Dear Editor,

We have revised the manuscript “The link between European warm temperature extremes and atmospheric persistence” according to the reviewers’ suggestions. We provide a detailed response in blue to each of the comments below. We would like to thank all Reviewers and the Editor for the time they have invested in supporting the improvement of the manuscript thus far, and hope that this revised version is now suitable for publication in ESD.

Best regards,

Emma Holmberg,

On behalf of all co-authors

Anonymous Referee #2

Second review of paper

The link between European warm temperature extremes and atmospheric persistence

by Emma Holmberg et al.

submitted to Earth System Dynamics

I thank the authors for the thoughtful and thorough revision of their paper. Many of my initial concerns have been addressed: (a) the introduction to/description of the persistence measure θ is much clearer (the authors’ response to my first review also helped me greatly in that regard). (b) The study gained substance by including the analyses of Z500 persistence (I find in particular the new Fig. 2 very interesting). (c) The temperature advection analysis is now also somewhat clearer, even though I still don’t find it entirely convincing (see comments 7–11 below).

Nevertheless, I believe that this paper now meets the quality standards of ESD and I therefore recommend this paper for publication after minor revision. Note that I still have a considerable number of minor comments remaining, which, however, almost exclusively pertain to the presentation of the material and ideas, rather than the content itself. I list those comments below and invite the authors to address them when producing their final version of this manuscript.

We thank the reviewer for their time and helpful comments. Below we detail our specific answers to each review comment. We have also changed way we visualise the significance stippling in our figures to make it more visible, however, no qualitative changes have been performed unless otherwise specified.

Minor comments

1. Abstract: I find several sentences too vague for meaningfully conveying the key results of this study to a first-time reader. Hereafter, I point to several very specific wordings that I have difficulties to fully and clearly understand what they mean. Moreover, I provide suggestions for how they could be written more clearly. I certainly do not expect that you adopt (all of) them exactly but please reword these sentences to increase their clarity.

a. Line 5: "… no clear persistence signal in the …" -> " … no statistically significant persistence anomaly in the …".

b. Lines 6–7: "…, while the surface signal is very weak." -> "…, while there are no significant persistence anomalies of the surface circulation pattern."

c. Line 7: "… suggesting that other radiative and dynamical processes, as well as local effects," -> Please specify what these processes and effects could be! Do you mean maybe "… suggesting that sensible heating or adiabatic warming …"?

Thank you for your suggestions, we have amended the abstract accordingly. On ll. 6-7 we however specify "few" Instead of "no", as two regions do show a statistically significant signal.

2. L54–55: I agree with this statement for temperature anomalies built up by diabatic heating or adiabatic warming, but not for temperature anomalies built up by advection. Very strong temperature advection often occurs in association with fronts and these are themselves clearly not persistent circulation patterns and they can surely also occur within short-lived large-scale circulation patterns. Therefore, please reword or delete this statement.

We agree with your statement that fronts are associated with very strong temperature advection, yet that they are not persistent circulation patterns. In our context we have a temporal requirement for heatwaves/ warm spells, thus we have clarified in line 54-55 of the original manuscript that we are considering large and long-lived temperature anomalies (in our specific definition, large positive temperature anomalies over a period of at least 5 days).

3. L62–63: The study of Miralles et al., (2014) goes in that direction. They showed that persistent anticyclonic conditions are needed to facilitate the accumulation of exceptional heat during recent mega heat waves. Consider whether or not that reference is appropriate here.

We agree that this is a prime example of a study suggesting as much, although we chose to err on the side of caution and not directly cite this paper. As you mention, this specific study focuses on mega-heatwaves and the build up of extremely high temperatures. As we only consider occurrence and not the intensity of heatwaves, we did not want to cite literature in this specific passage of text which either explicitly or implicitly focuses on intensity, so as to not mislead the reader.

4. L130: I find the symbol $Tpotential$ for potential temperature rather confusing. Firstly, in all other symbols you use the subscript to indicate the level (e.g., "surf", or "500"). Secondly, the standard symbol for potential temperature is θ, which, unfortunately, is already taken by your measure of persistence. Please nevertheless try to come up with a less confusing notation. Also, I find it strange to read the words "potential temperature advection" as part of a numbered equation. Please give this quantity its own symbol or introduce the quantity in an in-text equation, e.g., "The surface potential temperature advection, $-\boldsymbol{v}surf \times \nabla Tsurf\ potential$, where …"

Thank you for raising this issue, we have changed potential temperature to have potential in superscript, with subscript denoting the level e.g. surf, 500hPa.

5. L142–143: Please specify in which figures you have applied the FDR test.

We have now specified that this was applied to all figures with significance testing.

6. L178–180: I only see a "canonical blocked configuration" in panels (g) and (j), panels (h) and (i) just show zonally oriented wave train/dipole patterns.

Thank you, we have edited the text accordingly.

7. Figure 4: Please adjust the spacing of the color scale so that not only the temperature advection from onshore/offshore flows is highlighted.

For ease of comparison with other figures we see a benefit in maintaining an equal spacing colour scheme. Furthermore, with the updated significance stippling the colours are in general more visible.

8. Figures 4 & 5: Perhaps specify in the methods section how exactly you computed the gradients (centered differences?). The noisiness of the resulting advection fields can quite strongly depend on the way how gradients are computed.

We have specified in the methods section, directly after defining the equations for temperature advection, that we have used the function gradient from the python package NumPy.

9. Line 214: “potential temperature advection” or “temperature advection”?

Thank you, this has been corrected.

10. Line 215: I don’t think the term “dominant” is justified here, because you have not stated over “what” exactly, advection should dominate and because you have not yet quantified the importance of these other factors. Use “significant” instead, wherever the results are indeed statistically significant?

We agree with your point and have replaced dominant with significant on line 215 of the original manuscript.

11. L226: You allude here to a “strong advective signal”, but I just can’t quite see such a “strong” positive signal in Fig. 4a–f, except over coastal seas. This is somehow puzzling, as we are looking at data from winter, during which the oceans are surely warmer than the land. So how could a positive temperature advection occur over coastal seas? Also, over the land-regions in your boxes the positive values are modest at best. Therefore I’d highly appreciate if

(a) you could explain why the largest positive temperature advection values are found over coastal seas (and not coastal land regions) and

We have performed some additional analysis to investigate the larger positive anomalies in coastal sea grid boxes as compared to the coastal land gridboxes. Fig. 1 shows composites of sea surface temperatures during warm temperature extremes, with arrows corresponding to the wind direction overlaid. We focus on panels a, b and d as these appear to have the strongest signal for warm potential temperature advection over coastal sea gridboxes. We note that here the warmest sea surface temperatures during winter in the vicinity of the warm spell region do not occur directly at the coast line. Thus, one would expect a positive signal for warm temperature advection over this cooler coastal water. Furthermore, in the Baltic sea one would typically expect the presence of sea ice, which would have a colder skin temperature than open water. The weaker signal for positive temperature advection anomalies over coastal land gridboxes could be because we are looking at potential temperature advection at the surface, which could be subject interaction effects between the land surface and the atmosphere, in particular strong radiative cooling during winter. Furthermore, advection over land is expected to be smaller because surface winds over land are much smaller than over the ocean due to the greater surface friction over land. Consequently, we also computed temperature advection at 850hPa in an attempt to capture warm temperature advection anomalies closer to the surface, but not so close as to be impacted by land surface interaction effects. These figures can be seen in Fig. 2. We appear to see an orographic signal in Fig. 2, as well as Fig. 3. Given that we consider potential temperature at the surface, we suggest that this

orographic signal could be due to diabatic effects- namely that when warm maritime air reaches the coast and is advected over topography, rain is induced, which diabatically warms the air on the coastal side of orography.

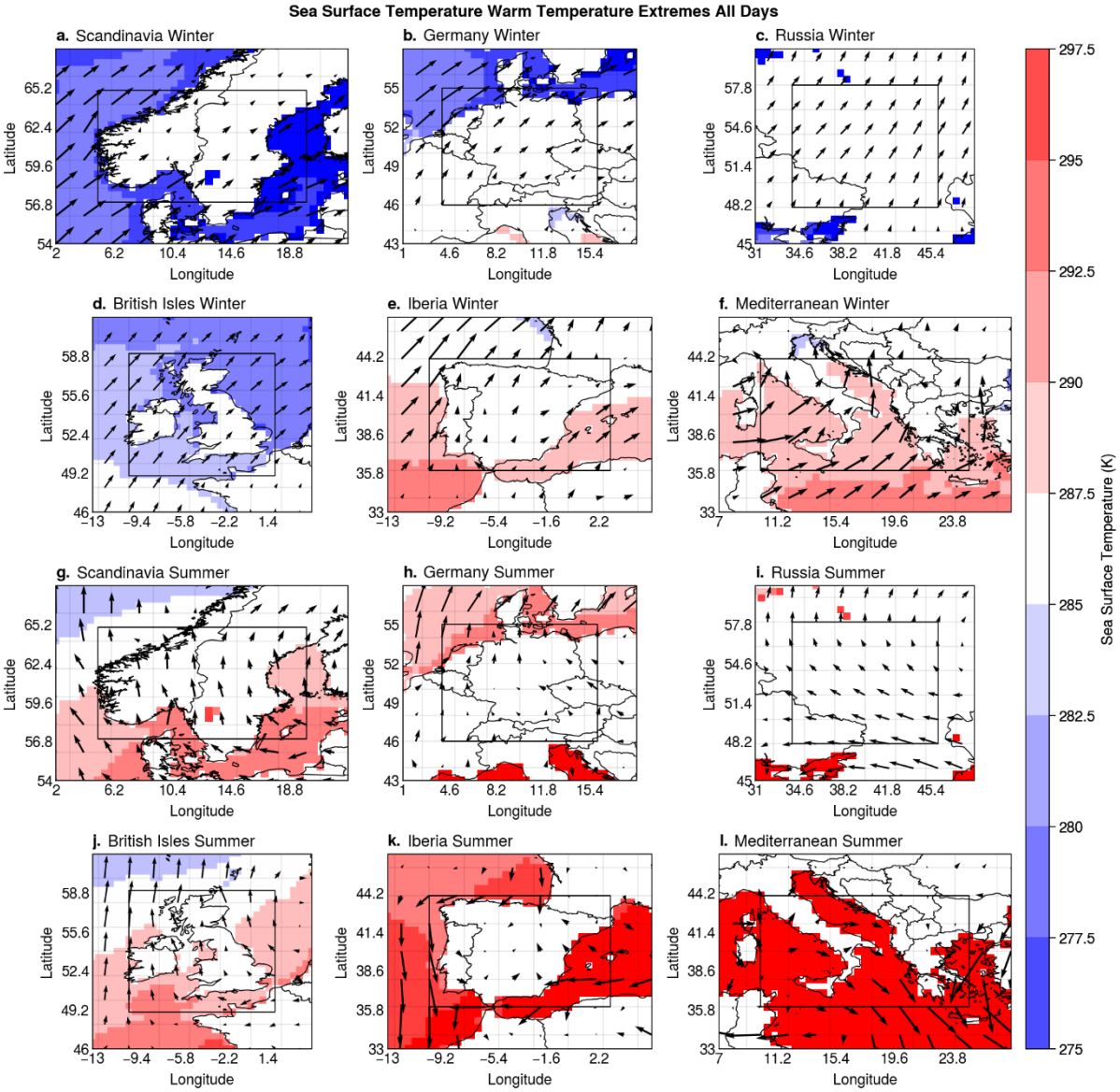

Figure 1: Sea surface temperature (K) during warm spell/ heatwave days in (a,g) Scandinavia, (b, h) Germany, (c, i) Russia, (d, j) British Isles, (e, k) Iberia, (f, l) Mediterranean, during winter (a–f) and summer (g–l). Arrows corresponding to the surface wind direction have been overlaid.

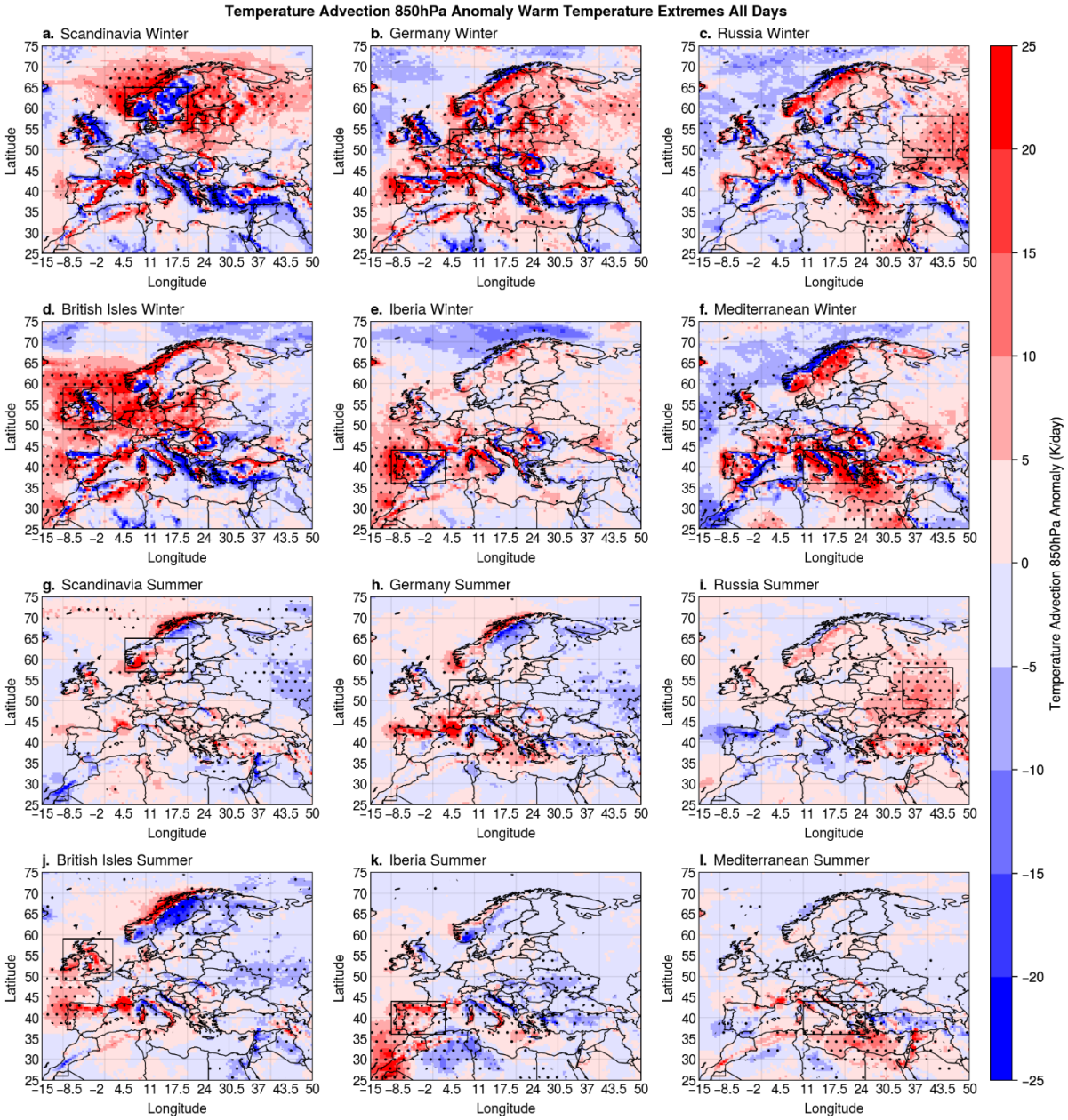

Figure 2: Temperature advection at 850 hPa (K/day) anomaly during warm spell/ heatwave days in (a,g) Scandinavia, (b, h) Germany, (c, i) Russia, (d, j) British Isles, (e, k) Iberia, (f, l) Mediterranean, during winter (a–f) and summer (g–l). Statistical significance is assessed as described in Section 2 of the manuscript and shown with stippling.

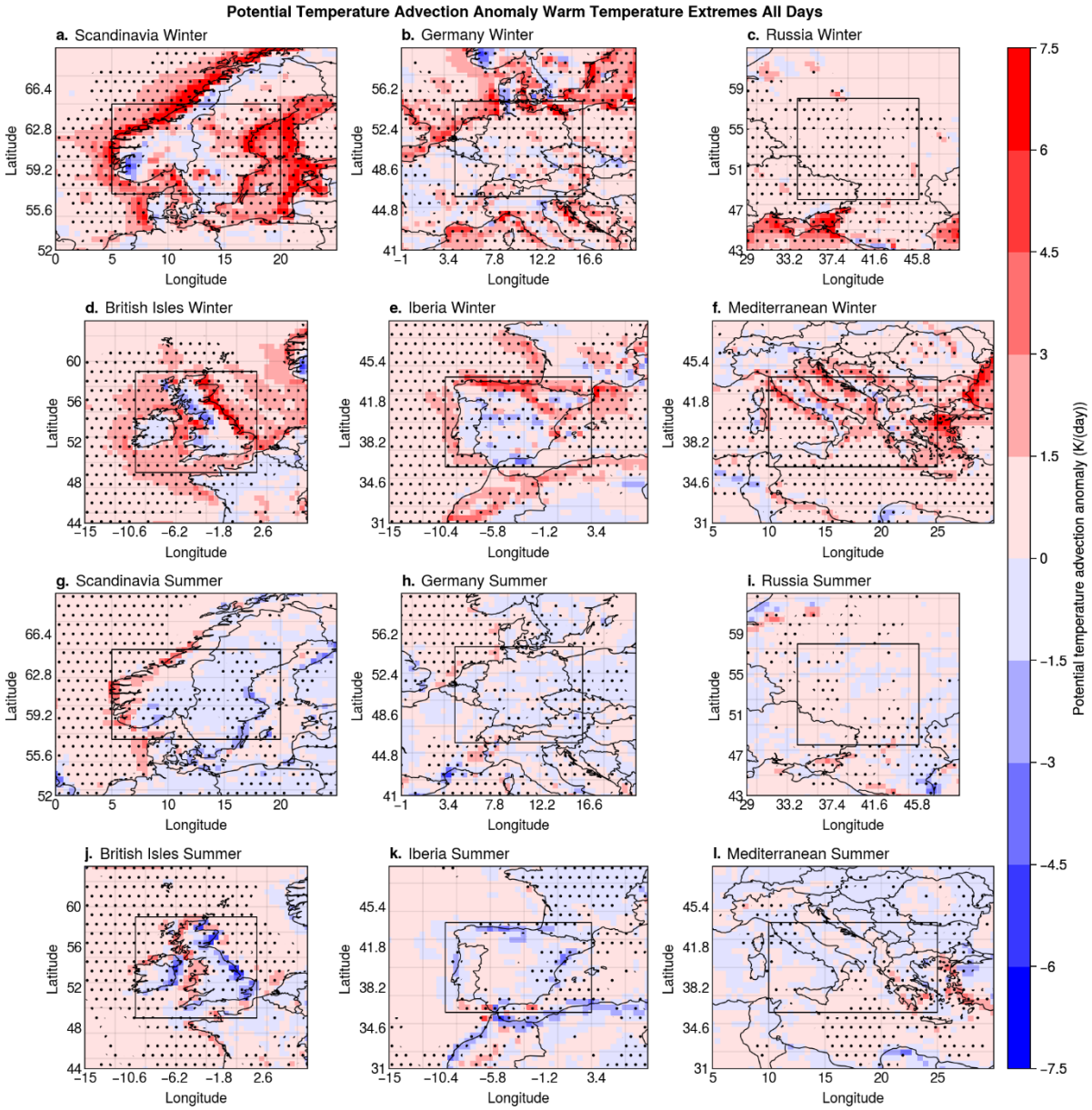

Figure 3: Surface potential temperature advection (K/day) anomaly during warm spell/ heatwave days in (a,g) Scandinavia, (b, h) Germany, (c, i) Russia, (d, j) British Isles, (e, k) Iberia, (f, l) Mediterranean, during winter (a–f) and summer (g–l). Statistical significance is assessed as described in Section 2 of the manuscript and shown with stippling.

(b) where exactly you see this “strong advective signal”.

We agree with the reviewer and have revised “strong” to “moderate”.

12. L249–251: I couldn’t quite follow here. I agree that during “blocking onset” I would indeed expect low persistence of the large-scale flow by your metric. However, I would also not expect any relevant heat waves during that phase in the life-cycle of blocks. Rather I’d expect heat waves when blocks attain their maximum intensity or even slightly later. For that later phase, however, I’d indeed expect a rather persistent (upper-level) large-scale flow.

A key point of the Lucarini and Gritsun (2020) paper is that they find the final phase of the blocks (which one would expect to be associated with heatwaves) to also be structurally unstable. We have edited line 251 of the original manuscript to clarify that we are referring to dynamical systems persistence in this context, which does not always have a 1:1 correlation with what is expected from classical large-scale dynamics.

13. L279–281: What do you mean here exactly with “new dynamical behaviors”? To me this reads like “new synoptic flow configurations will lead to heat waves”, but this would clearly not be substantiated by your results.

Here we would like to draw attention to the fact that the method of analogues in the form presented in this manuscript assumes a stationary climate. While we have shown that the last few decades are sufficiently close to stationary (from an atmospheric analogues’ perspective) that the method may be applied, this may no longer necessarily be valid in the future. We agree that our results do not substantiate the statement “new synoptic flow configurations will lead to heat waves” and this was not the message we intended to convey. We merely wish to draw attention to the fact that the assumptions made in our study may not be applicable to future climates. We have reworded the original statement to highlight this.

14. L290–291: Please be more specific about what “other mechanisms” could be.

We have amended the manuscript to suggest that adiabatic mechanisms such as subsidence could play a role.

15. L299: delete “likely”, as descending motion will undoubtably lead to adiabatic warming.

Thank you, this has been corrected.

16. Figure 8: Units are wrong in the color bar labels (Pa instead of hPa?).

Thank you, this has also been corrected.

17. Figure A11: Please increase the range of the color scale to avoid saturation in most parts of these plots. Also, the units in the color bar title do not match the units in the caption.

Thank you, this has been amended.

18. Figure A12: Please increase the range of the color scale to avoid saturation.

Thank you, this has also been amended.

References

Miralles, D. G., Teuling, A. J., Van Heerwaarden, C. C., and De Arellano, J. V. G.: Megaheatwave temperatures due to combined soil desiccation and atmospheric heat accumulation, Nat. Geosci., 7, 345–349, https://doi.org/10.1038/ngeo2141, 2014.

Anonymous Referee #3

The link between European warm temperature extremes and atmospheric persistence, by Holmberg et al

The authors have adequately addressed my comments from the first round. A few technical suggestions...

We thank the reviewer for their time and are pleased to hear that the reviewer is satisfied with the revisions in response to their comments from the first round. Below we detail our specific answers to each review comment. We have also changed way we visualise the significance stippling in our figures to make it more visible, however, no qualitative changes have been performed unless otherwise specified.

l45: "were for example used" --> "were, for example, used"

This has been corrected, thank you.

l49: instead of "simulate" here, do you mean "investigate"? I'm not really sure what is being simulated using the SLP field.

This has been amended, thank you.

l131: "where where" --> "where"

Thank you for spotting this, the repeated where has been deleted.

l189: It might be worth reminding readers here that negative theta anomalies mean greater persistence. I had to go back to the methods section to remember the sign convention.

Thank you for this comment, we have included the suggested reminder in the revised manuscript.