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Atmospheric Chemistry and Physics Discussions

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.

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We thank Steve Ghan for his thorough reading of the manuscript and helpful comments. Please find our responses inline below.

Page 1, line 4. Insert "by anthropogenic cloud droplet number change" after "radiative forcing".

Thank you for suggesting this clarification. We have adopted the change in the revised manuscript.

Page 3, lines 15–16. Consider the decomposition expressed by equations 6–8 in Ghan et al. PNAS 2016.

Thank you for pointing out the imprecise wording in this sentence. Our intended meaning was to differentiate between, on the one hand, "exact" methods that use the time-varying, three-dimensional state of the model (i.e., the model's direct knowledge of the anthropogenic perturbations); and, on the other hand, methods that require idealizing the cloud as globally homogeneous or performing statistical analysis such as linear regression on the model output. We have noted this in the revised manuscript. We have also changed "exact" to "direct", since we show later on that our method still carries uncertainties on the order of 0.1 W m^{-2}.

Page 4. I'm concerned about substituting a cloud property from one run into diagnostic radiation calculations from another run, since cloud properties vary in time. What is done when clouds at a particular time are simulated in one run but not in the other. How is the cloud property determined then? Using time mean property will work if cloud forms at least once at that point, but what if it never forms at that point in one simulation but does in the other? This issue is mentioned later: large artifacts that occur due to the decorrelated cloud property fields, and tested in the Appendix, but it does not address the question of how to specify properties of clouds not present in one simulation.

We were concerned, too, and we suspect this problem has dissuaded others from trying PRP earlier. When clouds are absent in one run and present in the other, we let the radiative transfer resolve the conflicting cloud properties in the same way as it does when the cloud microphysics and cloud cover schemes produce conflicting cloud properties, i.e., effective radii can only vary within the limits of the cloud optics lookup table. This is guaranteed to produce incorrect results for the model column in which the

mismatch of cloud properties occurs; in fact, the correct result is probably undefined. However, we would consider this to be the heart (or perhaps the logical extreme) of the decorrelation problem. This is the reason we designed the tests in Sec. A2, where the correct forcing components are known, and found that the forward–backward PRP results agree with the correct values to within 0.1 W m^{-2} accuracy, as you point out in your comment.

We agree that the issue of cloud presence in one run and absence in the other should be discussed in the text, and that the discussion should include a prescription for what to do when this case occurs. We have expanded the revised manuscript accordingly.

Page 6, line 7. Insert "global mean" before "forcing".

Thank you for suggesting this clarification. We have adopted the change in the revised manuscript.

Page 9, lines 29–32. Is PRP the most direct method? Is it more direct than the method described by Ghan et al. PNAS 2016? Why not compare the two methods? The Ghan method is simple to implement.

Thank you for pointing out the imprecise wording in this sentence. "Direct" was meant in the same way that "exact" was meant on p. 3, l. 15–16; we have clarified this in the revised manuscript. The suggestion of an intercomparison of methods is a good one, in particular as several additional decomposition methods are close to publication (Gryspeerdt et al., submitted, and at least one other study, private comm.). In the revised manuscript, we mention that the Ghan et al. (2016) estimate of $F_{\mathcal{L}}/F_{N_d} \approx 5$ is much greater than our result of ≈ 1. However, we feel that tracking down the sources of differences between methods is best left for a dedicated intercomparison study.

Figure 2 Caption has a question.

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

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