Comment on egusphere-2022-974
Anonymous Referee #2

Referee comment on "Changes in global teleconnection patterns under global warming and stratospheric aerosol intervention scenarios" by Abolfazl Rezaei et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-974-RC2, 2022

This study assesses changes in large-scale modes of climatic variability, such as El Nino/Southern Oscillation (ENSO) under scenarios of feedback controlled stratospheric aerosol injection (SAI), as modelled in two versions of the Community Earth System Model (CESM). The study addresses an important research question, about which there has been little research and which I believe is of interest to the community. However, I would not recommend publication of the study in its current form, as there are several important revisions required to demonstrate that the results of this paper are robust, and to bring clarity to the writing and figures.

Firstly, changes in values of climate metrics are consistently interpreted as representing a forced response to SAI and/or warming without reference to significance testing, and without placing the magnitude of changes in the context of internal variability. For example, Figure 1(i) is described as showing that “SAI in CESM2 effectively restores the projected changes [in the PDO]” (line 223-224), but it is not clear from the figure that this is the case. There is a slight increase in the median, towards it's historical value, under SAI, but the distribution as a whole as represented by the box and whiskers appears to see a decrease. No statistics are given from which to judge the significance of the change. Similarly, Figure 6 is described in the text (around lines 294-300), and implicitly by the red arrows marked on the figure, as showing changes in metrics of El Nino and La Nina, based on changes in medians between scenarios, some of which are very small and many of which sit well within the shown interquartile ranges. No quantification of the statistical distinguishability of the distributions behind the box plots is given.

This study mentions that in the only previous assessment of ENSO under SAI, by Gabriel and Robock (2015), SAI simulations may not have been long enough to detect changes. I assume that the large 20-member ensemble of GLENS may overcome this limitation, especially for short-period indices, since this represents ~1600 model-years. The authors should consider adding a discussion of whether this is the case, and for each index, the size of change which their analysis would allow them to detect. Perhaps the pre-industrial run for each model could be added to the analysis and used to characterize the tendency of each of these indices to vary on the timescales considered, to contextualise the scale of
changes shown in figures 1 and 6. Some discussion of multiple testing is also necessary, since multiple indices, with multiple features of each are assessed. In the absence of any theoretical argument for why we might expect particular changes in these indices under SAI, there is a danger of cherry picking the largest changes amongst many noisy time series.

More clarity would be useful over how the authors intend to treat agreement and disagreement between the two models and their SAI simulations. It is suggested that (line 120) the two members of the CESM family are different enough to explore ‘a range of plausibly real climate impacts’. There is, however, no discussion of how these two members of the CESM family compare to the inter-model spread in representation of these climate indices in, e.g., the CMIP6 ensemble, and to observations. More models could be added using GeoMIP simulations, should the authors wish to do so (albeit for a different SAI scenario).

The findings of suppressed long-period variability in the AMO under SAI relative to both historical and warming, and the un-restored long-period variability in the PDO under SAI, in CESM2 are perhaps the starkest changes seen, and worth more discussion. However, they are found only a 3-member ensemble for one model, and as such, the authors should consider more strongly caveating their statements, particularly in the abstract (line 33).

Finally, the figures, particularly figure 1, are complex and difficult to interpret. The authors should consider which elements are needed to make their argument and which might be consigned to supplementary material.

**Minor comments**

- The authors could consider adding to Figs 2,3,4,5 a row showing differences under SAI, and an indication of significance of the magnitude this difference. Without such a row it is difficult to interpret the impact of SAI in these figures. The authors might also consider moving all these spatial figures to supplementary.
- Figure 8 shows a 100-fold increase in NAO power in the high frequency end of the spectrum between the historical and the SSP5-8.5/SAI scenarios. This result is not discussed in the text but is very surprising. The authors should explain what is happening here, and address whether the finding casts doubt on the ability of CESM2 to capture NAO variability.
- The authors might consider removing Figures 7 and 8a-d, since they are somewhat misleading in suggesting that the historical run differs from the other runs in the high period end of the spectrum when in fact it is simply too short to represent this part of the frequency space.
- For the supplementary figures S1-S4, the authors should consider grouping these plots by index rather than by simulation, and showing all simulations vertically for each index so that a comparison can be made. I would also suggest adding at least one of these index timeseries figures to the main body of the paper.
- Line 18 (and throughout): The authors might consider their use of the phrase “climate
teleconnection patterns” to describe features such as the Atlantic Multidecadal Oscillation