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Interactive comment on "A classification algorithm for selective dynamical downscaling of precipitation extremes" *by* Edmund P. Meredith et al.

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The paper "A classification algorithm for selective dynamical downscaling of precipitation extremes" by Meredith et. al. presents an interesting strategy for a more efficient and targeted simulations of heavy precipitation with high-resolution convective-permitting regional climate models. They make use of the dependency of local rainfall on the large-scale (synoptic) conditions in terms of circulation patterns, and apply a cluster analysis to distinguish between days when the conditions are right for heavy rainfall and days when heavy rainfall is unlikely. Then they run a high-resolution regional climate model based on the first principles (physics-based) to simulate rainfall

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for the selected subset. This approach can in a sense be considered as a hybrid between traditional empirical-statistical downscaling and dynamical downscaling, since statistical techniques (clustering) were used to select times for simulations.

The analysis presented in Meredith et. al. are in my opinion scientifically sound and this paper merits publication, but there are a number of important caveats and there are a number of statements with which I think are wrong. I also think the paper needs to explain how the results of their strategy can be used and how they should not be used (I think there is a room for the misinterpretation of such results). A targeted selection of cases, which the clustering analysis implies, means that the results are no random selection of data that can be used in traditional projections. However, such results are useful for case studies, scenarios and in stress testing, and the strategy enables the establishment of a catalogue of weather events with more events than traditional simulations. These points could be made in the paper (in the Discussion).

I also found the paper a bit hard to read and digest, and the figure and table captions especially cryptic. The paper seems to be written for scholars who already are well-versed in the matter, but is less accessible for the wider community. Hence, the paper could benefit from rephrasing some sentences. I hope I have not misunderstood too much of the text.

Some of the caveats are connected with statistics and need at least some discussion. The observations consisted in gridded daily precipitation (REGNIE), but such products are associated with spatial inhomogeneity: because of small-scale features in precipitation, the amount recorded in neighbouring rain gauges are rarely as extreme as each other, which means that the gridded values which are a weighted sum of a number of rain gauge records tend to reduce the extreme values. Moreover, the individual gridded values tend to have a different statistical distribution to the individual underlying rain gauge data (which can be approximated as a gamma distribution). Furthermore, models with different resolution (grid box area) are expected to produce data with different statistical characteristics (area mean) which are not directly comparable to observa-

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tions (the closest is reanalyses). A related caveat is that a comparison between the area mean from different data sets with different resolutions implies comparing statistical samples of different size, which also are expected to differ merely because of the different sample sizes. To make this even more complicated, the models may generate grid boxes with greater inter-dependency than the observations and less real degrees of freedom. I think such caveats must at least be discussed in the paper, even if it is harder to find a good solution to avoid such shortcomings.

I found a number of statements both in the introduction and on page 19 with which I strongly disagree and think are misconceptions. One reason may be the narrow and biased review of the literature. First of all, statistical downscaling is a term that spans a wide range of techniques, and there have been some examples of poor exercise of statistical downscaling that have given it a bad name. Furthermore, the paper uses a false dichotomy between statistics and physics, which I find unfortunate - but this is also a common misconception.

While there are some types of statistical downscaling techniques which are just statistics (e.g. the analog model, neural nets), there are also statistical downscaling methods which are based on physical dependencies (e.g. regression-based techniques). I have emphasised the importance to use physics as a basis for statistical downscaling in a text book on statistical downscaling [1]. The passage 'the lack of a physical basis behind standard statistical downscaling techniques' is therefore a gross generalisation that is both misleading and incorrect.

While the sentence 'Widely used univariate approaches do not capture physical and spatial dependencies and thus physical and spatial coherence between different meteorological variables may not be maintained after downscaling (Maraun et al., 2010), leading to combinations which are suboptimal as boundary conditions for hydrological modelling' gives a false impression about the merit of statistical downscaling. It is important to stress that the statistical downscaling approach is tailored to a specific use to a much greater degree than dynamical downscaling, and if there has not been a need to preserve the physical and spatial dependencies, then univariate approaches are adequate. I think this part of the discussion suffers from a limited and biased literature review, as it is perfectly possible to use statistical downscaling for cases where spatial coherence between different meteorological variables is preserved [2]. Furthermore, the regional climate models also suffer from similar problems: (a) when they produce different precipitation patterns to the driving global models, the two levels of models are mutually physically inconsistent, and (b) when the the global and regional circulation models use different parameterisation schemes, they are physically inconsistent. In addition, the regional models tend to produce a smoother picture of the geographical patterns, partly due to the way the lower boundary is provided.

The notion of stationarity (p.2, L.15) is a problem for all models, and the passage 'in the absence of a physical foundation there is no intrinsic reason why a statistical downscaling method which performs well in the present climate should also perform well in a future climate' is a bit like shooting oneself in the foot (keeping in mind that the proposed strategy also makes use of large-scale predictors on par with statistical downsclaing) - in addition to being incorrect (statistical downscaling does not lack a physical foundation in general). All the general circulation models make use of parameterisation schemes (ironically called 'model physics') which essentially are ways to calculate bulk effect of various (unresolved) processes with the help of statistical models (the parameterisation schemes are upscaling rather than downscaling models). Whereas the degree of non-stationarity between scales can be examined in statistically downscaled results, it's much harder in dynamical downscaling and the global models where errors feed back into to model framework with a non-linear effect.

I also find the notion 'statistical downscaling method which performs impressively in one region or season may not work as well in other seasons or regions' somewhat misleading. There is no reason why one would use the same statistical downscaling approach everywhere, but it should instead be tailored to the specific problem. Furthermore, statistical downscaling models should be properly evaluated wherever and whenever they are applied (there have been poor studies where this has not been done properly). I can use my statistical downscaling framework over the whole world without problem, depending on the availability of good ground observations, but the models need to be tailored to the specific region. Moreover, statistical downscaling has an advantage over dynamical downscaling through low computational costs which makes it ideal for downscaling large multi-model ensembles of global climate model simulations [4]. The small ensemble size of independent dynamically downscaled results is major problem that is likely to produce misleading results according to the law of small numbers, even if the downscaling models themselves were perfect. It is therefore important to stress the need for both statistical and dynamical downscaling. The introduction of the paper and page 19 need a major revision with updated information. It is important to stop the spread of common misconceptions about both statistical and dynamical downscaling.

Minor details:

The concept of added-value is tricky and context-dependent (p.2, L. 20). At least, it needs to be defined, however, more details is not the same as added value. There have been criticism of regional climate models for the lack of added-value [3].

It's a bit of a stretch to use the term "extreme" (and 'PED') for the 99-percentile of rainfall applied to all days: that translates to 3-4 events per year. The label 'heavy rainfall' is more appropriate. (p. 5, L. 1)

Caption of Fig 1 is not easy to understand. Can it be improved?

I found line 30 on page 6 (p.6, L30) a bit cryptic and suggest rephrasing.

Please state the 'pan-European EURO-Cordex domain' (p.7, L-8). It will save the reader looking it up and it should not take much space in the text.

I think that 'internal solutions' is a more appropriate term than 'error growth' (p. 11, L.8) if I have understood the text correctly (the regional model can generate its own description of internal details which may differ from the GCM simulations used for boundary conditions?).

Table 2. Caption is not very helpful, and exactly what does ‘All Days’ mean?

What is ‘this’ referring to on p. 12 L.8 (‘... is far removed from this as . . .’).

Reference to Fig 5 & Fig 1 (p.12, L.13). The ECDF presented is for an area mean precipitation? Please state how many grid boxes/rain gauge stations this statistics comprises. The reason is that aggregated statistics such as sums and averages converge towards a normal distribution (‘∼N()’) with larger samples. If the obs and CCLM area estimates involve different degrees of freedom (sample size), then we should expect to see different types of curves. It would be easier to interpret these results if information of the number of grid-boxes were provided with some test results on the type of distribution (e.g. Kolmogorov-Smirnov against gamma & N()).

I suggest splitting the Summary and Conclusions into a Discussions section and a short conclusions section. This is useful for scholars who browse papers to see if it is of relevance and to make the take-home message clearer.

References:

[1] Benestad, Rasmus E., Inger Hanssen-Bauer, and Deliang Chen. 2008. Empirical-Statistical Downscaling. World Scientific. (free copy: http://rcg.gvc.gu.se/edu/esd.pdf)

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[4] Benestad, Rasmus, Kajsa Parding, Andreas Dobler, and Abdelkader Mezghani. 2017. “A Strategy to Effectively Make Use of Large Volumes of Climate Data for Climate Change Adaptation.” Climate Services. https://doi.org/10.1016/j.cliser.2017.06.013.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-660/hess-2017-660-RC2-supplement.pdf

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