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Interactive Comment

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

Anonymous Referee #3

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Summary

The manuscript seeks to identify changes in ENSO frequency and amplitude under various historical, projected, and geoengineering scenarios. The paper is well motivated and clearly written. That said, there are major caveats associated with limitations in available experiments that go under-appreciated in the text and greatly limit what can be done. I find the results therefore somewhat unconvincing. I recommend strongly that the manuscript provide a more direct appreciation for the inherent limits of the simulations used and provide better context for what is needed to really address this questions with greater certainty. I recommend the abstract be modified to reflect these limitations. I also find some figures to be gratuitous while some important issues go unaddressed. My concerns are detailed in greater depth below.

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Interactive Discussion

Discussion Paper

Comments:

The experiments used are useful but fail to provide a very tight constraint on the null hypothesis being posed because of the facts that 1) so few ensemble members are available for each model, and 2) ENSO is so poorly and variably simulated across the models (as alluded to in 3.3)- contributing to large error bars and hence coarse detectability of any potential change. There is considerable uncertainty associated with the changing mix models across the metrics being computed that is not adequately dealt with. It would seem to be essential to me to make this weighting constant across any comparisons being made.

I recommend that manuscript start with the most simple question: in a single scenario and for a single model, do any provide enough ensemble members to detect a change in ENSO? I presume the answer is ‘no’, since it is not dealt with - but pointing this out would be useful for motivating the need to create multi-model ensemble metrics. In cases where significant differences are identified - what is the role of changes in the model mix? These should be recomputed with such effects removed to see what role they play.

page 9184: line 15: Please put +- 1-sigma values on the model-mean numbers. I think this provides essential context. I think it would also be useful to get an idea upfront for what the internal variability in such a number would be based on a long control run. These numbers are presented implicitly as validation of the simulations but in reality the agreement is just happenstance (as one can likely find greater disagreement between 40-yr periods in the obs alone).

page 9184: line 25: please add 1-sigma bounds as above.

page 9185: line 10: detection of only two significant results in the context of the large # that have been done is at the limits of what may be expected by chance. An associated caveat should be added here.

page 9187 line 15: Why would you put more significance on the RCP4.5 finding that ones that have assessed the question in a broader array of models?

Doesn't the fact that SOI is not a useful ENSO proxy speak to the inherent deficiency of using a given model for this type of analysis? How can one expect to get a reasonable bearing on the dynamical-thermostat mechanism or other dynamical links of forcing to ENSO if the SOI relationship is so poor since essentially the dynamic component of ENSO (SO) also so poor? Shouldn't this be an additional constraint on which models to use?

How can one establish confident ENSO statistics from such a short duration/limited ensemble of runs? Model runs suggest that robust statistics of ENSO (particularly at its low frequency tails) require records of over a century. What has been done here (to group all of them together) might be justified if they all had the same ENSO statistics but clearly they do not.

The fact that some models have unrealistic ENSO behavior is hardly a new result and I don't think it requires 2 figures. A mere sentence in the text would suffice. Moreover, internal variability of ENSO could lead to periods of such low variability even with a reasonable ENSO and thus I'd base any such statement on multiple ensemble members or an extended control.

Maximum event magnitude (e.g. Fig 9) doesn't seem like a very robust metric to use given the limited length of these runs. Why not use total variance?

On the discussion: âĂć We already knew changes in ENSO were inconsistent across models (e.g. Guilyardi et al 2012). This is not new. âĂć It is likely that additional model runs should have been rejected based on the importance of dynamics in the science questions being posed and the lack of SOI fidelity. It seems odd that the authors used this as a basis for rejecting the SOI rather than the models! Perhaps a dynamical validation combined with a power spectrum validation would be a more appropriate way to screen models. âĂć The question of whether the 1966-2005 period is really

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adequate to validate modeled ENSO is never addressed but needs to be considered. ENSO statistics varied considerably through the course of the 20th C.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9173, 2015.