Author replies

Reply to reviewer 1

We thank Volkmar Wirth for taking time to evaluate this paper and appreciate the constructive remarks. We have responded to the points inline in blue font. The line numbers in the text refers to the first submitted version:

Reviewer 1

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.*

We want to highlight that the focus of this paper is the south-east Australian heatwaves and their relation to existing diagnostics presented in the literature before, one of which being QRA. We use the QRA metric for only an empirical study. We will clarify that the paper does not aim at further exploring the QRA mechanism itself. However, we are aware of Volkmar’s recent important work on this topic and will substantially extend the discussion section to highlight it. We agree with Volkmar that further research is necessary, ideally using idealized model experiments, to explore the limitations of the current formulation of QRA conditions and develop them further. However, this is not in the scope of this paper. Therefore, we acknowledge the offer to calculate zonalised background state, but we would leave it for a more suitable outlet. We will remove Figure 13 where we suggested interactions between the three features. We will also remove other instances where we might have suggested QRA in terms of a causal mechanism and will modify the title accordingly.

On the point of thermal forcing criteria used in the QRA metric, the second reviewer had a similar comment. Please see our reply to the second reviewer’s comment.

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).

We agree that the description was not clear. Petoukhov et al. (2013) implies interference between the forced wave and free waves of similar wavelength. It is true that it is based entirely on linear theory. We will clarify the description accordingly.

(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.

Thank you for pointing out this issue. It was not our intention to imply causation based on statistical correlation analyses only. Rather, we use several lines of evidence to infer or suggest causation. These lines of evidence include processes identified and described in the published literature summarized in the introduction and the discussion section, the results of case study analyses (not all of them included in the current manuscript), and our statistical analyses (explained in more detail in the reply to next comment). However, this has not come out very clearly yet from our text and there are indeed some misleading formulations, and we will change the text accordingly.

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.

Indeed, there is robust case study, climatological and theoretical evidence that large-amplitude RWPs do increase the odds of near-surface temperature extremes. However, at least for some impacts it is not only the "simple occurrence" of an extreme (however one defines the term) that matters, but very often it is the duration of the event that is important too. Exactly this aspect (the duration) of heatwaves can be understood with the aid of the Weibull-regression analysis as this analysis quantifies by how much each quantile of the spell duration distribution shifts per unit increase in any covariate (R in this study). Again, note that the Weibull-regression analysis ultimately only diagnoses "co-variability" of large R and long-duration hot spells and not causation. However, given the aforementioned physical understanding of how individual RWPs affect near-surface temperature we propose that the duration of hot spells is enhanced by the recurrence of the RWPs.

Also, please note that there are two separate statistical analyses and apparently the text was not clear which conclusions are drawn from which analysis. The first analysis links hot spell durations with RRWPs statistically using Weibull regression, while the second is a co-occurrence analysis of RRWPs, and QRA for SEA heatwaves. We will revise the main text and the abstract to make it more clear.

Fragkoulidis and Wirth (2018) analyze large-amplitude Rossby waves. Here, we analyze "recurrent" Rossby waves, which is a subset of amplified Rossby waves where the amplified waves recur in the same phase on a subseasonal timescale. We will clarify this point in the introduction (including the formulation of the research questions) and in the abstract, and we will include Fragkoulidis and Wirth (2018) in the literature overview. The recurrence is important for the persistence of the heatwave, in the case of Australia it ensures the recurrent formation over South-eastern Australia and hence upper-level conditions favorable for heatwave formation. show that the SEA heatwaves were composed of fast-moving Rossby waves recurring in the same phase as opposed to slow-moving Rossby waves which have been studied in the context of Northern Hemisphere heatwaves. We would further argue that more emphasis has been put on Northern Hemisphere heatwaves in recent years and thus, this work is an important study in the context of Southern Hemisphere heatwaves.

On a general level, we find the question of “how long-duration hot spells come about” is a relevant and valid research question and the presented Weibull-analysis goes beyond previous studies in this regard. Moreover, the statistical relationship is also not loose, as can be inferred from the rather rigorous significance testing, we performed.

Reply to reviewer 2

We thank the reviewer for taking time to evaluate this paper and appreciate their constructive remarks. We have responded to the points inline in blue font.

Synopsis: Ali et al. analyse the relation of recurrent Rossby wave packets (RRWPs), quasi-resonant amplification (QRA), and atmospheric blocks in the Southern Hemisphere with an emphasis on southeastern Australian heat waves. Two case studies for the prominent heat waves in 2004 and 2009 motivate a climatological analysis. The authors find a significant relation between RRWPs and QRA and demonstrate that heat waves are two times more likely during QRA conditions. Overall, the study is well written and the results are clearly presented. However, I have three concerns which need to be addressed. Once these have been addressed in a suitable manner I am very happy to provide a detailed review of the manuscript.

Major:

1) According to the QRA concept, resonance can lead to high-amplitude quasi-stationary planetary waves if the combined orographic and thermal forcing pattern is sufficiently large. For the Southern Hemisphere, I assume the orographic forcing term to be of smaller magnitude than in the Northern Hemisphere and thus the thermal forcing term to dominate over the orographic forcing term. Accordingly, my interpretation is that the thermal forcing, which includes the zonal gradient of the azonal temperature at 300 hPa, becomes particularly strong during heat waves. The interpretation of the thermal forcing is therefore tricky as it could in principle be a result of the temperature anomalies rather than a forcing for large wave amplitudes. For these reasons I have doubts that the QRA concept is easily applicable during such periods and care must be taken when interpreting the results. At least a detailed discussion of this issue and how one can distinguish between forcing and results of the forcing should be included in the manuscript.

As often, when discussing the interaction of temperature patterns and corresponding atmospheric circulation cause and effect are not strictly separable in this case. Next to theoretical considerations (see e.g., Petoukhov et al. 2013), idealized model experiments would be necessary to clearly separate cause and effect under these conditions. According to Petoukhov et al. (2013) a minimum amount of effective forcing is necessary, which is implemented as an above average forcing (amplitude within the highest 60%) in Kornhuber et al.

While we acknowledge the potential effect of surface temperature fields on upper-level temperature fields during events investigated (that might be highly barotropic), we find that in the Southern hemisphere, for wave-5 for example, the waveguide condition is filtering out most days, while the number of events changes by 15% only from 151 to 129 when applying the forcing condition that relies on the 300 mb azonal temperature fields. (see Tab. 2 in Kornhuber et al. 2017 J.Clim). We will add this point in the discussion section of the paper.

2) I very much appreciate that the authors decided to include two case studies. These nicely illustrate the approach that is later used from a climatological perspective. However, to my impression the discussion of the cases is not very goal oriented. This is perhaps also reflected in *the abstract which does not include a clear outcome of the case study analyses. For example, SST anomalies are shown (Figs. 6f, 8f) but their relation to the RRWPs, blocking, or QRA is not discussed at all. If the purpose of the case studies is to explain the methodology used later on, my suggestion is to only present one case study. If the purpose is to highlight certain dynamical processes, I think the discussions in Section 3.2.1 and 3.2.2 need to be revised in a sense that the main outcomes are immediately clear to the reader.*

Thank you for this suggestion. We will modify the heatwaves case studies with a clearer focus on RRWPs to bring out our main message and remove SST composites from the figures. We will also correct labeling mistakes with blocks and Rossby waves trains in the case study descriptions and the accompanying figures. However, we prefer to stick with the two cases as the two cases show some variability: e.g., both cases have amplified Rossby waves but one of them does not show up in the QRA-metric. The discussion section will also be modified accordingly. We will also make changes to the abstract to bring out the goal of the study more clearly.

3) One main conclusion is that RRWPs exhibit a significant relation to the duration of heat waves. To my understanding this result is plausible since southeastern Australian heat extremes, for example, occur in a highly amplified flow. Accordingly, prolonged periods of high amplitude Rossby waves favour the occurrence of several heat extremes which then are identified as one long-lasting heat wave. What is less obvious but probably at least equally important is the relation of RRWPS and the heat wave magnitude. A possible scenario would be that a first upper-level ridge leads to a heat extreme which dries the soil and thus favours higher temperatures later on through enhanced sensible heating. Could the authors therefore comment on the relation of RRWPs and heat wave magnitude, and how this differs from "ordinary non-recurrent RWPs"? Such an analysis would be extremely insightful in an operational context.

The reviewer raises an interesting hypothesis about the temporally varying formation processes of heatwaves and suggests that, through soil desiccation, the long duration of heatwaves associated with RRWPs might also lead to anomalously strong heatwaves. Physically, the suggested hypothesis is plausible, as soil desiccation is a well-known contributor to so-called mega heatwaves (Schumacher et al. 2020; Miralles et al., 2014 https://www.nature.com/articles/s41561-019-0431-6 https://www.nature.com/articles/ngeo2141). However, the processes involved in the formation of heatwaves are drivers and, besides diabatic heating (i.e., surface and turbulent heating in the boundary layer), also involve adiabatic compression in subsiding air and temperature advection. It is, therefore, not a priori clear to what extent the suggested mechanism is relevant on a global scale and/or for SEA heatwaves. Martius et al. (2021) find a contribution of the soil moisture to a heatwave of several degrees in a idealized model setting where soil moisture anomalies over Australia are kept at -1STD for an entire season (https://journals.ametsoc.org/view/journals/clim/34/22/JCLI-D-21-0130.1.xml) The Fig. 1 below shows a weak positive correlation between the R-metric and the daily-max 2-m temperature over SEA. Nevertheless, we feel that the hypothesis raised by the reviewer might warrant its own study, and we will consider performing more in-depth analyses on this topic in the future and we will add that point to the discussion of the results.

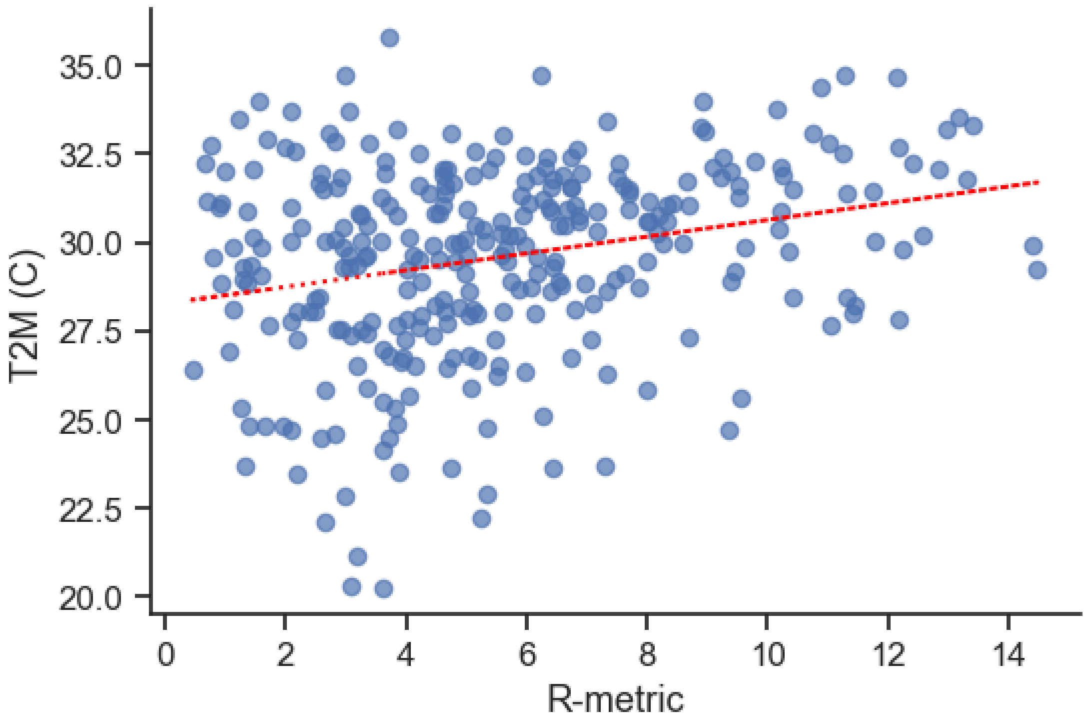

Fig 1: Relationship between daily-max 2 m temperature (T2M) over SEA and the corresponding R-metric values over SEA during days identified as part of heatwaves in this study. Daily max T2M values for SEA were calculated by averaging over 141E to 153E and 29S to 43S and applying a land-sea mask to remove values over sea. Red dotted line shows a linear fit with a slope of 0.23.

Final response to the editor

We thank Michael Riemer, the editor, for his constructive comments and suggestions. In the light of the editor's suggestions, we have omitted the QRA metric and the accompanying analyses from the revised version of the manuscript. The revised title reads as "Recurrent Rossby waves and south-eastern Australian heatwaves".

Another major comment raised by the editor and reviewer #1 was on the issue of correlation vs causation. Both the reviewer #1 and the editor interpret that from the "co-occurrence" or "co-variability" of R-metric with SEA heatwaves, we conclude that RRWPs increase the duration heatwaves. We noted in our previous response that there were indeed some misleading statements in the text, and we have removed those in the revised manuscript. We clarify the following points: the analysis from Weibull regression, where we evaluate the hypothesis whether increase in R-metric is statistically associated with increased duration of hot spells, shows that increase in R-metric is associated with statistically significant increase in hot spells, including over south-eastern Australia (SEA). This information is combined with process knowledge gathered in the published literature highlighting the important role of upper-level ridges and associated subsidence for the formation of heatwaves. We supplement this analysis with discussion of the two persistent and extreme heatwaves where we show how recurrent Rossby waves play a role to form recurrent ridges over SEA. Apparently, this point did not come out clearly from the manuscript for both reviewer#1 and the editor. So, we have stressed this point more in the revised manuscript. We again take up this point in the discussion. We have further included a short discussion on a potential soil moisture feedback.

Furthermore, reviewer #1 and the editor present an alternate causal pathway where the surface anomalies may be driving the amplified waves. We thank them for this suggestion as this possible pathway was missing from our discussion so far. We have now added a discussion of this point in the revised manuscript. Based on idealised simulations by Martius et al. (2021) we argue that the effect of the surface temperature anomalies on the upper-level flow and hence the R metric is very small. Martius et al. (2021) investigates the effects of soil moisture anomalies over Australia on the local and remote flow. An ensemble of 50 CESM simulations with soil moisture set to -1 and +1 STD over Australia are analysed. The soil moisture anomalies result in surface temperature anomalies of up to 4°C and can hence serve as a proxy for a heat wave (see Fig.1a below). The temperature anomalies do have a significant effect on the geopotential height at 250hPa (Fig.1b below) and the meridional wind (Fig. 2), however the absolute anomalies are small. The meridional wind anomalies are on the order of 0.5 to 1 m/s locally. Considering that the meridional wind is averaged latitudinally for the computation of the R metric, the absolute effect on the R-metric is very small. The link between the surface temperature anomalies and upper-level flow anomalies were derived using a model and not "simply" a hypsometric equation and therefore, include feedback processes between the temperature anomaly and the circulation such as changes in precipitation and therefore diabatic heating.

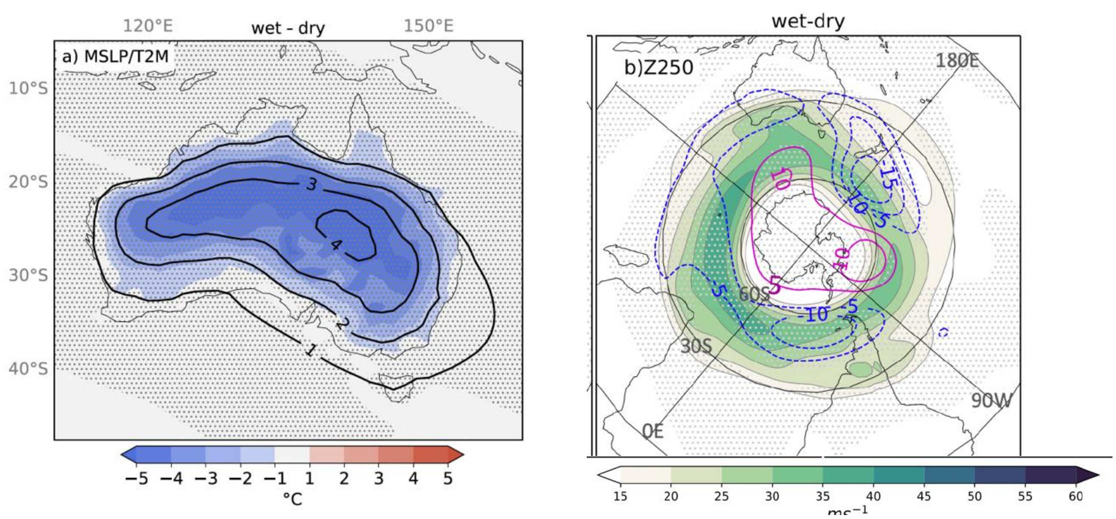

Fig. 1: a) adapted from Fig.2 in Martius et al. (2021). Colours show the ensemble two-meter temperature difference between the wet and the dry simulations, solid contours show the mean sea level pressure difference. b) adapted from Fig. 6 in Martius et al. (2021). Colours show the zonal wind in m/s and dashed lines the difference in geopotential height at 250hPa (5,10 m magenta, -5,-10m blue lines) between the wet and the dry simulations

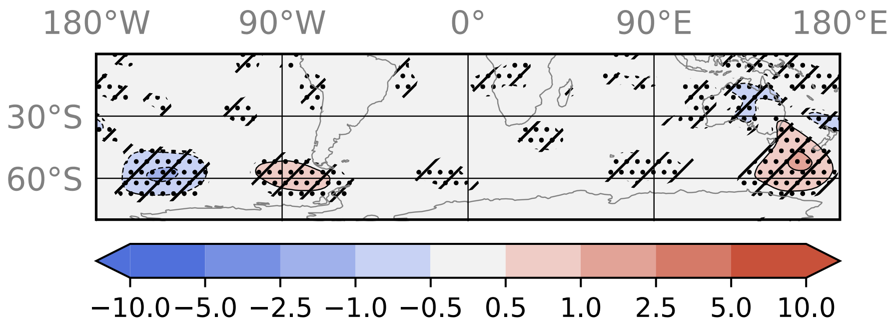

Fig. 2: Colours show the difference in meridional wind (m/s) at 250hPa between the wet and the dry simulations

These points have been taken up further in the discussion section of the revised manuscript.

Lastly, the editor also made an interesting outlook statement on the use of causal networks based on a Bayesian framework in climate sciences. In general, we agree with the remark. We add a further point below extending the editor’s statement.

As is the case with any correlation or regression-based analysis, the selection of the exposure (R in our case) and outcome variables (length of hot spells) should be carefully made with the underlying theoretical knowledge of the system. Spurious causal links between exposure and outcome may occur for example, due to confounders, that are hidden variables in the system driving both the exposure and the outcome. Such problems can indeed be addressed by using causal networks approach (Pearl 2009). As the editor points out causal networks have been applied in Climate Sciences recently to quantify causal processes, but only for process acting on a longer timescale from a week to month (e.g., Runge et. al, 2019). Some studies are, in parallel, attempting to apply causal networks on a sub-weekly scale (e.g., Ali et al., 2022). However, it should be stressed that even with a causal network approach used in the studies above, the network cannot account for hidden confounders by itself if the hidden variables are not included in the causal network setup. Although, the causal networks are mathematically quite robust and an upgrade to the currently used correlation-based approaches prevalent in climate science but, the theoretical knowledge of the system is still vital in designing and implementing any data-driven approach including causal inference.

References:

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List of changes

The major changes are explained in the reply to the editor document. Here, we list the major changes. The changes are the following:

1. QRA analysis has been omitted: Title, abstract, introduction, and conclusion sections are changed accordingly
2. Causal implications in the wordings have been changed
3. Section 3.3 has been extended accompanied with new figures (Fig. 8 and 9)
4. Figures 4,5 and figures 6, 7 corresponding to the case studies have been slightly modified with accompanying texts modified for any labelling errors
5. We have kept the two case studies as explained in reply to reviewer #2; however, we have shortened the case study description to focus on our main message.
6. Discussion section is completely changed