

General comment

The authors did a very thorough job in answering all my raised points and they modified their manuscript according to those suggestions, giving more explanations where I thought the original manuscript could be a bit clearer and they improved on highlighting their contributions to the research, which I find did help to improve the manuscript. Therefore I do not really have many comments left. Small minor points are given below under the “specific comments” section.

Specific comments

lines 49-52:

I would combine this paragraph with the following, as here the authors introduce the consideration of the meridional length scales with their short conclusion that a fourier transform along longitude circles does not make much sense. I find this paragraph a bit confusing, why introducing this new aspect when directly concluding that it does not make sense? At least in the next paragraph the authors present their explanations why investigating the meridional scale is not really necessary as it is linked to the zonal scale. I would suggest to combine this into one paragraph and maybe introducing it that the authors focus on the zonal scale of the investigated features. After this they can give their reasoning why further looking explicitly at the meridional scale is not necessary and hence, specify that in the following spatial scale always refers to the associated zonal scale. If considering the meridional scale individually is not necessary, then I would also exclude the part about the fourier transform along longitude circles as it does not contribute to their arguments.

line 116:

I would suggest to replace “normal” circulation by another description, because it is not clear what normal really means. The authors talk about a temporal averaged circulation to get rid of the large noisy day to day variability, so maybe it could be referred to as temporal averaged or averaged instead of normal.

lines 117-119:

This refers to one of my previous comments in the previous revision process, not seeing quasi-stationary signals necessarily based on time-mean fields. The authors refer to the monthly mean averages as the traditional method to identify the quasi-stationary signal and they also include a reference. So I guess this is the justification to refer to this part as the quasi-stationary signal and use this terminology with the explained meaning in their manuscript.

However, a quasi-stationary signal could also be identified for example by temporal low pass filter, excluding the faster transients and only keeping the slowly moving signals as the quasi-stationary part, there is no real reason why one should not do it like this. Depending on the exact separation between quasi-stationary and faster transients for

the time filtering, a monthly mean field and the associated monthly averaged temporal filtered field of the wave amplitudes could be different, because it is a temporal mean of a signal that is not necessarily stationary. Due to this, I am not sure I agree with the author's description that "This comparison is only possible from a time-mean perspective... quasi-stationary eddy transport requires a predefined time period over which the eddies are considered stationary". This conclusion is based on their definition of a quasi-stationary signal, which is based on using a time-mean, but from this perspective it is by definition "only possible from a time-mean perspective", but that is a consequence of their method. So the presented results can only be shown based on a time-mean perspective (due to the definition of the method), but it is not necessarily true that investigating the quasi-stationary signal is only possible based on a time-mean perspective.

line 162:

reference to the equation number not working (??).

line 256:

wavelength instead of wavenumber