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Comment on gmd-2021-176

Anonymous Referee #2

Referee comment on "A Norwegian Approach to Downscaling" by Rasmus E. Benestad, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-176-RC2, 2021

Review of “A Norwegian Approach to Downscaling”, by R. E. Benestad

This paper summarizes a list of procedures for statistical downscaling of climate variables and advocates their use in the scientific community.

General remarks

This paper is filled with rhetoric and subjective statements, and, surprisingly, does not contain any new scientific advancement (which are left to already published papers). What is called a “Norwegian approach” seems to be exclusively developed and used by the author (as shown by the list of publications). As the author seems to criticize “*mainstream approaches*” (quotation marks and italics), this implies that the whole scientific community in Norway is tied to the work of one author, which I find rather contradictory and worrisome, with all due respect to my colleagues there. So, **why** this paper? The one-paragraph introduction does not provide any answer to this elementary question.

The paper tries to explain the divergence of the author’s work to what he calls “mainstream”. But what is mainstream? From what I understand, “mainstream” looks like every paper without Benestad as a co-author, which might indeed sum up to many papers. This is not very informative. The author suggests that most (if not all) the climate community performs downscaling with regional climate models (RCMs) and is focused on CORDEX experiments. This sounds like a biased statement. CORDEX is a tool among others to provide normalized regional climate simulations. Many countries have their own regional climate simulations with spatial and temporal resolutions that far exceed those of CORDEX.

The fact that the author does not find selected keywords (e.g. "common EOF") does not prove that there is no reference that use approaches that are similar to his. It suffices that other authors use a different terminology. Hence, the reasoning behind l. 80 is flawed.

The author never gives a precise definition (with equations) of "downscaling". This implies that most of the statements in Secs. 2.1 – 2.6 are at most qualitative and subjective. If one writes down the mathematical challenge of downscaling, then climate research on this subject would be about outlining options to solve this challenge. I don't see why there should be a "best" way, as the expectations of the outcome of downscaling might differ on the communities. If one acknowledges this elementary fact, then the rationale for this paper seems rather weak.

I am surprised that the author decides NOT to discuss the precise details of his approach in the paper (for GMD), which hence requires reading many other papers (some of which are not in open access). It is even unclear whether the reported results appear elsewhere. So, what is the purpose of peer review?

What is called the "Norwegian approach" seems to depend heavily on assumptions on the probability distribution of climate variables. The author then does "something" (that is not precisely described in the paper) to rectify the parameters of probability distributions from a large scale to a small scale. Fine. No explanation is provided on the time dependence of those parameters (neither short term or long term), although this should be the crux of a scientific paper. I do not see a discussion on inter-variable dependence, which is also a major challenge of climate downscaling. Spatial dependence is not included in the procedure (it is just checked visually a posteriori).

The author does not try to compare his approach with other methods (dynamical or statistical), which is disappointing, as it would have given a concrete example of the advantage of using 'esd'. One can also wonder whether the Scandinavian example was chosen because the author is satisfied by the results, although his satisfaction of Figure 5 is subjective: the fact that qq-plots deviate from the 1-1 line might not be surprising but it contradicts Sec. 2.6 on extremes.

Specific remarks

The introduction looks like a biased statement of “state-of-the-art”. The goal of the paper is not stated. The author lists keywords (dynamical downscaling, empirical-statistical downscaling…) as if they were the only and exclusive pillars of downscaling.

l. 12-14 (“The climatic conditions to which […] hazards”). This sentence is very strange. There is ample literature that show how some human societies have been affected (even collapsed) due to changes in climate features since pre-history. I do not see what kind of “new weather-related hazards” the author refers to. I do not think that the SREX (2012) mentions them.

The author mentions several types of downscaling approaches. Is this supposed to be exhaustive? There seems to be a continuum of approaches, that mix several types of data and paradigms. The “Norwegian approach” is one among many others.

Section 2.1: as long as what “mainstream” is NOT defined, sections 2.1-2.4 are rather obscure.

Section 2.2, l. 47: what does linear algebra here? Is it because you are working on vectors and matrices of numbers? Anybody who deals with vectors or matrices of numbers does linear algebra. This is not very informative.

l. 50-55: Playing on words is not very helpful. Please state the mathematical formulas that are involved and how your approach differs. There are too many “ors” to be really rigorous.

I doubt that CORDEX can be considered as “mainstream” for downscaling.

l. 59: “Bias adjustment doesn’t involve the dependency between spatial scales […]”. Bias adjustment is a separate issue from downscaling. And there are bias adjustment methods that consider the dependency between spatial scales. The statement that “some will say that it gives the right answer for the wrong reason” sounds like slandering and dismisses the vast literature on bias correction and verification procedures.

Section 2.4, l. 115-125: I understand that a choice is made to model temperature by a Gaussian distribution and precipitation by an exponential distribution. No information is given on the temporal dependence. Are the variables IID? If one is interested in temperature extremes, there is ample literature that shows that it is not Gaussian. The way the parameters of the statistical laws are “downscaled” is nowhere explained or discussed in the manuscript. The strategy of “keeping things simple and elegant (mathematical)” is barely reflected in the manuscript as no precise equation is given, especially for the rules of inferences. Wanting to keep things simple and elegant often leads to “spherical cows in vacuum”. Having a list of nine rather heterogeneous criteria of evaluation (Section 2.7) is neither simple nor elegant.

Section 2.6: If you model temperature with a Gaussian distribution and precipitation with an exponential distribution, it is very unlikely that extremes of temperature and precipitation are correctly inferred. Figure 5 is an elementary counter example of the title of this section.

Section 2.7, l. 192: what are GCM grids? How do “common EOFs” deal with biases in mean values between observations and GCMs?

Section 2.8: What is the point of this section? There is no description of the method to store files, just an unverifiable statement that it is better than netcdf. Figure 6 does not demonstrate anything on the storage method.

Section 3 (conclusion): The three-paragraph conclusion leaves me still confused. What makes the author believe of “silo thinking”.