

**Final author's response for 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 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.*

*You all raise two somewhat similar major concerns. The first concern is about the fit in WCD, and the second concern about the length of the article. The revised manuscript will address these issues as follows:*

- **Fit in WCD:** *I am happy, that the editor confirms the fit into the journal. In a revised version I will more work out the novel and dynamical aspects of the manuscript. At the same time I will deemphasise the character of a purely technical paper as criticized by R#2. To do so, I will (a) improve the presentation of the results about potential trends in Section 6. As R#1 pointed out, the current presentation leaves readers confused. I will work on avoiding contradicting arguments and the clarity of the presentation of results. (b) I will work out better the other major novel dynamical aspect which is about regime behaviour and the relationship between the seven year-round regimes. This is currently hidden in the discussion, comparing the year-round regimes to the canonical regimes in Section 4.2.*
- **Length of the manuscript:** *In order to focus more on the novel and dynamical aspects, the text will be streamlined and technical aspects in the main body reduced as suggested by the editor and both reviewers. Parts of the technical description of Section 3 will be relocated to the Appendix and instead the main technical steps summarised in a single methodology section. The content of Section 4.2 will be focussed on the relationship between regimes, and the comparison to the canonical regimes reduced, as pointed out by R#1.*
- **Causal statements:** *Furthermore the text will be revised in order to avoid causal statements as pointed out by R#1.*

*Below I briefly state on how the individual comments will be addressed, where a specific note is possible at this stage of the final response.*

**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 will be shortened in the main body of the text as detailed above, in order to avoid deterring*

*future readers from reading the manuscript. 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 will be revised with a focus on avoiding statements about causality where no direct causation is evident.*

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)

*The section will be refocussed on the relationship between seven regimes and novel aspects of regime life-cycle behaviour.*

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 arises from. I will rework this section in order to avoid contradicting and confusing statements.*

### **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?
2. Line 171: “fuzzy clustering”. A brief description and citation (currently given later) would be useful.
3. Line 193: What about  $t_c = \text{February } 29$ ?
4. Line 203: Change “smoothed” to “running”?
5. Line 213: Change “up to” to “including”?
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  $Z500^{LT}(t)$

*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. Therefore the notion “running standard deviation”. I will check how to clearer explain these technical details.*

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(?)

*Yes, the fuzzy clustering is set up, to replicate k-means and actually attributes each time step uniquely to one of the clusters. I will more carefully explain the clustering and seeding. Yes, despite random seeds, the clustering converges to a common solution thus insensitive to seeding.*

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 and will be mentioned.*

3. Line 292: “statistical jargon”. I find this phraseology a bit too familiar for an academic paper.
4. Line 315 and 316: “to some degree”. Maybe add “(to be discussed later)”.
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?
7. Line 397: “into  $I_{EuBL}$ ”. Should this be “onto weather regime  $EuBC$ ”?
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?
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.
  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).
  11. Line 470: "and a weak positive anomaly south of Iceland". Change to "negative"?
  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). 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 for these detailed suggestions which will be considered in the revision.*

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

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?
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?
20. Line 654: "in terms of duration the mean" change to "in terms of duration of the mean"?
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.
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?
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?
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)"? 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(?)
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.
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.
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"?
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.
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 (?)

## Typos

*Thanks for spotting these. Will be corrected.*

1. Lines 15 and 172: seamingless replaced with seamless?
2. Lines 31-35: These two sentences are difficult to read
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.
4. Line 89: "an" to "and"
5. Line 617: Change "exemption" to "exception"?

**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. We are happy about your overall positive impression of the article.*

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.

*You are right, the article is structured, so that the real novelty is in Section 6. But also Section 4.2 gives new insight into regime life cycle behaviour. The revised manuscript will be streamlined in order to focus better on the dynamical aspects, and deemphasise the technical aspects. At the same time the length will be reduced. This also follows the editorial recommendation and hopefully improves the fit in WCD.*

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 hint, that the abstract should emphasise more the novelty of the work.*

2. L87, weather types are sometimes referred to as 'weather patterns' e.g. Neal et al., (2016) which you cite.
3. L169-185: This could be better placed in literature review than methods.

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.

*I will consider this for the revision.*

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

*The EOF-clustering attribution will be “noisier”, but the life cycle definition also filters data to longer periods. Former regime classifications were applied with and without filtering.*

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).
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.
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*

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?
10. L271: Remind readers this was first described in Bueler et al., 2021 and you're expanding here. Rather than this paper 'newly introducing'.
11. L308: I was not sure what you meant by 'catch up'.
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?
13. L328: Why is 5 days used as a threshold?
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?
15. L389: reference Figure 2 in first sentence.
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.
17. Section 4 lines 4.14 -430 can be removed. This information is present in the introduction.
18. Figure 3: This is interesting, but could be made supplementary and focus on Figure 5

*I will consider the above comments in the revised manuscript*

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 will avoid this jargon. I mean in a quantitative sense the amplitude of anomalies for 4 clusters are weaker than for 5,6,7 clusters.*

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?

*Agree, will be condensed in the revised manuscript.*

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.

*The use of same colours was intended to emphasise the relationship of regimes.*

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

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

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

25. L619: Are the differences statistically significant?

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

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.

*Thank you for this suggestion of shortening which will be considered in the revision.*

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 will be revised in order to better highlight novel and synthesising aspects of the study.*

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

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

**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. I will improve on fit in the journal and article length with the revised version as suggested.*

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