

Revision of manuscript Egusphere-2025-6385: A life cycle definition of year-round weather regimes in the North Atlantic European region

Reviewer comments are shown in normal font, author's responses in italic green font.

The author is grateful to the two reviewers and the editor for their thoughtful review and careful recommendations on the manuscript and their general appreciation of the work.

The reviewers and editor raise similar major concerns: First, there was a question about fit in WCD and novelty of the study vs. the paper being a technical study. Second, there was concern about the length of the article. Third, there was concern about causal statements made. The revised manuscript has addressed these issues as follows:

1. **Fit in WCD:** *I am happy, that the editor confirmed the fit into the journal. I followed her and the reviewers advice to emphasise more the novel aspects about regime life cycle characteristics and dynamical aspects in the abstract, results section and in a new Summary section. The paper now should have less the character of a purely technical paper as criticized by R#2. This has been achieved by*
 - o *reorganising Section 3, now focussing only on the essence of the technical implementation while leaving details to Appendix A*
 - o *disentangling content in Section 4, particularly Section 4.3, now shortened by one page and first giving an overview of the seven regimes and their relationships (Section 4.2) and then only briefly discussing the relationship to 4 seasonal regimes (Section 4.3).*
 - o *improving Section 6, now following R#1 detailed comments how to disentangle the argumentation and avoiding confusing statements*
 - o *Splitting up the conclusions in an own Summary section and a shortened Conclusion.*
- **Length of the manuscript:** *In order to focus more on the novel and dynamical aspects, the text has been streamlined and technical aspects in the main body reduced as suggested by the editor and both reviewers. The abstract has been shortened by 5 lines, reducing the part reviewing past studies using the regimes. Parts of the technical description of Section 3 are relocated to the Appendix. Only the main technical steps are now summarised and methodological discussion remains in the main text. The content of Section 4.2 has been focussed on the relationship between regimes, and the comparison to the canonical regimes relocated in new Section 4.3 and reduced, as pointed out by R#1. However, the split in a Summary and Conclusion required a bit more lines than previously the conclusions alone. All in all the main text is now about 15-20% (150 lines, 5 pages) shorter.*
- **Causal statements:** *The text has been revised in order to avoid causal statements as pointed out by R#1.*

In the remainder of this response to the reviewers and editor I state how the individual comments have been addressed. I refer to line numbers in the track-change version. I had some formatting issues compiling the latter. This corrupted some

Citations in the Introduction. Also I was not able to track-change the deletion or insertion of Figures. Instead deleted figures appear only by deleted caption text. Finally deletion of the numbering of section heading is not highlighted. Please apologize these issues in the track-change version.

RC1: ['Comment on egusphere-2025-6385'](#), Anonymous Referee #1, 11 Feb 2026
[reply](#)

This is an engaging and informative account of the development of the seven year-round weather regimes. I am convinced of the utility of the approach, and feel the author justifies the conclusions made. I did find the manuscript quite long, and wonder whether this would deter others from reading it fully. I do find some places where a re-write could reduce the length and, partly for this reason, I recommend “major revision”. Below, I give some general comments, more specific points, and finally highlight some typos and difficult to understand text

I thank the reviewer for their overall positive statements. The revised version has shortened the main body of the text by 20 percent, as detailed above. Thank you for the helpful general and specific points.

General comments

1. I several places the words “cause” and “affect” are used when the relationship might simply represent consistency: For example, correlations between surface conditions and the regime classification might not represent direct causation, but be due to a latent/hidden variable (namely the mean of the synoptic-scale weather), which affects both the regime classification and the surface conditions.

Thank you. The paper has been revised at several places now avoiding statements about causality where no direct causation is evident. This particularly concerns Section 5.

2. I think Section 4.2 (Overview of the seven year-round regimes and comparison to canonical seasonal regime definitions) could be re-focused and shortened (see below for specifics)

Thanks for this observation and the helpful specific comments. Section 4 has been revised and shortened. In particular the presentation is now disentangled, by first explaining why seven regimes have been found an optimal number (Section 4.1), second explaining the large-scale patterns represented by the seven regimes, and the relationship between the seven regimes themselves (new Section 4.2). This is a novel result not explained at that level of detail in previous studies. Finally the new Section 4.3 explains the relationship to the 4 seasonal regimes in much more brevity and is disentangled from the description of regime patterns compared to the initial version. Also as suggested adapted versions of Appendix Figures A3 and A4 now replace Figures 4 and 6 (Now Figure 4 and 5). The discussion of the artificial 4 year-round regimes has been removed completely.

3. I found Section 6 (Variability and trends in regime occurrence 1950–2024) quite confusing (see below for specifics)

Thank you for the detailed comments and pointing out where confusion arised from. I have reworked the section, removed apparent mistakes, and clarified statements in order to avoid contradicting and confusing statements. All specific points below have been addressed and detailed answers are given below.

Specific point

Thank you for the detailed list of specific points which will be addressed in the revised manuscript. I only comment on few of them in this author's response

1. Line 122: "This could only be achieved using a life cycle definition". Why only?

The study used the objectively identified regime life cycle stages, and only by temporally anchoring on the life cycle stages, the dynamical understanding could be gained. Mere classifications or less objective categorisations of regime activity or not would blurr the signals. The sentence has been slightly rephrased:

"This could only be achieved by focussing on objectively identified regime life cycle stages, e.g. regime onset." (now line 141)

2. Line 171: "fuzzy clustering". A brief description and citation (currently given later) would be useful.

For brevity I refrain from more description here, but removed the word "fuzzy" to remain even more general here. Also the citations have been relocated to the end of the sentence. (now line 200)

3. Line 193: What about tc = February 29?

I added the lines: "29 February is omitted. In leap years, the climatological data of 01 March is used for 29 February." (now line 1086)

4. Line 203: Change "smoothed" to "running"?

Thanks, changed accordingly. (now line 1093)

5. Line 213: Change "up to" to "including"?

Thanks, changed.(now line 1103)

6. Line 223-234: "The scalar is defined as the spatial average of the climatological "temporal 30-day running standard deviation..." I find this explanation difficult to follow. Is the following in agreement with process?:

(a) For each grid point and calendar time, we calculate the standard deviation of $Z500^{LF}(gp,tc)$ over the years 1979-2019.

(b) We then calculate the 30-day running mean of these grid point standard deviations.

(c) We then calculate the area integral of these running means

(d) These area integrals are used to normalise $Z500^{LT}(t)$

(now line 1112-1124) No, this is not fully in agreement, as in step (a) already the standard deviation at each gp is not computed for a calendar time alone, but for all calendar times in a +-15 day window around the considered calendar time. Thus (a) and (b) are slightly different, than inferred and computed in one step. Therefore the notion “running standard deviation”. It is difficult to rephrase without bringing in more confusion. As a slight correction I am now consistently using the term “30-day running’ standard deviation” and added some time variables.

1. Lines 256-257: “The EOF-clustering attributes each 6-hourly time step t unambiguously to one of the clusters by minimizing the intra- and inter-cluster distances in EOF phase space”. The word “unambiguously” suggests to me that the author is setting up the fuzzy-c-means clustering to replicate k-means clustering (with no fuzziness). I also suspect that the minimisation is just based on intra-cluster distances(?). I wondered in the author could also comment on sensitivity to cluster seeding. Looking later, maybe the similarity of clusters with increasing k (Fig. 3) also demonstrates that the regimes are insensitive to seeding(?)

(now line 1147-1149) Yes, the fuzzy clustering is set up, to replicate k-means and actually attributes each time step uniquely to one of the clusters. In fact the sensitivity to the random seed is tested by repeating the clustering 10 times with different random seeds. Despite random seeds, the clustering converges to same cluster centroids thus it is insensitive to the initial seeds. This has been clarified with an additional sentence:

“Clustering is repeated, to test sensitivity to the random initial seeds and converged to the same cluster in each of the analysed setups.” (now line 1147-1149, also Line 498-500)

2. Line 280: Is it fair to say that the regimes $Z500^{*wr}$ are not standardised, and that normalisation is done separately for the each weather regime index time series so that all regimes effectively get equal weight even if the rms of projections onto some weather regimes are stronger than for others? Maybe it would be worth noting this.

Yes, this interpretation is correct. The following lines have been added:

“As $\overline{\text{Z500}^{}_{wr}}$ are not normalized according to their amplitude, the absolute values of $P_{wr}(t)$ markedly differ depending on the regime. Therefore, in the next steps $P_{wr}(t)$ is normalised so that each wr gets equal weight when comparing projections.” (now line 1169-1171)*

3. Line 292: “statistical jargon”. I find this phraseology a bit too familiar for an academic paper.

Reworded: “... sometimes referred to as ...”(now line 1177)

4. Line 315 and 316: “to some degree”. Maybe add “(to be discussed later)”.

(now line 357,434) Added reference to the section with the illustrative example now Section 3.3.

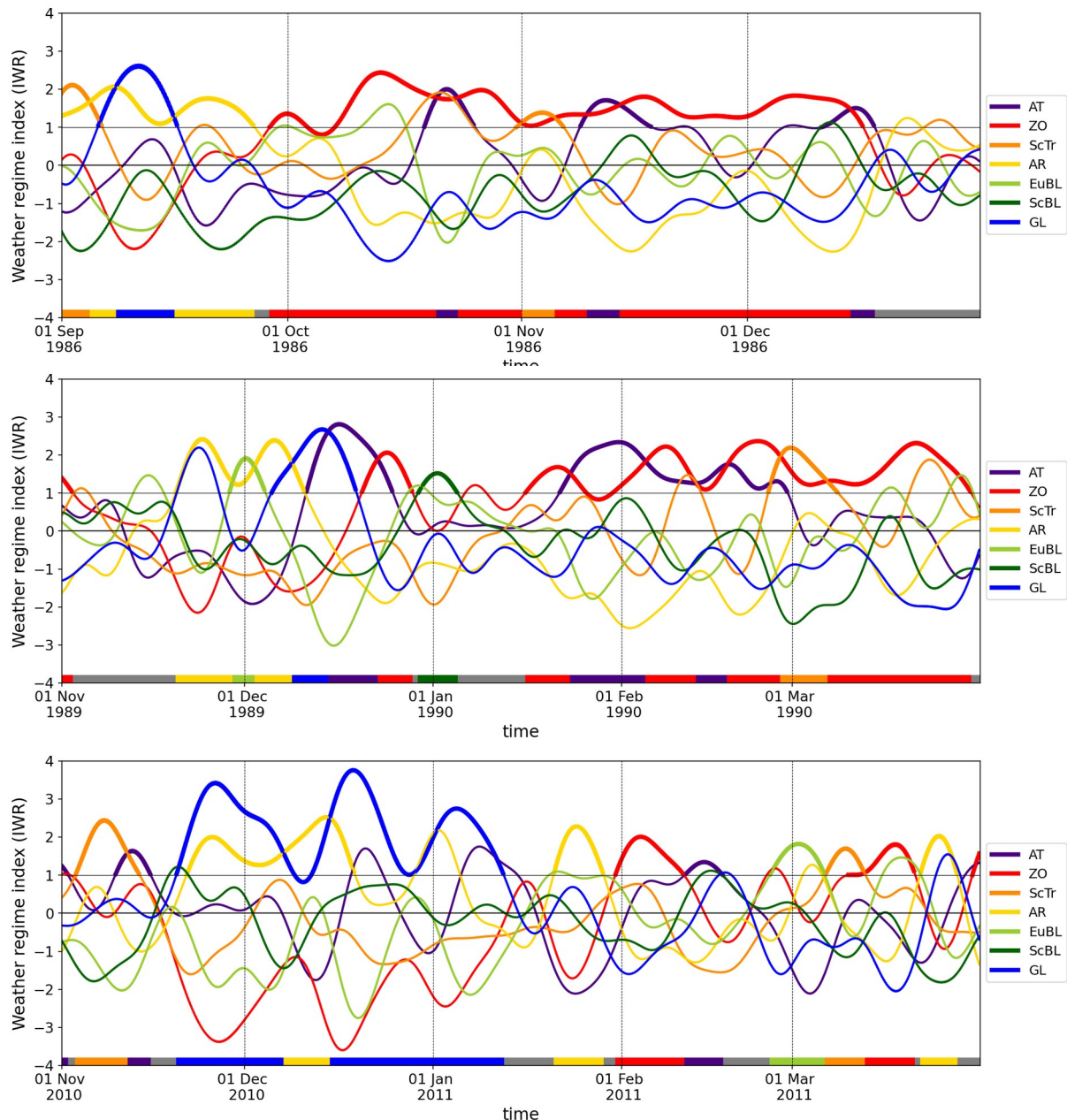
5. Line 333: I am a bit perplexed by the gradient approach to defining the start and end of a saturation period. If one imagines a Gaussian curve, there will be low gradients far from the maximum and also close to the maximum (with stronger gradients in-between). How does the approach choose between the two regions with low gradient?

This is only a secondary criterion after having identified the maximum at first. Thus it is centred around maxima. The criterion is adopted from Michel and Rivière 2011.

6. Line 352: 100 days sounds a lot. Is there some reason for this?

Roughly the 100 days cover the seasonal time scale. I noted few seasons, which were dominated by one regime, with intermittent interruption in the regime dominance. Inspection of the IWR curves would suggest that these periods should be seen as one life cycle as IWR was on average above 1.0. This triggered the implementation of the merge criterion. Set at a high threshold ensures, that such long periods will not be overlooked. It remains unlikely, that two max which are in fact apart will be merged, as other criteria such as the mean IWR will be violated.

Below I provide quicklooks for these long-lasting regimes / seasons (see also Table 1); ZO starting in September 1986, and January 1990, GL starting in November 2010



7. Line 397: “into I_EuBL”. Should this be “onto weather regime EuBC”?

Thanks, corrected. (now line 452)

8. Line 398-399: “At that time a transition into a GL life cycle begins and the ScBL life cycle ultimately decays on 3 March (dark green). This is also when – out of the perspective of the ScBL life cycle (dcto) – the transition tr to GL is objectively identified”. It looks slightly odd that the transition time is not closer to 1 March. Is it not possible (in reality) to have a transition prior to time dc?

(now lines 439ff) In the paper I decided to use only one out of the four variants of how one might define a regime transition: dcto=tr used here means by definition that, out of the perspective of the ScBL life cycle, the transition must occur after decay, but

within a 4-day window after decay. In both tr cases in the example, the transition actually occurs directly at dc, as another life cycle is already active then. You are right, one might also think that the transition should be closer to 1 March or even before. This would be the “trto” definition out of the perspective of the ScBL life cycle or, out of the perspective of the GL life cycle, the “onfr” variant explained in Section A2.3.

9. Figure 2. I see a transition “ScBL to GL” marked at 3 March, and the next transition seems to be “AT to GL”. Is there supposed to be a transition “GL to AT” in between? If not, some extra explanation would be useful.

(now Figure 1) No, there should not be a transition GL to AT in between, as the GL life cycle remains active. By definition a transition occurs only at the end of a life cycle, but not intermediately in between. However, out of the perspective of AT, the GL to AT transition is identified as the variant “trfr” out of the four transition variants. Because the question “what is a regime transition” highly depends upon the use case, I provide all four variants in the auxiliary data to users. The GL to AT transition is actually detected in AT LC id=274 as “trfr” at 20180307_12 (file “WR_lifecycle_information_AT.txt”). For brevity and simplicity I am only discussing the variant “dcto” in the paper.

10. Figure 3. Were different seeds used for each value of k? (Maybe there has to be by virtue of k changing?). If so, then this might also be a good opportunity to highlight that the clusters appear to be insensitive to the initial seed. I do wonder if this figure could be reduced to just rows 3 and 6 (perhaps with the no regime added and percentages altered accordingly).

*(now Figure 2) Yes, the clustering tests on sensitivity to the initial seeds and is repeated 10 times, thereby checking if the final cluster centroids will be the same. In all instances the clustering converged. I added a note here and in the methods, that the clusters are insensitive to the initial seeds. **Line 498-500:***

“The stability of the clusters has been tested by repeating each configuration (\$k=2-11\$) ten times. These repetitions for fixed \$k\$ always yield the same clusters. Thus, the clustering is insensitive to the choice of the random initial seeds.”

I kept the original figure, as in the literature also the number of clusters varies in different studies. The current panel shows nicely how, in a consistent detection algorithm, patterns differ depending on the number of prescribed clusters.

11. Line 470: “and a weak positive anomaly south of Iceland”. Change to “negative”?

*Thanks for spotting this. Corrected. **Line 535:***

12. Section 4.2 (“Overview of the seven year-round regimes and comparison to canonical seasonal regime definitions” is currently over 4 pages long in the double-spaced submission). I think this could be re-focused and shortened. For me, the two key points to make here are (a) that the seasonal regimes

differ over the annual cycle and (b) that all $4 \times 4 = 16$ seasonal regime patterns are well catered for in the seven year-round regimes. To this end, I would simply include in the main body of the text Fig. A3 (rows 2,3,4,5) to highlight the differences over the annual cycle, and Fig. A4 (a,b,c,d) to demonstrate that (with the possible exception of “ZO” in DJF) all seasonal regimes have a single preferred projection onto the year-round regimes. (This is in opposition to the four year-round regimes which generally project on 2 or 3 of the seven year-round regimes in Fig 6a). This means Fig. 4, 5, 6 could be eliminated (and Fig. A3 and A4 removed from the appendix).

*Section 4.2 has been shortened and simplified as suggested. The comparison of seven year-round to 4 seasonal regimes is **now in Section 4.3**. The two key points are made upfront at the begin of section 4.3: (a) high seasonal variability in the 4 regime patterns, and (b) that the seven regimes well cater for the seasonal regime patterns. A streamlined discussion has been added to highlight the key commonalities and differences between the seven and four regime definitions.*

*Figure 4 of the introducing 4 year-round regimes has been replaced with former Fig. A3 rows 2,3,4,5 (now Figure 4) to highlight the differences in the annual cycle of the 4 regimes, as well as former Figure A4 a-d inserted instead of Figure 6 to discuss the correspondence of 4 and seven regimes. **(now Figure 5)***

In any case, Fig. 4 (Fig. 5) seems to be the same as row 3 (row 6) of Fig. 3, except for a scaling difference(?). The other message from Fig. 3, that regimes become sharper as k increases, might be adequately portrayed by simply showing rows 3 and 6.

*Thanks, yes Fig. 4 was the same as row 3 in Fig.3, but Fig 5. is different as it is the mean only of the “life cycle attribution” and of the “no regime”. The “EOF attribution” in Fig. 3 row 6 showed all days attribute unambiguously to one of the seven EOF-clusters. **Fig 5. (now Fig. 3) is newly described in Section 4.2.***

13. Lines 583-587: Confusing to use the word “Europe” in the context of Scandinavian blocking, since “Europe” is used in the title for the other blocking regime. Maybe the seasonal cycle in dominance of EuBL vs ScBL relates to the seasonal cycle in mean latitude of the jet stream?

*The revised manuscript avoids this notion or changed it to e.g. “the two types of blocking over the European continent, European Blocking (EuBL, light green) and Scandinavian Blocking (ScBL, dark green), **.Line 613:..**”*

The important point is that both regimes are located more over the continent Europe compared to AR and GL. There is a seasonal preference which might be related to the mean latitude of the jet, but there are structural differences with EuBL featuring cyclonic wave breaking ScBL anticyclonic wave breaking, and both regimes can still occur in any season.

14. Line 601: “the very hot year 2003”. Change to “the very hot year 2003 in Europe”?

*Thanks changed. **Line 720:***

15. Line 602-604: “Already by visual inspection Figure 7b gives the impression of an increase of the frequency of blocked regimes (in particular ScBL, dark green, but also EuBL, light green, and GL, blue) recent decades”. Not very clear to me. Stacked plots can be a bit misleading. May be a graph with 8 curves would be clearer?

***Line 721-724:** This has been reformulated to better guide the reader and a reference to Section 6 is given. A graph with actual trends is discussed later in Section 6 Figure 15, now **Figure 14**.*

“A closer look to ScBL frequencies (dark green) in Figure 6b gives the impression of an increase of the frequency in recent decades (also for EuBL, light green, and GL, blue, but weaker)”

16. Line 612: “By definition regime life cycles have a minimum duration of 5 days”. Change to “By the current definition, regime life cycles have a minimum duration of 5 days”?

*Thanks, changed to: “by construction of the definition of regime life cycles used here, ...”**Line 732:***

17. Line 627-628: “However, for the canonical European regimes it is difficult to identify such clearly preferred regime transitions”. What has been tried and not worked? The weather regimes are based on 10-day low-pass filtered data. However, for classification, a minimum duration of only 5 days is required. Discussion of these choices (and sensitivities) would be useful. Is this choice of 5 days minimum duration inspired by/consistent with the timescales for propagation of Rossby waves (with commensurate scales to the regime patterns)? Maybe there is a timescale which optimises coherent transitions between regimes?

***Lines 747-755 :**Regarding the transitions, no tests have been done. I rather want to state here, while regime circuits might exist in East Asia, preferred transitions are poor predictors for regime sequences in Europe, as the variability of transitions is huge. Also from a Markov-chain perspective (as e.g. in Charlton-Perez et al. 2018), there are slight tendencies of preferred transitions, there are many other influencing factors triggering a regime or not.*

The choice of a 10-day low-pass filter followed a literature survey and the most common studies, e.g. Michel and Rivière (2011). Sensitivity has been tested on no filter, 2-day, and 5-day low-pass filter (e.g. in Büeler et al. 2021). However, less filtering resulted in “noisier” IWR time series, hindering the identification of life cycles. The minimum duration criterion of 5 days emerges from the discussion of simple life cycles in Michel and Rivière (2011), thorough inspection of the IWR time series, and tests with and without minimum duration criteria applied. Again the filter helps filtering out large-scale patterns, which emerge solely due to intermittent synoptic activity.

18. Lines 635-651: I am not sure readers will learn much from this discussion. Maybe it would be better to concentrate on just the most striking transitions (or lack of transitions) that the author can or cannot explain?

I kept the discussion but slightly streamlined it as I think, also the “null” result is important to note. It would be misleading to think regime X must follow regime Y. Instead this discussion emphasises that thinking in terms of related regimes is important when using regimes in e.g. a forecasting context.

19. Line 645: The transition GL to AT is striking. I’m not sure if the explanation is correct. Surely GL represents a weakening of the jet stream while AT represents a southward shift?

No, both GL and AT go along with a southward shift of the jet, in fact GL can feature a very strong southward shifted jet, as observed this winter 2025/26. However, as explained in Section 4.2, it is the question of the block (and positive Z500’) dominates, thus GL, or if the trough (and negative Z500’) and thus AT dominates. A very big advantage of the seven regims is the distinction of GL and the related AT as both go along with fundamentally different surface weather in some parts of Europe, as pioneered in Beerli and Grams 2019, and further transferred in the forecasting context by Gerighausen et al. 2025.

Line 767-770: *The text has been changed to “The latter reflects the relation of the NAO negative GL regime to the NAO positive AT, with both regimes featuring an equatorward shifted stormtrack but with altering dominance of either the block over Greenland or of the enhanced stormtrack (cf. discussion of GL and AT in Section 4.2)”*

20. Line 654: “in terms of duration the mean” change to “in terms of duration of the mean”?

Thanks, a comma was missing. The sentence now reads: “Different definitions of an extreme life cycle might be of interest, e.g., in terms of the life cycle duration, the mean I_{wr} , or the maximum I_{wr} .” **Line 775-776:**

21. Line 718: “also affect”. See general comment 1.
22. Line 723: “GL brings above average precipitation”. See general comment 1.
23. Line 738: “how regimes affect surface weather”. See general comment 1.

*Such causal statements have been avoided and reformulated: **see tracked changes in Section 5.4***

*“Weather regimes **also go along with patterns** in total precipitation similar as for wind”*

*“**During GL, above average precipitation prevails in Iberia and Western Europe,** often accompanied by atmospheric river landfall.”*

*“**Thus the composite mean can only provide a first indication of the surface weather conditions in a certain region during a specific regime.**”*

24. Figure 9 column 1, row 7 (GL DJF): Is it extreme warm anomaly to the west of the Greenland High in winter due to warm meridional advection?

Now Figure 8: *I have not investigated this specific feature. However, other studies discuss enhanced warm air advection, cloud cover, and precipitation in the region of the warm anomaly during GL but in summer, e.g. Pettersen et al. 2022*

Pettersen, C., Henderson, S. A., Mattingly, K. S., Bennartz, R., & Breeden, M. L. (2022). The critical role of Euro-Atlantic blocking in promoting snowfall in central Greenland. *Journal of Geophysical Research: Atmospheres*, 127, e2021JD035776. <https://doi.org/10.1029/2021JD035776>

Comments on Section 6 “Variability and Trends in regime occurrence 1950-2024”

25. Lines 774-776: “Recalling that the mean Z500 anomalies associated with the regimes are of an order of $O(100 \text{ gpm})$, the trends in Z500 are an order of magnitude smaller ($O(10 \text{ gpm})$) so that one may argue that the anomalies imposed by regimes still dominate over a potential signal by global trends in Z500”.

Is the author referring to Fig.1b? Does it make sense to compare magnitudes of intra-seasonal variability with inter-annual trends?

I agree, that the comparison might be far-fetched on first sight. The argument here is that from a climate mean perspective the deviations imposed by regimes currently matter more than the already detectable trends in geopotential height. In other words if the concern was, that due to an already existing trend, variability imposed by regimes would be obsolete, this comparison shows that deviations due to regime variability still dominate over the detectable trend. I have slightly revised the text to state this clearer:

now lines 908-910: “This means that deviations from mean conditions imposed by the intra-seasonal variability due to weather regimes is still larger than deviations arising from a potential trend in Z500. Therefore it is unlikely that trends affect the overall regime pattern.”

26. Lines 783-784: “We follow Lee et al. (2023) and compute the residual trend by subtracting the linear trend 1979-2019 of $5.398 \text{ gpm (10yr)}^{-1}$ from the full-trend (Figure 14c)”.

I am not sure I understand. Does the author mean “We follow Lee et al. (2023) and compute the residual trend by subtracting the area-integrated linear trend 1979-2019 of $5.398 \text{ gpm (10yr)}^{-1}$ from each the local linear trend (Figure 14c)”?

Yes, you correctly rephrased what we tried to express and we have adjusted the text accordingly.

*The confusion actually already started in original line 777-778 which wrongly referred to the full trend and Figure 14b. We have revised the paragraph as highlighted in the track-change version. **now lines 911-918***

It is also confusing to start with “We follow...” since it seems that this is only for Fig. 14c. After that, the area-integrated trend seems to be removed(?)

*The wording has been rephrased in order to avoid this confusion. **now lines 919-920***

now lines 911-920 read as follows: “Still, recent regime studies detrend Z500 anomalies in a simple manner by subtracting the area-averaged trend (Figure 14a) **from the Z500 field** (Dorrington et al. 2022, Lee et al. 2023). This accounts for the thermal expansion of the troposphere while retaining Z500 trends arising from circulation changes; i.e. changes in regime occurrence. **Lee et al. 2023 called the trend that remains after subtracting the area-averaged trend from the full-trend the “residual trend”** (Figure 14c). However,... **Here the residual trend is computed** by subtracting the **area-averaged** linear trend 1979-2019 of 5.398 gpm (10yr)⁻¹ from the **local full-trend at each grid point** (Figure 14c).”

27. Line 801-802: “The trend signal is likely of the same order of magnitude as internal variability of regime occurrence”. I am not sure I understand this, or why it should be likely.

*I removed these lines for brevity and to avoid confusion, as I feel there is no justification needed why trends in both variants of regime definitions is needed. **now lines 938-941***

28. Line 816: “depending on the period considered for linear trend analysis”. This caveat seems to confuse things. Moreover, are the cluster definitions AND the projections onto the clusters based on de-trended data. This is not clear.

This was not meant as a caveat but as a variant in computing the trend. I agree the wording was confusing and the sentence has been rephrased:

lines 951-954.

“In contrast, in the detrended regime variant, excluding the thermal expansion of the troposphere in a simple manner, no significant trend can be detected. Still, the linear trends vary in a similar range, as for the original definition, if the period for linear trend analysis is altered.”

*Yes, sure the detrended regime definition uses detrended Z500 as a starting point and thus for the cluster definition AND the projection needed to compute regime life cycles. This has been added in lines (**lines 925-926**):*

“Therefore, the regime identification has been repeated using detrended Z500 data for the computation of EOF clustering, L_{wr} and regime life cycles.”

29. Lines 819-820: “Detrending or not does not affect the overall regime identification, yet, but gives helpful insight in the interpretation of trends”. This is confusing. Maybe delete “or not does not affect the overall regime identification, yet, but”?

*Thanks, I removed these lines but retained the thought, that regime patterns are insensitive to the detrending:**now lines 957***

“2) Detrending does not affect the regime patterns and gives helpful insight in the interpretation of trends.”

30. Lines 823-824: “Notwithstanding, overall higher geopotential height amplifies the impact of blocking in terms of surface weather, in particular with respect to summer heat waves”. This seems very speculative to me.

*I have added a reference to Pfahl et al. 2012 and Chan et al. 2022 to corroborate this statement. They show a substantial contribution of the blocking-heat wave relationship to the amplitude of heat in past and changing climate. Also I rephrased the statement **now lines 961-964**.*

“Notwithstanding, overall higher geopotential height and temperatures likely amplify the local impact of blocking in terms of surface weather, in particular with respect to summer heat waves \citep{Pfahl:2012, Chan:2022}.”

Pfahl, S., and H. Wernli (2012), Quantifying the relevance of atmospheric blocking for co-located temperature extremes in the Northern Hemisphere on (sub-)daily time scales, *Geophys. Res. Lett.*, 39, L12807, [doi:10.1029/2012GL052261](https://doi.org/10.1029/2012GL052261).

Chan, P.W., Catto, J.L. & Collins, M. Heatwave–blocking relation change likely dominates over decrease in blocking frequency under global warming. *npj Clim Atmos Sci* 5, 68 (2022). <https://doi.org/10.1038/s41612-022-00290-2>

31. Lines 862-863: “This is likely due to the thermal expansion of the troposphere under global warming, which is regionally amplified in that region”. I thought the author was not accounting for regional anomalies in trends in the case were the ScBL signal disappeared (?)

*The statement here refers to the original data showing a significant trend, but yes the relative clause referring to regional patterns was wrong here and confusing. It is moved in order from the conclusions (**now lines 1032-1033**) to avoid redundancy with the new Summarising section. The statement is now made under bullet 4 of the summary (**line 1007-1008**).*

Typos

Thanks for spotting these. The Typos have been corrected.

1. Lines 15 and 172: seamingless replaced with seamless?
2. Lines 31-35: These two sentences are difficult to read

The sentences have been simplified by splitting them up.

3. Lines 80-81: “Another perspective looks at regimes in the eddy-driven jet and could be reconciled with the regime perspective”. The two uses of regime here is confusing.

The intent here is to briefly discuss other approaches than the weather regime perspective on large-scale extratropical circulation variability. This has been made clearer in the revised manuscript.

4. Line 89: “an” to “and”

Corrected

5. Line 617: Change “exemption” to “exception”?

Corrected

Citation: <https://doi.org/10.5194/egusphere-2025-6385-RC1>

RC2: ['Comment on egusphere-2025-6385'](#), Anonymous Referee #2, 20 Feb 2026
[reply](#)

This paper was a very interesting read - as someone who has followed the Gramms groups outputs since publication of the original ERA-interim based set of patterns in 2017. This is as the author intended, an excellent detailed summary of the methodology for any new users of the year-round weather regime methodology with access to the dataset in zenodo (this downloads fine and has a high quality documentation attached).

Thank you for your careful review. I am happy about your overall positive feedback.

My first question here – to the editors and author – is if weather and climate dynamics is the right home for this work as this is a dataset description paper, with minimal scientific advancement from the perspective of fundamental meteorology/climate science. From what I can see the advancement is limited to section 6, since upgrading from ERA-int to ERA5 makes next to no difference in the assignment/implementation of the methods. I emphasise I am not denying the usefulness of this work, just if a transfer could ease the review process. Some comments on the attached manuscript are below, but it may require some relatively major revision to the text/further analysis to sit in WCD.

I understand your concern and consulted the editor prior to pursuing with the final author response and revision. She answered in an editorial comment, confirming the scope in WCD. At the same time I agree, that novelty in terms of dynamical aspects must be better highlighted and were too hidden in Section 4 and Section 6. Both sections have been revised substantially as well as the abstract rewritten in order to better access the novelty right from the start. Technical aspects have been deemphasised and relocated in the appendix. A new Summary (Section 6) explicitly provides an overview of the novel insight by the study. With these revisions the fit in WCD should be assured.

General comments

1. Abstract: consider if keeping in WCD increasing the description of the novelty of this work as the first 15 lines currently review/introduce the methods history.

Thanks for the comment. The abstract has been shortened by 5 lines now counting only 20 lines. The review has been removed. Instead the second paragraph summarises the novel aspects straight away and the dual intent of the paper is briefly explained in the last paragraph. The latter concerns (a) novel insight in regime characteristics, newly including a discussion of potential trends, and (b) synthesising past work and providing an overview and fundamental documentation of the established year-round regimes.

2. L87, weather types are sometimes referred to as 'weather patterns' e.g. Neal et al., (2016) which you cite.

I now introduce and use the terminology "weather patterns", too. (now lines 105-108)

3. L169-185: This could be better placed in literature review than methods.

now lines 199-222. Yes, it is kind of a review, but with a focus on the technical aspects. As the introduction is already relatively long and in order to ensure fit in WCD, I kept the paragraph here when revising Section 3. It gives important background, needed for the argumentation of choices made for the implementation of year-round regimes. An alternate I considered, but also withdrew, was relocating it in the technical Appendix.

4. I would suggest moving Appendix A1 into the main text of the paper as this is the scientific advancement presented in this paper compared to previous studies. Similarly Figure A1.

Thanks for the suggestion. I left the comparison of the former ERA-Interim and new ERA5 based regimes in the appendix after careful consideration. The main reason is that this is more of a technical novelty, than dynamical insight. For the scope of WCD I aim to deemphasise technical aspects. Relocating Appendix A1 in the main body would have gone in the opposite direction. I still summarise the key finding of hardly any difference in the abstract and introduction.

5. L210: Is the result significantly different without the low pass filtering?

The EOF-clustering attribution will be "noisier" in terms of temporal coherence of EOF attribution of consecutive time step, but the life cycle definition also filters data to longer periods. Still we found the IWR time series noisier, and life-cycle definition trickier without filtering. Former regime classifications were applied with and without filtering. A main reason for applying the filter, is that removing synoptic scales with a low-pass of 10-days, allows focussing on the regime scale.

6. Figure 1: Can you add a axis label to the colourbar? Figure 1 also appears in the text before it is referenced (check throughout).

(Now Figure A1). A label has been added to the colourbar. The figure is relocated in the technical appendix. The position of figures has been changed to appear close to the reference.

7. L246: There is a mix of We/I in the paper, and a mix of active and passive voice that could be made consistent.

The use of We has been replaced by I, and generally tried to be minimised if other subjects of the sentence were possible, or passive voice can be used. At the same time for style reasons, I also learned that active voice reads better, so I have to compromise here.

8. L247: The other methods often use a cosine based latitudinal weighting on the grid to make sure that there are no biases to larger grid box areas. Have you also done this?

Yes, cosine weighting has been applied and this was mentioned in footnote 3 (now footnote 4 in Appendix A1, page 48).

9. L254: How does this work? e.g. The difference of the fuzzy-c-means from k-means that is used in the Cassou (2008) method?

The fuzzy c-means clustering is configured in a way, that it mimics k-means clustering, i.e. there is no fuzzy cluster attribution by a unique attribution to only one cluster as for k-mean. The sensitivity to the random seed is tested by repeating the clustering 10 times with different random seeds. Despite random seeds, the clustering converges to same cluster centroids thus it is insensitive to the initial seeds. Reviewer #1 had a similar comment.

“Clustering is repeated, to test sensitivity to the random initial seeds and converged to the same cluster in each of the analysed setups.” (now lines 1147-1149)

10. L271: Remind readers this was first described in Bueler et al., 2021 and you're expanding here. Rather than this paper 'newly introducing'.

***Now line 1157** The life cycles come with the year-round regime definition which was first described for ERA-Interim in the methods of Grams et al. (2017) but in a minimalistic way. If at all, the latter should be cited here. But none of the studies later using the life cycles has properly introduced the technical details. In Bueler et al. (2021) we tried to deliver at least some more technical details, but mainly concerning the normalisation. Notwithstanding, in these lines I mean, that “the year-round regime definition” itself introduces (and comes with) a life-cycle definition and am not referring to a particular study. To fill this gap the full technical description is only provided in the current paper. In that sense I am expanding here, but only in terms of the documentation in the peer-reviewed literature.*

11. L308: I was not sure what you meant by 'catch up':

*I meant “follow up” and changed the wording accordingly.
“Here I follow up the original idea and expand it” (now line 1187)*

12. L318: I found the quantitative descriptions more useful than the qualitative statements, so these could come after the quantitative descriptions for readers who require them?

Thanks for the hint. In the technical description (now Appendix A3.2.) the life cycles stages are now directly explained quantitatively and these qualitative descriptions

have been removed. They remain in the shortened main text with reference to the details in the Appendix (see now lines 356-360).

13.L328: Why is 5 days used as a threshold?

The length of the period of a mean increase / decrease centred around a maximum of IWR follows similar criteria for the simple life cycles defined by Michel and Rivière (2011). It also reflects the same time period that is required as a minimum duration of a life cycle. Together these criteria help filtering out large-scale patterns, which emerge solely due to intermittent synoptic activity. (See also answer to specific point 17 by Reviewer 1)

14.L363-365: I appreciate the statement about criteria selection but are you able to provide any more details as to why these decisions are optimal/ideal?

Here is a level of subjectivity which I transparently articulate in this paragraph. It is difficult to pin down why I see this an optimal/ideal choice. I try to argue in this response but did not add more to the discussion in the paper:

First, I widely shared the regime definition and presented it in the scientific community. I never experienced stark critics on the remaining subjectiveness. Second, from 2017-2023 I had the chance to dedicate research on the dynamics of the seven regimes with an entire group. We always questioned the subjective choices, like minimum duration, low-pass filter, etc. For the latter we considered removing it or weakening it (no filter, 2-day, 5-day low-pass (the latter as in Büeler et al. 2021), but in the end we were unhappy due to noisier life cycles and after six years of intensive research we agreed together that the original version, with the slide amendments made (as discussed in former Appendix A1, now Appendix B) would be an “optimal” choice.

This, many of the criteria follow established procedures in the literature. In fact the life-cycle definition only expands on choices already made in Michel and Rivière (2011). I hope this is a satisfying answer.

15.L389: reference Figure 2 in first sentence.

Added.now line 444

16.Fig 2: it's a bit hard to read the 'on' and 'oc' and 'tr' on the graph, can you offset them slightly from the line centre or give the text a white background? It would also be useful to have the IWR maximum regime (i.e. which is furthest above 1) as a bar like the unambiguous categorical definition so it is easier to compare the differences when using the lifecycle definition.

Now Figure 1, p15: *A transparent background has been added to enhance readability. I have not added the $\max(IWR) > 1.0$ in this example, as this hardly adds*

information in the example shown. In fact, there would be a bit of light green marking EuBL at the begin of the period. Otherwise it would be the same as the life cycle attribution. As the paper is already long, I am hesitant of further discussing this aspect. However, I concur that the max IWR is interesting in the context of forecasting. This has partly been discussed in Grams et al. (2020), and will be one of the option for operational forecast products which are currently being implemented. I think a forecast product documentation would be the right place for a discussion of alternate categorical attributions to the life-cycle attribution.

17. Section 4 lines 414 -430 can be removed. This information is present in the introduction.

*I admit that this information is also present in the introduction. However, the introduction focusses less on the technical identification of regimes and questions of the optimal number, as it is the case in this introductory paragraph to section 4. Furthermore the information is scattered across the introduction. I still see value in this introductory brief review focussing on earlier work about the 4 seasonal regimes, as the sections aim is to present convincing evidence, that in a year-round perspective seven regimes make sense and what their key characteristics are. The paragraph has been shortened and streamlined (**now lines 470-491**)*

18. Figure 3: This is interesting, but could be made supplementary and focus on Figure 5

*Figure 3 (**now 2**) has been kept, but the discussion in Section 4 streamlined in order to better make the point how seven regimes are determined. The regime patterns are only discussed along Figure 5 later, now Figure 3 in the new Section 4.2.*

19. L453: I don't not really agree about the patterns look blurry they look very similar to me, can you direct my eye to the key difference points I should see?

*The revised manuscript avoids this jargon and makes more quantitative statements about the amplitude of the anomalies shown. Key differences are e.g. for the 4 regime clusters the maximum positive anomalies of just 140 gpm for GL and AR compared to widely above 140 gpm, yet 180 gpm for seven regimes. Likewis the enhanced amplitude of negative (and positive) anomalies for ZO. (**now lines 518, 528, 534-535**)*

20. Section 4.2, Given the limited differences between these patterns and those described with ERA-int in Grams et al., 2017 this could be condensed?

Following your advice, and the comments of reviewer 1 and the editor Section 4.2 has been rewritten substantially and split into two sections. New Section 4.2 contains now first the description of the large-scale patterns. New Section 4.3 makes statements about the relationship to the seasonal four regimes, but in a very much condensed manner compared to the previous version.

21. In Fig 6 can you use colours for the 4 that are not used in the 7 to make the plot easier to interpret? I got a bit confused when I first used these on the double use of the primary colours.

Now Figure 5: The use of same colours was intended to emphasise the relationship of regimes. Following other comments the figure has been replaced with an Appendix figure, but only the correspondence of the 4 regimes with the seven regimes is show, avoiding the use of similar colours in the main text.

22. L578: EuBL and AR also look relatively similar for their year round occurrences in Fig 7?

now line 705: No, there is more seasonal variability in particular regime frequencies for EuBL almost double in July compared to November and vice versa for AT in Oct compared to June.

23. L585: There is repetition here from previous sections.

Yes, you are right. The relationship of EuBL and ScBL has been mentioned before. Still I find it important to thoroughly discuss their frequencies throughout season together, which requires some repetition.

24. Fig 8 caption, add (LC) so we know what the acronym stands for in the Figure.

Thanks for this hint. I added LC in the caption.

25. L619: Are the differences statistically significant?

Now line 739: I did not test the statistical significance in differences between GL LC duration in DJF compared to other seasons. For brevity I want to avoid digging deeper into seasonal differences of the year-round regimes and omitted discussing this aspect further.

26. L625-635: Move to appropriate point in literature review.

The paragraph has been moved into the introduction (now lines 96-101). Here a brief reference to that paragraph remains. (lines 747-753)

27. Section 5.4: Consider if this section is needed in so much depth or if the material could be moved to the supplement. It's noted that the results are present in other papers that are cited. For example, 'The following provides an overview of these studies' should not be the topic of a results section in a new science paper.

The new aspect here, is that with these panel figures a direct overview of differences in surface weather modulation in different seasons is given. This has not been done in such a "synoptic" view in previous work, but specific aspects have been investigated. As such I still Section 5.4. provides some novelty and deserves remaining in the main body of this paper.

28. Conclusions: Can you clarify the novelty vs. synthesis aspects of this work compared to the previous studies more clearly in the conclusions.

The conclusion has been split in a summary (Section 7) which highlights the key results and synthesising intent of the study

In fact the paper aims to provide a foundation for the further use of the seven regime definition. This is now articulated in the Summary. The Conclusion (Section 8) focuses on a discussion in the light of other studies.

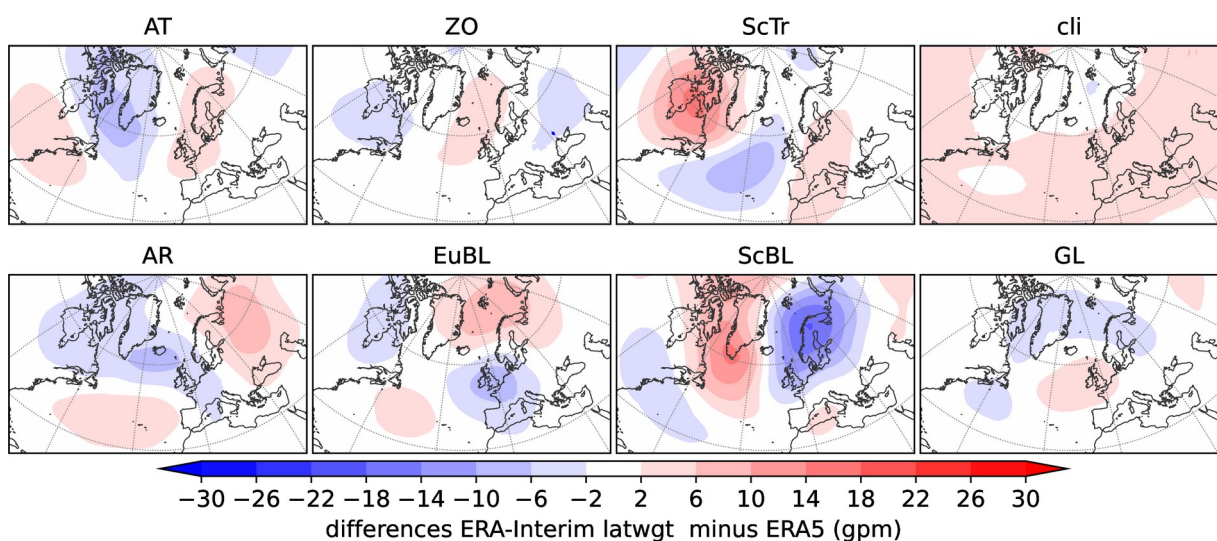
In an ideal world this paper would have appeared prior to the first application in Grams et al. 2017. I understand the editor, that also such a synthesising “review style” paper has its place in WCD. Still I made an effort to balance this intent more with the novelty aspects and hope having achieved this in a sufficient manner.

29. Figure A1: This would be interesting for the main text and methods given the update of the method for the new dataset.

I agree, there is some novelty in the method, still I consider this too minor to be prominent in the main – mostly for brevity. (now figure B1).

30. Figure A2: A difference plot here would be interesting as they look the same to me by eye instead of the comparison. (now figure B2).

Below I provide a difference plot which has quickly been compiled, but without a proper grid interpolation. (At each location of ERA-interim data at 1° resolution the corresponding value of the ERA5 data with 0.5° resolution has been subtracted). There are differences all over mostly below 2gpm, which are likely often due to the different resolution of the two data sets. The striking difference concerns ScBL as discussed, potentially due to the high frequencies in latest years 2015-2019 which are not covered in the ERA-interim data set.



EC1: ['Comment on egusphere-2025-6385'](#), Juliane Schwendike, 25 Feb 2026

The paper fits within the scope of WCD as dynamical weather regimes are investigated and characterized. These regimes are widely used and relevant for the atmospheric dynamics community. However, the framing of the paper could be improved to de-emphasize the technical aspects and focus more on the scientific findings, for instance regarding temporal variability and trends in weather regime occurrence. The novel aspects of the work could be highlighted more as well.

Thank you for taking your time considering the reviewer concerns and the fit of the manuscript in WCD. As detailed in the overview statement of this response letter I have tried to improve the fit in the journal hopefully in a satisfactory way. In particular I have restructured the manuscript in order to better carve out novel dynamical aspects. These are also better summarised in the abstract and a new Summary Section (Section 6), while the conclusion focuses on a discussion. Also I have shortened the article by about 17 percent, corresponding to about 150 lines or 5 manuscript pages. With that I hope I have found a balance between a kind of synthesising review-style article, novel aspects, and the intent to provide a fundamental paper for the now established year-round weather regime definition.

Citation: <https://doi.org/10.5194/egusphere-2025-6385-EC1>