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Interactive comment on "An adaptive two-stage analog/regression model for probabilistic prediction of local precipitation in France" *by* Jérémy Chardon et al.

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We thank the referee for this thorough review and for the numerous constructive suggestions that we will consider for incorporation in the modified manuscript.We give here the detailed responses to all his comments and questions.

1 Big Picture

1. The authors present and explore a methodology to simulate precipitation intensities. Yet, neither time series and/or spatial fields of simulated precipitation intensities are shown nor compared to observations (in a probabilistic manner as the title might suggest). While the methodology might be beautiful, I think this is the biggest missing thing in this paper. I am not a specialist in analog methods. I did my best to understand what is done here. Ideally my potential failings help to detect shortcomings in the paper and lead to improvements. Besides the analog part, I tried to help with general statistical hydrological comments.

The downscaling model first aims to issue probabilistic predictions of local precipitation at any given grid point of the SAFRAN grid. This prediction results in a probabilistic distribution function for each prediction day for each grid point. A times series representation, where (probabilistic) simulations and observations are compared, is thus not really convenient. The evaluation is here done with the CRPSS, which is frequently used for the evaluation of probabilistic predictions in a framework where we have to compare one value (the observation) with a whole distribution.

We also agree that the prediction of precipitation fields is an important issue. It was not in the scope of our work but further work should consider this issue. We will include a comment on this in the discussion section as a perspective.

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2 Hybrid Approach

2. The authors want to predict a variable (e.g., precipitation) for a given day (say, for the example of this review, May 30th 2018) at a given location (within France). Then they look at all 30-Mays in the past when precipitation amounts were recorded. Where exactly do the authors look? at the closest measurement station? Is an interpolation performed? What kind of spatial dependence between observations (and simulated values) is assumed / considered?

In the present work, we want to predict local precipitation only. We do not thus need any assumption on the spatial dependence between stations.

For each prediction target location (each target grid box), we only use precipitation data that were estimated within the SAFRAN reanalysis system at this exact location. Note that the SAFRAN reanalysis give an estimate of daily precipitation at each location from the closest measurement stations and from some information on the weather type for each day. To date, these SAFRAN estimates provide the only high resolution reanalysis of local precipitation over France for the 60 past years. We consider these estimates as pseudo-observations.

3. On p22 l1ff you write that "the predictors and regression coefficients of the regression models vary from one day to the other? – How much do they vary? And how much do they vary in neighbouring cells? Is there some kind of relationship between the variations in neighbouring cells? Can you show this?

Thank you for this very interesting point. We actually did not estimate these day-to-day variations. We will change this formulation for "the predictors and regression coefficients of the regression models can thus vary from one day to the other". We have

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actually an indirect estimation of the variation in the predictors with the different sampling frequencies obtained for the different weather types. If not straightforward, a more formal / direct evaluation would be probably worth. This will be suggested as a perspective of the work.

We agree that the regional consistency of the predictors is another very interesting issue. An evaluation of this consistency is indeed expected inform on the robustness of the downscaling relationship. From the results shown in the manuscript, we have a partial idea of this with the rather high spatial coherency of the selection frequency obtained for each regression structure considered in our work. This is illustrated in Figures 8, 9 and 11. In Figure 11, this coherency is also shown to be high (even if with different spatial patterns) for different weather types.

This spatial coherency is also suggested from other analyses not shown in the manuscript. Figure R2a below presents the percentage of time (over the 20 years used for the analysis) where the regression structure selected for a given location is the same than the regression structure used for a reference location. As illustrated, this percentage of time with same structures - varies from 35% to more than 50% in the close neighborhood of the reference location. Some regional consistency is also found for the regression coefficient obtained for the predictors. We find that the spatial pattern of this consistency depends on the weather situation. This is illustrated in Figure R2b below for the regression coefficient estimated for W in the regression structure no 4.

We will add a comment on this in the new manuscript version.

Figure R2a: Percentage of time (over the 20 years used for the analysis) where the regression structure selected for a given location is the same than that used for a reference location (the reference location is indicated with the blue dot in each figure). Results are shown for 16 different reference locations. (see supplementary material)

Figure R2b: Spatial distribution of the mean regression coefficients obtained for the predictor W and the regression structure n°4. Results are presented for the different seasons and for the 8 weather types considered in this work. (see supplementary material)

4. What if the observed time-series is not stationary? Are there any checks performed? Is stationarity assumed? How strong of an assumption is it?

No hypothesis of stationarity is assumed in the present work. The time series of precipitation can be non-stationary. In such a case, this non-stationarity would be expected to be reproduced as a result for instance of a change in frequency of weather types or as a result of a non-stationarity (i.e. temporal evolution) in some of the predictor variables.

An indirect illustration of this is the ability of the method to reproduce the interannual variability of annual precipitation (or of seasonal precipitation). This was illustrated by Lafaysse (2011) for three different downscaling approaches similar to the present one.

Figure 10.1 extracted p122 in Lafaysse(2011). Time series of winter (DJF) and summer (JJA) precipitation obtained with an Analog Prediction model (100 scenarios) for the Upper Durance basin in South-Eastern France (red = observation, green : median scenario, blue : first analog scenario) (see supplementary material)

5. The authors claim that values outside the range of observations can be simulated via "extrapolation" (p2 line 20ff.) – some background / assumptions / limitations of this extrapolation methodology is required. The previous statement seems contradictory to what is said on p2 lines 29ff.:

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In this paragraph, we wanted to highlight the issue of simulating non observed values. This obviously refers to values that are outside the range of observed values (including rare values): this is indeed extrapolation. This also refers to all values that are in-between two consecutive observed ones. In our context, precipitation is estimated as a function of different predictors. For a given prediction day, the statistical model give a prediction which is a kind of interpolation from what has been observed in configurations that are close to the configuration of the prediction day (in terms of predictors). We have mentioned these two different prediction contexts in our initial text but we understand that too much emphasize was given to extrapolation. We will modify it in the new manuscript version.

Using a statistical model for this simulation/extrapolation exercise is obviously current practices in statistical analyses. This is actually based on the assumption that the model is well suited for the data. We here follow the same assumption, with the difference that the statistical model is reestimated each prediction day based on what was observed for a set of analogs for this day. The limitations of this simulation/extrapolation are mainly related to the quality of the statistical model used to model the observations. We here consider the gamma model for precipitation amounts, model which has been widely used in the hydrological literature to model precipitation amounts. We have assessed the quality of the fit using qqplots for several days.

As mentioned in our response to question 21 of referee 1, other distributions could be considered in our approach to model this nonzero part (e.g. the extended GPD distribution used by Naveau et al. (2016). This may improve the prediction but this may also lead to estimation difficulties as more complex distribution models would require more data for a robust fit. Here, the number of data is voluntarily limited to the number of analog dates considered for the fit. A more flexible model (with more parameters) would require considering more analog dates. This may be detrimental for the prediction skill as poorer analogs would be integrated in the set of dates used for the *estimation of the regression. Another possibility would be to fit a more flexible model with the same number of analog dates than in the present case. Due to the estimation problems mentioned above, this would however likely lead to a much more frequent use of the backup analog prediction model in our case. Such an analysis would be worth to do and this issue will be mentioned in the perspective section.*

Note finally also that there is no contradiction between the above mentioned two paragraphs. We are going to improve the writing to make it clearer. The main point we wanted to highlight is the difference between both analog and transfer function approaches in their ability/inability to give predictions that we not already observed.

6. The author's method is able of extrapolation? is there any evidence of the quality of the extrapolation?

In the present work, we do not use our model for simulation but for the probabilistic prediction of precipitation. We do thus not do extrapolation although it would be an option in a simulation context.

As mentioned in Question5, the quality of the extrapolation would result from the quality of the model used for the considered prediction day. In our work, the quality of the regression models (for occurrence first and for non-zero amount next) is checked via the significance of the different predictors used in the regression. Extrapolation would be only possible with the model in the case of regression models with coefficients significantly different from zero. In the opposite case we use the backup analog prediction model as explained in section 3.3.

7. p2 line 28: I am not sure how a linear model can be "extended" to non-Gaussian data. If this is not to be a reference to what Maraun et al. (2010a) did, but the authors rather claim that their method is capable of simulating non-Gaussian data, then

there is some more extensive explanation required: What kind of non-Gaussian-ness is observed in the data and how can linear models mimic this kind of non-Gaussian data? How and where is this non-Gaussianness seen in the data and how is the model describing it?

We agree that these lines are somehow confusing. Generalized Linear Models (GLM) are regression models specifically introduced by statisticians to model non-gaussian data (see Nelder and Wedderburn, 1972). GLMs are an extension of linear regression models. They represent a large family of different statistical models which can all be described within a same theory. They were first used by Stern and Coe (1984) for the generation of precipitation. Another important application for precipitation was presented by Chandler et Wheater, 2002. The vector generalized linear models (VGLM, Yee and Wild 1996), closely related to the class of GLMs, are the most general class of linear regression models available. The work of Maraun et al. (2010b) is just one recent application of VGLMs for the case of precipitation. We will simplify this section and remove the mention to VLGMs, which is not necessary here.

Daily precipitation data are indeed non-gaussian. They are positive, have a mass in zero (the probability of null precipitation is strictly positive and high) and have classically a skewed distribution for non – zero amounts. To model precipitation, two different GLMs are generally used, one for the probability occurrence of precipitation, another for the distribution of non-zero precipitation. The occurrence of precipitation is classically modelled with a GLM using a logistic link function and a binomial probability distribution. The distribution of non-zero precipitation is often modelled with a GLM using a log-link function and a gamma probability distribution. We follow this two part modelling approach in our work with these two different GLM configurations.

8. From the abstract it did not become clear to me, what is meant with an hybrid(having two kinds of components that produce the same or similar results) approach. The title

is worded more suitably. On the other hand 'local' could be confused with 'small scale'

We thank the referee for this comment. The hybrid approach refers to the fact that two different methods, often used alone for the prediction, (the analog approach and the regression approach) are used in our approach as a combination. As suggested, we will also use "two-stage analog/regression model" in the abstract.

We also agree that "small scale" precipitation is more suited in the present context than "local". We will change it in the manuscript.

3 Setup and Language

9. At various places within the paper (see comments below) parts of the methodology are explained. I suggest that the introduction is reworded and a section of the introduction is established that clearly and concisely explains what is done in one paragraph. This should also include an explicit statement of the goal and the novelty of the research.

We thank the referee for those suggestions. We will modify the introduction accordingly.

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4 Major Comments

4.1 Section 2 Data

10. Here, there is a distinction between 'analog stage' and 'regression stage' – are these two stages what is mean when the authors refer to as a hybrid approach? This gets back to my original question: In the analog stage, are the authors looking for all May30's in the past or only those May-30's where the pattern of the geopotential field was similar on the May-29's? How was this similarity determined?

As mentioned in Question8, the "hybrid" approach indeed refers to the two stage analog/regression approach.

Let consider that the prediction has to be done for May-30's, 2018. In the analog stage, we only consider days that are analogs in term of atmospheric circulation to the atmospheric circulation state of this day. As stated in the manuscript, the analog days are identified within a restricted pool of candidate days, namely all days of the archive that are included in a calendar window of ± 30 calendar days centered on the prediction day. In the present example, all May 1st to all June 30's from all years of the archive period (1982-2001 ; 20 years) are considered as candidate days (this corresponds to 1200 candidates among which only 100 days will be selected). The prediction day (May-30's, 2018) and its 5 preceding and following days are excluded from the candidates. The similarity is measured via the Teweless Wobus score which

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compare the shapes of the geopotential fields.

We will adapt the text to make this analog selection step clearer.

11. why 13 predictors? Is this enough? For what goal?

The 13 predictors used for the regression stage gather most predictors considered in previous studies over Europe (e.g. Hanssen-Bauer et al., 2005; Wetterhall et al., 2009; Horton et al., 2012; Raynaud et al., 2016). They include predictors characterizing the thermal state of the atmosphere, its dynamics, the water atmosphere content, its thermo-dynamical instability.

As mentioned in the conclusion, the predictors classically used in similar downscaling works were selected owing to their prediction skill. This skill is classically the mean prediction skill evaluated for all days of a given time period. We show that some predictors could have no or fairly no prediction skill for most of the days but could be informative for very specific location/situations. Further works could thus indeed consider the interest of using other predictors, possibly non-conventional ones, as they may reveal, for very specific situations, to be very informative.

This point is already mentioned/suggested in the discussion but we will clarify it further.

4.2 section 3 The hybrid analog/regression model.

12. The hybrid analog/regression model the approach of using a distribution function with a portion of zeros is clear.what is not so clear, is how the parameters are estimated

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and why this is treated independently?

As mentioned in the manuscript, (line 26 p6), the parameters are estimated using the Iterative Re-weighted Least Squares algorithm (IRLS, Nelder and Wedderburn, 1972). The significance of the regression coefficients is assessed by the Z-test (resp. the Student t-test). Because of the mass in zero, the precipitation distribution is modelled in two parts: the probability of occurrence and the distribution of non-zero amounts. The estimation of the parameters is indeed done independently, for precipitation occurrence probability first and for non-zero precipitation amounts next. This is the way the estimation is classically done (e.g. Stern and Coe, 1984; Chandler and Wheater, 2002). This allows also the selected predictors to differ for the two variables to predict.

13. Should the amount of precipitation not be a random variable drawn from the distribution depicted in Figure 1? It could then be either zero or some precipitation intensity other than zero.

The amount of precipitation is indeed a random variable with a given distribution which varies from one day to the other. The objective of our approach is to model this distribution and its day dependency and to further use it as probabilistic prediction. For any given prediction day, we do thus not draw some realization from the distribution. If a single scenario would be required for the prediction day, a random realization could be indeed drawn from the distribution leading to either zero or some non-zero precipitation intensity.

14. Why is npi estimated separately from the parameters of the distribution function? (I am assuming parameters, even though Figure 1 suggests the use of an empirical distribution) Can those parameters not be estimated jointly? Now, it seems like currently

npi is estimated via a GLM, which seems to be an improved multiple regression with the secondary variables going into xËĘo (Eq.2).

The probability of occurrence pi is indeed estimated with a GLM where the x'o are the explanatory variables. In eq. 2, pi is the probability of occurrence estimated with the prediction model for the target prediction day. This prediction results from 1) the downscaling relationship estimated for this day from the n-analog dates (it thus depends on the predictors retained for this day and on the corresponding parameters estimated for this day) and it results also from 2) the values of the different predictors observed for the prediction day (used in a second step for the prediction with the downscaling relationship). pi is thus necessarily estimated after the parameters of the distribution have been estimated.

Figure 1 actually suggests that the distribution function is empirical. It is indeed empirical when it is estimated with the AM25 backup analog model. Figure 2 clearly shows however that this distribution can be updated thanks to a parametric model.

This will be clarified in the future manuscript version.

15. it is not clear what the difference between superscript o and superscript q is in Eqs 2 and 3.

The predictors identified for precipitation occurrence (o) can differ from those identified to predict the non-zero precipitation distribution. The different superscripts refer to the fact that the two sets of predictors are specific to the occurrence (o) or to the quantity (q). This notation will be clarified in the paper.

16. How does the Gamma distribution come into the game? Are you using this type of

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distribution to model the non-zero part of the distribution? Why Gamma?

The gamma is indeed used to model the non-zero part of the distribution. This distribution is a widely used distribution in the hydrological literature for the non-zero amounts (e.g. Stern and Coe, 1984, Chandler and Weather 2002). It has the advantage to be rather robust and it does require only 2 parameters to be estimated. Other distributions could be considered in our approach to model this nonzero part. See Question6 for further comments on this point.

17. Also, the logic in p6 lines 13,14 is off. I think you should use a distribution that fits somewhat well to the data and then fit its parameters to the data.

We agree that lines 13-14 are not well written. We use the gamma distribution for the strictly positive precipitation. The method of moments is used to estimate the shape parameter of the distribution. Equation (4) expressed the way the variance is computed. This paragraph will be rewritten in the new version of the paper.

18. what determines how "near" an analog is to the predicted day? (likely this is answered in Sect. 3.2).

As indeed mentioned in section 3.2, analog are identified based on their similarity in terms of the shapes of geopotential fields. The Teweless Wobus score is used to measure this similarity (see also no 10).

19. why is the threshold for precipitation 0.1mm?

This threshold is often used in the hydro-meteorological literature to define wet / dry days. It corresponds in France to the precision of the bucket capacity used in the raingauge devices used to measure precipitation.

20. p6 lines 23 ff. are difficult to understand. Say again you are trying to predict May-30 2018 in one grid location of France. Then you are searching for the "nearest" geopotential conditions for all May-29 in the past and then estimate npi based on the precipitation occurrences in those days.

Thank you for the suggestion. As suggested, we will clarify the process of analog selection.

21. The "nearest conditions" could be different for a neighbouring cell? What does this say about consistency and spatial dependence structure of precipitation fields.

Yes, the referee is right. The "nearest conditions" could be different for a neighbouring cell. They are however not as different, as illustrated in Chardon et al. (2014). In a configuration where the analog model is optimized for each location, Chardon et al. (2014) show that, for a given prediction day, the analog dates are very similar from one grid cell to the next. This close similarity covers rather large domains (up to a few 100's of kilometers) excepted in regions with significant relief (the "nearest conditions" can be actually rather different on the western side and eastern side of the "Massif Central" mountainous region for instance).

The similarity between analog dates makes possible the development of relevant spatial scenarios (which are especially coherent in terms of spatial structure) as a given analog date can be used as scenario for all locations of a given spatial region.

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22. Also for Jun-1 2018, again a potentially very different set of "nearest conditions" could be used? Or am I understanding this wrongly, and there are more constraints?

As we consider each prediction day independently from the previous ones, the nearest conditions of Jun-1 2018 could be indeed very different from those of May-31 2018. This is however not the case due to the strong persistence of the large scale atmospheric dynamics which makes one day often rather similar from the previous one. Note again that we do not aim to develop time series scenarios of precipitation and that we did thus not introduce any specific constraint to cope with this temporal issue.

23. Why are you using the BIC (and not another criterium)?

In our application we have also tested the Akaike information criterion (AIC). Both criteria give the same results.

24. I would suggest a more careful wording when the word "significance" is employed. Arguably, a predictor can be significant at a certain level, but not plainly not significant (p6 line 26ff) – what level of significance did you choose?

We used the 5% significance level. This information will be added in the new version of the paper.

25. p8, l21 you start to use a differently typeset "P" after the abbreviation "ESP" – please explain.

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Thank you for the remark. Yes the “typeset P” should appear the first time we introduce the abbreviation of a given Ensemble Prediction System, named P.

26. Figure 2: top right panel: should there not be dots on the black line? At least for the part "within" npi?

Thank you for the comment. You are right and we will correct it.

4.3 section Results

27. p11 l12ff: you write that the BSS gain is "very sensitive to topography". The coasts along the Mediterranean (E portion of southern coast of France) and the Atlantic (W portion of northern coast of France) have opposite BSS gains (Fig 3b). How does that fit to your explanation?

We agree that our formulation was confusing. A first result is that the BSS gain presents a high space variability. The gain is higher in the mountainous areas (Pyrenees, Massif Central, Alpes, Vosges) but topography is indeed not the only factor that influence the gain as important gains are also obtained for the whole Mediterranean coast. We will reformulate the text accordingly.

28. p11 l32: what do you mean by "greatly and thus significantly"?

We agree that the formulation was awkward as we did not evaluate in a statistical meaning the significance of the gains. We will reformulate the text accordingly.

4.4 section Discussion

29. Generally, this section reads as a strung together explanation of what is shown on several figures. What does it mean remains more unclear than the authors probably think...

The section "results" shows and discusses the improved prediction skill obtained with the hybrid approach. Within the "discussion" section, we wanted to give some illustration on the adaptive behavior of the model, both in space (with different selection frequencies of given regression structure from one place to the other) and time (with the differences from one weather type to the other for instance). We acknowledge that the meaning of the results presented in the figures of this section is not as clear and further work would be worth for a deeper analysis of the model's behavior. What seems to be clear however, is that the most interesting predictors cannot be considered the same all the year and everywhere. We are going to comment this point in the new version of the paper.

30. can the selection of structures (what is visualised by Figures 8 and 9) be done in a more quantitative way (contribution of each variable to the prediction)?

Another possibility would have been indeed to present the percentage of prediction

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days each predictor has been selected. This however prevents to identify which combinations of predictors are more often selected if any. We thought however that it was important to understand which predictors were associated for the prediction. This is the reason why we have preferred to present the selection frequency of the different structures.

31. p19 l1: Please describe first what your point is, then what is visualised on Figure 10).

We will reformulate the text as suggested. Our point is that one single regression structure is not necessarily the best one for all large scale situations. This is what is illustrated in Fig10 with important differences, from one weather type to the other, of the selection frequency of the regression structures considered here.

32. p22 l4ff: you write that the gain is "non-negligible". Then you write that it is "up to 0.1". – Can you quantify how much of a gain this really is?

Thank you for the remark. The gain is up to 10 percentage points (or in relative value up to 0.1). The best value of both the BSS and CRPSS score is 100 percentage points (or in relative value, 1). The gain is here thus non-negligible indeed.

5 Minor Comments

33. p 16, last word: necessarily

This will be done

34. p2 line 35: remove "obviously" or explain how this is obvious.

This will be done

35. throughout the paper: frequent use of "classic" and its funnily sounding adverb. What is classic in the sense of analog hydrological methods?

This refers either to a standard approach or frequently used approach. We will use more appropriate terms in the revised manuscript.

36. The authors mention multiple times (e.g. p3 l7, p3 l14) a relation of the presented methodology to physical (maybe deterministic?) Is the goal of the presented approach to be "physically realistic"?

The goal of our work is of course not to obtain a physically realistic approach. We will reformulate the text to avoid such confusing statement.

Statistical downscaling models have to extract a statistical relationship between some large scale variables and local scale precipitation. From a physical point of view, the main physical processes, responsible or partly responsible of precipitation, can change from one weather type to the other. The most relevant predictors are also expected to change. We just wanted to point out that the statistical relationship which has to be identified is expected to be more relevant if it is estimated for a subset of days which *are similar in terms of large scale atmospheric configuration. This is actually expected to allow for a better identification of the most important driving large-scale variables.*

37. How / in what sense does this lead to something "more relevant and robust" (p3 l14/15)?

The relevance is expected to be improved as discussed above.

From a statistical point of view, exploring the large scale- local scale downscaling relationship on a subset of days which are similar in terms of large scale atmospheric configuration is expected to allow for the estimation of a better quality statistical model. The prediction skill of the model is thus also expected to improve. We also think that the hybrid approach increases the robustness of the approach. This is however impossible to prove from a statistical point of view. We will therefore remove the notion of 'robustness'.

38. Table 1: H, W, PV: of what variables?

The vertical velocity at 700hPa is the vertical wind at the 700hPa pressure level. This predictor is particularly useful to locate synoptic fronts which are characterised by strong upward winds leading to condensation and precipitation. The 700hPa level is usually chosen as vertical velocities reach a maximum in the mid-troposphere.

The helicity quantifies how the horizontal wind vector changes in intensity and direction with the altitude. As an example, the following figures called hodograph presents how to compute the helicity. V0, V1, V2 are the horizontal wind vectors at different pressure levels (from 1000hPa to 500hPa in our case). This helicity is simply the area marked out by these vectors. In addition to the usefulness of this parameter for convective *systems, a brutal change in helicity over a short distance can help detect synoptic fronts.*

Figure R3 Scheme for the computation of wind helicity from wind vectors (see supplementary material)

Finally, the potential vorticity is a parameter that can highlight the areas conductive to the development of low pressure systems. In our case, looking as the PV field at 400hPa, it is rather used to locate some potential instruction of stratospheric air (used called PV anomaly) which can trigger some strong vertical velocities and precipitation.

The full mathematical definitions of these 3 parameters can be found in Holton and Hakim (2012).

We will modify the description of these 3 variables for :

- potential vorticity of the atmosphere at 400hPa

- vertical velocity (vertical component of wind) at 700hPa pressure level

- helicity of horizontal wind integrated from 1000 to 500 hPa pressure levels

39. p11 l4: "two" or "four" or something else?

We have two predictor sets with four predictors each. This will be clarified.

40. p13 l19: what does "next too low" mean?

We agree that the text was confusing. It should have read "thus too low" and not "next too low". This will be modified.

When all analog days are dry (no wet days in the set of analogs retained for the prediction day), it is obviously impossible to fit any occurrence probability model. The same applies when only a few analog days are wet. There is actually no fixed value for this "too low proportion of wet analog days" for which no model can be estimated. It roughly corresponds to 0 to 20% of the days but it depends on the value of the predictors corresponding to those days. As mentioned in the manuscript, the regression model is retained or not according to the significance of the regression parameters.

41. Style: there are many abbreviations, and it's easy to forget what they all mean.

We agree that we use a lot of abbreviations in the paper. We will add an appendix with a glossary

42. sometimes you write "metropolitan French territory", sometimes "France". It seems like you never looked at Paris or the major cities specifically, hence I suggest to use "France" everywhere.

This will be accounted for.

43. p20 l2: I don't think the interest has been explored. Rather, the model itself has been explored?

We agree and will reformulate the text accordingly.

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