Response to reviewer 2.

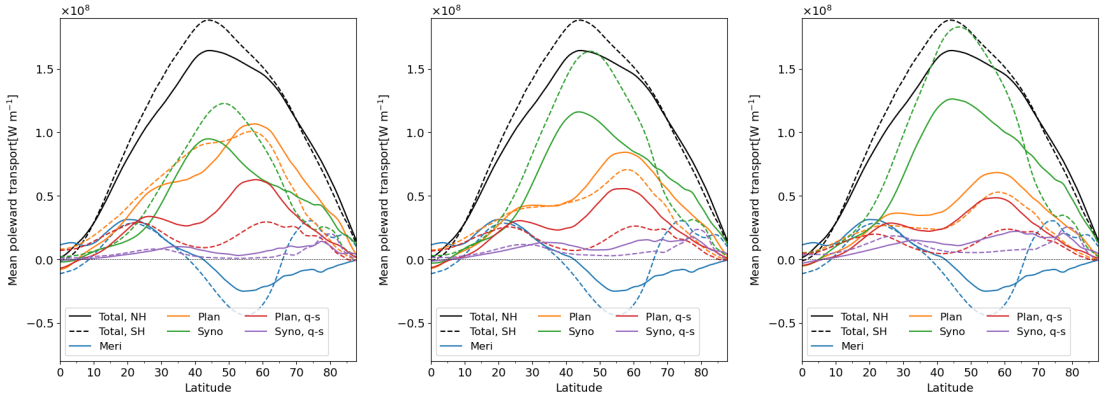
We thank the referee for the thorough and constructive review. The comments are contributing to an overall improved manuscript. Note that some formulations in the new manuscript are slightly different than here since they were changed again in a proofreading process.

Reviewer: *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.*

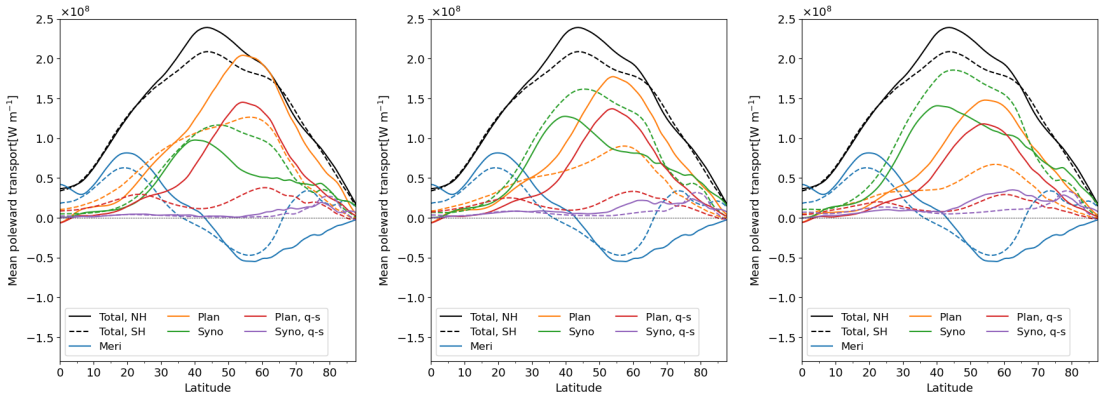
Response: We attempt to be more clear on the main new contributions of our study. We therefor reformulate some of our arguments. For example at the end of the second paragraph we changed the formulation "Here we revise the traditional separation and compare it to a partition based on the spatial scale."
to: "In this study, we combine the traditional separation of the meridional energy transport with a revised partition by spatial scales to improve our understanding of the manner the atmosphere transports energy polewards."
To connect our decomposition with the underlying physical mechanism, we add a dynamical argument for our separation captured by a new Figure 3: The here-defined synoptic energy transport at scales between 2000 - 8000 km is associated with enhanced meridional temperature gradients a few days before, hence appears to be of baroclinic origin, whereas the planetary transport appears to be little influenced by the meridional temperature gradient and some planetary waves are stronger when the temperature gradient was reduced a few days before.

Reviewer: *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.*

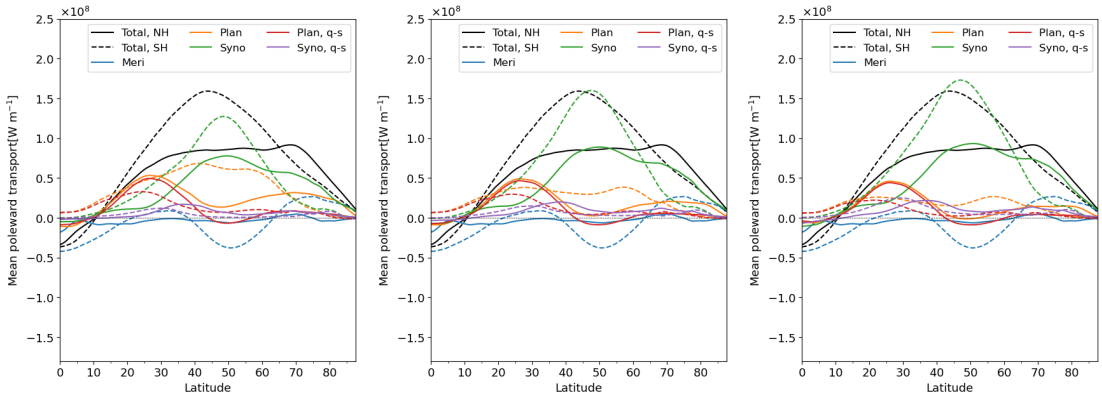
Response: Generally, the new Figure 3 provides more evidence to separate at 8000 km in order to separate between baroclinically-induced synoptic transport and planetary transport originating from other mechanisms. Hence, the sensitivity analysis is not that important. However, we provide a sensitivity analysis in Figure R1 that separates the annual-mean and seasonal-mean transport between the synoptic



(a) $\widetilde{vE}$; annual; 6000 km (b) $\widetilde{vE}$; annual; 8000 km (c) $\widetilde{vE}$; annual; 10000 km



(d) $\widetilde{vE}$; winter; 6000km (e) $\widetilde{vE}$; winter; 8000km (f) $\widetilde{vE}$; winter; 10000km



(g) $\widetilde{vE}$; summer; 6000km (h) $\widetilde{vE}$; summer; 8000km (i) $\widetilde{vE}$; summer; 10000km

Figure R1: (b) As Figure 5 of the manuscript the annual-mean energy transport by different components, but including the quasi-stationary component of the synoptic transport. (a) and (c) as (b) but separating between the synoptic and planetary transport at a wavelength of 6000 and 10000 km, respectively. (d-f) as (a-c), but for the winter-mean transport and (g-i) for summer-mean transport.

and planetary transport at the wavelength of 6000, 8000 and 10000 km. It also includes individually the quasi-stationary contribution of the synoptic transport, that is rather small in all panels and hence for simplicity not depicted as an own component in the manuscript. Note, however, that the quasi-stationary contribution of the synoptic transport is part of the synoptic transport.

Comparing the separations at different wavelengths: Clearly the strength of synoptic and planetary transport is dependent on the separation wavelength. For example, the synoptic transport is much stronger when separated at 10000 km than at 6000 km and consequently the planetary transport smaller. This is not surprising since the band of waves with wavelengths between 6000 and 10000 km transports a considerable amount of energy, as can be seen in Fig. 2. However more importantly, the form of the curves in synoptic and planetary transport is little effected by the separation wavelengths, hence the characteristics of the transport components does not depend on the threshold. Also the different transport patterns in summer and winter, where synoptic transport is approximately equally important in the former, whereas planetary transport becomes relevant in the winter, is not effected by the threshold. Hence, we are confident that our results are quite robust independent of the exact chosen separation wavelength.

Reviewer: *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.*

Response: This is a misunderstanding, since the quasi-stationary synoptic signal is not neglected in the study. It is part of the synoptic signal, just not investigated individually.

Reviewer: *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.*

Response: We provide more evidence in Figure 3 that the separation is successful to capture baroclinic versus non-baroclinic transport. We expand our discussion in Section 3 and in the Conclusions as we point out later in the specific comments.

Reviewer: *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.

Response: We present an extended sensitivity study for different seasons in Figure R1.

Specific comments

Reviewer: *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.*

Response: We agree and adapt the formulation "faster-varying".

Reviewer: *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. Rothlisberger 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.*

Response: We agree and thank for the reference. We rewrote the following formulation: "These studies separate the transport by a wavenumber which is independent of the latitude as depicted in Figure 1b. However, the wavelength associated with a given wavenumber is latitude dependent (Fig. 2). Therefore the partitioning by wavenumber, for example between wave 3 and 4 as performed in many of the previously mentioned studies, leads to convergence of all eddy transport to the planetary scale towards the poles, whereas synoptic transport may be overestimated at low latitudes (Fig. 1b). Wave 4 for instance corresponds to a wavelength of 8200 km at 35, but only to 2600 km at 75, which can be interpreted to represent different spatial scales. Accordingly, Heiskanen et al. (2020) recommend to consider the

threshold for separation between two wavenumbers with care."
to: "These studies separate the transport by a wavenumber which can be associated with a wavelength (spatial scale) for a given latitude. For investigation of the scale of the transport across a specific latitude (or a small zonal band) a fixed wavenumber for separation is appropriate. However, the wavelength associated with a given wavenumber is latitude dependent which needs to be accounted for when defining the wavenumber separating spatial scales (Heiskanen et al., 2020). Wave 4 for instance corresponds to a wavelength of 8200 km at 35°, but only to 2600 km at 75°, which can be interpreted to represent different spatial scales.
So far the energy transport across all latitudes has only been separated by a fixed wavenumber as shown in Figure 1b and presented by (Graversen and Burtu, 2016). Such a partitioning by wavenumber, for example between wave 3 and 4 as performed in many of the previously mentioned studies, has two caveats. 1) Towards the poles all eddy transport convergence to the planetary scale (Fig. 1b). 2) In the subtropics, wavenumbers 1 - 3 appear to miss parts of planetary transport, as can be inferred from quasi-stationary eddies in the subtropical SH (Fig. 1a), being considerably larger than the planetary transport captured by wavenumbers 1 - 3 (Fig. 1b)."

We refer to the study of Wolf and Wirth (2017) in terms of a latitude-dependent separation by wavelength in the method section. However, this was not applied on the energy transport.

Reviewer: *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.*

Response: In the here-applied "traditional" decomposition by Oort and Peixóto (1983), the energy transport by monthly-mean eddies is referred to as quasi-stationary, whereas transport anomalies from the monthly mean are transient. This is formulated in L29 - 30 (old version): "Traditionally, the eddy transport is separated into a quasi-stationary and a transient component (Fig. 1a), with the former representing monthly-mean eddies and the latter faster-varying deviations from this mean (Oort and Peixóto, 1983)." Following the definition of the traditional decomposition, quasi-stationary eddies may vary from month to month, are however strictly distinct from transient eddies.
Here, the new contribution is the wavelength-based decomposition which we compare to the traditional decomposition. We argue that the new wavelength-based

decomposition is more generally applicable to all latitudes than the wavenumber-based decomposition.
We would be interested to know which studies are with "Although many other studies also highlight this point, or modify their wavenumber based method to account for the latitude dependent spatial scale effect".

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

Response: We added a short overview of the manuscript: "These questions are targeted in Sections 3 and 4. The main results are then summarised and discussed in Section 5. However, first the utilised data and methods are presented."

Reviewer: *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.*

Response: Good suggestion. We rearranged the paragraph and changed the sentence: "Hence, to compare the local importance of the atmospheric energy transport across all latitudes, we take a zonal-mean perspective which provides the transport through an atmospheric column with one metre width. "

Reviewer: *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.*

Response: The differences is simple, the zonal integral is the zonal mean multiplied by the longitude circle. We try to make this more clear by changing the formulation from: "In this study, we take a zonal-mean perspective of the atmospheric energy transport, which provides the transport through an atmospheric column with one metre width. Hereby, it provides a local measure of the transport, and differs from other studies that zonally integrate the transport along each longitude circle (Graversen and Burtu, 2016; Peixoto and Oort, 1992; Trenberth and Caron, 2001). However, the computed zonal integral of the energy transport from ERA5 (Fig. S1a) confirms the transport in these studies. For instance, the zonal-integrated poleward transport peaks at 4.8×10^{15} W in the NH and 5.6×10^{15} W in the SH at 41° latitude in both hemispheres. The latitude of maximum zonal-mean transport is slightly higher at 45°(Fig. S1b). Further, the average transport in the polar regions is more easily assessed by the zonal-mean transport as it is not influenced by converging latitudes."

To: "The zonal integral of the energy transport from ERA5 (Fig. S1a) confirms the transport in found in previous studies (Graversen and Burtu, 2016; Peixoto and Oort, 1992; Trenberth and Caron, 2001). For instance, the zonal-integrated poleward transport peaks at 4.8×10^{15} W in the NH and 5.6×10^{15} W in the SH at

41° latitude in both hemispheres. By computing the zonal integral of the energy transport, which depends on the length of the longitude circle, the transport becomes small at high latitudes since the longitudes converge (Fig. S1a). However, the local transport, expressed by the zonal mean, is considerable also in the polar regions (Fig. S1b). Hence, to compare the local importance of the atmospheric energy transport across all latitudes, we take a zonal-mean perspective which provides the transport through an atmospheric column with one metre width. Hereby, for example the latitude of maximum zonal-mean transport is at 45° latitude (Fig. S1b)."

Reviewer: *line 76: ylabel: length instead of lenth*

Response: Thanks for spotting mistake. We changed the y-labels to "Mean transport".

Reviewer: *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?*

Response: Indeed the smoothing is not really necessary and we remove everywhere beside before the computation of the derivatives for the convergence to reduce the noise.

Reviewer: *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.*

Response: To solve the mentioned issues and to be more precise, we change the formulation from: "This comparison is only possible from a time-mean perspective, since the quasi-stationary transport is normally derived based on monthly-mean fields (Oort and Peixóto, 1983)."
To: "This comparison is only possible from a time-mean perspective, since the computation of quasi-stationary eddy transport requires a predefined time period over which the eddies are considered stationary, which is traditionally set to be one month (Oort and Peixóto, 1983)."

Reviewer: *line 91: Only v appears in this equation, not v, so I would suggest not to refer in the notation explanation to the full wind.*

Response: We change the notion.

Reviewer: *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?

Response: We agree that the formulation was not very clear and hence change it: "We decompose the total atmospheric energy transport, $\widetilde{vE}$, in three ways that can be applied in succession:"
In the following, we reformulate that section in order to explain how the decomposition is performed in succession.

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

Response: Thanks for spotting mistake.

Reviewer: *line 104: Replace reference to "Fig. 1a" by "Fig. 1".*

Response: It is intended to refer to Figure 1a which displays the traditional decomposition.

Reviewer: *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).*

Response: That is correct. We hence add some explanation to the method section: "This partition is applied to the instantaneous co-variability, vE in Equation 1, resulting in the wave-separated total transport, as well as to the transport by monthly-mean fields, $\overline{v}\overline{E}$, comprising term 2 and 3 of Equation 3, resulting in the wave-separated quasi-stationary transport, $\widetilde{vE}^{q-s}$. The annual-mean transport by each quasi-stationary wave ($[\overline{v}\overline{E}]_n$ in Eq. 7) as function of latitude is displayed in Figure 2a). The wave-separated transient transport, $\widetilde{vE}^{tran}$, (Fig. 2b) is derived by subtracting the quasi-stationary from the total transport, computed from the instantaneous co-variability. "

Reviewer: *line 126: Continuous separation shown in Fig. S2b, not S2a.*
Response: Indeed, we changed the order in the manuscript.

Reviewer: *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.*

Response: We show in Figure 2 that little transport of eddies at scales smaller than 8000km is of quasi-stationary character.

Reviewer: *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.*

Response: For clarification: At 45° our separation assigns wavenumber 3 and more than half of wavenumber 4 to the planetary scale.
As suggested, we rephrase the paragraph: "The synoptic scale is supposed to include most energy transport associated with eddies developing by baroclinic instability (Holton and Hakim, 2013; Vallis, 2017). The synoptic eddies are perceived as cyclones and anticyclones in the sea-level pressure that are interacting vertically with an upper-tropospheric oscillation of the jet stream, often associated with transient synoptic Rossby waves (e.g. Ali et al., 2021; Röthlisberger et al., 2019). The theoretical scale (wavelength) of baroclinic eddies is given by 3.9 times the Rossby deformation radius, L_d, and hence estimated to be 4000 km by (Vallis, 2017, p.354) and 4,800 km by Stoll et al. (2021). Note, that a low (high) pressure system spans half a wavelength, and has accordingly a typical size of around 2000 km.
A wavelength band between 2000 - 8000 km appears appropriate to capture the majority of the transport associated with baroclinically-induced synoptic eddies for two reasons: (i) Synoptic eddies, such as cyclones and anticyclones, feature some variability in their size, but with a typical diameter between 1000 and 4000 km. (ii) The non-local Fourier decomposition of the energy transport in situations of localised synoptic cyclones captures considerable amount at neighbouring waves to the cyclone (Heiskanen et al., 2020, Fig. 3)"

Further, we add another analysis that demonstrates the chosen threshold at 8000km to separate between baroclinically-induced energy transport and transport created differently.

Reviewer: *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 category uncertainty, excluding it fully seems a bit surprising.*

Response: The quasi-stationary component of synoptic transport is still included in the synoptic transport, just not investigated separately since it is small (see Fig. 4 and Fig. R1). We think our formulation and reasoning was a bit unclear and reformulated the paragraph. It now includes a reason for not further investigating the quasi-stationary synoptic transport. Our method of scale separation is different from the "traditional separation", if both would agree 100%, our method would be redundant. So, we do not share the interpretation that this is "category uncertainty".
Old paragraph: "..., the synoptic transport is mainly (70 - 100%) of transient nature at all latitudes, which coincides with the transient character of synoptic cyclones and Rossby waves of short wave length. Hence in the following, the quasi-stationary component of synoptic transport is not further investigated. "

New paragraph: "..., the synoptic transport is mainly (70 - 100%) of transient nature at all latitudes, which coincides with the transient character of synoptic cyclones and Rossby waves of short wave length. The small quasi-stationary contribution (0 - 30%) to the synoptic transport is attributed to preferred spatial locations for synoptic activity. For instance, the NH Atlantic sector features more cyclonic activity than other longitudes, which in the time-mean reveals as increased quasi-stationary transport. This can be inferred as Rydsaa et al. (2021) show a large time-mean synoptic transport in the Atlantic sector for strong latent transport events in winter at 70° N. However, in a zonal-mean perspective the quasi-stationary contribution to the synoptic transport is small ($< 30\%$) compared to its transient part, and hence for the sake of simplicity the synoptic transport is not separated into a transient and quasi-stationary contribution in the remainder of this study.

Reviewer: *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 mid-latitudes (synoptic much stronger for 10000km, 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.*

Response: The supplement includes a short discussion of the difference mentioned by the reviewer and the similarities that we refer to in the manuscript:
Supplement (slightly rewritten): "In order to test the sensitivity of the scale separated energy transport for different values of the wavelength used for separation, the latter is varied (Fig. S6). Clearly, more (less) transport is associated with the synoptic scale when separation wavelength is increased (decreased). This is simply a result from the wavelength band between 6,000 and 10,000 km comprising a considerable amount of the energy transport (see also Fig. S3). Hence, the strength of the synoptic as compared to the planetary component is influenced by varying the separation wavelength. However, the important features of planetary and synoptic waves are similar, such as the maximum in the synoptic transport around

45°latitude, the maximum of the planetary around 60°latitude, almost symmetrical structures in both hemispheres, and similar seasonal behaviour (not shown)."

In the manuscript, we add a short reference to the supplement by the last part of the following sentence: "The main results of this study are not affected by the exact choice of the separation wavelength which is shortly discussed in the Supplement."

Reviewer: *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.*

Response: First part: We agree that it is better omit the reference to Figure S6.
Second part: We understand that the different x-axis scales can confuse the comparison of the panels. The reason is that in a to c, we attempt to show the transport at each latitude which we consider best visible by a linear x-axis. In d to f, we scale the axis "such that the integrated convergence in each component becomes zero" (which we add to the legend of the figure). This way it appears more intuitive that the energy in each component is redistributed. We would be interested if you have a opinion on how to best combine these competing considerations.

Reviewer: *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.*

Response: We agree and removed "almost".

Reviewer: *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)?*

Response: Indeed the inverse sine function is a bit difficult to see when both hemispheres are plotted together. Hence, we reformulated the paragraph and now also discuss the differences between the hemispheres.

Old: "To a first order, the annual-mean moisture transport, $\widetilde{vQ}$, resembles the inverse of a sine curve in each hemisphere with an exponentially decaying tail towards the poles (Fig. 5b). Hence, moisture transport in the tropics is equatorward and poleward in the subtropics and extra-tropics with a maximum around 40° latitude. This leads to moisture divergence in the non-equatorial tropics and sub-tropics and convergence in the equatorial regions and extra-tropics (Fig. 5e)."

New: "The total annual-mean moisture transport, $\widetilde{vQ}$, of both hemispheres fea-

tures equatorward extremes at around 10°(Fig. 5b), a poleward maxima at around 40°and decaying tails towards the poles. This leads to moisture divergence in the non-equatorial tropics and sub-tropics, whereas moisture convergences in the equatorial regions and extra-tropics (Fig. 5e). The moisture transport is mainly stronger in the Southern than Northern Hemisphere, likely due to more evaporation on water surfaces of the Southern Hemisphere. Further, some moisture is transported from the SH across the equator leading to the highest convergence of moisture at around 7° N, which is in the annual mean the approximate location of the intertropical convergence zone (ITCZ)."

Reviewer: *line 229: Maybe referring 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.*

Response: As suggested, we reformulate "features a plateau" to "is plateau-like".

Reviewer: *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.*

Response: We try to make our point more clear and reformulate the paragraph. Old: "The planetary energy transport is almost similar in both hemispheres, different from the quasi-stationary transport that is mainly relevant in the NH (Fig. 5a). The planetary transport is similar in the subtropics and low mid-latitudes and only approximately 20% weaker in the higher mid-latitudes of the SH than the NH. This is in contrast to the previous interpretation that planetary transport being represented by the quasi-stationary component is mainly relevant in the NH (e.g. Trenberth and Stepaniak, 2003). This is the case since the planetary transport has a highly relevant transient component in the SH (Fig. 4)."

New: "The planetary energy transport is similar in both hemispheres, different from quasi-stationary transport which is mainly relevant in the NH (Fig. 1a, 5a). The latter is in agreement with Trenberth and Stepaniak (2003) pointing that quasi-stationary transport is a primary factor in the extratropical NH. They associate this quasi-stationary transport to the planetary scale, which they do not prove but which is confirmed by this study (Fig. 4). A new finding, that could partly be inferred from Fig. S3 of Lembo et al. (2019), is the almost symmetry of the planetary energy transport in both hemispheres. This symmetry could not been anticipated by the consideration of quasi-stationary transport since the planetary transport in the SH is mainly of transient character (Fig. 4), in agreement with Mo (1986).
In both hemispheres, the planetary transport is similar in the subtropics and low

mid-latitudes and only approximately 20% weaker in the higher mid-latitudes and polar region of the SH than the NH. Hence, eddies at similar spatial scales are transporting the energy in both hemispheres (see also Fig. 2), which is likely due to similar physical mechanisms in both hemispheres forming the energy-transporting eddies."

Reviewer: *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.*

Response: We agree with the reviewer and our formulation was a bit imprecise. Hence, we changed from: "The most remarkable difference between the hemispheres is that planetary waves are transient in the extra-tropical SH, whereas often quasi-stationary in its northern counterpart, which agrees with Peixoto and Oort (1992)."

To: "These planetary waves are mainly transient (Fig. 4: 70%) in the mid-latitudinal SH, whereas more often (60%) quasi-stationary in its northern counterpart, which agrees with Peixoto and Oort (1992). However, in the high latitudes of the SH, considerable amount of the planetary transport is quasi-stationary."

Reviewer: *lines 295-296: This is linked to a previous point, referring 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.*

Response: See Fig. R1, the quasi-stationary contribution to the synoptic transport is small in summer.

Reviewer: *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.*

Response: It appears that most of this is due to a misunderstanding that we hope is partly resolved by the response to lines 198-199. The synoptic transport includes both a transient and quasi-stationary component, just the separation is not presented.

Reviewer: *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).*

Response: As mentioned earlier, the quasi-stationary synoptic transport is included in the synoptic transport, just not presented individually (in the manuscript. However, it is shown in Fig. R1. Since it is small in all seasons, it is not considered to be of major interest.

Reviewer: *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?*

Response: It would be indeed relevant to investigate the timescale of planetary and synoptic transport events. As work of a follow-up study, we find that planetary events feature a mean lifetime of around a week (somewhat shorter in the SH), whereas synoptic events last for around 3 days.

Reviewer: *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.*

Response: We rewrite, from: "In contrast to the total transport, the variability of the moisture transport components, $\widetilde{vQ}$, are rather summing up to the total variance (Fig. 7b)."
To: "In contrast to the total energy transport, the variability of the total moisture transport, $\widetilde{vQ}$, is larger than the variability of its individual scale components (Fig. 7b). Hence, the moisture transport components are not compensating each other in the same manner as the total energy transport components. Instead, the compensation of the components is in form of the dry energy (Fig. 7c)."

Reviewer: *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.

Response: We add a sentence for clarification: "The large total variability in the moist and dry energy transport is almost entirely due to variability in the meridional components, which is not surprising since the meridional components are responsible for most of the moist and dry energy transport in the tropics (Fig. 5b,c, 6b,c,e,f)."

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

Response: It is the orange line in Figure 7a. We rewrite the sentence: "In the extratropics, the planetary transport, $\widetilde{vE}_{plan}$, exhibits the largest inter-annual variability and varies by approximately 10% in the mid-latitudes and 15 - 20% in the polar regions (Fig. 7). "

Reviewer: *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.*

Response: As explained before, the q-s synoptic part is included in the synoptic part.

Reviewer: *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.*

Response: We expand the discussion on this topic: "We demonstrate that a separation between synoptic and planetary eddies at a wavelength of 8000 km is physically useful since it distinguishes between waves preceded by enhanced and reduced meridional temperature gradients. Hence, that synoptic eddies at wavelengths smaller than 8000 km, are mainly baroclinically induced, whereas different physical mechanisms are at work for larger eddies. The same wavelength is also in approximate agreement with the traditional separation between transient and quasi-stationary eddies, as most wave transport at wavelengths smaller than 8000 km is of transient character, whereas most quasi-stationary transport occurs at the planetary scale larger than 8000 km. Despite the latter, considerable planetary energy transport is of transient character, especially in the extratropical SH.

The separation between synoptic and planetary eddies at a wavelength of 8000 km appears large. However, most baroclinically-induced and transient energy transport organises at a wavelength around 5000 km at all latitudes (Fig. 2, 3), well in agreement with the predicted length by dry-baroclinic theory (Vallis, 2017). However, the baroclinically-induced and transient energy transport occurs in a wavelength band approximately between 2000 and 8000 km, hence separating at around 5000 km would be misleading. It should further be noted that one synoptic wave includes both a low and a high pressure systems, hence that synoptic cyclones and anticyclones feature a typical diameter of between 1000 and 4000 km, or that the typical distance between two independent (anti)cyclones is between 2000 and 8000 km. This appears appropriate from comparison with weather maps."

Reviewer: *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 sensitivity 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.*

Response: See the second response that presents the Figure R1.

Reviewer: *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.*

Response: Indeed, the narrow appears misplaced and we removed it. Hence the sentence was changed from: "It is astonishing that despite all possible eddies, waves at scales in the rather narrow band between 2,000 and 8,000 km are responsible for the majority of the meridional energy transport for the whole extra-tropics."
To: "It is remarkable that in the large range of atmospheric eddies, those at scales in the band between 2,000 and 8,000 km are responsible for the majority of the meridional energy transport for the whole extra-tropics."

Reviewer: *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?*

Response: As stated earlier, this is a misunderstanding.

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