

Interactive comment on “The Analogue Method for Precipitation Forecasting: Finding Better Analogue Situations at a Sub-Daily Time Step” by P. Horton et al.

Anonymous Referee #3

Received and published: 16 July 2016

This manuscript tests the sensitivity of an analogue downscaling method for precipitation to an extension of the potential analogue situations to 6-, 12- and 18-hour shifted analogue dates, with a high temporal resolution archive. Such an extension appears to increase the skill of the method as applied to 6 stations in Switzerland. The manuscript thus addresses a relevant scientific question within the scope of HESS. As far as I know, this idea have not been explored before and the conclusions make it sound appealing. The method seems sound, even if not clearly enough detailed at times, and the results support well the conclusions. The title wrongly suggests that these conclusions are valid only in a forecasting context, while they actually have much more general implications. A proper literature review is missing, and the number of tables is

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too large. These conclusions are detailed in sections below. I therefore recommend the manuscript to be reconsidered after major revisions.

General comments

1. Structure of the paper: the manuscript is strangely structured and should undergo many changes to improve its readability:
 - The introduction is for a large part a presentation of the analogue downscaling tool used and thus belongs to the “methods” section,
 - One consequence is a lack of a proper introduction, with a scientific context, a proper literature review, and so on. Positioning this study within the wider issue of the archive length (as noted by Referee #1) – that has been studied for quite some time (see e.g. Ruosteenoja, 1988; Van den Dool, 1994), and notably recently by Radanovics et al. (2013) – would be quite relevant. More generally, various properties of the analogue approach have been largely studied recently, like temporal transferability (see e.g. Dayon et al., 2015; Caillouet et al., 2016), or spatial transferability (Chardon et al., 2014), in more climate-oriented contexts. The moving time window proposed here would be perfectly suited for improving the different variants of the analogue methods in such contexts, and this should also appear in the revised version of the introduction or conclusion.
 - There is no discussion section, while several paragraphs from the “data and methods” section – or even the introduction – should belong to such a discussion section,
 - Several paragraphs from the “Results” section – or from section 4 – should belong to either the “methods” section or the “discussion” section.

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2. 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
3. Number of tables: there are much too many tables in the manuscript that could be either be put in a supplementary material or summarized through graphs.
4. Tense: please use the present tense for all description and analysis of the work carried out.

Specific comments

1. L3, “on the geopotential [...] gradients”: please rephrase
2. L7-8: sentence without verb
3. L15-17: Is it not rather because heavy precipitation events are rarer?
4. L24-25: I don’t understand
5. L33: Horton et al. (2016) is not published, even in GMD. You cannot therefore refer to developments and findings made in such a manuscript.
6. L33: Ben Daoud et al. (2016)
7. L34: What improvements? Please include this in the literature review.
8. L43-48: I presume this is for a 6h-6h precipitation totals, but it should be clearly stated here
9. L49-43: The archive should be described here.

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10. L63: What are the parameters to calibrate? Please list them.
11. L70: Please either provide a peer-reviewed reference for this decomposition or detail it here.
12. L76: Please detail the computation of the climatological distribution.
13. L81-82, “otherwise [...] them”. Please rephrase.
14. L92-97: Some parts of this paragraph belongs to the discussion section.
15. L93-94: I don’t understand.
16. L111-112: Please remove the sentence or remove the corresponding par in the next section.
17. 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.
18. L120, “validation”? Please describe in detail the experiment set-up.
19. L123, “based on [...] shown)”: please detail or remove.
20. L131: again, Horton et al. (2016) is not available, so you should provide a description of the calibration procedure.
21. L133-136: This belongs to the results section.
22. L146-147: An increase with what?
23. L148-149, “the latter [...] selection”: I don’t understand.
24. L156-157, “This [...] distribution”: already written above.

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25. L162-167: This belongs to the discussion.
26. L168-174: This analysis is done for different classes of precipitation values. Whether this relates to the intensity of circulation dynamics is another issue.
27. L180-183: This belongs to the discussion.
28. L192-193: Figure 8 is not necessary. Please remove or put it in a supplementary material.
29. L202-206: This belongs to the discussion.
30. L208-214: This belongs to the methods.
31. L215, “performance scores”: specify that these are CRPS values.
32. L219-220, “No relationship [. . .] criteria”: I don’t understand.
33. L246-250: Is it shown somewhere in the manuscript?
34. L273-281: This belongs to the methods section.
35. L286-298: This belongs to the methods section.
36. 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.
37. Figure 11: The bottom right panel is identical to the one to its left. Plus, choosing a display set-up with rows showing accuracy and sharpness, respectively, would allow different y scales and increase the readability.
38. Table 13: The choice for preselecting these 4 points should be somehow justified.

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Technical corrections

1. L2: “precipitation”
2. L145: “shape”
3. L162: “dynamics”
4. L209: “obligatory” → necessarily

References

Caillouet, L., Vidal, J.-P., Sauquet, E., and Graff, B.: Probabilistic precipitation and temperature downscaling of the Twentieth Century Reanalysis over France, *Climate of the Past*, 12, 635-662, doi:10.5194/cp-12-635-2016, 2016

Chardon, J., Hingray, B., Favre, A.-C., Autin, P., Gailhard, J., Zin, I., and Obled, C.: Spatial similarity and transferability of analog dates for precipitation downscaling over France, *Journal of Climate*, 27, 5056-5074, doi:10.1175/JCLI-D-13-00464.1, 2014.

Dayon, G., Boé, J., and Martin, E.: Transferability in the future climate of a statistical downscaling method for precipitation in France, *Journal of Geophysical Research*, 120, 1023-1043, doi:10.1002/2014JD022236, 2015.

Radanovics, S., Vidal, J.-P., Sauquet, E., Ben Daoud, A. Bontron, G.: Optimising predictor domains for spatially coherent precipitation downscaling, *Hydrology and Earth System Sciences*, 17, 4189-4208, doi:10.5194/hess-17-4189-2013, 2013.

Ruosteenoja, K.: Factors affecting the occurrence and lifetime of 500 mb height analogues: a study based on a large amount of data, *Monthly Weather Review*, 116, 368-376. doi:10.1175/1520-0493(1988)116<0368:FATOAL>2.0.CO;2, 1988.

Van den Dool, H. M.: Searching for analogues, how long must we wait?, Tellus A, 46, 314-324. doi:10.1034/j.1600-0870.1994.t01-2-00006.x, 1994.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-246, 2016.

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