Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-309-RC2, 2017

Hydrology and Earth System Sciences
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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.

Anonymous Referee #2

Received and published: 22 October 2017

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 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.

SPECIFIC REMARKS

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.

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.

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.

LP6 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 15P7 L6ff: I think that the “SCF” (see above) should also be flagged as a parameter “requiring site-specific calibration”.

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.

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

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

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?

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.

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

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.

P20 L11: The wording seems somewhat unfortunate to me: I wouldn't call a 54P20 L20ff: 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.
P20 L34ff: The reported changes in winter runoff appear to be very large since they are expressed in 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.
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...
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).

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Discussion paper