Review on:

Extreme temperature and precipitation response to solar dimming and stratospheric aerosol geoengineering

Duoying Ji et al

April 20, 2018

The authors analyze GeoMIP experiments G1 and G4 on extreme values. So far, this has only be done for G1 results (solar dimming) but not for injection of sulfate into the stratosphere. They try to estimate differences in the efficiency of the two models, despite the large differences in the forcing.

The paper is mostly well written. I recommend publication when the following after the authors addressed the following comments and questions.

General:

Some results in Figures 6, 7 and 8 are not clearly described and need further explanations. Also the amount of single figures within these figures can be reduced.

The paper will gain if the differences in the forcing of the two experiments are clearly stated in the beginning. Mean values of temperature and precipitation might also be helpful for the reader.

The hypothesis given in the abstract have to be better based on results and further explanations. You should avoid a propbably in the abstract. This is the place for new results. So yave should base the probably on results and you can give a clear answer. E.g. show that you see the claimed respose of the stratospheric dynamic in the results, like a change in the polar vortex.

The forcings of the two experiment are very different. Therefore I recommend to normalize the results when possible add add this to the figures.

The analyses in (Xia et al., 2017) and Niemeier et al (2013) on the differences of solar and aerosol forcing may provided some answers to these results, especially on the hyrological

cycle.

Specific comments

Page 2

Line 1: A hypothesis like this is not great in the abstract. You may better provide results.

Line 7: Again, better avoid probably in the abstract. You can name the differences here in case they are significant.

Line 24: In case you want to cite cloud brightening here, you should add Alterskjer et al (2013) with G5. But cloud brightning is not relevant for your topic.

Page 3

Line 5: Neither Geoengineering nor the future produce warming or cooling. Please rephrase.

Page 4

Line 9: A number of the mean forcing would be good.

Page 5

Line 21: It is not clear to me if you calculate the PDF for the ensemble mean or for each model and average thereafter.

Line 24/25: Reformulate to include both experiments.

Page 8

1st paragraph: Forcing differences to the control run are much stronger in G1 than in G4. Readers not familiar with this experiments may not realize this right away. You may add a figure to highlight this differences, e.g. time series or give a number of the forcings earlier in the text. OK, you do later. Maybe you reorder your paragraphs.

How different are the PDF results to Curry et al?

Line 14: TOA forcing (SW +LW) is more general than SW at the surface. For sulfate CE the LW part is important. In case TOA forcing adds some information, you may add this to Figure 2.

Line 25: In case you talk about single models you may take into account that the forcing from the sulfate layer may be very different between the models (see (Pitari et al., 2013))

Page 9

Line 2/3: Climate under G1 is pre-industrial while G4 is 2020. This can play a role here.

Figure 2: I wonder about the hatching in c). Only small areas with values close o one are significant. Is this correct and do you have an explanation?

Page 11

Figure 3: You may add some words that Rx5days and G4 is partly positive, increase in precipitation, which indicates nicely the climate variability.

Page 12

Line 2: Do mean values of p and t scale the same in G1 and G4?

Fig 4: Please add normalized values here. You may normalize with mean T and P or with the TOA forcing difference G1-4xCO2 and G4-RCP45.

I am not sure I understand what you did in Fig 4i) You may add an equation. Again, hatching and significance seem not to fit. Are this area not significant? Otherwise your area with significant results is very small and you should discuss the relevance of your results in this case.

Page 13

Line 6 to 16: You give a list of related topics here but I miss the clear relation to your results.

Line 17 to 24: The paper would gain of you can explain this feature better

Line 28: 'Except Eastern China' this is a very small area. I guess this is a regional feature which is usually not very well represented in the models. Otherwise an explanation would be great.

Line 29: Better use solar dimming and stratospheric aerosol here also, so decide for one naming and stick to it.

Page 14

Line 11 to 13: I got lost. Can I see this in any figure?

Line 17/18: As you mention them, you may summarize the more complex interactions.

Line 28: Can you show this in a figure, link to existing figure or literature. In general, the small signal to noise ration is important for G4.

Page 17

Line 1 to 5: The forcing varies stronger in G4 between the models than in G1. In case a model simulates of provides most sulfate in the tropics cooling at the poles would be relatively low. You may show TOA of the single models in an appendix.

Line 9: Better cite Schmidt et al, 2012 or Kravitz et al, 2013 here. They have shown the shift of the ITCZ.

Line 15:Any explanation for the different results over central Asia. This results is also different to Aswathy et al (2015).

Page 18

Fig 6: I am not sure I understood how you normalize the right column. I understood (i) as b/e and (j) as c-f. But than the pattern should be similar.

Increase the contrast of Fig (c) and (f), thus darker blue.

Can you add significance to (i) and (j)? Values around 1 and 0 should be white (also in Fig 7 and 8)

Page 19

Line 8: Which figure? Cannot see this in 6c) or 6f).

Line 18: 'affected more by solar dimming....' Where? It is largely blue. I may not get all positive and negative changes right. You may explain this a bit more.

Line 19: Quit dark red around 60S

Page 21

Line 9 to 15: You are on quite weak ground here. The polar stratospheric vortex may weaken due to less sea ice and more wave propagation, sulfate warming the stratosphere may strengthen the vortex (Ferraro et al, 2014) etc.

Page 23

Line 16: Yes, but this depends also on the meridional distribution of the aerosols which is not given in the paper and may be different between the models.

Line 21: 'at high latitudes winter solar dimming ...' How can this be? There is no sunlight, so no SW reduction. < Line 23/24: 'Stratospheric vortex' I doubt that you see this with significant signals in the models even it seems to be a reasonable explanation. E.g. Driscoll et al. (2012) showed that climate model do not represent stratospheric dynamic repose of volcanic eruptions very well. You may show the zonal wind (DJF) and significance of the single models to approve the hypothesis.

Page 24

Line 1 to 6: Does this result it to Jones et al. (2017)?

References

Some are missing in the list. Please check.

References

Driscoll, S., Bozzo, A., Gray, L. J., Robock, A., and Stenchikov, G.: Coupled Model Intercomparison Project 5 (CMIP5) simulations of climate following volcanic eruptions, Journal of Geophysical Research: Atmospheres, 117, doi:10.1029/2012JD017607, 2012.

Jones, A. C. J. M. H., Dunstone, N., Hawcroft, K. E. M. K., and Jones, K. H. A.: Impacts of hemispheric solar geoengineering on tropical cyclone frequency, Nature Communications, 8, 1383, doi:10.1038/s41467-017-01606-0, 2017.

Pitari, G., Aquila, V., Kravitz, B., Robock, A., Watanabe, S., Luca, N. D., Genova, G. D., Mancini, E., Tilmes, S., and Cionni, I.: Stratospheric ozone response to sulfate geoengineer-

ing: Results from the Geoengineering Model Intercomparison Project (GeoMIP), Journal of Geophysical Research, 119, 2629–2653, doi:10.1002/2013JD020566, 2013.

Xia, L., Nowack, P. J., Tilmes, S., and Robock, A.: Impacts of stratospheric sulfate geoengineering on tropospheric ozone, Atmospheric Chemistry and Physics, 17, 11 913–11 928, doi:10.5194/acp-17-11913-2017, URL https://www.atmos-chem-phys.net/17/11913/2017/, 2017.