Review of the manuscript ”WCD Ideas: Teleconnections through weather rather than stationary waves” by C. Spensberger, submitted for publication to Weather and Climate Dynamics

Recommendation: Important revisions

General

This manuscript challenges the traditional idea that long-distance teleconnections can fundamentally be understood in terms of long-lived quasi-stationary Rossby wave trains; rather, the author suggests that these teleconnections should be re-interpreted as due to a chain of transient nonlinear events on the synoptic scale, i.e., in terms of weather features. Part of the question is whether or not the synoptic-scale variability is too strong to allow a meaningful analysis in terms of a mean background state plus (linear) stationary wave theory.

In my eyes this manuscript is a welcome contribution to the discussion, raising issues with a long-held perspective and, at the same time, suggesting an avenue towards further progress. In some places the text appears quite opinionated with sharp formulations, while the author sketches supporting arguments. Some of the arguments may not stand the test of time, and in my text below I indicate a few points where I am skeptical. To be fair, the author does not exclude the possibility that his hypotheses might eventually be (partly) refuted (lines 107, 108). Despite these caveats I believe that the proposed hypotheses are interesting and relevant, and it is hoped that a publication in the present format will trigger further discussion.

Below I raise a few major issues. Regarding two of them (items 1 and 2) I have a rather strong opinion and I urge the author to revise the text accordingly. Regarding the remaining issues, I am aware that this idea-format differs from a regular paper: rather than presenting full analyses and final results, it is meant to talk about ideas and thereby instigate discussion. The question is to what extent the author wants to anticipate some of the discussion in his text.

Major issues

1. It is important to be fair regarding earlier work — especially in this new format of “WCD ideas”. Therefore, all relevant previous papers need to be quoted, papers that have discussed or at least mentioned similar ideas, or possibly even the same idea.

   I think that the current text is deficient in this respect. Strictly speaking, the basic idea presented in this manuscript is not new. One important previous paper that quite explicitly discussed this idea in the context of an observed episode is that by Davies (2015). And there are probably further papers that may have touched upon this or similar ideas (e.g., Moore *et al.*, 2010), although perhaps in a somewhat implicit fashion. These earlier references

should be mentioned and briefly discussed. Also, a statement such as “no attempt has been made yet to interpret and understand long-distance teleconnections in terms of variations of weather” (lines 25, 26) seems unfair.

Similiarly, doesn’t this text beg for mentioning the recently suggested concept of “recurrent Rossby wave packets” (Röthlisberger *et al.*, 2019; Ali *et al.*, 2021)? In these papers the persistence of dry and wet spells is interpreted as the recurrent occurrence of Rossby wave packets, with the underlying idea being quite analogous to the idea presented in the current manuscript. In both cases a longer-term (seasonal or sub-seasonal) phenomenon is re-interpreted as the reoccurrence of a synoptic-scale weather-like phenomenon that “happens” to add up and result in the observed seasonal anomaly.

2. I have an issue with the discussion of causality (in particular the paragraph around line 70). The author seems to imply that stationary wave theory is fundamentally unable to establish cause-and-effect relationships. I would argue with this point of view. To be sure, I am aware that it is difficult to infer causality from a diagnostic relation that does not contain an explicit time derivative (something which, of course, has been known for a long time, e.g., Lorenz 1967). Geostrophic balance is but one particular example. However, I would argue that it is possible to talk about cause and effect in a forced-dissipative system in which the forcing can be considered as sufficiently “external”, i.e., independent of the response. A prime example of such a situation is the theory of stationary planetary waves. To the extent that the forcing (e.g., orography) is independent of the response (the Rossby wave train emanating from the orography), I would argue that the orography can and should be considered as causing the Rossby wave train. Therefore, I disagree with the author that stationary wave theory per se is unable to infer causality. In my eyes, the issue is not the lack of causality in the original argument of Hoskins and Karoly (1981), but rather the question of why and how such a linear theory can (seemingly) be applied to long-range teleconnections in the light of strong weather systems (i.e., strong non-stationary non-linearities). The latter issue is explicitly formulated by the author in lines 63-64.

   Let me be somewhat more fundamental and philosophical. To be sure, the succession of several synoptic-scale events can provide a causal chain. However, an “explanation” in terms of a complete physical chain of individual weather events (lines 98-100), although certainly being causal, may turn out to be pretty useless regarding a deeper “understanding”. Extending this general approach to its extreme would mean that, for instance, the atmospheric circulation (discounting, for the sake of the argument, moist physics and further complications) can be “explained” by the validity of Newton’s second law. In this perspective one simply states that each air parcel in the Earth’s atmosphere is accelerated in proportion to all forces acting on the parcel divided by its mass. This statement is both true and causal! But would it help to “understand” important dynamical phenomena such as baroclinic instability? In my eyes a theory like the Eady or Charney model are more “useful” in this respect. Couldn’t a similar argument apply to the issue of long-range teleconnections? I would argue that linear wave

theory, if applicable, does have some explanatory power in this case. To say it again: The crucial question is to find out why the Hoskins-Karoly theory is (seemingly) so successful.

3. The second part of the manuscript describes an avenue toward further progress: in an attempt to diagnose Rossby wave activity, the author suggests to perform some kind of data assimilation into an idealized model that is able to represent Rossby waves only. This part is rather sketchy, fair enough, and for me a number of open questions remained. However, what's more important, could it be that the author becomes subject to his own criticism? Projecting the complex reality onto a reduced dynamical model is interesting, but the results inherit some statistical nature. Possibly this prevents a solid causal interpretation, because all the processes that are not captured by the idealized model are missing in the alleged causal chain so produced.

4. There may be other avenues to increase our understanding of teleconnections in the presence of large-amplitude eddies. For instance, there have been recent developments of a theory for wave mean flow interaction that is valid for finite-amplitude eddies, such as Nakamura and Solomon (2010) or Methven and Berrisford (2015). As we argued in a recent opinion paper (White *et al.*, 2022), such new concepts may prove useful in the current context: they have the potential to push wave theory towards applications where finite-amplitude eddies play a major role, and that is exactly at issue in the teleconnection problem.

Minor issues, typos etc.

1. Line 39, "small changes in the flow structure. . . ": do you mean: small changes in the background state / background flow?

2. Line 50, better "a first step. . . "

3. Line 51, "North Atlantic strom track is. . . ."

4. Line 79: This section seems to suggest that linear wave theory is not a sound theory as opposed to, for instance, quasi-geostrophic theory. I do not agree. The problem is not that linear wave theory lacks foundation; rather, the problem is that it is unclear whether or why linear wave theory is applicable to the problem of teleconnections.

5. Line 86, what is a "linear mean state"? Do you mean a basic state used in the framework of linear theory?

6. Line 144, 197, "allows one to . . . ."

7. Line 183: Typo (reanalyses)

Mainz, 27 November 2023

Volkmar Wirth

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