Response to Reviewer 1

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.

Kang et al. reported that anthropogenic aerosols significantly weaken the Northern Hemisphere midlatitude storm tracks, particularly in the Pacific. The mechanisms behind this weakening are complex. The authors explained it using a regional energetic framework:

First, shortwave radiation is transparent to the atmosphere. The increased shortwave radiation, resulting from aerosol reduction over Eurasia and North America, affects the atmospheric energy budget by altering surface sensible and latent heat fluxes. Second, the increased contrast in the atmospheric energy budget between land and oceanic regions strengthens stationary energy transport. This enhanced transport outweighs the initial changes in the energy budget gradient induced by surface fluxes poleward of the storm tracks. Consequently, the poleward energy transport accomplished by transient eddies weakens. Finally, the reduced shortwave radiation at the surface, related to increasing aerosol emissions over South and East Asia, exhibits similar but opposite effects: decreasing surface turbulent fluxes and weakening stationary energy transport. These effects further weaken the storm tracks because they occur equatorward of the storm tracks, still demanding a reduction in poleward energy transport.

The findings are extremely relevant and interesting!! I personally truly enjoy reading the paper. There are many deep thoughts involved. I see many potential follow-up studies for this research. The novel aspect, however, is quite theoretical. I elaborate more on the novel aspect in the following paragraph and let the editor judge whether it suits AGU advances.

Thank you for your evaluation and the accurate summary of the energetic mechanisms we explained. In particular, your summary was valuable in revising the result section more succinct.

Based on my understanding, the key novel aspect of the study is the mechanistic understanding outlined above. Previous studies have discussed the influence of aerosols on the Northern Hemisphere's jets and storm tracks, including those over the North Atlantic and Eurasia. Emphasizing the differences between the Pacific and Atlantic storm tracks does not represent a significant breakthrough. Additionally, the only two analyses on reanalysis data or observations are the weakening of storm tracks and surface shortwave fluxes. There isn't sufficient evidence to show that the mechanisms discussed, based on the aerosol-only experiments in DAMIP, are operating in the observed trend. In other words, I don't see new evidence convincing readers that aerosols cause the weakening. The key breakthrough of the study lies in the mechanistic understanding of the weakening.

The reviewer is correct that a few papers have examined the circulation response to aerosol forcing for the annual mean (Wang et al. 2015, Diao et al. 2021, Needham et al. 2023). Other studies focused on the zonal-mean storm tracks in summertime (Chemke and Coumou 2024). Thus, to our knowledge, the regional impact of aerosol forcing on the weakening of summertime storm tracks has not been reported before. We acknowledge that Dong et al. (2022) focused on the summertime jet trends.

In the following, we provide additional evidence that the energetic mechanisms discussed in our paper are operating in the reanalysis trends.

To make a more convincing case that aerosols do cause the weakening, which would certainly attract a wide interest across many disciplines, more work is needed:

(1) I don't think we trust surface fluxes for reanalysis, and I don't see a strong land-sea contrast for TOA shortwave fluxes in CERES (Figure 4 in Quaas et al. 2022, cited by the authors). Please clarify. Would a JJA plot for CERES be helpful?

Thank you for your suggestion. We calculated the trends of JJA TOA shortwave radiation from 2000 to 2020 in CERES and ERA5, and added the result to the supplementary information as Figure S5 below.

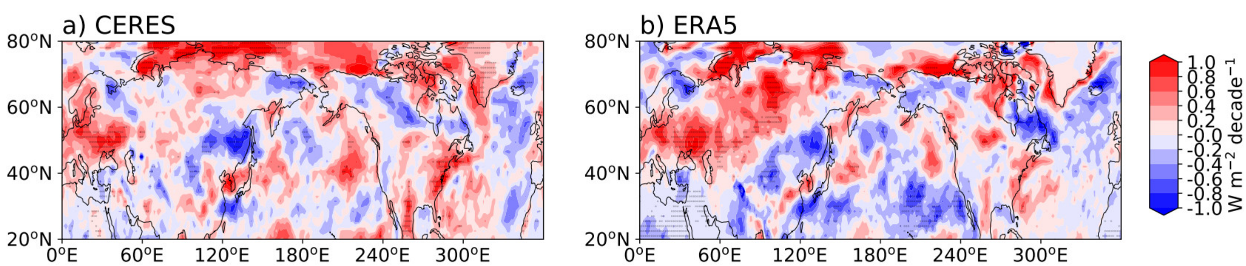

Figure S5: **Spatial pattern of JJA top-of-atmosphere shortwave radiation trends from 2000 to 2020 in (a) CERES and (b) ERA5. Statistically significant trends at the 95% confidence level are stippled.**

In Figure S5, positive TOA shortwave flux trends are seen over land in CERES and ERA5, particularly over Europe and eastern North America. The spatial correlation of the two trends is 0.62 (20–80°N). This gives confidence to ERA5 shortwave trends from 1980-2020 which are shown in the manuscript (Fig. 4). In the DAMIP simulations, the land shortwave radiation trends are dominated by the response to aerosol forcing (Fig. 4). We discuss this in the revised

manuscript [see lines 347–348].

In addition to the analysis above, we became aware of recent work that showed TOA shortwave radiation trends from 2001-2019 (CERES era) are driven by aerosol forcing (Hodnebrog et al. 2024). We added this reference to the manuscript [see lines 93, 345].

(2) Do you see similar patterns in ERA5 reanalysis if performing analyses like Figures 5-7?

Thank you again for the suggestion. As the reviewer noted, evaluating trends as in Figs. 5–7 in reanalysis is difficult due to the uncertainty of surface energy flux trends, and their trends might be inaccurate. Nevertheless, previous work showed that ERA5 turbulent flux trends over midlatitude land (focused on North America and Europe) are reliable compared to in-situ observations (Martens et al. 2020). This allows us to perform a similar analysis as Figs. 6a-c (land) using ERA5 following your suggestion.

We quantified trends in turbulent flux (TF), radiative cooling (Ra), and atmospheric energy flux divergence ($\nabla \cdot \mathbf{F}$) over land from ERA5 from 1980 to 2020, and added it as Figure S6. We also quantified the trends in DAMIP ALL simulations to be compared with the ERA5 trends. The selected regions are the same as those in Figs. 6a–c.

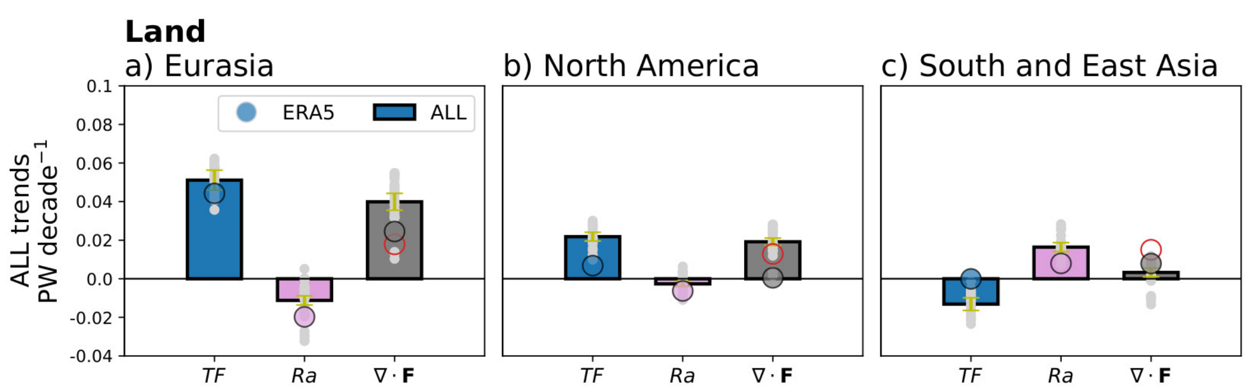

Figure S6: (a) Linear trends in surface turbulent flux (blue), radiative cooling (pink), and MSE flux divergence (gray) from 1980 to 2020 in ERA5 (circles) and ALL simulations (bars: ensemble mean, dots: ensemble members) over Eurasia domain in Fig. 6. (b, c) Same as (a), but for (b) North America and (c) South and East Asia. Red circles represent MSE flux divergence trends due to stationary circulations ($\nabla \cdot \mathbf{F}_{\mathrm{SC}}$)

The results show that there are positive surface turbulent flux trends over Eurasia and North America (blue, Fig. S6a and b) that lead to the export of energy (gray, Fig. S6a and b) via stationary circulation (red circles), similar to Fig.

6. This suggests that a similar mechanism is operating in the reanalysis. We discuss this in the revised manuscript [see lines 328–330, 368–369, 382–383].**

Additionally, since calculating trends in MSE budget from ERA5 is affected by reanalysis uncertainty, we investigated the temperature trends consistent with previous work (Dong et al. 2022) to further demonstrate the contribution of aerosols and added Figure S3. Focusing on the role of aerosols, we find that reanalysis and ALL simulation temperature trends are similar over the Pacific, and exhibit cooling trends across the subtropical regions (20-40N, Fig. S3a and b). This cooling trend, which is related to weakening temperature gradients consistent with weaker storm tracks, is dominated by aerosol forcing (Fig. S3d). We briefly discuss this in the revised manuscript [see lines 316–318].

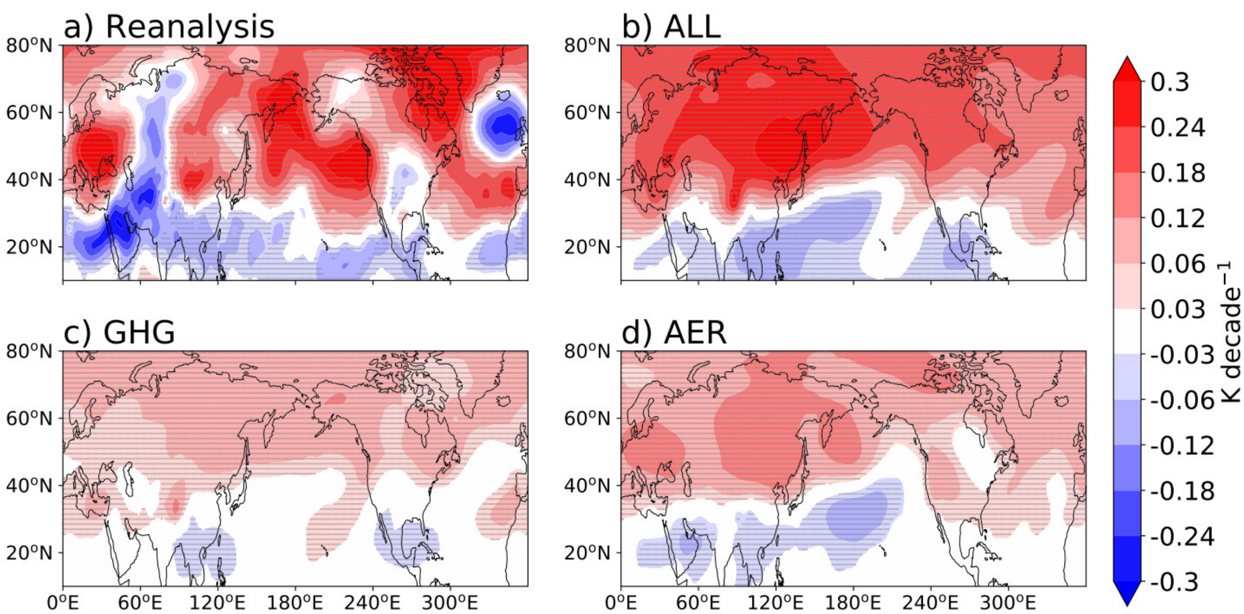

Figure S3: **Similar results to Figs. 3a–d, but for 500-hPa temperature trends with the global-mean removed the emphasize the gradients.**

An alternative factor that may cause the stormtrack to weaken is the negative IPO trend.

To investigate whether IPO trends are important for the NH summertime storm track weakening trend, we quantified unfiltered IPO trends following Henley et al. (2015) in NAT simulations. Specifically, we first investigated whether there is a relationship between storm track and IPO trends emerging without greenhouse gas or aerosol forcings. The results are added as Fig. S11 in the revised manuscript.

In Fig. S11, the IPO and storm track trends in NAT simulations are shown in gray circles. We find that the relationship between IPO and NH summertime storm track trends is statistically insignificant (p-vaulues > 0.05). Moreover, the relationship, built without considering anthropogenic forcing, does not explain

observed trends (black circles). Thus, IPO trends have a weak impact on the NH summertime storm track trends in the satellite era, suggesting the role of anthropogenic forcing such as aerosols. This is briefly mentioned in section 4 in the revised manuscript [see lines 554–556].

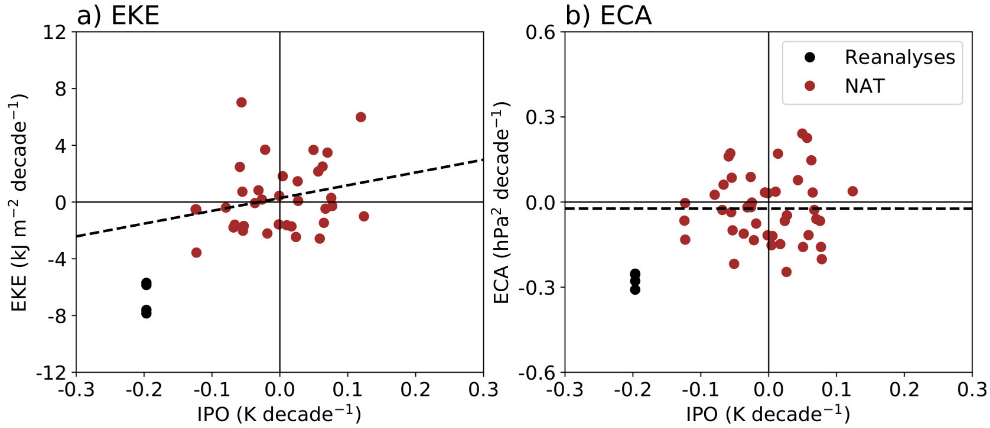

Figure S11: **(a) Scatter plot of IPO (x-axis) and NH JJA EKE (y-axis) trends from 1980 to 2020 in (black) reanalyses and (purple) NAT simulations. (b) Similar results to (a), but for ECA trends.**

I don't see convincing evidence showing the contribution of aerosols (explaining the observed trend, not the DAMIP single forcing) in the current analysis in the article. The two additional analyses mentioned above will make a stronger case.

If the authors cannot make a more convincing case via these additional anlayses, I think some of the key points of the paper should be softened. They are based on DAMIP, not the observed trend.

Thank you again for your useful suggestions. We think the additional analyses provide multiple lines of evidence to suggest anthropogenic aerosols have significantly weakened summertime storm tracks in the Northern Hemisphere.

If the editor decides convincing evidence for aerosol causing the weakening in the observed trend isn't necessary, the key breakthrough of the article would then be the theoretical advances and the mechanistic understanding. In this case, I still think the study has a high impact, probably more so in the climate dynamics community than a broader community though. Related to the mechanistic understanding, I have a few comments regarding the descriptions:

(1) The method section is extremely compact and difficult to follow. I think about the energy budget often, and I still find it difficult to follow. For example,

Thank you for your comment. To address your comment that the method section

is difficult to follow, we significantly revised section 2.4. Specifically, we added more steps to help readers understand the derivation of regional storm tracks. Moreover, the implications of the energy budget equations are explained as we demonstrate the equations [see revised section 2.4]. We also included further mathematical details in Appendix A.

(1.1) The authors refer to Donohoe and Battisti 2013 for the calculation of the MSE flux due to transient eddies. Reading through the Appendix of Donohoe and Battisti 2013, my understanding is that they calculate the total MSE flux, not the transient eddy component. Please clarify. Even if they do calculate the transient eddy component, it will be helpful to spend a few sentences describing how they did it.

To clarify, we previously used the MSE flux divergence calculation proposed by Donohoe and Battisti (2013). This method has the advantage that energy flux divergence calculations "can be done without explicitly balancing the mass budget with a barotropic wind correction" (see their appendix). From their work, monthly-mean MSE flux divergence is calculated as $\{\overline{\mathbf{U}\cdot\nabla m}\} + \{\overline{\tilde{m}\nabla\cdot\mathbf{U}}\}$, where $\tilde{\cdot}$ denotes deviation from the vertical average and U is the horizontal wind vector. Accordingly, we followed their formulation and calculated MSE flux divergence due to transient eddies as $\{\overline{\mathbf{U}'\cdot\nabla m'}\} + \{\overline{\tilde{m}'\nabla\cdot\mathbf{U}'}\}$.

However, as the reviewer implied, we find that applying this formulation to the transient eddy component is unnecessarily complicated. Therefore, in the revised manuscript, we no longer take this approach and calculate $\nabla\cdot\mathbf{F_{TE}}$ by explicitly taking the divergence of $\mathbf{F_{TE}}$. The new approach does not change our results at all since the transient eddy components, which are mostly non-divergent, are not affected by mass inconsistency in reanalysis and models. We clarified how we calculated the transient eddy components in the revised manuscript [see lines 214–216].

It seems easier to calculate the stationary component (with monthly mean v and m), why is it estimated as a residual?

It is more difficult to accurately calculate the MSE flux divergence due to stationary circulation from a dataset that has a coarse vertical resolution (8 vertical levels) such as DAMIP simulations. The MSE flux due to stationary circulation is often a small residual of competing larger fluxes, such as dry static energy flux aloft and latent energy flux near the surface. Using coarse vertical levels could not properly resolve this process and lead to large errors. For this reason, the stationary circulation component is often calculated as a residual (Shaw et al. 2022) or a mass adjustment was applied to it (Donohoe et al., 2020). We calculate the MSE flux divergence due to stationary circulations as a residual and added related text [see lines 216–218].

(1.2) As of now, it is impossible to understand the regional energetic framework discussed in the paper without reading Boos and Korty 2016. While the mathematical derivation may be too long to be included, it is worthwhile to describe the physical concepts. For example, most of the related literature demonstrates their results by plotting the energy flux potential (in shading) and energy transport (in vectors). This can be done for analysis like Figure 5. In particular, my understanding is that the stationary circulation transports most of the excessive energy from land to ocean and its influence on the meridional energy gradient over the ocean is the key component that weakens the poleward energy transport accomplished by the eddies. This is not discussed in the text at all. It is thus not straightforward for the readers to understand the linkage of the two if Equations (5) and (6) only seem to account for energy transport in the meridional direction.

Thank you for your comments. We broke down your comments and responded below.

Regarding your comment that more description of the physical concept is needed, particularly those that explain energy transport from land to ocean in the zonal direction, we made sure to describe the physical concept in the method section. With these changes, we clarified the linkages between Eqs. (6) and (8). We emphasize the following changes in section 2.4.

[Lines 185–187, for Eq. (3)] "the decreasing sulfate aerosol emissions and increasing surface shortwave radiation trends over Eurasia and North America can impact the atmospheric energy budget by increasing the surface turbulent (sensible and latent) fluxes"
[Lines 195–196, for Eq. (4)] "The aerosol-induced surface turbulent flux trends over land can then impact the atmospheric energy export from land."
[Lines 203–204, for Eq. (4)] "The increased contrast in the atmospheric MSE budget between land and ocean strengthens land-to-ocean energy transport."
[Lines 219–220, for Eq. (5)] "Land-ocean energy coupling is mainly in the zonal direction (Donohoe & Battisti, 2013) and accomplished by the stationary circulation, $\mathbf{F_{SC}}$."
[Lines 239–243, for Eq. (8)] "From above, $-I_{SC} = 2\pi a \cos\phi \partial_y \mathcal{L}(-\nabla \cdot \mathbf{F_{SC}})$ shows that storm track intensity will depend on the meridional gradient of $\mathcal{L}(-\nabla \cdot \mathbf{F_{SC}})$ or similarly $\nabla \cdot \mathbf{F_{SC}}$. Thus, oceanic storm tracks can weaken when stationary circulations converge energy at high latitudes as a result of energy transport from upstream land."

To help readers understand, we changed the notation of regional storm track intensity from $2\pi a \cos\phi F_{TE}^{y,\star}$ to I_{TE} (Eq. 8). The new notation is adopted because it poses less emphasis on the meridional direction when the zonal energy flux is also important.

Regarding your comments on energy flux potential, we agree with the reviewer that the energy flux potential is a very useful concept. As suggested, we utilize the concept of energy flux potential in Appendix A. Although we can add diver-

gent MSE flux trend vectors to Figures 5d and e (Fig. R1) we decided not to add energy flux potential contours or MSE flux vectors in the manuscript, since it could add complexity.

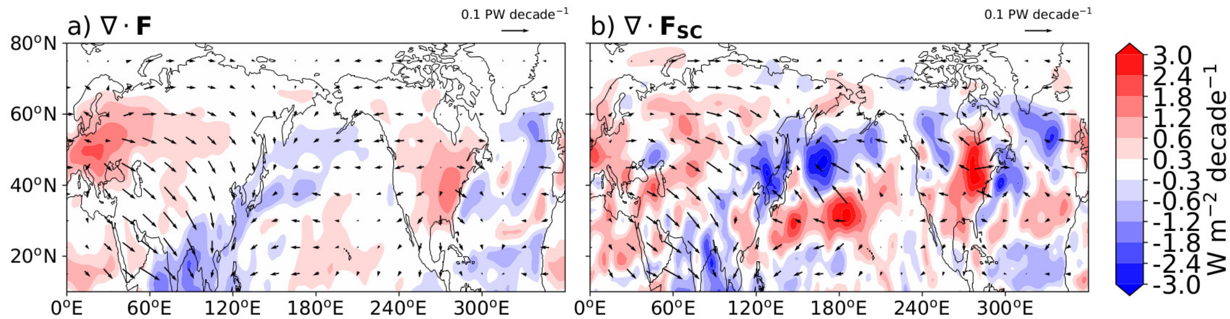

Figure R1: **Same as Figs. 5d and e, but with divergent MSE flux trend vectors.**

We believe the changes we made above make it easier for the readers to understand the physical concept and the method section.

(2) The stationary component holds the key in explaining the stormtrack weakening and deserves more explanation.

(2.1) To me, the paragraph starting from Line 492 is the clearest paragraph written. I was quite lost while reading all of the bars and numbers for spatial correlations in the result section. From the atmospheric MSE budget perspective, SW does not affect the energy budget and turbulent fluxes explain the divergence of total MSE transport in Figure 5d. These concepts and how the altering energy budget demand changes in atmospheric energy transport should be illustrated more explicitly.

Thank you for your comment. To address your comment that the result section is difficult to follow, we significantly revised sections 3.2.2 and 3.2.3 to explicitly explain the physical concepts. The two sections are now merged into section 3.2.2. In this section, we emphasized: (i) increased land-to-ocean energy contrast due to aerosol emission trends over Eurasia and North America, (ii) increased land-to-ocean energy transport due to stationary circulations, (iii) energy convergence over ocean that demands weaker poleward energy transport by storm tracks (weaker storm tracks), and (iv) similar but opposite processes related to South and East Asia aerosols, as the reviewer summarized [see lines 371–401].

(2.2) Figure 8 and Section 3.2. 4 are difficult to understand. What determines the structures and values in Figure 8b?

To address your comment that section 3.2.4 (section 3.2.3 in the revised manuscript), the equations are now presented with the physical concepts they represent [see lines 450–464].

In Fig. 8b, the area-integrated MSE flux divergence trend due to stationary circulation over land is distributed over the downstream ocean as a constant value at each grid point (cosine-weighted in the figure). This is to represent the land-to-ocean energy flux trends at the zeroth order. We revised the text to clarify what determines the structure and values in Fig. 8b [see lines 457–462].

(2.3) Please explain processes controlling stationary energy transport in more detail. For example, it seems like the wave number 2 patterns described in Shaw 2014 or Shaw and Voigt 2015 can explain the role of the zonal and meridional energy transports?

We agree this process is similar to and related to land-to-ocean energy contrast. This process is dominated by vertically integrated MSE advection by stationary circulation (Fig. R2). We added related text in the revised manuscript [see lines 442–444].

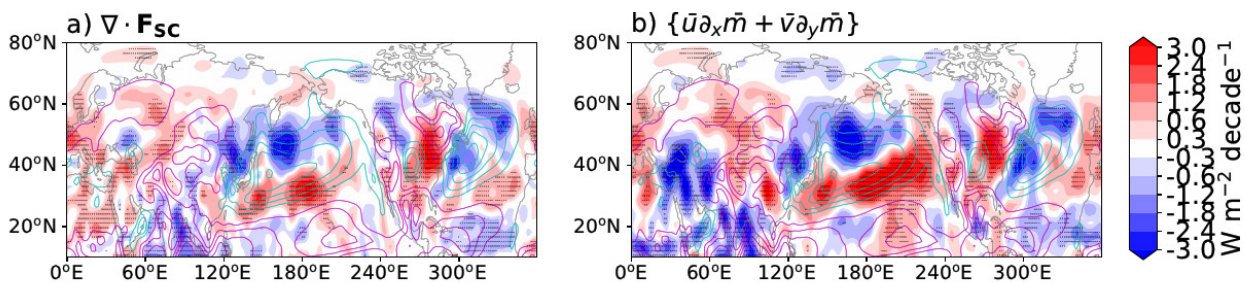

Figure R2: **(a) Same as Fig. 6e. (b) Trends in vertically integrated MSE advection.**

(2.4) The compensation between stationary and transient component implies one cannot get the first order picture of aerosol's influence on energy transport based on a simple diffusive perspective. This may be worth ellaborating.

We agree with the reviewer that the transient component along cannot be predicted simply from a diffusive perspective, but the total energy transport (when not decomposed) is diffusive.

References

Donohoe, A., Armour, K. C., Roe, G. H., Battisti, D. S., and Hahn, L. (2020). The partitioning of meridional heat transport from the last glacial maximum to CO2 quadrupling in coupled climate models. *Journal of Climate*, 33(10):4141–4165.