

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

Anonymous Referee #2

Received and published: 13 August 2019

General comment: The manuscript covers a topic of high scientific relevance as a reduced uncertainty in the aerosol forcing is crucial in order to constrain the anthropogenic impact on climate. The method the authors present may well be valid and interesting and if model and observational estimates do have a closer agreement than what have been presented so far, this is very interesting to the scientific community. Although well written, I found that lack of satisfactory explanations made it hard to fully understand their approach. The research quality is good, but modifications to how it is presented is needed before publication.

Specific comments:

- Generally, full explanations of how numbers are achieved, what experiments in what models are used etc is lacking. This needs to be fixed before publication.

- The text would benefit from a bit more focus on reminding the reader what you are doing – linking the results you show to the method and explaining why the results actually show and improved agreement. Not a lot is needed, just a few sentences here and there.
- P1, L6: “different decompositions” is too vague. Suggest writing this more clearly to get the reader on board with what you are doing.
- P3, L8: “. . . a decomposition is introduced. . .” Decomposition of what? The reader is left hanging here.
- P3, L13: “. . .for decomposing changes. . .”. Suggest changing this to “. . .for decomposing forcing changes. . .”?
- P3, L17: “. . .and second from Nd changes (the RFaci).” Are you saying here that RFaci arises only because of changes in cloud droplet number. This sentence is a bit misleading.
- P3, L26-27: Use cloud cover and cloud fraction interchangeably. I suggest you stick to one term to avoid confusion.
- P4, Eqs 3 and 4. I suggest labelling the terms and refer to these in the explanations from L10.
- P4, L11: Approximating RFari in this manner seems to ignore surface albedo change as well as aerosol above cloud? (α_{clr_NoA})
- P4, L27: “linear regression”: A more thorough description is needed here. For what water amounts does this linearity assumption hold? Cloud albedo, like emissivity, reaches saturation, though of course at much higher liquid water paths. And what is this known change in liquid water path from PI to PD? Is this a global value?
- P5, Eq 8: Please specify that this is to get the models to resemble the observations.
- P5, L9: Suggest adding a subheader here, for example “datasets”.

[Printer-friendly version](#)[Discussion paper](#)

- P5, L10: The descriptions of the experiments should be made clearer. Suggest separating the AeroCom and the CMIP5 explanations. As it reads now it is hard to follow. The experiment name abbreviations are explained two sentences further down.
- P5, L16. Please explain the set up for the anthsca simulations more thoroughly. "...whilst using the same preindustrial simulation." This is hard to follow.
- P5, L17: "This demonstrates the impact of changing only the aerosol distribution, rather than also the cloud parametrisations." But you do not change the parametrisations between the runs, do you?
- P5, L20: "Change is liquid" to "change in liquid"
- P5, L24: Are these numbers based on numbers from your decomposition method. If so, please specify this.
- P5, L27: How do you calculate this residual? Against what?
- Caption Table 1: The first sentence needs to be more specific. As it reads now, this is total ERF. Also, I would argue that it is the third, not the second column that "identifies the nature...".
- Table 2: Please specify that RFaci, L and fl refers to three forcing estimates in the decomposition (?). Suggest header above these column and explanation in the caption. In the caption, only RFaci is mentioned. Please add a description of the other two as well.
- Table 2 and text: Please explain how you got the numbers in the table, what model and experiments were used.
- Page 7, L2: "...aerosol-dependent cloud adjustment (CND)..." The notation used here is misleading. I assume the CND refers to an experiment where these adjustments are removed? Please rephrase and specify with what model and how these simulations were carried out.

[Printer-friendly version](#)[Discussion paper](#)

- P7, L1-3: Yes, RFaci is within 10% for the CND runs, but the L and fl are not. Please discuss this in the text.
- P7, L10-12: Does it show that this is a suitable method or that the method is not very sensitive to the choice of threshold?
- Figure 1: Please separate the panels more so that the axis labelling becomes more clear.
- Figure 1, caption: Please add “are shown” or similar to “other estimates. . .”.
- P8, L8: You use Delta alpha_c here but this is the same as Delta In AOD in Figure 1a?
- P9, L1-3: Is this shown somewhere? Fig 1.b?
- P9, L4-6: Is this shown somewhere? Figure in supplementary?
- Figure 2: Is there a residual cloud albedo change? Does the positive value in 2a indicate a negative change in cloud droplet number?
- P9, L13-14: “However, uncertainties from both S1 and S2 are shared with the RFaci estimate.” Please write this more clearly.
- P10, L17: The overall pattern of the forcing from fl changes. . .” Please add “in the models” or equivalent.
- P10, L10: A bit harsh to state that liquid water path decreases with increased N in observations. This is much debated and state and cloud type dependent. This should be reflected in the text here.
- P11, L18: The variability of. . .
- Figure 4: Please separate the panels more so that the axis labelling becomes more clear.
- Figure 4: Mark which models are AeroCom and which are CMIP?

[Printer-friendly version](#)[Discussion paper](#)

- Figure 4: Caption (ΔSW_c) is this the ΔSW_{α} used earlier? If so, chose one notation.

- Figure 4: “longwave changes from cloud properties. . .” is microphysics a more fitting term here? Fraction is also a cloud property?

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

Printer-friendly version

Discussion paper

