Peer Review History for 2024AV001318

Reviewer #1

[Reviewer Comments begin on the next page.]

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.

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.

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?
(2) Do you see similar patterns in ERA5 reanalysis if performing analyses like Figures 5-7?
An alternative factor that may cause the stormtrack to weaken is the negative IPO trend. 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.

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

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

Reviewer #2

[Reviewer Comments begin on the next page.]

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.

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?
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).
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.
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).
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)? In figure 8(c), why is there no trend in MSE flux divergence due to transient eddies over the land? Why do storm track decompositions predicted by idealized model in figure 9 (d) show less variance than AER simulations in figure 7 (d)?
6. Line 390: what does "The opposite" refer to?
7. Line 523 and Fig. S9, no significant Arctic sea ice loss under the aerosol forcing in the past decades is

consistent with other modeling studies. For example, Wang et al. revealed a cancellation effect of aerosol forcings from Europe and East Asia mediated by ocean dynamics in the Arctic (J. Climate, 31(1), 99-114, 2018).

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Reviewer #1

Kang et al. present convincing evidence that aerosols significantly weaken the Pacific storm tracks. Moreover, both emission reductions in Europe and increases in Asia - located poleward and equatorward of the storm tracks - contribute to this weakening. I find that the revised manuscript has improved significantly. The authors provide additional observational evidence that strengthens their arguments. They have also extensively rewritten many sections, making the analyses and mechanisms much easier to understand. I appreciate the authors' efforts in addressing my comments and recommend the article for publication.

Reviewer #2

The authors have adequately addressed my questions.