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Discussions

Interactive comment on "Projected cryospheric and hydrological impacts of 21st century climate change in the Ötztal Alps (Austria) simulated using a physically based approach" *by* Florian Hanzer et al.

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PREMISE

The way I got to review this manuscript tells me one more time that the current peer-review system is close to collapse: I declined the invitation two times, and had no longer the courage of declining a third time, since I assumed that - at that stage - the desperation of the handling editor must have been larger than mine. Too many papers for too few reviewers seems to be at the heart of the problem. Here is a suggestion

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Discussion paper

for a s simple fix: No decision shall be taken on any manuscript before the submitting group of authors has themselves provided three reviews (the typical number of reviews for a manuscript) to some other articles. In the ideal world, these three reviews could be provided to any journal with decent quality, but since different publishers seem not to have any interest in collaborating between each other, the rule will probably be implementable only at the level of individual publishers. I may have overlooked some pitfalls that would go with such a suggestion, still, I'm convinced that it is worth a thought.

SUMMARY

Hanzer and colleagues present an analysis of future runoff evolution in a high- mountain catchment in the Ötztal Alps, Austria. Their analyses are based on the latest climate projections, i.e. in the results provided in the frame of the EURO-CORDEX initiative, and the physically-based model AMUNDSEN. The used methodology is sound, the paper is well written, the figure are illustrative, and the overall quality certainly adheres to HESS's standards. I have only series of minor remarks that the authors may or may not find useful to improve their work. Other than that, I look forward to see this contribution published soon.

First of all, we wish to express that we share the reviewer's concerns regarding the current state of the peer review system. While it will likely not be an easy task to fix this problem, we wish to thank you for your valuable thoughts that we will take over as good ideas to be thoroughly considered in the community. Most importantly, we are thankful that you nevertheless took the time to review our manuscript and for the many helpful and constructive comments. Please find below our replies to the individual comments.

P1 L7ff: The abstract is well written. I was wondering, however, if the authors would be able to add half-a-sentence or so to better highlight the novelty in their contribution.

We will add the following sentence to the abstract to highlight the novelties in our work: "The high level of process representation within the model, the high spatial and temporal model resolution, and the large number and range of considered climate model runs make these findings a novel contribution to the possible impacts of future climate change in the Ötztal Alps in particular and in high-elevation Alpine catchments in general."

P3 L12ff: The wording seem to suggest that the methodology used by the authors is "better" that what has been used so far. Asked provocatively: How can the authors proof that? If such a proof is provided later in the manuscript, the authors may want to "announce" that here already.

We certainly did not intend to imply that our methodology is "better" than other approaches. While we state in the introduction that more physically based models are potentially better suited for the application under changing (climatic) conditions than simple conceptual models, in the study we emphasize also the limitations of our methodology and attempt to quantify the uncertainties in the results. To follow up on this, in a future study we will compare our results with those obtained from applying a semi-distributed conceptual model in the same region and using the same forcing data, which will allow to investigate the uncertainties induced by different modeling approaches in more detail.

P6 L14-15: Here, it was not clear to me how the authors will handle temperature lapse rate in simulations for the future (since it is said that the lapse rates are "calculated from point measurements"). One sentence of clarification could be helpful.

For the application in this study, temperature lapse rates were not calculated dynamically but rather prescribed in the form of static monthly values which do not change in the future. In the case of dynamic calculation, the lapse rates are calculated separately for each time step by regressing the point data (which can be either actual measurements or e.g. downscaled climate model data) against elevation. We agree that the term "point measurements" in P6 L15 is too specific and will replace it by "point data".

P6 21ff: This is one of the very few points that I found conceptually problematic: The authors use a "snow correction factor (SCF)" that, basically, increases the modelled *amount of precipitation when precipitation is in its solid form. What does that mean for future simulations, i.e. for simulations in a warmer climate? I see the danger for the changes in precipitation to be overestimated: Since more precipitation will be liquid in future than it was in the past, less of the precipitation will be affected by the correction factor. With a correction factor chosen to be 1.15 (L198), my suspicion is that the effect could be as large as 15*

We agree that this is a valid concern. The increase in snowfall amounts by 15 % (which is applied additionally to the wind speed and temperature-dependent precipitation adjustment) is an empirical correction, however this value has been derived using a very thorough validation procedure taking into account various validation data sets such as areal precipitation, point-based snow depths, lidar-derived snow depth maps, and multi-year glacier mass balances (Hanzer et al., 2016). Although it has been shown that it is absolutely necessary to correct the observed precipitation amounts for undercatch in the study region, we agree that this fixed value might change given that climate conditions change as well. Utilizing the RCM-simulated snowfall fractions and precipitation fields instead of downscaling to point locations could reduce this uncertainty. Promising advances in downscaling methods such as quasi-dynamical approaches as e.g. in the ICAR model (Gutmann et al., 2016) could bridge this gap between statistical and dynamical methods and allow deriving more realistic small-scale precipitation fields. While this was not feasible for the present study, the revised version of the manuscript will include a short discussion on the possible uncertainties in model results induced by the precipitation downscaling and snow correction approach.

P7 L6ff: I think that the "SCF" (see above) should also be flagged as a parameter "requiring site-specific calibration".

You are right. We will change this sentence to: "Apart from the parameters of this linear reservoir model which have to be calibrated individually for each catchment, most parameters in the model have a physical meaning, and in general no site-specific calibration beyond the parameters for the runoff module and the precipitation correction method is performed."

P10 L3ff: I might have missed it later in the discussion, but here I thought that having at least one sentence addressing the limitation of quantile mapping methods would be appropriate. I refer to the limitations in handling extremes in particular.

We agree. We will briefly expand on the limitations of QM in the respective section of the article.

P10 L9: Here I'm not sure to understand the wording "to retain intervariable relations". Maybe the authors can rephrase?

We will change the wording to: "(. . .) and can preserve intervariable dependency structures".

P10 L30: Several options for the number of considered grid-cells are named (1x1, 2x2,etc.). Which one was used at the end?

The number of grid cells used for the averaging is not statically defined, but rather determined dynamically for each model, variable, and station using the methodology described in section 3.3 (P11 L1–6). As seen in fig. 4 of the manuscript, a value of 1x1 is the most common one, however all values of up to 10x10 are occurring.

P12 L2-3: Here, the authors seems to additionally downscale the temporal resolution of the EURO-CORDEX results. Can a sentence be provided that explains why this is necessary?

We will change the first sentence of section 3.4 to: "For the calculation of the snow and ice surface energy balance in AMUNDSEN, 1–3-hourly meteorological input time series are required in order to capture the diurnal variability of the contributing energy fluxes. As the EURO-CORDEX simulations were however only available in daily temporal resolution, an additional processing step was necessary."

P 12 L8-9: Still related to the above downscaling step: I found it rather problematic that *the step apparently does not preserve the daily average temperature. Can the authors give a hint on how large the introduced deviations are, and whether these deviations are systematic? If so, an additional de-biasing step would seem appropriate.*

We agree that ideally the daily mean temperature should be preserved, however given daily values of Tmin, Tmax, and Tmean, with the implemented disaggregation schemes (either assuming a sinusoidal temperature course or an "average" temperature course derived from hourly observations) it is possible to preserve only two of them (either Tmin and Tmax, or Tmean and (Tmax – Tmin)). Preserving Tmin and Tmax in combination with the other chosen disaggregation methods yielded better results with respect to the multilevel validation of the AMUNDSEN model results for the past, hence this method was used. The mean absolute errors of hourly observed vs. disaggregated temperature values are similar for both methods (1.12 °C for preserving Tmean and 1.19 °C for preserving Tmin and Tmax (values are averages over all stations)), however preserving Tmin and Tmax leads to a general tendency towards underestimated temperatures (0.38 °C on average).

P13 L6-7: I have difficulty in understanding the author's wording. Maybe they can rephrase?

We will rephrase P13 L5-7 to: "For relative humidity, an additional disaggregation method based on Waichler and Wigmosta (2003) was implemented. Hourly humidity values are generated using [month, hour, dry/wet day] categorical mean values with the additional option to preserve the daily mean humidity."

P15 L17-21: The result that the Oetztaler Alps are projected to warm significantly less that the rest of the Alps seems an important one to me. Can the authors comment on whether this is likely to be a robust results, or whether it may just be caused by the comparison between studies using different methodologies? If the former (= robust result), a speculation on the causes could be very insightful.

Thank you for pointing this out. In fact, further analyses show that this result can likely be attributed to the different methodologies used in the individual studies (comparison of downscaled and bias-corrected RCM data interpolated to a 100 m grid (our study) vs. raw RCM results (the other studies)). A robust comparison of future climate change in our study region vs. the Alps would need to be conducted using data sets generated with the same or comparable methodology. As this paragraph is not essential for our study, we will remove it in the revised version of the manuscript.

P20 L11: The wording seems somewhat unfortunate to me: I wouldn't call a 54

Unfortunately, parts of your comment seem to be missing, however likely you argue that a 54 % reduction in glacier area cannot be called "moderate", to which we agree. We will remove "comparatively moderate".

P20L20ff: Different reactions in terms of runoff evolutions are noted for different sub-catchments. It would be useful if the authors could add some explanatory sentences for why this is the case.

The differences are mostly due to differences in glacierization and total ice volume/average ice thickness in the respective catchments. We will add some explanatory remarks to the respective section of the manuscript.

P20 L34ff: The reported changes in winter runoff appear to be very large since they are expressed in

Indeed, the relative changes in winter runoff are very large however still corresponding to very low absolute values, which we also emphasize in the manuscript. Unfortunately, here again parts of your comment seem to have been cut off, hence we would politely ask to resend it if you suggest making changes to this part of the manuscript.

P23 L21ff: I'm not entirely convinced about the "fairness" of the analysis investigating the effect of spatial and temporal resolution: Obviously, changing the resolution without re-calibrating the model will impact on model performance. The question for me would rather be about the changes in model performance once the model has been *recalibrated. But maybe I simply misunderstood the authors' intentions.*

In the model, it is generally aimed to reduce the need for calibration. As stated above, the only parameters that have to be calibrated are the linear reservoir coefficients and possibly the snow correction factors. The former were in fact recalibrated for the assessment of the changes in model performance due to the changes in resolution and forcing data, however in the case of the latter the SCF of 15 % used in the original model setup (in combination with the temperature and wind speed-based correction and the openness-based snow redistribution) still yielded the best overall results with respect to the multilevel validation procedure. Hence, we believe our conclusion that model performance is not majorly affected by the changes in temporal and spatial resolution is appropriate.

P28 L5ff: Here (last part of the conclusions), I would have appreciated if some quantitative statements would have been included as well. Maybe, however, is just a matter of preferences ...

We agree that some quantitative statements would be appropriate here. We will replace the respective section of the conclusions by: "While some uncertainty in the results is due to the model configuration, the largest uncertainty can be traced back to the climate projections. This leads to a considerable range in the projected snow coverage, glacier extents and hydrological regimes. Snow cover is projected to decrease by up to 80 % in elevations below 1500 m a.s.l., while only comparatively moderate decreases (up to 25 %) are found for high-elevated areas (> 2500 m a.s.l.) due to strongly increasing winter precipitation which partly compensates for the increased warming. Glaciers will continue to recede strongly throughout the century. Until 2050, glacier volume will decline by approx. 60–65 % largely independent of the emission scenario, whereas by the end of the century 80–96 % of the original ice volume will be lost. Consequently, glacier runoff will diminish proportionally and summer runoff will strongly decrease in all investigated catchments by up to 55 %, resulting in a shift of the annual runoff peak from July towards June. Winter runoff volumes on the other hand will increase, however to still low absolute values. While the total annual runoff volumes stay approximately constant during the early 21st century compared to present-day levels, they gradually decrease throughout the rest of the century. Only for some catchments and scenarios runoff volumes slightly exceed present-day levels, indicating that the peak water period of maximum runoff is currently under way or has already passed in this region."

Figure 4: I have difficulties in understanding why the median deviations for "G" and "WS" (including the full name of the variables in the figure caption would be very helpful!) are only positive. To me, this is an indication that the model debiasing is not working correctly (the mean deviation should be "zero" in that case).

Global radiation is the variable with the least amount of available stations (3). For two of these stations, the mean deviation (MD) of bias-corrected vs. observed values is (absolutely) less than 0.025 W/m^2 for all models, whereas for one high-altitude station (Vernagtbach, 2640 m a.s.l.) it amounts to up to 1.1 W/m^2 (mean: 0.80 W/m^2), which however corresponds to a deviation of only approx. 0.5 percent in relative terms. Similarly, bias-corrected wind speed values partly are very slightly positively biased, however amounting to a maximum MD of 0.08 m/s over all models and stations. Hence, we believe these very small remaining biases are negligible for our subsequent analyses.

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