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Date: 06 Nov 2016
To: "Pascal Horton" horton.pascal@gmail.com,pascal.horton@unil.ch
From: "Journal of Hydrology" hydrol-eo@elsevier.com
Subject: HYDROL21431 - Editor decision - revise

Dear Dr. Horton,

I can now inform you that the reviewers and editor have evaluated the manuscript "Using Genetic Algorithms to Optimize the Analogue Method for Precipitation Downscaling in the Swiss Alps" (Dr. Pascal Horton). As you will see from the comments below and on
http://ees.elsevier.com/hydrol/, moderate revision has been requested.

Please consider the reviews to see if revision would be feasible. Should you wish to resubmit you should explain how and where each point of the reviewers' comments has been incorporated. For this, use submission item "Revision Notes" when uploading your revision. Also, indicate the changes in an annotated version of the revised manuscript (submission item "Revision, changes marked"). Should you disagree with any part of the reviews, please explain why. To facilitate further review, add line numbers in the text of your manuscript.

Please strictly follow the formatting requirements as presented in the Guide for Authors.

Given that the requested revisions are moderate the new version is required within 1 month.

To submit a revision, go to http://ees.elsevier.com/hydrol/ and log in as an Author. You will find your submission record under Submission(s) Needing Revision.

When resubmitting, please present any figures, tables etc. as separate files. See the Artwork Guidelines on the home page right menu for further file naming conventions and format issues.

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I hope that you will find the comments to be of use to you and am looking forward with interest to receiving your revision.

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Thank you for submitting your work to this journal.

With kind regards,

Andras Bardossy, Dr-Ing
Editor
Journal of Hydrology

Note: While submitting the revised manuscript, please double check the author names provided in the submission so that authorship related changes are made in the revision stage. If your manuscript is accepted, any authorship change will involve approval from co-authors and respective editor handling the submission and this may cause a significant delay in publishing your manuscript.

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COMMENTS FROM EDITORS AND REVIEWERS

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Dear authors,

You have been served by two very constructive and detailed reviews of your paper. Based on those reviews and my own appreciation of your paper, I recommend to accept it after "moderate revision". Please provide an item-by-item response to each of the issues raised by the reviewers and indicate clearly in the revised manuscript how and where you have adjusted it. Finally, both reviewers recommend that you try to reduce the number of tables and figures and one of the reviewers also recommends to reduce the length of the paper as such. Please try to accommodate those requests (within reasonable limits). I look forward to handling a revised version of your paper.

Best regards,

Remko Uijlenhoet
Special Issue Guest Editor

Reviewer #1: The manuscript „Using genetic algorithms to optimize the Analogue Method for precipitation downscaling in the Swiss Alps" by Horton et al. addresses the problem of precipitation prediction by analogue methods (AM). The main goal of the paper is to present and test a new optimization method based on Genetic Algorithms (GA) which has in my opinion two main advantages to the traditional sequential procedure: (a) it optimizes all predictors jointly and independently; and (b) it allows for a free selection of the spatial integration window (and time) and can give weights to different pressure levels. The paper shows improvement compared to a reference method optimized by the sequential procedure. The analysis is conducted over the Rhone Basin in Switzerland divided into 10 different regions. Predictors come from NCEP-NCAR reanalysis, predicted precipitation comes from Meteoswiss stations averaged over 500 km2 grids.

My main impression is that this is solid work for people interested in improving AM for predicting precipitation. As far as I can tell methodologically the paper is correct and in my opinion the results are well explained. But I do have a concern about the interest for a general hydrological audience and suggest that the authors in their revision try to make the paper shorter and more focussed on the message and expand some discussion on the actual usefulness and application of the method to precipitation prediction. Also a clearer statement on the novelty of this paper compared to the recommendations of Horton et al. (2016b) is necessary (line 40-41). The paper can be published in JH after revision. My main questions/issues which the authors may wish to consider and/or expand on are listed below.

1. Problem of non-uniqueness. It is true that the global optimization method provides more freedom in the combination of parameters (predictors) and therefore performance. However this increase in the degrees of freedom is to me not necessarily a win-win situation. As the authors point out several different combinations of parameters may now lead to similar model performance, especially in the two analogue version. I do not see a constraint on parameters as necessarily negative. I have a feeling the authors could engage with this question more in their work.

2. The use for prediction. I am not very familiar with applications of AM for prediction of precipitation, but as I understood it from the paper only observed precipitation amounts and patterns can be reproduced (from the historical archive), even though N1 analogues can be selected and a "probabilistic forecast" from them derived. Do these N1 cases cover the entire uncertainty, including high precipitation (extremes) which have not been observed? In other words, what is the advantage of these methods against stochastic predictive models conditioned on similar (circulation and moisture) predictors? Do the authors have an opinion on this?

3. RCM is not an option? Although I agree that GCM precipitation performance is poor, in all fairness I think it should be mentioned in the Introduction (lines 11-20) that limited area RCMs are an alternative at space and time scales that are getting pretty close to station data, or rather to the spatial averaging scale for precipitation used in this study.

4. Methodology. I appreciate the approach in Chapter 5 starting from the two fixed pressure level reference and then releasing constraints, i.e. adding degrees of freedom, and eventually a second analogue (moisture). Although I agree that the added level of performance is not incredible, there is measurable improvement in the calibration. There is also consistency in the results, in that some pressure levels, etc., are consistently chosen over others. Finally it was concluded that 4 predictors are optimal because more dropped the performance in validation and the authors considered the model then over-parameterized. I would like to read some physical explanation of this feature in the paper connected to the fields of pressure and moisture and precipitation formation in the Alps. Why should it be that including more parameters, if they are responsible for rain formation, becomes eventually counterproductive?

5. Performance dependent on precipitation depth. The authors present that the performance of their GA-AM method is best for days with heavy rain. I am wondering what is the reason for this, especially considering that I would also expect the predictand derived from station observations averaged over 500 km2 grids to be most uncertain then. Meaning similar pressure/moisture analogues may lead to more different precipitation on the ground during heavy rain which is concentrated in space, but this would not reflect in the grid-averaged precipitation because the spatial density of gauges is not high enough to capture the large spatial variability all of the time.

6. Cross-compatibility. This analysis (lines 263-265, 431-438) is really interesting. Is there any spatial pattern evident in the performance drop in space that the authors find physically explainable? There is an attempt starting on line 308 which I do not fully follow.

7. Please explain in a sentence what accuracy and sharpness are (line 295) so that readers don't have to go to Bontron (2004).

8. Adding moisture analogue. The performance after adding the moisture analogue improved further, although 2 parameters were sufficient for this analogue. The transferability however starts to be a problem. This is an expected result because I expect atmospheric moisture to vary more significantly in space (one of many reasons?). It is however not super clear to me what the

authors recommend: should moisture be used as a predictor or not?

9. Optimization window. The preselection period of 4 months was also tested, without much effect on the results. I understood this was +- 2 months around the target date. Would the results improve if seasons are instead considered, for example the window is fixed for selected summer months to capture convective events, etc, regardless of the target date in that season?

10. Finally, I find the paper very long and cumbersome to read. It has 17 figures and 9 tables. I wonder if some of the figures/tables can be passed to Supplementary materials to help the reader focus on the main messages. This is only my subjective opinion.

11. Some editorial issues
> abstract: parameter inter-dependencies, not parameters inter-dependencies (check also elsewhere in text)
> line 38: presents
> typo in the subscript in Equation (1)
> line 138: Figure 17 where?
> line 173: what are left side valleys?

Reviewer #2: Dear authors,
please find in attachment my review.

Loris Foresti
Your Sincerely

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