Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2021-85

Anonymous Referee #1

Referee comment on "Meridional-energy-transport extremes and the general circulation of Northern Hemisphere mid-latitudes: dominant weather regimes and preferred zonal wavenumbers" by Valerio Lembo et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-85-RC1, 2022

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 our more explicitly what is new in the current paper.

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!

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.

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. 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.

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.

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?

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

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

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….).

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.

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.

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

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

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".

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

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

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.

Typos etc.

Line 34: must be "…. a poleward transport…."

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