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Hydrology and Earth System Sciences Discussions

Interactive comment on "The relevance of glacier melt in the water cycle of the Alps: an example from Austria" *by* G. R. Koboltschnig and W. Schöner

Anonymous Referee #1

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This paper addresses an important issue that is 'a hot topic' in current climate. The authors provide an extensive review of previous studies in the field of water yield from glaciers. They compare runoff in August 2003 from 27 catchments with a wide range of glacierization to 10-year mean August runoff. These data are used to make qualitative inferences on the importance of glacier melt runoff.
Overall, my opinion agrees fairly well with the Interactive Comment already posted by B. Schaefli: In its present form the conclusions of the paper are too vague. Some relation between the catchment glacierization and runoff in August 2003 is revealed, but the interpretation by the authors does, at the moment, not allow for new insights. The

main conclusion is – equal to Zappa and Kan (2007) – that catchment glacierization shows a relation with the anomaly in August 2003 runoff. What can be learnt from this for practical use? The authors want to address this issue in the paper, but I was "left with more questions, than answers" at the end of the paper (following the words of B. Schaefli).
I have a number of specific comments (see later), and some major points that refer to the methodology itself, and the general interpretation of 'glacier melt' data:

1. At page 2899, line 4, and elsewhere, I asked myself, how the authors define 'glacier melt'. In order to provide answers that can be understood unambiguously throughout the entire hydrological community, a clear concept – a definition of 'glacier melt' – is required.
   What is glacier melt? (1) Is it bare ice melt? (2) Is it melt over a glacierized surface (which then includes a fraction of snow melt)? (3) Or is it glacier storage change (the authors cite Lambrecht and Mayer (2009) relating to this possibility)? The quantity of glacier melt according to options (1) and (2) depends greatly on the hydrological model used, and whether it is able to correctly reproduce depletion processes of the glacier surface in space. Option (3) seems to be less delicate from a hydrological point of view, as it can be directly constrained with the water balance equation (discharge measurements).
   The authors should clarify what they are talking about. Otherwise drawing conclusions from the data presented is difficult.

2. I have some doubts if the proposed methodology is well suited for answering the principal question: What is the contribution of glacier melt to the water cycle?
   Following my assumption that the authors define glacier melt as bare ice melt (according to case (1), see above): In August 2003 the glaciers were completely snow-free (references in Chapter 3). In 'normal' years glaciers are however often snow-covered to a percentage of more than 60% in August. Can empirical relations derived for the extreme case of 2003 be used for deriving bare ice melt

in years with substantial snow coverage? According to the last sentence of the Introduction this is the rationale of the present study (which is however not further developed later in the paper).
Furthermore, the overall glacier mass balance in the years 1997-2006 used for comparison was also negative (according to WGMS, 2008, to most negative decade since the beginning of the records in the 1950s/60s). Glaciers were on average far away from equilibrium conditions and provided much stronger water input into the hydrological cycle than in years with zero glacier storage change. Is this decade really suited for the considerations made in this paper?

3. My overall impression is that the manuscript is too qualitative. After reading it I did not understand how the factor q_{A03} can actually be used to estimate numbers of glacier melt contribution as provided for example in the last paragraph of the Introduction. Besides some qualitative consideration of the correlation between glacierization and q_{A03} no link to the estimation of glacier melt was established, as was promised in the Introduction.

4. Review of previous literature almost entirely fills chapters 1-3, and also some parts of chapters 4 and 6. I'm not against a thorough review of existing literature, but I think the paper would highly benefit from a better discussion and a stronger focus on the methods proposed in this work and its implications.

5. Before a revised submission the language needs to be improved. Currently, there is a number of spelling and grammar errors. The manuscript would benefit from being corrected by a native speaker.

Detailed comments:

- **Abstract:** Compared to the length of the paper, the Abstract is too long, too specific, and provides too many numbers. The Abstract could be written more concisely focussing on the general conclusions and the benefits of the method.
- **Page 2898, line 13-16:** Difficult to understand, please reword.
- **Page 2898, line 17 and 21:** The word pairs are: (1) glacierized - glacierization; (2) glaciated - glacation. Normally, (1) is used in the spatial, and (2) in the temporal context. (1) seems to be more appropriate to me. 'Glacierized' and 'glaciation' are mixed up throughout the paper. The authors should at least be consistent.
- **Page 2899, line 2:** 'Retreating' glaciers provide *more*, and not *less* meltwater, as it is stated here. Only if the glaciers are gone (or very small) the statement made by the authors is correct.
- **Page 2899, line 7:** Related to my first comment above: I assume that the authors define 'annual glacier melt' here according to my option (1) – melt over bare ice surfaces. If yes (please correct me if I'm wrong), could the authors elaborate on the information content of this variable? Snow melt over glacierized surfaces also reduces the annual mass balance of the glacier, i.e. its water storage. Why can this term be excluded from a hydrological perspective? The quantity of melt occurring over the bare ice surface can not be used for a closure of the water balance, because it only represents a part of the annual (or monthly) storage change.
  If numbers of 'glacier melt contribution' (whatever the definition) are stated, details on how these are obtained should be shortly described in this paper, although Koboltschnig et al. (2008) is referenced.
- **Page 2900, line 5:** 'DEMs' - abbreviation should be explained
- **Page 2900, line 17:** In a recent study, it was shown that high elevation melt rates were higher in the 1940s than in recent years (Huss et al., 2009, Geophysical

Research Letters). This was attributed to significantly higher global radiation in this period. It seems possible that the currently lower runoff volumes noted by Collins (2008) in comparison to the 1940s and 1950s could also be explained with the importance of radiation, and do not need to be purely attributed to glacier area change.

- **Page 2900, line 19:** Why is there a disagreement between the studies? One relates to the past, one to the future. There is no overlap of the study periods, so there is no potential for 'complete disagreement'.
- **Page 2901, line 21:** "visible" – What does this mean? In my view, this is too qualitative for scientific paper. Because this is a central concept upon which the entire paper is based, the authors should try to be more specific.
- **Page 2902, line 4:** "no day below 0 deg C" – this statement is relative to the elevation of the measurement! Providing a temperature offset of x degrees compared to normal conditions would be more useful.
- **Page 2902, line 22:** "most extreme" – compared to what?
- **Page 2902, line 24:** What are "laboratory conditions" when talking about processes in high mountain areas? I would avoid this term – nature is always unlike a closed laboratory.
- **Page 2903, line 15:** Repetition of page 2902, line 4.
- **Page 2903, line 16:** I find this argumentation questionable from a statistical point of view. Is it allowed to 'discard' some data points, because they do not fit well into the concept (thunderstorms and rainfall events)? On page 2905, lines 9-15, the authors even state that significantly different results are evident when NOT excluding these data points.
  I see that the authors would like to separate a period which is dominated by the

presence (or absence) of melt. But Figure 1 shows that there are also considerable amounts of precipitation in mid-August – so there is a "non-laboratory" mixture of processes anyway.
If a subperiod of August 2003 is chosen, the comparison to the measurements for 1997-2006 should be performed over exactly the same period, for consistency.

- **Page 2903, line 29:** Is there an estimate by how much the glacier area changed between 1998 and 2003?
- **Page 2904, line 11:** Basically, the glacierized area for the upper Inn valley is available from the Swiss Glacier Inventory of 1999/2000 (see Paul et al., 2004, Geophysical Research Letters).
- **Page 2905:** As already mentioned in the Interactive Comment by B. Schaefli the Results-section is too short to reveal and adequately discuss new and potentially interesting findings.
- **Page 2906, line 7-8:** What do the authors mean here by "fluctuations of glaciers ..."?
- **Page 2906, line 12-13:** There are two points about this sentence: First, this paper does not state any correlation coefficient originating from a statistical analysis. The reader can see some qualitative relation between the variables in Figure 3, but no numbers are given. If the authors would like to stress this point, they should perform a quantitative statistical evaluation of their data. Second, in a statistical analysis it is not allowed to just "neglect" outliers (because they do not fit into the general picture).
- **Page 2906, line 17:** "... glacier melt is insignificant" – what does 'insignificant' mean in this context? Significance is, again, a statistical term that should be related to some statistical analysis or test. Here, it seems to be used purely qualitatively. And it is not clear what the basis for this qualitative judgement is.

- **Page 2907, line 1-2:** The methodology by Lambrecht and Mayer (2009) is here termed 'misleading'. The authors argue that it "gives the impression that glaciers only contribute to runoff during periods of negative mass balances". The word 'misleading' should be replaced and the sentence be rewritten. If the annual mass balance is positive, glacier do *not* contribute to total runoff! During the summer months glaciers contribute to runoff with positive and negative annual mass balance. This is, however, not in contradiction to considering annual glacier storage changes.
- **Page 2907:** The conclusion terminates with another review of literature. The authors should summarize their findings and clearly focus on their value for hydrological applications.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 2897, 2010.