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The Cryosphere
Discussions

Interactive comment on "Modelling the future evolution of glaciers in the European Alps under the EURO-CORDEX RCM ensemble" *by* Harry Zekollari et al.

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Zekollari and colleagues present projections for glaciers in the European Alps using a glacier model that explicitly resolves ice dynamics and is forced by GCM projections dynamically downscaled in the framework of CORDEX. The manuscript presents a timely step forward for modeling of glaciers on large regional scales. It is written well and succeeds to make the assumptions and limitations of the model accessible. I am particularly impressed with high quality of the presentation of the results in the Figures. Overall, I have no doubt that this is a valuable contribution to the literature and eventually should be published. However, there are a number of minor points that

should be addressed by the authors, as well as three somewhat major issues:

Major comments:

Use of CORDEX data: This is the biggest issue I have: the authors use the projections from CORDEX, present a relatively thourough validation (some comments on this below), but do not at all address a question that seems very obvious to me: from the perspective of modeling glaciers, is there added value in CORDEX? I.e., what difference does it make applying the downscaled data opposed to applying the GCM data directly? I'm aware that because of internal variability, there is not much use in using the CORDEX (or GCM) data during the 2003-2017 validation period in the same way the E-OBS data are used. But there is still some insight to be gained concerning, e.g., temporally accumulated area and volume loss. Also: through comibination with differen RCMs, the ensemble size of CORDEX is increased - however, there are GCMs not part of CORDEX. Taking the ensemble as a measure of uncertainty, how does the addition of the RCM "axis" affect the ensemble uncertainty? Is is equivalent to taking a larger GCM ensemble, or can insights be gained through the downscaling? Of course, this point is not a weakness of the manuscript. But I see the approach taken by the author as great opportunity to address some questions that are very relevant, since dynamical downscaling is very expensive, and its value is somewhat debated. I think it would be great if the authors took this opportunity.

Initialization: Why is glacier length chosen instead of glacier area? It seems to me that area is better constrained than glacier lenght, since the length is to some degree an arbitrary choice resulting from the representation of the flow line. Also, using the 1961-1990 climatology, was variability preserved? Where the forcing timeseries during that period detrended? As the authors say, there is no assumption about an equilibrium state of the glacier, but an assumption that the glacier woud be able to undergo the transition from an equilibrium state in 1990 to the obsserved transient state in (typically) 2003). Doesn't this correspond to an assumption that the response time of the glaciers is shorter than 13 years? You discuss this in Sect. 6.3, but it would be good to have some arguments presented already in Sect. 3.3.

Validation: The geodetic mass balances used for calibration are most probably not independent of the in situ measurements used for validation. For those glaciers that have geodetic observations that do not temporally overlap with the in situ measurements, it would be good to recalibrate using these, and revalidated using the temporally independent in-situ values. I'm also wondering about the choice of using geodetic observations for calibration, and in-situ for validation. The opposite woud seem like the more natural choice to me, since geodetic MBs can cover longer time periods, and thus allow to include effects from ice dynamics in the total uncertainty. Is there a specific reason not to make the calibration based on in-situ observations, and validate using geodetic MBs?

Minor comments

Main Text:

P1 L10: ... ice flow processes, _of_ which the latter ...

P1 L10: you could specify it is not included explicitly, since the (simple) parameterizations used are supposed to parameterize its effect.

P2 L21: specify "glacier model parameters".

P2 L17: either delete "using various methods", or briefly specify.

P2 L18 (and P15 L27): volume/length/area or volume-lenght-area scaling

P2 L21: I suggest RCP 8.5 should not be called "extreme", since the different scenarios are not based on probabilities, nor is there a physical reason to view RCP8.5 above some limit value.

P3 L2-4: Not sure why glaciers in the Canadian Rockies should not be controlled by topography and local effects. Either specify, or delete; I think there is value in different approaches to similar problems by itself, so I don't see a need for a very

strong justification of your study here.

P3 L9: you might also refer to Goosse et al. (2018), which has transient simulations (DOI: 10.5194/cp-14-1119-2018)

P4 L7: delete "in these little-known connections" (or specify why they are lesser known than the rest of the glacier).

P4 L12: I would prefer "Climate data" or "Atmospheric boundary conditions"- but that is maybe a matter of taste.

P4 L19: "... has a higher resolution than the reanalysis data used in Huss & Hock (2015, ERA-INTERIM) and goes back further in time."

P4 L21: rephrase around "several chains used for", since it is not quite clear here what "chains" you are referring to.

P4 L28 (and throughout the study): perhaps it would be better to call them "model combinations" than "chains" (these "chains" have only two links anyway...).

P4 L30: "... a peak-and-decline scenario ..."

P4 L32: "Note that while country-specific ..."

P4 L32: "... such as the projections recently released for the CH2018 report, ..." (or something similar - i.e., distinguish between scenarios and projections).

P5 L13: because of the difference in resolution between E-OBS and the RCMs, there is probably not a direct equivalent in the grind points. Please specify how you handle this.

P6 L13: "closest to the glacier" - there are probably glaciers that are covered by multiple grid cells - are then multiple grid cells taken into account, or are you using the glacier center point? Please specify.

P6 L15 (and L19): "temperature-index melt model"

P6 L26-30: While precipitation is the least constrained boundary condition, the degree-day factors are probably the most potent paramteters for tuning. Why is the order chosen like this, and how is the defaul degree-day factor chosen? Please specify.

P8 L25: It is fine to seperate the validation of the SMB model from the ice dynamics, but there should not be much difference if the dynamics are behaving well. Also, the SMB observations are probably not taking into account the RGI outlines, such that there is some disagreement anyway. Have you tested how much the results differ if you switch on ice dynamics?

P8 L29-30: I don't agree: this correlations measures the skill of the model to reproduce more positive mass balances at the upper part of the glacier than at the lower parts. A better measure for the SMB model's ability to represent the MB profile would be a comparison of the (temporally averaged) MB profiles, or correlating the deviations from the "climatological" (i.e., mean) profile.

P10 L20: delete "1x".

P10 L27: delete "In the literature".

P10 L30: "over the period 1973-1998/9".

P11 L19: "evolution is independent of the scenario".

P12 L6: I don't understand this sentence, please rephrase. Do you mean volume change?

P12 L11: "strong below 3200 m".

P12 L18: delete "may" (you already say it's only under certain model combinations).

P12 L21: "This is illustrated for Langtaler ..." Sect. 6.1.1: Just out of curiosity, it would be interesting to know about the committed mass loss past 2100.

P14 L6: correct Section number.

Appendix:

P19 L16: delete "1" at the end of the line.

Tables:

Table 1: The "committed loss" line is a bit confusing, since the committed loss should not be time-dependent. Perhaps you can call this, e.g., the "realized fraction of committed loss"?

Figures:

Generally - but particularly Fig. 9: I'm not a big fan of grids in figures. If there is a need to read numbers/differences from the figures, these numbers should be mentioned on the figure or in the text, and no grid would be necessary; if there is no such need, the grid only adds clutter. Please consider removing grids.

P 27 L3: "relative to 1961-1990" (instead of "with respect to").

P27 L4: is that the mean of all "alpine" EUROCORDEX grid cells, or just the ones that contain glaciers? If the latter, I think it would make more sense to also weight them by glacier area.

Fig. 2: I have a hard time seeing the transparent bands; I suggest deleting them, since that information is already shown in the distribution of the individual (thin) lines.

Figs. 5, 6: are there uncertainty estimates available for the observed velocities? If so, it would be good to include them (e.g., just a single error bar so that the observation uncertainty can be compared to the differences with to modeled velocities).

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