Response to reviewers – "WCD Ideas: Teleconnections through weather rather than stationary waves"

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I sincerely thank the two reviewers, Volkmar Wirth and Daniela Domeisen, for their re-assessment of this manuscript and the again constructive and useful comments. A point-by-point response to the comments appears below in blue.

Reviewer 1 – Volkmar Wirth

General comment

The author revised the manuscript carefully and addressed most of my concerns - except the issue about causality, which I am still not happy with (see below).

While reading the revised manuscript I found a few minor issues, and I am not sure whether these are new ones of I just did not stumble acrosse them upon my first reading. In any case I think these minor issues should be addressed before publication.

Overall I think this is thought-provoking and useful contribution which should definitely published.

I am happy to read that I could address most of the reviewer's concerns, and I hope that I will be able to resolve a few more here.

Minor issues

Line 42-43 you may want to add a recent reference about the limit of (intrinsic and practical) predictability in midlatitudes, namely T. Selz, M. Riemer, and G. C. Craig: The transition from practical to intrinsic predictability of midlatitude weather. J. Atmos. Sci., 79, 2013–2030, 2022.

True, thanks for suggesting the addition. Added.

Line 50 "They further require spatial variations in the mean state to be gentle enough to not interfere with wave propagation." Is that really true? To be sure, linear wave theory requires quadratic terms to be small compared to the linear ones, but does it make any assumptions about spatial variations? The latter sounds as if you are referring to the WKB approximation. However, the WKB approximation is not part of linear wave theory (although it is sometimes made in addition to linear wave theory).

Thanks for pointing out this mistake, I indeed had the WKB approximation in mind when formulating this sentence. I removed the sentence.

Line 93 I still do not see how causality (in general) is lost in stationary wave theory. As I said earlier, the (external) forcing is the cause for the emerging wave train. In my eyes, this is NOT analogous to the diagnostic relation between the geostrophic wind and the pressure gradient, which clearly precludes any interpretation in terms of cause and effect.

Thanks for bringing that point up again, I think I now understand a bit better what you wanted to criticise. It is my impression that the disagreement is mainly due to the reviewer interpreting the analogy more strictly than I had intended. In a strict sense, I agree, if all conditions for stationary wave theory were met, the theory could provide a causal explanation. Only, the conditions are clearly not met in practice.

Beyond the questions of stationarity and the degree of externality of the forcing discussed earlier, may be the conceptually largest problem is that stationary wave theory implicitly assumes that there is no weather happening on top. Any cyclone development anywhere along the stationary wave would constitute a non-linear interaction of a finite-amplitude eddy with the stationary wave, and thus clearly invalidate the assumptions underlying the theory. Returning with this thought to the question of causality, I see the problem not with the conceptual construct of stationary waves, but in the practice of applying the concept to wavy patterns observed in a time-mean.

I realise I previously used the concept of stationary wave theory and the practice of interpreting time-mean wave patterns interchangeably, thus muddying my criticism in the analogy. I now rephrased the analogy to be more precise with my criticism, i.e., such that it only applies to time-mean wave patterns. I further moved the discussion of how stationary wave theory is not really filling the gaps in our understanding of how to interpret time-mean wave pattern closer to the analogy. I hope in now becomes clear now also in my writing that I agree with the reviewer in that the problem is not the theory itself but its application.

I am repeating myself, but still: thanks for bringing this up again, allowing me to sharpen in my arguments!

Line 118 "[...] because it transforms teleconnections from statistical relations to a causal chain of events [...]": well, to the extent that linear wave theory applies, it does provide a causal explanation in my eyes. Again, the problem is not that the Hoskins-Karoly theory lacks causality, the problem is rather to understand why or whether it can be applied to the problem of teleconnections.

Here I am not directly referring to Hoskins and Karoly, but only about "shifting the focus from monthly and longer time scales to synoptic time scales". I thus trust that this comment will be solved indirectly by the more precise formulation of the analogy around Line 93, which provides a more precise context for the statement here.

Line 152 better "[...] the EP-flux, the divergence of which [...]"

True, thanks for the correction.

Line 169 "[...] the required mean state cannot represent zonal asymmetries [...]", well, this drawback can also be overcome, at least in a practical sense, see C. Polster and V. Wirth: A new atmospheric background state to diagnose local waveguidability. GRL, 50:DOI:10.1029/2023GL106166, 2023.

Thanks for making me aware of this latest development. I am happy to mention the study here.

Line 182 "[...] the mean state (Fig. 3a, d) remains a poor representation of the varying conditions non-stationary Rossby waves might encounter [...]": just to be sure, the novel background state of Polster and Wirth (2013) can be computed from just a single snapshot, and it does vary (smoothly, though) from day to day.

You make a good point in Fig 3. However, the problem may be not so much the need to choose a background state in order to define a "wave", but rather that in the past the chosen background state was inappropriate.

I agree, your approach in Polster and Wirth (2023) appears quite promising for extracting wave guides. Still, given that Polster and Wirth (2023) was published only a few weeks ago, I would claim the final verdict here is still out. In any case, good to have several avenues to pursue!

Reviewer 2 – Daniela Domeisen

Comments

Line 26 it's not clear what "this idea" refers to here. It sounds like it refers to "The stationary wave paradigm" used in the sentence before, but I don't think that's what the author intended.

Thanks for pointing out this potential source of confusion. I rephrased using first person voice, i.e. "[...] I challenge the paradigm [...]" instead of "[...] the idea challenges the paradigm [...]".

For the parts describing the MJO teleconnection, I have a few more suggestions for references that may be useful, especially with respect to the prediction aspect, I'm listing them in the "references" section. I leave it up to the author to decide which ones (if any) are useful for this manuscript.

Thanks for pointing me to all the additional references, I read them with interest. In the end, I included Garfinkel et al. (2014) as additional context for the MJO-North Atlantic teleconnection and Vitart (2017) and Stan et al. (2022) as additional context for practical predictability through this teleconnection.

I think all the references to Figure 3 should refer to Figure 2 instead, as Figure 2 is not referred to in the text, please correct (apologies if I missed it).

Thanks for pointing out this mistake! You are absolutely right, there are only two Figures. I corrected the references.

Overall, the manuscript is still rather Europe-centric. I understand this might be the goal, in which case I do not want to interfere here, but I would like to point out that it would be rather straight forward to make the manuscript more globally applicable by considering e.g. MJO teleconnections to other extratropical regions, such as e.g. line 46: the MJO is important in S2S prediction not just for the North Atlantic region.

I agree with the observation and I agree that it's a caveat of the manuscript as it is organised. I still prefer to keep the Euro-centrism. In the same way as the narrowing from tropical Indo-Pacific to only and specifically the MJO has helped clarify the arguments, I fear that opening to MJO teleconnections in other regions would make the arguments less clear again. Considering, for example, a hypothetical jet streak over the North Pacific which is related to strong tropical convection in the tropical Pacific. For such a case, I would not even be sure if I wanted to apply the label "long-distance teleconnection" as the connection between the regions can (in this hypothetical case) be explained solely by a variation of a single dynamical entity, that is a locally and temporarily amplified Hadley circulation.

Lines 46-51 I think it needs to be made clear here that the first sentence talks about deterministic predictability, while S2S prediction, which is the focus of this study, is entirely based on ensemble prediction. I would recommend to avoid mixing the two concepts here. We considered the deterministic limit here, maybe interesting: Domeisen et al: "How predictable are the Arctic and North Atlantic Oscillations? Exploring the variability and predictability of the Northern Hemisphere." Journal of Climate 31.3 (2018): 997-1014.

Once more, thanks for pointing out this potential for confusion. I now make it clear that I initially only refer to deterministic probability. The transition from deterministic to ensemble predictability was already explicitly marked in the text. Thanks also for pointing me to this study; I agree it provides interesting context here and included it in the reference list discussing deterministic potential predictability.

Line 93 hence, a "teleconnection event" would be a case where the MJO has a teleconnection to the North Atlantic

Yes. I reformulated to emphasize this aspect a bit more.

Section 6 I appreciate the additions in this section which helped a lot to clarify the plans / concept of this study.

Good to read, thanks for the positive feedback!

Technical comments

Thanks for pointing these additional mistakes, they are fixed.