Hydrol. Earth Syst. Sci. Discuss., 10, C1984–C1986, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C1984/2013/

Hydrology and Earth System Sciences
Discussions

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

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Received and published: 24 May 2013

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

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.

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.

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

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 and the rest of the paragraph.

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.

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.

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

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

10. On p. 4037, line 4, you name for the first time this "break line", and it is not clear to

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the reader exactly what you are talking about (becomes clearer later).

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.

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

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

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.

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