Response to reviewer 1.

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.

General response: *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.

Response: We thank for the overall positive feedback.

Reviewer: *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.*

Response: In the revised manuscript, we convey more clearly that the synoptic scale transport is largely associated to baroclinic eddies. Also in other aspects, we rewrite the manuscript in order to be more clear on advantages of the novel methodology.

Reviewer: *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.*

Response: If the reviewer has references showing that baroclinc instability is carrying out the meridional energy transport we are interested to include them. We agree that baroclinic instability is responsible to form synoptic scale eddies in a manner to transport energy. However, also planetary eddies transport energy in the mid-latitudes and these appear not to be of direct baroclinic origin (see new Figure 3). Hence, we are not aware of a quantification of how much of the meridional energy transport in the mid-latitudes can be associated directly with baroclinic instability. Our separation now provides an estimate of such an quantification.

Reviewer: *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).*
Response: First of all, it would be helpful if the reviewer could specifically refer to the literature in order to prevent misunderstandings. Three papers that we associate with the mentioned ones, however do not appear to discuss "the overlapping notions of quasi-stationary waves and Rossby waves". Surely, these interesting works investigate wave activity, hence are related to the topic of our manuscript, do, however, not focus on the main topic of our manuscript being the organisation of the atmospheric energy transport. Hence, we would ask the reviewer to specify in which respect our manuscript requires to refer to these works.

Specific comments

Reviewer: *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;*

Response: To 1: We add considerable discussion along the meridional Fourier decomposition:
"The Fourier decomposition is non-local, hence the whole circle influences the obtained eddies. This makes the Fourier decomposition useful if the circle is governed by similar eddies, which we observe from meteorological weather maps within the different climate zones separated by latitudes. Theoretically a Fourier decomposition could be performed along longitude circles, e.g. along 0 and 180°. However, a circle going around both poles and crossing the equator twice features eddies of all climate zones, so it is questionable if we would gain useful understanding from a meridional Fourier decomposition.
However, arguably the zonal scale is connected to the meridional scale of eddies,

becoming their general scale. From investigation of meteorological weather maps, we know (i) that synoptic-scale cyclones have an approximate similar zonal and meridional size since they are to first order circular, and (ii) that the meridional extend, i.e. the amplitude, of planetary Rossby waves, appears to roughly agree with the distance between a trough and a ridge, featuring half a zonal wavelength. Further, we show later (e.g. Fig. 2) that most of the mid-latitude transport occurs at zonal wavelength between 2000 and 8000 km, which is in broad agreement that events of extreme transport in the mid-latitudes are mainly coherent between 10 and 30° latitude (Lembo et al., 2019, Fig. 1d-g), considering that the event, such as a cyclone, has the size of half a zonal wavelength."
To 2: We emphasize two aspects that are misleading with a "steady" wavenumber-based partitioning: "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)."

Reviewer: *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.*

Response: Indeed, we use the wavelength to separate the spatial scales, so the terms are tightly connected. We state this more clearly by writing in line 39ff: "These studies separate the transport by a zonal wavenumber which can be associated with a zonal wavelength for a given latitude. As many of the previously mentioned studies, we interpret the zonal wavelength of the eddies as their spatial scale."

Reviewer: *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;*

Response: First of all this is just a hypothesis and we are not at all sure it is correct and do not have any prove. However, linear theory describing the growth of baroclinic eddies surprisingly well, is only valid in the initial baroclinic phase, as non-linear terms become large afterwards. Still, the linear theory well predicts the structure and scale of the evolving baroclinic eddies, hence we think that the initial phase is relevant for setting the size.

We do not understand where the reviewer sees a contradiction in our here formulated hypothesis that cyclones are propagating from the mid-latitudes to higher latitudes in using the wavelength to identify the synoptic eddy transport at different latitudes. If the reviewer still sees a contradiction, we would be glad if (s)he could explain it.

However, we change the manuscript with the intention to make our argumentation more convincing. From: "It may appear surprising that the scale of maximum transient energy transport, $\widetilde{vE}^{tran}$, is independent of the latitude, since the deformation radius estimating the size of baroclinic eddies depends inversely on the Coriolis parameter, and depends linearly on the layer depth which decreases with latitude (Vallis, 2017). However, these are parameters important for the cyclogenesis which is mostly active in a confined region: Most cyclones originate from the mid-latitudes, where the horizontal temperature contrast is largest. The size of a cyclone is set during the genesis stage when the fastest-growing mode is prevailing. Many cyclones propagate to higher latitudes along the diagonal axis of the storm tracks (Shaw et al., 2016) and may keep their size."
Changed to: "It is surprising that the scale of maximum baroclinically-induced transport anomaly is independent of the latitude, since the Rossby deformation radius, $L_d = \frac{NH}{f}$, estimating the size of baroclinic eddies (Vallis, 2017), depends inversely on the Coriolis parameter, f, increasing with latitude, and depends linearly on the depth of the troposphere, H, and the tropospheric static stability, N, which mainly decrease with latitude. Hence, baroclinic eddies would be expected to be smaller at higher latitudes. A hypothesis for the latitude-independence of the baroclinic eddies is as follows: Most cyclones originate from the mid-latitudes, where the meridional temperature contrast is largest. The size of a cyclone is set during the genesis stage when the fastest-growing mode is prevailing. Many cyclones propagate to higher latitudes along the diagonal axis of the storm tracks (Shaw et al., 2016) and may keep their size."

Reviewer: *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?*

Response: Indeed, planetary and quasi-stationary transport (as well as synoptic and transient) are partly overlapping, however, not similar as we demonstrate in Section 3.3.

Reviewer: *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;*

Response: Indeed, Lembo et al. (2019) is Fig. S3 compares the transport of both hemispheres and similarities can be recognised that are in agreement with our findings. We adapted the paragraph to include this and how it hints towards similar mechanisms in both hemispheres:

"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, that 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). 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. 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 leading to the energy transport."

Reviewer: *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;*

Response: Thanks for the positive perspective. Surely, it has been shown that baroclinic instability is responsible for a large amount of the eddy activity, however, we are not aware of studies that quantify the meridional energy transport of baroclinic eddies. We considerably rewrote the conclusions.

Minor comments

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

Response: As advised, we remove the sentence and replace it by "The atmosphere transports energy polewards in form of circulation cells and eddies."

Reviewer: *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;*

Response: We disagree that the atmosphere is set in motion by rotation and angular momentum convergence as well. These mechanisms clearly influence the motion of the atmosphere by, most importantly, the Coriolis force. However, the Coriolis force does not work at rest, so it can not set anything in motion. Would the atmosphere would be in rest, the largest term in the momentum equation is the pressure gradient force. The pressure gradient is set up by differential solar heating. Would our planet not receive differential heating, the first order terms in the momentum equation vanish, then lower order terms become relevant, such as the centrifugal force, and the motion in the atmosphere would arguably be quite different.

Reviewer: *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;*

Response: We slightly changed the formulation: "In the tropics and sub-tropics, where the Coriolis effect is small, energy is predominantly transported by a zonally-symmetric meridional overturning circulation, known as Hadley cell (Hadley, 1735), and monsoon systems, organised by quasi-stationary cells (Fig. 1a)." The reader should be able to identify the meridional circulation in the figure since it is denoted in the legend.

Reviewer: *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 synonym of "zonal wavelength", but the extent to which the interoperability of the two terms can be used is not clear to me;*

Response: At this point in the manuscript, we are still rather general and do not provide the "definition" of scale. However, in the following paragraph, we become more specific on our interpretation by adding the last sentence: "The scale separation of the meridional energy transport by a zonal Fourier decomposition became popular in recent years as it was applied to study the effect of energy transport for the Arctic (Graversen et al., 2021; Hofsteenge et al., 2022; Papritz and Dunn-Sigouin, 2020; Rydsaa et al., 2021), and for the mid-latitudes of the Northern Hemisphere (NH) (Lembo et al., 2019). These studies separate the transport by a zonal wavenumber which can be associated with a zonal wavelength for a given latitude. As many of the previously mentioned studies, we interpret the zonal wavelength of the eddies as their spatial scale."

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

Response: A we added short outline 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: *ll. 65-66: the authors do not need to refer to ERA-Interim;*

Response: We removed that part of the sentence.

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

Response: We guess the reviewer mean ll. 71-72 and was not sure how we mean by the zonal-mean perspective. Hence we try to make the difference between the zonal integral and the zonal mean 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: *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;*

Response: Indeed the longitudes converge. Thanks for spotting the mistake.

Reviewer: *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.*

Response: We add a sentence to the discussion that mentions the usage of the latter technique: "An option for investigating the spatio-temporal scale of eddies is the usage of Hayashi spectra as in Dell'Aquila et al. (2005) that performs a Fourier decomposition in both space and time." If the reviewer can point us to studies performing as 2D wavelet decomposition of the atmospheric dynamics, we would include them as well.

Reviewer: *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.*

Response: To 1: We changed the definition of the wind vector to "where v is the meridional wind".
To 2:
Reviewer: *l. 95: this dry component is not the dry static energy (DSE), or is it? It should not include a kinetic energy term;*

Response: Indeed, the dry-static energy from the first version of the manuscript also includes the kinetic energy, strictly it is the dry energy without "static". The kinetic component is some orders of magnitude smaller than the other two (Peixoto and Oort, 1992), hence both are essentially similar. However, for being precise we remove the "static" in the manuscript.

Reviewer: *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;*

Response: We introduced a sentence: "Note, that the cross terms a_n^E and a_m^v with $n \neq m$, and similarly b_n^E and b_m^v, vanish since the Fourier components feature an orthogonal basis."

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

Response: The discussion of the chosen wavelengths is dedicated the entire Section 3, so it would be to long to insert it here. The method is in general independent from the chosen wavelengths, we only mention it to improve the interpretability for the reader.

Reviewer: *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;*

Response: We do now mention it: "The theoretical scale (wavelength) of baroclinic eddies is given by 3.9 times the Rossby deformation radius, and hence estimated to be 4,000 km by (Vallis, 2017, p.354) and 4,800 km by Stoll et al. (2021)."

Reviewer: *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 expectation about baroclinic eddies in mid-latitudes. I wonder if a space-time decomposition could be provided in order to show this relation;*

Response: These are interesting thoughts. We include a comparison of our results to that study: "The spectral decomposition of the annual-mean energy transport, $\widetilde{vE}$, at different latitudes reveals that most eddies smaller than 8000 km are of transient nature, whereas most of the quasi-stationary transport is at scales larger than 8000 km (Fig. 2). This is in good agreement with Dell'Aquila et al. (2005) finding in the average of the extra-tropical NH, that most standing eddies occur at zonal wavenumbers 3 - 5 and that propagating eddies at wavenumber 3 feature a typical time period of around a months whereas small eddies, here associated to the synoptic scale, are characterised by weekly and daily periods." However, a space-time decomposition is outside the scope of the current study.

Reviewer: *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?*

Response: This finding may not be completely new, however, we are not aware of studies comparing the traditional with the scale separation of atmospheric energy transport. If the reviewer is aware of a study that compares the different composition methods, we would gladly refer to it here.
We outline the problematic that both are sometimes considered similar in the Introduction: "... quasi-stationary transport is often associated with the planetary scale, which appears to imply that planetary transport is irrelevant in the SH (e.g. Trenberth and Stepaniak, 2003a). In contrast, transport by transient eddies is often associated with baroclinic eddies at the synoptic scale (e.g. Trenberth and Stepaniak, 2003a). However, transport at other scales could be of transient character as well."

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

Response: We mention our interpretation of "seamless" a few sentences before, which is a term utilised in the cited study: "The annual-mean, zonal-mean poleward energy transport, vE, for both hemispheres (black lines in Fig. 4a) as noted by Trenberth and Stepaniak (2003b)."

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

Response: We remove the analytical form which is somewhat difficult to recognise in the perspective of plotting both hemispheres together as done in Figure 5. Indeed, the maximum and minimum in the poleward moisture transport are caused by different mechanisms leading to the moisture transport, which is long known and which we explain in the remainder of the section.

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

Response: We add two sentences on the topic: "Different to previous studies by for example Peixoto and Oort (1992) a NH polar cell is not evident in the here-utilised ERA-5 dataset, neither in the annual-mean nor in the summer or winter season (Sec. 4.2). In the Arctic, energy transport is dominated by eddies, whereas zonal symmetric katabatic flows, as observed in the Antarctic, do not develop due to the lack of a large ice dome centred over the pole."

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

Response: We add the last part of the sentence to the manuscript to answer the questions: "Due to its negligible role, we include the mesoscale into the synoptic transport for the remainder of this study, such that the sum of all components yield the total transport"

Reviewer: *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;*

Response: Studies like the below cited indeed indicate approximately a symmetry in the ITCZ. However, we changed the text a bit: "The location separating northward and southward total transport, the energy flux equator (Adam et al., 2016), is at around 10°latitude in the summer hemisphere (Fig. 6d). This is linked to the zonal-mean ITCZ, associated with the ascending branch of the Hadley circulation (Adam et al., 2016)." Surely their are different definitions of the zonal-mean ITCZ so it is a bit challenging. If the reviewer disagrees with our interpretation, we would be glad if she or he could point towards some literature.

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

Response: Thanks for the remark. Indeed it could be recognised in the wavenumber-based scale separation as well. Hence, we add a subsentence: "... in broad agreement with results from wavenumber separated transport of the NH mid-latitudes in Lembo et al. (2019) ..."

Reviewer: *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 subsequent paper;*

Response: We expand a bit with a short argumentation: "A subsequent study points towards that the annual-mean energy transport is induced by the meridional energy gradient in the manner of a diffusion process with a globally almost constant diffusion coefficient, hence larger transport in one component reduces the temperature gradient, leading to less transport in another component. Differently, moisture is a tracer of the atmospheric circulation and therefor not described by a diffusion process such that the components do not compensate in a similar manner as for the energy transport. "

Technical corrections

Reviewer: *l. 20: replace "hereby" with "thereby";*

Response: Thanks

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

Response: Done as advised.

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

Response: The dashed lines are replaced by solid lines. Further we replaced: "The wavenumbers corresponding to some wavelengths are presented by black curves. The solid curves at 2,000 and 8,000 km denote the separation between meso, synoptic and planetary scale."
by: "The wavenumbers corresponding to wavelengths of 2000, 4000, 6000 and 8000 km are depicted by black curves. The first and last of the curves separate the transport into the meso, synoptic and planetary scale."

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

Response: Thanks for the suggestion, we replaced it by remarkable and slightly changed the formulation of the sentence after suggestion of reviewer 2.

Reviewer: *l. 345: replace "mechanism" with "mechanisms";*

Response: Thanks.

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

Response: Thanks.

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