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Interactive comment on "Seasonal weather regimes in the North Atlantic region: towards new seasonality?" *by* Florentin Breton et al.

Anonymous Referee #1

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Overall comments:

This study aims to investigate changes in the weather seasonality in the North Atlantic. Seasonal weather regimes (SWRs) are defined using cluster analysis based on the first principal component on raw Z500 data. Results for four and seven regimes are presented for ERA-Interim and 12 CMIP5 models. Models results are first compared to ERA-Interim for the period 1979-2017. The authors investigate changes in the patterns and frequency of the regimes by comparing CMIP5 simulations for present (1980-2008) and future (2071-2100) climate.

The paper includes a lot of Figures (10 in the main document and 22 in the supplement), with little explanation and description. The authors compare SWRs to the classical weather regimes, but no apparent link exists between the two concepts. The

seasonality analysis is based on daily data but it is not related to weather phenomena (i.e. during winter only 1 regime is found).

Specific comments:

Major:

1. Definition of seasonal weather regimes (SWRs) and comparison to classical "weather regime". I have trouble to see the link between the author's definition of SWRs and the classical weather regimes. The authors compute the first EOF of raw Z500, which should represent the seasonal cycle (no Figure of EOF1 is provided). SWRs are then defined by clustering PC1 (a single 1D variable). So these clusters should represent the strength of the seasonal cycle, which can be seen for example in Figure 2 by the Z500 contours (stronger Z500 gradient in winter R1, weaker in summer R4). It is also clear from Figure 1 that during winter only R1 occurs. Does it mean that there is no weather in winter and all days look like R1 (cf. for example with Figure 2 in Michel and Riviere 2011)? I find therefore misleading to match the patterns in Figure 2 to the classical weather regimes (see also comment on Figure 2), as those methods are considering two different things. To calculate classical weather regimes a cluster analysis is applied to more than 10 PCs (that explain at least 80% of the variance) and the seasonal cycle is removed from the raw data (e.g. Cassou 2008, Vrac and You 2010). Thus, the mix of different concepts makes it hard to understand what is the main goal of the paper and what the authors attempt with their analysis. a) If the goal is to explain changes in the seasonality by analyzing PC1 of raw Z500 (what it seems so far), I would recommend comparing EOF1 patterns and the distribution of PC1. Would this be a different way of investigating seasonality as compared to previous studies (stated in the Intro, l. 25, without many details)? b) If the goal is to understand future changes in seasonality by looking at the changes in the frequency of weather regimes, those regimes should be defined accordingly (see for example Cattiaux et al. 2013 or Grams et al 2017). However, this task might be challenging, as most climate models have a large bias in weather phenomena (such as blocking, e.g. Masato et al 2013).

2. I miss several explanations in the data and methodology. For example, figure S5 shows seasonal anomalies of TAS, but I could not find how these are defined (neither in the methods, l. 75 nor in the text l. 153). Since one of the main points of the paper should be about changes in the season, the definition of seasonal anomalies needs to be clarified. The same is true for Z500 anomalies.

3. The authors argue for 4 and 7 regimes based on the BIC (without showing it). However, it is not clear what is the main advantage of using 7 regimes. With 4 regimes, R1-4 patterns for 1979-2008 and 2071-2010 are still similar (Fig. S6), while this is not the case with 7. For example, R7 in 1979-2008 (Fig. 3) represents 54 days (0%) and it is argued that this regime becomes more frequent in the future (24%), but R7 for 2071-2010 is very different from R7 in 1979-2008. My recommendation is to present a complete analysis for either 4 or 7 regimes, but not jumping back and forth (i.e. some Figures for 4 are in the supplement, some in the main text).

Specific comments:

Title: I find the word “weather” in the title non-appropriate and confusing. It is strange to call “weather” something that persists for one winter/season. Circulation regimes or seasonal regimes (without weather) might be a better option.

l. 32 “North Atlantic atmospheric patterns are the results of two systems operating at different scales”. Please rephrase this paragraph, adding the relevant Literature. A few critical points: low-frequency structures are not “systems”. What is the “atmospheric dynamic variability”?

l . 34 anticyclones are also important.

l. 42 How can climate dynamics have a strong seasonal feature? The climate in the extratropics has a strong seasonality. Should atmospheric blocking (time scales of 1-2 weeks) be an example of climate dynamics?

l. 30-40: There is missing relevant literature (for example, there is a bulk of literature

on the eddy-driven jet more relevant than Cassou et al 2008, l. 35). See also some suggestions that could be useful at the end of the document.

l. 49-51 what is the link between the representation of the atmospheric circulation and the seasonality in the model? I agree that it is important that both are correctly represented, but I can not see the link between the two concepts yet. Are the authors suggesting for example, that if the jet stream moves too slowly polewards through the season, this will have an impact on seasonality?

l. 83-84 I expect the first EOF of raw data to be the seasonal cycle. Is that correct?

l. 84 What is the main advantage of a GMM if only a single PC is used for clustering? How different is this method from dividing PC1 into quantiles? Is the PDF of PC1 non-gaussian?

l.104: Instead of only adding the formula in the appendix, it would be very useful to have a figure showing the BIC for the different k in the appendix/supplement

l.112: why adding the seasonal cycle of 2017 and not the seasonal cycle of 1979-2017? What is the reason beyond this choice? This is particularly relevant if the same is done for temperatures.

l. 130 “They are also visually similar to the usual North-Atlantic weather regimes from the literature (e.g. Cassou (2008), Yiou and Nogaj (2004)).” I do not think that this is true. Weather regimes are defined by removing the seasonal cycle and mostly using only winter months, why here the “regimes” represent the seasonal cycle. I can not see any Atlantic ridge, or blocking regime here! R3 does not have a ridge over the Atlantic (see comments below Figure 2).

l.162 What is increasing 90hPa? Z500 should be in m or gpm

l. 165 “GFDL-CM3 stands out from the other GCMs by showing the emergence in the future of a new summer regime that did not exist in the past.” I see actually a discrepancy between the GFDL-CM3 model for 1979-2008 (figure S8, solid line) and

1979-2017 (Figure 1), so I have trouble to understand this statement.

l. 173-177 It is not clear which trends are meant here and how they are calculated. I do not understand how these trends are calculated since the regimes are not continuous. More explanation is needed here.

l.183-184 Are the regimes (EOF and GMM) calculated for 1979-2100 and then separated in 1979-2008 and 2071-2100? If so I would expect to see a change in the frequency, but not a change in the patterns. For the 7 regimes, I do not see a good correspondence between the patterns (e.g. R2 and R7 in Fig 3 and 4, both shading and contours). Also, I expect the period 2071-2010 to be warmer than 1979-2008, but there are no regimes with warm TAS. This might be linked to how TAS anomalies are defined (please see main comment 2).

l. 185 I see that R7 occurs in summer, but I do not see from any Figures that this is linked to blocking over Scandinavia (Z500 anomalies are over most of the North Atlantic). Also, the percentage (54 days or 0%) is very low compared to blocking frequency (see for example Figures 2 and 3 in Pfahl et al 2012). Moreover, why is R7 much more frequent in the future, but not showing any temperature anomalies? Heatwaves are expected to be linked to blocking also in the future (see e.g. Schaller et al 2018).

l. 218-220: I am not sure to understand the logic behind detrending the data and then calculate the trend of the detrended data. I would understand to i) detrend the data to compute TAS and Z500 anomalies and then compare these for 1979-2008 and 2071-2100 or ii) detrend the data to compute the regimes and look at the trends in the regime occurrence (e.g. trends of Figure 6 and 7), but I do not understand why computing the trends of the detrended anomalies.

l. 220: I can not see the disagreement between the models in Fig. S17

l. 225-235: As I do not understand what has been done to "examine the regime spatial

trends with LGI but without the seasonal shift" I can not comment on this part. Also, what are the "large-scale increases in Z500"?

l. 240-245: Which Figure leads to this conclusion? Why are ERAI and CMIP5 models similar in Figure 1, but very different in the supplement (Figure S8, 1979-2008, solid lines). Also, what is the "increasing frequencies of historical summer conditions of atmospheric dynamics"?

l. 248-270 Which Figures are showing that? Please, add a reference to help to follow the train of thoughts. (I.e. where are cold spells and heatwaves in the analysis? I see only temperature anomalies of a few degrees)? Also, how can a regime be replaced?

Appendix C: Why not having everything in the section "Seasonal weather regimes based on detrended data". Is the same trend removed at each grid cell? Calculated over which region?

Figures:

I think it would very useful to show EOF1 and the explained variance for ERA-Interim and CMIP5 and compare them, before starting calculating and comparing regimes.

Figure 2: I do not see any resemblance of R1-4 with the weather regimes (e.g. from Cassou 2008, https://www.nature.com/articles/nature07286, Figure 1), and I would not expect to see any.

Figure 10: Why are TAS patterns opposite over land and ocean in 1979-2008 and 2071-2010? Is this because the same trend (if I understood it correctly) is removed at each grid point? I expect trends over land and over ocean to be very different (see e.g. Hegerl et al. 2018, Figure 1). Why we do not see this behaviour in Z500? Are the trends in TAS much larger than Z500?

Supplementary Figure 18: Why is the sum of the detrended trends not zero? I would expect some regimes to have positive trends and other regions to have negative trends at the same grid point. Since it is not clear how these trends are calculated, it is difficult to interpret these Figures.

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