Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-199-RC2, 2018

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "On the use of Weather Regimes to forecast meteorological drought over Europe" *by* Christophe Lavaysse et al.

Anonymous Referee #2

Received and published: 14 August 2018

Review of manuscript nhess-2018-199 entitled "On the use of Weather Regimes to forecast meteorological drought over Europe" by C. Lavaysse, J. Vogt, A. Toreti, M. Carrera, and F. Pappenberger

This study proposes and evaluates a novel approach to predict meteorological drought on monthly time scales based on the forecast of large-scale flow patterns. The authors show that in the ECMWF extended forecasting system drought forecasts based on weather regime (WR) occurrence outperform drought forecasts based on direct precipitation forecast in most regions of Europe. Particularly those regions (British Isles, Scandinavia, NE Europe) benefit from WR-based drought forecasts, which show a strong link of drought and the large-scale weather regimes. The linkage of WR and drought as well as its predictability is thoroughly investigated; stratified according to

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seasons, and sensitivities to drought intensity and previous drought conditions tested. It is shown that the WR approach has even more benefit for stronger droughts. Furthermore, the linkage is stronger in winter than in summer. Finally it is shown that the forecast captures well the linkage between WR and drought, but has difficulties in correctly representing WR frequencies.

Overall this study presents a very important contribution to research on monthly and sub-seasonal predictability and novel applications of now existing operational NWP data. It thoroughly documents that forecast products based on atmospheric fields that are easier to predict in NWP (e.g. geopotential, temperature) than more complex variables (e.g. precipitation, wind) can be effectively used to predict weather impacts due to the linkage of flow patterns and surface weather. The paper is well organised, clearly written in most parts and the figures carefully designed and chosen to support the storyline. Only at few places I struggled to follow and some references (to tables) were misplaced. Despite the long list of comments (which are all minor) I recommend to accept after one round of revisions.

Broader Comments:

1. Several studies document that predictability on monthly time scales primarily arises from predictability in week 1 and week 2, while it vanishes in week 3-4 (e.g. Ferranti et al. 2018, Vigaud et al 2017, 2018). Did you check weekly or two-weekly forecast skill for the drought events? How would a two-weekly stratification look like?

2. To me the usage of the term "teleconnection" is misleading. I understand under this term large-scale linkages from e.g. the Madden-Julian-Oscillation or SST or ENSO on weather regimes. In this paper I would talk of a linkage between the weather regimes and smaller-scale local weather/precipitation.

3. Please carefully revise and check how you introduce your terminology. Sometimes different terms are used for similar items, some terms are poorly or not introduced. This makes the paper in partly difficult to read. Details are given in the line-by-line

comments.

4. Please also explain some of the methodology in more detail.

5. Some more literature could be cited: E.g. studies by Lavers et al. 2016ab and Ferranti et al. 2018, also support the idea that large-scale fields provide more predictability for a local weather phenomenon than trying to predict the phenomenon itself. Linkage to climate change could be mentioned e.g. with Santos et al. 2016 or Schaller et al. 2018 in the outlook. Linkage of weather regimes to other surface variables e.g. wind could be mentioned (e.g. Grams et al. 2017).

6. Table references are mixed up. Also order these in their order of occurrence in the paper. I found it difficult to directly understand tables and figures, due to too little information in the caption - in particular for tables. All Supplemental Figures should also be cited in the main text in their order of appearance.

Detailed comments:

reference order: Does NHESS require stating the most recent literature first? If not please revert.

l51: refer also to Ferranti et al. 2018 as a recent study on WR and cold extremes. For wind Grams et al. 2017 might be an appropriate reference

l52: avoid talking of being teleconnected -> linked/associated with

l55: up to here you nicely introduced into the WR concept. It becomes confusing (l55-60) to now talk of NAO+-, without having clarified the differences between the NAO and WR concepts and without having clarified that the two NAO phases are two of the 4 winter regimes. So consider to first contrast NAO (as only describing part (i.e. 30%) of the large-scale variability on monthly/seasonal time scales) to WR (as describing most of the variability (i.e. 75%) on monthly time scales).

Section 2b: Do you do the k-means clustering in physical or phase space? E.g. is

an EOF analysis performed? Do you use time-filtering? You should provide few (2-3 sentences) more details on the WR definition and also more details on how individual days are attributed to a WR (e.g. in physical or phase space l130ff). You repeatedly state why you only use WRs based on a k-means clustering in ERAI. Once justifying this approach is sufficient and convincing! Remove the redundancy.

l140/l146: You refer to Table 2 here!? I found it difficult to directly understand Table 2. Re-Reading these lines, I understand it now, but it would help if you repeat the definition of a WR combination in the caption of Table 2. "WR combinations are defined as either additions or subtractions of monthly WR frequencies" Also in Table 2 I would replace WRa, WRb, ... by a, b ....

Table 1 is really helpful but hardly referred in the text. (as Table 6?). Definitely keep it.

l113: season definition appears confusing. Please explicitly state if winter is NDJF or DJF, ...

Section 2c: some more details on how SPI is computed would help pleasae indicate a current (WMO) reference.

l178: should state Table 3? Table 3 is quite standard, needed?

l198: please specify in the text what is meant by "best correlation criterion". Do you compute at each grid point for each of the 16 WR/combination freq. time series the correlation to the SPI-1 time series? Then the WR/combination with highest correlation is shown in Fig. 1? This procedure needs to be stated.

Section 3: clearly introduce the names/terminology used for the different setups/methods presented in Table 1 and subsequently please use it consistently. The definition of the fourth vs. third method remains obscure.

l212: please name this first method as the "Reference" method (as in Table 1) l213: Table 1! l217-219: the sentence in brackets is not needed / redundant. l216: Suggestion for a slightly clearer formulation: "The third forecasting method, called "operational"

in Table 1, computes MOAWR from ensemble data attributed to the WRs based on ERAI (see Section 2b)." l219ff: Here I am confused: Do you still compute WR-k-means clustering based on ENS data, or do you mean, that WR anomalies are computed wrt. ENS climatology not ERA-I climatology.

l228: The term "Reference" is not introduced, yet.

The bulk of the result sections is well-written, therefore I have less detailed comments in the following.

l281: it would be nice to show evidence for these findings on the other seasons in the Supplement.

Section 5a: The WR evaluation section is a bit weak. Please refer to studies by Ferranti et al. 2015, 2017 or Matsueda and Palmer for approaches of WR evaluation. E.g. how are weekly WR freq. evolving with time?

l303: the statement about blocking is vague. Do you refer to the Blocking regime (so one of the 4 WR) or blocking anticyclones in general?

l304: The sentence starting in line 304 and ending in line 309 is complicated. Perhaps directly start with what is shown in Figure 7 then go into the interpretation. I do not understand the argumentation of causes and effect for the anomalies. Try to rewrite lines 298-312 in clearer language. I wonder if Fig. 6 and 7 show really different things, or if only one of the two is sufficient. I prefer Fig. 7 but would elaborate on its description and interpretation.

l311: you mean Table 4. But except here, the Table is hardly used. Do you really need it?

l404: Ferranti et al. 2015, Weisheimer 2016, Magnusson 2017 and/or Grams et al. 2018 could also be cited to highlight the challenges in predicting WR. Furthermore you could insert a statement on the evolution of WR under climate change e.g. Santos et al. 2016, Schaller et al. 2018.

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