Reviewer 1

This paper uses the MiMA GCM to investigate how three surface inhomogeneities, land-sea contrast, topography, and ocean heat transport, individually and jointly shape the zonal localization and hemispheric asymmetry of midlatitude storm tracks. The authors quantify both isolated and full nonlinear responses to each building block, and use a moist static energy budget to attribute the stronger SH storm tracks to specific terms in the energy balance. The main conclusions are that (i) all three building blocks contribute to storm track localization with substantial non-additivity, (ii) land-sea contrast is the most important factor for the NH/SH asymmetry, and (iii) observationally poorly constrained ocean heat transport can have significant effects on the non-additivity of the different surface inhomogeneities.

The experimental framework is well-designed and represents clear progress over earlier idealized studies by combining realistic geography with the flexibility to isolate individual forcings, and by comparing the NH and SH. The conclusion that storm track strength is very sensitive to uncertainties in ocean heat transport is an important result and should inspire future research. This study contains a wealth of results, for which there is simply not enough space for discussion. I believe the authors did a great job at summarizing the detailed analysis and condensing out the most important take-home messages. Nevertheless, I feel two aspects deserve more attention in the discussion of the results: (i) the role of moisture, and (ii) the interpretation of the weakening effect of land-sea contrast on the storm tracks.

Thank you for your constructive comments and careful reading.

General comments

The role of moisture: Moisture has significant effects on the organization of storm tracks through latent heat release (e.g., Schemm, 2023; Auestad et al., 2025), and its effects on blocking anticyclones are well documented (e.g., Steinfeld et al., 2020). Hence, I assume that the presence of moisture will also have a role in partitioning MSE fluxes into transient eddy and stationary eddy contributions, which will likely be modulated by the separate building blocks as well. While this is beyond the scope of the current analysis, the progress in our understanding of the role of moisture for storm track structure since Brayshaw's studies, in my opinion, would motivate a short discussion in the outlook section of this paper.

We agree that moisture has a key role in storm tracks and that a substantial portion of the transient and stationary MSE flux is via moist processes. We have added to the methods section:

"To fully isolate and understand the statistical properties of storm tracks, it is necessary to systematically add or subtract key processes to a nonlinear moist GCM

(Hoskins, 1983) due to the pronounced role of latent heat release in storm track structure (Shaw et al. 2018, Shaw & Graham 2020, Schemm, 2023; Auestad et al., 2025)" [line 74-75]

We have added to the Discussion section: "Future work could explore the relative roles of dry vs. moist processes for the results found in this paper" in the context of a new paragraph that discusses why results may differ between TKE and MSE perspectives [line 613-622]

The weakening effect of land-sea contrast: The result that land-sea contrast weakens storm track strength was a bit unintuitive to me at first. I believe that in this study, this can be mostly understood as a localization of the storm tracks, and with that the introduction of stationary waves, and so I agree with the reasoning of the study. However, from a weather perspective, land-sea contrast in the North Pacific and North Atlantic storm tracks has been shown to invigorate cyclone development (Brayshaw et al. 2009). First, such air-sea interaction can again impact the organization of the storm tracks (e.g., Wenta et al., 2024). Second, the scope of WCD also aims at connecting weather and climate dynamics, and here there would be an opportunity to achieve this by adding some more context to the presented results, that would also strengthen the relevance of the work for other communities.

We have revised the portion of the Discussion section that discusses land-sea contrast to note that in the midlatitude North Atlantic (and also North Pacific), LSC leads to a strengthened storm track (consistent with Brayshaw et al 2009). LSC strengthens the meridional T gradient in this region, and so our results are fully consistent with Brayshaw et al 2009. But this effect is outweighed in the zonal mean by a weakening of storms over continents

The weakening of the storm tracks occurs when the stationary eddy flux gets larger. This occurs when topography is added to prescribed sea surface temperature atmospheric general circulation modelManabe & Terpstra 1974 and a slab-ocean atmospheric general circulation model Shaw et al. 2022 (already cited). LSC is a source of stationary eddy activity, and so this effect is consistent with the stationary/transient interaction. This is discussed elsewhere in the paper, and also in response to reviewer 2.

Wenta et al 2024 is now cited when we discuss the experiment in which OHT into the North Atlantic is increased (Section 6, near line 418), as this experiment demonstrates that a stronger Gulf strengthens storm tracks locally. But as for LSC, the hemispherically averaged effect is to weaken storm tracks. We also note this

localized strengthening but weakening elsewhere in the Discussion section near line 602.

Specific comments

L198: In section 3, the T45 run (Fig. 1d) also struggles with the tilt of the NA storm track when compared to T85 (1b) and ERA5 (1a). The tilt is a key zonal asymmetry in the NH with large relevance for the downstream climate, so I suggest adding this aspect to the discussion of the model differences here.

Good point. We have added "The T42 configuration also underestimates the southwest-northeast tilt of the storm track in the North Atlantic" [line 218]

L232: You mention a damping effect of TOPO on the SH TKE, particularly for Fig. 6f. But there is also a pronounced equatorward shift of the TKE. Visually the weakening tendency seems more prominent in the SH. First, is there a good reason you do not mention the meridional shift effect? And second, why is there an opposing sign in this shift between the hemispheres (poleward shift in the NH, but an equatorward shift in the SH)?

We now include "The weakening over the Southern Ocean also projects onto an equatorward shift of storm tracks (consistent with Patterson et al 2020)." Patterson et al provide many diagnostics of this effect, and a detailed analysis of this effect seems beyond the scope of this paper.

Note that this might be a model dependent result. Manabe & Terpstra 1974 and Shaw et al. 2022 do not see a clear equatorward shift in response to flattened topography. Peterson et al 2020 on the other hand do see such an equatorward shift. We are therefore reluctant to make a major point out of this change.

The Tibetan Plateau induces a downstream meridional dipole in storm track strength, with stronger storms near 50N and weaker near 35N. This feature is evident to some degree in Figure 7 of Lee et al 2013: eddy activity in the subtropical Pacific weakens. We now note this as well. Diagnosing why is beyond the scope of this work, but is discussed in Lee et al (though more in the context of intraseasonal changes, which are well beyond the scope of this paper). We now cite Lee et al 2013 near line 251.

Lee, S., J. Lee, K. Ha, B. Wang, A. Kitoh, Y. Kajikawa, and M. Abe, 2013: Role of the Tibetan Plateau on the Annual Variation of Mean Atmospheric Circulation and Storm-Track Activity. *J. Climate*, 26, 5270–5286, https://doi.org/10.1175/JCLI-D-12-00213.1.

L432-433: In afar could the strengthening of stationary eddies and the weakening of transient eddies through the introduction of surface inhomogeneities simply be understood as a localization of the storm tracks in longitude?

That is a reasonable interpretation - we have added "Thus as surface inhomogenities are added, more MSE is transported by the stationary eddies, and so the transient eddies weaken ***and become zonally localized". This localization is also evident in Figure 2 of Shaw et al 2022. [line 463]**

Technical comments

Figure 3 caption: ?? equation

corrected

L159: Check citation format Oort and VONDER

corrected

References

Auestad, H., Shibu, A., Ceppi, P., & Woollings, T. (2025). The latent heating feedback on the mid-latitude circulation. Geophysical Research Letters, 52(18), e2025GL116437.

Schemm, S. (2023). Toward eliminating the decades-old "too zonal and too equatorward" storm-track bias in climate models. Journal of Advances in Modeling Earth Systems, 15(2), e2022MS003482.

Steinfeld, D., Boettcher, M., Forbes, R., & Pfahl, S. (2020). The sensitivity of atmospheric blocking to upstream latent heating–numerical experiments. Weather and Climate Dynamics, 1(2), 405-426.

Wenta, M., Grams, C. M., Papritz, L., & Federer, M. (2024). Linking Gulf Stream air–sea interactions to the exceptional blocking episode in February 2019: a Lagrangian perspective. Weather and Climate Dynamics, 5(1), 181-209.

Brayshaw, D. J., Hoskins, B., & Blackburn, M. (2009). The basic ingredients of the North Atlantic storm track. Part I: Land–sea contras

Reviewer 2

In the present manuscript, the authors investigate how different types of surface forcing modulate storm track intensity and the established NH-SH hemispheric asymmetry. The main forcing mechanisms of investigation are three realistic surface inhomogeneities, namely land-sea contrast, topography, and ocean heat transport. Furthermore, the prescribed albedo is varied to assess the influence of simplifying cloud radiative effects. The authors make use of different storm track metrics and an energy budget framework to quantitatively diagnose the effects of different forcing mechanisms on storm track strength. It is found that adding individual surface inhomogeneities generally shifts the partitioning of heat transport from the transient towards the stationary component of the flow. This is more pronounced in the NH and land-sea contrast is determined as the most influential which can explain the NH-SH asymmetry in storm track strength. Realistic imposed northward ocean heat transport acts in a similar way, yet the transient transport regains importance when ocean transport is increased further. Finally, varying albedo has a minor effect on heat transport due to compensations in short and longwave radiation.

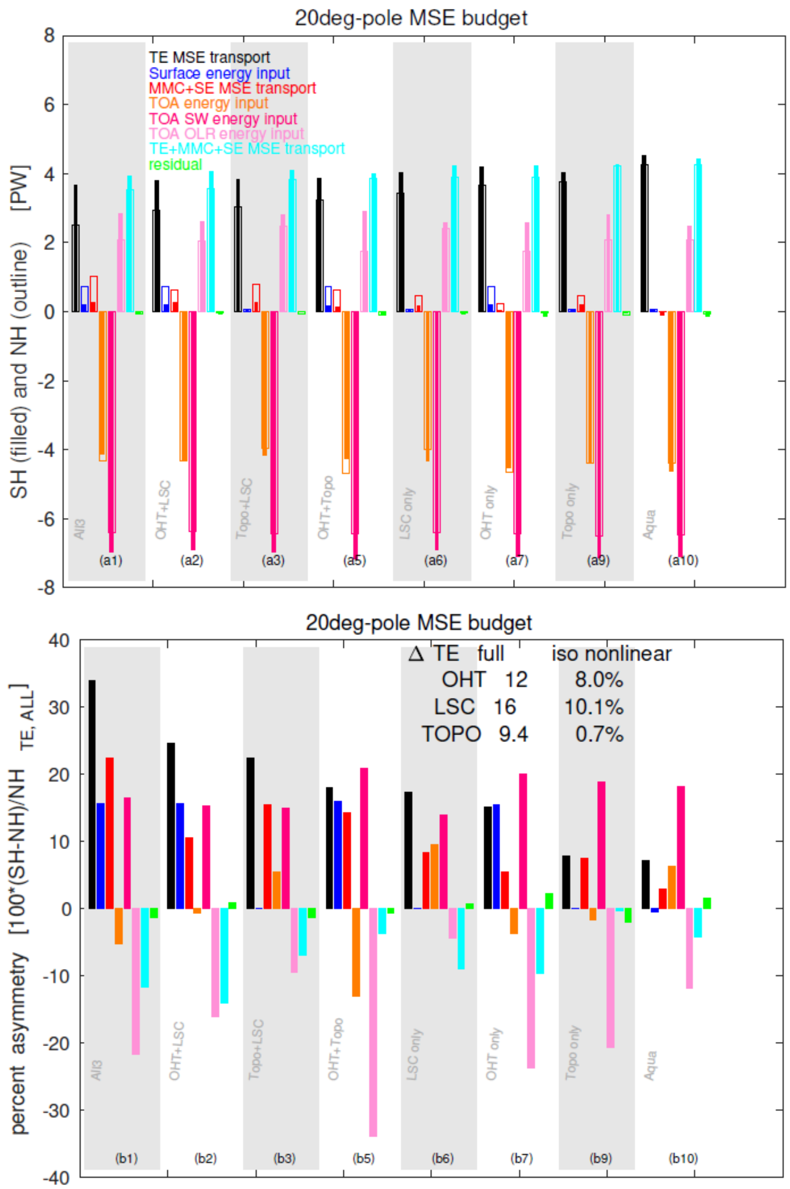
In my view, the authors pose an intriguing overarching research question and the intermediate-complexity model represents the fitting tool to assess it. The systematic approach of including all experiments and isolated vs. full nonlinear responses, showing local changes in storm tracks (instead of zonal means only), and complementing with temperature and wind fields potentially make this manuscript a useful reference for future research on storm track dynamics. The complexity introduced by the number of mechanisms would likely allow for many more in-depth analyses, but considering the current length of the manuscript I believe the authors have already addressed and discussed an appropriate number of aspects. While I would appreciate the analyses within this manuscript to be added to the existing literature, I see a few major points that need improvement or clarification. Specifically, this concerns the determination of the most important inhomogeneity for the SH-NH asymmetry and uncertainty quantification. In addition, at a few instances the presentation of figures and the discussion deserves polishing. Please find further remarks in my comments below.

Thank you for your constructive comments and careful reading.

Major comments

- L301 If the residuals in each simulation are ≤ 0.12 PW, then the change in the residual from one simulation can be ≤0.24 PW. While this is still tiny compared to the total flux, it might be comparable to the reduction of the TE MSE transport in one hemisphere (Fig. 9a). This could become relevant for the hemispheric asymmetry percentage and the interpretation whether one inhomogeneity combination matters more than the other. It would be helpful if the authors could provide an estimate of the uncertainty of the asymmetry percentage of TE and SE+MM with respect to the budget residual. If the authors have tested the sensitivity with regards to the latitudinal integration bound (defining the polar cap as e.g. 40-90°), this might also add confidence to the results.

We have added onto figure 9 and 11 (and also the comparable figure in the supplemental material for the White et al 2021 configuration) green bars for the residual (and also cyan bars for the total AHT). These revised figures confirm that the residual terms are negligible when considering changes in TE vs. SE+MM.



- The interpretability of the results would benefit from showing the response in the total atmospheric heat transport. For instance, the argumentation surrounding the energy deficit related to imposed OHT in L330 or L434 appears flawed to me. Keeping the solar radiation constant but adding a northward ocean heat transport by design leads to a larger “deficit” in the NH, as in more energy has to be lost by longwave radiation. From a TOA perspective, the MHT has to increase, but isn’t this increase achieved by the imposed OHT, such that total AHT does not necessarily have to change (which is stated in L330 “necessitating an increase in overall meridional heat transport in the NH even as TE weakens” and L437 “a more pronounced gradient in net energy input in the NH requires stronger total moist static energy flux in the NH ”)? Indeed, from the bars in Fig. 9a6,8 it does not look like

TE+(SE+MM) increases when adding OHT. Changes in total atmospheric heat transport could be shown as additional bars or in the supplement.

In this light, the formulation in L454 “OHT preferentially fluxes heat to the extratropical NH more than the extratropical SH, which necessitates a reduction in transient eddies mostly in the NH” is not fully precise.

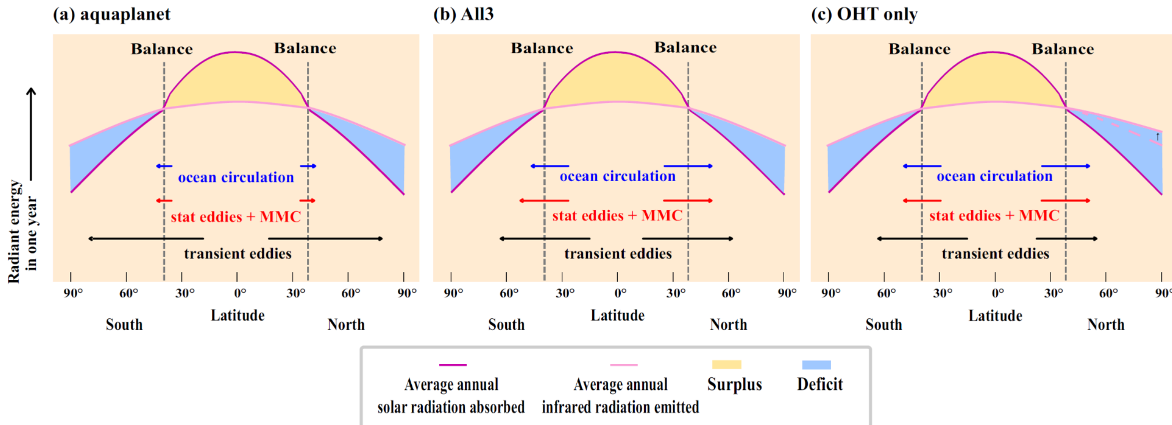
We have added on to the revised figure 9 and 11 cyan bars for the total AHT (=MMC+SE+TE; see above). AHT clearly changes across the various experiments, even though we agree that it doesn’t necessarily have to change and the compensation could be perfect.

It appears something different is happening in these runs than in Cox et al, though we note that in Cox et al the AM3 runs with clouds and full radiation also show less complete compensation. The appropriate place to discuss this point appears to be the Discussion, and so we have left the Results mostly unchanged other than adding the residual and total AHT bars, and also left the paragraph previously found on lines 431 to 439 unchanged. The wording in the paragraph previously near line 454 was imprecise - we agree. It has been changed to “OHT preferentially fluxes heat to the extratropical NH more than the extratropical SH, which necessitates a reduction in atmospheric heat transport mostly in the NH. Most of this is accomplished via transient eddies. However, the stationary eddy term is also involved in the net MSE flux response, as the zonal component of realistic OHT strengthens stationary waves preferentially in the NH, and so this also necessitates a reduction in transient eddies in the NH relative to the SH. This text is now found near line 483.

We now cite and discuss Cox et al 2022 near line 445-448 and at the end of the Discussion (line 605,622). See the responses to specific comments below

Moreover, if I got it right in Fig. 4 the intention behind panel b is to show that OHT takes over more of the poleward heat transport in both hemispheres, but if it is labelled as “All3” shouldn’t the OHT arrows also be slightly asymmetric as in panel c?

This is a good point, and we have made the arrows slightly longer in the NH. See the revised Figure 4 below.



- In L490, you conclude that land-sea contrast is the most important factor for the storm track asymmetry. Based on the MSE framework, the isolated effect of OHT is not too far behind, though, as is topography (Fig. 9b5,6). Is it really justified then to frame the discrepancy between this study and Shaw et al. 2022 as “LSC” vs. “ocean and topography”? Furthermore, both TKE measures in Fig. 7 suggest that OHT instead has the most pronounced effect on the asymmetry (notably, in White et al. 2021 (Fig. S4), it seems to be land-sea contrast) while the response to a combination of two inhomogeneities is very method dependent. Given that the divergence of the total MSE transport is strongly shaped by the boundary currents in the NH reanalysis (Mayer et al. 2024), the comparably weak role of topography for seems consistent with literature. Overall, though, it seems to me that not all storm track metrics that have been investigated are considered for the conclusion.

Thanks for the feedback and for the opportunity to clarify the connection to previous work. We toned things down by changing the title of section 8.1 to “Comparison to previous results on the hemispheric asymmetry”. In Shaw et al (2022) the role of LSC is inferred to be small based on the asymmetry after removing topography and OHT (Fig. 2F F+S). However the role of LSC was never tested explicitly by Shaw et al (2022). Another important factor to consider is that the climatology of the MSE budget based storm track asymmetry in the ECHAM model used by Shaw et al (2022) matches ERA5 closely (see their Fig. S2).

In MiMA none of the metrics we consider is LSC’s share less than a third of the total asymmetry. However, this might be a function of the climatology of the MSE budget based storm track asymmetry in MiMA and the fact MiMA does not have a realistic land surface. The climatology (Fig. 9 All3 b1) shows differences with ERA5 including

a larger MMC+SE transport term (red). We added this as another point in section 8.1 (see lines 550-555).

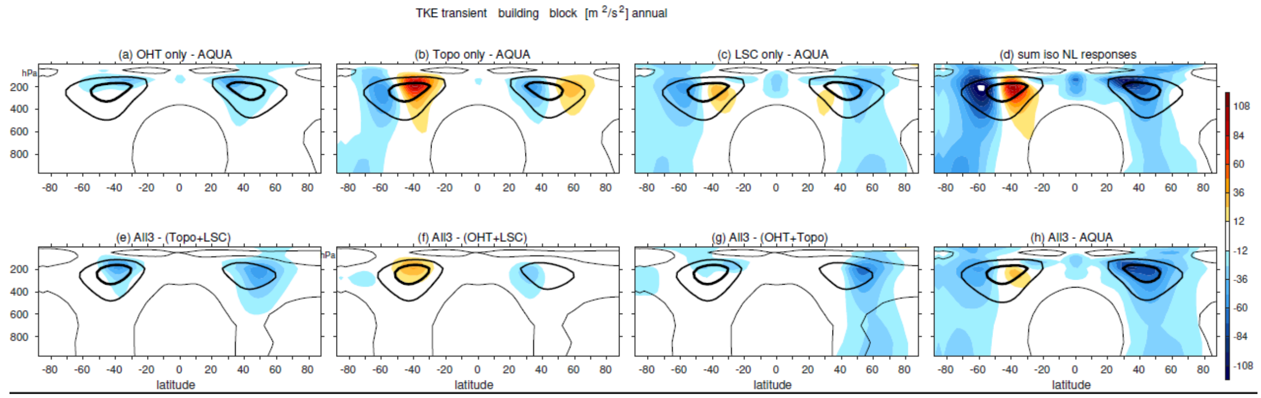
The text now reads “Land-sea contrast is the most important factor for the SH vs. NH asymmetry in storm track strength when using the MSE perspective (Figure 9a4,b4) or the full nonlinear TKE perspective (Figure 7; though notably OHT is more important for the isolated nonlinear TKE perspective in Figure 7)”

Regarding Figure 7a, OHT indeed has the biggest effect for isolated nonlinear, but for full nonlinear the three have roughly equal importance with LSC slightly most important. On monthly timescales (Figure 7b) topography indeed has a weak impact for the isolated nonlinear response, but this is likely because the quasi-stationary eddies forced by topography appear to be mishandled by such a definition, and we prefer not to focus on this point. We include Figure 7b primarily because it allows for comparing to the MSE budget perspective which relies on deviations from monthly means.

We now cite Mayer et al when discussing uncertainties in observational estimates of the surface energy flux term, and especially the sensitivity to methodological choices near the boundary currents (see lines 117, 599).

- In the discussion and outlook of this work, the vertical structure of the storm track response may deserve more attention. While transient MSE fluxes are “bottom-heavy” due to the presence of moisture, band-pass TKE is rather dominated by upper levels flows. This possibly explains some method disagreement (Fig. 7 left vs. right vs. 9b) regarding the most important inhomogeneity for the NH-SH asymmetry. While these analyses help us get some insight to hemispheric asymmetries in storm track intensity, going beyond vertical integrals could help understanding the non-additivity of forcing mechanisms.

We have added a figure showing the vertically resolved 2-8 day bandpass filtered TKE for each of the core 8 experiments. See below.



This figure shows the contribution of each of the core building blocks to the vertical and latitudinal structure of annual-averaged 2-8 day bandpass filtered TKE. (a,e) response to OHT; (b,f) response to TOPO; (c,g) response to LSC; (d) sum of the isolated nonlinear responses in panels a through c; (h) All3 minus AQUA. Each panel also includes the time mean TKE in black contours for (a-d,h) Aqua and (e-g) All3.

TKE is indeed dominated by upper level flows, and non-additivity is evidence for all building blocks though less strongly for OHT. This figure has been added to the Supplemental Material. We have added to the Discussion:

"We used two distinct metrics to track storm tracks: moist static energy fluxes and transient kinetic energy. In many aspects the two metrics gave similar answers, however the role of OHT is somewhat larger for transient kinetic energy (Figure 7 and 9). While TKE is largest in the upper troposphere, transient MSE fluxes are influenced by moisture and therefore peak in the lower troposphere (Figure S8 and S9). This might explain some of the disagreement regarding the most important inhomogeneity for the NH-SH asymmetry. More generally, Figure S8 and S9 reveal non-additivity in the latitudinal vs. height distribution of TKE. Future work should consider going beyond vertical integrals and surface temperature changes to help understand the forcing mechanisms of non-additivity." [line 615-622]

Minor comments

- Entering the manuscript through the abstract left me confused how many building blocks are subject of investigation (or what component is considered a building block) since e.g. the list in L2 includes albedo but later it is referred to three blocks in L9 ("the other two"). It would help specifying that albedo is prescribed and fixed when investigating the three zonal inhomogeneity set-ups.

We changed “other two” to “others”. Explaining in the abstract that we treat albedo differently from the rest seems too detailed for an abstract. This is stated more clearly in the main text.

- L3 The use of the expression “Building on previous work” is not clear to me. Do you mean “Based on...” or “consistent with...”?

“Building on” changed to “Consistent with”

- Section 2: A very brief description of how the land-sea contrast is implemented would be appreciated.

We have added “The land-sea contrast includes different heat capacities, moisture availability, and roughness lengths, for land vs. ocean (Garfinkel et al 2020ab).“

- L104: Rather “The second major change made to …”?

Changed as suggested

- L104 and following: While the implied OHT of the White et al. 2021 configuration is too low, what makes it “good” (e.g. is it similar to observed patters)? If the strength of the surface flux anomalies has a qualitative effect on the TE-SE partitioning, the location of anomalies may too, so I think the motivation for this set-up should be clear.

The pattern is reasonable as compared to observations, it just underestimates the total hemispheric transport. This is now clarified.

- L179 Please add further details on how the flux decomposition and vertical integration is performed on sigma levels. Do you use a time-independent, zonally averaged surface pressure as some other studies that apply such a decomposition? Did you actually test L181 “due to performing the calculation on sigma levels” or have a reference that using pressure levels gives a much larger residual on the hemispheric and annual mean scale? In any case, further information could improve reproducibility.

For the sigma level computation, we use the daily varying lat vs. lon surface pressure.

We actually first performed the decomposition on 30 standard pressure levels (similar to the ERA5 ones, but without the mesosphere) with the interpolation using the time varying lat vs. lon surface pressure, and residuals were on the order of 0.3-0.4PW rather than less than 0.1PW. A very early draft of the paper was based on those results, but we were very happy to switch to sigma levels after seeing the decreased residuals.

This is now noted. " This small residual is due to performing the calculation on sigma levels - residuals are a factor of 3 to 4 larger when the calculation is performed after interpolating to standard pressure levels even when using daily varying latitude versus longitude surface pressure for the interpolation."

- L234 Isn't it appropriate to speak of an equatorward shift of the SH storm track rather than a dampening?

We have added "The weakening over the Southern Ocean also projects onto an equatorward shift of storm tracks (consistent with Patterson et al., 2020)."

Note that this might be a model dependent result. Manabe & Terpstra 1974 and Shaw et al. 2022 do not see a clear equatorward shift in response to flattened topography. Peterson et al 2020 on the other hand do see such an equatorward shift. We are therefore reluctant to make a major point out of this change.

.

- L243 Is there any previous literature on the isolated and/or non-linear response of introducing land in the SH that deserves referencing at this point?

The three studies we are aware of that focus on SH storm tracks do not contrast the isolated vs. full nonlinear response. More generally, we contrast our results in the SH to previous work in the Discussion section.

- Figure 9a7: Adding topography leads to SE taking over TE transport in the NH. This is expected from Cox et al. 2022, yet there seems to be a comparable change in the TOA energy input that is not found in Cox et al. (their total atmospheric heat transport remains largely unaffected). This difference when using realistic topography seems noteworthy.

We have added later in the paragraph "These sensitivities appear to be larger than in the simulations of Cox et al 2022 in which idealized orography is added to an aquaplanet." in the context of trying to understand changes in OLR across the experiments.

- Regarding the combining of SE+MM in Fig. 9: In the extratropics, instantaneous eddy heat transport is anti-correlated with the overturning heat transport (e.g. Cox et al., 2024). How does this look like in a 3-term framework for your simulations? Is TE anti-correlated with SE+MM, TE+SE with MM, or TE+MM with SE?

Exploring the submonthly behavior of TE and MM is beyond the scope of this work. (Due to the way we saved our model output, it would require substantial rerunning of MiMA to make these plots.)

We have added to the Discussion: Future work could explore the relative roles of dry vs. moist processes for the results found in this paper, and also explore why some models show stronger compensation between stationary eddies, the mean meridional circulation, and transient eddies than we find here (Donohoe et al., 2020; Cox et al., 2022, 2024).

Are changes in MM small enough to be excluded from Fig. 4?

We have added MM to Figure 4

- L376 Please specify whether this is MHT or OHT change.

"Ocean" - now added

- L380 The changes of 0.1 PW are rather hard to detect by eye from Fig. 11a. If this figure is intended to fit the full page width, you have enough space to indicate the numeric values next to the bars, for instance. Notably, the values are in the residual range that you specify in Sect. 5.

We have added the residual term to Figure 11 in green, and it is essentially unchanged in 11a1 vs 11a2. So this change in TE is genuine. We considered adding the numerical values to the Figure, however the Figure now has many new bars due to adding in the residual and total AHT. We prefer to instead include the numerical values in the text only.

- When motivating the study with model biases (e.g. L425) I suggest that the authors formulate the added benefit more explicitly as e.g. "… could help assessing the possible contribution of zonal inhomogeneities to model biases" since there are also other model biases such as resolution or clouds.

Good point. We have added "and in particular it could help assess the possible contribution of surface zonal inhomogeneities to model biases"

- L440 Consider adding “weaken atmospheric storm tracks in both hemispheres” to indicate that the hemispheric asymmetry is discussed later on.

Changed as suggested

- Discussion section:

1. Arguably, the “Discussion” already starts with contrasting the findings to Shaw et al. 2022 in L494.

Yes, you are correct. We have moved the section heading to here.

Apart from the labeling, I found these paragraphs rather difficult to follow. It is written that there are “at least two possible reasons” which is followed by three bullet points, while the bullet points contain a back and forth including for instance “… however, … . In contrast, …, however, … . … though…”.

We have added subsection labels which we hope helps with the flow.

Sorry the typo about two reasons when three were listed - the text now read “at least **three possible reasons” - sorry!**

2. L531 and onward: It is a nice idea to discuss the zonal structure of the SH storm track given that you have an appropriate dataset to address this. I’m not against including this but since it is not related to the main research question about NH-SH asymmetries it seems inappropriate to be the first paragraph of the discussion section. In contrast, the discussion of the North American storm track from L541 to me are more relevant to the NH-SH asymmetry.

We swapped the order of the NH and SH paragraphs.

- Appendix B: Is the global integral of the flux divergence equal to zero?

Yes it is. We have added “The global average of these perturbations is zero.”

Technical corrections

- Equation 4: suggest consistently using one symbol for latitude.

Changed to \phi

- Fig. 3: Remove “stationary waves” from subpanel title or put within parentheses?

corrected

- Fig. 4: “east-west OHT only” should mean “south-north”? “FLAT” is not introduced.

Corrected to match the acronyms elsewhere

- Fig. 6e and later: replace “CONTROL” with “All3”.

fixed

- You use OHF in figures but OHT in-text.

Fixed

- Fig. 14 What are the grey crosses?

Gray crosses indicate the climatological jet maximum in (left) All3 and (right) AQUA. Now added to caption.

- L495 It would help if “symmetrized” was clarified as “hemispherically and zonally symmetric”.

Changed as suggested

- L532 “that the zonal structure in SSTs is”

Changed as suggested

- Please add DOIs to your references.

corrected

- Please fix the references by TH Vonder Haar.

Corrected

References

Cox T, Donohoe A, Roe GH, Armour KC, and Frierson DMW (2022) Near invariance of poleward atmospheric heat transport in response to midlatitude orography. J Clim 35:4099–4113. https://doi.org/10.1175/JCLI-D-21-0888.1

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Mayer, M., Kato, S., Bosilovich, M., Bechtold, P., Mayer, J., Schröder, M., Behrangi, A., Wild, M., Kobayashi, S., Li Z, and L’Ecuyer, T. (2024) Assessment of Atmospheric and Surface Energy Budgets Using Observation-Based Data Products. Surv Geophys 45:1827–1854, https://doi.org/10.1007/s10712-024-09827-x