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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "North Atlantic Oscillation response in GeoMIP experiments G6solar and G6sulfur: why detailed modelling is needed for understanding regional implications of solar radiation management" *by* Andy Jones et al.

**Anonymous Referee #1**

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General concerns:

This study is based on the analysis of the GEOMIP simulations of two prominent models from those participating in the MIP, UKESM1, and WACCM6. The authors compare G6solar and G6sulfur experiments from GEOMIP. These experiments are devoted to reducing warming in the SSP5-8.5 IPCC scenario to the SSP2-4.5 scenario, decreasing solar constant (G6solar), or injecting SO2 in the lower stratosphere (G6sukfur). The solar or sulfate aerosol forcings are not alined in different experiments and two models, but calibration is based on the global surface air temperature. The title of the



paper is misleading. The NAO response in G6sulfur is the most exciting result, but it is not only about this. The paper is well written and logically organized. Still, the authors fail to put their findings, at least their NAO-response results, in a context of a few decades-long research of NAO/AO sensitivity to solar and volcanic forcings. Suppose the authors search using names of Hans Graf or Kunichiko Kodera. In that case, they will find plenty of publications with a wealth of information that is closely related to what is discussed in the current paper.

The authors mention several times that SAI has a significant advantage of other geoengineering techniques because it has an "imperfect" but useful natural analog - volcanic eruptions. But unfortunately, they never use this. E.g., it was never asked if the equator-pole temperature gradient in the lower stratosphere calculated within UKESM1 and WACCAM-6 is realistic. It is known that. e.g., WACCAM overestimates stratospheric temperature response to volcanic eruptions.

Finally, the authors should more clearly formulate their study's objective and what they want to achieve.

Specific comments:

L72: Graf and Kodera discussed this phenomenon much earlier.

L74: It is an incorrect interpretation of the point stated in (Polvani et al., 2019). They reject a casual link between volcanic forcing and AO's positive phase following the 1991 Pinatubo eruption. Stenchikov et al. (2006) and Driscoll et al. (2012) discussed the signal's low amplitude in the existing models.

L85-90: Please formulate the objectives.

L165: UKESM1 and WACCAM6 have absolutely different climates by the end of the 21st century. It deserves a little more explanation.

L168-172: Is it your objective? Why is it here?

L174-176: This is Pinatubo-size emission annually, a colossal forcing.

L202-205: The equatorial lower stratosphere is overheated by 10K. Was it realistically calculated? Could you put this in the context of model results for the 1991 Pinatubo eruption?

L213-214: This is the incorrect statement. Stenchikov et al. (1998) attributed 1/3 of stratospheric heating to solar radiation absorption by sulfate aerosols near IR and 70% - IR absorption.

L216-217: It is not only a lack of solar radiation but also a low IR flux because of low temperatures.

Section 4.7: The first simulations of volcanic impact on climate were conducted by reducing solar constant (see Soden et al. 2002). So the difference between SO2 injection and changing of the solar constant was known. The reduction of the solar constant, by the way, should cause a negative NAO response. This will add in the G6sulfur-G6solar signal. Another point is that G6sulfur produces an extreme temperature meridional gradient in the lower stratosphere, five times made by the 1991 Pinatubo eruption. This is the reason why we have this stable winter warming response. Weather models calculated this response correctly remains open until it is tested in observations.

L307: Strange to use this argument here, when UKESM1 and WACCAM6 produce absolutely different climates with strongly different AMOC intensity.

L359: It is not only the amount of cooling that matters but a change in circulation

L378: It was not observed but suggested. The question is still open.

L381-384: This is a misinterpretation. Stenchikov et al. (2002) stated that along with the stratospheric mechanism (suggested by Graf and Kodera), the polar cooling due to polar ozone depletion and tropospheric planetary wave response could contribute in the positive NAO response.



There is a vast literature on NAO response to solar forcing (see Kodera's papers). It would be useful to compare the G6solar responses with that results. That would be a test for the models. There is no discrepancy (as for volcanic sulfate aerosols) between solar geoengineering forcing and natural forcing. Because in both cases, solar forcing is stationary.

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