Response to Reviewer 2

Thank you for the valuable comments which significantly improved the manuscript.

Our responses to your comments are presented in bold. The line numbers referenced in our response correspond to those in the revised manuscript.

Although not requested by the reviewers, we replaced JRA55 with JRA3Q (Kosaka et al. 2024), the most recent reanalysis dataset from the Japanese Meteorological Agency, to keep our analyses up to date. In general, JRA3Q trends are close to ERA5 and do not impact our main conclusions.

The manuscript analyzes the trends in storm track intensities in Eurasian and south and east Asia in summertime in the satellite era. The CMIP6 Detection and Attribution Model Intercomparison Project (DAMIP) simulations were employed to attribute the trends to different anthropogenic forcings. The authors find that resemble the reanalysis's trends in weakened summertime circulation and show that individual forcing has linear response by the circulation. The authors also find that decreasing aerosol emissions in Europe would increase incoming solar radiation and thus moist static energy (MSE). Increased aerosols from the south and east Asia decreases shortwave radiation and thus MSE. Stationary circulation acts to transport MSE downstream from land with transient eddies compensating for MSE flux trends. An MSE-budget-based idealized model is developed to predict how the magnitudes of aerosol forcing alter the intensity of storm tracks over both oceans. The study suggests that aerosol forcing and greenhouse gas forcing play equal role in storm track weakening, and the former plays larger role over the Pacific Ocean due to larger emission variations and latter plays larger role over the Atlantic Ocean. Comparing to the Atlantic Ocean, the storm track weakening is larger over the Pacific Ocean due to stronger trend in shortwave radiation driven by aerosol forcings.

This study is important for understanding the aerosol-climate interactions and relative importances of different forcings (e.g. aerosol forcing, greenhouse gas forcing) on climate responses. The study is also insightful for predicting future climate change based on in-depth theoretical understanding of the leading factors in regulating storm track responses to external forcing. Therefore, I think the research is a valuable contribution to the field and can be accepted for publication with AGU Advances. Meanwhile, there are a few minor revisions that need to be address.

Thank you for your positive feedback.

1. It should be noted that East Asian anthropogenic emissions (mainly the Chinese emissions) of aerosols peaked around 2010, and there was a significant declining trend after that (Zhang et al., Clim. Dyn. 53, 5881-5892, 2019). Hence, I'm wondering whether the storm track trends would be more robust if the analyzing period stop at the year 2010?

Thank you for pointing this out. Previous work demonstrated that East China

AOD trends in CMIP and observations differ after 2006 (Zhang et al. 2019, Wang et al. 2021). We performed a similar analysis as our Figure 1, but for EKE trends from 1980 to 2006 (Figure R1). Changing the end year does not make the reanalysis trend more robust. The difference between the ALL simulation ensemble-mean EKE trend and the reanalysis trends can be attributed to a larger influence of internal variability in a relatively shorter period (27 years compared to 41 in the main text). Our main result that greenhouse gas and aerosol forcing contribute similarly to the storm track weakening does not change. We added related text in the revised manuscript [see lines 269–271].

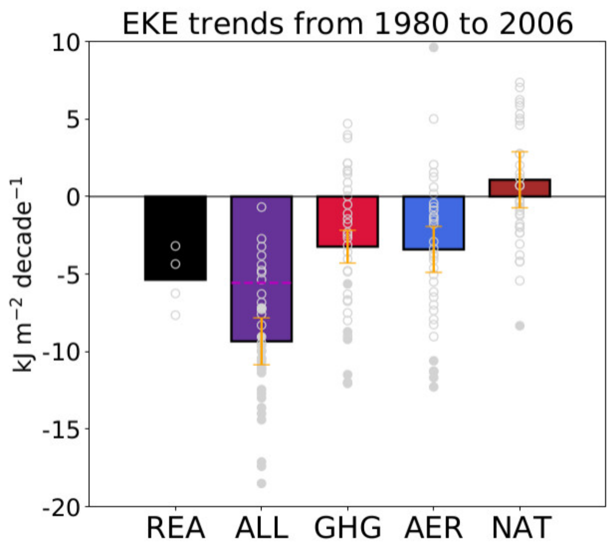

Figure R1: **Similar results to Fig. 1b , but for EKE trends from 1980 to 2006.**

2. It would be better in include more details about the aerosol species in aerosol emission trends of different regions in CMIP5 DAMIP simulations because they can bring different aerosol forcings spatially (e.g. black carbon described in section 3.2.2 and sulfate described in section 1). Previous studies suggested different effects of absorbing aerosols and scattering aerosols in altering atmospheric wave propagations (e.g. Dow et al., J. Climate, 34, 1725–1741, 2021).

Thank you for your suggestion. We additionally described the aerosol forcing in the hist-aer simulations in the method section. We also cited Dow et al. (2021) in the introduction [see lines 89, 134–138].

3. The NA term (non-atmospheric fluxes) in equation (3) is not analyzed in the manuscript, please briefly explain why it is less important in contributing to MSE budget and fluxes in this study.

The NA term represents surface heat storage and flux divergence. It is less important over land since land does not flux energy horizontally and storage is

small due to small heat capacity in seasonal time scale (summertime average). We added related text in the manuscript [**see lines 191–192**]**.**

4. Comparing Fig. 5a and 4a, the northwestern Pacific between 20-30N clearly shows larger negative all-sky SW flux than the clear-sky. It indicates the pronounced aerosol-cloud interactions and related aerosol indirect forcing in such a region as reported by previous studies (e.g. Wang et al., Nature Comm., 5, 3098, 2014).

Thank you for catching this. In the revised manuscript, we mentioned the role of aerosol-cloud interaction in that region [**see lines 356–357**]**.**

5. The results by idealized model described in section 3.2.3 need more descriptions/ justifications: In figure 8(b) and 8(c), why are MSE fluxes convergence and divergence due to transient eddies constant for the entire three downstream ocean sectors(which is in contrast with figure 5 (e) and 5(f) which shows larger magnitude in convergence/divergence in the middle of the oceans than the periphery)?

Thank you for your comment. We added details explaining Figure 8 in the revised manuscript. The MSE flux divergence/convergence trends over the ocean are constant (cosine weighted in the figure) in the theoretical model to represent land-to-ocean energy flux trends at the simplest level. We currently do not have a theory to predict the specific pattern of the trends, but even with this simplification, we find seasonal structure in the storm track trends. We added related text in the revised manuscript [**see lines 457–462**]**.**

In figure 8(c), why is there no trend in MSE flux divergence due to transient eddies over the land?

Since the goal of the theoretical model is to describe the storm track response to aerosols only with the leading order energy balance, the MSE flux divergence trends due to transient eddies are neglected over land. Over land the dominant balance is between surface turbulent flux and MSE flux divergence due to stationary circulations.

Why do storm track decompositions predicted by idealized model in figure 9 (d) show less variance than AER simulations in figure 7 (d)?

The variance in the idealized model is controlled by TF**, the only term we take from the AER simulations. Since the variability in** TF **is smaller than circulations from the AER simulations (e.g, Fig. 7), the variability is much smaller. We added related text in the revised manuscript** [**see lines 474–478**]**.**