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Discussions

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Comment on acp-2021-898

Anonymous Referee #1

Referee comment on "The impact of stratospheric aerosol intervention on the North Atlantic and Quasi-Biennial Oscillations in the Geoengineering Model Intercomparison Project (GeoMIP) G6sulfur experiment" by Andy Jones et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-898-RC1, 2021

The paper investigates the impact of injections of stratospheric aerosols as an attempt of geoengineering. Aerosols are injected to reduce the global mean surface temperature of the high forcing scenario SSP5-8.5 to that of SSP2-4.5. The experiments have been performed in 6 different models with some variations in the details of the aerosol injections. The paper focuses on the changes at the end of the century and in particular on changes in the NAO (and related temperature and precipitation fields) and in the QBO.

Among the conclusions is that the NAO becomes more positive in the geoengineered experiments and that changes in the temperature and precipitation reflect what is expected from a more positive NAO. Another conclusion is that there is a risk for the QBO shutting down in the geoengineered experiments although there is no real consensus across the models on this point.

I find the results interesting and the presentation very clear. I also believe that the analysis is solid. I have only a few comments that the authors should consider before the paper is accepted.

Major comments:

1) The definition of the NAO is unusual. Normally the NAO is defined as the difference of normalized (to unit variance) pressure in the two regions giving a dimension-less index. Here it is defined without the normalization. I see that the same definition is used in Stephenson et al. 2006 and Baker et al. 2018, but I did not find any motivation in these papers. The authors should describe the background for choosing a non-standard definition. They should also describe if the conclusions differ when using the more standard method.

2) In Figs. 5 and 6 the multi-model means are shown. The intermodel consistency --

significance of a non-zero signal? -- is calculated as where at least 4 out of the 6 models agree on the sign. If I understand this correctly, then even for random signs this agreement will happen with a probability of more than 50 %. The authors should estimate the significance with a more strict method.

Minor comments:

l163: In order to assess .. I find this sentence rather convoluted. Why not compare directly to the ssp585 experiments?

l183: Christiansen 2018 (10.1175/JCLI-D-17-0197.1) explains why the model mean is better than individual models and could be cited here. As mentioned above, I don't think the inter-model consistency shows much.

l225: In general the QBO will probably more or less vanish in an average of many experiments as its phase is almost random.

Given the length of the paper it contains many figures. Perhaps a few of them could be discarded (maybe Fig. 10).