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

The concept of a Rossby waveguide has recently found increased interest. This recent interest may be partly due to the hypothesis of Petoukhov et al. (2013), who considered circumglobal waveguides and investigated the possibility of Rossby wave resonance during specific episodes. Their method to diagnose the existence of a waveguide from observations was based on arguments used earlier by Hoskins and Karoly (1981) and Hoskins and Ambrizzi (1993), which in turn start with linear wave theory, make additional assumptions (like the so-called WKB approximation), and finally arrive at the concept of a refractive index. Under special circumstances, the basic state may feature two so-called turning latitudes for a given wavenumber, and this is then interpreted as a perfect zonal waveguide for the respective wavenumber. In the remainder of this text I will refer to this framework with the "Two Turning Latitudes" as TTL analysis or TTL thinking. The current paper has, broadly speaking, two parts. In the first part the author produces and discusses, for the first time, a climatology for waveguide occurrence based on TTL analysis. As a particular feature the author considers a background atmosphere that is allowed to have a smooth variation in longitude, such that her waveguides, too, include a smooth variation in longitude. In the second part, the author goes on and correlates waveguidability as diagnosed from TTL analysis with Rossby wave amplitude, where both waveguidability and wave amplitude are allowed to vary smoothly with longitude. In both parts, the background atmosphere (needed to define a waveguide) is obtained through a combination of temporal and spatial filtering.

In the past, I have raised two major issues with the TTL analysis as a method to diagnose waveguides and waveguidability and how it is usually applied. First, in an idealized modeling framework I designed a method to diagnose "true" waveguidability and compare it with TTL-based waveguidability (Wirth, 2020). My "true waveguidability diagnostic" does not make use of the WKB assumption, which underlies the TTL analysis but which is badly violated in realistic situations. Therefore, in case of discrepancies between the two methods, my "true waveguidability" would naturally be given priority. As it turns out, TTL-based waveguidability is severly flawed in that it is unable to reproduce the gradual increase in true waveguidability as the strength or the narrowness of a jet is increased (see also Manola et al. 2013). In particular, the association of the existence of two turning latitudes with a (perfect!) waveguide for the corresponding zonal wavenumber was shown to be highly problematic. My second issue concerns the fact that the TTL-based analysis may be subject to artefacts in the event of large wave amplitudes, if the used background state is based on zonal averaging (Wirth and Polster 2021).

I do not contend that these two issues reduce the utility of TTL-theory to zero, but I would certainly say that TTL-theory is unable to represent certain (possibly important) aspects regarding waveguides and waveguidability. Unfortunately, we do not have a satisfying understanding yet that would allow one to distinguish those aspects for which the application of TTL-theory is appropriate from those for which it is not. This situation calls for a high level of care that needs to be exercised. Incidentally, as far as I can see, earlier papers such as Hoskins and Karoly (1981) and Hoskins and Ambrizzi (1993) never based their conclusions on TTL analysis alone; rather, their work typically included some independent statistical analysis and/or numerical modelling, and whenever all these approaches yielded consistent results, the TTL-diagnostic was used for interpretative purposes.

Despite the issues that I raised above, the TTL analysis of waveguides and waveguidability enjoys ongoing popularity, and the caveats associated with this approach are sometimes simply ignored. The current paper seems to continue along this tradition. To be sure, the author quotes all the relevant papers including the two critical ones I just mentioned. She even says explicitly that caution is needed when interpreting results that are based on TTL theory (line 90). But then she simply goes on and does not really attempt to critically discuss the implications of these papers for her own work. In my eyes this is not satisfactory. I would expect that the author gives strong arguments why she thinks that the issues I raised are irrelevant in the context of her paper. Otherwise, the reader is left alone with the question: "why learn more about a diagnostic that was shown to be severely flawed in relevant applications"?

Interestingly, when I read the paper for a second time, it occurred to me that the results from both parts can actually be interpreted in a way that further supports the criticism raised by Wirth (2020) and Wirth and Polster (2021) — see my further explanations in the major issues below. I believe that such an interpretation was not the original intention of the author.

Given this situation, I am not sure what to recommend. One option might be that the author is able to argue in favor of TTL thinking and dismiss my issues on the basis of compelling arguments. This option would probably result in minor revisions to the manuscript, like for instance adding a new paragraph that contains the relevant explanations. Personally, I cannot see how this could work, but I may be biased or miss an important point, and I am happy to get involved in a discussion and learn more. As another option I could imagine a statistical/climatological comparison of the TTL diagnostic with an alternative diagnostic like, for instance, the one of Polster and Wirth (2023). This option would make use of most of the work performed in preparing this paper, but of course it would mean a complete rewrite.

I thank the reviewer for his time reviewing this manuscript. I agree that care needs to be taken with interpretation of the TTL definition of waveguides, and I tried to convey this in the manuscript, but I will add in more discussion of some of the points raised by the reviewer. I believe that, whilst the method certainly has limitations (as, I would argue, do all research methods trying to study waves and waveguides in a time-varying and spatially-varying context), its usefulness has not been disproven, and the research I present shows some of the potential uses of this theory in understanding connections between blocking and localized waveguides (stepping away from a zonal mean perspective), and between waveguides and co-located quasi-stationary waves. I believe this research provides a useful step forward, and publication of this paper and dataset will allow the community to further investigate this methodology and the topic of interactions between waves and waveguides.

I agree that there are some advantages to the PV-gradient methodology as raised by the reviewer, however, given that daily zonal wind on upper tropospheric pressure levels is available directly from many CMIP6 climate models (https://github.com/cmip6dr/data_request_snapshots/blob/main/Release/dreqPy/docs/CMIP6_MIP_tables.xlsx) the TTL method provides some advantages for studying future changes, if the methodology is found to be valid and useful. Given this, I believe that continued community investigation, through peer-reviewed papers on the use, and similarities/differences between the two methods of waveguide detection, will be useful in furthering our understanding of this complex topic.

The reviewer has indeed shown, in Wirth (2020) that, under very specific circumstances, with idealized high amplitude perturbations, the zonal mean background flow does not represent a 'true' background flow and waveguides in the zonal mean flow can appear as an artefact of the methodology. However, the research in my paper is an explicit attempt to move away from the zonal mean, partly for this reason. Waves of sufficiently long wavelength may alter the flow such that they then impact waves of higher wavenumber. This, I believe, is suggested by the research in the current paper and is an important result.

I will certainly include more of a comparison with the waveguides of Polster and Wirth (2023), particularly as there are some similarities between these two methods in terms of northern vs southern hemisphere asymmetries in waveguide frequency. Unfortunately Polster and Wirth (2023) only studied wintertime waveguides, and so a comparison across seasons is not possible. I agree that a more complete statistical comparison of these two datasets of waveguide frequency would be valuable, but is outside the scope of this current paper; I will suggest this in the section on future research.

I also agree with the reviewer that some of the results from this work are supportive of the results of Wirth and Polster (2021), namely that strong blocks may create waveguide conditions. The reviewer argues that, when looking at zonal waveguides the causal relationship is from block to waveguide. I agree that this may indeed be the case in some circumstances (although I do not think it has been proven that it is always the case), and I also agree that the composites of zonal wind in this current paper support this idea. I agree that more emphasis should be put on this result, and intend to include composites of geopotential height anomalies, showing that, in some regions, the presence of strong waveguides is associated with a region of enhanced geopotential height **poleward** of the waveguide location, consistent with the zonal wind changes that create the double jet structure shown in the previous composites. This result is consistent with the idea of an atmospheric block helping to create the local double jet which helps create the waveguide conditions. Taking a zonal mean of the zonal composites shown in this paper, would very likely show a double jet structure; however, importantly, the 'double jet' found in these composites is a localized double jet, not one with hemispheric extent, and a hemispheric average would likely be misleading, as stated by Wirth and Polster (2021).
The goal of this study, however, is to look at localized waveguides. Whilst indeed it seems that blocks may well cause an increased probability of a waveguide equatorward of the block, in the final section I show the relationship between **co-located** quasi-stationary waves and waveguides. Because I use the wave envelope of meridional wind to define the wave amplitude, one would expect any wave amplitude associated with the block to be poleward of where the waveguide exists, and thus this cannot be the explanation for the positive correlations I show. The results presented here show that, at some (but likely not all) longitudes, high latitude blocks likely play a role in *creating* the conditions for a waveguide to exist at lower latitudes through their influence on the localized zonal winds. The positive correlations with co-located waveguides suggest that such waveguides may then play a role in trapping quasi-stationary waves of higher wavenumbers at lower latitudes. This may help explain some of the 'double quasi-stationary wave' pattern (waves of different wavenumber seen in different latitudes) seen in several extreme months (e.g. White et al. 2022), and the coincidence between blocks and recurrent Rossby waves (which would show up in the quasi-stationary wave metric used here) found by Mubashshir Ali et al. 2022.

I understand the reviewer's concern about the idea that, as soon as there are two turning latitudes, there is a 'perfect waveguide'. I agree that this perspective would be problematic, but I do not think that this is the only interpretation of the turning latitude theory. Inspired by the reviewer's previous work (Wirth 2020), I chose to define a 'waveguide amplitude' (previously 'waveguide depth') metric to acknowledge that waveguides can exist with a spectrum of strengths, even in the two turning latitude perspective.

Major Comments

The first set of results (section 4.1) presents climatological properties of TTL waveguidability, and it turns out that (broadly speaking) waveguidability is stronger or waveguides are more frequent in summer compared to winter. This is in striking conflict with the fact that jets are generally stronger in winter than 2 in summer, and the latter implies stronger (true) waveguidabiliy in winter compared to summer according to Manola et al. (2013) or Wirth (2020). The author mentions this conflict on line 168 and adds later on line 263 that "further research is required to fully investigate this result". I find this not very satisfying. One way to resolve this conflict would be to admit that TTL-based waveguidability is flawed and inappropriate for a reliable diagnosis in this context, consistent with the arguments of Wirth (2020).

The waveguides depend on the stationary wavenumber, Ks, which is related to the 2nd meridional gradient of the zonal wind (see Eqs 1 and 2 in the manuscript) rather than the strength of the zonal wind in the jet (and, indeed, the zonal wind strength appears in the denominator of the Ks equation), and so the width of the jet plays a key, and indeed, perhaps stronger, role relative to the strength of the jet. This point was also highlighted by Manola et al. 2013. I therefore don't believe the results found in this paper are inconsistent with theoretical expectations. Indeed, in Hoskins and Woollings (2015) Fig. 2, a stronger climatological stationary wavenumber can be seen in the summer vs winter seasons, which implies (although the link is not direct) higher waveguide frequency in summer. As suggested by reviewer 1, I will include climatological maps of the stationary wavenumber in a revised manuscript, allowing a more complete discussion of the hemispheric and seasonal differences.

Hoskins, Brian, and Tim Woollings. 2015. "Persistent Extratropical Regimes and Climate Extremes." *Current Climate Change Reports* 1 (3): 115–24. https://doi.org/10.1007/s40641-015-0020-8.

Manola, I., Frank Selten, Hylke de Vries, and Wilco Hazeleger. 2013. "'Waveguidability' of Idealized Jets." *Journal of Geophysical Research: Atmospheres* 118 (18): 10,432-10,440. https://doi.org/10.1002/jgrd.50758.

I also believe there is more of a difference between localized waveguide presence and 'hemispheric waveguidability' than the reviewer is considering here, with the latter 'waveguidability' defined as the ability of a background flow to trap waves within a certain latitude band across the whole longitude range, i.e. considering hemispheric waveguides. Such hemispheric waveguidability is important for circumglobal Rossby waves, but I am interested in whether localized waveguides can be important for localized Rossby wave amplitude. I recognise that this distinction could be made more apparent in the paper, and will adjust the text to do so.

2. Figure 6 of the paper indicates that the "black boxed regions" with strong waveguide occurrence are characterized by weaker than normal zonal wind. Again, this result is in striking conflict with the results of Manola et al. (2013) and Wirth (2020), who showed that strong waveguides are generally associated with stronger rather than weaker jets. As far as I can see, the author does not resolve this conflict. One way to resolve the conflict would be to acknowledge that the TTL-waveguide diagnostic is fraught by the artefact discussed in Wirth and Polster (2021, see my further comments below), and that previous authors who used the TTL-diagnostic are subject to the same artefact.

The winds are only weaker in the northern half of the boxes, and stronger in the south, consistent with enhanced second meridional gradients in zonal wind, which is consistent with the theory of waveguides. The waveguides in these composites are typically in the southern half of the boxes, as shown by the pink contours. To try to make this point clearer, I will replot the figures using waveguide regions with a smaller latitudinal extent, and further emphasise the importance of the meridional gradient over just the maximum strength of the jet.

3. The second part of the paper (section 5) shows, broadly speaking, a (weak, but statistically significant) positive correlation between local Rossby wave amplitude and local TTL-waveguidability in certain regions (Fig. 10). At first sight this result was surprising to me, given that the author does not really give a motivation why one should expect such a correlation (e.g., on lines 98 and line 280ff, where such a connection is more or less assumed to be given). To be sure, in the case of circumglobal waveguides, a motivation might arise from the contested quasi-resonance arguments of Petoukhov et al. (2013). But this line or arguments cannot possibly provide a motivation for the present analysis, because Rossby wave resonance requires circumglobal waveguides, while the author here diagnoses local (even gridpointwise) waveguides. On line 210, the author mentions two papers in which such a correlation allegedly was hypothesized, but as far as I can see the waveguides in these papers were assumed to be circumglobal rather than local. Therefore, I disagree with the author's statement in the discussion section (line 279) that such a connection was hypothesized "to some extent" in previous articles. However, after second thought I realized that this correlation may be a result of the artefact which I discussed in Wirth and Polster (2021). The author herself provides a hint in her Figs. 6 and 7, where she shows that strong TTL-waveguides are associated with tripole-like anomalies in the zonal wind corresponding to a "double jet structure". This result, in combination with the positive correlation between local TTL-waveguidability and Rossby wave amplitude, offers the following interpretation: according to the argument of Wirth and Polster (2021), these strong TTL-waveguides may simply be artefacts arising from strong Rossby wave amplitudes. Large wave amplitudes would distort the total (= background plus wave) flow pattern such that one obtains a tendency towards a double-jet structure in the zonal average (see, e.g., the schematic in Fig. 1 of Wirth and Polster 2021).

To the extent that there is no alternative plausible motivation for the correlations in Fig. 8, my argument suggests that Fig. 8 could actually be 3 interpreted as an independent (data-based) confirmation of the Wirth-Polster criticism. A way to test this hypothesis would be to use the novel zonally varying background state from Polster and Wirth (2023). In fact, the author mentions this idea on line 293. It would not be too hard to perform this analysis, since Polster and Wirth published her code along with the paper. The basic state from Polster and Wirth is based on a "rolling zonalized" background field, which is not subject to the Wirth-Polster artefact — in contrast to the background state used in the current paper. If the correlation vanished upon the use of this (presumably more appropriate) basic state, one would have produced an independent piece of evidence for the statement that, indeed, the tripole-structures in Fig. 6 and 7 essentially reflect the artefact discussed in Wirth and Polster (2021).

Waveguides are regions where wave energy is more likely to be trapped within a particular latitude range (e.g. Hoskins and Karoly, 1981; Hoskins and Ambrizzi, 1993). Hoskins and Karoly also propose that waves will be refracted towards regions of higher stationary wavenumber, i.e. towards and into a waveguide. The presence of waveguides is thus indicative of regions where wave energy is more likely to be guided into, and trapped within this latitude. In addition, regions of higher stationary wavenumber would allow higher wavenumber quasi-stationary waves to be present. I will add these arguments into the introduction of the paper, making it clear that the hypothesised connection between waveguides and waves does not require zonally symmetric waveguides in this case, as I am not studying circumglobal waves.

As discussed above (see a more complete response there), in the section you refer to here, I show the relationship between **co-located** quasi-stationary waves and waveguides. Because I use meridional wind to define the waves, one would expect any wave energy associated with blocks that may be helping to create waveguide conditions to be poleward of where the waveguide exists. Thus, I consider the co-located quasi-stationary waves seen in the correlations to not be related to the block itself, and therefore the positive correlations found suggest that waveguides do lead to a higher probability of high amplitude quasi-stationary waves (although the direction of causality is not proven)

It may be the case that a block poleward of the waveguide leads to the changes in zonal wind, which helps create the waveguide conditions, which leads to a higher probability of quasi-stationary waves equatorward of the block itself – this idea will be hypothesised in the revised manuscript, but it is outside the scope of this current paper to prove this connection.

4. In Fig. 4, the author introduces a novel metric for "waveguide depth" Wd. This is an interesting idea, because Wd represents a somewhat more "integral" measure for the strength of a waveguide, in contrast to the search for two turning latitudes; the latter only relies on the intersection of the Ks-profile with a line representing a fixed wavenumber and introduces an artificial "waveguide vs. no-waveguide" dichotomy (Wirth 2020). Fig. 4 in Wirth (2020) suggests that stronger jets are generally associated with stronger Wd as defined here. However, Wd, too, is unlikely to represent the increase of true waveguidability with increasing jet strength beyond a certain limit. This was explicitly discussed in Wirth (2020) in connection with his Fig. 6a, where true waveguidability increases from about 48% to about 75% while jet strength was increased from 20 to 40 m/s, and this increase in true waveguidability would be completely missed by Wd. In fact, I believe there are better (and simpler) measures for waveguide strength, such as the horizontal PV gradient.

Indeed, my metric for waveguide strength (renamed from depth at the suggestion of reviewer 1) was inspired by your work in Wirth (2020). However, I believe that this metric DOES capture some of the increasing waveguidability for stronger/narrower jets. For example, in Fig. 4 in Wirth (2020), the "waveguide strength" (difference between k and maximum Ks in the waveguide) for a wave of wavenumber 5 increases from around 1.6 for a 10m/s jet to approximately 2.1 for a 40m/s jet, and so this increase in waveguidability is not entirely miseed by this method.

I thank the reviewer for the minor comments, and will address these in a revision.