I have copied the major comments of the reviewer in black, along with my responses in blue.

Major comments:

a) Abstract: the notion of waveguide depth is obscure there. When I read the abstract I thought it was considering the depth in altitude. It would be good to add words to qualitatively define the waveguide depth. For me, the waveguide depth is closely related to the number of zonal wavenumbers for which the waveguide exists.

Thank you for this helpful feedback – I will adjust this terminology. I am considering 'waveguide amplitude' instead of depth to avoid the confusion with altitude (feedback on this idea is welcome!), and I will give more description on what the definition of this term to provide more clarification.

b) Introduction (lines 25 to 29). It would be good to add some physical interpretation of why people think there is a link between waves amplitude and waveguides. In my opinion, this is potentially because waves are not dissipated as there are no critical latitudes in a waveguide. But I am not sure this is what the papers cited in line 28 have argued.

Yes, I agree that the lack of dissipation of wave energy within the waveguide, and channeling of wave energy into the latitude band of the waveguide is at least part of the physical interpretation. It is also possible the typically enhanced stationary wavenumber within a waveguide, allowing waves of higher wavenumber to become (quasi-)stationary, also plays a role. In addition, if the waveguides are quasi-stationary, this would allow quasi-stationary channeling of wave energy into this particularly latitude band. I will expand on this part of the introduction in a revised manuscript.

c) Method: it would be good to highlight that the background flow and the waves are separated by the spatial scale ($k<2$ for the background flow and k between 4 and 15 for the quasi-stationary waves). Since the 15-day running mean is used for the detection of both the background flow and the waves, the reader might be confused by the separation between these two parts of the flow.

Thank you, I will add clarification text on this. I have also repeated the quasi-stationary wave analysis using k between 6-15 to provide even greater separation, and results are very similar. I will add this to the sensitivity analysis.

d) Section 4.1 and waveguide frequencies:

- before starting that section it would have been nice to show Ks for the time mean flow of JJA and DJF, i.e repeat Figures 3c of Hoskins and Ambrizzi (1993) and 11c of Ambrizzi et al (1995). Maybe it would be good to do it by considering the climatological flow for $k<2$ to be close to what is done in the present paper for the time-evolving waveguides. Such additional figures could help to better visualize the difference between summer and winter and between SH and NH. The argument made lines 160-165 to qualitatively explain why the summer NH has more frequent waveguides than the winter NH could be better understood by showing the time mean U and Ks for both seasons. Furthermore, maybe the additional argument is the fact that the jet is probably narrower in summer than winter and both the planetary vorticity gradient and relative vorticity gradient play a role in the difference between summer and winter.

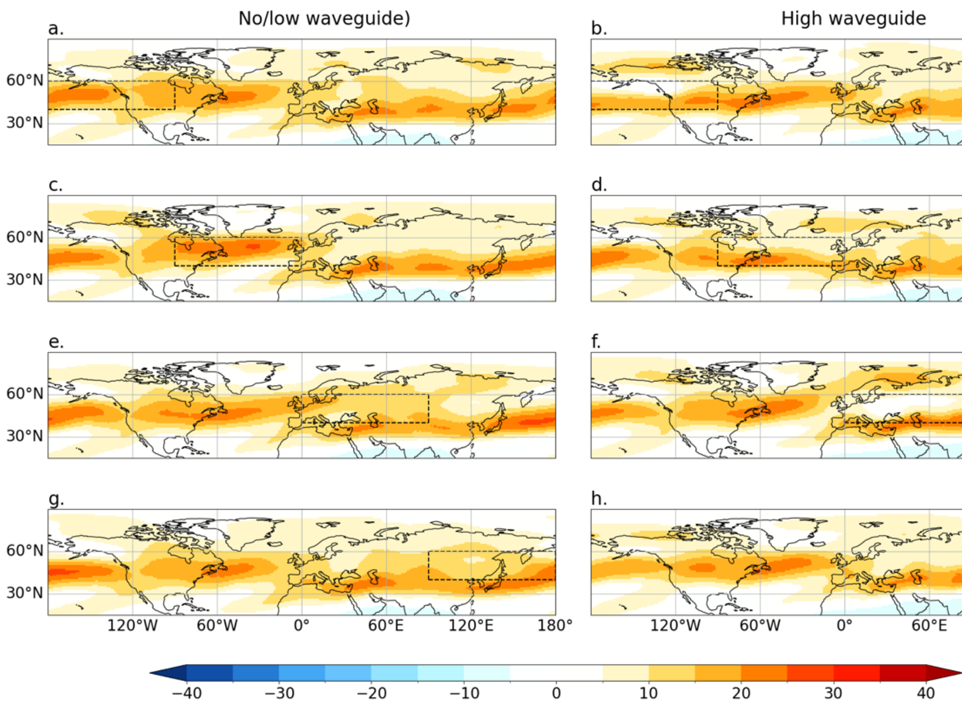
This is a nice idea, thank you. I will add climatologies for the time mean flow; although, because Ks is non-linear, the Ks for the time mean flow may be quite different from the time mean Ks. But I agree that plots of the climatological fields may help provide more physical interpretation of the results, particularly in the context of the discussion for the next point.

- I am surprised that the SH has less waveguides than the NH as the double jet structure (separation between subtropical and eddy-drivent jet) is more marked there, at least in the climatologies.

Yes, I agree that this result is interesting, although slightly stronger waveguide frequency in Northern vs Southern hemispheres can also be seen in Polster and Wirth (2023) in Fig. 3, and so this result is consistent with previously published analysis. I will add references to this paper and add more discussion around this topic in a revised manuscript. It is also worth noting that the waveguide frequencies are giving the frequency of a waveguide existing at that latitude, not the frequency of a waveguide at that longitude in that hemisphere – thus jets with more variable latitude may show a reduced waveguide frequency at any one latitude. Some evidence in support of this idea can be seen in Manola et al. 2013 (Fig. 5 e and f), although this is comparing SH summer with NH winter. There may also be an influence of differences in the width of the jets between the northern and southern hemisphere. For example. Manola et al. 2013 (Fig. 5 c and d) show some differences between the jet widths (again, comparing SH summer with NH winter), and in the SH jet widths of up to 16-18 degrees latitude are more common than in the NH. This importance of jet width can also be seen in the new figure you suggested, which I have added below, looking at composites of zonal winds (not just anomalies) for strong vs low/no waveguide days - the differences are often more in the narrowness of the jet, and not always in the maximum strength. Lastly, Hoskins and Woollings show the climatological stationary wavenumber for both hemispheres and both seasons; poleward of 25S/N, the climatological stationary wavenumber in the jets tends to be stronger in the NH than in the SH. It is possible that the jets in the SH are in fact "too" separated, such that the meridional gradients of zonal wind are not as stronger as in the NH. As suggested above, I will plot the climatological stationary wavenumber distributions, and discuss the hemispheric and seasonal differences of waveguide frequency in the context of the climatological distributions of stationary wavenumber.

e) Section 4.2: this is the part of the results where I am less convinced by the conclusions. For instance, Figure 6a shows an anomalous tripolar pattern in zonal wind when computing the difference between high and low waveguide strengths. This anomaly could be the result of different changes: a more pronounced double jet structure is one possibility but it could result from a widening of the jet or some latitudinal shifts. So it would be very nice to compare composites of high waveguide strengths and low waveguide strengths separately before (or rather than) showing the difference.

Thank you for this suggestion. I have made these plots of the composites (see below) and will add these to the paper to allow more physical interpretation of the anomalies currently shown. As discussed above, these composites do show a double jet structure in many regions, however this is also related to a southward shift in the lower latitude jet relative to the no/low waveguide condition.



Zonal wind (m/s) for NH summer composites. Left column: no/low waveguide strength; right column: high waveguide strength. Different rows denote waveguides in the different boxed region, ad in Fig. 6 in the paper.

f) Section 5: It is surprising that the correlations are strong in the Atlantic and over Asia and not in the Pacific while the waveguide depths are similar in the North Atlantic and North Pacific. What would be a possible explanation for that? Or if you do not have hypothesis it would be nice to comment these results by referring to other studies. Were the studies on the relationship waveguide-wave amplitude focused in the North Atlantic and Asian regions. Do you know studies that also considered that relationship in the North Pacific?

I agree that this is a somewhat surprising result, and may perhaps be some combination of longitudinal variations in waveguide strength, longitudinal waveguide extent, and the location of wave sources relative to the waveguides, however I do not have a clear hypothesis. I will look into this more for a revision. There is still a positive correlation across much of the Pacific region in most sensitivity analyses - the correlation is weaker and not statistically significant, however it may be physically real.

g) Sensitivity tests: I think it would be good to have a sensitivity test by changing the mean pressure level (e.g. 500 hPa?). Held et al. (1985) computed a barotropic equivalent level near 425 hPa and Charney (1949, see section 6) found a barotropic equivalent level closer to 550-600 hPa.

This is an interesting idea, thank you – I will add this to the sensitivity tests.

Thank you for your minor comments. I will add in these suggestions and corrections.