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

The atmospheric waveguide has a profound influence on the propagation path of stationary Rossby waves, thereby affecting when and where these waves impact surface weather and climate. In recent years, studies on the atmospheric waveguide have gained popularity among the climate community due to its significant connection to extreme events. The investigation of waveguides can be traced back to early works, notably Hoskins and Karoly (1981), followed by Hoskins and Ambrizzi (1993) and Ambrizzi et al. (1995). This current study aims to extend previous research by examining the waveguide in the context of spatially and temporally varying mean flow. Most of the analysis is focusd on this issue.

This is a nice manuscript that uses refractive index as a perspective to understand atmospheric waveguide and its connection to stationary Rossby waves. In my opinion, it holds the potential to be considered for publication in a WCD. However, I have several major concerns about the methodology and interpretation of the results. I have listed my major comments below and would like to invite the authors to address them:

I thank the reviewer for their thoughtful comments. Please see my responses below.

1. The separation of mean flow and perturbations is always a controversial issue when studying wave-mean flow interactions. This issue becomes even more critical when large-amplitude eddies appear in the mean flow (e.g., Wirth and Polster, 2021). However, the present study heavily relies on the separation method, and most of the findings are based on the assumption that the waveguide and Rossby waves are well-separated and independent. Therefore, I question the significance of the results, as many intraseasonal waveguide behaviors are actually reflected by long-lasting waves.

I agree that sub-seasonal changes in waveguides can be caused by long-lasting waves; however the changes that are caused by those waves, particularly waves of low zonal wavenumber, can cause local changes in waveguides, which may influence higher wavenumber waves. In this work I attempt to separate out the low and high wavenumber waves through wavelength decomposition, with wavenumbers 2 and lower included in the background flow, and wavenumbers 4 and higher considered as waves. In response to the reviews, I have repeated the quasi-stationary wave analysis (correlation with co-located waveguides) using wavenumbers 6 and higher for the waves (retaining the definition of 2 and lower for the background flow); the results are relatively insensitive to this, suggesting that overlap between what is counted as background flow and what is counted as waves likely contributes little to the results shown in Figs. 8 and 10. That is not to say that there is no influence on non-linear perturbations (e.g. blocking) on the waveguides; indeed, I think this work shows a potential connection between blocks and waveguides. New results created in response to the reviewer comments, looking at composites of geopotential height anomalies, 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. Such a waveguide may then play a role in trapping quasi-stationary waves of higher wavenumbers at lower latitudes. This may 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 Fig. 1), 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.

2. Regarding the methodology, using the traditional turning point perspective to identify the waveguide could be misleading, despite its extensive use in recent studies such as Petoukhov et al. (2013) and many subsequent papers. The limitations of this method have been thoroughly discussed by Wirth (2020). Therefore, the authors need to demonstrate the limitation of the method used here is nontrivial and confirm the appropriateness of the method.

The limitations discussed by Wirth (2020) primarily concern the zonal mean perspective in the presence of very idealized, high amplitude non-linear perturbations to the flow. In this work I move away from the zonal mean perspective, partially due to some of these limitations. As discussed above, composites of geopotential height anomalies suggest that, at some longitudes, the presence of waveguides is indeed associated with the presence of a block (high geopotential height anomalies); however, the relative location, with the block located to the north of the waveguide, suggests that it is more likely that the block acts to increase the probability of the waveguide through its impact on the zonal winds. This new analysis will be added to a revised manuscript.

3. I doubt about the characterization of the atmospheric circulation associated with the waveguide strength as "double jet streams" (Figure 6), as the zonal wind anomalies are only confined to a local scale. Additionally, as related to my major comment 1, long-lasting waves might play a role in this structure. Therefore, it is possible that the pattern seen in Figure 6 is not "double jet streams", but prominant Rossby wave activity itself.

I did not mean to imply that these double jet stream existed on a hemispheric scale; indeed, Figure 6 shows clearly that they do not, and they are associated with localized enhanced waveguide presence. I also agree that the composites of zonal wind in this current paper support the idea that nonlinear anomalies such as block may in fact lead to the waveguide conditions, rather than causality in the opposite direction. I will put more emphasis on this result in a revised manuscript, and, as discussed above, 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, supportive of some of the ideas of Wirth and Polster (2021). 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).

4. The current study primarily focuses on the waveguide effect along the subtropical jet, based on the refractive index, which essentially represents the gradient of absolute vorticity. However, recent studies (e.g., Xu et al. 2019, doi: 10.1175/JCLI-D-18-0343.1; Xu et al., 2020, doi:

10.1175/JCLI-D-19-0458.1) have presented compelling evidence of the existence of stationary Rossby waves along the eddy-driven jet, where the waveguide effect arises due to the gradient of potential vorticity. As mentioned by the author herself, this important aspect has been neglected due to the limitations of the methodology used in this manuscript. The authors briefly touch upon this issue in the manuscript, but in my opinion, more in-depth discussion is required.

The frequency of higher latitude waveguides is not zero in this dataset (see Fig 2); however, it is notable that the positive correlation between waveguides and quasi-stationary waves is not present poleward of 50N. It is possible that stability plays a more important role in the presence of waveguides at higher latitudes, as demonstrated in the two papers suggested by the reviewer. As suggested, I will include a more in-depth discussion of this, including those papers, in a revised manuscript.

Notably, even the PV-based methodology of Polster and Wirth (2023) shows weak waveguide frequency at high latitudes; however, the Polster and Wirth (2023) definition of waveguides could be easily adjusted to have a lower PV gradient threshold, that may detect more frequent waveguides at higher latitudes – this would need to be explored, but is outside the scope of the current work. It would be interesting to repeat the correlation analysis between waveguides and quasi-stationary waves on such PV-detected waveguides, to see if positive correlations are found further polewards; I will suggest this in the section on future research.