

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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The authors present a very interesting and thorough study of the effects of a changing climate on cryospheric and hydrologic processes in the Austrian Alps over the next century. The authors use a well-established physically based model, which employs a full energy balance approach to simulate snow cover evolution, and a comprehensive future climate data set to look in detail at several processes in the Ötztal Alps. Additionally and commendably, the modelling method used in the study considers the change of glaciers in terms of ice thickness and areal extent throughout the modelling

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period. Results are presented for the development of the glaciers in the study area, the changing depth and length of the snow covers, and for the amount and timing of the runoff in the area.

While comparable studies have been carried out in other parts of the European Alps (particularly Switzerland) this seems to be the first comprehensive study in the Eastern (Austrian Alps). This along with the already mentioned fact that the current study employs an energy balance model and includes glacier evolution in their analysis make this study very interesting for a larger readership. Overall, the paper is very well written. In total, it seems to be a bit on the long side. Maybe the authors could go over the paper again, trying to find some sections that could be shortened a bit. Especially the Method section seems to have some potential for shortening (in my opinion). Furthermore, some of the individual sentences seem to rather long which makes it a bit hard to read at times. It would probably be better to simply subdivide these sentences into two or three separate ones. Again I would encourage the authors to have one more look at the manuscript with this problem in mind.

The Abstract seems to me to be very concise while addressing all the major important points of the chosen method and the most significant results of the study. The introduction provides an adequate overview of previous studies in this field. The actual goals of this study are mentioned in one (rather short paragraph) at the end of the introduction. Here I would encourage the authors to maybe expand this part a little, explaining why this study area and this method was chosen and what sets this study apart from others before. The study site and data presentation are good and complete. The “Methods” section is very thorough, maybe even, as mentioned, a little on the long side with some potential for shortening. The “Results and Discussion” has a very logical setup and presents all relevant results clearly and concisely. Also included are appropriate comparisons of the results of the current studies to previous related studies. I especially applaud the authors for including an extensive section about the uncertainties in the modelling. Especially in modelling studies of the impact of future climate scenarios,

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such a section is highly valuable. Finally the "Conclusions" present the major results of the study in a concise form. The conclusions are well based on the results and give the reader a good summary. The Tables and figures are adequate and present the information in an easily understandable form.

Overall, this is in my opinion a well thought out and well presented study. It covers a topic that is one of the most pressing questions of the coming decades not only in a scientific sense, but also for society as a whole (winter tourism, freshwater availability, etc.). The chosen geographic location, which to this date has not been studied in this context and the use of a completely physically based hydro-climatological model along with an algorithm tracking the evolution of the glaciers throughout the model period present, in my opinion, a significant new addition to the overall scientific knowledge. The paper's topic falls well within the scope of the journal and is of interest to other scientists, but also to the general public and political decision makers. I would therefore recommend publication after minor revisions. As noted above, these revisions could address the overall length of the paper and the sometimes excessively long sentences, as well as the following minor specific comments.

We would like to thank the reviewer for their thorough evaluation of our manuscript and the very helpful and constructive comments, which are very much appreciated. With regard to your general comments, we agree that the paper is comparatively long and could benefit from some shortening. As a result of some of your comments and those of reviewer 2, we will remove some paragraphs in the revised version of the manuscript. In addition, we will expand on the goals and novelty of the study in the introduction. Please find below our replies to the individual comments.

I'm guessing that the layout of the article is not the final version, but as it is now, some of the Figures are quite far away from where they are discussed in the text. This makes it somewhat hard to follow the discussion. I would make sure that the Figures are placed closer to the text discussion of them in the final version.

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We definitely agree that the placement of the figures in the discussion paper is unfortunate, and will make sure that this is improved for the final version.

p.1 line 7-9: I would cut this sentence into two by putting a period after "century" and describing the situation below 1500m asl in a second sentence.

We will change this sentence to: "Results show generally declining snow amounts with moderate decreases (0–20 % depending on the emission scenario) of mean annual snow water equivalent in high elevations (> 2500 m a.s.l.) until the end of the century. The largest decreases, amounting to up to 25–80 %, are projected to occur in elevations below 1500 m a.s.l."

p.2 line 7-9: This sentence ("These general ... ") reads very awkward. Please try to rephrase.

We will change this sentence to: "Since these projected impacts are mainly triggered by increasing temperatures (shift from snowfall to rainfall, earlier onset of snowmelt), the likelihood of occurrence is very high. In lower-elevated catchments on the other hand, projected changes in precipitation exhibit a larger impact on changes in runoff (Horton et al., 2006)."

p.4: The authors mention three climate scenarios. However, the scenarios are not explained any further until in the results section. Maybe you could add a small paragraph explaining these scenarios i.e. what assumptions they make, how they are situated in the overall "scenario ensemble" (high, medium or low change assumptions)

We will add a short paragraph explaining the main characteristics of the three RCPs to the respective section of the manuscript: "For the scenario simulations until 2100, we used the EURO-CORDEX climate change projections (Jacob et al., 2013) as climatic forcing, while considering the scenarios RCP2.6, RCP4.5, and RCP8.5. The latter is a scenario assuming no implementation of climate mitigation policies, resulting in considerably and steadily increasing emissions and concentrations of greenhouse gases over

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time. RCP4.5 is an intermediate scenario consistent with peaking emissions around the mid-century and a strong decline afterwards, resulting in stabilizing concentrations by the end of the century. Finally, the intervention scenario RCP2.6 is at the very low end of the spectrum in terms of future emissions and radiative forcing, assuming a peak in CO₂ concentrations in the middle of the century and a slow decline afterwards along with negative emissions toward the end of the century."

p.6 line 15: Was terrain orientation and slope included in the calculation of the incoming solar radiation or were the model grid cells assumed to be "flat"?

Yes, in the radiation model for each grid cell a unit vector normal to the surface is calculated which is the basis for the subsequent calculations. We will add this information to the manuscript and change P6 L15f. to "(. . .) while taking into account terrain slope and orientation, hill shading, transmission losses and gains due to scattering, absorption, and reflections (. . .)".

p.6 p.32 Could you add one more sentence as to what modifications for climate conditions under forest canopies were applied?

We will change the sentence to: "In forested areas the interpolated meteorological fields are modified in order to capture sub-canopy conditions, resulting in reduced shortwave radiation, precipitation, and wind speed, increased longwave radiation and humidity, and an attenuation of the diurnal temperature cycle. Additionally the effects of the forest snow processes of interception, sublimation, and melt unload are accounted for (Strasser et al., 2011)."

p.7. Line 1: How was ground heat flux considered? Did you have ground temperatures (either measured or modelled) or was a constant value used?

Ground heat flux was assumed to be constant at 2 W/m².

p.8 line 20 and following: Is this paragraph really needed? It mainly discusses a method that is not needed for the current study. Maybe you could just explain what you did and

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omit the rest.

We agree. We will remove this paragraph from the manuscript.

p. 13 line 19: "This is partly explainable from the fact" reads very awkward. Maybe replace with "This can be attributed to the fact"

Done.

Results Section: Generally, comparable studies start out by showing that the chosen model "system" start out by showing that the model system was able to simulate the current state (here 1997 to 2006) adequately. There is a Table (Table 3) much later, where some model efficiency values for the modelling of the current state are shown. However the focus of this Table is on showing original versus disaggregated values. I would welcome a short section showing how well the employed model system does for the historic phase at the start of the results section.

The validation of the model setup for historical conditions has been described in detail in a previous paper (Hanzer et al., 2016), which is why we generally refer to this reference and only concisely discuss the changes in model performance regarding the few changes in model setup (reduced spatial and temporal resolution, temporal disaggregation of forcing data) in section 4.5. We agree that this might not be immediately clear from the article and will add a short paragraph for clarification to the start of the results section.

p.13 and 14: It is not entirely clear to me why observed data was used for the 1997 to 2006 period of the "regular" model runs, while RCM data was used for the 1971 – 2005 "snow cover model runs". Is this due to a lack of observed data from 1971 to 1997 or what is the reason for this procedure? Please explain.

Yes, the reason for using the historical RCM data instead of observed station data was to avoid introducing inhomogeneities in the simulation results due to the highly varying temporal coverage of station observations in the past. By using the historical RCM

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simulations and applying the same downscaling and bias correction methodology as for the future period, homogeneous and gap-free time series for the period 1971–2100 are obtained for all stations and variables. The model runs using observed data on the other hand were used (i) for validation, (ii) for deriving the ice thickness distribution for 2006 used for initializing the scenario runs, and (iii) as reference for calculating the future changes in runoff (figs. 15–17). We will clarify this also in the manuscript.

p. 15 line 17: "Comparing" should be "Compared"

Done.

p. 15 line 30: End sentence after "century".

Done.

p.15 line 32: "However, also for the" is not a correct English sentence. Please rephrase.

We will change this sentence to: "For the other two scenarios comparatively small decreases in average SWE are simulated in this period, amounting to 427 mm (–18 %) for RCP4.5 and 357 mm (–31 %) for RCP8.5."

p. 15 line 35: For "the" station Obergurgl

Done.

p. 17 line29: "occur already" should be "already occur"

Done.

p.24 line 13: End sentence after (Huss et al 2010)

Done.

References

Hanzer, F., Helfricht, K., Marke, T., Strasser, U. (2016). Multilevel spatiotemporal

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