Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2021-176-RC1, 2021
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Comment on gmd-2021-176

Anonymous Referee #1

Referee comment on "A Norwegian Approach to Downscaling" by Rasmus E. Benestad, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-176-RC1, 2021

Summary: The manuscript is a review of the statistical downscaling strategies followed by the author over the past decades, highlighting the differences to other mainstream approaches . This review also includes some related questions, such as strategies to archive data from ensemble of simulations that can be retrieved more efficiently.

Recommendation: I am rather sceptical about the added value of the manuscript. It is clearly a review - it does not contain original model or methodological descriptions - but being a review it focuses almost exclusively on the work done by the author. Even then, many of the descriptions/conclusions refer to a list of previous papers without offering deeper insight. Also, the reasoning concerning a few technical aspects is dubious. All in all, the manuscript to me rather reads like a project report or a report of a laboratory for an external evaluation - seasoned with a generous portion of self-praise- than a proper scientific paper . I provide more detailed justifications for this opinion in the following

1) Title: The title actually provides a good example of my concerns on the paper. It is clearly not informative about the contents - what should a reader interpret from 'Norwegian approach' ? I do not think that is a descriptive scientific term to inform the reader.

2) The abstract is also uninformative: on what is the downscaling method based? is it statistical or dynamical ? to what extent is it different or better ? is is applied to mean climate or to extremes ? The reader decides after reading the abstract whether the manuscript is useful for them, but this abstract does not provide any information towards

that decision.

3) ' Long experience with both downscaling, and working with impacts and society, has shaped the typical Norwegian strategy for downscaling.'

Is that different from other approaches ? Other groups have also long experience and deal with impacts and society. .

4) 'The description of this experience serves as a stock-taking on where the science stands on downscaling in Norway,'

It seems that the work by the author is representative for whole Norway. I do not know if that is the case but is this relevant in a scientific paper ?

5) 'The latter take on downscaling also highlights the difference between downscaling and bias-adjustment,'

This whole paragraph on bias-adjustment is misplaced. Why focus precisely out of the blue on bias-adjustment ? It is debatable whether bias-adjustment is a type of downscaling.

6) The paragraph on common EOF-s is, in my opinion, very weird:

' In spite of the success with utilising common EOFs, a Google scholar search on ‘"common EOFs" downscaling’ only had 63 hits (of which about 40 referred to our own work), despite more than 20 years since they first were introduced in ESD and the widespread need for climate change adaptation and downscaled results. This surprising results suggests that they are not appreciated, and this is supported by the fact that common EOFs are not mentioned in text books such as Maraun and Widmann (2018).'

In my interpretation, the author assumes that common EOFs is a superior technique. It

was presented by the author already in 2001, and used by the author and collaborators in following papers. However, the authors seems surprised that the scientific community as a whole has not applied his approach more often. I guess that a valid hypothesis to explain this discrepancy is that the scientific community does not share the author's opinion about the superiority of common EOFs.

7) ' It turns out that statistical properties are surprisingly predictable, are oftenstrongly connected to single predictor variables, and that predicting statistical properties tends to be more robust than predicting individual outcomes (Benestad and Mezghani, 2015;'

what does 'individual outcomes' mean in this context? climate downscaling is never about the prediction of individual events, but actually changes in the statistical properties, i.e. climate changes.

8) 'we tend to use multiple regression because parameters aggregated over seasonal scales tend to approximately follow the normal distribution according to the central limit theorem'

This is not true in general. The estimator of some statistical parameters are normally distributed, but not for others-. For instance, the estimator of a variance cannot be normally distributed, and in a bayesian setting the prior for variance are other types of non-normal distributions.

9) 'The median can only be an integer number (or halfway between two integer numbers), and is not

a pure rational number such as the mean spell length that is needed to represent the parameters of a pdf#

This sentence refers to the distribution of spell-lengths. As such, any distribution has to be discrete, and therefore the median has to be an integer or the mean of two integers. What is wrong with that ? The mean is a different parameter, which may be more useful to write down an analytical expression for the pdf, but the median has other advantages, especially for the end-user. I do not really see the point of this paragraph

In addition, the mean of a discrete distribution can be a real number (also irrational), it does not need to be a 'pure rational' number' , whatever this means mathematically.

10) 'The statistics of maxima is often cluttered and involves a high degree of uncertainty, is not a

rational number,'

I do not understand this paragraph. The mean of the spell-length maximum in a sample of size L can be a real number as well . The mean is also a statistics. The author likely means, not the statistics, but one realization, and confounds both terms.

11) 'Strategy for storing large volumes of multi-model ensemble ESD output-..'

I fail to see the direct link of this section to the other sections. In principle, the question of efficient archiving of data from ensemble of simulations is independent of 'downscaling', Moreover, this section only provides superficial information about how efficient this system is supposed to be, without explaining how. I think they reader will not gain very much from this section.

12) Hence, their results do strictly not represent the same aspects as those observed. RCMs nevertheless have great value in the context of experiments'

The same could be said of GCMs. GCMs are however considered to be the central tool for climate projections

13) 'Even the traditional Euro-CORDEX RCM ensemble is a case of not using the right information in correct way, as all RCMs in the ensemble may have systematic biases with the same sign because of common physical inconsistencies in terms of OLR and common shortcomings in coupling with surface/ocean/lakes or treatment of aerosols'

This is a very sweeping and potentially very assertion, As in the previous point, the same can be said of CMIP5 and CMIP6. Each model have systematic biases, which possibly can be of the same sign, and common physical inconsistencies. Does this mean that the CMIP5 and CMIP6 ensemble is a 'case of not using the right information in a correct way`?.

14) 'These results are robust because they are derived from large multi-model ensembles of GCM simulations '

which is the conceptual difference between ensemble of simulations with GCMs and ensemble of simulations with RCMs ? I am really confused that in previous paragraphs, ensemble of RCMs are criticized because of model biases and inconsistencies, and ensemble of GCMs are considered free of those problems.