Review of the manuscript ”Time-varying Atmospheric Waveguides — Climatologies and Connections to Quasi-Stationary Waves” by R. White, submitted for publication to Weather and Climate Dynamics

Recommendation: Minor revisions or a complete rewrite

General

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.

Major issues

1. 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

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).

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.

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 interpreted as an independent (data-based) confirmation of the Wirth-Polster criticism.

A way to test this hyopthesis 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).

4. In Fig. 4, the author introduces a novel metric for "waveguide depth" W_d. This is an interesting idea, because W_d 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 K_s-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 W_d as defined here. However, W_d, 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 W_d. In fact, I believe there are better (and simpler) measures for waveguide strength, such as the horizontal PV gradient.

Minor issues

1. Line 15 :... it can be associated with extreme weather occasionally, but certainly not always.

2. Line 20: I assume that the issue with stationarity is just as severe in connection with precipitation as it is in connection with heat. E.g., the flooding events in Germany (2002), Pakistan (2010), and Germany (2021) were associated with quasi-stationary circulation patterns.

3. Line 25: are you here referring mostly to circumglobal jets? Line 209/210: again, why should a strong local waveguide be associated with strong wave amplitude? Petoukhov et al (2013) hypothesize such a connection in the case of circumglobal waveguides, but you have a very local (grid-point wise) perspective on waveguides.

4. Line 43: ".... theory provides qualitatively useful insights....": how is this possible, if the underlying assumptions are not valid? Is this by pure chance? How about the issues in Wirth (2020), who showed that there are relevant

aspects, in which TTL-theory does not provide even qualitatively realistic results? If the theory provides sometimes useful results and sometimes not: how can one distinguish between these two alternatives?

5. Line 88: Here you seem to refer to Wirth and Polster (2021), not to Wirth (2020).

6. Line 94: to some people, a “nonlinear wave” is an oxymoron. I would prefer to speak about “nonlinear eddies” or “nonlinear perturbations” and reserve the term “waves” for linear dynamics.

7. Line 98: .... but this is true only if the waveguides are circuglobal!

8. Line 100: a running temporal mean?

9. Line 101: do you really mean “planetary wavenumbers of interest” ....? It appears to me that you are, rather, aiming to extract synoptic-scale wavenumbers here.

10. Line 103: “wave envelope”: Do you really mean the wave envelope of the planetary waves?

11. Line 103: “15-day running mean”: yet another temporal filter? Haven’t the data already been filtered temporally (line 100)?

12. Line 104: the Hilbert transform is usually applied to compute the wave amplitude, not the wave itself. What do you mean here?

13. Line 132: A westerly jet with strength 0.5 m/s is not very impressive in my eyes, and somehow conflicts with the idea of Manola et al (2013) that a jet needs to be both narrow and strong to be a good waveguide. In addition, the desired narrowness of the jet would suggest rather a criterion that restricts to a maximum width (rather than a minimum width, as you suggest).

14. Line 135: ... show that the results... “

15. Line 145/146: To me it seems as if you identify frequent waveguide occurrence with large waveguide amplitude, which I think is dangerous. Waveguide frequency and waveguide strength should be distinguished and not mixed together.

16. Figure 2: Can you explain the solid contours in the figure caption!?

17. Line 153: You find more waveguides in summer compared to winter, although the jet in summer is usually weaker. Isn’t this inconsistent with the results of Manola et al (2013) and Wirth (2020), who show that stronger jets are usually better waveguides, hence one would expect higher waveguide frequency in winter than in summer?! Can you resolve this issue? A similar problem appears on line 167/168 when comparing Northern and Southern Hemispheres.

18. Line 170 and following: here you show that your results are consistent with the formula from the theory that you apply, but at the same time they are inconsistent with results from Manola et al. (2013) or Wirth (2020). Does this

mean that you trust the TTL-theory more than the results of Manola and Wirth? That seems dangerous, because the latter do not rely on this (somewhat questionable) theory.

19. Line 179: similar as above, you find stronger waveguide "depths" in summer compared to winter, although other work suggests that weaker jets in summer should be weaker waveguides. Can you resolve?

20. Line 210: Well, that's not quite right. Petoukhov et al. hypothesize such a correlation only for circumglobal waveguides (which is needed for Rossby wave resonance), they do not hypothesize such a correlation between local wave amplitude and local waveguidability.

21. Line 260-265: here you seem to play down the conflict between your results and those of the literature; I would expect a lucid discussion and explanation how you think that these discrepancies can be "explained"

22. Lines 280-290: you mention a few results from the literature, but it did not become clear to me how they relate to your results. In particular, it did not become clear to me how you would address the criticism formulated in some of these papers. As I argue in the first part of my review, I believe that some of your results even provide additional support for some of the criticism formulated.

23. Line 296: Can you explain how your data set potentially can shed light on causality?

Mainz, 3 May 2024

Volkmar Wirth

References

Hoskins, B. J., and T. Ambrizzi 1993. Rossby wave propagation on a realistic longitudinally varying flow. *J. Atmos. Sci.* **50**, 1661–1671.

Hoskins, B. J., and D. J. Karoly 1981. The Steady Linear Response of a Spherical Atmosphere to Thermal and Orographic Forcing. *J. Atmos. Sci.* **38**, 1179–1196.

Manola, I., F. Selten, H. de Vries, and W. Hazeleger 2013. "Waveguidability" of idealized jets. *J. Geophys. Res.* **118**, 10,432–10,440, doi:10.1002/jgrd.50758.

Petoukhov, V., S. Rahmstorf, S. Petri, and H.-J. Schellnhuber 2013. Quasiresonant amplification of planetary waves and recent Northern Hemisphere weather extremes. *Proceedings of the National Academy of Sciences 110*(14), 5336–5341, doi:10.1073/pnas.1222000110.

Polster, C., and V. Wirth 2023. A new atmospheric background state to diagnose local waveguidability. *GRL* **50**, DOI:10.1029/2023GL106166.

Wirth, V. 2020. Waveguidability of idealized midlatitude jets and the limitations of ray tracing theory. *Weather Clim. Dynam.* **1**, 111–125, https://doi.org/10.5194/wcd–1–111–2020.

Wirth, V., and C. Polster 2021. The problem of diagnosing jet waveguidability in the presence of large-amplitude eddies. *J. Atmos. Sci.* **78**, 3137–3151, https://doi.org/10.1175/JAS–D–20–0292.1.