Lines 79-80 and figure 1. Why exactly that region? Is there a process-knowledge approach behind this choice, a literature review or a regression/correlation between Z500 and rain over each station was applied? Whatever, the choice, it should be justified by means of references or, in the last case, with a graph/map that certifies the link between Z500 and precipitation.

Line 98. Why did the author choose exactly 20 analogues? Would not be better to base the choice on a maximum Euclidean distance?

Line 100. The 4-day time embedding is not clear to me. Why is it necessary? Why does it preserve the temporal derivative of the atmospheric field? Please explain.

Lines 113-118. Despite the mechanism is quite clear to me, the sentences "In order to go […] precipitation between t_0 and $t_0 + T$" are not well formulated. Since this is a crucial part for the understanding of the method, I would rephrase and expand this part.

Line 120. "of the properties" is redundant.

Line 123. More than the average value, the persistence consists in the *anomaly* between $t_0 - T$ and t_0. Also, the climatological forecast takes.

Lines 128-134. Again, it is not clear upon which basis those domains (in Fig. 1b) have been chosen and the final domain selected among the four attempts. Also, the entire paragraph needs a language revision.

Line 150. A verb is missing (meet?).

Figure 2. A visual legend is needed. Also, what's in the y-axis? Are the 5th and 95th percentiles calculated over the 1948-2019 time series?

Line 184: six??

Lines 201-205. It seems to me that part of the methodology is described here, where a description of results is expected.

Line 206 and following. Very little description is given for figure 3. First of all, I think it is useful to illustrate all the stations in the main text, instead of showing only Orly while relegating the others to the Appendix. This is one of the main results of the study and deserves a better stage (instead, I would recommend to place Fig. 4 the Appendix, since the WRs are not a result of this study). Besides, fig. 3 and fig. A1 show a very interesting characteristic that should be discussed: most of the times, in fact, the *summer* SWG forecast vs. persistence improves with lead times, which is somewhat unexpected. Any thoughts about it?

Line 217. De Bilt?

Line 225. They help describe.

Line 232. Missing year for the Cassou reference.

Lines 246-255. Fig. 5 (B1) should be described with more care. What do "Good forecasts (low quantiles of CRPS)" and "The low quality forecasts (high quantiles of CRPS)" mean? The caption for figure 5 is totally unclear and does not describe the plot. On the top of the blocking bar (panel b) a group of dots appear.

Lines 257-276. This paragraph is hard to read, there are many inconsistencies between subjects and predicates and other grammar errors. Also, the first paragraph is not clear: what is it meant to demonstrate? Maybe the lower skill of the ECMWF forecast? The latter is calculated over the entire European domain, how can it be compared with a forecast over single stations? A table with both ECMWF and SWG CRPSS would be more informative than a few words, if the authors find the way to make a fair comparison between the two.

Line 279. *The input of our model was analogs of geopotential heights at 500 hPa (Z500).* This sentence should be rephrased.

Line 283. I cannot accept this conclusion. The only way to test it is to compute the analogs with the 70-year ERA5 dataset, now available since 1950. This is a very important test that should be included in this study, because it clarifies the role of the different reanalyses as well as the role of database length.