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Discussions

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

Anonymous Referee #1

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The authors present a novel approach to provide early warnings for drought events over Europe, based on ECMWF's ERA-Interim reanalysis and ENS forecast system. I really like their basic idea, as it combines a scientifically interesting result with a very practical and applications-oriented framework. Furthermore, the authors have clearly performed an extensive set of analyses in support of their results.

However, I think the manuscript falls short of publication requirements in its current form. In addition to some comments of a more scientific nature, my main concern is the poor form of the submission. The text is often grammatically flawed or very unclear; references to tables are incorrect (and in one case to a table that doesn't exist, at least in the version of the paper I reviewed), and some existing tables are never referenced. Overall the manuscript has a very unrefined feel.

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Discussion paper

I reiterate that I do find the motivation behind the study and the authors' approach of great interest, so I would encourage them to perform a very thorough review such that their results may be published in NHESS.

Major Comments

1. As stated above, I deem the manuscript to be in an advanced draft form rather than at a publication-ready stage. The issues range from simple typos/grammatical errors/incorrectly referenced tables/oversights (some examples here:

l. 25 "While, the"
l. 29 "and onset drought events"
l. 69 "teleconnections in between"
ll. 84-85 "observational daily station-based" This is a bit redundant. If something is station based it is very likely to also be observational.
ll. 96 Space between "10" and "days" and "32" and "days". Remove multiple spaces between "Tl319" and "64".
l. 97 "1-degree" no need for hyphen.
l. 102 "launched" –> "initialised"
l. 111 "done, exclusively"
l. 121 There is a lone parenthesis
l. 121 "previous studies mentioned earlier" –> "aforementioned studies".
l. 130 "identify The closest"
l. 146 "Table 1". Do the authors here mean Table 2? The real Table 1 actually does not seem to be referenced anywhere in the text.
l. 156 "datasets from observations" –> "observational datasets"
ll. 157-158 "The choice ... has bee verified in Lavaysse et al. (2015) and shown that this assumption"
ll. 159-160 "some grid points the significant tests are not verified"
l. 178 "Table 2" –> "Table 3"?
l. 183 "dump" –> "lower"

l.185 “Peirce” –> “Pierce”
l. 198 “leads to coherent picture”
ll. 201-202 “they are generally higher teleconnections”
ll. 207-208 “the potential beneifts is assessed”
l. 213 There is no table 6 in the version of the paper I have reviewed (indeed the tables stop at 4).
l. 218 “to avoid potential problem”
ll. 227-228 “also called and presented in the previous section as Reference”
l. 229 “forecasts” –> “forecast”
l. 247 “anomaly” –> “anomalies”
l. 261 “2.5l. 269 “for all the domain shown in the previous Figures” –> “for the whole domain shown in the previous figures”
l. 311 Again a reference to Table 6.
l. 316 “e.g.” or “i.e.”?
l. 328 “intensities” –> “magnitudes”?
l. 344 “showing a more complex observed than forecasted teleconnections”
l. 346 “the relative good representation”
l. 347 “The correlation between the WRs forecasted and the observed precipitation”
l. 385 “the dynamic of precipitation”
l. 390 “The skill scores is”
l. 405 “with regard to” –> “for”
l. 408 Acknowledgements are missing.
Tables 3 and 4 are never referred to in the text.
Fig. 8 caption “GSS*2:w”
The figures in the SI aren’t prefaced by “S” which makes it hard to figure out whether the authors in the SI refer to the SI figures or those in the main text.
l. 72 in the SI: “?” in place of a reference.)

to some very unclear or contradictory passages that I would recommend the authors re-phrase (some examples here:

ll. 30-32 This is a bit confusing: if the MOAWRs are anomalies of occurrence how can they "depict" a large-scale atmospheric pattern?
ll. 48-52 The authors first discuss precipitation and then switch to wind gusts and temperature extremes without any apparent connection. How are the latter two fields relevant to the study?
ll. 202-203 Why speak of northern and southern Europe and then shift to central and north western Europe?
ll. 212-224 This passage is very important (the authors explain a key aspect of their approach) but also occasionally difficult to understand. For example, what does "uses ENS for the WRs assignation" mean? Do the authors mean that the WRs are defined using the ENS dataset? Similarly, what does "modelled precipitation" refer to? Do the authors mean ENS precipitation (ERA-Interim is a model too)? Linked to the above, the passage describing the forecasting methodologies in the SI (to which the authors point the reader) never explicitly mentions the "idealized" approach, although this is included in Fig. S1. I would suggest the authors simply include the full description of the methodologies in the main paper (the text in the SI is not that much longer than that already in the main paper), leaving the details of the attribution and MDA analysis in the SI. Again linked to the above, since the authors have given names to the different forecasting approaches, they should use them! For e.g. the caption in Fig. 4 never names the apporach it depicts. This is one example, but there are a number of other similar cases throughout the submission.
ll. 228 Actually, this is never called "Reference" in the previous section.
ll. 305-307 The authors first speak about "overestimation of low occurrences in the observations", which is confusing because observations (or rather reanalysis, if I understood what the paragraph talks about) here are taken as the ground truth and so cannot under or overestimate occurrences. Next they mention the "larger number of forecasted events compared to observed ones with durations shorter than 5 days". I take this to mean that the forecasts produce a larger number of short events than the reanalysis. However, from Figure 6 it seems that short events are more frequent in

ERA-Interim than in the ENS data. I may have misunderstood the whole passage, but in that case other readers may well have the same issue.
ll. 346-351 From this sentence I understand that the correlation values are low where the teleconnection with precipitation is the strongest, while Figure 9 (and logic) suggests the opposite.
ll. 361-363 Do the authors mean that the results clearly point to the ability in forecasting the large-scale atmospheric circulation as a factor limiting the skill of this approach?
ll. 391-393 I think that I understand what this means, but the phrasing is very awkward.
Figs. 4 and 8 I would suggest briefly mentioning that GSS is multiplied by 2 so that the same scale as for the other metrics can be used.)

to some figures that need to be refined before they may be published

(Fig. 3 The authors should mention somewhere the different intervals used and the fact that the FAR colourmap is inverted
Fig. 9 Why is there no land mask in panel d)?
Fig. 9 If the correlation in panels b) and d) is to be compared to that shown in the previous figures (e.g. Fig. 2) as suggested in the main text, then the colourmaps should be the same.
Fig. 9 Why is the colourbar labelled differently wrt Fig. 1? It would be better if the two were consistent (either numbers or text is fine).
Fig. S3 Depending on how you chose to change the correlation colourmaps in Figs 2 and 9, please ensure that this is consistent too).

2. The results presented in the study are very application-oriented. Why not enhance this aspect by providing the equivalent of Fig. 4 for the operational and optimised forecasts?

3. The reader finds out about the leave-one-out approach only on l. 245. This needs to be discussed before and in more detail, as it is a crucial aspect of the methodology. Related to this, have the authors re-caclulated the WRs every time without the “left out”

year to ensure no information leakage between training and test data?

4. Fig. 1: Why is this only shown for winter if all four seasons are then discussed?

5. ll. 281-287 This is one of the most interesting passages of the paper. Since the authors have clearly performed a comprehensive set of analyses, I would encourage them to expand the discussion of the physical drivers that may be behind these results.

Other Comments

1. l. 36 When they say “models” are the authors referring to climate models, NWP models in general, deterministic forecasts, ensembles or what here?

2. l. 53 This repeats what just said above: “WRs are highly teleconnected to ... precipitation” and “They are well known to ... either favour or inhibit precipitation in Europe”.

3. It might be helpful to mention the four canoncial weather regimes over the North Atlantic.

4. l. 59 “The WRs also have an impact on extreme events” is a repetition of what said on l. 49 and l. 52.

5. l. 65 As not all readers may be familiar with WRs, the authors should mention that they are often (although not exclusively) diagnosed using 500 hPa geopotential height and maybe provide a reference.

6. l. 91 This is a very odd choice. The authors state that they upscale E-OBS and ENS to 1 degree but then use ERA-Interim at 1.125 degrees (which is certainly not its native resolution). Why not use it at the same resolution as E-OBS (or alternatively at the highest recommended resolution of 0.75 degrees, if it will be at a different resolution from the other data anyways)?

7. ll. 100-103 Would be more logical to have these earlier in the paragraph, before discussing the regridding.

8. l. 129 Are there other studies that recover 3 WRs? If so, cite them. If not, provide a more detailed explanation of why you find a different number. With regards to the possible sources of discrepancy, one may argue that, assuming a more-or-less stationary system, if the number of WRs depends on the period chosen, then the length of the period is simply too short to define WRs in the first place.

9. It would be nice to see the equivalent of Fig. S3 for ENS (even though it is not mentioned in the caption I guess Fig. S3 uses ERA-Interim and E-OBS only?

10. ll. 166-168 Do the authors have some evidence or reference to support this? Do any of the national civil protection services or other public services in Europe routinely make use of this type of information?

11. l. 170 vs l. 108 I am a bit confused as to what is computed up to 2014 and what up to 2013. This is a detail, but if some of the figures/results are indeed computed using 2014 too, then a time interval column could be added to Table 1.

12. l. 233 An improvement with respect to what?

13. l. 279 Is a forecast with such a score useful in an operational context? More generally, can the authors make an assessement of where (geographically speaking) and to what extent their method would actually provide "operationally useful" information? I want to clarify that I am not suggesting the paper would be less valuable if no such operational information can be obtained from the results presented in it. However, I think that an honest discussion of this aspect would make the paper more useful to both the research and public service communities.

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