Response to editor

The authors have addressed most of the reviewers' concerns from the first round, with fairly substantial revisions made to the manuscript. Both reviewers are satisfied, and point out only some small remaining issues. I encourage the authors to look after these final points, in particular the comments of reviewer 2 about clarifying the writing and the fact that "quasi-stationary" can be defined in more than one way.

Response: We thank the editor for the overall positive response and we respond to the reviewers as recommended. We include a short mention that quasi-stationary eddies can be defined differently.

In addition, I have three notes:

1) In the caption of Fig. 2, I don't understand this statement: "At each latitude the wave of maximal poleward energy transport is denoted in grey, where values are masked if the wave is responsible for less than 5% of the transport at the latitude with maximum transport." The "where" suggests that the masked values are associated with the grey markings, but I don't see any masking. Or are these two separate statements?

Response: Maybe the word mask is not accurate and we understand that it is communicated a bit unclear. The "where" should refer to the grey markings: If look at Fig. 2b you see that the markings are missing in the tropics.
To improve the formulation we change "At each latitude the wave of maximal poleward energy transport is denoted in grey, where values are masked if the wave is responsible for less than 5% of the transport at the latitude with maximum transport."
To: "At each latitude the wave of maximal poleward energy transport is displayed with a grey dot. The dot is not displayed at latitudes where this wave is responsible for less than 5% of the total quasi-stationary/ transient transport of the latitude with maximum transport."

2) Reviewer 1 has noted some remaining typos/grammatical errors but there are also quite a few others throughout the manuscript. In some cases, the errors interfere with the meaning (e.g., L311-312: "In the SH subtropics..." I'm assuming the "which" clause is not meant to refer to the NH, as currently written, but rather the (larger) transport in the SH?) I recommend that the authors do a thorough proofreading pass.

Response: Indeed that passage was a unclear formulated. We rephrased it to the following: "However, in the subtropic and mid-latitudes the energy transport is $\sim$15% larger in the SH than the NH, which is balanced by more oceanic transport in the NH (Trenberth and Caron, 2001)."
We also do a thorough proofreading as the editor suggests.

3) I would tend to use "partitioning" rather than "partition" whenever you are talking about the way in which the energy transport is separated. I believe it is the more commonly used terminology, and also sounds more natural to me. However, I leave this up to the authors.

Response: We agree and replace "partition" by "partitioning".

References

Trenberth, K. E. and Caron, J. M.: Estimates of meridional atmosphere and ocean heat transports, Journal of Climate, 14, 3433–3443, 2001.

Response to reviewer 1

We thank the reviewer for the positive feedback and for spotting multiple typos.

l. 16: remove the first "is";

Response: Done as adviced.

l. 115: remove "Swanson and Pierrehumbert, 1997";

Response: Thanks.

l. 162: Missing reference to Equation;

Response: Thanks.

l. 195: replace "to" with "with";

Response: Thanks.

l. 312: replace "in the than in the the NH" with "than in the NH";

Response: Thanks.

l. 349: replace "to" with "with";

Response: Thanks.

l. 444: replace "Consequentially" with "Consequently";

Response: Thanks.

l. 464: insert "that" between "such" and "the";

Response: Thanks.

l. 491: replace "as has long been" with "as long";

Response: Thanks.

l. 499: replace "Also" with "Other";

Response: Thanks.

Response to reviewer 2

We thank the reviewer for coming with some interesting thoughts and comments that further improved the manuscript.

lines 49-52: I would combine this paragraph with the following, as here the authors introduce the consideration of the meridional length scales with their short conclusion that a fourier transform along longitude circles does not make much sense. I find this paragraph a bit confusing, why introducing this new aspect when directly concluding that it does not make sense? At least in the next paragraph the authors present their explanations why investigating the meridional scale is not really necessary as it is linked to the zonal scale. I would suggest to combine this into one paragraph and maybe introducing it that the authors focus on the zonal scale of the investigated features. After this they can give their reasoning why further looking explicitly at the meridional scale is not necessary and hence, specify that in the following spatial scale always refers to the associated zonal scale. If considering the meridional scale individually is not necessary, then I would also exclude the part about the fourier transform along longitude circles as it does not contribute to their arguments.

Response: We agree that these two paragraph are a bit confusing and too specific for an introduction. Therefore, as advised, we combine the paragraphs, considerably shorten them and move them to the end of the method section. It now reads as:
"Generally, the scale separation based on the Fourier decomposition is non-local, implying that the whole latitude circle influences the obtained eddies (Heiskanen et al., 2020). Therefore the Fourier decomposition is useful if the transport across the circle is governed by similar eddy scales, which we can observe from meteorological weather maps along zonal bands. Arguably, the zonal and meridional scales of atmospheric eddies match: from the investigation of meteorological weather maps, we know (i) that synoptic-scale cyclones are to a first order circular, and (ii) that the meridional extent, i.e. the amplitude, of Rossby waves, appears to roughly match the distance between a trough and a ridge in the zonal direction. Hence, as many other studies (Graversen and Burtu, 2016; Lembo et al., 2019, e.g.), we interpret the zonal wavenumber of an eddy, which is associated with a zonal wavelength at a given latitude, as its spatial scale."

line 116: I would suggest to replace "normal" circulation by another description, because it is not clear what normal really means. The authors talk about a temporal averaged circulation to get rid of the large noisy day to day variability, so maybe it could be refered to as temporal averaged or averaged instead of normal.

Response: We follow the suggestion of the reviewer and replace "However, here we focus on the annual-mean and season-mean energy transport, firstly to investigate the "normal" circulation, and secondly to compare the traditional separation of the transport by quasi-stationary and transient eddies with a separation based on

spatial scales."
with: "However, here we focus on the annual-mean and season-mean energy transport, firstly to investigate the time-mean behaviour of the atmospheric circulation, and secondly to compare the traditional separation of the transport by quasi-stationary and transient eddies with a separation based on spatial scales. "

lines 117-119: This refers to one of my previous comments in the previous revision process, not seeing quasi-stationary signals necessarily based on time-mean fields. The authors refer to the monthly mean averages as the traditional method to identify the quasi-stationary signal and they also include a reference. So I guess this is the justification to refer to this part as the quasi-stationary signal and use this terminology with the explained meaning in their manuscript. However, a quasi-stationay signal could also be identified for example by temporal low pass filter, excluding the faster transients and only keeping the slowly moving signals as the quasi-stationary part, there is no real reason why one should not do it like this. Depending on the exact separation between quasi-stationary and faster transients for the time filtering, a monthly mean field and the associated monthly averaged temporal filtered field of the wave amplitudes could be different, because it is a temporal mean of a signal that is not necessarily stationary. Due to this, I am not sure I agree with the author's description that "This comparison is only possible from a time-mean perspective... quasi-stationay eddy transport requires a predefined time period over which the eddies are considered stationary". This conclusion is based on their definition of a quasi-stationary signal, which is based on using a time-mean, but from this perspective it is by definition "only possible from a time-mean perspective", but that is a consequence of their method. So the presented results can only be shown based on a time-mean perspective (due to the definition of the method), but it is not necessarily true that investigating the quasi-stationary signal is only possible based on a time-mean perspective.

Response: Here we refer to the quasi-stationary definition utilised by Peixoto and Oort (1992) and Trenberth and Stepaniak (2003a). We agree that quasi-stationary transport can be defined in a different manner as the mentioned temporal low pass filter. Indeed, defining the quasi-stationary transport in such a manner, one would not need to perform the comparison based on temporal-mean fields. Hence, we rewrote the introductory paragraph to Section 2.3:
"The atmospheric energy transport and its components are characterised by a large day-to-day variability (Messori and Czaja, 2013; Swanson and Pierrehumbert, 1997). However, here we focus on the annual-mean and season-mean energy transport, firstly to investigate the time-mean behaviour of the atmospheric circulation, and secondly to compare the newly-introduced separation of the eddy transport based on spatial scales – described below – with the conventional separation into quasi-stationary and transient transport introduced by Oort and Peixóto (1983) and commonly used in the literature (Kaspi and Schneider, 2013; Trenberth and Stepaniak, 2003a,b, e.g.). The conventional separation obtains quasi-stationary eddies from monthly-mean fields; hence a comparison is only possible from a time-mean perspective. Quasi-stationary eddies could be defined differently, for example, by

application of a temporal low pass filter, such the methods could be compared without taking time averages. However, this would prevent a direct comparison with the literature and require a new ad-hoc analysis, and is beyond the scope of this study."

line 162: reference to the equation number not working (??).

Response: Thanks for spotting the mistake. We changed some formulations in the subsection and the references to the equations were checked.
line 256: wavelength instead of wavenumber

Response: Thanks for spotting the mistake.

References

Graversen, R. G. and Burtu, M.: Arctic amplification enhanced by latent energy transport of atmospheric planetary waves, Quarterly Journal of the Royal Meteorological Society, 142, 2046–2054, 2016.

Heiskanen, T., Graversen, R. G., Rydsaa, J. H., and Isachsen, P. E.: Comparing wavelet and Fourier perspectives on the decomposition of meridional energy transport into synoptic and planetary components, Quarterly Journal of the Royal Meteorological Society, 146, 2717–2730, 2020.

Kaspi, Y. and Schneider, T.: The role of stationary eddies in shaping midlatitude storm tracks, Journal of the atmospheric sciences, 70, 2596–2613, 2013.

Lembo, V., Messori, G., Graversen, R., and Lucarini, V.: Spectral Decomposition and Extremes of Atmospheric Meridional Energy Transport in the Northern Hemisphere Midlatitudes, Geophysical Research Letters, 46, 7602–7613, https://doi.org/https://doi.org/10.1029/2019GL082105, 2019.

Messori, G. and Czaja, A.: On the sporadic nature of meridional heat transport by transient eddies, Quarterly Journal of the Royal Meteorological Society, 139, 999–1008, 2013.

Oort, A. H. and Peixóto, J. P.: Global angular momentum and energy balance requirements from observations, in: Advances in Geophysics, vol. 25, pp. 355–490, Elsevier, 1983.

Peixoto, J. P. and Oort, A. H.: Physics of climate, American Institute of Physics, 1992.

Swanson, K. L. and Pierrehumbert, R. T.: Lower-tropospheric heat transport in the Pacific storm track, Journal of the atmospheric sciences, 54, 1533–1543, 1997.

Trenberth, K. E. and Stepaniak, D. P.: Covariability of components of poleward atmospheric energy transports on seasonal and interannual timescales, Journal of climate, 16, 3691–3705, 2003a.

Trenberth, K. E. and Stepaniak, D. P.: Seamless poleward atmospheric energy transports and implications for the Hadley circulation, Journal of Climate, 16, 3706–3722, 2003b.