

Review of the revised manuscript “Thermodynamic and dynamic responses of the hydrological cycle to solar dimming” submitted for publication in ACP by Smyth et al.

This review refers to the revised version of the manuscript as attached to the comment AC1 from Jan, 13, 2017.

The authors aim to “improve our understanding” of the effects of SRM on the hydrological cycle. To this end, they do basically two types of analyses: An estimation of effects of thermodynamics and changes in near-surface relative humidity on P-E following an approach by Byrne and O’Gorman (2015), and a simple plotting of stream functions related to the Hadley circulation. I think that there are two interesting results: a) thermodynamics have only a small influence on the P-E response pattern to SRM, and b) the ITCZ shift resulting from SRM seems mainly to respond to the relative cooling of the respective summer hemisphere). In principle, I think these results are sufficiently new and interesting to warrant publications. However, I think that both presentation and analysis have a couple of deficiencies that should be dealt with before the manuscript can be considered for publication. My major issues are:

The motivation for this study in the introduction is given as “to help improve our understanding of this issue” (impact of SRM on the water cycle). I think this is much too vague. There is a large number of papers that has dealt with this issue (with respect to GeoMIP e.g. Schmidt et al., 2012, Tilmes et al., 2013, Kravitz et al. 2013; and many others with and without connection to GeoMIP), and also have stated that the model response in the tropics is less conclusive than in middle and high latitudes. I think the introduction needs to briefly summarize what the state of knowledge and what the open questions concerning “this issue” are and to provide a more specific motivation for this study.

The analysis in 2.1 mainly relies on an “extended scaling” estimate of the P-E change presented by Byrne and O’Gorman (2015). The authors just use two of the four terms of the original equation. They state that they “exclude changes in the horizontal gradient of H_s ” but don’t mention that they also exclude potential changes in the temperature gradient. It’s not sufficient to argue with the “sake of simplicity”, in particular when in the end the residual is interpreted as “driven by atmospheric circulation”. There needs to be a discussion of why the two excluded terms are considered unimportant.

One of the main conclusions (3rd sentence of the “Conclusion”) seems to be that “thermodynamic scaling and relative humidity changes may be important for “smaller scale responses to geoengineering”. A similar statement is made at the end of section 2.2. I’m wondering why the authors do not attempt to substantiate this claim. In fact, if I haven’t misread 2.2, little effort is made there to analyze the spatial pattern of the effect of relative humidity changes that goes beyond what had been said already in 2.1, although 2.2 is introduced to provide this. 2.2 tries to summarize a lot of earlier work, but it is difficult to identify a clear goal of this section and an analysis that justifies the statements

mentioned at the beginning of this paragraph. Later in the conclusions it is said that “we also present evidence that land-sea contrasts in evaporation rates, resulting in land-sea contrasts in relative humidity contribute to small changes in P-E with solar dimming”. This evidence is hard to find in the manuscript. In fact, spatial patterns are in general very little discussed, as are the precipitation and evaporation patterns presented in Fig. 3. With the existing discussion of results, I don’t see the use of this Figure.

The final suggestion are studies of “targeted solar geoengineering”. However, my main impression is that in the perceived focus region of this study, the tropics, results seem to be very model dependent. This doesn’t come as a surprise as the simulated tropical hydrological cycle is strongly influenced by parameterized convection. Earlier studies have discussed uncertainties introduced by convection schemes. I think such studies need to be better referenced here. Instead of suggesting another sensitivity study that may be hampered by the same issues, I’d rather suggest to more concisely discuss potential reasons for the apparent difficulty to estimate tropical responses and suggest potential ways forward if there are any.

Minor issues:

P4I5 and I8: Why are temperature anomalies “minimal” and why mention as a contrast that hydrological effects are not eliminated? Temperature are not eliminated, either.

P4I17: It is said that the “ensemble mean reflects strong reductions ... in the subtropics (Fig. 3)”. I don’t see such reductions in the subtropics.

P4I20 “stronger ... effects that cancel out in the ensemble mean (Fig. 4A)” Stronger than what? Why not show the ensemble mean? I don’t think that effects cancel out.

P5I15: This sentence is confusing. Is that true only for high vertical resolution models? Can the models of this intercomparison be considered of high vertical resolution?

P5L26 abrupt4xco2 is not a GeoMIP but a CMIP simulation.

P8I31 It is stated that the damped seasonal ITCZ migration “would likely mean a reduction of precipitation in areas ...” If this is considered an important result, why not look at it in the models at hand?

P9I2 The second sentence of the conclusions is confusing because it compares two things (“thermodynamic scaling captures the general spatial structure of P-E changes under global warming and “large scale rainfall changes in ... geoengineering”) which seem difficult to compare. If comparing global warming and geoengineering simulations one should do the comparison with respect to the same parameters for both cases.

The caption of Fig. 4 is inaccurate in several places (“ $\delta P - E$ difference” etc.).