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Interactive comment on "Dynamical Downscaling Data for Studying Climatic Impacts on Hydrology, Permafrost, and Ecosystems in Arctic Alaska" *by* Lei Cai et al.

Anonymous Referee #2

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General comments The paper has important and valuable objectives that is, to provide a suitable dataset for the climate change impact communities in the Arctic Alaska region. Unfortunately, because of significant shortcomings in the methodology, it fails to provide a useful methodology and dataset.

One of the major issue is related to the choice of only one scenario. Taken alone, even with the best methodology, it makes the resulting dataset useless for climate studies. If this was the only issue, one could still use the methodology to generate needed multiple needed scenarios to properly evaluate uncertainties, but the remaining steps in the methodology also has its own problems that will be detailed in the specific scientific issues/comments.

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Discussion paper

For those reasons as well as the ones described in details in the following specific comments, I greatly encourage the authors to work on those issues and to re-submit the manuscript as a new one.

Specific comments

Introduction:

P3L10: Why RCMs are needed in Arctic in particular? References needed here, as RCMs are mandatory for any regional scale climate change studies

P3L15: Same comment as previous: a more detailed explanation is needed here, which RCM products are needed for Alaska and not elsewhere? Alaska Northern Slopes are not the only region where high-resolution spatial and temporal are needed for forcing hydrological models.

P3L25: Why linear scaling? The authors need to prove why one should use this specific bias correction method.

Data sources: P4L20-26: There exists other reanalysis than ERA-Interim. The choice of the reanalysis is crucial, and the authors should also explain why they did not use MERRA or other reanalyses, as the 2012 paper cited is already missing the new re-analyses available today.

P5L8-9: Only one ensemble member? Why #6? One realization of the model is not enough to take properly into account uncertainties in climate model simulations.

P5L10: The same as the previous comment for using only one RCP. The argument that the global temperature warming of the first decade is "braking" is not accountable by itself. Especially if one is interested in the second part of the 21st century, it is mandatory to use more than one RCP scenario.

P5L24-25: The author need to argument why those five stations are qualified and not the other ones.

P6L7: Why a 10-km grid spacing? What are the reasons of this choice? How does it impact the parameterizations?

P6L10: The authors need to argument why they chose a spin-up time of 6 months.

P6L23: Could the authors precise what "reasonably well" means in the context of the current study?

P7L7-11: Why did you choose this method for comparison? There is a need of references there, as the method to compare gridpoint data from the model and reanalysis, which represents a large surface, to station point data is crucial in the analysis being done here.

P8: Tmax and Tmin comparison methodologies can lead to large errors, as the maximum and minimum does not always occur at 3pm and 3am local time respectively. We can think of large temperature inversions during winter time, also the impacts of low cloud cover during the day and high cloud cover during the night, especially. The problem is that from the discussion it is impossible to know what are the impacts of this method of comparison on the results discussed in the followed section (statistical coherence).

Section 4.1.2: The statistical differences are stated, but no explanation is provided.

P10L15-16: This is a strong statement that needs to be proven, as it impacts the whole rest of the methodology. It is not obvious that a global reanalyses is the best dataset to use to represent precipitation at high resolution over a complex terrain.

Section 4.2: One cannot choose a bias correction methodology without doing a proper analysis of several methods. I suggest the author to, notably, take into account the paper Maraun (2013) in Journal of Climate: Bias Correction, Quantile Mapping, and Downscaling: Revisiting the Inflation Issue. What about extremes? No word is given about this aspect.

P10L23: How the author account for the non-gaussian PDF of precipitation? What about zero values?

P11L10: Variables are bias corrected individually, thus breaking the coherence between variables. How is that taken into account in the current study?

P12L1: Basing the whole scaling bias correction parameter from only one station is not correct. This impact the whole methodology and the remaining of section 4.3.

P12L5-25: Strong biases are corrected by the method. It is not obvious that those bias correction parameters do not vary among seasons, weather circulations/types: with those large corrections more biases can be introduced if the differences between the PDFs are not the same.

P12L23: Those variation are significant, proving that the bias correction parameters should not be the same for all seasons.

Discussion and Application sections: I will not repeat the arguments stated above about the numerous significant problems in the methodology: they apply here too making the discussion to be re-assessed once the methodology has been modified appropriately.

Technical corrections No specific detailed technical corrections have been done, in light of the major problems in the methodology. A general comment about the writing is that the authors should stick to a passive OR active phrasing. Also, the Application Section is too long and should rather be included in a small discussion at the end of the paper.

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