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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global direct aerosol radiative forcing, as constrained by comprehensive observations" *by* Chul E. Chung et al.

Anonymous Referee #2

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In this paper, "Global direct aerosol radiative forcing, as constrained by comprehensive observations", the authors use a suite of observations from satellite and in-situ platforms to determine the optical properties of aerosol, from which the direct radiative effect can be determined when incorporated with a radiative transfer model and some information from aerosol models. The authors use the fine mode aerosol without sea salt and dust as a proxy for anthropogenic aerosol, and find that the radiative effect is more positive that previous estimates, stating that it is close to zero.

Main points

One key issue throughout the paper is the distinction between direct radiaitve forcing (DRF) and direct radiative effect (DRE). Generally, aerosol radiative forcing is used

when talking about the change in radiative effect relative to pre-industrial levels. Here the analysis is concerned with the direct radiative effect, no comparison to the pre-industrial aerosol radiative effect is made (other than in the final section). For clarity, this needs correcting throughout the paper to only use radiative forcing where the anthropogenic component is being considered, and care also taken to make sure comparisons are not being made with numbers from other literature that is concerned with the aerosol radiative forcing. Furthermore, if the authors intend to consider the anthropogenic aerosol radiative forcing then I think a more detailed consideration of the pre-industrial aerosol composition is necessary. Otherwise the result of this paper is really the fine mode DRE from non-dust and sea salt aerosols, not the anthropogenic DRF.

My second main point relates to the attribution of the fine mode aerosol to the BC, OC and inorganic species...

It seems strange that the fine-mode aerosol radiative effect quite abruptly ends at the west coast of Africa (Figure 3b). If this estimate includes natural aerosol, as stated, then I would expect a contribution from fine dust aerosol over the mid-Atlantic. Does the radiative effect from fine dust really drop away that fast? Figure 5b suggests that there is a significant negative effect in the mid-Atlantic from dust and sea-salt, is this really balanced by a positive impact of other aerosol in this region?

Related to the above, it is not clear from Figure 3 if this is surface or TOA, so perhaps I'm conflating two different metrics here.

More importantly, the authors allude to the strange positive forcing from the 'anthropogenic' fine aerosol over the Sahara being the result of northward transport of biomass burning aerosol and bright desert surfaces. This is a testable hypothesis: there should be a relatively clear seasonal cycle in the impact, with most biomass burning emissions in the boreal winter time in West Africa. Is this the case?

Also, do the regions of highest radiative effect in Africa in Figure 6a align with the brightest regions of the surface reflectance assumed? The regions of high DRE do align with the locations of high AOD in Figure 1A that are generally considered the result of strong dust sources at those locations. There is potential for a similar problem in the Middle East and the Asian deserts too (it looks like there is non-dust fine mode AOD in the Teklemakan desert, which would be strange). This might indicate mis-attribution of dust aerosol as non-dust fine mode aerosol. The authors make efforts to consider the uncertainties involved in the estimate, which is appreciated; however, it is not clear how well the sensitivity studies would account for this likely mis-attribution. In this case, any error would cause a more positive radiative effect from the fine mode aerosol. This is a key point of the paper, so I think it is important to understand any potential bias.

Smaller points

I think the language in the final sentence of the abstract is too strong. Consider replacing "near-zero" with "closer to zero than previous estimates".

In the final paragraph of the first section I think it is important that the authors state when they use model information in this study. Based on that description it seems like models are not used, but this is not really true. An expanded discussion here would allow a better description of how this study builds upon the other studies cited, related to the minimization of reliance on models.

The single sentence after the Section 2 heading either needs expanding or removing.

Do you have some metric of the nudging that is applied to MODIS and MISR that can be stated in the paper? The change in the slope perhaps?

With the 'nudging' of the GOCART model to the AERONET observations, in regions for which there are no AERONET observations is the result essentially just the model? Were any other functional forms than the 1/dˆ4 used? That suggests a very rapid relaxation back to the model values. Do you have any maps or numbers to indicate

how strongly this nudging impacts the SSA and the ASY?

The equations on pg4 are not very clear, consider using an equation editor and symbols instead of simply writing as text.

The high FMF from models at dust sites, relative to observations (Figure 4b), may be an issue with dust emissions favoring too small sizes (Kok et al., 2011). The authors should probably mention this, unless those models have been updated to reflect the latest emission distributions.

At the end of the first paragraph in section 6, the way the numbers are displayed is unnecessarily confusing. I think the final range can be quoted alone.

Section 6: "At least > -0.28Wm-2" please reword, e.g. "more positive than -0.28Wm-2", "unlikely to be less than -0.28Wm-2"

I'm not sure about the final sentence of the paper. There is still some reliance on models, albeit less than previous studies. Please consider revising the closing remarks.

Table 1 - break the first column into two so that the property e.g. fine-mode DRE, can be displayed in the first column and then the specifics of that case put in the second column for ease of reading.

Fig 1b and Fig 3b - state whether surface or TOA Fig 1b says DRE correctly, but then Fig 3b is called the aerosol forcing, please correct this throughout the paper Fig 6 - would it be useful to show the surface fine mode DRE here as well, for completeness?

Bellouin et al. (2008) reference has n/a errors for page numbers

References

Kok, J. F.: A scaling theory for the size distribution of emitted dust aerosols suggests climate models underestimate the size of the global dust cycle, PNAS, 108(3), 1016–1021, doi:10.1073/pnas.1014798108, 2011.

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