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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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This study aims to investigate the response of ENSO to SRM geoengineering in the GeoMIP ensemble. The study investigates ENSO properties through changes in the ENSO 3.4 sea surface temperature index and calculates the number of El Nino and La Nina events and their strength in a range of models across the GeoMIP experiments G1-4. They report no statistically significant change in ENSO event frequency or strength.

No previous study has investigated the response of ENSO to SRM in depth (although Lunt et al. 2008 made some brief reference to ENSO in their results) and as such this is a novel contribution. However, the methodological approach is very crude and has a number of serious flaws which the authors do not reflect on in their discussion. Given that no statistically significant change in ENSO frequency or strength is found it seems

somewhat of a missed opportunity to dedicate the entire results section to this finding. The paper would be much richer if this negative result were supported by analysis of more-easily-detected changes in the Pacific which would influence ENSO behavior or be indicative of changes in ENSO behavior. The authors also missed an opportunity in the discussion to help clarify what would be needed in future SRM studies to detect changes in ENSO event properties.

Specific Comments

Given the difficulty of detecting changes in ENSO event properties it is understandable that the authors have followed in the footsteps of others and tried to draw on as wide an array of models and experiments as possible (as Cai et al. 2014 do). However, the way in which the authors have done so is problematic and may have led to spurious reports of statistical significance. There are two major problems – the aggregation of results from very different experiments and the uncoordinated nature of the large number of comparisons.

As detecting changes in ENSO properties is statistically challenging, Cai et al. 2014 and other studies, group many different models and experiments together into two groups and test differences between them, one of which is effectively a control case with little or no global warming (Pre-industrial control and 20th century) and the other is the global warming case (1%CO2 pa and BAU scenarios). The authors of this study attempt the same kind of aggregation however their groupings often exhibit very large intra-group differences. Importantly the authors do not justify why they've chosen the groupings they've investigated or discuss the properties of these groupings. The authors make a comparison between all experiments (control, elevated CO2, RCP 4.5 and all GeoMIP experiments) and observations of the historical period; given that the whole purpose of this study is to determine changes in ENSO between these experiments, I do not see what this comparison is meant to demonstrate. In another comparison they split all experiments into two groups, all GeoMIP (geoengineered) and all non-GeoMIP experiments, but these groups are not neatly distinct in the same way as

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the control and warm groups used by Cai et al. 2014. Both groupings have a mix of warmer and cooler climates with some warming and some in steady-state, all of which would be expected to affect ENSO properties. It is unclear what would be demonstrated if there were statistically significant differences between these groupings as there doesn't seem to be a consistent approach for choosing the constituents. Similar issues arise for other groupings that the authors make.

The authors also test so many different groupings of models and experiments that it is unsurprising that they detect statistically significant changes at the 5% level. How many comparisons were made and how many false positive results would be expected if all of the data were drawn from the same sample? No effort is made to test whether such spurious statistical significant results were likely nor is any note made of this basic problem. The authors could consider some form of bootstrapping resampling approach which may help to give an idea of how robust the statistics derived from these groupings are.

Putting these issues to one side, the main finding of this study is that the effect of SRM on ENSO are not detectable in 40 year records of ENSO 3.4 temperatures. However, was this not obvious beforehand? ENSO behavior is notoriously variable and would be expected to show substantial variations in 40-year statistics. The authors should do more to explain and investigate the challenges of detecting a change in ENSO behavior. The authors should also reflect on the methodologies of previous model studies of ENSO changes – what length of simulation and number of ensemble members were used?

The authors could tackle this detection issue directly by analyzing the ENSO 3.4 behavior in the long pre-industrial steady-state simulations for each of the 6 models used. This could then make clear how variable 40-year ENSO statistics are. Alternatively the authors could refer to existing results or theoretical considerations from the literature to develop these expectations. There are studies which investigate variations in ENSO statistics in those long pre-industrial simulations in CMIP5 which should be referred to

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(see Zhang et al. 2014).

That the authors found no statistically significant changes in ENSO properties is fine but it would have greatly enriched the study if there had been some discussion of what requirements would be needed for an experiment to investigate the effects of SRM on ENSO. This would add greatly to the value to this article to the community. What averaging periods and number of ensemble members would be required? Are steady-state simulations to be recommended over transient simulations? What recommendations do the authors have for SRM deployment strength in such experiments? And what do these recommendations suggest about the possibility of detecting changes in ENSO in the next generation of GeoMIP experiments?

No statistically significant results were found for ENSO which is an important result but given this I was surprised that this result was not supported by some statistically significant results on other related measures of change in the Pacific. The authors identify a number of important factors which will influence ENSO behavior which would seem to be more readily detectable: Pacific zonal SST gradient, the strength of trade winds, upwelling strength, thermocline depth, etc. (see Guilyardi et al. 2012 for more). In addition, whilst not necessarily definitive, results on changes in the mean and interannual variability of ENSO 3.4 temperatures would be far easier to analyze and would be of interest.

A final minor point is that the authors draw on the literature of the response of ENSO to transient cooling from volcanic eruptions to motivate their work but do not investigate or discuss the transient response to SRM deployment. The authors hypothesize in essence that if the volcanic forcing were persistent (as in SRM simulations) there would be a permanent shift in ENSO behavior. An alternative that goes uninvestigated is that this change in ENSO behavior is solely a transient phenomenon, one which may also be evident in the initial phases of some of the GeoMIP simulations. However, the authors discard the initial 10 years and so such transient phenomena are removed. I'm unsure an investigation of this transient behavior would be fruitful but G1 and G4 should

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produce a transient cooling that may give rise to the same phenomenon. The authors should note that the observed volcanic response may be a transient phenomenon and discuss the implications for SRM.

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