

Interactive comment on “Downscaling future precipitation extremes to urban hydrology scales using a spatio-temporal Neyman–Scott weather generator” by H. J. D. Sørup et al.

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Dear Hjalte et al.

I was in the assessment committee for Sørups PhD in December 2014, and have therefore seen a draft of the paper before. I can see that some of the issues that I pointed out at the defense have been well addressed.

The paper contains a calibration of a weather generator to local observations. The calibrated weather generator is forced with different climate scenarios in order to produce extreme rainfall in the future climate in Denmark.

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General comments The paper is well written in a clear understandable language and has an appropriate number of references.

You argue that you need fine temporal scale rainfall for urban hydrology, but is 1 hour fine enough? Please add references related to this issue and discuss.

It would be worth also to show IDF-curves for the climate projected data for different scenarios. This is an important result and would increase scientific dissemination of results to a broader audience than the authors themselves.

The paper only focuses on extremes. How would the WG produce more average rain. Yearly or monthly averages? This is also important in urban hydrology. eg. in order to investigate environmental impacts from CSO's, waste water treatment plants, etc.

In the paper a lot of advanced statistics is applied to describe different parameters related to rainfall statistics. However comparing the statistics of the WG and statistics of the observations is very subjectively. Phrases such as: “Generally the fit seems reasonable...”, “...is considered satisfactory...”, “...result in comparable ... values...” are used. This makes the comparison of the WG and observations quite vague. It would have been nice to see the same level of statistical detail also applied in the comparisons, e.g. by some goodness of fit measures instead of visual evaluation of plots.

One question remains reading the paper. Could the WG have been calibrated differently or better? It is stated that the fit is satisfactory, but compared to what?

Specific comments Figure 3: Please specify the levels (mm between) of the isohyets.

Page 2570 top and table 3. How are the weighting factors assessed? It seems quite subjective? (this also relates to the my last question of the general comments).

Page 2572 line 4. Please specify all durations used.

Figure 4. For all of the plots is an obvious variation over the year, but not for the two

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top plots. Please comment.

Page 2576 line 3 It is concluded that the “the fit seems reasonable for all variables”. Without any measure of this fit this statement is useless. I would say that the correlation between WG and SVK is quite poor.

Figure 5. Index letters are missing. Please add units

Page 2577 line : 10 and 100 (as written in fig 7) years or 10 and 50 (as written in the text). Please comment on the fact that you show a 100 year return period of a SVK dataset which is significantly shorter than 100 years.

Figure 7. Differences seem quite small but since this a double-log plot so difference especially of the small values are difficult to read. Could you give some sample in the text.

All in all a good paper, but I would like to see the issues on validating the WG addressed.

Best regards Søren Thorndahl, Aalborg University

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