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Interactive Comment

Interactive comment on "Elevational dependence of climate change impacts on water resources in an Alpine catchment" *by* S. Fatichi et al.

B. Schaefli (Editor)

bettina.schaefli@epfl.ch

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The three reviewers and the additional comments are all rather critical about the scientific significance and the quality of the research in the form it is presented in the submitted manuscript. The discussion of the paper shows however that the submitted manuscript essentially failed to convincingly report the novelties of the research and the relevance of the results and conclusions. I am convinced that the research underlying the manuscript is very interesting for the readers of HESS but the manuscript requires some substantial reworking. As far as I can see, this will not involve many more new simulations but a better presentation of what has been done.

Given that the paper deals with a topic and a case study which I know very well myself

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Interactive Discussion
Discussion Paper

(see namely Hingray et al., 2010;Tobin et al., 2012), I will provide an additional Editor review in a separate comment. Hereafter, I would like to summarize the discussions and the main comments of the reviewers.

Main comments

1) What are the novel contributions of the paper / new findings?. The authors summarize the objectives of their work and new results very well in their response to reviewer 2. This should be reflected in the new paper, namely in the abstract, the introduction and the conclusion.

2) Relevance of the results given that only one GCM and 2 RCMs are used for one greenhouse gaz emmission scenario. This important critic has been convincingly discussed by the authors arguing that they account for the stochastic variability of climate and that their projections do not go beyond 2050. This point should become very clear in the new manuscript.

3) Future scenario without modified water management / hydropower production rules. The authors argue that keeping the management equal to the present-day situation is the best they can do. Furthermore, they argue that the future simulations with the present-day management still convey interesting findings whereas, in response to reviewer 3, they argue that a future scenario simulation without hydropower infrastructure is not interesting. This is not entirely convincing. The authors report elevation-dependent climate change impact effects which are processed through the hydraulic infrastructure with irrealistic management rules (present-day for future scenario). What is the relevance of such results? Would it not be more interesting to also report the "natural" response? Reporting this natural response would be particularly important for comparison with subsequent work in other Alpine regions. Providing material for comparative studies is indeed of prime importance. I am furthermore not convinced that keeping the management to present-day situation is the best and only option at this stage (please refer to my editor review).

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4) Too simplistic glacier model (uniform ice thickness). The line of argumentation of the authors is essentially "that's the best we can do" and they discuss the fact that related errors would essentially appear as timing errors (in terms of when the new regime will appear).

5) The paper reports climate change impact on extreme events without showing evidence that the modeling system does a good job in simulating extreme events. This point is to my view very critical. Most climate change impact studies do not discuss extreme events because the researchers think that neither their meteo scenario production scheme nor their hydrological model does a good job in simulating extremes. Both points should be extensively discussed. Personally, I do not believe that a model, which does a good overall job (water balance, seasonality, spatial distribution of components), does necessarily a good job for extreme events. And this problem is certainly not "smoothed out" by analyzing differences in extreme events rather than absolute values. I would even argue that a model that is calibrated (with whatever method) with observed meteo data (station data, gridded data) has a very low probability to produce reliable results for extreme events if it is run with *generated* data (spatially or temporally downscaled precipitation and temperature).

6) Used methods are not well presented. I have to agree with the reviewers that I do not think that the level of presented details for the climate change scenarios is sufficient. The corresponding subsection just gives a suit of references without sufficient details. Even after reading the paper several times, I do not have a complete picture of all the steps. I would like to have supplementary material (online only) discussing in sufficient detail the used methods. This should include a table with the change factors (I guess they are monthly factors) which might be re-used by later studies. It would also be nice to have a sketch of the entire scenario production procedure, including the step of producing input to the gridded hydrological model (the interplay of gridded RhiresD data and station data is not very clear). Supplementary material might also be of use for other methodological details.

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7) Use of a manual rather than an automatic calibration procedure, absence of uncertainty analysis. The paper does not present any details about the parameters, neither on their values, nor the spatial variability. Are all the parameters distributed in space? How many of them have no direct physical meaning? How were the soil properties related to model parameters? Were the manually adjusted parameters distributed in space? At the moment, the reader is left with the impression that the results just fall out of the model and that we have to believe them without any further insights. And we do not get new insights into how to set up a spatially distributed model for similar case studies. There should also be a *better justification for the absence of an uncertainty analysis*, especially for extreme events. And finally: there is a huge literature on selecting hydrologically meaningful model performance criteria; from my point of view, computational time is the only limiting factor to automatic calibration and uncertainty analysis.

References

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