Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-533-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Surprising similarities in model and observational aerosol radiative forcing estimates" *by* Edward Gryspeerdt et al.

Anonymous Referee #1

Received and published: 30 July 2019

The impact of aerosols on the climate system can be quantified through the framework of radiative forcing (RF), whereby aerosols exert an energetic imbalance on top-of-atmosphere (TOA) radiation, which the climate system attempts to restore. Aerosol radiative forcing (RF) is highly uncertain in both models and observations, so determining how to compare and weight the relative value of these estimates remains an open question. This work shows observations and models are in better agreement than previously documented, when care is taken to properly decompose aerosol RF into contributions from aerosol-radiation interactions and aerosol-cloud interactions; both of which can be further decomposed into contributions from direct forcing and rapid adjustments.

I found this work to be novel, relevant for the ACPD reader base, and well written.

However, some clarifications in the methodology and results, and improvements to the discussion surrounding the presented results, would help improve the manuscript. If the authors can address the minor comments below, I recommend the work for publication.

General Comment 1: The authors allude to how previous observational estimates of aerosol RF have generally been smaller than model estimates, and that the presented decomposition brings them into closer agreement. To paint this picture more clearly it would be helpful to provide some numbers from the literature of just how much observations and models were in disagreement previously. One can nitpick and say most of the observational estimates are still on the high end of the presented RFaci model range (Fig 1a). Putting their general agreement into clearer context with previous work, however, will help explain to the reader just how much of an improvement this is.

General Comment 2: The authors briefly allude to the notable divide in the magnitude of the adjustments (and their enhancement of RFaci) between CMIP5 models and AeroCom models, but it would be useful to present some comments on why CMIP5 models are so much smaller, beyond just stating that their LWP change is smaller. Presumably this occurs because most of the CMIP5 models did not really include treatment of indirect effects. Outside of the analysis of RFaci, should we ignore these models then? For the analysis of adjustments, that would only leave us with a few truly different AeroCom models (since most of the data points are just variations of the base same model). Where the authors make claims about the level of agreement/disagreement in the adjustments between models and observations, it would be good to clarify these points.

Specific comments:

Page 3, line 13: An opportunity is missed in this section to explain why this new approach is more suitable than others. The authors could highlight that other methods, like PRP, are too expensive for analyzing an ensemble of models, for example.

Page 4, equations. The individual terms deserve more explaining. Is the subscript “cs” different than the “clr” subscript? It appears “cs” is some sort of scaling of clear-sky conditions. In that case, is “cld” plus “cs” supposed to be all-sky conditions? Is “c” different from “cld”?

Page 5, line 10: repeated word typo

Page 5, line 9-10. Recently, Muelmenstadt et al. in ACPD found that using daily or monthly data instead of 3-hourly biases estimates of forcing in the PRP method. Here 3-hourly output is used from AeroCOM but only daily output is used from CMIP5. Does this bias the results? I imagine it cannot fully explain differences between the AeroCom and CMIP5 models, but it may not be a negligible effect. I recommend the authors test this, or at least explain why it may/may not matter with their methodology.

Page 7, Table 2: Are the results presented in this table only from the ECHAM model? If so, that should be specified in the caption.

Page 8, Figure 1a: Why is the black circle for C in a different spot along the y-axis for the left-most section of 1a versus the right-most section of 1a? Different studies? Same question for marker B.

Page 10, around line 15. Models and observations seem to be in much better agreement on the magnitude of the fl adjustment (Fig, 1b) than on the fl enhancement of RFaci (Fig. 1c) where only one ensemble member of one model is within the lower observational bound of $\sim$130%. That would infer the models have a larger RFaci in order to match the magnitude of fl adjustment from the observations. That does not seem to be the case, however (Fig. 1a). It would be helpful to clarify this disconnect. Presumably this means models are getting fl adjustment magnitude right but for the wrong reasons, as the authors allude to on Page 10, lines 33-34.

Page 10, line 31: It is not clear to me from the figure that models have a larger LWP adjustment and weaker fl adjustment. Are the authors specifically referring to the

ECHAM-HAM and CAM5 models? Even for those models alone however, the observations of fl adjustment seem to fall in line with most of the ECHAM ensemble members (Fig 1b).

Supplemental: -The final x-axis tick label in Figure S2a is cut off in my copy

---

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-533, 2019.