Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2022-1-RC1, 2022

Comment on wcd-2022-1

Volkmar Wirth (Referee)

Referee comment on "Recurrent Rossby waves and south-eastern Australian heatwaves" by S. Mubashshir Ali et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-1-RC1, 2022

The current paper investigates the relation between Southern Hemispheric atmospheric blocking, recurrent Rossby wave packets, so-called quasi-resonant amplification conditions, and heatwaves over Australia. The work is based on (1) investigating two relevant episodes and (2) performing a more systematic statistical evaluation using reanalysis data.

I have two major issues with this work.

(1) First, I have a major issue with the way you try to diagnose resonance conditions. Essentially you use an algorithm from previous work, more specifically: from Kornhuber et al. 2017. This algorithm has recently been shown by Wirth and Polster (2021) to lead to spurious results: in the presence of large-amplitude waves, the algorithm is prone to diagnose two turning latitudes, which you then interpret as proof of a waveguide; however, as shown by Wirth and Polster (2021), the occurrence of two turning latitudes in the presence of large-amplitude waves is likely to be an artifact rather than an indication for a waveguide. Those who read the paper by Wirth and Polster (2021) realize that the algorithm of Kornhuber et al. (2017) is NOT appropriate to determine resonance conditions (unless you can prove otherwise). For this reason, you cannot simply quote the Wirth and Polster paper and then, for the rest of your paper, ignore their important result and continue without any further comment and detailed analysis.

One possibility to test whether or not the issue of Wirth and Polster applies to your case would be to repeat the analysis based on the zonalized background state instead of the zonal mean background state. If you obtain two turning latitudes with the zonalized background state, you could be somewhat reassured that this part of your analysis is free from spurious effects and, then, continue your argument. Christopher Polster from my working group would be happy to assist you with computing the zonalized background state if needed.

Indeed, I did refer to the Kornhuber paper "for more details" and found a second issue with the algorithm that, to my knowledge, has not been pointed out in the literature yet. In your algorithm you require a second criterion based on the amplitude of the forcing: "…the combined amplitude of the thermal and orographic forcing …. [must be] of sufficient magnitude". To be sure, orographic forcing is given and fixed and can be assumed to be "external". However, in addition to orographic forcing you use the observed temperature perturbation (= deviation from zonal mean) as a proxy for a "thermal forcing" (whatever this may be in the framework of a barotropic model). But in contrast with orography, the temperature perturbation is highly "internal" and must be considered as a *result* of the large wave amplitudes rather than a *forcing*. In other words, you cannot simply compute the observed (large) temperature anomalies during episodes with large wave amplitudes and use them as "forcing" in an argument that is meant to explain the large wave amplitudes. This logic is highly circular and, therefore, meaningless.

Incidentally, on line 70 you provide a very misleading description of resonance. The phenomenon of resonance (as used in Petoukhov et al. 2013) is entirely based on linear theory, so expressions like "nonlinear amplification" and "interaction between wave A and wave B" are not in place here. In linear wave theory, you can always superimpose two solutions in order to get a new solution; generally, this leads to constructive or destructive interference, but it will never give you anything resembling resonance (see my further comments in Wirth 2020b). It would be very desirable not to perpetuate such misconceptions; rather the authors should provide a lucid description that is compatible with the fundamental concept of resonance from theoretical physics and with the early work of Haurwitz (1940) and Charney and Eliassen (1949).

Irrespective of my remark in the last paragraph, I think that the algorithm used by the authors to determine resonance conditions lacks substance (for the reasons given) and cannot be used the way it is used in this paper. This challenges all reference to so-called "QRA periods" in the paper including the title.

(2) My second issue refers to the way how you interpret the results of your statistical analysis in terms of causal connections (line 95) in parts of the paper. In particular, statistical co-occurrence or increased conditional probabilities do not imply any "interaction" or causal relationship; but this is how you seem to interpret (some of) your results, for instance you use terms like "interaction" (lines 104 and 439ff), "played a role in…." (line 458), "relevance of…. for …" (line 101), "dynamical driver" (line 18, line 35), “have an effect on …” (line 181, line 207), and numerous other occurrences.

As a consequence, I do not agree with your conclusion on line 469ff (“relevant for…”, “play a role…”). To be sure, you have shown that during RRWP episodes there is a larger probability of heat wave occurrence. However, this does not imply that “RRWPs increase the duration of …. hot spells” (line 469/470). The latter formulation suggests that the existence of RRWPs makes an active contribution towards the occurrence of a hot spell. But it could be just as well the other way around: episodes of persistent hot spells (associated with quasi-stationary large-amplitude Rossby waves) may lead to your diagnostic of RRWPs indicating large values.

Let me explain. One key question that you address in this paper is: “Are heatwaves more likely to occur during periods of RRWPs or QRA?” We know that both RRWPs and QRA have a strong association with large Rossby wave amplitudes (in the case of QRA this results from what I said above), and large-amplitude Rossby waves are known to increase the likelihood for heatwaves in summer (Fragkoulidis and Wirth, 2018, and several other papers). From this perspective it appears fairly natural to expect that the likelihood for heat waves over Australia increases in case of RRWP or QRA conditions, and the question does not appear to be very interesting, or (to put it more scientifically) your hypothesis is not very daring.

The intrinsic logic of your argument and my criticism can be illuminated by a simple example. Let us ask the following question: “Are hot summer temperatures in my home-town more likely to occur during days on which the major church casts a shadow?” (Of course!, you may be inclined to say, because days with a church-shadow are those days on which the sun shines, and sun shine increases the likelihood for a hot summer day…) In the following we do a statistical analysis and find that, indeed, the answer to the question is “yes”. Everything is fine so far, because you know very well hot to perform a sound statistical analysis. However, this does NOT mean that the shadow cast by the church “causes” the hot temperatures, nor that the church-shadow is of any “relevance” for the heat, nor that the shadow interacts with any other phenomenon that may be associated with the hot summer temperatures.

To be sure, in contrast with you I am not an expert in statistical analysis. But it seems to me that after removing your somewhat inflated interpretation what remains is a (loose) statistical connection between several physical phenomena that are somehow connected by definition --- namely connected by the fact that they are all associated with large Rossby wave amplitudes.

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