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 well-established 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.

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.

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 large-scale circulation pattern, that not always exhibits this level of persistence. 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.

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.

D) I believe Fig. 4 and the quantity B does not help to understand the contribution of large-scale 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 $\boldsymbol{B}$ 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.

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.

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.
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.

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.
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.
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.
6. L89: Is it really 1978–2018 or 1979–2018? Did you use the ERA5 back extension? If so, why only back to 1978?
7. L91: Maybe 12 UTC instead of "noon"?
8. L93: Something is wrong with the longitudes of your domain.
9. L94: Is "daily climatology" the same as "calendar day climatology"? If so, consider using the latter term.
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?
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?
12. L123: Did you include any latitude weighting when computing the Euclidean distances?
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.
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.
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.

16. Figure 3: The axis labels are difficult to read. Please enlarge them.
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.
18. L171–172: I do not understand this sentence. How exactly is “Russia an exception”?
19. L201–203: The authors refer to “other atmospheric structures”. What exactly is meant here?
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). 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.
21. 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.

References

Benjamini, Y. and Hochberg, Y.: Controlling the False Discovery Rate: A practical and powerful approach to multiple testing, J. R. Stat. Soc., 57, 289–300, https://doi.org/10.1111/j.2517-6161.1995.tb02031.x, 1995.

Kautz, L.-A., Martius, O., Pfahl, S., Pinto, J. G., Ramos, A. M., Sousa, P. M., and Woollings, T.: Atmospheric blocking and weather extremes over the Euro-Atlantic sector – a review, Weather Clim. Dyn., 3, 305–336, https://doi.org/10.5194/WCD-3-305-2022, 2022.

Martius, O., Wehrli, K., and Rohrer, M.: Local and Remote Atmospheric Responses to Soil Moisture Anomalies in Australia, J. Clim., 34, 9115–9131, https://doi.org/10.1175/JCLI-D-21-0130.1, 2021.

Masato, G., Hoskins, B. J., and Woollings, T.: Winter and summer Northern Hemisphere blocking in CMIP5 models, J. Clim., 26, 7044–7059, https://doi.org/10.1175/JCLI-D-12-00466.1, 2013.

Miralles, D. G., Gentine, P., Seneviratne, S. I., and Teuling, A. J.: Land–atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges, Ann. N. Y. Acad. Sci., 1436, 19–35, https://doi.org/10.1111/NYAS.13912, 2019.

Pfahl, S., Schwierz, C., Croci-Maspoli, M., Grams, C. M., and Wernli, H.: Importance of latent heat release in ascending air streams for atmospheric blocking, Nat. Geosci., 8, 610–614, https://doi.org/10.1038/ngeo2487, 2015.

Scherrer, S. C., Croci-Maspoli, M., Schwierz, C., and Appenzeller, C.: Two-dimensional indices of atmospheric blocking and their statistical relationship with winter climate patterns in the Euro-Atlantic region, Int. J. Climatol., 26, 233–249, https://doi.org/10.1002/joc.1250, 2006.

Schwierz, C., Croci-Maspoli, M., and Davies, H. C.: Perspicacious indicators of atmospheric blocking, Geophys. Res. Lett., 31, L06125, https://doi.org/10.1029/2003GL019341, 2004.

Shutts, G. J.: The propagation of eddies in diffluent jetstreams: Eddy vorticity forcing of “blocking" flow fields, Q. J. R. Meteorol. Soc., 109, 737–761, https://doi.org/10.1002/qj.49710946204, 1983.

Tibaldi, S. and Molteni, F.: On the operational predictability of blocking, Tellus A, 42, 343–365, https://doi.org/10.1034/J.1600-0870.1990.T01-2-00003.X, 1990.

Wilks, D. S.: “The stippling shows statistically significant grid points": How research results are routinely overstated and overinterpreted, and what to do about it, Bull. Am. Meteorol. Soc., 97, 2263–2273, https://doi.org/10.1175/BAMS-D-15-00267.1, 2016.

Wirth, V.: Waveguidability of idealized midlatitude jets and the limitations of ray tracing theory, Weather Clim. Dyn., 1, 111–125, https://doi.org/10.5194/WCD-1-111-2020, 2020.

Wirth, V. and Polster, C.: The Problem of Diagnosing Jet Waveguidability in the Presence of Large-Amplitude Eddies, J. Atmos. Sci., 78, 3137–3151, https://doi.org/10.1175/JAS-D-20-0292.1, 2021.

Woollings, T., Barriopedro, D., Methven, J., Son, S.-W., Martius, O., Harvey, B., Sillmann, J., Lupo, A. R., and Seneviratne, S.: Blocking and its response to climate change, Curr. Clim. Chang. Reports, 4, 287–300, https://doi.org/10.1007/s40641-018-0108-z, 2018.