-- Referee 1 --

The authors have addressed most of my comments on the original version of the manuscript. Nevertheless, I think that while the paper seems otherwise useful and scientifically sound, it is still somewhat let down by lack of clarity in writing, especially in Section 2. Most of my remaining suggestions (most importantly, comment 2) are concerned with this.

➔ We incorporated all the suggestions.

-- Referee 2 --

The authors have made efforts to improve the manuscript with extra text and shifting away from conclusions about the anthropogenic aerosol forcing; however, there are still some clarity issues. I see that the other reviewer also had trouble reading the paper. The additions unfortunately do not do much to improve this. In many places the language is opaque and additional statements like "overestimate or underestimate" and "higher or lower" do not allow concrete conclusions to be easily drawn. I've tried to cover some of the clarity issues below but this list is by no means exhaustive.

I'm still not fully convinced by the explanation for the large positive radiative effect observed over the Sahara (and Africa in general) after dust and sea-salt have been removed (Fig 6a). These are almost equal in magnitude to the DRE over the regions that are far more affected by biomass burning. It is also at odds with other estimates e.g. Bellouin et al. (2013; Fig. 4) and Heald et al. (2014; Fig 2). I realize that these are at least partially model-based, but the discrepancy in the TOA DRE is very large and needs to be addressed. The strong positive radiative effect at TOA shown in this paper suggests very little scattering impact of organic aerosol in the biomass burning aerosol. Other studies suggest the OA and BC contributions cancel to produce forcings closer to zero (e.g. Myhre et al., 2013) The results of this paper may be correct but they need contrasting with other studies and the reasons for differences explaining. It is worrying because the conclusion of the paper is that the radiative effect of the anthropogenic aerosol is less negative than previously thought. If there is a general overestimation of positive DRE then that might invalidate the conclusion. I don't think this is clearly explained and may not be adequately covered by the sensitivity tests.

➔ Fig. 4 of Bellouin et al. (2013) and Fig. 2 of Heald et al. (2014) show clear-sky aerosol radiative effect (which includes all the natural aerosols). All of our aerosol radiative effect calculations include cloud. (To be clear in this regard, we inserted “All the aerosol radiative effect estimates made by the MACR model in this study include 3D cloud effects” in Table 1 caption.) Furthermore, Fig. 4 of Bellouin et al. (2013) and Fig. 2 of Heald et al. (2014) are about aerosol direct effect while Fig. 6a of our paper shows fine-mode aerosol radiative effect without dust and sea salt. Thus, comparing Bellouin et al. (2013) and Heald et al. (2014) with our study is like comparing apple with orange. Having said this, the reviewer's concern about too positive TOA radiative effect in Fig. 6a of our study is understandable. However, please note that even Fig. 1B (total aerosol direct effect) shows positive forcing over the Saraha, while Fig. 4 of Bellouin et al. (2013) and Fig. 2 of Heald et al. (2014) show somewhat negative to neutral forcing. The differences between our study and their studies are multiple, one of which might be organic aerosol (OA) SSA. OA SSA is normally treated between 0.96 and 1.0 at 550nm but the true value is very uncertain due to brown carbon (e.g., Magi 2009 and 2011 estimated OA SSA to be 0.85). Our observation approach does not need to specify OA SSA while model based studies need to. OA SSA value can make very large impacts on the overall sign of biomass burning aerosol forcing. In view of this, we added and edited the 2nd para of section 1. See "Plus, processing the calculated aerosol distribution by a radiation model requires the specification of parameters such as the single scattering albedo (SSA) of organic aerosol which has been treated as 0.96~1.0 at 550 nm in the modeling community (Myhre et al. 2013b) but might actually be much lower (e.g., 0.85 estimated by Magi 2009; 2011).". Again, we have given the most observational estimate of fine-mode aerosol forcing. Thus, the differences with model based or semi-empirical studies are obviously due to this methodology difference. Exploring the reasons for the result differences require another investigation leading to a separate paper.

The 5th assessment total aerosol radiative effect and forcing estimate is not entirely from models, as is somewhat implied in the final paragraphs of this paper. I'm sure the authors realize it does include partially-observational evaluation (e.g. Bellouin et al., 2013; Su et al., 2013) as well as the AEROCOM study that is used for individual components (Myhre et al. 2013), but this is not clear in the text. In the IPCC report, the authors do point out that both model and observations give similar results (see Chapter 7.5 and Chapter 8.3.4.2 Radiation Forcing of the Aerosol–Radiation Interaction by Component). The range on the direct aerosol effect given is -0.85 to +0.15 W/m2, encompassing the result of this study. Although this study has a narrower uncertainty I still find it hard to accept that this is a legitimate reduction rather than possibly not accounting for certain uncertainties (for example my point about the strong positive fine-mode radiative effect that is surprising and potentially presents a positive bias in the direct aerosol effect).

➔ We are fully aware that the 5th IPCC report estimate includes semi-empirical studies. We read our paper over and over again and cannot possibly see any implication that the 5th IPCC report estimate does not include these semi-empirical studies. For example, we wrote "and these estimates depend heavily on aerosol simulation"; "The large spread among direct aerosol forcing estimates (Myhre et al., 2013a) is attributable largely to these simulation uncertainties". These words ("heavily", "largely") imply that the IPCC estimates came mostly (but not entirely) from models.

Plus, in our final paragraph, we discussed the uncertainty of fine-mode forcing (instead of aerosol radiative forcing). We did not claim that we reduced the uncertainty of aerosol direct radiative forcing estimates.

After reading it several times I feel that while the premise of the paper is interesting and the work has merit, I'm finding it difficult to recommend for publication in the current form because (1) I'm not fully convinced that the range in the uncertainty estimate encompasses the real uncertainty in this estimate, (2) the conclusions drawn about the less negative aerosol DRE are not robust as a result of the uncertainty, and (3) other readers are likely to struggle to understand the details of the paper considering both the reviewers here did. Furthermore, criticisms of the methodology in Lee and Chung (ACP, 2013), that is regularly cited in this work, were made by reviewers in the ACP discussions and do not seem to be fully addressed. I only bring this up because similar criticisms were raised during the initial review of this paper, so it may be worth the authors taking the time to better justify their methods and the uncertainties involved so that readers are convinced this methodology is robust.

➔ As we stated above, the reviewer did not clearly understand our paper. As stated earlier, we have not discussed the uncertainty of aerosol direct forcing estimates in our first revised paper; instead we have discussed the uncertainty of fine-mode forcing estimates. Regarding the methodology details, we improved the clarity during the revision. The issues raised by the reviewers regarding Lee and Chung (2013) were either incorporated or rebutted but the editor did not send the rebuttal comments to the reviewers at the time.

I hope the authors understand that the criticisms here are made in order to improve the manuscript and to ensure that the conclusions are justified so that readers are convinced by the results.

Below are some minor comments.

pg2 ln 52 "simulated aerosol" or "modeled aerosol"

➔ Done as suggested.

pg2 ln52 the uncertainty isn't really "addressed", I would change that to "explored"

➔ The reviewer is mistaken. We did address the uncertainty in used aerosol simulations. Again, we did not address the uncertainty of aerosol direct forcing estimates in our study, and that is not a focus of our revised study either (see the title).

pg2 ln79, multiple references to Lee and Chung (2013), here I think you should reference O'Neill et al. (2003) as that is the source of the AERONET SDA.

➔ We added the reference by O'Neill et al. (2003).

pg3 ln89, the Chin et al. reference is in a strange location, please alter.

➔ This was by design. Chin et al. (2002) give the AOD for each aerosol species but doesn't give the SSA.

pg4 ln 104, I dont think the "but a clear explanation..." is necessary here.

➔ We inserted this phrase in response to the request by the other reviewer.

pg4 ln155-117, I still think that these would benefit to be written in an equation editor, with symbols for the AOD (either "AOD" ot tau) and subscript used for the species types.

➔ This is about the style, and a use of equation editor might create confusion (since regular texts and equation editors use different font styles). However, we made each equation start each sentence to improve the clarity during the revision.

pg8 ln241, "SPRINTARS"

➔ There is no mistake here.

pg9 ln286-289, this is very confusing and does not really add any information unfortunately.

➔ We inserted this para in response to the other reviewer's concern but we understand possible confusion here. To improve the clarity, we revised this para. See "Scaling the simulated dust FMF to match AERONET FMF over dust-dominated sites may still have an overestimation or underestimation of dust FMF outside of dust dominated regions. Plus, dust-dominated regions have non-dust particles, and thus the scaled dust FMF might still underestimate or overestimate dust FMF even over dust dominated regions. This is why we conduct sensitivity runs even after the scaling of the simulated dust FMF."   This para helps to understand the uncertainty of Fig. 6 and so we prefer to retain it.

pg10 ln309, the uncertainty range of -0.85 to +0.15 W/m2 should be quoted here for clarity

➔ Here, we meant to discuss the consensus. We revised this sentence. See "The consensus of global aerosol direct radiative forcing as shown in the 5th IPCC report"

pg10 ln316, "< -0.11 W/m2" means more negative, not closer to zero. I don't think this is what the authors actually mean.

➔ This mistake was also pointed by the other reviewer. It is now corrected.

pg10 Figure 6 - year/period needs adding to the caption

➔ We added the period in the caption.