Dear Reviewer 2,
Thank you for your valuable suggestions and feedback on our manuscript. We provide our responses below in blue.

Review of paper

The counter-intuitive link between European heatwaves and atmospheric persistence

by Emma Holmberg et al.

submitted to *Earth System Dynamics*

Holmberg et al. examine the persistence of the European-wide SLP field during summer heat waves and winter warm spells using a previously developed measure of persistence that is based on wellestablished concepts in dynamical systems theory. The authors find that winter warm spells are associated with increased persistence (compared to climatological persistence values derived from all days) while for summer heat waves persistence anomalies are found to be moderately positive during heat waves in Scandinavia and even negative during heat waves in other regions. The authors identify in particular the absence of positive persistence anomalies for summer heat waves as “counter intuitive”. The study is overall well written and the figures are, for the most part, clear. Also, I appreciate that authors’ attempt to leverage concepts from dynamical systems theory to put the notion of “circulation persistence” onto more solid theoretical foundations. In that sense, I would like to encourage the authors to continue exploring their avenue of research.

However, despite the good intention and interesting overarching research goal of this study, there are a considerable number of issues that significantly compromise the value of this paper in its current form. I therefore suggest this study undergoes major revisions before it can be re-evaluated again. Some of my major comments below relate to the presentation of the methodology and I expect the authors will be able to address those rather easily. However, I also see more fundamental issues related to the authors choice of meteorological analysis techniques as well as to their interpretation of their findings.

We thank the Reviewer for their time and helpful suggestions, and have addressed below how we would edit the manuscript, if invited to provide a revised manuscript.

Major comments

A) The description of how “persistence” is defined and computed in Section 2.3 is not detailed enough to convey a physical intuition of what the authors exactly mean by persistence. I appreciate that the authors employ a rigorous and mathematical definition of persistence and I am aware that previous studies already introduced this definition of persistence. However, in some instances the authors challenge extremely well accepted reasoning within the atmospheric dynamics community (Blocking patterns during European heat waves are persistent flow features) and thus they should provide sufficient detail and explanations such that peers from this and other fields (i.e., scientists outside dynamical systems research community) can understand what exactly is meant by “persistence” without first consulting other papers. For instance, why exactly is the unit of theta days^{-1}? The frequency of what is it exactly? Moreover, is its inverse something like an e-folding time of some quantity? Or on lines 127–128: Why exactly is a generalized Pareto distribution fitted to the negative log of the analogue distances? Or on lines 129–131: What is the method of Süveges (2007)? And after all: How does a measure of clustering in extreme value theory (L69–70) inform about “persistence”? Note that I am not questioning your approach, based on the current Section 2.3 it just appears overly opaque to me.

Thank you for your comments, and detailing exactly which sections need further clarification. A revised manuscript would contain a significantly more detailed methods section, with the aim of providing a more self-contained explanation of the methodology. Specific answers to these questions, which would also be included in an extended form in a revised manuscript, are as follows:

The unit days^{-1} is used because 1/theta is a measure of the amount of timesteps a given atmospheric state (or “map”) remains qualitatively similar to itself, or in other words the expected number of timesteps for a cluster of similar maps. A cluster here is simply a continuous succession of similar maps, namely a succession of several days with a similar atmospheric state. An estimate of cluster length is therefore an estimate of persistence. We use daily data, and our theta is thus in units of days^{-1}, giving a persistence time in days. Theta is not derived as inverse e-folding time.

We fit a generalized pareto distribution (GPD) to the negative log of the distances because the mathematical theories we use are asymptotic theories which hold only in the limit of a sufficiently large data set, thus we do this to ensure that we have a sufficient sample size. Furthermore, the theory requires that the negative log of the distances converge to a certain family of distributions, of which the GPD is one, and convenient to work with. In a revised manuscript, we will clarify that the method of Süveges (2007) pertains to the algorithm used to estimate theta.

B) Related to A): On line 125 you state that you choose the 5% of days with the smallest distances as analogues to any day of interest. However, presumably not all of these analogues occurred during heat waves/warm spells. It is unclear to me how that fact affects the interpretation of your results. I read in your reply to the Community Comment by Dr. Alexandre Tuel that your methodology characterizes "local properties of the attractor". I don't fully understand the exact meaning of that statement, but it nevertheless appears plausible to me that a certain large-scale circulation (defined by its European-wide SLP pattern) can be persistent in some cases, but much less so in others. That is, it is quite plausible that heat waves occur in a unusually persistent manifestations of a given largescale circulation pattern, that not always exhibits this level of persistence.

We believe that there is a misunderstanding of what "local" means, which we will make every effort to clarify in the revised manuscript. When we estimate the persistence of a given atmospheric pattern, this is not the same as the persistence of its analogues. Say that we have a heatwave on e.g. the 5^{th} July 2020, and one of its analogues happens to be the 20^{th} August 2018, which was not a heatwave day. The persistence of the atmospheric pattern of the 5^{th} July 2020 will not be the same as that of the 20^{th} August 2018. Or better said: it could in theory be the same and one could build a synthetic dataset where this is the case, but in practice for "real" data it will not be. That is because the calculation of theta rests on the distribution in time of the closest 5% of analogues of each day, and the 5% of closest analogues of the 5^{th} July 2020 will not be the same as the 5% of closest analogues of the 20^{th} August 2018, even if the two are analogues of each other. Our method enables a heatwave to be attributed to an unusually persistent configuration compared to its analogues. The term 'local' in this context indicates that the measure of persistence is specific to a single map, which refers to a day in our dataset.

For instance, previous studies have shown that soil moisture anomalies associated with heat waves can have an "anchoring effect" on the associated anticyclonic circulation (e.g., Martius et al., 2021), which only affects the persistence of a given circulation pattern when a heat wave and the associated soil moisture anomaly occurs, but not in situations in which a similar circulation pattern occurs without a heat wave. How do the authors ensure that the persistence they quantify from sets of days including both heat wave and non-heat wave days is representative for heat wave days only? In case this comment simply results from a misunderstanding of your approach, I strongly recommend rewording the description of your approach.

As suggested by the Reviewer, we will better highlight the role of non-atmospheric drivers of heatwaves in our introduction and discussion sections in the revised manuscript. Concerning the methodological part of the question, we refer to our above answer, namely the fact that our method assigns different persistence values to a given day versus its analogues. That is, it allows for the fact that one map in a set of similar maps can be more persistent than the others. To clarify this point, we show below (Figure 1) a box-and-whiskers plot of theta for a heatwave day over Germany (the cross) and for its 5% of closest analogues. As can be seen, this specific heatwave day has an anomalously low theta (high persistence) compared to its analogues. Other heatwave days may instead have an average or unusually low persistence relative to their analogues. We will endeavor to clarify this aspect of our methodology in the manuscript in order to reduce future misunderstandings. In this respect, we trust that the expanded explanation of the methodology we will introduce in response to the Reviewer's comment A will be of help.

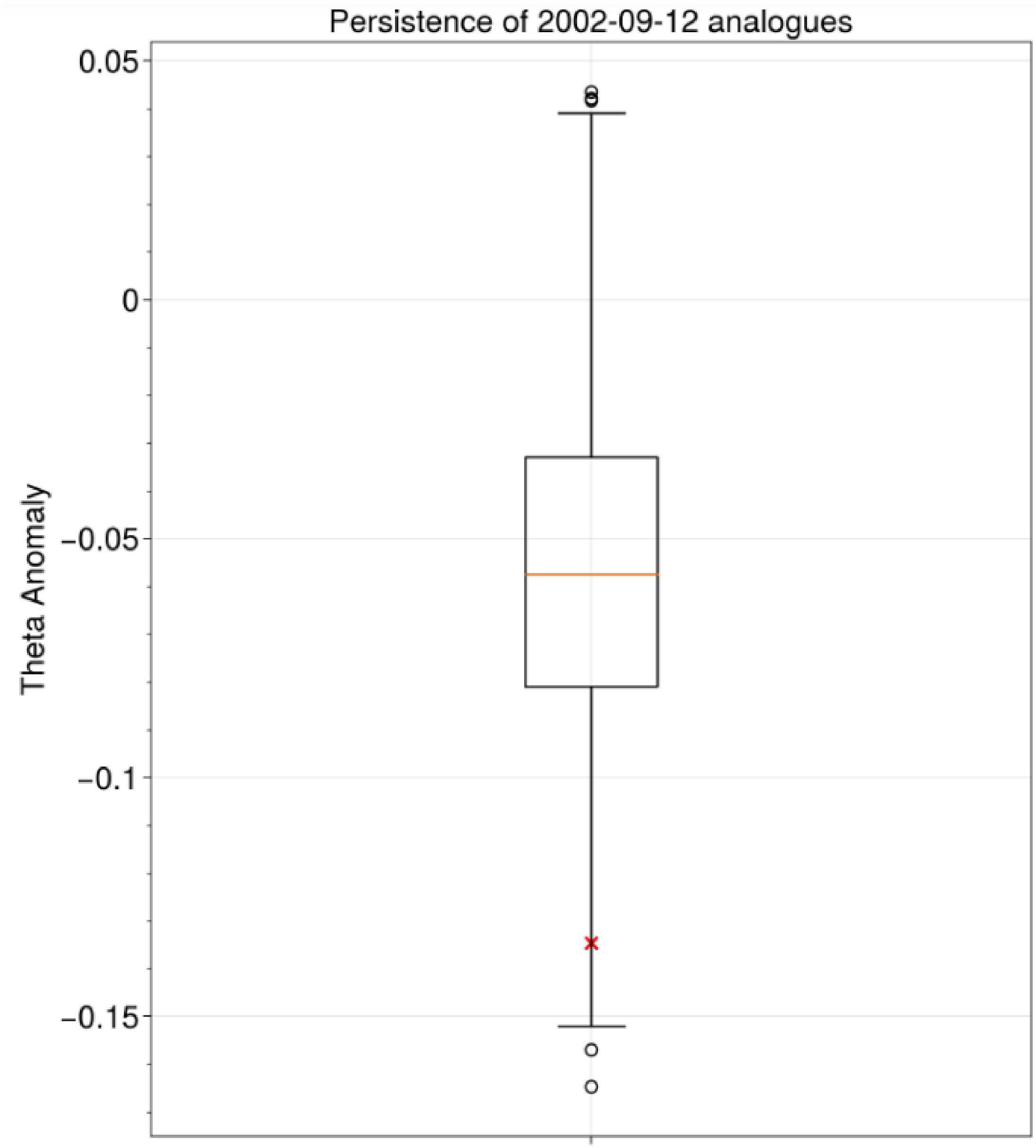

Figure 1: Box plot of θ [$days^{-1}$] anomalies calculated for all analogues of September 12th, 2002. The red cross denotes the theta anomaly for September 12th, 2002.

C) A key finding of this study is that during summer heat waves in many parts of Europe, the circulation is not anomalously persistent and the authors claim that this is at odds to the well accepted notion that European summer heat waves (at least in Central and Northern Europe) often occur in association with "persistent blocking" (e.g., lines 157–158, or 180– 182). However, a contradiction is not particularly apparent to me here. It is well possible that zonal flows (which often lead to wet and thus not anomalously hot summer conditions across central Europe) are more persistent by the author's metric than blocked flows. Nevertheless it may still be true that severe summer heat waves occur preferentially during the most persistent blocking episodes (e.g., lines 42–44 and references cited there). I therefore suggest that the authors more clearly frame their research question and better explain what exactly is counter-intuitive about their findings.

We agree with the Reviewer's point. In a revised manuscript we would clarify that our approach gives a single number quantifying the atmospheric persistence of a specific map. As mentioned in our reply to comment B, when considering a given heatwave associated with a blocked-like configuration, we are not providing the persistence of all blocked configurations, we are providing the persistence of that one specific blocked configuration associated with the heatwave we have identified. Thus, the Reviewer is correct that our results do not contradict the notion of a link between blocking persistence and heatwave persistence. We agree that we need to make the distinction between looking at the persistence of individual heatwaves and looking at the average persistence of blocked versus zonal flows. In a revised manuscript, we would clarify this point.

D) I believe Fig. 4 and the quantity B does not help to understand the contribution of largescale warm air advection to heat waves/warm spells, for two reasons: Firstly, it identifies essentially just regions of gradients in the surface elevation. The vector ***B*** is computed from SLP (which everywhere is indicating the pressure *at sea-level*), while two-meter temperature is dependent on the height of the terrain and thus large horizontal (i.e., terrain following) gradients of two-meter temperature are first and foremost indicating steep terrain. Second, the implicit assumption motivating the definition of ***B*** is presumably that by the geostrophic balance the gradient of SLP is indicative of the surface winds. However, especially near the surface the geostrophic balance is often quite substantially disturbed by surface drag and the near-surface flow is not as close to geostrophic as the upper-level flow. Therefore, I believe the authors should repeat their analysis and either consider horizontal temperature advection (i.e., $-\boldsymbol{v}\nabla T$) on a near-surface pressure level explicitly or, alternatively, consider the advection of potential temperature by the 10m winds.

We thank the Reviewer for this helpful suggestion and would now consider the advection of potential temperature by the 10m winds. 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 treat Russian warm spells with caution due to the small, noisy potential temperature advection signal, and because the significance stippling also appears noisier. We have included the revised figure below, see Figure 2.

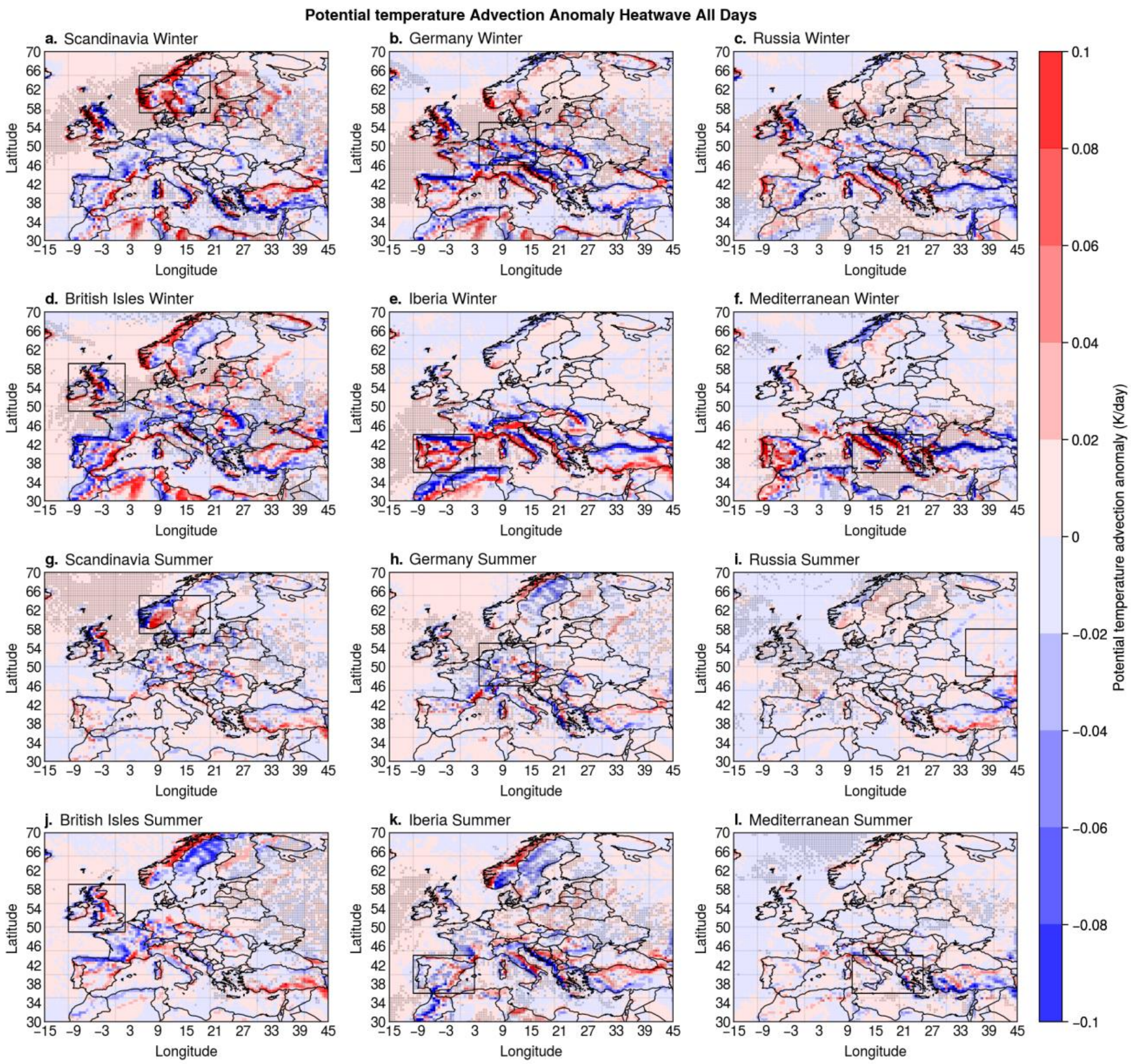

Figure 2: Potential temperature advection (K/day) anomaly during warm spell/ heatwave days in (a,g) Scandinavia, (b, h) Germany, (c, i) Russia, (d, j) British Isles, (e, k) Iberia, (f, l) Mediterranean, during winter (a–f) and summer (g–l). Statistical significance is assessed as described in Section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

E) The choice of SLP as variable to quantify the persistence of the large-scale circulation appears peculiar and suboptimal to me, in particular in the context of this study, which often makes reference to atmospheric blocking. Firstly, it is well known that during heat waves so-called heat lows can develop and thus the evolution (and hence potentially also the persistence) of the mid- to upper-tropospheric flow may differ substantially from the evolution of the near-surface flow. Secondly, atmospheric blocks often feature rapidly amplifying disturbances on their upstream side, which feed into the blocks and thereby enhance their persistence (Shutts, 1983; Pfahl et al., 2015). In the SLP field these upstream disturbances manifest themselves as extratropical cyclones (i.e., with strong signals in the SLP field). Thus, one does not a priori expect a "persistent" European-wide SLP pattern during persistent atmospheric blocks. Thirdly, it is anyways quite a stretch to argue about the persistence of blocking flows based on SLP, as blocking is predominantly a manifestation of the upper-level flow (Kautz et al., 2022; Woollings et al., 2018). The authors could easily exclude these issues, e.g., by performing their persistence quantification with (de-trended) Z500 fields or, even better, isentropic PV fields rather than SLP fields, as these quantities much more directly inform about the large-scale circulation.

Thank you for this valuable comment. Indeed, your arguments on the dynamical relevance of SLP have highlighted to us that this aspect of the manuscript requires further, detailed analysis. Isentropic PV is a relatively noisy field, yet may be worth attempting to study using our persistence indicator, perhaps after some spatial smoothing. Our original motivation for not using Z500 was that trends in the variable one computes the analogues on may lead to bias results (since one risks finding analogues concentrated in the years around the reference day, due to the trend). Detrending a variable prior to analysis may itself be problematic, since the trend is typically subtracted at each gridpoint, leading to complex distortions of the system's phase-space and analogue distances.
We will consider testing isentropic PV, and alternatively perform additional tests on how well the analogues from de-trended and non de-trended Z500 fields satisfy the assumptions underlying our computation of theta. In reasoning on your comment, an additional thought we had was to extend our computation of theta to consider two variables at the same time, which in this case could be both SLP and Z500 (subject to our additional tests convincing us that Z500 can be used to compute theta). This would reflect both the surface and upper level structures, while retaining the simplicity of having a single persistence value for each atmospheric map. As there is a significant amount of work required, we do not have any detailed results to present on these points at the time of writing this response.

Minor comments

1. L5: I do not think your study indeed "reconciles" the dynamical systems and traditional views on persistence. It rather demonstrates that your approach leads to results that diverge from traditional views on persistence.
We will reword this accordingly, however, in a revised manuscript we would still endeavor to contribute a piece of literature which helps clarify this approach in terms of the traditional views on persistence.

2. L16–17: The authors should clarify what exactly they mean with the word "extreme". Do they mean "rare", e.g., as on line 20 where the authors refer to "Uncommonly high temperatures", i.e., "extreme" in the sense of a large return period? Or do the authors perhaps mean "extreme" in the sense of "hot enough to cause impact"? If the authors mean "extreme" in the sense of "rare" then the sentence on lines 16–17 is contradictory. If extremely hot summers are defined via their rareness then, by definition, they cannot become more likely. Rather, the temperature corresponding to a certain return period will increase, i.e., events of equal extremeness will become more intense.
Here our definition of extreme is based on a percentile threshold calculated for a given time period. We will clarify that our intention was to highlight that if one defines the threshold for hot extremes based on the value of a fixed percentile of the historical distribution, in the future one will have more hot extremes. As the Reviewer points out, it is natural to assume that recomputing the value of the same fixed percentile for the new temperature distribution would result in events of equal extremeness becoming more intense.

3. L43–44: The authors could explain here in more detail how the "current understanding of the link between blocks and summertime heat waves" works physically. How exactly are long-lasting blocks supposed to increase the odds of intense heat waves? Perhaps mention here the adiabatic warming in subsiding air, the clear-sky conditions, drying of soils (due to the persistent local weather within the block), etc.
Thank you for your suggestions, in a revised manuscript we will add further detail to a physical explanation of the link between blocking and heatwaves, incorporating your suggestions. As mentioned in other review comments, we are planning to reword our comments about heatwave intensity, as we believe we have inadvertently placed emphasis on heatwave intensity as opposed to occurrence.

4. L57–60: The authors refer to a series of papers that developed the Quasi-resonant amplification (QRA) hypothesis. The theoretical basis of this hypothesis has been severely challenged by a number of recent publications (e.g., Wirth and Polster, 2021; Wirth, 2020). So far the authors of the QRA papers have not been able to

respond to or address this critique. Therefore, if the authors of this paper decide to refer to the QRA papers they should at least indicate that the validity of the reasoning in these QRA papers is severely contested.
Thank you for highlighting this, we will ensure that this point is made in the paper, with the appropriate references cited. Alternatively, we may consider removing these references entirely, as they are not central to any of the conclusions we draw in our study.

5. L69–71: Here you introduce the concept of persistence from a dynamical systems perspective. I'd find it very helpful if you could add here what "persistence" means in that context, i.e., if you could convey as physical intuition for what that term means in the dynamical systems context and, perhaps also to what extent it is comparable to the persistence of an individual blocking anticyclone identified with the feature-based perspective.
   We will endeavor to provide more physical intuition; however, this is a mathematical definition at its core so the explanation will likely always remain somewhat technical. One example which may provide a more physical understanding of persistence in the context of dynamical systems is the following: consider persistence to be a number quantifying how many days in a row a given map is expected to remain similar to itself. We interpret this as how many days in a row the atmosphere remains, in terms of the underlying mathematical system, similar to itself. This could be akin to how many days in a row do we have a similar structure of high and low pressure systems driving the atmospheric circulation pattern. We also plan to either add a graphical schematic or move the case study example to a location earlier on in the manuscript.

6. L89: Is it really 1978–2018 or 1979–2018? Did you use the ERA5 back extension? If so, why only back to 1978?
   This typo will be corrected from 1978 to 1979.

7. L91: Maybe 12 UTC instead of "noon"?
   We will edit the manuscript accordingly.

8. L93: Something is wrong with the longitudes of your domain.
   Thank you for spotting this, we will edit the manuscript accordingly.

9. L94: Is "daily climatology" the same as "calendar day climatology"? If so, consider using the latter term.
   We are using a calendar day climatology and will clarify in a revised manuscript.

10. L97: What exactly is “the local distribution”? Do you take any forced trend into account when identifying your events of interest? If not, why is that choice justified in this case?

The local distribution refers to the distribution for a given grid box. We do not take forced trends in temperature into account. Whilst there is certainly a trend, our analysis should not be sensitive to how the heatwaves are distributed in time, since we look for atmospheric analogues of each heatwave over the full time period considered. What could cause issues is if the variable we are computing the analogues on has a strong trend. This is why we originally select SLP as opposed to temperature itself, or Z500.

11. L109–114 and Figs 1, 2, 4 and 5: Given that several of the authors are excellent statisticians I am surprised that you did not apply the False-Discovery-Rate test of Benjamini and Hochberg, (1995) as detailed in Wilks, (2016). Why did you not follow the procedure suggested by Wilks, (2016) even though you clearly perform multiple test simultaneously?

The Reviewer is correct in pointing this out. We apologise for missing this important step and will include this in further analysis.

12. 12. L123: Did you include any latitude weighting when computing the Euclidean distances?

No, this was neglected due to the limited latitudinal extent of the domain. We have however verified that applying this weighting does not affect our results, and indeed the correlation between the thetas computed with and without the weighting is 0.9. We have added latitudinal weighting to the updated figures in our responses, and we will do the same for the figures in the revised manuscript.

13. Figures 1, 2, 4 and 5: The stippling indicating significance is difficult to see. Can you make it better visible? Also, please add the boxes indicating the regions to Figs. 4 and 5, so that the reader always knows what the areas of interest are.

We will amend these figures as suggested.

14. Figure 1: I’d be curious to see an analogous figure to Fig. 1 but displaying the Z500 anomaly variance. This would show where (in space) the circulation is (or is not) variable (i.e., not persistent) during your events.

We will include this figure in the supplementary material, and have included the figure below, see Figure 3. This should however be interpreted with care as it provides a local as opposed to domain-wide view of persistence.

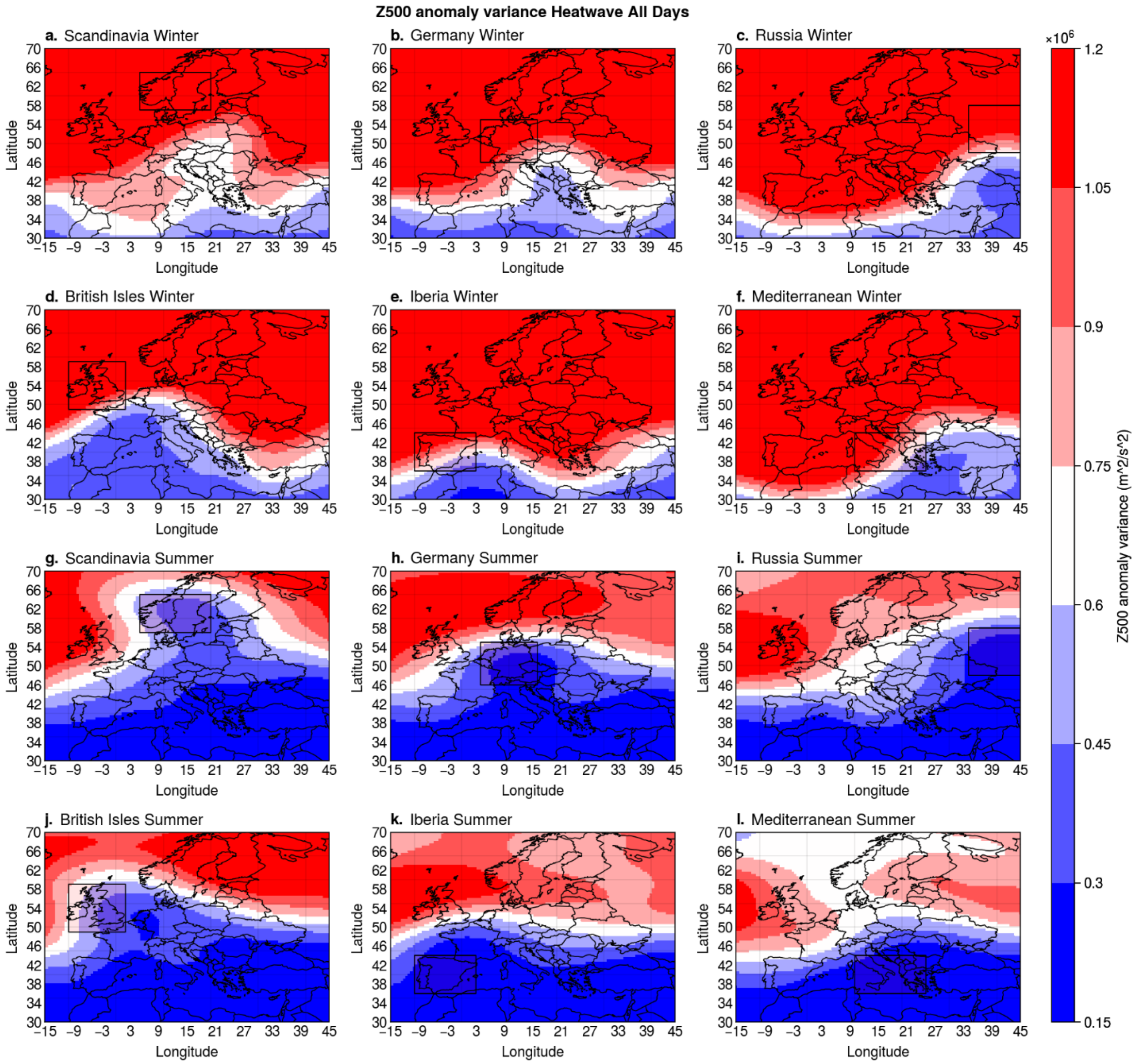

Figure 3: Z500(m^2/s^2) anomaly variance during warm spell/ heatwave days in (a,g) Scandinavia, (b, h) Germany, (c, i) Russia, (d, j) British Isles, (e, k) Iberia, (f, l) Mediterranean, during winter (a–f) and summer (g–l).

15. L154–155: I wonder to what extent your European wide circulation persistence compares with the persistence of the "local" weather during your events (e.g, measured by the average/median duration of hot spells during your events or by the T2m variance during your events). If you could indeed bring together these two views on persistence then I think the word "reconciling" in your abstract would be justified.
This is a good suggestion but we believe it hides considerable complexity. Indeed, it is similar to the question of how the persistence of e.g. a block is related to the persistence of a coinciding heatwave, something which is currently somewhat of a knowledge gap. Indeed, we were unable to find studies explicitly relating duration of blocks with duration of the associated heatwaves (while many studies implicitly look at this by imposing a minimum duration to both detect blocking and heatwaves). We intend to investigate the correlation between persistence and the duration of a heatwave, although we do not anticipate a definitive outcome. We will assess whether to incorporate this analysis into the revised paper once we have examined some initial findings.

16. Figure 3: The axis labels are difficult to read. Please enlarge them.
We will amend these.

17. L165–166: The orographic signatures in Fig. 4 are just a consequence of the definition of B. I do not think we learn anything from these signals about the importance of downslope winds for hot extremes.
We are planning to significantly revise this section following from other review comments as well, and will consider the advection of potential temperature by 10m winds, as discussed in response to comment D, where we include an updated figure.

18. L171–172: I do not understand this sentence. How exactly is "Russia an exception"?
We find that Russia appears to have a less clear case, however we will revise the manuscript to word this less strongly.

19. L201–203: The authors refer to "other atmospheric structures". What exactly is meant here?
This could be for example small low pressure system or other features in the atmospheric map which may be distinct from the most 'dominant' large scale structure.

20. L225–228: I find this statement on the importance of persistence for local (i.e., diabatic) and remote drivers (i.e., advection of air from climatologically warmer

regions) of heat waves misleading and suggest to reword this statement. Firstly, in particular for heat waves driven by radiative effects the persistence of (local?) weather (i.e., dry conditions, clear skies) is well known to be important because the amplification of land-atmosphere feedbacks, i.e., soil drying and associated increase in sensible at the expense of latent heat fluxes, take time (e.g., Miralles et al., 2019).

We will reword this to clarify that this is a key point we wish to make, namely differentiating between local conditions and domain-scale persistence, which corresponds to the large-scale structure of the atmosphere.

21. Furthermore it is unclear why warm air advection would require persistence. Even if the circulation changes over time, long-range transport of air from climatologically warm regions may occur. Moreover, at least at synoptic scales, near-surface warm air advection tends to be largest ahead of cold fronts which I would not characterize as persistent weather situations. I understand that the authors here refer to the persistence of “the large-scale atmospheric configuration”, but as it is written now the statement (i.e., the authors interpretation of their results) is at odds to existing literature and, in my opinion, not sufficiently well substantiated by their results. The issue might be resolved if a distinction could be made between the persistence in the large-scale circulation pattern and the persistence of local weather.

We will endeavor to revise the manuscript to highlight where we make a distinction between local and large-scale persistence. In our interpretation we actually make a point that the UK does not show a persistent signal during wintertime warm spells, despite showing a signal for temperature advection. We hypothesise that a persistent signal may be expected for regions where the advection of warm air is expected to come from a more specific direction, as this would likely have a stronger requirement for the large-scale circulation pattern to remain similar. The Reviewer is accurate in pointing out that our manuscript does not utilize a Lagrangian-style tracking algorithm, and our examination of advection is founded on composites. We will elaborate on this aspect further in the revised discussion section.

22. L237–239: The “visual appraisal” does not suffice to claim that “a blocking algorithm would detect a blocked flow persisting for several days”. Firstly, the large family of blocking identification algorithms derived from the original approach of Tibaldi and Molteni (1990) usually consider gradient reversals in the absolute (not anomaly) Z500 fields, which is not apparent from your Fig. 6. Secondly, the “visual appraisal” in Fig. 6 does not convince me that, e.g., the 5-day persistence criterion for negative upper-level PV anomalies in the Schwierz et al. (2004) algorithm would be fulfilled. I suggest to tone down the interpretation of Fig. 6 in this regard.

We will tone down the interpretation accordingly, however we would like to note that the first quote of your comment has dropped the word “suggests”, which in itself tones the statement down somewhat.

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