

I commend the authors on a much improved manuscript. The significant investment of effort into a sentence-by-sentence rewriting of the manuscript by the entire author list has made the conclusions much easier for the reader to grasp. As a minor comment, since “larger audience” is mentioned multiple times in the authors’ reply, I do want to clarify that the issue was never that the reader had to be an expert, but rather that even the experts were left confused.

My very minor **comments use the differences document attached to the authors’ responses** because this was the only document I had access to at the time due to computer problems while traveling.

p. 4, l. 10: I am still unclear on the definition of F_{ari} (and aci). From the author replies, I am led to believe these are not radiative flux differences, but rather . . . what? The physical processes themselves?

p. 5, l. 25: what does “interpretation” mean?

p. 6, l. 18: I know what you mean, but you might want to rewrite this sentence to make it easier to understand why this is efficient; on the previous line, “three experiment” → “three experiments”

p. 7, l. 13: state explicitly that “ADJ” is the semidirect effect except for EC-EARTH. I am a little confused that the adjustments are positive; I thought that they were typically negative in GCMs. Is this because the Twomey effect is weaker than the direct aerosol effect in MAC-SP (which I think is also unusual)? It might be good to provide a few sentences of context for these MAC-SP results compared to other multi-model ensembles (AeroCom, CMIP5); I think most readers will be convinced that they should expect lower intermodel spread when the Twomey effect is prescribed, but they may share my surprise when effects change sign compared to the conventional wisdom, i.e., all-sky effect stronger than clear-sky, semidirect effect negative).

p. 7, l. 21: remove “possible”

p. 8, l. 3: it might be good to add a little more discussion of these results in light of the prevailing opinion that preindustrial/background aerosol properties constitute a large (and irreducible) uncertainty on the anthropogenic forcing; perhaps this result suggests that this problem is not so severe?

p. 8, l. 7: “to” → “from”

p. 8, l. 12: In light of this statement, it would be interesting to see the clear-sky values added to Tab. 2. If the Twomey and direct effects are relatively close in MAC-SP, then it would not be surprising that cloud parameterization doesn’t affect the forcing.

Sec. 3.5: I like this additional text very much. I believe you should rephrase the sentence starting on p. 12, l. 7 (“A clear saturation. . .”) to reflect more clearly that the largest efficiencies occur at the (arbitrarily chosen) edges of the plumes, one of the reasons we would expect MAC-SP to provide a lower bound on the strength of the Twomey effect. (See my first comment on the previous manuscript version.)

p. 13, l. 19: I thank the authors for humoring my comment on nudging (which I realize in retrospect made no sense in the context of their methods). I think it is good to include this conclusion, and I agree with it except for the part about interfering with adjustments. For example, Ghan et al. (2016), of which some of this study’s authors are also coauthors, derive adjustments based on nudged simulations.

Figure 4 b: update the label to “interannual”