

Interactive comment on “Separating radiative forcing by aerosol–cloud interactions and fast cloud adjustments in the ECHAM-HAMMOZ aerosol–climate model using the method of partial radiative perturbations” by Johannes Mülmenstädt et al.

Johannes Mülmenstädt et al.

johannes.muellenstaedt@uni-leipzig.de

Received and published: 10 June 2019

We thank the reviewer for his or her thorough reading of the manuscript and helpful comments. Please find our responses inline below.

Page 2, line 17: In RFar you are using the ARI abbreviation for the first time. You should more clearly spell out what this abbreviation means (even though you mention the phrase “aerosol–radiation” in the line above).

C1

Thank you for pointing out this omission. We have defined the abbreviation in the revised manuscript.

Page 3, Line 1: On a similar note, please formally define f_c as cloud fraction. I don't see it defined anywhere.

Thank you for pointing out this omission, as well. We have defined the variable in the revised manuscript.

Page 3, line 17: “. . . but this decomposition does not correspond to the forcing-and-adjustment decomposition.” More or clearer explanation about why APRP does not fit the forcing-adjustment framework would be helpful. This was a bit vague.

We thank the reviewer for pointing out the vague language. In the revised manuscript, we now explain that APRP decomposes the cloud property changes into changes in area fraction, cloud albedo, and cloud absorption. The change in area fraction maps well onto the cloud fraction adjustment in the forcing–adjustment framework, but the APRP cloud albedo change includes both the effect of the anthropogenic N_d change and the \mathcal{L} adjustment.

Page 6, line 20. A specific example reference of the observational studies you talk about would be helpful here.

We have expanded the discussion in this paragraph, also in light of the reviewer's comment on Fig. 1 below. We now separately discuss changes over ocean and over land and compare the patterns we have derived for each to a number of observational or observationally constrained modeling studies. We have also factorized the geographic distributions of the ERFaci components into an anthropogenic N_d perturbation and a model sensitivity to the perturbation, to facilitate comparison to observational estimates of aerosol susceptibilities.

Table 1: Does RFar account perfectly for the difference between the sum of the ERFaci and the total ERF? Or is there some error associated with the PRP method in that

C2

difference? It would be good to quantify RFari. Perhaps with double-call calculations or the Ghan method.

Very good point; the RFari and ice-cloud ACI are not part of our decomposition, but they are estimated for a very similar model run in Gryspeerdt et al. (submitted; should be in ACPD by the time this manuscript is published). The total ERF also includes the ice-phase ACI effects (-0.59 W m^{-2} in the SW, 0.88 W m^{-2} in the LW), RFari (-0.17 W m^{-2} in the SW), and a negligible surface-albedo contribution (-0.01 W m^{-2}). The sum of the components thus balances at approximately the 0.2 W m^{-2} level, a relative error similar to the 0.1 W m^{-2} estimated uncertainty on the ERFaci components.

Figure 1: Any explanation for the local maximum in forcing/adjustment terms along the eastern boundary currents? Right along the west coast of N. America, S. America and Europe? It seems these are also regions where the backwards and forwards PRP calculations differ notably (Figure A1)

We have expanded the discussion in Sec. 3.1 to better describe and explain these local maxima: these are regions where low clouds are abundant (\mathcal{L} is large) and anthropogenic aerosols mix from the continents into the cleaner maritime air masses.

Regarding the last sentence of the comment, the forcing estimate, by construction, is highest where the differences between forward and backward PRP are greatest.

Figure 2: The caption seems to include an editing note by accident.

Thank you for pointing out this leftover editing detritus. We have removed it from the manuscript.

A3: I'd prefer the appendix discussion and figure about temporal averaging to be included in the main section of the note, especially since it is given a prominent spot in the abstract. Given the recent push for large model comparison projects to include forcing diagnosis (where temporally averaged data is the norm), this result seems important.

We agree; it is a bit strange to have to consult the appendix for one of the main points

C3

of the paper. We have moved Appendix 3 into the results section (Sec. 3).

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1304>, 2019.

C4