Reviewer 1

This paper presents a systematic analysis of extremes in the zonally averaged meridional heat transport and how they are related to weather regimes and preferred zonal wavenumbers. The work is based on several decades of reanalysis data and draws on the results from earlier publications. Overall, the authors argue that their results are consistent with previous results regarding weather regimes, dominant wavenumbers, and how they are related to heat transport extremes. The current analysis makes explicit the role of planetary versus synoptic scales in this context. The question of extreme events is of primary importance in our science, and a detailed analysis such as the present one is welcome. I can see this as a publication in WCD.

Yet, I have a few issues. To be sure, I need to say that I am not an expert in the present topic, rather I consider myself as representative of a typical reader of WCD. As such, I had a hard time in several of the more technical sections to understand what the authors have really done. This is probably due to the fact that the text seems to be primarily directed at the expert, who is familiar with a string of earlier publications from the same group. I have no doubt that the analysis is performed in a proper way; however, I suspect that this is hard to appreciate by the average reader of WCD.

As a way out I suggest that the authors should make a serious attempt to more pedagogically introduce the concepts used in their analysis as well as in their results sections. Instead of just providing the references to multiple generations of previous publications, assuming that each reader is familiar with those papers, the authors should add some advice to the not-so-expert reader trying to introduce and/or summarize these earlier developments on a conceptual level. This would increase the readability of and add great value to the paper.

In addition, the paper would benefit if the authors could add some non-technical guidance to the reader as to what these results mean in more meteorological terms and what the implications are. To be sure, you draw a few interesting conclusions. However, you should make a more serious attempt to connect these conclusions to the more technical parts of the paper. Again, I do not doubt the validity of the results or the conclusions; I just feel that this paper would make a much stronger impact if such meteorological guidance were available and if the technical and the interpretatory parts of the paper are connected in a more seamless fashion. Also, you often point out the consistency with earlier results, and by doing so some readers may get lost and left unclear about what is really new about this paper; therefore, it would be good you could point out more explicitly what is new in the current paper.

We wish to thank the reviewer for their careful assessment of our work, insightful comments and constructive criticisms. We acknowledge that the technical description of the methodology is somewhat difficult to follow for a non-expert reader. In the revised manuscript, we will address the details of the methods in an appendix, in order to ensure reproducibility, and focus more explicitly on the qualitative interpretation of the methodology in the main text. We will further restructure the discussion to distinguish between the results that confirm previous analyses and the results which lead to novel conclusions. .

Examples

Let me provide a few examples illustrating the major issue made above. As I said, some work for improvement would be appreciated in the interest of a broader readership.

For instance, equations (3) and (4) were unclear to me at my first reading. If you do a Fourier decomposition of a field and multiply two such fields (as you have to do to compute a heat flux), you obtain a double sum, one for each expansion. You can, then, sort this double sum according to the resulting zonal wavenumber, and this results in each Fourier coefficient of the heat flux being a sum of many terms from the individual terms (v and E) that just happen to add up to the zonal wavenumber in consideration. This is what I would have expected in equations (3) and (4), but your method is different.

To be sure, I could have read the quoted papers in order to educate myself (to be honest, a cursory look into Graversen and Burtu 2016 did not help me a lot), but I would not be too optimistic regarding the readiness of the average WCD reader to do so. Instead, I would have appreciated not just a short "summary" of those earlier methodological developments, but rather a conceptual introduction on a somewhat higher "meta-level".

In the end, the point here is that you consider zonally integrated fluxes, and Parseval's theorem allows one to express the zonal integral of a quadratic quantity as a single sum over all wavenumbers like in (4). The other important point here is that the sum of all individual components such as (3) and (4) is equal to the total, zonally integrated heat flux, which you refer to as "wavenumber decomposition" later in your text. Implicitly, you heavily draw on this property in the rest of the paper. A corresponding hint in the method's section would have helped me a lot!

The reviewer correctly points out that the Fourier decomposition method has limitations, that our analysis is constrained by consideration of zonally integrated fluxes, and that this has not been sufficiently brought up, neither in the introduction, nor in the Methods section. This caveat explains why the transports have to be interpreted in hemispheric budgetary terms. Further, as the extreme detection, being zonally integrated, does not give information on localized features of the dynamics, we start our analysis from weather regimes identified in several regions. This is complemented by looking at the composite means of geopotential anomalies. Therefore, we will expand our introduction, better framing the context of our wavenumber decomposition, and split as discussed above the methods section, with a more interpretative/qualitative description evidencing assumptions and caveats of the analysis in the main and an extended technical description in an Appendix , .

To provide a second example, in Fig. 4b it was not clear to me at first why the extremes do not just represent the tails of the distribution from the color fill (just like in a box-and-whisker plot). This is what I would have expected initially. The same problem arises in the text on line 232: how can possibly the "equatorward and poleward extremes largely overlap"? Shouldn't the extremes represent opposing ends of a PDF? If so, it is hard to see how they can overlap. The solution to this problem probably depends on how you defined the extremes and their PDF: the extremes are defined without reference to a wavenumber, and this implies that the existence of an extreme does not have to be reflected in the PDF of each and every wavenumber. Is that right? Other readers may have a similar problem, and some explanation would be very helpful. In addition, reading this (and related) plots is made more difficult due to the fact that the caption does not give contour intervals for the dashed isolines.

We agree with the Reviewer that it is not clear from the current text how the meridional energy transport extremes have been selected. This is done on the population of total heat transports. As a consequence, the PDFs of the wavenumber contributions to the extremes in the two tails of the distribution can overlap. We will state it more clearly at the beginning of section 3.1, and we will change Figures 3 and 4 in order to account for the contour intervals of the extreme events PDFs.

In the last section, you draw some interesting conclusions, which I was not always able to relate to the core of your analysis. For instance, you say that "planetary scales determine the strength and meridional position of the synoptic-scale baroclinic activity with their phase and amplitude": where exactly have you shown this? How can you make statements about the wave's phase, which (as far as understand) is unavailable from just looking at the zonally integrated heat transport? Similar reservation I have with the conclusions on lines 371-373. I feel that you need to tell the reader somewhat more explicitly how you arrive at these conclusions and which part of the analysis your conclusion is based on.

The zonally integrated approach does not allow to explicitly address the phase of the waves, as correctly pointed out by the reviewer. However, the combined view of dominant wavenumbers, weather regimes and, to some extent, composite analysis, allows us to infer qualitatively how scales interact when total meridional heat transport extremes occur. We argued, in particular, that DJF extremes are the result of a planetary-scale modulation of synoptic-scale eddies, given that:

- the planetary-scale component is the dominant feature of DJF transports, especially north of 40N, with the two tails of the extremes well separated (Figure 3) and previous evidence (Lembo et al. 2019) that synoptic-scale and planetary-scale component extremes are rarely co-located;
- k=2-3 are the dominant zonal wavenumbers for the transport anomalies, as shown in Figure 7, no matter what the sign of the extreme is;
- as Figure 5 shows, poleward (equatorward) extremes are denoted by increased (decreased) frequency in NAO-/AO/PT regimes and decreased (increased) frequency of NAO+/ALR regimes. This is also partly resembled by Figure 10, where z500 composite means are shown;

Based on this we qualitatively argue that planetary scales modulate synoptic-scale baroclinic activity in the population of extremes, with the largest changes in weather regimes related to increased/decreased blocking frequencies and such ultra-long planetary-scale waves dominating the heat transport in the population of extreme heat transports. We plan to rephrase ll. 340-341 and ll. 346-347, l. 371 accordingly, avoiding referring to the phase of the waves.

Take another example: you say on line 385 that "...our results emphasize that the modes related to energy transport extremes are hemispheric in scale". What part of your analysis is this statement based on? My point here is that the chief instrument in your analysis is the investigation of the zonally integrated heat flux, and this leads (almost by design) to "modes" that can be expected to be hemispheric in scale rather than very local or small-scale. In summary, all of these conclusions may be well justified, it was just not easily visible for me. The authors should make an attempt for improvements in this direction.

As above, we acknowledge that the zonally integrated approach somehow limits the opportunity to observe regional-scale features of the meridional heat transport extremes across the NH mid-latitudes. The rationale behind looking at weather regimes in relation to these occurrences was that there could be in principle some mid-latitude regions where the atmospheric circulation is more sensitive to excited meridional heat transport. Looking at the changing occurrences of preferred weather regimes, together with dominant zonal wavenumbers, we found that this is probably not the case, and that the peculiar role of planetary scales may influence several regions at the same time, as also suggested by the consideration of the 2010 Russian heat wave case. Once again, the significance of composite means (see reply to reviewer 2) complements this. In the revised text, we will clarify the logical reasoning that brought us to draw the conclusion stated on l. 385, and the fact that it is based on a qualitative evaluation of our results. This conclusion is consistent with the referenced works by Comou, Petoukhov, Kornhuber on quasi-resonant amplification (QRA), and we hope that we can expand on the dynamical linkages between co-recurrent blockings and meridional heat transports in a future work.

Minor comments

Line 16: This is somewhat advanced material for the start of an introduction. Presumably you talk about vertically averaged moist static energy, right? In the tropics the vertical change of moist static energy is close to zero, because the increase of potential temperature with altitude is, to a large extent, compensated by a decrease of water vapor mixing ratio.

Agreed. We believe it is sufficient, in this context, to refer to it as simply “heat”.

Line 125: you remove the linear trend only in certain latitude bands. Why does this not create awkward discontinuities at the boundaries of these ranges?

The conditional removal of the linear trend was accomplished with the sole purpose of a rigorous application of the EVT-based selection algorithm, particularly to the convergence analysis of the percentile threshold. Thus, it was not applied to the subsequent analysis.

As this was also brought up by reviewer 1, we show in Figure R2 how the thresholds look like if the trend and seasonal cycle are removed everywhere. Whereas the detrending alone (not shown) does not noticeably change the results, the deseasonalizing has a slight effect in DJF, as it can be seen in the first two rows of the figure. There is (left) a small discontinuity at latitude 45°, coinciding with the latitude north of which no deseasonalization has been originally performed. This small discontinuity disappears when the transports are deseasonalized at each latitude (right). However, the change is really minor, especially for the selected thresholds marked by the blue dots, thus it does not affect our results. In case of JJA, there is no noticeable change between the two procedures (left vs. right).

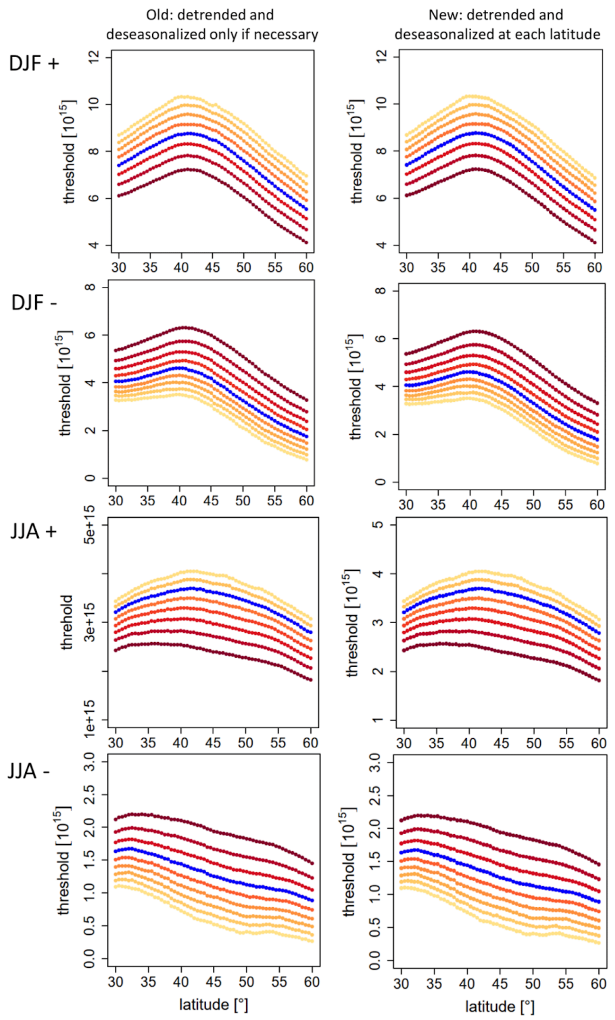

Figure R1: Meridional section of threshold values for meridional energy transport extremes selection considering different percentiles (the selected threshold is highlighted in blue, as in Figure 1 of the manuscript). In the left column, transports have been deasonalised and detrended only where necessary, in the right column everywhere: (1st row) DJF, poleward, (2nd row) DJF, equatorward, (3rd row) JJA, poleward, (4th row) JJA, equatorward.

I suggest to increase the size of the panels in Fig. 1 and 2.

Agreed, we will do this.

Panel 1c, y-axis-label: the threshold should have dimensions, right?! How about the physical dimensions of the scale and the shape parameter?

The shape parameter is a non-dimensional parameter and the scale parameter has the dimension 10^15 W; we will state it more clearly in the figure caption.

Fig 3 and 4: How did you normalize the PDFs? It seems to me that integrating by eye over the heat transport at a fixed latitude one may obtain values larger than 1. Or put the other way: what units does the plotted PDF have? Is it really (10^15 W)^(-1)? How should I read the red and blue dashed contours corresponding to the extreme situations (no contour interval given….).

PDFs are normalized at each latitude by their maximum value, so that their maximum value is 1. In order to account for the different number of extremes at different latitudes, the Friedman-Diaconis rule (Friedman and Diaconis, 1981) is first applied to determine the correct number of bin elements for the discretized PDF, then the kernel smoothing estimate of the PDF (Bowman and Azzalini 1997) is computed. A preliminary condition is applied on the number of bins, that has to be at least 2. PDFs are then normalized at each latitude by their maximum value, so that their maximum value is 1. For graphical purposes, the obtained PDFs for the extremes are finally interpolated on the same number of bins as the filled contour plots of the overall population, which is the same at all latitudes. All this will be detailed in the caption to Figure 3 of the revised manuscript.

Line 217: ".... the PDF steeply decays towards the high latitudes....", I understand what you want to say, yet, it is not really well expressed. You probably want to say that the mean or median of the PDF decreases as one goes to higher latitudes.

That is indeed what we meant to say, even though the PDFs closely follow the meridional behavior of the mean. We will better phrase it in the revised manuscript.

Line 232: (see my general marks earlier): Why can the positive and negative extremes overlap? In my simple-minded thinking, the extremes of a PDF represent the opposite tails of the PDF, so I do not understand why and how these can "overlap". I probably did not understand your definition of "extreme", but it may help other readers if you could say here why this is so.

We agree that this needs to be clarified in the revised manuscript - see our reply to the comment above.

Line 233: What do you mean here by "pattern"?

We acknowledge that the word "patterns" might be confusing in this context, and we will replace it by "features".

Line 268: shouldn't it be ".... higher zonal variability in the former...."?!

If the reviewer refers to line 286, that is correct. We will switch the order of JJA and DJF at the beginning of the sentence.

Line 311 (and similar at some other line): you talk about a "midlatitude channel", but this term is misleading as it should be reserved for a geometric setup with walls at the southern and northern boundary of the channel. As far as I can tell, you are dealing with spherical geometry, never with true "channel geometry".

We will replace the term "channel" with a less specific notion. We would however like to observe that a cylindrical geometry is indeed taken into account for the Fourier decomposition, as its symmetry is required in order to obtain the different wavenumber modes of the transport.

Line 318: a heatwave cannot possibly be a "case study". You probably mean that this heat wave is a "case".

Agreed. We will remove the word "study".

Line 345: what are “higher-scale eddies”? I would prefer the term “smaller scales”.

Agreed. We will edit the text accordingly.

Line 385 ff: (see my earlier remarks): Do your results really suggest that the modes associated with heat flux extremes are hemispheric in scale? It seems to me that this is a necessary consequence of your methodology that focuses on individual wavenumbers. If so, it cannot possibly be a result of your study.

We will rephrase the sentence in order to state more clearly what we mean, as per our reply to the aforementioned general comment.

Typos etc.

Line 34: must be “.... a poleward transport....”

Fig 9 and 10: letters a and b missing to denote the two different panels.

These typos and missing labels will be rectified in the revised manuscript.