Dear reviewer,

We would like to express our sincere appreciation for your comprehensive reviews and comments that greatly help us to improve our manuscript. This manuscript has been thoroughly revised based on your comments. The revisions we make based on each comment is explained point-by-point.

1) **It seems to me that discussion on physical reasons for changes in upper tropospheric thickness, atmospheric circulation, and AR activity are not sufficient. Previous studies identified that forcing factors and ocean warming result in different patterns of changes in thickness and atmospheric circulation over East Asia (e.g. doi:10.1007/s00382-014-2073-0, doi:10.1007/s00382-014-2146-0, doi:10.1038/ngeo2449, doi:10.2151/sola.2018-010). Such discussion, especially in terms of land-sea warming contrast, should be added to help better understanding on physical mechanisms of different changes in upper troposphere in response to different forcing factors.**

Authors:

We thank the reviewer for the criticism on the discussion of the environmental factors associated with changes in ARs. In response to this, additional figures (Figure 3 and Figure S5) and more discussion on the changes in the land-sea thermal contrast in terms of the absolute changes in surface temperature have been added to the main text and the supplementary information. We note that the changes in surface temperature provide additional evidence of the increased land-sea thermal contrast in the projected warmer climate and the associated increases in AR activity over East Asia. As discussed in lines 216-219, some patterns of changes in the land-sea thermal contrast, tropospheric thermal expansion and EAJS exhibit similarities to the studies of Kamae et al. (2014) and Endo et al. (2018) and we thank the reviewer for recommending these helpful references. We have also discussed that the mid-latitude ocean warming under the simulated SAI may lead to reversed "cold ocean/warm land" (COWL) pattern that enhances atmospheric blocking (Mullen 1989), which partly explain the increase in the mid-latitude AR activity (Pohl et al., 2021; discussed in lines 199-201).

Unfortunately, the GEOMIP protocol does not provide AMIP-style simulations for presenting the contrasting patterns of environmental responses under the radiative forcing of CO_2 increases compared to those under only the warming of the SSTs (Kamae et al., 2014; Shaw and Voigt, 2015). We acknowledge that such an important mechanism should be supplemented in future works on geoengineering simulations (lines 356-360).

2) **I also recommend to include global maps of thickness, geopotential, and wind patterns in response to SSP, sulfur, and solar forcing. Such global figures may help more reasonable understanding on changes in atmospheric river activity over East Asia.**

Authors:

We agree with the reviewer that a bigger picture of the AR-related large-scale environments is needed. In response to this, we have enlarged some of the maps to include not only East Asia but also most of the North Pacific, Bay of Bengal and the East Indian Ocean. However, presenting the global distributions of fields may include too many irrelevant systems (e.g. the jet stream across the Roaring Forties and Furious Fifties of the southern hemisphere) – as the title says, we are focusing on East Asia, not on the globe as a whole. We also do not include too much information on the monsoonal flows over South Asia and the upstream Somalia region as ARs and the associated environments over these regions are out of the scope of this study.

3) **Line 211: I don't think the results of these simulations support the SAH expansion. Increasing geopotential height are found outside of SAH region. The increase in geopotential height at 40-60N doesn't indicate SAH expansion but enhanced land-sea contrast over East Asia.**

Authors:
In response to the comment, this statement has been removed so that the discussion on the land-sea contrast is better focused in this paragraph.

4) **I also think more discussion on future changes in summertime storm track is needed for better understanding on changes in AR activity. Previous studies pointed out shift and weakening of storm track over the summertime North Pacific (doi:10.1029/2020JD032701, doi:10.1007/s00704-008-0083-8). Such changes in storm track and associated changes in jet stream should be investigated in more detail, because such mid-latitude disturbances should be essentially important for AR activity.**

Authors:
Following the comment, in lines 235-244 and Figures 6 and S6 we have made additional analyses of the summer storm tracks using the root-mean-square field of MSLP bandpass-filtered by the Laczos resampling method according to one the suggested reference (Harvey et al., 2020). Please note that, although ARs have been frequently observed to be related to extratropical storms, the storm track does not provide good explanation for the changes in the semi-stationary AR flows over the upstream region where the societal impact of ARs is apparent (lines 287-290).

5) **In figures 9 and 10, the authors investigated changes in heavy rainfall associated with ARs. The results are very interesting. However, when you discuss fractional contribution of ARs to heavy rainfall, you need to discuss effect of tropical cyclones. As indicated in many previous studies (e.g. doi:10.2151/sola.2017-002), future changes in tropical cyclone frequency/intensity are primarily important for future changes in heavy rainfall over the western North Pacific. Therefore, discussion on AR frequency itself is not sufficient for discussion on fractional contribution to heavy rainfall.**

I recommend the authors to add discussion on future changes in TC-related heavy rainfall over this region in these simulations.

Authors:
We acknowledge that tropical cyclones (TCs) can bring destructive winds and extreme precipitation to the coastal communities and their comparison with ARs is useful to understand the roles of different high-impact synoptic systems in regional climate. However, we decided not to include analyses of TC precipitation as this study is based on a GCM that has a relatively coarse resolution in which TCs are not well resolved. Diagnoses of TCs in such models usually rely on empirical indices that quantify the genesis density of TCs by considering the large-scale thermodynamical factors (e.g. Webster et al., 2005; Jones et al., 2017; Emanuel 2021) but such a method is unable to examine TC-related precipitation.

Thank you for your consideration of publication.

Sincerely yours,
Ju Liang (first author, corresponding author)
and
Jim Haywood

Reference:

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