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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Optimising predictor domains for spatially coherent precipitation downscaling" *by* S. Radanovics et al.

S. Radanovics et al.

sabine.radanovics@irstea.fr

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Text in bold face correspond to the reviewer's comment, while the authors's answers are in normal font.

The paper is interesting and do contain some original analyses on the spatial coherence of the predictor domains of the analogue method.

The authors would like to thank P. Horton for this positive comment.

Please consider some specific comments:

1. You state that the spatial domain is rarely optimized with respect to the target predictand location (p. 4016, lines 5-6). However, this is not exactly true: most

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applications of the analogue method use a spatial domain that is optimized for a catchment or a region. It may not be a fine tuning for very local time series, but most users do calibrate the spatial domain, for example using the basic algorithm you describe further on.

The authors agree on this comment. However, in most published downscaling studies, the optimisation of spatial domains is not mentioned, and if mentioned (see e.g. Marty et al., 2012), the description of the optimization process is rather vague.

2. You mention twice our optimization by means of genetic algorithms (p. 4020, line 11 + p. 4040, line 18). You`re right when you say that it requires substantial computational costs. However, the optimization is not only about predictor domains, it`s a global optimization for all parameters of the analogue method: the choice of the atmospheric levels, spatial windows that can be non-overlapping between the chosen atmospheric levels, corresponding temporal windows, weighting of the different atmospheric levels, number of analogues, etc. Maybe you can refer here to the thesis (Horton, 2012) rather than to the EGU abstract.

The text has been changed specifying that the optimisation in Horton (2012) is a global one. The references have been changed as requested.

3. When you describe the method (p. 4024, line 9 and following), you may specify that the predictor domain is overlapping for both atmospheric levels.

This is a quite relevant comment, and it is now made explicit in the text.

4. You should specify earlier what is your starting point, from which your optimization starts (e.g. in p. 4027, line 11).

The starting point is now specified in the second paragraph of section 2.5.

5. On p. 4027, line 19 and following, it is not clear if you expand 5 domains or every possible domain. Please be more consistent between the first sentence

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and the rest of the paragraph.

Like in the basic version the search is started from a given 2×2 grid point domain, here the nearest one to the target zone where not indicated differently. After calculating the CRPS this domain is expanded in 4 directions by adding one gridpoint east, west, north or south, calculating the CRPS for each of them. For the second step all 4 domains from the previous step are expanded. This gives 16 domains, but only 10 actually different ones, so 10 new domains are explored. From this 10 domains in the second step the 5 best are selected to be expanded in the next step. Theoretically there are up to 20 domains (5×4) to explore from step 3 on, but there is some redundancy or some domains already explored in a previous step so that between 13 and 18 actually new ones were found. In the end the five best domains found during the whole procedure, in general stemming from different steps, are returned. The text has been changed according to the above description.

6. Figure 6 is not very useful as you don't analyze the spatial distribution of the differences in CRPSS. It brings no information and I find the explanation on p. 4031 (lines 6-10) sufficient.

Figure 6 has been removed.

7. The beginning of section 3.3.1 (p. 4033) is not clear; particularly the first paragraph (lines 20-23). A reformulation would be welcomed.

The first paragraph and the beginning of the second one have been reformulated.

8. The beginning of section 3.3.2 (p. 4035) is a bit redundant with previous paragraphs (p. 4034).

The redundant sentences have been removed from the previous paragraph.

9. The influence of the archive length is interesting (section 3.3.3, p. 4035). As you identified different predictor domains according to the archive length, it would be interesting to quantify the loss of CRPSS when you switch the domains

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and the archives, especially for the locations with high inter-annual variability.

The authors would like to thank P. Horton for this very interesting suggestion. An additional calculation has been performed to answer this comment. For the Ardèche case study zone that has high interannual variability the CRPSS has been calculated over the 20 years period using the predictor domains optimised for the 44 years period. In this case the loss in CRPSS is 2.4% compared to the CRPSS obtained using the domains optimised for the 20 years archive. Using the domains optimised for the 20 years period for calculating the CRPSS over the 44 years leads to a loss of 1.8% compared to the case where the domains optimised for the 44 years archive are used. A paragraph has been added in section 3.3.3.

10. On p. 4037, line 4, you name for the first time this “break line", and it is not clear to the reader exactly what you are talking about (becomes clearer later).

The paragraph has been reformulated.

11. One of your main conclusions is on the assumption of a common predictor domain. I agree with you that your fine tuning will certainly improve a bit the skill of the analogue method. However, you also showed that there is a variability in between near-optimal predictor domains that has a minor influence on the CRPSS. Thus, you can certainly reduce variability without losing significant skill. Then, it would be nice to quantify your gain regarding a more global approach with a few predictor domains for large areas of the France territory. What is lacking here is the quantification of what you gain by doing this fine discretization.

An additional experiment has been performed using only one common domain optimised for the average precipitation over France. For details please have a look at the answer to specific comment number 22 of referee1, the new figure 5 and the revised section 3.2.1.

12. On p. 4038, lines 6-8 are not clear.

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Changed to: This method has been initially developed for the Seine basin by Boé et al. (2006, 2007). As seen in Figs. 7 and 8, for the Seine basin the unique predictor domain assumption appears reasonably valid because the basin is situated in a region with only minor variations in the optimised domains.

13. Figure 3 is not easy to explore due do some colors conflicts, especially between red and orange.

It is difficult to plot multiple overlapping domains on a plot such that it is still easy to explore. We could unfortunately not find a better way of displaying such information.

References: Horton, P.: Améliorations et optimisation globale de la méthode des analogues pour la prévision statistique des précipitations. Développement d'un outil de prévision et application opérationnelle au bassin du Rhône à l`amont du Léman, Thèse de doctorat, Université de Lausanne, Switzerland, 2012.

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Boé, J., Terray, L., Habets, F., and Martin, E. (2007). Statistical and dynamical downscaling of the seine basin climate for hydro-meteorological studies. *Int. J. Climatol.*, 27(12):1643–1655.

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Marty, R., Zin, I., Obled, C., Bontron, G., and Djerboua, A. (2012). Toward real-time daily pqpf by an analog sorting approach: Application to flash-flood catchments. *J. Appl. Meteorol. Clim.*, 51:505–520.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 4015, 2013.