Dear Reviewer 1,

Thank you for your helpful comments. We have provided our responses below in blue.

The authors present an analysis of atmospheric circulation persistence with a focus on summer heat waves and winter warm spells in Europe. Their method is based on atmospheric circulation analogues that are used to estimate the persistence of a atmospheric circulation configuration. The study is overall very interesting and the approach appears to be promising. In the current version there is a lack of clarity in the interpretation of results and the conclusions drawn from the analysis.

We thank the reviewer for their valuable time and helpful comments, and detail after each point the planned changes we would make, if we are invited to submit a revised manuscript.

Major comments:

In the current state, the manuscript lacks some clarity on the interpretation of the main findings. Some passages indicate, that the presented analysis that is based on a dynamical systems viewpoint contradicts main findings coming from the atmospheric blocking community (line 158-160, line 180-183, line 196-197). In the discussion the authors explain the methodological differences between their approach and the blocking approach (line 197-...). I would say that the main difference is the definition of "persistence". The authors use a definition of persistence that analyses the atmospheric circulation over a larger region. They can therefore quantify persistence for any day in the observations and compare the persistence of heat-wave days to other days. When analyzing the persistence of blocking there is a focus on a specific atmospheric circulation pattern and the persistence of blocking is not analyzed relative to the persistence of other flow patterns. It seems as if two approaches that are useful for different research questions are compared to each other which makes some of the interpretations of the paper misleading.

We thank the reviewer for highlighting that this section of the manuscript requires further clarification, and would edit the text so as to improve the clarity of this section. We do indeed use a different definition of persistence, and do agree in part that different definitions of persistence may have more or less applicability in various situations. We agree that the persistence of blocking not being analysed with respect to other regimes is true for blocking algorithms e.g. Davini et al. (2012), however, this is not true when blocking is considered as one of the four North Atlantic weather regimes (Vautard 1990). In the latter case, the persistence of blocking is directly compared to that of the other three regimes. In a revised manuscript we would specifically highlight the difference in definitions and clarify when we refer to specific atmospheric features and when to the persistence of the regional circulation. We want to emphasize that it was never our intention to mislead the reader, and we are committed to being transparent and providing a clear and accurate representation of our research findings.

I would suggest to describe the research question more precisely and frame the interpretation of the results and the discussion along this research question. Is the research question "Is the atmospheric circulation observed during heat waves more persistent than the average persistence?" or is it "Does longer persistence of a atmospheric circulation pattern that favors heat waves lead to more intense heat waves?" or is the research question "would it be more appropriate to describe the persistence of heat waves with a dynamical systems approach".

Thank you for your feedback. Based on your first suggestion, we plan to revise our manuscript to place a stronger emphasis on a clear research question. Specifically, we aim to explore whether the circulation patterns observed during heatwaves are related to circulation patterns that display above-average persistence. In doing so, we will also explain why we have chosen to use a dynamical systems approach, as suggested in your third comment. We want to clarify that our intention is not to focus on how atmospheric persistence may modulate the intensity of individual heatwaves. Rather, our primary objective is to investigate the connection between heatwave circulation patterns and above-average persistent circulation patterns.

If the main focus of the paper is a comparison with statements from the blocking literature I would also recommend to explain these statements in a bit more detail in the introduction to allow for more clarity in the discussion.

We will add more detail in the introduction according to the reviewer's suggestion. Whilst one aspect of this manuscript is to discuss different understandings of persistence, we also aim to explain how the dynamical systems approach works to readers from a more conventional atmospheric dynamics background. We wish to highlight the differences between these approaches and familiarize a broader audience with the technique we propose, so that it may be applied in research questions where it could be useful, and not remain on the mathematical or theoretical fringes of the climate science community.

One example of an interpretation that I would question:

As the authors explain, the most persistent atmospheric flow is zonal flow (see line 71-72). If summertime heat waves occur when the zonal flow is blocked, one would expect, that the atmospheric circulation during heat waves is not anomalously persistent. To me everything seems to be as expected so far. Therefore, I would write the sentence in line 157-158 differently. To me this seems to be a misunderstanding: It might be true, that more persistent blocking leads to more severe heat waves. And this can be true irrespective of whether zonal flow is generally more persistent than blocking. I therefore also disagree with line 180-182.

In a revised manuscript we would adjust lines 157-158 to reflect that the work presented in the manuscript to that point does not necessarily contrast the existing literature on blocked flow patterns. We would like to highlight that this manuscript is only considering the occurrence of heatwaves, rather than how individual heatwave severity may be modulated by persistence. Consequently, we would revise lines 180-

182 to reflect this refining of the argument. Furthermore, we would revise lines 42-44, as upon reading the review comments it has become apparent that we have inadvertently placed weight on the link between the persistence of blocks and the intensity of heatwaves, as opposed to only their occurrence.

Advection analysis and figure 4:

Looking at figure 4 it seems as if the advection that is analyzed here shows rather small scale features. Do these small scale features really represent the large scale flow that is shown in figure 1? Due to this (potential?) inconsistency I do not find the lines 160-166 convincing. In my view, more analysis would be needed to really interpret the role of warm air advection.

Based also on other review comments, we plan to significantly revise the section on temperature advection and would investigate an alternate metric for warm temperature advection. Specifically, we plan to use the advection of potential temperature by 10m winds, and have included a revised figure below, see Figure 1. Ultimately this does not change our qualitative conclusion that winter time warm spells appear to be associated with warm temperature advection in all regions except Russia, whilst there is a comparatively weak signal during summertime heatwaves. We approach Russian warm spells with caution because the potential temperature advection signal is small and noisy, and the significance stippling also appears to be noisy.

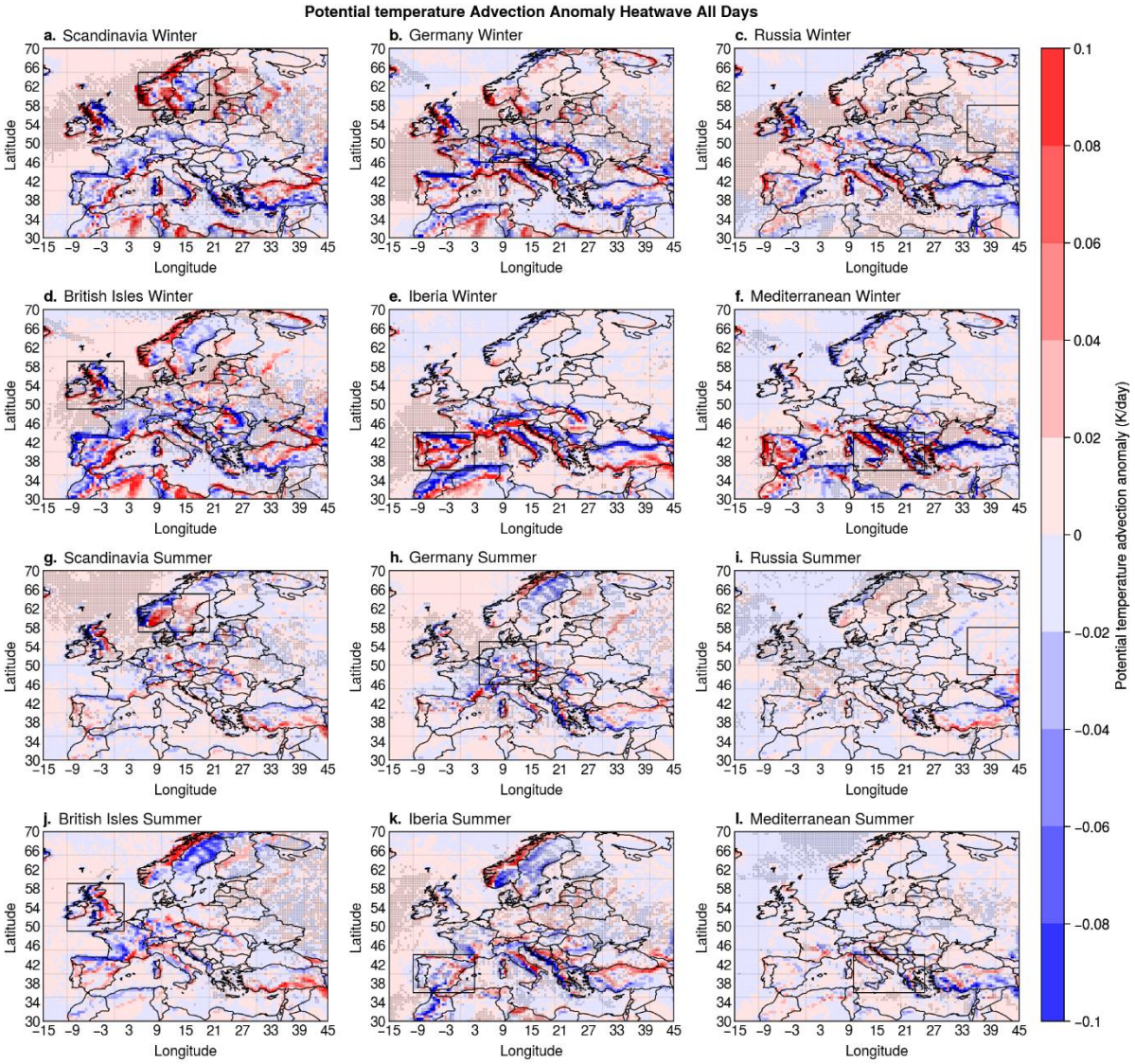

Figure 1: Potential temperature advection (K/day) anomaly during warm spell/ heatwave days in (a,g) Scandinavia, (b, h) Germany, (c, i) Russia, (d, j) British Isles, (e, k) Iberia, (f, l) Mediterranean, during winter (a–f) and summer (g–l). Statistical significance is assessed as described in Section 2 of the manuscript and shown with grey stippling.

I would assume that the analysis is sensitive to the domain over which the atmospheric circulation is analyzed. A justification or an explanation of the choice of the Europe wide domain is lacking in section 2.1. There is one sensitivity test with a shifted domain which is great, but the interpretation of this sensitivity analysis is lacking in the main part of the manuscript. Furthermore, I think it would be more interesting to test the sensitivity to the size of the domain.

The chosen domain roughly encompasses the regions over which we define the heatwaves. This will be clarified in the revised text. In a revised manuscript we would also elaborate further on the domain sensitivity tests. We have run two additional sensitivity tests for a smaller (32.5-67.5N and -12.5-42.5E) and a larger domain (28.5-72.5N and -17.5-47.5E), which we include below in Figures 2 and 3 respectively. We see that the further sensitivity studies appear to show no qualitative effect for summer. For winter we do see a qualitative change with only Germany and Scandinavia, and Germany, Scandinavia and Russia for the small and large domains respectively, showing significant negative anomalies in theta. The sensitivity studies in the previous manuscript, in particular for heatwave/ warm spell duration, also highlighted that our wintertime results appear to be less robust than those for summer. While we do see some evidence of a link between atmospheric circulation persistence and wintertime warm spells, we interpret this with caution due to the apparent sensitivity of these results. This analysis does not change our qualitative conclusion that atmospheric persistence measured using SLP fields does not appear to be a necessary requirement for summertime heatwaves.

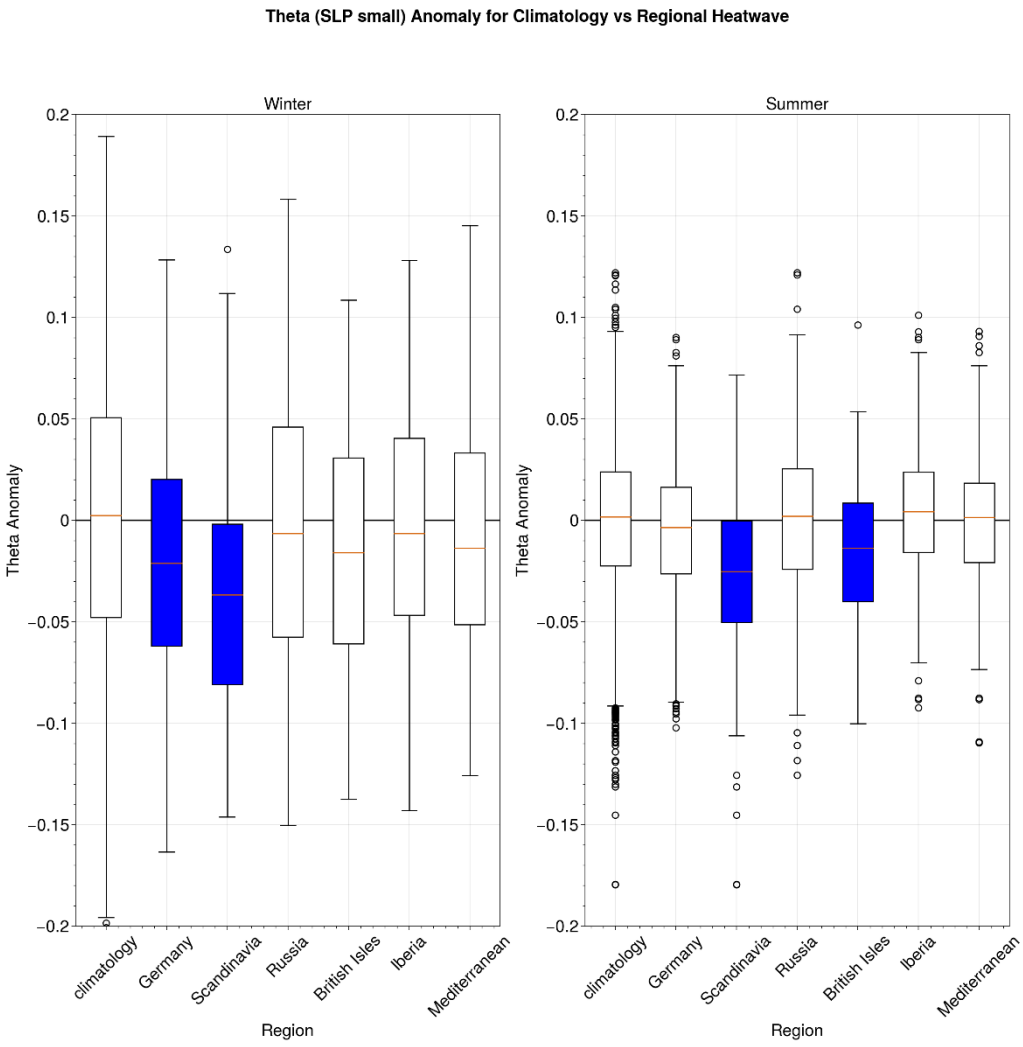

Figure 2: Box plots of θ [days^{-1}] anomalies for all heatwave or warm spell days in the considered regions. Theta has been calculated on a reduced domain 32.5-67.5N and -12.5-42.5E. Blue boxes indicate statistical significance, assessed as described in Section 2 of the manuscript.

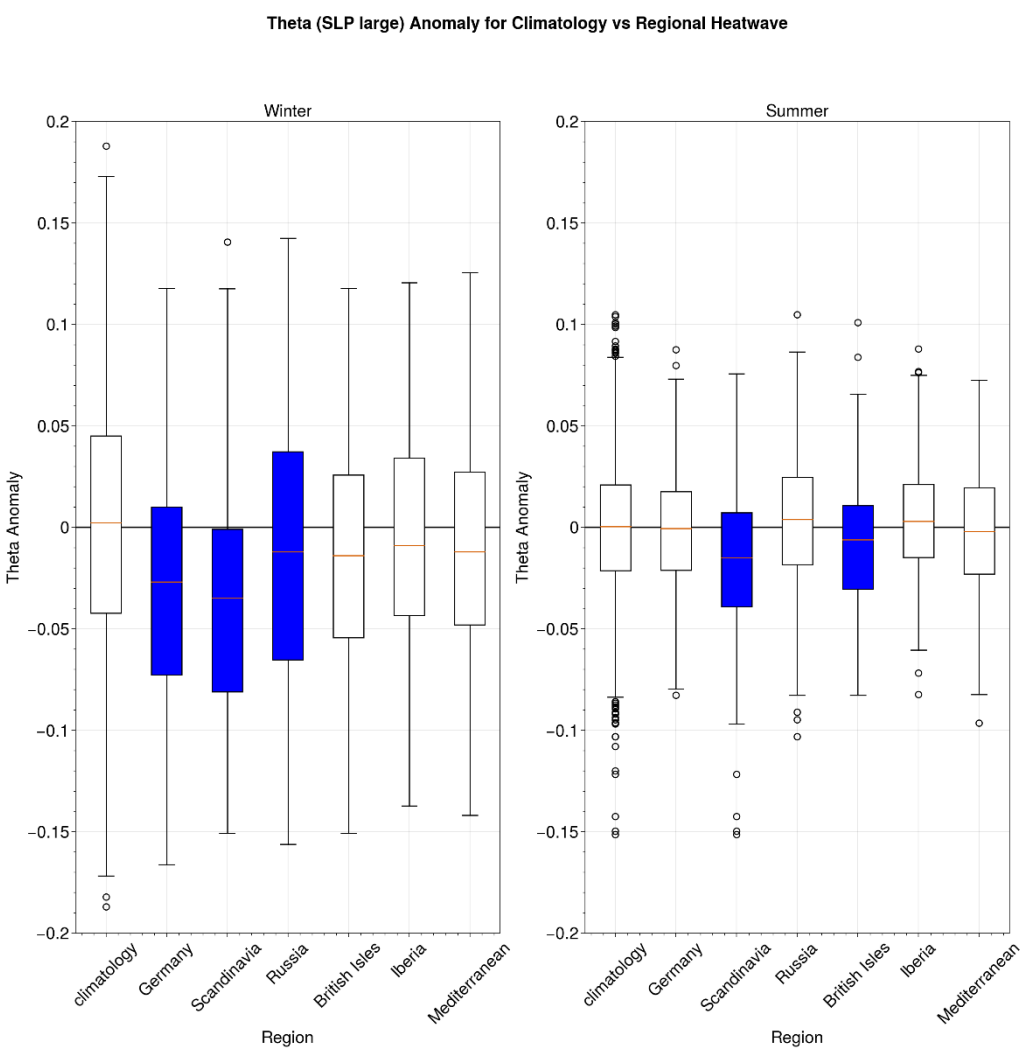

Figure 3: Box plots of θ [days^{-1}] anomalies for all heatwave or warm spell days in the considered regions. Theta has been calculated on an expanded domain 28.5-72.5N and -17.5-47.5E. Blue boxes indicate statistical significance, assessed as described in Section 2 of the manuscript.

Minor comments:

L11: exact?
Exact is meant as a verb in this sentence.

L129: Is it relevant that the reader understands the method of Süveges? If yes, please explain it in more detail. If no, I'm not sure if you need to compare it to another method (Ferro and Segers) which is not expleained either?
While the details of the estimator of theta are not directly relevant to our results, we do agree that it is important to ensure the self-contained reproducibility of our article. Based on this, and other review comments, we therefore plan to significantly expand and elucidate the method section of this manuscript, specifically adding further detail on the method of Süveges, with the goal of making the methods section as self-contained as possible.

L130: Please write the package name here (I assume it is not "Robin")
What we meant was the package “by Robin (2020)”. This will be fixed in a revised manuscript, with the package name (CDSK) included.

L184-185: This formulation could lead to a misinterpretation of the results (see my major comments above). The presented analysis does not study the link between more persistent anti-cyclonic configurations and the intensity and persistence of heat-waves.
Thank you for highlighting this and we agree with your second sentence, thus we will clarify the manuscript accordingly as this is not the message we wish to convey.

Figure A1: This sensitivity test is helpful and important. Would it also be possible to do a sensitivity test where the domain over which atmospheric circulation is analyzed is smaller, for example only the Mediterranean?
Following one of the previous comments by the Reviewer, we have run additional sensitivity tests on the domain size. Concerning shrinking the domain to a regional scale, this is in theory possible but may lead to noisy results due to the small number of gridboxes which would then be used to determine analogues. It would likely work for a higher resolution dataset, but would then require a separate analysis for each heatwave domain. We would leave this sort of analysis for future work.

References:

Davini, P., Cagnazzo, C., Gualdi, S., and Navarra, A.: Bidimensional diagnostics, variability, and trends of northern hemisphere blocking, Journal of Climate, 25, 6496–6509, https://doi.org/10.1175/JCLI-D-12-00032.1, 2012

R. Vautard, “Multiple Weather Regimes over the North Atlantic: Analysis of Precursors and Successors-Mon,” Weather Reviews, Vol. 118, 1990, pp. 2056-2081. http://dx.doi.org/10.1175/1520-0493(1990)118