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Comment on wcd-2022-26

Anonymous Referee #1

Referee comment on "The global atmospheric energy transport analysed by a wavelength-based scale separation" by Patrick Johannes Stoll et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-26-RC1, 2022

The manuscript "The global atmospheric energy transport analysed by a wavelength-based scale separation", by P.J. Stoll and R.G. Graversen (ID: wcd-2022-26) describes a wavelength decomposition of meridional energy transports in the atmosphere. Revisiting a common approach that has been often used in recent literature, overcoming the partitioning of eddies in a transient and quasi-stationary component and instead discerning between planetary, synoptic and mesoscale eddies according to their zonal wavenumbers, the authors emphasize the importance of distinguishing different thresholds of spatial scale separation for the different eddies as a function of latitude. The authors apply the proposed wavelength decomposition to the overall energy and its components, focusing on moisture and latent energy, discussing their annual mean features, the seasonal cycle and interannual variability. The manuscript focuses on the advantages of adopting this methodology, compared to previous ones, emphasizing the emergence of some crucial features of the dynamics, e.g. the role of planetary scale transports in the Southern Hemisphere.

Overall, I think that the manuscript is reasonably well written, contains an in-depth discussion of the caveats often overlooked when using a well-established methodology, and addresses some theoretical aspects of the general circulation of the atmosphere that, although not unprecedently seen, are enlightened in a clear and unambiguous way, allowing for potential development on these specific topics. What I find surprising, though, is that the authors do not actually focus on conveying in a convincing way neither the potential of the novel methodology, nor the implications for our understanding of the dynamics.

I was wondering if this may be due to a partial lack of context, and mistaking established facts as new findings. For instance, it is well known that meridional energy transport in midlatitudinal eddies is carried out by baroclinic instability mainly. Also, there are several works attempting to overcome the overlapping notions of quasi-stationary waves and Rossby waves, by looking into Rossby wave packets and local wave activity (Chang 2005; Grazzini and Vitart, 2014; Ghinassi et al. 2018). Expanding on some hypotheses and considering available literature might help overcoming the feeling of "speculative thinking" that sometimes underlies arguments contained in the discussion (e.g. the statements about the role of monsoons in summer planetary waves through moisture advection).

Therefore, I recommend accepting the manuscript once these relatively minor concerns are taken into account. General comments are detailed in the following.

Specific comments

ll. 32-43: This is one of the parts of the manuscript where I think that the authors fail at defending the importance of the methodology they introduce. Two aspects remain undiscussed: 1. The authors focus on zonal wavelengths, which is perfectly understandable, but do not comment on what would happen if one would consider meridional wavelengths, instead. 2. Their argument is in favor of choosing scales partitioning wavelength-wise instead of wavenumber-wise, given the diversity of scales across the latitudes. But there is nowhere shown that aspects of the transports that are emphasized with their methodology would not be seen when using a "steady" wavenumber-based partitioning. A counter-factual example would help in this sense;

l. 126: same as above, the authors use the terms "wavelengths" and "spatial scales" almost in an interchangable way. I am a bit confused by this choice, as the claimed rationale behind this work is to capture the different scales of the eddy-driven transport at different latitudes.

- ll. 180-183: I think this is one the main issues with the methodology here described. What latitude matters most for the definition of the eddy, the one where it starts to develop, the one where it grows, or where it decays. I think this has to do with the latitude at which the eddy is at its apex, and as a consequence transports more energy meridionally. The authors suggest here that the preferred spatial scale for synoptic scales relates to the latitude where the cyclogenesis occurs, i.e. the mid-latitudes. But then why do we need to care about latitude, in order to provide a relevant scale for separation between synoptic and planetary scales? This seems a bit of a contradiction, but it might be that I am missing something;

- ll. 203-205: when comparing planetary scales and quasi-stationary components in Figure S6, it appears to me that the scale separation has to do with the scale of the maximum transient eddy activity (as shown in Figure 2), so that the larger the scale separation is, the more you find an overlap between quasi-stationary and planetary scales. As the separation scale gets smaller, the quasi-stationary component tends to vanish. This seems to me to suggest that quasi-stationary eddies are only those located in the ultra-long tip of the wavenumber spectrum, and the rest of the spectrum is mainly composed by transient waves. The two approaches to characterization of the eddies (wavenumber or time dependent) would then actually be coincident, for the right choice of the separation scale. Is that what you are aiming to show?

- ll. 249-251: the relative symmetry of NH and SH planetary-scale transports is actually something new, to the best of my knowledge. I can think of some similar results in the Supplementary Material of Lembo et al. 2019, but nowhere this was actually expanded. This is something that shall probably discussed, in terms of dynamical implications, in order to give a hint of how the methodology allows for a better understanding of the physical mechanisms;

- ll. 343-345: in the conclusion, the authors mention among relevant results that the extra-tropical meridional energy transport is mediated by baroclinic instability. But this is somehow known, and it has been shown, also analytically, in previous works. I can think, among others, of a few recent papers by Lenka Novak (Ambaum and Novak, 2014; Novak et al. 2015). As mentioned above, the authors evidence throughout the manuscript results that are genuinely new and potentially relevant, in order to understand the dynamics of heat exchanges (e.g. the role of planetary scales in the SH, of monsoons in moisture transport during the NH summer season). It is worth putting more emphasis on them in the conclusion as well;

Minor comments

- l. 1: this sentence is more appropriate for an Introduction than an abstract. Consider removing;

- ll. 19-20: I am not entirely convinced that it should be stated in this way. The atmosphere is set in motion by rotation and angular momentum convergence as well, whereas it is clear that the atmospheric motions redistribute energy in order to contrast the differential diabatic heating between lower and higher latitudes;

- l. 26: it is not entirely clear how the Hadley circulation appears in Figure 1, possibly some very quick description (as it is given below) could be provided;

- ll. 30-31: I wonder if the authors could expand on the definition of spatial scale here. In this work, it is often used as a synonim of "zonal wavelength", but the extent to which the interoperability of the two terms can be used is not clear to me;

- l. 61: a summary of the manuscript at the end of the manuscript is always needed, in my opinion;

- ll. 65-66: the authors do not need to refer to ERA-Interim;

- ll. 70-71: not clear what the authors mean here, possibly rephrase;

- ll. 76-77: are the authors referring to geometrical constraints, when referring to "converging latitudes". If so? Please clarify why the zonal mean transport would be an advantage;

- l. 86: mentioning time-mean comparisons, it might be worth mentioning other decomposition techniques, allowing for space-time decomposition, e.g. 2-D wavelet decomposition or Hayashi spectra.

- l. 91: I have a few comments about the definition here. 1. why do you need to define the vector **v** if you are only using the v component? 2. You propose a "formal" definition of energy in eq. 1, but this is not actually the energy that you define in eq. 2. Consider using different notations, in order to avoid confusion.

- l. 95: this dry component is not the dry static energy (DSE), or is it? It should not include a kinetic energy term;

- ll. 111-116: it is clear that because of cylindrical symmetry, cross terms in eq. 6 and 7 cancel, but this should be stated explicitly;

- ll. 124-125: the choice of the mentioned wavelengths for scale separation shall be rather commented here than in Sect. 3;

ll. 139-141: I am surprised that the most basic constraint to the width of the synoptic-scale eddies, i.e. the Rossby deformation radius, is not mentioned;

- ll. 168-169: this finding clearly suggests that eddies below this scale possess a dispersion relation (cfr. Dell'Aquila et al. 2005) and this is in line with expectaion about baroclinic eddies in mid-latitudes. I wonder if a space-time decomposition could be provided in order to show this relation;

- ll. 189-190: is it something new? Wasn't it already found in other works on the topic of wavenumber vs. traditional transient/quasi-stationary decomposition?

- l. 222: what does "seamless" mean in this context?

- ll. 225-226: is this "analytical form" of the transport reflecting any physical mechanism?

- l. 271: I wonder if it could be possible to comment on the absence of a (even weak) polar cell in the NH;

- ll. 280-281: if the mesoscale component is negligible, why would you need to include it in the synoptic transport?

- l. 291: this seems to suggest symmetry in the location of the ITCZ, whereas we know that the ITCZ is located about 8N in the annual mean;

- ll. 295-296: is it something new, or was it already seen by performing more naive scale separations in the past?

- l. 312: given that you are discussing some hypotheses here, I think it makes sense to expand a little bit on this, rather than barely referring to a susbequent paper;

Technical corrections

- l. 20: replace "hereby" with "thereby";

- l. 66: authors could be more specific on the choice of the variables. Replace "temperature" with "air temperature" and "humidity" with "specific humidity" (?);

- Figure 2: in the caption dashed lines shall be also defined, together with solid lines;

- l. 341: "astonishing" does not seem the right term in this context. Consider changing it (maybe "surprising", "remarkable"?);

- l. 345: replace "mechanism" with "mechanisms";

- l. 353: remove the first "of" and comma before "to";