Review of "How relevant are frequency changes of weather regimes for understanding climate change signals in surface precipitation in the North Atlantic-European sector? – a conceptual analysis with CESM1 large ensemble simulations" by Luise Fischer et al.

The paper focuses on the decomposition of the precipitation response with climate change in terms of different weather regimes and defines a novel metric to quantify the relative importance of dynamic (related to regime frequency) vs thermodynamic (related to regime intensity) changes. Although the metric is not able to definitely disentangle the two components, the changes in regime intensity dominate for most of the domain, indicating that regime frequency changes are of secondary importance to understand future changes in surface precipitation.
I think the results are very interesting and relevant for the community. The paper is well written, with attention to the details, denoting a great accuracy both in the work and in the presentation of the results. I invite the authors to consider a couple of doubts regarding the methodology and a minor issue regarding the assessment of the significance of the results, along with some more specific comments.

General comments

- Significance of the results. I would appreciate a brief discussion of the significance of the changes in the composites with respect to the internal variability. For example, considering the standard deviation of 10-year chunks in the historical period would give an estimate of the variability of the regime-specific composites. This could also be a more quantitative metric to assess the skill of the regime in decomposing the precipitation field (e.g. comment at line 260). Also, this relates to comment at line 277.

- Removal of the climatology. The authors remove the climatology of the two periods separately to get the geopotential anomalies, which are then projected on the observed ERA5 regimes.
Since the anomalies refer to different mean states, this may have an impact on the actual dynamical configuration of the regimes and hence on the precipitation composites. Out of curiosity, have you checked whether the composites of some more dynamical field differ between corresponding eoc and hist regimes (e.g. the zonal wind)? In that case, this may constitute a dynamical effect which is now by construction inside the "regime intensity" component (related to discussion at lines 435-443).

- The intensity change term (i) in equation 5 contains not only the change in the regime-specific precipitation anomaly ($Phi^*_{eoc,i}$ - $Phi^*_{hist,i}$) but also the change in the overall precipitation climatology between the two periods ($\underline{Phi}_{hist}$ - $\underline{Phi}_{eoc}$). The climatology part has instead been removed from term (ii) since it would have summed to zero over all regimes (term iia), so I was wondering whether this creates by construction an asymmetry between the two terms that make up gamma (making gamma smaller). It may be helpful to see the contribution of the climatology separated from the regime-specific anomalies in term (i) (so e.g. subtracting the field in Fig. 2c from the second column in Fig. 4).

Specific comments

L45. The matter is quite debated, I would be softer on affirming that there is "clear" evidence that the regimes "represent physical modes of the atmosphere". For example, the number of regimes to be considered varies in literature, and there is no clear indication that 7 is the "true" number (to my knowledge). I agree that the atmospheric flow shows a tendency for non-linearity and preferred states, but I also think that the classification in regimes is always artificial to a certain extent. I would suggest the review by Hannachi et al. (2017) for an historical perspective on this point.

Hannachi, Abdel., David M. Straus, Christian L. E. Franzke, Susanna Corti, and Tim Woollings. "Low-Frequency Nonlinearity and Regime Behavior in the Northern Hemisphere Extratropical Atmosphere." *Reviews of Geophysics* 55, no. 1 (2017): 199–234. https://doi.org/10.1002/2015RG000509.

L51. I would suggest to add Madonna et al. (2021), which discuss the link between various regime frameworks and seasonal precipitation/temperature anomalies in Europe.

Madonna, Erica, David S. Battisti, Camille Li, and Rachel H. White. "Reconstructing Winter Climate Anomalies in the Euro-Atlantic Sector Using Circulation Patterns." *Weather and Climate Dynamics* 2, no. 3 (August 25, 2021): 777–94. https://doi.org/10.5194/wcd-2-777-2021.

L66. The result by Huguenin et al. (2020) should be moved inside the discussion at lines 74-78 regarding the "rather low consensus on future changes in WR occurrence". Also, regarding this point, it may be worth discussing other evidence of circulation changes (in winter) in warmer climates, for example the work by Oudar et al. (2020) and Peings et al. (2018).

Oudar, Thomas, Julien Cattiaux, and Hervé Douville. "Drivers of the Northern Extratropical Eddy-Driven Jet Change in CMIP5 and CMIP6 Models." *Geophysical Research Letters* 47, no. 8 (2020): e2019GL086695.

Peings, Yannick, Julien Cattiaux, Stephen J Vavrus, and Gudrun Magnusdottir. "Projected Squeezing of the Wintertime North-Atlantic Jet." *Environmental Research Letters* 13, no. 7 (2018): 074016.

L156. How is the seasonal normalization coefficient computed for the historical and future model fields? I expect the coefficient would be different from the one computed for the ERA reanalysis, and would differ for the future and historical periods too. How does this choice impact the seasonality of the regimes and their projection on the ERA EOF space?

L165. What is the sensitivity to this threshold? i.e. what was the proportion of no-regime days with the original threshold of 1?

L176-180, Figure 1. Even if this is a single model study, a quick comment and comparison of these results with others in literature would be needed here.

L233. The sign of the ratio may be also interesting to investigate (i.e. do dynamic and thermodynamic effects contribute in the same direction?).

L242. The order of magnitude estimate is fine, but I would avoid a more quantitative estimate at this stage (e.g. 10 -> 9-11%), since the frequency change could in principle be much larger.

L260. For a "very good WR classification".. I understand that you expect delta Phi for each regime to be small with respect to the typical regime anomaly of Phi. However, the estimate is quite arbitrary (in principle, the regime intensity could increase/decrease by a more significant fraction) and a low value of this ratio could be due to other causes, rather than to the quality of the regime classification.

L277. I think it would be more appropriate here to set a more objective threshold on the climate change signal, e.g. where the signal is significant with respect to internal variability (e.g. considering the historical variability in a 10-year random sample). What does the 30% threshold correspond to in terms of climate change signal?

Fig. 2. I suggest to use the green/brown colorbar for the precipitation differences, as done in other figures. Also, it would be nice to add a title with the respective seasons on top (just a suggestion).

L286. Also, negative values are found in the Mediterranean region, it may be worth commenting on that.

L335. "weaker WR-specific anomalies". I think it would be worth adding in a supplementary a figure with the P* fields (hist and eoc, and/or the delta P fields), to allow comparison of the relative magnitude before multiplying by additional factors. Fig. A.17 and A.19 of the doctoral thesis would be very useful here.

L345. "but is more variable within a specific WR". I don't think this is indicated by the precipitation anomaly composite.

L435. I appreciate the observation regarding the fact that intensity changes could also contain a dynamical signal, for example in terms of the amplitude of the dynamical anomalies. Do you see a way to extract some more information out of this, i.e. to separate a general thermodynamic response to regime-specific features?

L440. "delta Phi is fairly uniform across WRs". This is not true for all regions, for example the central North-Atlantic and the iberian peninsula.