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Hydrology and Earth System Sciences Discussions

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

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Dear Editor Bettina Schaefli,

Please find below our responses to the major comments of reviewer-1. This is by no means intended to be the final rebuttal letter with a point to point response to all of the reviewer's comments but just a contribution to the ongoing open discussion on this manuscript.

Sincerely,

S. Fatichi, S. Rimkus, P. Burlando, R. Bordoy, and P. Molnar

Responses to reviewer 1

Referee 1: GENERAL COMMENTS

Referee *1) WRITING STYLE - Although certainly not badly written, the paper has a writing style that is irritating at times. Often sentences are unnecessarily "prosaic" and low in information content. In other cases (and this is especially true when the methodology is presented), artificially complex wording is used, without adding substantial information. This gives somewhat the impression that the authors try to "hide" behind some impressive wording, rather than aiming at giving a clear description of what they have actually been doing. Examples are found at page 3749 line 21-22 (why such a general sentence?), P3750 L19-20 (what is the need of the statement after the comma?), P3753 L20-21 (what is the need of introducing a concept that is then not used?) or P3758 L17-18 ("re-parameterize the multisite Neyman-Scott Rectangular Pulses and the multivariate Markovian models" sounds impressive indeed, but an explanation of what was actually done would be more appropriate...). Please refer to the "Specific comments" and "Stylistic comments" sections for more details, and suggestions for improvements.*

Response: We thank the reviewer for his/her insightful work in providing extensive comments on writing and stylistic style. We will account for most of the comments in the revised version of the manuscript. However, we have to clarify that we certainly did not intend to hide anything concerning the methodology used. We recognize than in the effort of conveying to the reader just the most important information, without excessively increasing the length of the methodological part, we might have excluded some explanation that would have facilitated the understanding of the manuscript, especially in the description of how we dealt with snow and glaciers. We will fix this point in the revised version.

We respectfully disagree with the comment of the reviewer regarding the lack of clarity and use of artificially complex wording in the description of the stochastic models used for precipitation and air temperature generation and their parameterization (specific comments P3757 L 20-23 and P3758 L16-17). The Neyman-Scott Rectangular

Pulses and Markovian models are well established methods, well known to readers of hydrological journals and they do not require detailed explanations in addition to the references we provided. However, we will provide additional references in the revised version of the manuscript to better clarify this point. Regarding how these models are re-parametrized to account for climate model realizations (i.e., climate change) in the stochastic downscaling, this is fully described in Bordoy (2013) and in Bordoy and Burlando (2013b, under review), both cited. We believe that a more detailed description of the method is not necessary in this paper and would in fact distract the reader from the main message we are conveying.

Referee *2) LINE OF ARGUMENTATION - Sometimes, the line of argumentation exposed for justifying some particular assumption is weak – certainly too weak for making it acceptable in the current form. Examples include the reasoning exposed when justifying the choice of a uniform ice thickness for glaciers (P3753 L10-14), or the choice of not using an automatic calibration procedure when calibrating the model parameters (P3756 L22 – P3757 L14). Again, refer to the "Specific comments" for details.*

Response: We agree with the reviewer that the approximation chosen to initialize ice thickness needs additional explanations and mostly needs to be emphasized in the discussion. This will be done in the revised manuscript (see also response below to comment 3).

We respectfully disagree on the criticisms of not using an automatic calibration procedure. It would have not been difficult (if we exclude the computational time) to define a feasible range of parameters and run an automatic calibration procedure. However, we believe that calibrating the model using streamflow (strongly influenced by anthropogenic disturbances) would just lead to a compensation of errors originated by the imperfect description of the infrastructure. This would likely affect parameters controlling components of the hydrological process (evapotranspiration, subsurface flow, etc.) that cannot be constrained due to an absence of observations. Furthermore, it won't add anything to the correct physical hydrological description of the system rather than

producing better (most probably just marginally better) numerical results in terms of efficiency and determination coefficients for streamflow. Since, we are using the model to simulate relative changes due to a modified climate forcing and not for "flood forecasting", we are convinced that it is scientifically more sound to make a "best" expert choice of the parameters given the available data and information, and only marginally fine-tune those to observations. We have compared our "best" expert choice with automatic calibration of Topkapi-ETH in other studies and found that the performance improvement after automatic calibration is usually marginal and not justified.

Referee *3) WHAT'S ABOUT GLACIERS? - One of the conclusions of the paper is that "Ice melt contribution was identified as a crucial Process [. . .]" (P3772 L16-17). Why isn't there, thus, a single word on how the glaciers are modeled in the study?? The only information the reader is given is on how the initial ice thickness was computed (P3753 L1-14). But how was the glacier evolution accounted for? Currently, the reader can only guess. My guess is that glaciers were simply downwasted non-dynamically (i.e. melted locally with the melt rate computed with the temperature-index model named at P3750 L1-2, without taking into account any ice dynamics), but this is only a guess, since no sentence state it explicitly. If this guess is correct (and the unrealistic results that are given for "ice melt" in Table 2 seem to point at that), the methodology is definitively not adequate and needs to be adapted. Also in this case, refer to the "Specific comments" for more details.*

Response: We will provide better explanations in the methodology of the revised version of the manuscript regarding the description of glacier modeling also in response to the specific comment (P3753 L1-14) and (Table 2). The reviewer is correct in saying that glaciers are melted locally without accounting for ice dynamics. The reviewer suggests us to consult the "glaciology institute" for advice (Specific comment Table 2). This work was part of the ACQWA project and the "assumptions" on how to initialize and model glaciers at this spatial scale were actually agreed upon with several glaciologists, partners of the project. We would like to emphasize that the size of the basic computational elements (grid elements) we used in the hydrological simulations is 250m x 250m. While this resolution could be considered relatively detailed for distributed catchment hydrology, especially for simulations at the scale of the entire upper Rhone, it is still far from allowing to solve properly detailed surface features. Accounting for glacier dynamics would be meaningless at these scales, for at least two reasons. First, because the morphological features of specific glaciers, very important for ice flow, could not be resolved, second because in a 50 year period glacier mass can travel down, in most cases, a maximum of just one or two cells. We implemented and tried the Huss et al. (2010) parameterization for glacier mass re-distribution in other studies, but it was found ineffective in terms of hydrological consequences. We will clarify this in the revised manuscript together with the fact that we neglect permafrost and that the model is partitioning snow and rain according to a temperature threshold. Snow is accumulated at very high elevations where it does not melt. This snow that in reality is lost through sublimation or redistributed through wind and avalanches does not contribute much to the overall hydrological budget. We calculated that the areas where we accumulated snow (>5m at the end of the simulations) are 1.6% of the overall catchment area and are likely to be hydrologically irrelevant.

Therefore, we believe that these assumptions, probably unacceptable for a detailed glaciological study, are not limiting for our study given the grid size we used (for instance snow redistribution is mostly expected to occur within a single computational element) and are not affecting the results we obtained for most parts of the upper Rhone basin.

The only assumption that we believe is in fact "critical" is the initialization of the ice thickness at the beginning of the simulations. This can have important consequences on the timing of glacier thinning (retreat) and ice melt reduction. We already explicitly stated this limitation in the original manuscript (P3769 L14-17). However, we will expand the discussion of this problem in the revision, just to be sure this limitation can be made clear to every reader. A constant ice thickness for each glacier is probably responsi-

ble for the very abrupt decrease of ice melt we simulated in our study. In principle we could consider that all the glaciers are initialized with a different thickness (computed in relation to their area), inducing some variability throughout the catchment. With a cell size of 250m x 250m the extension of most glaciers (except the bigger ones) would be characterized by just a few cells; (mean cells for glaciers is equal to 40), therefore a more accurate representation of distributed ice thickness would likely not change significantly the overall result of the simulations.

Together with the abrupt transition another problem related to the initialization of the ice thickness is the timing of when most of the glacier retreat occurs. If we overestimated the initial ice thickness we have delayed the glacier retreat. If we underestimated it, we have accelerated the process. This is probably the case in our simulations but as shown by Gabbi et al., (HESS 16, 4543–4556, 2012) the acceleration is likely to be less than 10-15 years. In both cases, all the conclusions of the manuscript and especially the elevational dependence of climate change remain valid but just shifted in time by some years. This will be discussed in the revised manuscript. Further, because we do not aim at providing exact projections for specific years and we rather discuss the differential character of the response between high elevation and downstream rivers we are persuaded that this is not a crucial weakness of the study.

Referee *4) ADEQUATE INTRODUCTION? - Another conclusion (and this is repeated several times in the manuscript, e.g. P3756 L18-20, P3768 L1-5) is that the amount of water used for domestic, industrial and agricultural uses is negligible in the catchment. Is then the introduction, in which the importance of hydrological studies for correct water management etc. is stressed, adequate? In particular, why should it be important to work out future projections within such a complex modeling scheme if the resource that shall be managed (water) is only exploited to a negligible amount anyway? If statements such as reported at P3771 L12-14 ("broad impacts of climate change in water resources of the entire Alpine areas might have been overestimated in the past") shall be the main message of the study, an introduction pointing at the fact that the* *focus of previous studies was mainly on selected high-alpine basins, would be more appropriate, I believe.*

Response: We agree that the managed water resource is very small for domestic water consumption and relatively small for irrigation. However, this is not the case for the management of water induced by hydropower production where it is very important. Since we provided relatively detailed quantifications of all these uses at a fairly large scale, we thought it was important to give them a space in the introduction. In this regard, this quantification was found useful by many partners of the project and we believe it will be found useful also by a border readership.

We dedicated a fairly large part of the introduction to state the importance of carrying out simulations for the entire upper Rhone and that previous studies were mostly focused on high-elevation basins (P3746 L1-L27). The difference in the impacts between high elevation glacierized basins and more downstream station is indeed one of the major results of the analysis. Hence, also the title, which emphasize this result that we consider very robust regardless of all the uncertainty of the study.

Referee *5) ADVENTUROUS INTERPRETATIONS - Beside the quite spectacular claim mentioned above, the authors dare some interpretation on statistics of extreme events (minimal and maximal discharge in particular). At P3767 L5-6 for example, the statement reads that "a lack of change is the most probable projection" for "minimum discharge [...]and maximum 30 days discharge". If such a claim (which is potentially everything else than harmless if fed to the wrong place...) is made, it should definitively be based upon a serious assessment. In the paper, however, an assessment on the ability of the model in modeling extreme events is completely missing! No validation of any statistics concerning extreme events was performed for the time series modeled for the past, and at least this would be required before making any interpretation in that direction.*

Response: The comparison in terms of simulated extremes for the observational pe-

riod was done but not included in the current manuscript. Presenting all the results of this study in the manuscript would have made it excessively long and likely unsuitable for publication. We thought that since changes are presented in relative terms between the climate simulated control scenario and simulated future scenarios, both accounting for stochastic variability, the overall assessment was robust and fair. In other words, if the model reproduces realistically the general hydrological behavior of the Rhone (as we demonstrated in the manuscript) is not much important to capture with very high precision extremes in order to give a rough estimate, including uncertainty, of the relative percentage changes.

However, we provide in Figure 1 (see below) the comparison of annual maximum streamflow for 1 hour and 1 day and of annual minimum streamflow for 1 day and 30 days aggregation time. The results are overall satisfactory. The model is reproducing well the magnitude of annual minimum flow for the different stations, although is not able to reproduce the interannual variability (not shown) which is anyway very limited and strongly controlled by specific dam operations that we cannot simulate. The model is reproducing well the interannual variability of annual maximum streamflow (not shown) but tends to overestimate the peaks of a factor of 1.6-1.7 during the major floods, as we wrote in the original manuscript (P3760 L27-29). This also reflects the fact that specific reservoir operations undertaken to reduce the flood peaks are not simulated in the model as explicitly indicated in the manuscript (P3770 L23-26).

Referee - *Table 1: Honestly I'm very surprised about the very good statistics for all stations. If they are true (sorry, but leave me at least express the doubt ;-)), that's great!*

Response: We want to underline that the good performance in terms of metrics of comparison of the hydrological model is mostly due to having a very accurate gridded precipitation product and having checked and implemented one by one, manually, all the known river diversions for hydropower operations. We hope the reviewer did not intend to doubt or challenge our scientific integrity in his/her inappropriate comment above.

We didn't emphasize this good performance as "great" because indeed it should not been extremely surprising given the thorough setup we implemented. The gridded precipitation product, unavailable until recently, strongly improved all the performances of the model (especially the correct amount of simulated mean discharge) showing how boundary conditions are of paramount importance in process-based model or model with a strong physically-oriented basis as Topkapi-ETH.

The strong seasonality of streamflow contributes to increase several of the metrics used in the comparison (at daily and monthly scale), since it is not difficult to capture higher streamflow in the melting season and lower streamflow in winter. This also highlight how metrics such as R2 or NS for variables containing a strong seasonal cycle should be evaluated but not overrated because they provide partial information only. The detailed inclusion of all the water diversions is what makes the results of Figure 4 in the manuscript (the comparison with "pre-dam" period) possible and this is indeed an extremely valuable and a partially unexpected result given all the involved uncertainties and assumptions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3743, 2013.

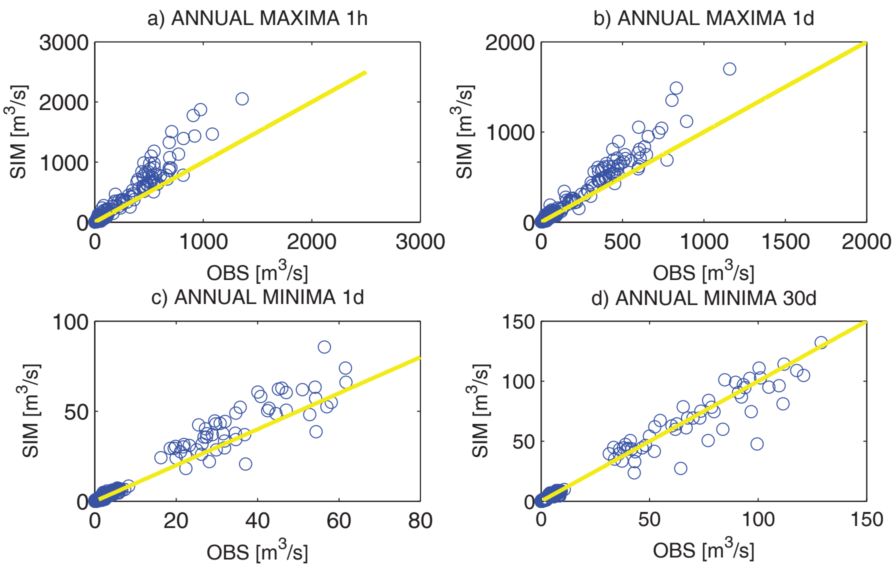

Fig. 1. Observed and simulated annual maxima for 1 hour and 1 day (a, b) and annual minima for 1 day and 30 days (c, d). Results for all years (1990 – 2008) and for the 15 river sections are combined