Bridging the scale gap: Obtaining high-resolution stochastic simulations of gridded daily precipitation in a future climate

Qifen Yuan, Thordis L. Thorarinsdottir, Stein Beldring,
Wai Kwok Wong, Chong-Yu Xu

We thank the editors for handling the manuscript, and are grateful to two anonymous reviewers for their insightful comments. We have done our best to revise the paper according to their suggestions. Point-by-point responses are given below. Please note that the line numbers in reviewer's comments refer to the original manuscript "hess-2020-673-manuscript-version1.pdf", while in our reply refer to the revised version.

Response to Referee 1

This is a very meaningful study that puts forward a combination of statistical methods and approaches to produce fine scale resolution rainfall estimates for catchments (small to medium/large-ish sizes). It combines aspects of the weather-generator type approach with spatial statistics, acknowledging the need to also consider temporal dependencies and structures.

The language and graphics are clear and concise, explicit formulas are provided, accompanied by relevant references to underpinning methods and reference to used R-packages/methods.

Whilst I think that there are a one or two of issues to address method wise before it can be applied in a climate change context, I think it is a meaningful suggestion to build on. Overall, in my opinion this manuscript offers a very meaningful contribution to the peer-literature on statistical downscaling and merits publication with only minor edits.

Reply: Thank you for your positive evaluation of our work. We have modified the manuscript following your suggestions. Please find details below.

General comments

Given the focus of this paper, I suggest a few changes that would make the paper more accessible to a wider audience that should pay attention to this work but would find it

difficult to detangle the methodology (the paper has a very strong statistical flavour and assumes that readers are familiar with statistical terminology). *Firstly*, I suggest that you put in a conceptual graphic, that outlines the different components in our analysis and how they fit together, this will help the reader understand how the different model parts fits together, and where the 'change' is applied (change between the two time periods). *Second*, I think it will help the reader if you outline when you expect the model to work well, and when you expect there to be difficulties in the methods section. Some of the performance issues discussed in the discussion section are entirely predictable and I think it will help the reader understand the method better if you have a paragraph that speaks about the expected strengths and expected weaknesses before you conduct the experiments, this will get the reader introduced to how the method works, and then the results will make more sense. I think the manuscript will be more accessible and thus more successful if it embraces a wider audience.

Reply: Firstly, we have included a conceptual graphic in the introduction section, see Figure 1 in the revised manuscript. Second, we have added a paragraph in the introduction section (lines 61-69) where we state the two main strengths of our method as well as its two main assumptions.

One aspect not discussed in this paper is the applicability in a climate change context. Whilst the two time periods do exhibit some climatological differences, changes that one might expect in 20-50 years time are likely to be more pronounced. In this context, you may need to include a representation of oscillatory behaviour in the spatial dependency structure (in addition to a seasonal representation). This may not be the case for Norway, but other regions with more variable rainfall climatologies (strongly influenced by e.g. ENSO or IDO ... or any other large-scale climate oscillation or teleconnection pattern. I think it would be very meaningful if you could elaborate on this need in your discussion section, and perhaps propose a couple of tests to see if this is necessary, e.g. compare spatial correlation in subsets of RCM rainfall fields that capture certain seasonal/oscillation modes – are these still similar enough (how to judge?) to what we see in the coarse resolution observed rainfall fields?

Reply: This is indeed a very important discussion point for a practical application of our method in climate change impact studies. We have added a paragraph to the discussion section (lines 433-441).

Four smaller issues

(1) I think it is always useful to be critical of the RCM – hence I think the authors should point out the need to demonstrate that the RCMs selected for the analysis do indeed capture the spatial and temporal patterns of the variable of interest on its resolution before downscaling (where the downscaling is drawing on a relationship derived from observed data).

Reply: It is useful to check whether the spatial and temporal patterns of the RCM outputs are realistic at its own scale. In the newly added paragraph in the introduction, we have pointed out that for the success of the method the climate change signal projected by the RCMs must be correct and transferrable between the two spatial scales (lines 66-69).

(2) Using empirical scaling (or decile scaling) as a reference is meaningful because that is a very common method. However, many water-related studies would use daily scaling rather than monthly scaling as you are draping a scaling coefficient of monthly resolution onto a variable with daily resolution, then evaluating on the daily resolution. The comparison seems a little unfair. I see no need to redo your analysis, but I would point out how the monthly scaling could impact the comparison.

Reply: We have now briefly discussed this in Section 3.5 (lines 275-278).

(3) Sequencing of dry days (and wet days) is very important in many regions, hence if straight forward I would contemplate including a figure that captures this information.

Reply: In the results section (lines 395-398) we have added that we found the distribution of dry spells is similar across different methods for any given catchment in our case study. We have chosen not to show the figures. Nonetheless, we are happy to reconsider this if needed.

(4) It would be good to have a climatology map and a DEM for the catchment shown in Figure 3, it would help with interpretation of bias pattern.

Reply: We have added this in the results section, see Figure 5 in the revised manuscript.

Minor comments

P1. Line 17. What do you mean by 'changed runoff"' (magnitude, seasonal flow, low/high flow metrics?). Perhaps qualify this a little, otherwise it looks a little 'hand-wavy'.

Reply: This has been corrected, see line 17.

P2. Line 24. RCMs can explicitly resolve some process, though at 10-15 km, many processes relevant to rainfall events are parameterised. It would be more relevant here to speak about the convective permitting regional climate models that operate on 1- 4 km resolution (the recent UK national projections provide output on grid resolution just over 2km). The RCM modelling community is now pushing more towards the CPM scale rather than the 10-15 km scale. There are many recent overview type publications that you could cite here e.g:

Prein, A.F., Rasmussen, R., Castro, C.L. et al. Special issue: Advances in convection-permitting climate modeling. Clim Dyn 55, 1–2 (2020). https://doi.org/10.1007/s00382-020-05240-3

Reply: Thank you for bringing these recent publications to our attention. We have cited the mentioned paper in the revised manuscript (line 26) .

P3. Line 29-30. This sentence has a few errors in it, 'which' should be 'with' and 'differ' should be 'different'.

Reply: We have rewritten this sentence, see lines 30-32.

P4 Line 87 – whilst I am not too familiar with Norwegian climatology, I would suggest that Norway as a whole is maritime, compared to continental climates.

Reply: Since Norway has a complex topography and the seNorge data we use has very fine spatial resolution, we carefully consider that the nine catchments belong to two climatic groups: oceanic/maritime and continental, according to the modified Köppen-Geiger climate classification method applied by Yang et al. (2020). Therefore, we have not made any change to the text which now appears in lines 100-101 in the revised manuscript.

L403-408. To the reader this is a little confusing:

"Looking at the linear trend coefficient in the probit model, it seems that the seNorge data upscaled to 12 km resolution are generally able to capture the change that there are proportionally more wet days in the test period than in the training period, while the RCM data either project strong negative changes or simply no change in most catchments. For the gamma model, both RCMs seem to have projected correct changes in the trend and seasonality. For this reason, it cannot be expected that the RCM runs perfectly reproduce the precipitation patterns in the finer-scale seNorge data"

The penultimate sentence in this section says " For the gamma model, both RCMs seem to have projected correct changes in the trend and seasonality" which is a positive aspect of the model. Therefore, when reading the last sentence "For this reason, it cannot be expected that the RCM runs perfectly reproduce the precipitation patterns in the finer-scale seNorge data" one is a little surprised. Presumably the performance issues relate to the first sentence in this section? To make this a little more understandable, I would replace "For this reason" in your last sentence with the actual reason, presumably the somewhat different trend coefficient in the RCMs? It is also worth noting that RCMs are not hind-casts (unless you were using reanalysis forced RCMs), rather largely free running models (following the global response to observed emissions as simulated by the driving GCM), hence you could easily end up with somewhat different temporal trends, particularly on such short time frames.

Reply: Thank you for pointing out this contradictory messaging. We have rewritten the overall statement, see lines 422-431.

Response to Referee 2

General comments

This manuscript is very interesting for the topic of rainfall fields at hydrological scales.

My comments only regard suggestions of minor revisions, in order to slightly improve the quality of this interesting manuscript.

Reply: Thank you for your positive evaluation of our work. We have modified the manuscript following your suggestions. Please find details below.

Comment 1

In the introduction, Authors should enrich the state-of-the-art of stochastic models, by mentioning Neymann-Scott and Bartlett-Lewis families, also available for transient versions (Burton et al., 2008, 2010; Cowpertwait et al., 2002; De Luca et al., 2020)

References:

Burton, C.G. Kilsby, H.J. Fowler, P.S.P. Cowpertwait, P.E. O'Connell, RainSim: A spatial–temporal stochastic rainfall modelling system, Environmental Modelling & Software, Volume 23, Issue 12, 2008 https://www.sciencedirect.com/science/article/abs/pii/S1364815208000613

Burton, A., H. J. Fowler, C. G. Kilsby, and P. E. O'Connell (2010), A stochastic model for the spatial–temporal simulation of nonhomogeneous rainfall occurrence and amounts, Water Resour. Res., 46, W11501, doi:10.1029/2009WR008884

Cowpertwait, P. S. P., Kilsby, C. G., and O'Connell, P. E., A space-time Neyman-Scott model of rainfall: Empirical analysis of extremes, Water Resour. Res., 38(8), doi:10.1029/2001WR000709, 2002.

De Luca, D.L.; Petroselli, A.; Galasso, L. (2020). A Transient Stochastic Rainfall Generator for Climate Changes Analysis at Hydrological Scales in Central Italy. Atmosphere, 11(12), 1292. https://doi.org/10.3390/atmos11121292 (https://www.mdpi.com/2073-4433/11/12/1292)

Reply: Thank you for your suggestion. We have given a proper review of these state-of-the-art stochastic models in the introduction, see lines 35-39.

Comment 2

In Section 3, I suggest to insert a flow chart in order to make clearer for a reader the several steps of the proposed procedure.

Reply: We have added a conceptual graphic Figure 1 in the introduction, and using that as a guideline we introduce the main steps of the proposed procedure to our readers already in the introduction, see lines 61-69.

References

Yang, X., Magnusson, J., Huang, S., Beldring, S., and Xu, C.-Y.: Dependence of regionalization methods on the complexity of hydrological models in multiple climatic regions, Journal of Hydrology, 582, 124 357, URL `https://doi.org/10.1016/j.jhydrol.2019.124357`, 2020.