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Earth System Dynamics Discussions

Interactive comment on "Can a reduction of solar irradiance counteract CO_2-induced climate change? – Results from four Earth system models" *by* H. Schmidt et al.

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Before I start this review, I would like to mention a few things that may be taken into consideration in evaluating my remarks:

1. I usually make it a rule not to submit to or review for discussion journals primarily because (a) they promote the dissemination of unvetted grey literature and (b) a primary function of a journal is to filter and let me know what literature is worth reading and this function is largely abdicated by discussion journals. I am reviewing this paper because I think it is worthwhile to help improve this paper.

2. One of my employees, post-doc Ben Kravitz, is involved in the GeoMIP project.

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3. I have published papers that I like to see cited.

Overall comment:

Good paper. Should be published after some revision. The authors need to be more consistent in their treatment of 4xCO2 – control and G1 – control. If an important purpose of the paper is to assess the extent to which SRM can offset effects of excess CO2, these results must be shown in the context of the excess CO2 results without SRM.

I am not clear on the overall politics, but this manuscript is widely perceived to be the first publication of the GeoMIP project but, according to my understanding, this is a paper by a subset of the GeoMIP group presenting results from a subset of the GeoMIP models. Some sort of forward reference (perhaps to GeoMIP, in prep) with some sort of statement as to the relation of this paper to the more complete GeoMIP analysis would be useful. It can be appropriate for subsets of model intercomparison projects to publish sub-analyses of their results early, but some rationale and motivation for doing so should be offered.

Specific comments:

1. Title: The title I far too ambitious for what was actually done. The paper does not do a thorough evaluation of whether (and to what extent) solar irradiance reduction can counteract CO2-induced climate change. It merely looks at a single scenario and does not perform a thorough and broad ranging examination such as would be a suggested by the title. A more accurate title would be “solar irradiance reduction to counteract climate change as simulated in four Earth System Models”.

2. Page 34: line 17: This issue of decrease in precipitation should be discussed clearly. I think the two points are: (i) per degree of temperature change, solar irradiance changes precipitation/evaporation more CO2 changes do, thus an SRM scheme can recover global mean temperature or precipitation but not both simulataneously.

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Putting this in terms of drying only oversimplifies this issue too much; (ii) SRM cools everywhere and CO2 heats everywhere, whereas both SRM and CO2 produce different complex patterns of change, thus when SRM simulations are examined at smaller spatial scales temperature changes are reduced nearly everywhere whereas in some places precipitation changes may become larger.

3. Page 34: line 28: Here the paper uses the term “climate model” and elsewhere it uses the term “earth system models”. Are these terms synonymous as used? Usually, when the same thing is intended in scientific writing, the same word should be used. I would suggest that when the term “earth system model” is used, it should be defined so the reason for the use of this term can be understood.

4. Page 27: line 7: Pongratz et al (2012) also used both CAM3.5 and HadCM3L to do doubled CO2 with and without SRM scenarios. (Pongratz, et al., Crop yields in a geoengineered climate, Nature Climate Change 2012).

5. Page 37: line 1: This description of the models is wholly inadequate. The main characteristcs of these models should be described here in words. Earlier in this paper, it was mentioned that Matthews and Caldeira (2007) found differences in land-sea precipitation differences, but this was using a model that was not even able to change monsoonal flows with different land-sea temperature contrasts nor was it able to represent changes in the vertical structure of the atmosphere resulting from increased CO2 concentration, which is known to affect precipitation and evaporation, thus those results need to be interpreted with caution. If all of these are three-dimensional coupled ocean atmosphere models that represent dynamical changes resulting from changes in greenhouse gas and solar radiative forcing that would be useful to know. If all of these have land surface models with plant stomata that respond to changes in CO2, that would be useful to know. I am not asking for great detail, but just a pointer as to the class of model.

6. Page 39, line 5-17: It is nice to tell us the model results. It is more useful to tell us

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why the model got those results. So, a sentence or two here explaining why ETSI is between 0.72 and 0.85 for all models would be useful. Is it the same cloud-feedback mechanism in all models? I think it would be worth a sentence or two explaining this cloud feedback, instead of just referring to Gregory and Webb (2008). Table 2 indicates this put is a little opaque. If the main point is the ratio of the bottom two lines, it would be a good idea to add a line that is this ratio (i.e., 205%, 209%, 191%, 147%). HadGEM2-ES seems to be the outlier. Any idea why? I am wondering whether you should simply refer forward here to the section on cloud cover and say that you will discuss efficacy of solar forcing in greater detail there.

7. Page 41, line 3: Work at PCMDI has shown that median values from multi-model ensembles computed gridpoint by gridpoint are usually are better predictors than are mean values from multi-model ensembles. In the case of a four member ensemble, this would mean making a median result which for each field and grid point taking the mean of the two models that do not have extreme values. I think part of the idea is that the addition of a bad model should not be allowed to significantly affect the conclusions of a model intercomparison project. (I tried to track down a good reference, a start is Gleckler, P. J., K. E. Taylor, and C. Doutriaux (2008), Performance metrics for climate models, J. Geophys. Res., 113, D06104, doi:10.1029/2007JD008972). I do not think the choice of mean model is out of line, but my sense is better practice to focus on median results in climate model intercomparison projects.

8. Page 43: line 20 and following: As above, this precipitation discussion needs to be nuanced recognizing that global mean precipitation reduction is a property of the specific scenarios being analyzed and that another scenario could be devised where the solar irradiance was reduced to recover pre-industrial global mean precipitation, and in this scenario there would be residual global mean warming. So, SRM to offset means either precipitation reduction or residual warming. It does not necessarily mean a precipitation reduction.

9. Page 34, line 19: I think it would be useful to show a figure of P-E. Virtual paper is

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cheap.

10. Page 46, line 13: This title is a little misleading. I suggest reheading this section something like '5 efficacy at counteracting co2-induced climate change', with 5.1 being '5.1 differences between 4xCO2 and preindustrial climate' and 5.2 being '5.2 climate change with 4xCO2 and solar irradiance reduction relative to climate change in 4xCO2 without solar irradiance reduction.' This is perhaps the most important part of the paper. It would be helpful to define some metrics of difference and be a little more quantitative (i.e., reduction in rms differences relative to preindustrial control).

11. Page 50, line 8: Again, insufficiently nuanced. If the set point of the experiment had been to recover pre-industrial global mean precipitation, there would have been residual warming, but the SRM would not have led to differences in globally averaged precip, so to imply that a reduction in global mean precip relative to pre-industrial is a necessary consequence of SRM deployment is not only not certain, it is probably wrong. What is certain is that multiple climate objectives cannot be optimally achieved using a single control.

12. Page 50, line 21: I think a cite to Ban-Weiss and Caldeira (2010) would be appropriate here: Ban-Weiss, G. A., & K. Caldeira. 2010. Geoengineering as an optimization problem. ERL, doi:10.1088/1748-9326/5/3/034009

13. Page 51, line 2: Here I am really starting to feel like an excessive self-citer, but it seems here that it would be appropriate to cite MacMynowski et al (2011): MacMynowski, D. G., Keith, D., Caldeira, K., and Shin, H.-J., "Can we test geoengineering?" Energy and Environmental Science, 2011. This directly addresses Robock's contention.

14. Figure 1 and others: Nearly all of the maps figures should be made into two column figures with 4xCO2 – control in the left column and G1 – control on the right column. Color scales should be the same for the left and right columns. We need to be able to put these changes in context of the magnitude of the climate change that the solar

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irradiance reduction is trying to address. An important question is: what fraction of the change is being addressed by the SRM?

15. Figure 2: This is too many lines for 1 figure. Make this a multi panel figure with one field per panel. The left column should show results for 4xCO2 – control and the right panel should show G1 – control.

16. Figure 3: Again, this needs a panel for 4xCO2 – control on the same color scale.

17. Figure 4. I think you need to show the results for 4xCO2 – control on this figure so that people can get a sense of the residual change relative to the amount of change the approach was designed to offset.

18. Figures 5 and 6: Again, you need the same figure for 4xCO2 – control

19. It is not clear why it was felt it was OK to show different fields in the same figure in Fig 1 but then Figs 5 and 6 each need their own panel. I think it is clearest to make an array of figures where rows are different fields and the left column is 4xCO2-control and the right panel is G1-control.

20. Fig 7: Again, you need to show changes caused by 4xCO2 on the same scale either in this panel or a separate panel in this figure.

21. Fig 8, 9, and 10: Again, you need to show the same results for 4xCO2 – preindustrial.

22. Fig 11: This needs to be shown with Fig 4 on the same scale. Fig 4 can have two panels. The left panel can be Fig 11 with the Fig 4 lines plotted on it, so you can see the residual temperature changes on the same scale. Fig 4 could then have a second panel that is like the current Fig 4.

23. Fig 12. Again, this should be incorporated with Fig 8 and shown on the same scale.

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