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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 #2

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Review Comments: This manuscript tries to investigate the impacts of solar geoengineering strategies on our climate, especially the interannual variability in the tropical Pacific, referring to ENSO phenomenon. In detail, the mean state of tropical Pacific, ENSO intensity and ENSO frequency are analyzed by comparing the results from piControl, 4×CO2 and G1 simulations. This work emphasizes that the results from 1000 years long simulation which is much longer than previous used, can be more significant to detect the ENSO changes. Highly based on the definition in Cai et al. (2014), the extreme ENSO increases in G1 compared to piControl. Obviously, the analyses are very detailed in describing the results, including the changes of sea surface temperature, precipitation, zonal wind stress, ZSSTG, MSSTG, thermocline, PWC, ENSO amplitude, ENSO frequency, extreme ENSO. However, major revision is needed considering

unclear physical mechanisms involved in this paper.

Major comments: 1. To study the ENSO changes under solar geoengineering, the results are all based on one single model HadCM3L. In Cai et al. (2014) and Collins et al. (2001), the model they used is HadCM3. I admit that HadCM3L and HadCM3 are identical in most aspects, but there are still differences between these two simulations. The differences should be mentioned in this study because the HadCM3L may not be skillful in reproducing ENSO variabilities, and thus the sentence in P4 L32-33 may not be completely correct. I suggest that the ENSO simulated in HadCM3L should be addressed first, regarding its magnitude and pattern. For instance, the EOF analyses can be carried out on the piControl simulations. It will help us to have a general idea of how capable the HadCM3L is in simulating the ENSO and its diversity, and what's the biases compared with observations. As pointed out in Cai et al. (2018), the magnitude and location of ENSO events are inconsistent among models. The averaged SSTA in a fixed box to measure the intensity of ENSO can be tricky. A look at the ENSO pattern in HadCM3L can also facilitate a better ENSO extreme definition, i.e. the Nino indices may not be best to define ENSO intensity. At least, a glimpse of the Figure 8 reveals that ENSO simulation is not good enough, especially the shape, maximum location and horseshoe-shaped cold SSTA in the western Pacific during El Nino events. 2. The change of extreme ENSO under solar geoengineering is a major concern in this study. This paper shows adequate results to uncovering the phenomenon that may happen but lacks the investigations on underlying mechanisms. The magnitude of ENSO is mainly driven by the positive and negative feedbacks involving air-sea interactions. In the manuscript, the major atmospheric and oceanic components are depicted, such as the thermocline, zonal wind stress and zonal SST gradient. A clear physical process is needed to understand how ENSO can be modified in G1 and 4×CO2. The Bjerknes feedback, thermocline feedback and heat flux feedback can be evaluated under different scenarios. This may be helpful to illustrate why ENSO in G1 can be modified even though the thermocline, zonal SST gradient and zonal wind stress are not well separated in G1 and piControl. Also, it's necessary to go deeper into the reason why the responses of El Nino and La Nina are different for magnitude change and same for frequency change. 3. This manuscript pays a lot of efforts on how mean state of tropical Pacific might be modified under 4×CO2 and G1. A connection between mean state change and ENSO change is simply built by using the previously proposed conclusions, i.e. the reduction of MSSTG in both 4×CO2 and G1 indicate increase of extreme El Nino. However, more detailed explanations should be reviewed before applying this theory.

Minor comments: 1. In P11, L24, the calculation of skewness of SST should be clarified in the context. 2. In P9, L22-24 and P11, L36-39, the independent paragraphs seem abrupt for the context. Better to immerse in the other paragraphs. 3. In P12, L6-10, please clarify why quadratic trend to the time series of rainfall data should be excluded. 4. In P13, L13-15, the central Pacific El Nino is not mentioned in the introduction. Also, the question backs to the major comment 1. The HadCM3L may not be able to capture ENSO diversity. 5. In Figure 4c, why the thermocline depth is not significantly changed over the eastern Pacific. If this is the case, is it due to the choice of 24 isotherms? 6. The significance level is 90% for differences between G1, 4×CO2 and piControl. How about 95% or even 99%? Will the significant regions be much less? 7. In P23, the height of color bars for figures can be smaller to enlarge the main part of figures. In Figure 2 d & e, symmetric colors are better to represent the negative and positive shadings. 8. In Figure 6 d & e, it's better to set the color bar range with the same ratio as in Figure 5 d & e.

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