

Geosci. Model Dev. Discuss., author comment AC2
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Reply on RC2

Joel Fiddes et al.

Author comment on "TopoCLIM: rapid topography-based downscaling of regional climate model output in complex terrain v1.1" by Joel Fiddes et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-60-AC2>, 2021

REVIEWER #2

We thank the reviewer for his thoughtful comments which have undoubtedly helped to improve this work. We also apologize for the somewhat delayed response. We provide here a point-by-point response where reviewer comments are in bold type and author responses in non-bold. Changed or inserted text is given in italics.

As a general comment, this manuscript lacks a clear explanation of to what extent the method used is novel. The interest and aims of the method are clearly described, but its novelty and added-value (compared to Fiffes and Gruber, 2012, 2014, but also to previous similar downscaling and bias-adjustment methods) is not. A corollary comment is the fact that the method and results would benefit from being put into a larger context. For instance, results in section 5.3 should be compared to other studies of the impact of climate change on Alpine snow cover (which would also better fit with the current title of section 5.3 about "Alpine" snow cover, while currently only Swiss snow cover is discussed).

Useful references include (but are not limited to):

- - Steger et al. (2013): <https://doi.org/10.1007/s00382-012-1545-3>
- - Marty et al. (2017): <https://doi.org/10.5194/tc-11-517-2017>
- - Frei et al. (2018): <https://doi.org/10.5194/tc-12-1-2018>
- - Verfaillie et al. (2018): <https://doi.org/10.5194/tc-12-1249-2018>

The main novelty is that a modular system to generate high-resolution (slope scale) forcing data for impact studies is developed, documented and provided. Even though the individual components (TopoSUB, TopoSCALE, and quantile mapping) are perhaps well established, establishing the coupled workflow is novel and generates important model forcings that are currently, to our knowledge, not available to impact modellers. In particular, it allows us to get climate impact projections for the cryosphere (in this case snow) at the hillslope scale by feeding the bias corrected future atmospheric forcing through cryospheric models (in this case FSM). Perhaps most importantly, especially for applications in data scarce regions, this scheme does not require in situ data. The hillslope scale (order 100 m resolution) is often not addressed in previous climate change studies despite its importance in regulating the stores and fluxes of water, energy, and carbon (e.g.: Fan et al., 2019, <https://doi.org/10.1029/2018WR023903>), precisely because of the

lack of forcing availability at this scale.

In particular we have made the following edits in the introduction to make these points clearer:

This is especially the case in heterogeneous terrain such as mountain regions where topographic variability is high over short horizontal distances. High surface variability requires modelling at the hill slope scale (c.100 m) in order to adequately capture fluxes and stores of energy, water and carbon (Fan et al. 2019). Various methods of downscaling can be utilised to achieve this goal.

In this study we address the problem of impact-model ready (i.e. hillslope scale) climate timeseries with a new modelling framework called "TopoCLIM".

Importantly, using these pseudo-observations we are able to debias climate timeseries in regions lacking ground observations.

Based on this and Reviewer 3 comments we have adjusted Section 5.3 title to a more appropriate: *Climate change impacts on Alpine snow cover across Switzerland*

In Section 5.3 we add discussion on previous studies while making the point that they are not comparable to our results in a straightforward manner due to resolution (25km v 100 m), parent climate models (ENSEMBLES v CORDEX) and/or scenarios (ie SRES v RCPs) used. However, these previous studies do nicely highlight the gap we try to fill with this work:

Several previous studies have investigated the impacts of climate change upon Alpine snow cover (e.g. Steger et al. 2012, Marty et al. 2017, Frei et al. 2018, Verfaillie et al. 2018, Bender et al. 2020), however direct comparison is often problematic due to model resolution, analysis period, parent climate models and/or emissions scenarios used. This highlights the importance of model intercomparison studies whereby these important variables controlling model results can be standardised. Comparison to these previous works highlights the contribution of this study in that most were conducted at RCM resolution of 25 - 12 km (Steger et al. 2012, Frei et al. 2018) or local scale (Verfaillie et al. 2018, Bender et al. 2020) or reliant on in situ data (Marty et al. 2017, Bender et al. 2020). In this study, we demonstrate a method that generates results over large modelling domains at hillslope scale (100 m), a scale which is extremely important in regulating the stores and fluxes of water, energy, and carbon (Fan et al., 2019), and therefore critical to modelling snow cover in mountainous terrain. Additionally, this approach does not rely on in situ data and therefore is appropriate for data-scarce regions.

Specific comments

Several sentences lack commas, which would facilitate the reading, e.g. line 37 before "e.g.", line 43 before "which", line 109 before "e.g.", line 127 before "it does not", line 133 before "e.g.", line 152 before "i.e."

Corrected these and other occurrences.

Line 52: repetition of "the".

Removed.

Lines 47-58: to me, a key reference for quantile mapping that should be cited here somewhere is the pioneering work of Déqué et al.

(2007):<https://doi.org/10.1016/j.gloplacha.2006.11.030>

Thank you for this reference which we now have included in this section.

Line 92: the "required forcing variables" are not listed here. In fact, they are listed in Table 1, but Table 1 is never called in the text...

We added the reference to the section describing the climate data, L.178-79 now reads:

"A full description of CORDEX variables is given in Table 1 and model chains used in Table 2."

Line 98: this sentence seems incomplete. Please edit.

Edited to:

For example, during the conversion from a "360-day" to a "standard" calendar, the output from the linear scaling will result in a 365 day timeseries (in the case of non-leap year) and be missing the following dates: January 31st, March 31st, June 1st, July 31st, September 30th and November 30th.

Line 104: "NWP" should be defined here.

Defined now as Numerical Weather Prediction (NWP).

Line 116: "IMIS" should be defined here instead of line 203-204.

Defined now at l. 116 as suggested. Edited both lines for consistency.

Section 2.4: Am I correct in thinking that the quantile mapping method you employ is univariate (i.e. the different variables are corrected independently from each other)? If so, this should be stated here and its implications discussed in section 5. Indeed, if temperature and precipitation are corrected independently from each other, this could have potentially large impacts on snow cover estimates (important in mountainous areas and presented in section 5.3). Could you please explain if you found a way to circumvent this issue (in section 2.4) and the related uncertainties (in section 5)?

Yes this is correct, but variables are corrected towards a physically consistent dataset in the form of downscaled ERA5 data, so we argue that while the method is univariate it will not produce physically inconsistent results. The validation during the current climate also supports this claim (Table 3-5). This is now stated explicitly in 2.4 as:

"It should be noted that while the variables are bias corrected independently, they are corrected towards a physically consistent dataset in the form of downscaled ERA5 data, so we argue that while the method is univariate it does not produce physically inconsistent results. The validation during the current climate also supports this claim (Table 3-5)."

Line 171 and elsewhere in the text: a distance value is always separated from its unit by a space, e.g. "44 km", "20 cm" (line 249), "100 m" (line 258), etc.

Corrected all occurrences.

Line 174: "the number of file downloads is large".

Corrected the grammar.

Section 5.1, Figure 3 and Tables 3-4: please use a consistent naming convention for your experiments. I found conflicting occurrences of "QM", "QM_QM" and "QMAP", "QM_MONTH" and "QMAP_MONTH", and "T-MET" and "T-CLIM". At line 218, you could include the acronym "CLIM": "CORDEX ensemble mean (CLIM)".

Edited for consistency.

Line 225: there is a typo ("residual error").

Corrected.

Line 227: Please remove the capital letter in "Percentage".

Corrected.

Line 244 and caption of Figure 8: "time period".

Corrected.

Line 245: "The shortest station record is 10 years, therefore the dataset nominally represents the period 1996-2018": I don't understand this statement...

Edited for clarity as:

"IMIS station measurements are given as reference, however it should be noted that the time-period covered by each station is variable within the period 1996-2018. The shortest station record is 10 years."

Line 275: I would split this sentence into two parts after "HIST".

Sentence split as suggested.

Line 279: please remove the capital letter in "Century".

Corrected.

Line 308: I would also split this sentence after closing the parenthesis.

Corrected.

Line 334: "historical reanalyses" (plural).

The sentence has been changed with respect to reviewer 1 comments and the plural form is no longer used.

Caption of Figure 4: "The coloured envelopes indicate".

Corrected.

Caption of Figure 5: what does "WFJ2" stand for?

We have mistakenly used the ID for the Weissfluhjoch station here - but it is confusing in this context. We have made this consistent by calling it Weissfluhjoch station as elsewhere.

Figure 6: please change the colour palette for this figure as it is not colour-blind friendly. Currently, the colours for HIST and RCP8.5 cannot be distinguished from each other by a colour-blind person. Why not use the same colours as in Figure 5?

As suggested we have adjusted the palette based on Fig 5.

Figure 7: what do "Hist_1981_2010.1" and "Hist_1981_2010.2" stand for? Isn't there only one historical scenario used in this study? If so, why are the top left and top right panels slightly different? I don't think this is explained anywhere in the text or the figure caption.

They are indeed identical datasets, however, due to the png output resolution, the output device (x11) appeared to do some sub-pixel interpolation that gave the slight appearance of slightly different datasets. The .1 and .2 was an artefact of plotting the same dataset twice in R package RasterVis (levelplot function) - the default naming scheme appends a .1 and .2 in this case. We have replotted at a higher resolution which has removed this artefact and we have corrected the panel titles.