

Interactive  
Comment

## ***Interactive comment on “Stratospheric geoengineering impacts on El Niño/Southern Oscillation” by C. J. Gabriel and A. Robock***

**Anonymous Referee #2**

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This study examines changes in ENSO strength and frequency in future geoengineered and non-geoengineered scenarios using the Earth system models participating in GeomIP. It also provides an evaluation of the reliability of the model ENSO representation by comparing the model results with the observed ENSO 3.4 index between 1966 and 2005.

### **General comments**

This manuscript presents a novel contribution to the literature, but I find the approach slightly superficial. The authors find no significant difference in ENSO frequency or amplitude under geoengineering scenarios, but do not investigate any other ocean related variables that might more easily show significant changes, like surface winds.

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The comparison between models and observations is very useful to establish the credibility of the results. The conclusion from this comparison, however, is that the participating models do not simulate ENSO very well. The authors do state this clearly in their manuscript and are open about this caveat, but I fear that the negative results of this comparison invalidate any conclusion on ENSO that can be drawn from these models. A more extensive discussion on the reasons why the models do not simulate well ENSO might help in reestablishing confidence.

To increase the sample size the authors first compare all GeoMIP Vs all non-GeoMIP runs (p9184 L20), in order to increase their sample size. Given that the latitudinal distribution of the radiative forcing in G3 and G4 will be different from G1 and G2, couldn't these two classes of experiments have very different effects on ENSO? Shouldn't they therefore be kept separately, given the different experiment design? Do the authors assume a-priori that there will be no large difference among experiments?

The authors show that “disagreement between models was far more significant than that between different experiments and scenarios” (p9185 L18). Wouldn't this suggest that a multi-model mean is not the best way to proceed, and the authors should rather analyze ENSO changes among different scenario in each single model?

Do the authors think that prolonging the simulations might lead to significant changes? Is there any trend or change in the ENSO index during the 40 years analyzed?

**Specific comment** p9187 L15: The authors state that one statistically significant result the ENSO frequency in RCP4.5 simulations is diminished with respect to historical simulations. Couldn't this also be because of changes in forcing? RCP4.5 emissions should be decadal, and I do not think that they contain any interannual variability. Do historic runs include interannual variability of emissions, which might have an effect on SSTs in the Pacific?

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