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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropical Pacific Climate Variability under Solar Geoengineering: Impacts on ENSO Extremes" *by* Abdul Malik et al.

Anonymous Referee #1

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This study investigates what would happen to ENSO variability if geoengineering is applied. To do this, it employs millennial integration of a coupled climate model under pre-industrial, 4xCO2, and geoengineering (G1) scenarios. The very long integration is key to detecting statistically significant changes in ENSO, which weren't found by previous studies using short data of about 50 years. This study extends the analysis from previous G1-based studies to look into extreme ENSO which has been projected to increase in frequency under business as usual emission scenarios by Cai et al. The topic is an important one given the global impact of ENSO, and the paper is informative. However it would benefit from a careful revision as there are various instances that are not clear. Comments are provided below for the authors' considerations.

Main criticism is that as it stands this paper sounds more like throwing out results,

from a single model, without giving it further perspectives on the mechanisms. For instance, it is not clear exactly why the modeled ENSO changed from 4xCO2 to G1 in this model? Is it because of the air-sea heat fluxes act more less as a damping in the eastern equatorial Pacific associated with the mean state change in G1? More interestingly, why G1 does not recover many of the climatic states of piControl? Initial thought would be the ocean state never fully recovers. But as stated in the paper the change in thermocline depth is not statistically different between G1 and piControl. I don't think I came across a plot of subsurface temperature, e.g., depth-longitude differences between 4xCO2 and G1 vs piControl. Perhaps while the thermocline depth statistics do not change, there are still changes in the subsurface ocean temperatures in certain areas.

Nonetheless this leads to the question: How large are the differences in mean state and ENSO statistics between G1 and piControl state in comparison to the internal variability in piControl? For example P9, L20-21: the reduction in MSSTG is 9% in G1, is this this substantial compared internal variability in piControl and to that seen during an El Nino?

In many of the plots showing differences between experiments and piControl, the confidence level was set to 90%. Given the long time series of the model output, it should be increased to 95% or even 99%. This would perhaps show more regions in G1 where the differences are not significantly different from piControl.

The conclusion section could provide the reader with a little perspective on whether it is worth it to do the geoengineering solution in the context of projected increase in extreme ENSO activity. A relevant paper to help the discussion: Trenberth KE, Dai A (2007). Geophys Res Lett 34:L15702. doi: 10.1029/2007GL030524

Other specific major comments:

P11, L36: Picking a result on one model sounds rather odd as we know that the change in ENSO amplitude varies widely across models (e.g., Collins et al. 2010). In a recent study by Cai et al. (2018, Nature, https://www.nature.com/articles/s41586-018-0776-9), however, there seems to be a stronger inter-model agreement on the increase in ENSO amplitude in models that are able to simulate ENSO flavors (see their Extended Data Fig. 8b), as implied in the PC1-PC2 space. So does the HadCM3L model capture the nonlinear relationship between PC1 and PC2 as observed? Here PC1 and PC2 refer to the first and second eigenmodes of tropical Pacific SST (see their Fig. 1). Also, it is relevant to discuss the results of Cai et al. (2018) in 1st paragraph of Page 3.

P7, L10: make clear the results are in *qualitative* agreement with previous studies. Not all of the cited studies are based on 4xCO2.

P.7, L13: some studies argue against the use of "El Nino-like" term in describing the mean-state change under greenhouse forcing (e.g., Collins et al. 2010; see also Xie et al. 2010 https://journals.ametsoc.org/doi/10.1175/2009JCLI3329.1). Cautionary is needed to avoid confusions. A relevant reference on the mean state change: diNezio et al https://journals.ametsoc.org/doi/full/10.1175/2009JCLI2982.1

Fig. 2d, e: title of the figure states +0.21 mm/day, -0.23 mm/day. Please explain in the caption that those numbers correspond to the area average difference between experiment and piControl in the tropical Pacific (state domain).

P9, L22-24: This sentence needs a rework. Avoid the word "observe" on model analysis (models are not observations). I think Wang et al. (2017) was referring to zonal temperature gradient between the maritime continent and central Pacific, not eastern Pacific. The difference is not significant in RCP2.6, but should be significant in RCP8.5 (Cai et al. 2015, Nature Climate Change on extreme La Nina).

Fig. 7: Please indicate clearly in the caption that the timeseries have been detrended with non ENSO related trend removed following Cai et al. (2017). Otherwise it would create confusion as other studies show that the 2015/16 Nino3 rainfall is close to the 5 mm/day threshold and is thus classified as an extreme El Nino (Santoso et al. 2017). In panel c, d, it must be rainfall anomalies that are shown because there are negative

rainfall values, so wouldn't the 4 or 3 mm/day threshold be applied here? Panel a and b also have negative rainfall values. Please double check.

P12, L28-31: under 4xCO2 the rainfall skewness is dramatically reduced. Does that mean there are less extreme El Nino based on the rainfall definition? If so, this does not seem consistent with the PPE results of Cai et al. (2014) using the same model.

P13, L28-39: The characterization of extreme La Nina is based on Nino4 (Cai et al. 2015), so it is not clear how Nino3 and Nino3.4 indices are used here to infer changes in extreme La Nina.

Figure presentation

Fig. 1e, some areas look white (e.g., eastern equatorial Pacific which is supposed to be approx. -0.2C p7, L9) while the colorbar does not have white on it.

Figure 10: the color limit does not seem correct, which shows much larger values in e, f G1-piControl than the composite anomalies themselves in panels a-d.

The colorbar of Fig. 2, right panel especially is not ideal. It is hard to immediately see which are positive or negative without referring to the colorbar.

Might be best to have the same color scale for comparing the results of 4xCO2 – piControl vs G1 – piControl. This is to convey the message the difference is much smaller for G1 – piControl than for 4xCO2.

Minor points

Page 4, L34: that sentence is due to Cai et al. (2014).

P4, L35: delete "the northern part of" – the ITCZ is located north of equator, and that rainfall band moves equatorward during strong El Nino events.

P5, L23: "ggradients"

P6, L2: extreme El Ninos are not resulting in just "anomalous rainfall" but unusually large rainfall in the eastern equatorial Pacific.

P6, L 35: “depicts this SSTasymmetry between the western and eastern equatorial Pacific well (Fig. 1a).” – not clear since the observed counterpart is not presented.

P8, L10: “problem” – not clear, in what way it is a problem?

P9, L19: repetitive: El Nino being stronger than La Nina already implies asymmetric amplitude.

P9, L29: the shoaling of thermocline is also due to increased stratification associated with surface intensified warming in response to greenhouse forcing.

P9, L32-36: why not use the maximum of vertical temperature gradient as a proxy of thermocline depth for all scenarios?

P14, L6-7: for extreme El Nino events, are the PWC, SST, and rainfall anomalies strengthened as well?

P14, L23-25: this must be referring to the difference between G1 and piControl. Please make that clear.

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