climate. Curr. Pollut. Rep., 1, no. 2, 70-81, doi:10.1007/s40726-015-0007-z.

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