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Atmospheric Chemistry and Physics Discussions

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

Edward Gryspeerdt et al.

e.gryspeerdt@imperial.ac.uk

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: 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 ad-

justments.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.

Reply: We thank the reviewer for their comments and have addressed them below in turn. We note that a few changes have been made to the method and results in this work which are not mentioned below:

1. Correction of the bug in the LWP adjustment calculation - The original LWP adjustment calculation included scenes with no cloud as having zero LWP. This biased both the PD-PI change in LWP and the linear regression of LWP against cloud albedo. In addition, the adjusted forcing from cases with thin overlying ice cloud was applied incorrectly to the RFaci only (and not the LWP adjustment). Correcting these issues results in a small change for the RFaci and LWP adjustment in some models, but the main conclusions remain unchanged.

2. Inclusion of the constraint from Toll et al. (2019) - A new constraint on the LWP adjustment has been included in Figs. 1 and 2. This does not affect the conclusions, but provides an important new constraint that can be compared to the models.

3. Inclusion of the RFaci constraint from (Hasekamp et al., subm) - This is a new constraint based on an improved aerosol retrieval. Again, this does not affect the conclusions, but provides an important new constraint that can be compared to the models.

4. Update of the forcing values from Andersen et al. (2017) - The forcing values from Andersen et al. (2017) were incorrectly determined from the data available in the paper. These have now been updated, but do not affect the conclusions drawn.

5. Inclusion of UKESM1 - To provide an example of how the decomposition can change between model versions, UKESM1 (a descendent of HadGEM2-A and HadGEM3-UKCA) has been included.

Line numbers given are in the "track changes" version of the manuscript.

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.*
Reply: Thank you for this suggestion. We have included the ranges from (Boucher et al., 2013) in the introduction section and modified the conclusion to highlight the better agreement in this work.

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.
Reply: We agree that the number of models used in this analysis is limited and that the number of model families involved is smaller still. However, only three of these families (CanESM2, IPSL, MPIESM) exclude liquid cloud adjustments. Even within a single model family, there is significant variation in the cloud adjustments (CAM5) and the RFaci (HadGEM/UKESM). Although this does not ensure the uncertainty is fully spanned, the agreement between the models and observations (where it exists), is encouraging evidence of progress in constraining the ERFaer and components.

Although the liquid cloud adjustments in the CMIP5 models are typically smaller than those from the AeroCom simulations, it is not clear that this can be explained purely by a lack of sophistication of the parametrisations. Of the six model families in the CMIP5 collection, half of them are a level two (liquid cloud adjustments) or higher. The end of this paragraph (P11L31) has been modified to mention this. "The CMIP5 models tend to have less sophisticated aerosol schemes (Tab. 1), which may explain these weaker adjustments. However, as some models with higher levels of sophistication (e.g. UKESM1, MRI-CGCM3) also have weak adjustments, model sophistication is not the only factor influencing the strength of the adjustments." This is clearly an area of interest for future work and CMIP6/AerChemMIP may provide the chance to do this, with a larger set of harmonised models.

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.*
Reply: The suggested improvements have been included at the end of the introduction section (P3L11) and combined with the original paragraph that leads the methods section (P3L24), leading to a more logical separation between the sections.

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"?*
Reply: This has been amended to harmonise the subscripts

Page 5, line 10:: *repeated word typo*
Reply: Amended

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.*
Reply: We have repeated this decomposition for the AeroCom models using daily data. The change in the values from the decomposition is small. The IWP limit has to be adjusted, with only gridboxes with an IWP more than 40 gm^{-2} being considered as ice clouds (Table 2.). This is noted in the discussion of the IWPlim (P8L11). Following on from the previous submission, results from the UKESM1 model have been included. This model has similarities to both the HadGEM3-UKCA and HadGEM2-A models, but it was run in a similar setup to the AeroCom simulations. This shows that the negative ERFaci_ice (Fig. 4b) is not due to the setup of the CMIP5 simulations. This hints that the difference between the AeroCom and the CMIP5 simulations may be a chance occurrence based on the chosen ensemble, rather than a consequence of the slight variations in experimental setup.

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.
Reply: Amended

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.*
Reply: Thanks for spotting this. The labels referred to the wrong studies - this has now been amended.

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 ≈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.*
Reply: Thanks for pointing this out. We have updated the text at P11L12 to highlight this and explain that we believe the agreement on the magnitude of the adjustment is (as suggested) due to the models getting things right for the wrong reasons.

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).*
Reply: This was intended to refer to the adjustments an enhancements of the RFaci, rather than the absolute values (Fig. 1c). In general, the models have a smaller fl enhancement, even though the magnitude is the same, as they tend towards the up-

per end of the RFaci estimates. The majority of LWP adjustment estimates suggest a slight decrease in the LWP (e.g Toll et al, (2019). This sentence has now been amended (P12L16).

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

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