General comments

Overall the authors present a nice study about the global atmospheric energy transport, based on different scale separations. The authors argue that a wavelength-based spatial scale separation, compared to the wavenumber-based separation, is useful to better understand the atmospheric circulation and its impact on the local climate. The authors focus on an annual mean analysis, but also checked how their results vary interannually and for different seasons. The results, separated by different scales for both the northern and southern hemisphere, does include the total energy transport, as well as the moisture and dry static transport part.

In general, I think the presented arguments and discussion in this manuscript could be a bit clearer. A stronger focus could also be on the dynamical understanding of the contribution of the different spatial or temporal scales. I think this is important, as the goal of the paper is to use the somewhat new wavelength based consideration to get a better understanding of the atmospheric circulation. Maybe the authors could also make it clearer what exactly is the new contribution of their study , because this wavelength vs wavenumber consideration was also discussed in previous studies. It is not fully clear to me, if it is the application to the energy transport analysis, the more in detail and systematic approach of the comparison with more traditional scale separation or if the authors see this procedure as something fully new. In terms of getting a deeper understanding, I would also suggest to give more context and insight into the sensitivity test, e.g. the sensitivity on the choice of the wavelength scale is only done for the climatological annual mean signal as well as for the impact of the quasi-stationary contribution to the synoptic scale, but the results are then analysed for different seasons without discussing how those time and spatial scale choices might impact those results. In particular the neglect of the quasi-stationary synoptic signal should be discussed in more detail, as especially for different seasons one might expect a shift of the contributions from the planetary to the synoptic scale or the quasi-stationary to the transient scale, dependent on how the threshold was chosen. I have the feeling the reader might not get a much deeper understanding from the authors introduced wavelength based consideration, as those mentioned aspects are not really explained sufficiently and the reduced sensitivity testing might leave the reader with several open questions. E.g. in the conclusions the choice of the wavelength threshold is said to be based mainly on the intuition of the authors, which is not very convincing. Without a clearer sensitivity testing (which should also be discussed in the conclusions) it might be hard to convince the reader of this approach. So in general, I think, highlighting the different approaches is already very useful, so therefore this manuscript is already quite useful. But I think the authors should slightly improve in the presentation of their results and put more effort into highlighting what deeper understanding the reader can get from this approach. This also includes a more convincing sensitivity study to better understand the impact of the threshold choices on the differences between the scale contributions, not only for the annual mean signal but also the different seasons , because that is when I would expect the largest impact (e.g. dominant wavenumbers in midlatitudes are quite different in summer and winter). In the following I included further specific comments.

Specific comments

Introduction

line 30:
"fast-varying" means everything faster than a month here? As the authors highlight different disturbances, such as polar lows, maybe faster varying would be a better description as also much slower disturbances are part of the same "fast-varying" group.

lines 38-41:
This sentence somehow seems to suggest that the previous studies were missing an important point, i.e. the latitude dependent spatial scale of a wavenumber. However, this might not be that relevant for their studies, e.g. Röthlisberger et al. (2019) are interested in the occurrence of wave patterns in the midlatitudes. As they are not primarily interested in the meridional transport of energy and how the wave separation changes with latitude, this seems to be another question and is therefore not necessarily a disadvantage of their method. I would suggest that the authors make this point clearer, i.e. using a fixed wavenumber range might be fine for studies that look at specifics dynamics at a fixed latitude range, but it could be misleading if one does investigate the dynamics across a large range of latitudes.

It might be worth mentioning that similar studies of wave patterns in the midlatitudes, based on wavenumbers, did account for this by varying the wavenumber depending on the latitude, e.g. Wolf and Wirth (2017, Diagnosing the horizontal propagation of Rossby wave packets along the midlatitude waveguide, see their Fig. 6). So using a method based on wavenumbers, does not necessarily mean that one cannot account for this effect.

line 54:
Referring to: "However, transport at other scales could be of transient character as well." and "In this study, we are pointing out that the separation..." I would suggest the authors be more specific here and more clearly about the point they try to make or slightly reformulate this paragraph. This paragraph seems to suggest that this point (other scales can be of transient character, too) is an important new point tackled in this study. However, there are studies which explicitly highlight the point that quasi-stationary waves can be transient, as it is already part of their name (quasi-stationary, so not necessarily stationary). So I think the authors should highlight their real contribution better, namely doing this more systematically. Although many other studies also highlight this point, or modify their wavenumber based method to account for the latitude dependent spatial scale effect, this study is systematically investigating this issue in more detail.

line 61:
I would suggest to mention all following sections here, not only the data section.

Data and methods

line 71:
Maybe "In this study, we take a zonal-mean perspective of the local atmospheric energy transport, ..." to make it right away clearer to the reader that this is a local approach.

lines 71-74:
Maybe the authors can be more specific here about the differences of the two approaches, as from this description I don't really see the difference between a zonal mean or the zonal integral. Maybe I am missing an important point, but not fully obvious to me why I should expect the peaks at different latitudes.

line 76:
ylabel: length instead of lenth

line 81:
Why is this extensive smoothing necessary, so why not only before the calculation of derivatives (to get rid of possible large unrealistic gradients) but also afterwards again?

lines 86-87:
Is this formulation ("only possible from a time-mean perspective") justified? In general the quasi-stationary transport does not need to be based on monthly mean fields, so if this would be adapted then it would also be possible on smaller temporal scales. Further, the authors mention that other studies are "normally" basing this on monthly fields, which suggests that not all are following this procedure, which would not support the "only possible from" formulation.

line 91:
Only v appears in this equation, not $\mathbf{v}$, so I would suggest not to refer in the notation explanation to the full wind.

line 93:
Maybe the authors could be more explicit by what they mean when they say "in three ways that can be applied in combination". Those are three different ways to identify energy transport, so what do the authors have in mind if they talk about combining those separations?

line 94:
Replace "archived" by "achieved".

line 104:

Replace reference to “Fig. 1a” by “Fig. 1”.

lines 117-118:
Equation (7) represents the zonal mean for one particular time step, correct? Figure 2 however shows a temporal average, so I would suggest to explicitly mention this in lines 117-118 so that the reader does not get confused with the notation ($[\cdot]$ only representing zonal mean).

line 126:
Continuous separation shown in Fig. S2b, not S2a.

Wavelengths utilised for scale separation

lines 144-145:
I find it difficult to follow the conclusion for the use of a band between 2000-8000 km. The reference threshold is about 4000-4700 km, which would correspond to a wavenumber of 6-7 at about 45° latitude. According to the authors there is some variability associated with the scale of synoptic cyclones and in the fourier decomposition also neighbouring wavenumbers contribute strongly to the energy transport. But 2000 km (8000 km) at 45°N would be represented by a wavelength between 14 and 15 (3 and 4) and those larger scales could also be associated already with stationary or quasi-stationary longitudinally extended waves. Those wave patterns can still be associated with the smaller than planetary wave scales and therefore be considered as part of the smaller synoptic band, but the authors explicitly highlight the link to the much smaller synoptic cyclones. To me it is not clear from this paragraph what the authors want to have included in their synoptic band. Also from the sentence with the reference to the synoptic Rossby waves (lines 145-146) it is not clear if the authors want to have those included or if they just tolerate this to be able to capture most of the energy transport with the synoptic scales they are interested in. I suggest the authors to rephrase this paragraph to make this clearer. If the authors are indeed only interested in the smaller scales as synoptic cyclones, I think there is some more justification necessary for the choice of the upper threshold of 8000 km, because I would expect no synoptic cyclone at 45°N would be represented by a wavenumber 3 to 4.

lines 198-199:
Why is this contribution (quasi-stationary component of synoptic transport) not further investigated? The reason seems to be that it its contribution to the synoptic scale is rather small (although up to 30 %) and doesn’t really fit into the category of quasi-stationary planetary scale? Excluding this contribution seem to suggest that the introduced categories of synoptic and planetary or transient and quasi-stationary have some difficulties capturing the processes they are supposed to capture. This part therefore could also be seen as some measure of categroy uncertainty, excluding it fully seems a bit surprising.

lines 204-205:
Maybe the authors should specify here a bit more to which main results they refer, as the lines for different wavelengths are very different, e.g. in terms of the contribution from synoptic and planetary scales in midlatitudes (synoptic much stronger for 10000 km, but weaker for 6000km). So the wavelength has a huge impact on the separation between planetary and synoptic scales (qualitative different conclusions). Therefore I think the authors should give some more context here, for what results/analysis this separation does not matter.

Organisation of the global energy transport

lines 211-213:
I would suggest to avoid the reference to Fig. S6, as this figure is about the impact of using different scales. Further Fig. 4 is the relevant figure, which shows the signal for both hemispheres, so there does not seem to be any need to additionally refer to another figure. Further, is a particular reason that only the x-axis for panel d to f is scaled, but not for panel a to c? I found it initially a bit confusing when I tried to compare transport and convergence fields.

line 216:
I would exclude "almost", because the curves are similar in the sense that they have the shape. I guess almost refers to the amplitude difference, but this is explained in the following.

lines 225-226:
Do the authors really mean an inverse sine function in Fig. 4b? I find it hard to identify this curve behaviour in this plot. Further isn't there a difference between NH and SH (next sentence in lines 226-227 seems to suggest this is not the case)?

line 229:
Maybe refering to the curves as showing a plateau is a bit too much, at least for both hemispheres. Maybe this can be rephrased slightly with saying "more plateau-like" or something similar.

lines 249-250:
But isn't that what Fig. 4a is showing, that plan q-s is much stronger in NH than SH? This statement refers to previous interpretation of this manuscript or other studies (Trenberth and Stepaniak, 2003)? I don't really understand the contradiction, because the statement seems to agree with the figure. If the contradiction refers to the similar curves of the planetary signal for both hemisphere, then I also don't understand the contradiction, because individual parts of this signal (q-s and transient) do not necessarily need to have the same behaviour. I would suggest to rephrase this paragraph to make this clearer.

lines 260-262:

Why is this exactly most remarkable, or as this agrees with other studies do the authors have an explanation? As the strength of the wave guides in the NH and SH are very different, with the SH having a stronger jet, a separation by an identical time filter for identical wavenumber signals would lead on one hemisphere (SH) to the identification of a transient signal whereas on the other hemisphere (NH) as a quasi-stationary signal.

lines 295-296:

This is linked to a previous point, refering to lines 198-199. During winter there is a stronger wave guide and if considered in a power spectra spaned by wavenumber and latitude, more power at all latitudes is shifted towards smaller wavenumbers (compared to summer). This means, during summer the center of the power distribution will be located higher wavenumbers. It is therefore possible that the contribution of quasi-stationary signal is included in the synoptic scale. The authors mentioned in lines 198-199 that this part (quasi-stationary signal within synoptic scale) will not be considered as it does not represent such a large contribution. However, in summer this contribution could be larger. As the authors investigate all season (annual mean) and all seasons individually, I think they should be more specific about this point, e.g. when discussing this contribution in lines 198-199 they should already consider the seasonal differences. If this contribution would be larger in summer, their argument of not considering this contribution because of their small contribution seems more problematic. Further, the authors discuss the seasonal differences with Fig. 5 while excluding this part completely (synoptic and quasi-stationary). I would find it very interesting to see this contribution also included in those plots as it also shows the sensitivity of the analysis to the defined classes (synoptic, planetary, q-s, etc) and differences in the dynamics for the different seasons.

lines 296-297:

As mentioned in the previous point, it could also be that synoptic signals dominates in summer because the q-s is no longer mainly part of the planetary signal, but during summer part of the synoptic signal (but not considered). This somehow is strongly linked to the important point of this paper, highlighting the point that defining patterns on wavenumbers can be problematic because of the latitude dependence. The authors show convincingly the relevance of this point in great detail. But isn't it also relevant to consider the timescale of the wave patterns as function of the season, as it was for the spatial scale as function of latitude?

lines 309-310:

What do the authors mean by "are rather summing up to the total variance"? The signal is not summing up, but that is also not really expected that the variances sum up, as already stated by the authors in the previous sentence. The variability fraction also shows that the individual parts show stronger variability than the total signal, so there is the same kind of compensation between the different signals with overall smaller values. I would suggest the authors specify more in detail why this panel is so much different to the other one to better support their statement and following hypothesis.

lines 313-315:
This statement is explicitly about the Meri-part? If so, the authors should make this clear, because the statement in its general form does not seem to be supported by Fig. 6.

line 318:
Is approximately 10 % correct? It seems that all values in the extratropics exceed 10 %, with values up to about 20 %.

lines 320-323:
This is again linked to my comments about lines 198-199 and 295-296. The planetary variability is strongly linked to the variability of the q-s component. If the q-s signal is linked to the strength and/or location of the wave guide, isn't it possible that part of it fall into the synoptic part for specific years? This would then be visible in the synoptic q-s part, but this is not part of this analysis here.

Discussion and conclusion

line 332:
I think it is not a really strong and convincing argument to base the choice of length scales on the intuitive understanding. For example, with my intuitive understanding I would have chosen a slightly different range of length scales. I understand that any choice will always be subjective, because there is no truth for doing the separation, but it can be stated like this or also refered to similar length scales in other studies, I would however suggest to not base the argument on intuition.

lines 333-336:
This spatial separation is such an important feature of the presented analysis. Therefore, I think the authors should include a comment here in the conclusion about the sensitivity. I included a statement in the result section about this sensitivty as well, which might be relevant here as well. I would include this sensitivity test (Fig. S6) even in the main manuscript and discuss the identified differences. If there are no relevant differences then I would agree to keep it in the supplementary material and just say that the results are not sensitive to the exact choice of length scale. However, as included in my previous comment about this issue, I think there are relevant differences. If the authors agree on this point, I think it makes sense to include it here in the main manuscript, if the authors disagree, then I think they should make it clearer what relevant part of the results are similar for the different length scales.

line 342:
Referring to "rather narrow band". Is this really a narrow band? This range represents a wavenumber range of about 3.5 to 14.2 at 45° latitude, which doesn't seem very narrow.

line 347:
I think it should be stated somewhere in the conclusion that the q-s part of the synoptic scales is excluded. I would further suggest to include a whole paragraph to discuss this exclusion, why it was done and what possible impacts could be for the results or the sensitivity of the study. How relevant is this excluded part for the different seasons?