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Hydrology and Earth System Sciences Discussions

Interactive comment on "Impacts of climate change scenarios on runoff regimes in the southern Alps" *by* S. Barontini et al.

Anonymous Referee #1

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This paper describes the application of GCM data for calculating the impact of climate change on stream-flow runoff in the Southern Alps. The rise in the altitude of the tree-line and the change in glacier coverage are accounted for. The validation and the downscaling of GCM output is addressed. In total, this paper presents a comprehensive modelling study which will be of interest for the scientific community. However, I see several topics that need further discussion and I also disagree with some components of the presented methodology. The major problems are listed below and addressed in detail later in the review letter.

- Are GCMs really suited for regional impact studies in mountainous regions? Can a GCM that covers the entire European Alps with an handful of grid cells that barely represent the effect of a mountain range resolve the future changes in the

temperature and precipitation regime? This issue should be carefully discussed. The work presented in this study has a strongly regional and not a global character. However, there is no reference in the whole paper to Regional Climate Models (RCMs). What is the reason for not using RCMs? For the Alpine region there are many RCM results available providing information on a much finer grid (e.g. Christensen and Christensen, 2007). The authors should investigate whether GCMs predict trends in the future changes in temperature and precipitation that are equivalent to RCM based results for the alpine region. In any case, a justification for using GCM results and not RCMs for this regional study should be provided.

- Could the authors give some estimate of the uncertainty in their results? What is, for example, the impact of only using temperature and precipitation data for the modelling and neglecting the other variables also provided by GCMs, e.g. humidity, cloudiness etc?

- I have several objections against the method to update future glacier extent. I basically agree with the concept to calculate the change in equilibrium line by Kuhn. Its application to obtain the change in glacier extent, however, is, first, not clearly enough described and, second, does not correspond to the current state of glaciological research (see comments below). Furthermore, it is known that methods ressembling equilibrium line concept lead to a strong underestimation of future runoff from highly glacierized catchments.

- What are the reasons for a decrease in runoff with (almost) constant precipitation? This question needs to be prominently discussed, including the uncertainties. To what percentage can the differential changes in runoff and precipitation be attributed to evapotranspiration and to the change in glacier storage of water? The 'discussion'-section implies that only evaporation changes matter and glacier changes are – although considered in the modelling – irrelevant.

- In this paper, many details are described in great detail, e.g. in the introduction. However, some very important components are almost completely omitted, e.g. the field data sources or some important points in the description of the hydrological model. I suggest shortening some paragraphs in favour of extending some other central issues.
- The text of the manuscript should be read and corrected by a native English speaker.

Detailed comments are listed below:

Abstract

- **e.g. Page 3090, line 6:** *'glaciated'* refers to a period of glacier cover (time), *'glacierized'* to the spatial extent of ice-covered areas. I suggest using 'glacierized' everywhere in the text.
- **Page 3090, line 7:** "better reproduce ..."? than what?
- **e.g. Page 3090, line 10:** "should" – in my opinion inappropriate word choice. Better 'expected' / 'projected' / 'simulated' ...
- **e.g. Page 3090, line 22:** "changes of ..." – 'changes in ...' is more correct in English.

Introduction

- **Page 3091, line 5:** Why do the authors cite exceptions here? They do not matter in the context of this study.

- **Page 3091, line 20:** "... mass loss of more than half a metre water equivalent ...". A mass loss should be given in m^3.
- **Page 3093, line 8:** What do the authors mean by 'autumn freezing'?
- **Page 3094, line 18/20:** Why are two uncertainties for the same variable given?

GCM scenarios

- **Target areas:** The authors refer to two investigated catchments. In fact, the paper discusses three: Oglio, Lys and Lake Gabiet.
- **Page 3096, line 15:** The unit of 'total runoff' is m^3 and not mm.
- **Page 3097, line 8-27:** This review of IPCC-emission scenarios is too long in my opinion. Why do the authors discuss four of them, when they use only one for their calculations in the end? Furthermore, it should clearly be stated here, which emission scenario is chosen to produce the results presented in this paper.
- **Page 3098, line 15:** Reference to subfigures with **a/b**.
- **Page 3097, line 17:** The authors talk about 'a network' of weather stations. I strongly suggest that this network is described in more detail – Where are the stations (including a figure)? How many? What variables are recorded? Who provided the data?
- **Page 3098, line 18:** "The GCM data..." – GCM results for one grid cell? Averaged for several nearby GCM grid cells? Which grid cells? This could also be shown in a figure.

- **Page 3098, line 25:** Essential information for understanding the content of tables should be provided in the Table Caption and not in the text.
- **Page 3100, line 15:** Temperature changes for two emission scenarios are given here. Above, four are discussed, the final results are produced with only one. The authors should either focus on one scenario or produce runoff results for all of them. It would be interesting to know the possible range of change in runoff due to different emission scenario input.

Spatial downscaling of PCM data

- **Page 3101, line 6-10:** The temporal resolution of GCM data used in this study is daily – why discussing the downscaling of monthly data to a daily scale?
- **Page 3103, line 18:** There is no correction applied for liquid precipitation? It is known from several studies that the measured precipitation is an underestimation of the actual one, also in the case of liquid precipitation (e.g. Sevruk, 1985).
- **Page 3111, line 8:** The description of the downscaling procedure is very detailed and long. However, after reading through it, it was unclear to me what it exactly produces: A time series for each station of the network? A time series for each pixel of a fine grid (if yes, which resolution)? Some summary sentences would help.

Hydrological simulations

- **Page 3112, line 14:** The rise in the altitude of the tree-line is extensively discussed. However, in alpine environments also the upper boundary of vegetation

(grassland-rock) will probably rise with climate warming (if the tree-line does). This might also have a significant impact on evapotranspiration. Is this factor considered in some way?

- **Page 3112, line 24:** It seems quite speculative to me that the rise of the tree-line is only related to air temperature. What is the impact of changes in precipitation sums or the precipitation regime? I imagine that droughts in summer (climate models expect less precipitation in the summer months) might be unfavourable for vegetation development and the subsequent rise of the vegetation lines.
- **Page 3112, line 28:** And what was done for the first time window?
- **Page 3112, line 1:** "... consequent reduction ... " In a dynamically changing climate system there is not a directly 'consequent reduction' of glacier extent after a rise of the equilibrium line! Glaciers may be much larger than their current equilibrium line altitude implies, therefore providing much more runoff than if they are assumed to be in equilibrium with the current climate.
- **Page 3113, line 2:** Does this mean that glacier extent was not updated for the Oglio catchment? And for Lake Gabiet?
- **Page 3113, line 15:** One important point is missing in the calculation of the equilibrium line altitude: accumulation changes (Ohmura et al., 1992). The authors assume the variation in cloudiness to be negligible – are changes in the solid precipitation negligible as well? It is stated at several places in the paper that the precipitation regime is subject to change and that the fraction of liquid precipitation increases. This has an important positive effect on the rise of the equilibrium line.
- **Page 3113, line 19:** Glaciers are not expected to reach 'equilibrium states' throughout the 21st century. This approach, thus, poses some problems.

- **Page 3113, line 20:** I have no objection against Kuhn's equilibrium line concept. However, I doubt if he intended it to be applied in this form. The change in the elevation of the glacier terminus is not directly related to the change in the equilibrium line, as it is implied here! There are simplified approaches to calculate glacier extent from equilibrium line change based on the so called Accumulation Area Ratio (Schaefli et al., 2007; Paul et al., 2007). However, discharge from highly glacierized catchments in transient simulations over the 21st century can be underestimated by more than 30%, when deriving glacier extent from the equilibrium line altitude using the Accumulation Area Ratio (Huss et al., 2008).
- **Page 3113, line 22:** The change in the glacier terminus elevation of 240 m until the end of the 21st century is not realistic (Zemp et al., 2006; Paul et al., 2007; Huss et al., 2008). On several Alpine glaciers the glacier terminus altitude has already increased by up to 500 m throughout the last century with a temperature increase of only one degree.
- **Page 3113, line 24:** Was glacier extent updated for both time windows? The authors should definitely provide the change in the glacierization in percent for 2050 and 2090.
- **Page 3114, line 9:** How large are these units? How is the extent of a unit with hydrologically similar behaviour determined?
- **Page 3114, line 20:** Changes in runoff are primarily attributed to an increase in evapotranspiration. In glacierized mountain ranges (in the Lys catchment I estimate an amount of 2-3 km^3 of ice to be stored) the wastage of glaciers will provide an important source of runoff over the next decades (Stahl et al., 2008; Huss et al., 2008). I am not sure if the methodology described in this paper is sufficient to estimate the glacier contribution to runoff, which might be – at least for the first time window – almost equally important as evapotranspiration changes.

- **Page 3115, line 11:** Radiation has the unit of mm d^{-1}? This needs some explanation.
- **Page 3115, line 15:** What 'empirical relations'? Extraterrestrial radiation can be calculated with trigonometry only.
- **Page 3116, line 14:** This is the first time a spatial resolution for the hydrological modelling is provided. The question arises whether a 1 km DEM is sufficient in alpine terrain to resolve the spatial variability of accumulation and melt processes. Are some sub-gridcell schemes included? For example, I expect most glaciers in the catchment to be smaller than 1 km^2: Are these small ice masses just non-existent in the modelling? Or taken into account in some way?

Results and discussion

- **Page 3116, line 19:** Which 'measured' data? How many gauging stations? Where are they located? What is the data quality? I suggest describing these data together with meteorological records in a 'field data' section at the beginning of the paper.
- **Page 3117, line 8:** The description of the hydrological model is very short in this paper. Therefore, the reference to a 'snow-melting factor' is not understandable. The authors should state, how melt is calculated for snow and glacier ice. It is as well never mentioned, how accumulation is computed.
- **Page 3117, line 22:** Why only March?
- **Page 3117, line 27:** Which 'coefficient of correlation'? The Nash-Sutcliffe-Criterion R^2, or the squared coefficient r^2 of linear regression?

- **Page 3118, line 2:** The authors should describe here how they tuned their model! Which parameters were calibrated? Based on which measurements? Figure 5 is much too small and does not allow an assessment of the quality of the model validation.
- **Page 3121, line 14:** I do not understand what is meant by "term of reference ..."

References

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