Reply to comments of referee #1

First of all, thank you for carefully reading the manuscript and for the constructive feedback.

Some referee comments are recalled in italics and followed by the authors' responses. Others are addressed here, but without being recalled. The technical corrections or rephrasing are not discussed here, but will be performed.

- Introduction: The structure will be changed and the description of the method moved into "Data and method". Additional references will be added, and different variations will be presented, instead of relying on another paper to cover the literature review. The introduction will thus be rewritten.

- *The issue discussed in the manuscript goes beyond this "fixed/moving" window issue. It more generally consider influence of the size of the data set (including the length of the archive) on the prediction skill. I am not aware of works that explored this issue but references to those, if any, and associated results have to be presented in the introduction.*
  The issue of the archive length will be better described, according to the relevant literature.

- *The final objective of the work is to increase the prediction skill of the method. Other means are possible for this. They should be also mentioned.*
  We will mention other means to increase the prediction skill in the introduction.

- MTW strategy vs longer archive: We agree that the MTW might not be as performant as a 4 times longer archive, for the reasons provided. We indeed observed a gain in performance similar to doubling the archive. Your point on the transient climate is also interesting. We will add a discussion on these aspects in the paper.

- Number of analogs: we totally agree with the referee that when the choice of the predictors or the parameters are improved, leading to a better prediction, the optimal number of analog situations thus decreases. However, when the length of the archive increases, the optimal number of analogs increases too, for a better performance, up to a certain threshold. This has been demonstrated in the PhD of G. Bontron (2004, p. 227), and we can also see it in Fig.7 of Hamill et al. (2006). This increase in the number of analog situations results objectively in better performance skills in our case. The MTW enriches the pool of available situations, even though they are not fully independent. We will improve the clarity of this analysis. It is true though that the choice of predictors can be improved and auxiliary predictors are missing. The chosen methods are not the most recent ones, but are benchmarks as they have been used by several studies as references, and the results are easier to interpret than more elaborated methods. A note on that will be added.

- *Explain both terms of the decomposition. Another well-known decomposition of the CRPSS is that of Herbasch. Please mention it also and clarify the advantage of that of Bontron. It is not clear at this stage.*
  We will remove the analysis of the sharpness and accuracy, as it brings unneeded complexity without being very instructive.

- *Why using a non conventional notation for the CRPS score. Please use the classical one (CRPSS) or justify*
  It is the journal convention for variables in equations ("Multi-letter variables should be avoided. Instead use single-letter variables with subscript (e.g. E_{RMS} instead of RMSE, or E_T instead of ET)."). It has been changed elsewhere.

- Resolution of the atmospheric reanalysis: it was shown by some studies that the resolution of the reanalysis dataset does not improve significantly the performance of the analogy of the atmospheric circulation. However, a full analysis of this aspect is out of the scope of this publication, and is a topic we are working on right now. Nevertheless, we can assume that it does not alter the fact that we can find better analog situations at different hours of the day.

- Seasonal stratification: yes, we consider a seasonal stratification. It is mentioned in l. 36-39. This should be more clear after the change in the paper structure.

- *You use the mean intensity of rainfall as a proxy of "dynamism of the atmospheric situation". Another proxy could be the intensity of the variations within the geopotential fields (e.g. mean gradient value). Why did you not explore this? It would better fit to your "dynamism" concept.*
  Yes, that could be a possibility. However, even though the link between the dynamism and the precipitation amount is not direct, the interest in analyzing it this way is that it highlights improvements we are directly interested in: a better prediction of high precipitation amounts. This will be rephrased to focus more on the precipitation thresholds rather than the dynamism of the circulation pattern.

- Sharpness and accuracy: We will remove the analysis of the sharpness and accuracy, as mentioned previously.

- Figure 11: this figure is complex and not very instructive. It will be removed along with the sharpness and accuracy analysis.

- Tables: we agree that there are many tables. We will remove some unanalyzed data, such as the spatial windows in Tables 3, 4, 7, and 8 in order to group the remaining information. Tables 10 and 11 will also be removed along with the sharpness and accuracy analysis.

- The other unmentioned detailed issues will be fixed

Reply to comments of referee #2

The authors would like to thank Referee 2 for his/her positive comments on the manuscript.

Some referee comments are recalled in italics and followed by the authors' responses. Others are addressed here, but without being recalled. The technical corrections or rephrasing are not discussed here, but will be performed.

- Sampling of extremes: The topic of extreme values within the analog method should be addressed in details, we agree on that and plan to work on it. However, it is out of the scope

of this paper. The proposed MTW improvement does not change the limitation of the maximum observed values in the archive, but it is not the topic of the present paper. We will however add a note on that issue in the paper.

- Introduction and applications in hydrology: The introduction will be rewritten, as explained to referee #1, and references will be added. Some applications to hydrology will also be cited.

- *Given the focus of the paper on the sub-daily time step, I suggest to refer more on the needs in urban hydrology.*
  The sub-daily time step is introduced in the search of analog situations, so at the predictors level. The predictand remains a 24-h precipitation total, which limits the application to urban hydrology.

- How the sample sizes N1 and N2 are determined and optimized: Indeed, this has not been detailed here. We will provide more insight on the calibration procedure.

- The other unmentioned issues will be fixed

Reply to comments of referee #3

We would like to thank Referee 3 for his detailed and relevant review.

Some referee comments are recalled in italics and followed by the authors' responses. Others are addressed here, but without being recalled. The technical corrections or rephrasing are not discussed here, but will be performed.

- Introduction: as replied to referee #1, the introduction will be rewritten, with an improved literature review and context description. The method description will be moved to the methods section.

- *The title wrongly suggests that these conclusions are valid only in a forecasting context, while they actually have much more general implications.*
  We agree that the application of the MTW is not limited to forecasting. The title will be changed to a more generic term.

- The structure will be improved, with a better separation of methods, results and discussion.

- *Notations: Please use conventional abbreviations for commonly used quantities: Teweles-Wobus Score → TWS or S1, continuous ranked probability (skill) score → CRP(S)S, root mean square error → RMSE*
  It is the journal convention for variables in equations ("Multi-letter variables should be avoided. Instead use single-letter variables with subscript (e.g. E_{RMS} instead of RMSE, or E_T instead of ET)."). It has been changed elsewhere.

- The number of tables will be decreased, as explained to referee #1: "We will remove some unanalyzed data, such as the spatial windows in Tables 3, 4, 7, and 8 in order to group the

remaining information. Tables 10 and 11 will also be removed along with the sharpness and accuracy analysis, which is not very informative."

- *L15-17: Is it not rather because heavy precipitation events are rarer?*
We cannot exclude this argument, and it might be a mix of both factors. We will add a note on that aspect.

- *L24-25: I don't understand*
This reports to section 4, but might not be necessary in the abstract as it brings some confusion.

- *L63: What are the parameters to calibrate? Please list them.*
More details will be provided on the calibration procedure.

- *L70: Please either provide a peer-reviewed reference for this decomposition or detail it here.*
We will remove the analysis of the CRPS decomposition, as it brings complexity without being very informative.

- *L76: Please detail the computation of the climatological distribution.*
A description will be added.

- *L93-94: I don't understand.*
This will be removed as it relies on partial analysis.

- *L113-114: Please justify the use of such an outdated global reanalysis (I understand this is partly for having a long time coverage). And add also the potential of using more recent and products with higher quality to the discussion.*
See answer to referee #1. These points will be addressed in the discussion.

- *L168-174: This analysis is done for different classes of precipitation values. Whether this relates to the intensity of circulation dynamics is another issue.*
We will reformulate this section.

- *L192-193: Figure 8 is not necessary. Please remove of put it in a supplementary material.*
The figure will be removed.

- *L219-220, "No relationship [: : :] criteria": I don't understand.*
This will be removed.

- *L246-250: Is it shown somewhere in the manuscript?*
No it is not shown, as the figure globally is very similar to Fig. 10.

- *L350-354: Given recent studies on RCM biases, I have serious doubts that RCM precipitation is reliable enough for it to be use as observed series in this context.*
We agree with the referee and will change this sentence.

- Figure 11: We will drop the analysis of the CRPS decomposition, as it brings complexity without being very informative.

- *Table 13: The choice for preselecting these 4 points should be somehow justified.*
  You are right. These are simply the points surrounding the catchment. It will be explained.

- The other unmentioned issues will be fixed

Significant changes in the manuscript

The manuscript was heavily reprocessed, which makes the changes tracking (next pages) a bit useless… The major points are:

- The whole structure changed, following the referees' advices. It should be much clearer now.
- The introduction has been re-written and include more references to other works.
- The decomposition in sharpness/accuracy has been removed, along with its figure.
- 8 tables were removed and some information were integrated into a new figure.
- The discussion has been extended to cover several questions asked by the referees.
- Globally, there is almost not a sentence from the original document that hasn't changed.
- The document has also been through English editing.