# \*\*\*\*\*\* Response to Reviewer #1 \*\*\*\*\*\*

Response to general feedback: Thank you for your positive feedback. We removed many figures and tables and some secondary analyses in order to focus on the main message. We also added applications examples for hydrology mainly in the introduction. Please note that this paper is part of the special issue “Precipitation measurement and modeling: uncertainty, variability, observations, ensemble simulation and downscaling”, and so the focus on precipitation is justifiable in our view. Horton et al. (2016b) focused on the parametrization of GAs in order to optimize AMs successfully, not on their application on more elaborated AMs. This has been clarified.

Response to comment No. 1: We agree and added some sentences in the discussion (section 5): l. 575-580

Response to comment No. 2: The following has been added in the discussion about extremes: l. 635-645. The following has been added in the introduction about advantages of the AMs: l. 46-51

Response to comment No. 3: Some sentences were added in the introduction (l. 37-45).

Response to comment No. 4: About the optimal number of 4 predictors: the text was maybe not so clear, so it has been reworked (l. 319-334) and illustrated in figure 2. The general term “predictor” might be ambiguous here, because only geopotential heights were considered at this stage. So, there is no moisture predictors at this stage, only atmospheric circulation data. As shown in figure 2, with 4 geopotential heights, most of the predictive information seem to be integrated. Adding other thermodynamic predictors will increase the prediction skill, as shown with the moisture index.

Response to comment No. 5: As mentioned to reviewer #2, larger precipitation values contribute more to the performance score. Thus, GAs will optimize these days more thoroughly than the sequential calibration as they are more powerful and can handle supplementary degrees of freedom. The following sentence was added in section 3.3: l. 390-393

Response to comment No. 6: Yes, it is related to different climate properties of the subregions, mainly for heavy precipitation events. We developed a bit further this point in section 3.3 (l. 394-410) and we hope it is now more understandable.

Response to comment No. 7: For the sake of conciseness, the analysis of the CRPS decomposition into sharpness and accuracy has been removed as it was not extensively analyzed

Response to comment No. 8: A paragraph was added with recommendations (last one of section 4.2, l. 525-534). Another part was added to discuss the spatial variability of moisture variables (as the main difference between the 2 less transferable subregions with other regions is the spatial windows on which the moisture is considered).

Response to comment No. 9: Maybe the sentence was not clear. We use the 4-month preselection window anyway. It was the optimization of the length of this window that was unsuccessful. The 4-month preselection window was introduced some decades ago as an improvement of a fixed seasonal preselection. It is expected to be better than a fixed seasonal window.

Response to comment No. 10: The number of figures was decreased to 10 and the number of tables to 6. We removed the following elements:

- The analysis of the CRPS decomposition into sharpness and accuracy, as it was not extensively analyzed, along with Fig. 4 and 10.

- Tables 3 and 7 (containing the resulting parameters for the Chablais subregion) were removed as the same information can be found in figures for all subregions.

- Table 4 was removed (values of the CRPSS score) and figure 3 and 8 were changed in order to represent the values of the CRPSS score instead of the relative difference.

- Figure 9 (CRPSS of the 4Zo-4Mio method) was removed as the results are very similar to those of the 4Zo-2Mio method, and thus not so interesting.

- Figures 5 and 6 were merged into a single figure, as well as figures 13 and 14.

- Figure 12 (relationship between the different number of analogues) was removed as it is a bit redundant with figure 11.

- Figure 15 (Optimized weighting for the pressure levels of the 4Zo method) was removed and standard deviations were added to figure 16 in order to show the variability.

Response to other editorial issues:

- abstract: parameter inter-dependencies, not parameters inter-dependencies (check also elsewhere in text): Corrected, thanks.

- line 38: presents: Corrected, thanks.

- typo in the subscript in Equation (1): We didn’t find any mistake here… The score name is written S1.

- line 138: Figure 17 where?: Some clarifications were added

- line 173: what are left side valleys?: This was changed for “southern valleys”

# \*\*\*\*\*\* Response to Reviewer #2 \*\*\*\*\*\*

Response to general feedback: Thank you for your very nice feedback.

Response to comment No. 1 (Page 4, Line 66): We changed that, thanks.

Response to comment No. 2 (Page 5, line 97): Good point. We added a note on that.

Response to comment No. 3 (Page 5, line 8-9): There is a slight increase in performance due to the smoothing of local variability when working on local averages. The difference is however rather low. A comment has been added.

Response to comment No. 4 (Page 6, line 122): The climatological distribution of precipitation usually considered is the one from the entire archive. It could be discussed that a climatological distribution built on the +-2 months’ window (thus seasonal) might be more relevant. However, most applications use the distribution of the full archive. This does not play a major role in this paper, as we are interested in the improvement relatively to reference methods.

Response to comment No. 5 (Page 9, line 200-202): No overlapping constraint of the spatial windows means that they can differ from one pressure level to another (this has been specified in the article). “what the sequential calibration cannot do” was changed for “which cannot be achieved with the sequential calibration technique”.

Response to comment No. 6 (Page 10, line 226): The assessment of the optimal number of predictors has been performed again on 3 subregions in order to consider the weighting between the predictors in that process. Consequently, this has slightly changed the results (not anymore a clear decrease on the VP), but it has not changed the conclusion that 4 seems to be an optimal number of predictors. A figure (Fig. 2) has been added to illustrate this aspect.

Response to comment No. 7 (Page 13, line 316): This is a good idea. This point was added in the discussion section (l.597-606).

Response to comment No. 8 (Page 14, lines 351- 356): There are two different elements here: Indeed, larger precipitation values contribute more to the error function. The following sentence was thus added in section 3.3: l. 390-393. However, the reference methods are calibrated by means of the sequential procedure, which also aims at reducing the error function. GAs can reduce the errors to a greater extent than the sequential procedure thanks to more efficient techniques and more degrees of freedom. We realized that the establishment of the references is not clear enough and thus we added a section on the sequential calibration (2.5).

Response to comment No. 9 (Page 17, line 435-438): It is likely to play a role. Indeed, the main difference with other regions is the spatial windows on which the moisture is considered. We added a comment on that in the end of section 4.2 (l. 518-524).

Response to comment No. 10 (Page 17, line 450): We now mention this point: l. 548-551

Response to comment No. 11 (Page 20, line 536): We consider this idea interesting, but not feasible, because of the unlimited possible combinations. Indeed, the predictors are considered at different pressure levels, temporal and spatial windows.

Response to comment No. 12 (Figures and Tables): We removed 7 figures and 3 tables. See the last comment to referee #1 about what was removed.

Response to minor comments:

- Abstract, line 2 - “provided by global models” is a bit too general. I would rather use general circulation models or numerical weather prediction models: Corrected, thanks.

- Abstract, par 2, line 2 - “strong limitations”. You could complete the sentence by listing a couple of them: Some information were added.

- Page 2, line 9 - “Other predictands are also often considered”. Here I would also add which ones, e.g. …: Examples and references were added.

- Page 2, Line 15 - “get down” 🡪 resolve, compute, forecast. I would use a more appropriate term: Corrected, thanks.

- Page 2, Line 21 - “made” 🡪 “designed”?: Corrected, thanks.

- Page 3, Line 26 - “criterion itself” or “criteria themselves”: Corrected, thanks.

- Page 3, lines 27-29 : A sentence referring to this work was added at the end of the paragraph.

Page 3, Line 30 - I would find a better term for “reconsidering”: Corrected, thanks.

Page 3, Line 31 - “pressure levels” 🡪 “optimal pressure levels”: Corrected, thanks.

Page 3, Line 44 - “on precipitation predicting” 🡪 “for precipitation prediction”: Corrected, thanks.

Page 4, Line 74 - “of the geopotential height”. I would add “, which represent better the upper level flow direction”: Added to the text, thanks.

Page 5, Line 81 - “both North and East directions”: Added to the text, thanks.

Page 5, line 102 - “Predictors are generally extracted from reanalysis datasets”: Added to the text, thanks.

Page 6, line 111 - It would be interesting to mention that you are trying to verify the performance of an ensemble-probabilistic forecast technique: A sentence has been added.

Page 7, line 133 - “complex surface”. You could add that you are trying to find the global optimum of a complex high-dimensional error function having multiple local optima: A sentence has been added.

Page 8, line 165 - Here you could add that the high spatial variability of precipitation is due to complex orography: Added to the text, thanks.

Page 9, line 196 - “are not provided in this paper”: Added to the text, thanks.

Page 9, line 204 - “respectively, w.r.t. the reference method based on Z500 and Z1000” : Added to the text, thanks.

Page 9, 205 - “tremendous” 🡪 “very significant”, “large”: Corrected, thanks.

Page 9, line 209 - “other parameters (…)”: Information added.

Page 9, line 211 - “and may” 🡪 “but may”: We meant “and”, because both points are negative consequences.

Page 10, line 225 - “but always more to a smaller extent” could be rephrased: Corrected, thanks.

Page 10, line 229 - “another region than Valais” to clarify that it is not another region within your domain: Clarification added.

Page 10, line 239 - “name” 🡪 “named”: Corrected, thanks.

Page 11, line 263 - “cross-compatibility and spatial coherence of the optimized parameters”: Information added.

Page 11, line 276 - “significant preference in the AM” is not clear: The sentence has been removed.

Page 14, lines 360-365 - Does this mean that the two levels of analogy bring complementary information (not independent)? This is a good finding: Thanks. A note has been added.

Page 15, line 382 - “spatial shift”? : No, a vertical shift (clarification added)

Page 18, line 481 - “does not”: Corrected, thanks.

Page 20, line 522 - “what the sequential calibration” is a strange expression to me: Corrected, thanks.

Page 21, line 544 - “dependence in the selected parameters”: Corrected, thanks.

Page 21, line 555 - “significantly more improved” 🡪 “improved further” or other: Corrected, thanks.

Figure 2 and 7 - Would it be better to put the actual pressure levels (Z500, Z1000, etc) instead of the four levels (Z1, …, Z4)?: Corrected, thanks.

Table 1-3 - It is not clear if the provided hour (12h, 24h) is for the day before the target day that we want to forecast: It has been clarified. It is the hour within the target day. In a forecasting application, the AM is applied to the NWP model outputs, thus letting the temporal extrapolation to the NWP model. It is then more an “adaptation technique”.

Table 6 - In the caption I would make clear whether the improvement is w.r.t. climatology or the reference method: Clarification added.