Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-224-RC2, 2019

Geoscientific Model Development Discussions

Open Access EGU

Interactive comment on "Statistical downscaling with the downscaleR package: Contribution to the VALUE intercomparison experiment" *by* Joaquín Bedia et al.

Anonymous Referee #2

Received and published: 24 November 2019

The authors present the R package downscaleR. In principle this is a very useful contribution and worth publishing in GMD. But before publication I ask the authors to address the following major issues, plus a series of minor but still important ones.

Major issues

1. In section 4 the authors consider a pan-European setting, and explore whether models using a European predictor domain with additional local predictors perform equally well as the corresponding models with predictors defined on regional domains as used in VALUE. If these models would indeed perform well, this would mean a substantial simplification, e.g., for large-scale ESD applications such as in EURO-CORDEX. I am

Printer-friendly version

Discussion paper

afraid, however, that the reasoning is not quite stringent. The validation is based on ERA-Interim predictors, which should well represent local predictors given that local observations have been assimilated. In a GCM context, these local predictors may not fulfill the perfect prog condition, i.e., they may not be bias free. If this were the case, the GCM-based projections could be substantially biased, and the use of local predictors were not permitted. In fact, biases may also affect the climate change signal. I therefore ask the authors to test the PP assumption: first, they should use the historical simulations of their GCM-predictor experiment and check the perfect prog assumption. And second, they should investigate whether the climate change signals simulated by the local implementations differs from those of the VALUE implementations. If the PP assumption was not fulfilled, and/or if the climate change signal was modified, the authors should change their conclusions correspondingly. Even in a positive result, the authors should mention that care is required for the reasons given above.

2. I am wondering how downscaleR is placed relative to ESMValTool. This is a widely used tool mainly (but not exclusivel) in the GCM community, and it should be possible to combine analyses and results from the different tools. It would be disappointing if the two packages would not be compatible (beyond the exchange of NetCDF files), so a discussion is absolutely necessary, and compatibility very much desired.

3. The conclusions are quite weak. I would really appreciate if the authors could discuss what the purpose of the package is, and where it sits in the wide landscape of downscaling and evaluation tools in climate sciences, and what the specific advantages are. This has been touched in the introduction, but here it should be referred back, and some substantial statements should be made.

Minor issues:

In general, some minor grammatical errors (e.g. l 192 "analogs performance") need to be corrected.

l 5: VALUE is a network, not a project. You might also call it an initiative.

l 25: "are not suitable" This is not always true. Please replace by "are often not suitable"

l 32: "SD" here you could refer to a recent review or introductory text, e.g., Maraun & Widmann, CUP, 2018.

l 40: "It must be noted" is a zero phrase. Start with "SD techniques are..."

l 45: Here it would be fair to cite Barsugli et al., EOS, 2013.

l 55: Here it would be useful to cite the synthesis article, Maraun et al., IJC, 2019, highlighting that this article aims at giving an overall assessment of relative merits and limitations.

l 66: "It is worth mentioning here": Again, a zero phrase. You could rather state "This toolbox complements/adds to other existing tools..."

l 106: somewhere in the introduction you should mention ECMValTool

l 113: here you should really also refer to Maraun & Widmann, CUP, 2018. It is the most comprehensive discussion of the two approaches in a climate change context.

l 119: no - the term "perfect" refers to the assumption that the predictors are bias free. In particular in weather forecasting, also for MOS the day-to-day correspondence is given. For the MOS discussion you should make clear that the limitation of having homogeneous predictor-predictand relationships applies only in a climate context. This is also the reason why MOS in climate research is typically just bias correction. In weather forecasting, you are as free as in PP.

l 130: you may consider presenting the updated assumptions formulated by Maraun & Widmann, CUP, 2019. They are more precise and include the often neglected requirement that the model structure should be applicable.

l 169: you may consider to add a comment that often predictors are proxies for physical processes, which is a main reason for nonstationarities in the predictor/predictand relationship, as amply discussed in Maraun & Widmann, CUP, 2019. In this context, you should mention that predictor selection and the training of transfer functions are carried out on short term variability in present climate, whereas the aim is typically to simulate long term changes of short term variability (same reference, and Huth, J. Clim., 2004)

l 194: it should be pointed out that this is true only for analog methods, which use the same sequence of analogs for different locations. Otherwise spatial coherence is underestimated. This has been demonstrated by the cited Widmann et al., IJC, 2019.

l 196: this statement could be formulated much stronger. I am not aware of any region in the world, where climate change will be so moderate, that the analog method still applies in the far future, when temperature and directly related variables are considered.

l 205: somewhere you should mention that the main advantage of GLMs is to simulate (non-normal) variance not explained by the predictors (e.g., von Storch, J. Climate, 2000, although, strangely, not all models make use of that).

Fig 5: the violin plot needs some explanation. It is not quite clear what the distribution shows. Densities across stations? Is theres some kernel smoothing applied? Also: is this an annual analysis? The same holds for the following figures as well.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-224, 2019.