

Dear Editor Bettina Schaepli,

Please find below our responses to the specific comments of Referee-1. Together with the first response provided in the online discussion this completes the rebuttal letter with the point to point response to all of the referee's comments.

Sincerely,

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

Responses to reviewer 1 specific and stylistic comments

Title: Why is the focus on the “elevational dependence” of climate change? This is certainly not the main finding or the main novelty in the paper. If you really feel “comfortable“ with the claims mentioned in the general comments “4” and “5”, they are definitively better suited for a “headline-making” title. Alternatively I would definitively stress the fact that the study addresses the hydraulic structures in the catchment with an unprecedented detail.

Reply: We respectfully disagree, we think that describing the basin response across the entire river network, from small headwater glacierized catchments to major valleys downstream, and therefore investigating the *differential effect of climate change with elevation and basin size* as measured by streamflow characteristics and other hydrological variables is one of the major finding of the paper (see also online rebuttal to reviewer 2 and 3).

P3744 L22-23: Remove the sentence about biodiversity. This was not addressed whatsoever in the paper!

Reply: Done

P3747 L9-11: Point at the section in which you describe your method or give a hint on what you will be doing.

Reply: Done

P3747 L26-28: The fact that in the future, hydropower dams will be operated according to the exact same scheme observed in the past doesn't seem likely at all! State at this stage that you are aware of this fact (you say it later).

Reply: Done, see also response to editorial review and online rebuttal to reviewer 2 and 3.

P3748 L1-2: Well, this is in contradiction to what you stated one line earlier! What do you mean? Reformulate the sentence.

Reply: The sentence was modified, however, we don't think this contradicts previous statements, changes in streamflow imposed by “climate change” are still interacting with the current infrastructure, the only assumption, which we stated multiple times, is that of invariant operational rules, which is unrealistic for short term operations but much less for seasonal operations.

P3748 L10 (and throughout the manuscript): Call the period 1991-2010 (sometimes you use 1991, sometimes 1990, sometimes even 1992. Be consistent!) “reference period” and not “control scenario period” (why “scenario”? You are using observed data, aren't you?).

Reply: Periods are correct throughout the manuscript, hydrological model was tested in reproducing observations in the period 1990–2008, the factors of change in the stochastic downscaling were derived for the period 1991 through 2010, and control scenario simulations are analyzed in the period 1992–2010, because 1991 was discarded to avoid initial condition problems. Simulations in the period 1992–2010 are referring to a control scenario period because are coming from “stochastically simulated” meteorological inputs and not from observations.

P3749 L25-26: Check your wording – the “monthly correction factor” is probably for distinguishing the annual cycle, and not for distinguishing the land use: : :

Reply: We modified the sentence. The monthly correction factor is used for both distinguishing among different land uses and introducing seasonality.

P3750 L25: How is the “efficiency” of a “diversion” (you never defined “diversion” properly) defined? Give an explanation.

Reply: We removed the concept of efficiency of a diversion. River diversion is a well defined concept, which we think will be known to HESS readers.

P3750 L25-28: But you know the position of the in- and out-flows, you said. So why you don’t assume a constant average flow velocity in this pipes, and calculate a time lag according to the distance? It is certainly a crude estimate, but certainly better than “1h time lag no matter the length”!

Reply: We know the intake and return points of the diversion but we don’t know the exact path and slope and therefore the length of the diversion. Furthermore, many diversions are not simple pipes but composed of different conduits and small basins and some including pumping systems. Giving all the related uncertainty, we really think that additional efforts in determining the travel times which are mostly between less than 1 hour and few hours would not add meaningfully to the accuracy of the analysis.

P3751 L3-7: This description is definitively insufficient! How are these “specified withdrawal functions” defined? In which way are they “based on the number of inhabitants” and how do they “follow pre-defined seasonality and intrannual variability of water consumption”? How much is the “fixed fraction of losses” that is subtracted?

Reply: A more detailed description is now provided in the supplementary material (Text S1).

P3751 L17+23: You never defined a “computational element” (probably you mean “each grid-cell the catchment is discretized in”).

Reply: Computational element is a more general definition with comparison to grid-cell, however, in this case they coincide. This has been specified.

P3751 L18: Why do you mention shortwave incoming solar radiation again? It just sounded like you wouldn’t need it: : :

Reply: Observed values of shortwave radiation were compared with clear sky shortwave radiation simulated using the weather generator, AWE-GEN, (Fatichi et al., 2011), in order to estimate values of cloud transmissivity (which are needed in Topkapi-ETH) for each station and hour of the day with non-zero radiation. This is explained later in the text.

P3751 L21: What is an “elaborated product”?

Reply: It is a product that has been elaborated by Meteo Swiss. This has been better clarified.

P3751 L25: Replace “at the stations” with “at the nearest station” (that’s what you do, right?)

Reply: Done

P3752 L1-2: How often does this happen? Give the number of days. And to which 7-hour period of the day you assign the precipitation?

Reply: This does not happen often and only for very light rainfall intensities. Precipitation is assigned from 5 to 9 and 13 to 14, in an arbitrary way. However, given that the case is rare and that precipitation depths are small, we argue that this assumption does not influence the results.

P3752 L3-6: Remove this sentence. It is not clear what you want to say and the reader is left with more questions than answers.

Reply: We rephrased the sentence and added additional explanations.

P3752 L6-7: Remove this sentence or explain what it is based upon.

Reply: We modified the sentence and combined it with the previous one.

P3752 L7-9: The assumption of a constant temperature lapse rate is certainly not appropriate for the study region you are addressing. Especially in winter and spring, inversions in the valley systems are very common. This may not be a big deal in midwinter, but certainly have an effect on snow melt during spring. Although not a “clean” solution, imposing an annual cycle on the lapse rate may alleviate the problem.

Reply: We agree that inversions are playing an important role which is difficult to account for if lapse rate are not computed internally to the model at the hourly scale but imposed externally. Unfortunately, the structure of Topkapi-ETH did not allow for such a flexibility. We respectfully disagree on the added value of having a seasonally varying lapse-rate, differences between the different months are rather small with typically higher (smaller in absolute value) lapse rates during winter because of the inversion layer. This will have as a major effect leading to underestimation of snowfall at intermediate elevations because of the effect of inversions, which are mostly happening during clear sky nights, and will therefore create an even worse approximation.

P3752 L12-14: A single value for the daily cloud transmissivity for the entire catchment? This is certainly not realistic! Again, this may strongly influence your results concerning snow and ice melt. At least discuss that problem if you cannot come up with some idea for getting a handle on it!

Reply: Our position is that given the overall uncertainty in estimating cloud transmissivity comparing observed shortwave radiation and simulated clear sky radiation, the idea of averaging at the catchment scale such a value can alleviate some problems, especially compensating errors. In other words, we argue that cloud transmissivity from single stations could be considered much less robust, although it could partly preserve spatial variability. We also argue that the “strong influence” in the results has to be proven, but since the “real” cloud transmissivity is not available this cannot be done. We added a sentence clarifying this simplification.

P3752 L20: State where the “additional maps for lakes and glaciers” where “available” from.

Reply: We now introduced in the Supplementary Material a Table for referencing the used products.

P3752 L24-27: Why would the terrain underneath a glacier be different than what is found on its forefield? And what does happen to the soil type when the glacier has retreated? Does it changes?

Reply: We now specified better what happens in case of glacier cell undergoing full melting.

*P3753 L1-14: (A) There is no word on how glacier evolution is accounted for! What scheme is used for updating the glacier surface? As stated in “General comment 3” I believe that you simply downwasted the glaciers non-dynamically. This is certainly not appropriate and severely compromises your results (not in the reference period of course (so don’t argue with the good performance metrics for highly glacierized basins!) but in the future scenarios!). For a simple way of accounting for glacier evolution, check out Huss et al., HESS, 2010. (B) More information is required on how you deal with snow as well. When is the model generating snowfall (according to a temperature threshold I guess)? Is this snow redistributed (no, I guess)? How is the snow melted (according to the “enhanced temperature-index model” I guess)? What do you do with snow that accumulates at high elevations, where no melt occurs according the melt model (no idea)? Are you piling up snow uncontrolled (hopefully not) or is there a solution for preventing that (I very much wonder which solution it is)? (C) A by far less important question: Any thought about permafrost? (D) For the formulation, replace Lines 6-10 with “For each glacier, ice thickness h_{ice} (m) was assumed to be uniform, and calculated from the glacier area A (km^2) as $h_{ice}=33*A^{0.36}$ (Bahr et al, 1997).”. This is more than sufficient.*

Reply: How glacier evolution, permafrost, snow/rain partition, snow redistribution, snow at high elevations are accounted or non-accounted for has now been clarified in the manuscript (Section 2.1). Glaciers are static and simply melt locally, see also the response on the online document “First Response to the reviewer comments of Referee #1”. Snow redistribution and permafrost are not accounted for. We decided for this solution rather than the Huss et al., 2010 parameterization of ice-retreat (which is, however, implemented in Topkapi-ETH) because of the grid size ($250 \times 250 \text{ m}^2$) we used in this application. We believe that, given the size of the computational element, not specifically accounting for ice-flow and snow redistribution are not serious limitations to our analysis (see discussions in Sections 2.2.3 and 4.0). (D) We prefer to keep the full sentence for clarity, as it is very likely that a considerable part of the journal readership might not be very familiar with this type of parameterizations.

P3753 L10-14: Remove these sentences – the way of argumentation is very weak: Although it is true that available estimates of the ice thickness distribution of many glaciers have important uncertainty, they are certainly not as bad that they would justify a replacement with estimates derived by using volume-area scaling and the assumption of a uniform ice thickness. Check out the work by Gabbi et al., HESS, 2012 for how future runoff projections can be biased when assuming “uniform glacier thickness”.

Reply: We removed the sentence.

P3753 L20-21: Why do you need to introduce the concept of “efficiency” (if you think that this is really necessary, explain what it is!), if you set it then to a value which corresponds to neglecting it? I suggest removing the sentence.

Reply: We removed the sentence and efficiency concept since indeed it is not used in this study.

P3753 L23-24: This is very confusing since it contradicts what you stated at L19-20. Where is the “capacity” (by the way, what is your exact definition of it?) coming from at the end?

Reply: We now better specified that former L19-20 was referring to capacities of diversion from natural rivers and former L23-24 to capacities of conduits from the dams to the hydropower plants and pumping pipes.

P3753 L25-27: What do you mean? Have you only checked if the coordinates you had were consistent with the position of your river network? In this case you did not “introduced” the “diversions/conduits”, right? Or did you added the “diversions/conduits” manually one by one?

Reply: We added all the diversion/conduits one by one manually (as wrote in the text) to check and be sure of their correct positioning in the river network implemented in the model, which does not perfectly overlap with the real network, for obvious reasons related to the automatic extraction of the river network from the DTM.

P3755 L9-13: I think the method is smart, because of its simplicity. However, show a plot for the 7 volume-level curves that are available, so that the reader can have an idea on how “similar” these curves are actually (and display quantiles of the curves, as you did in most of your figures). Moreover, the non-adequacy of the fundamental assumption (i.e. same “target levels for the future”) should be stressed more. Of course it would be difficult to make a better job by maintaining the same degree of simplicity, but the assumption of unaltered hydropower operations is certainly not realistic.

Reply: We now discussed in Section 4.0 explicitly the assumption for target levels in the future, see also response to the editorial review. We don't think it is necessary to show volume-level curves for all the reservoirs which are available. Since volume-level curves are part of the geometry of reservoirs and we think this is out of the scope of the article.

P3755 L21-22: Does this number (i.e. “0.7 mm h⁻¹”) applies to all “districts” (define what a “district” is)? Didn't you say that you accounted for the “highest monthly withdrawn discharge” (P3753 L19-20)?

Reply: The number applies to all the districts. Districts are defined arbitrarily on the basis of geographical proximity. The statements of former P3753, L19-20, refers to diversion for hydropower not for irrigation, which are not known, this section is now moved to the supplementary material.

P3755 L27: Where is the number “160 x 10⁶ m³ yr⁻¹” coming from? Is it a model result?

Reply: Yes it is. This has been specified.

*P3756 L1-2: I don't understand the logics of this argument: Why don't you choose a different value for the “maximum capacity of the water supply infrastructure was” (P3755 L21) in order to match the two numbers? I would imagine that “0.7 mm h⁻¹ * (1-0.6)” would yield a better agreement if you overestimate the amount by 60% (P3755 L27).*

Reply: We agree with the reviewer but we became aware of the true estimate only after having performed all of the simulations and elaborated the results. While re-running the “historical case” would have been very easy we could not change the set-up for re-running all the 60x3 future simulations, due to the high computational time. We believe that this approximation, as argued further above, does not change the overall message of the paper.

P3756 L11-13: Well, probably exactly the opposite applies!

Reply: This will be arguable since different cities or local communities have a more or less complicated system of water supply composed of wells, withdrawal from rivers and/or springs and from reservoirs. However, given the lack of knowledge of the specific withdrawal point and

quantities, every assumption will remain arbitrary. The critical issue is thus to mimic a realistic amount of water withdrawal from a water body, which is not too far away from the location of the community, especially for mountain villages.

P3756 L15-16: More information is required. What are these “daily and seasonal fluctuations” that are “imposed using modulation coefficients”? What are “modulation coefficients”? Consider showing a plot for these “modulations”.

Reply: All these concepts are well known in technical literature on hydraulic structures and water supply systems (see, e.g., Milano, 1996, or other general books for design of water supply systems).

P3756 L 17-20: Here, and already at L3-4, the questions arises why in the introduction you stressed so much the importance of your study in the context of water management. Here you are basically saying that water is available almost unlimitedly: : :

Reply: As we wrote in our first response, we agree that the quantity of 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 because of hydropower production, which is conversely very important and highly impacting. Since we provided relatively detailed quantifications of all these uses at a fairly large scale, we thought it was important to give them space in the introduction, which is anyhow rather limited.

P3756 L 21-28: This sounds like a contradiction again: You just stated that “water used for irrigation” (L3-4) and “domestic/industrial water consumption” (L17-20) are in the order of a few % only, and now you argue that automatic calibration wouldn’t be suitable because of “strong anthropogenic disturbances”. Where is the problem?

Reply: Once more we would like to stress that, the strong anthropogenic disturbances are an effect of river diversions and reservoirs used for hydropower production and not of water resource management for domestic water consumption and irrigation, which are both indeed very small if compared to the impact hydropower related management.

P3757 L1-14: This line of argumentation is incredibly weak, and sounds, at this stage, like a unnecessarily complex formulated excuse: It wouldn’t be hard defining a physically motivated range in which parameters are allowed to vary once a procedure for automatic calibration is set up, would it?

Reply: As we wrote in the first response, 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 lack of observations. Furthermore, an automatic calibration will not 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 specific purposes such as “flood forecasting”, we are convinced that it is scientifically sounder 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. See also response to editorial summary.

P3757 L20-23: Although the two methods certainly doesn't need to be described in detail, some more information is definitively required in order to have an idea what these methods are! What is the particularity of this "spatiotemporal Neyman-Scott Rectangular Pulses model"? Why "spatiotemporal"? Why "rectangular pulses"? Where can the "RAINSIM package" be found? What is it thought for? And why is the "Markovian model" "multivariate"? Which "variates" does it includes? Why would it be better than other approaches?

Reply: We now included in the text additional explanations and references to the multi-site Neyman-Scott Rectangular Pulses used to simulate precipitation and to the multivariate autoregressive (MAR) model used to simulate air temperature. We also add explanation on the supplementary material. We used consistently multi-site instead of spatio-temporal. Rectangular pulses comes from the way raincell are simulated in time. The reference to RAINSIM package with all the explanations about the model is in Burton et al., (2008). We clarified better what a Multivariate AutoRegressive (MAR) model is and we now provide references. The model is "Multivariate" because it accounts for multiple stations and the air temperature correlation among them. We think that the references we provided are more than sufficient to clarify all the doubts about the methodology and will satisfy any interested reader.

P3757 L26: Remove the two citations: The "delta change approach" is certainly nothing introduced by Fatichi et al. or Anandhi et al.!

Reply: We remove the reference to Fatichi et al., 2011 but not to Anandhi et al., 2011 who gave an interesting overview of change factor methodologies for climate change impact assessment which is more than the "delta change approach".

P3758 L5-7: To my understanding one of the main ideas behind the "delta change" approach is to be bias-independent (because only changes are considered). Why would, thus, a bias correction be of any benefit? And give some hint on how the chosen bias-correction method works.

Reply: It is true that the factors of change are typically applied to remove the bias present in climate models, however there are literature arguments (e.g., Bordoy and Burlando, 2013a) that suggest that since the bias of the climate models is very high also the "factors of change" might be biased because changes are influenced by the underlining climate. Applying a bias correction procedure before estimating the factors of change is thought to alleviate this problem. Please note that if the bias correction is "linear" there is no difference between the factors of change computed before or after the de-biasing. However, a non-linear de-biasing can change, even significantly the computed factors of change. The bias-correction method is fully explained in Bordoy and Burlando, (2013a), we do not see the reason to explain details in this manuscript again.

P3758 L9: Where is the "non-linearity" coming from?

Reply: Non linearity is coming from a non-linear function aX^b used in the correction (see Leander and Buishand, 2007 and Bordoy and Burlando, 2013a for details).

P3758 L9-14: What parameters are you talking about? This is impossible to understand! And how can there be a "validation period" if you are addressing "climate PROJECTIONS"??

Reply: We referred to the parameters of the bias correction. This has been now clarified. The periods for which data were available was divided in two sub-periods (time-splitting procedure), the debiasing parameters where estimated using data for a single period and validated in the other

period. The two periods showed different “air temperature” conditions. Validation is realized for the period not used for calibration. See Bordoy and Burlando (2013a) for details.

P3758 L15: Which ones are these “several [: :] statistics”?

Reply: We now explicitly stated which are the statistics.

P3758 L16-17: HOW did you “re-parametrized” the “multisite Neyman-Scott Rectangular Pulses model”? Why “re-“? And why is the model now “multisite”, and no longer “spatiotemporal” (P3757 L21). This is probably the best example for the sometimes very annoying wording :-)

Reply: The description of how the model is re-parameterized is provided in Bordoy (2013) and Bordoy and Burlando (2013b), which the reader is referred to. We provide two references, since the second paper is still in review. However, additional details have been included both in the text and in the supplementary material. We use the term “re-parameterized” because the model is first parameterized for the observational period and then “re-parameterized” for the future decades following the same procedure as in the observational period, but using climate statistics (e.g., mean, standard deviation) modified using the factors of change. We substitute everywhere “spatiotemporal” with the more correct definition “multisite”.

P3758 L20-23: And what’s about the step between decades, if individual decades are considered to be stationary on its own?

Reply: There are “climate steps” between the four different considered decades that are intrinsic to the methodology, which simulates temperature and precipitation that are outcomes of a stationary process in each decade, but representative of four different climates for each of the four decades, thus being decade-stationary. Since the results are analyzed comparing the different decades this does not represent, in general, a significant issue. Moreover, note that the “steps” are not appreciable in a single member of the stochastic ensemble because stochastic variability is larger than any small step due to a change in temperature and precipitation climatology.

P3758 L29: What “statistical properties”?

Reply: We changed “statistical properties” with “statistical distribution”.

P3759 L1: What quantity was randomly sampled? Not the “statistical property”, right? That is, however, the subject of your sentence: : :

Reply: We explicitly wrote the subject, i.e., “cloud transmissivity”.

P3759 L5-8: This is not understandable. (By the way, what are “climate generated data”?)

Reply: We modified and re-phrased the sentence.

P3759 L11-13: Does it mean that there are regions in which the precipitation for each is given by the data of one particular station only? If yes, say that clearer before and show these regions.

Reply: This part has been re-written to better clarify its content. Gridded precipitation data are available only for the observational period. Therefore, in the control scenario and in the future, we have only simulations in multiple points (multi-site) corresponding with the location of the raingauge. However, a full spatial distribution of precipitation is maintained using a correction factor (seasonally dependent) which was applied to each grid cell. The correction factor represents the

ratio between the climatological precipitation in a given cell and the climatological precipitation in the cell containing the station that is used as precipitation forcing for that given cell.

P3759 L24-26: And what's about data about glacier retreat and mass balance? The region you are looking at has probably the best coverage worldwide on this respect! Check out the work of the different universities working in your Country and include those data in the validation! Again, if one of the major conclusion is that components of the cryosphere play a decisive role, more attention should be given in validating the results of this compartment!

Reply: See our response about glacier dynamics and retreat in the online document “First Response to the reviewer comments of Referee #1” and see also Finger et al., 2011, for a comparison between Topkapi simulations and glacier mass budget.

P3760 L18-21: Honestly, I'm very surprised about the so good performance of the model, especially at the shorter aggregation times and the regulated catchments: : :

Reply: See our response in the online “First Response to the reviewer comments of Referee #1”.

P3760 L25-27: Only the amplitude? And only “similar”? Please give the same metrics of performance as you gave above.

Reply: The sentence is used to say that, while we cannot simulate the exact effect of hydropower operations for a given day and hour, we are still able to simulate a behavior consistent with the expected oscillation induced by their operations. We think that a more precise and thorough comparison (which is also difficult to define, in terms of analyzed periods and metrics) would not add anything to the manuscript.

P3761 L15-17: Although the argument is valid to some degree, you certainly cannot “get away” without considering any spatially distributed validation. At least aggregate your averaged values into elevation bands, otherwise you are throwing away most of the information!

Reply: We now presented in Figure 5 comparison for different elevation bands, which accounts for spatial distributed information.

P3761 L20: Quantify the “small delay” (e.g. how many days?).

Reply: This is typically less than one week, this has been now clarified.

P3761 L22-24: Where is this result coming from? Didn't you just say you compared average values only?

Reply: This part has been re-written to describe the new Figure 5.

P3763 L2-3: This sentence leads me to the conclusion that the section is not described well: I was under the impression that you were comparing measured and simulated data both referring to the “pre-dam” period. Now, however, it sounds like you would compare “pre-dam” measurements with “post-dam” simulations in which the hydraulic infrastructure has been removed in the model. This latter option doesn't makes much sense to me: :

Reply: The observations in the “pre-dam” period are compared with simulations without hydraulic infrastructure in the period 1990-2008. This is why they are compared in terms of seasonality only. This makes totally sense to us, at least to catch the effect of the major disturbances. Such comparison is the only possible given the fact that the necessary meteorological forcing to run the

model (hourly time series, gridded precipitation data), were not available for the “pre-dam” period. This is now better clarified in the text.

P3763 L8: Why “stochastic variability”? Which other “variability” did you considered, and how do you distinguish between them?

Reply: Stochastic variability simply refers to climate internal variability. This is the reason why we do not have every year the same representation of precipitation and air temperature even in absence of climate change.

P3763 L9: What are the numbers in parenthesis? Why is there a range? To what is the range referring to?

Reply: The number in parenthesis is an approximate range of values for the standard deviation of precipitation in a 10 year period. This standard deviation is different for different decades and driving climate models. The range is used to give an order of magnitude to the reader since a precise value would not add more information.

P3763 L16: (A) Add an explanation starting with “which causes: : :” after “(Table 2)”. The increase in temperature alone does not explains a decrease in discharge, does it? (B) Why is the “spin-up time” mentioned only at this stage? This should happen earlier.

Reply: The decrease in discharge is due to the reduction in ice-melt. Explanation are widely provided in the text that follows and that we think does not need to be anticipated here. However, we slightly modified the sentence. Specifically, we modified the sentence about spin-up, since it’s not a proper spin-up but just the fact that we didn’t use the first year of simulations to avoid initializations problems.

P3763 L20-24: Not sure, but is this something present in the original RCM runs or something introduced by your downscaling method?

Reply: This is a combination of the original climate change signal in RCM realizations, the non-linear de-biasing procedure and the stochastic downscaling methodology.

P3764 L13-16. Well, these results (and in particular the unrealistic 65% decrease in glacier melt within one decade stated in Table 5!) can easily be explained with the crude way glaciers are represented in the model.

Reply: We think that these results are likely caused by the ice thickness initialization and by the spatial resolution used as we now widely discuss in the “Discussion” section 4 of the paper (see also our reply in the online document “First Response to the reviewer comments of Referee #1”).

P3765 L9: You never said that the model projection show an increase in winter precipitation! And Figure 7 doesn’t show it either – so, don’t point at it.

Reply: We have introduced and explicit mention of the fact that there is a slight increase in February to April precipitation in the overall ensemble of simulations, especially for stochastic downscaling driven by RegCM3 and REMO.

P3765 L14-15: What do you mean with “unchanged”? Only in total, right? Not in the timing: : :

Reply: We now wrote explicitly “total snow melt”.

P3765 L23: Give an example for gauging stations that are not affected by glaciers.

Reply: Done.

P3766 L6: Quantify how much the “larger increase” is. And how good is the model in estimating evapotranspiration there? (By the way, how did you validated the results for the evapotranspiration at high altitudes?)

Reply: Evapotranspiration is simulated to roughly double for high elevation catchments (+30÷60 mm/yr) in comparison to catchments at lower elevation (+5÷30 mm/yr). However, since the evapotranspiration component is the least “physically-based” process in Topkapi-ETH, these estimates remain very uncertain, and we prefer to report just the differential increase. Moreover, we added a paragraph in the discussion (Section 4) including references where we illustrated the problem of computing evapotranspiration with a simplified method, as we did.

P3766 L 18-19: Explain what you mean with “both emphasize and reduce natural climate change effects” (by the way, why “natural”?).

Reply: We removed the word “natural”. River diversions tends to both emphasize and reduce climate change effects which would occur in absence of hydraulic infrastructure depending on the direction of change and on the ratio between natural flow and diverted flow. This has been now clarified.

P3766 L21-28: Honestly, how much do you believe in the upper quantiles of your modeling results? If you interpret those results, you definitively need to present a plausible validation of the model performance for the according statistics first!

Reply: We now included a comparison of simulated extremes in the observational period (see also our reply in the online document “First Response to the reviewer comments of Referee #1”). It is undoubtedly true that these results are very uncertain and we widely acknowledge this in the text (Section 4). However, we are also confident that since they are obtained in an ensemble-mode that accounts for stochastic variability they have some value, which justifies their inclusion in the framework of this paper.

P3767 L5-6: Do you really want to make this claim?

Reply: We now modified the sentence to underline the involved uncertainty.

P3768 L2-5: Does this mean that your introduction was “oversold”?

Reply: We do not think so. See our response in the online “First Response to the reviewer comments of Referee #1”.

P3769 L17-19: Another weak argument: : : The fact that the interannual variability during your control period was reproduced well may indeed be a hint that you had the correct glacierized area. However, it certainly doesn’t allow you to draw any conclusion about the appropriateness of your ice thickness distribution: You could have placed 10km of ice everywhere, and your model wouldn’t produce much additional melt water, since the glacier area is given, and hardly changed during that short period. Remove the sentence (and use something more realistic for your glacier modeling: : :).

Reply: We removed the sentence, we agree with the referee.

P3771 L3-4: Why “lower hydraulic head”? Water intakes have usually a fixed height, and the difference in water pressure should be negligible: : :

Reply: While it is true that for these alpine basins the percentage of hydraulic head given by the water in the reservoir is small in comparison to the total hydraulic head (being the hydropower plants located at significantly lower elevations), sometime having a 50 m lower head is not so negligible. However, we eliminated the statement to simplify the reading.

P3771 L12-13: This sentence is too general. The studies you mentioned earlier focus mostly on headwater catchments, and certainly don’t do claims for the “entire Alpine Areas”.

Reply: We modified the sentence.

P3771 L21-24: Remove this sentence: Why would rivers at lower elevation not “support biodiversity of aquatic environments” (and by the way, what’s the definition of “biodiversity of aquatic environment”)?

Reply: We respectfully disagree, we think the hint to problems related to biodiversity of high-elevation stream is very important, see the reference quoted in the paper.

P3772 L16-18: I’m repeating myself, but if that is your conclusion, shouldn’t you focus more on the according processes?

Reply: See our response in the online “First Response to the reviewer comments of Referee #1”.

— *STYLISTIC COMMENTS* —

P3744 L2-3: Remove this sentence – it is too general.

Reply: We removed the sentence.

P3744 L5: A) At this stage it is unclear what “pristine” means. Avoid the wording. B) “choosed” or “addressed” instead of “used”

Reply: We prefer to leave the word pristine. We change “used” with “chose”.

P3744 L17: “reduction” of what?

Reply: Streamflow. This has been clarified.

P3744 L25: Add “and” between “hourly” and “daily”?

Reply: Done

P3745 L3: Add “and” after “services,”.

Reply: Done

P3745 L7: “Swiss Alps” instead of “Alpine region of Switzerland”.

Reply: Done

P3745 L9: Add “Europe” after “central”.

Reply: Done

P3745 L10: Begin a new sentence with “Moreover: : :” since “which [: : :] their” is difficult to understand (what is “their” referring to?).

Reply: We now clarified the sentence.

P3745 L13: “authorities”, not “managers”.

Reply: Done

P3745 L17: Add “here” after “The”.

Reply: Done

P3745 L19: Remove “large” after “European”

Reply: Done

P3745 L21: Replace “)(“ with “,”.

Reply: Done

P3745 L22: “case” instead of “ground”

Reply: Done

P3745 L28: Add “the basin” before “particularly”. P3745 L29: Remove “it”.

Reply: Done

P3746 L6: Add “and” after “on”.

Reply: Why?

P3746 L8: “an increase in” instead of “enhancing of”

Reply: Done

P3746 L9: “These changes” instead of “Therefore, changes”

Reply: Done

P3746 L12-14: “Even” what? Begin the sentence with “This is true even for: : :”

Reply: Done

P3746 L18: “fill this information gap” (or similar) instead of “provide an answer to such a request”.

Reply: Done

P3746 L20: Remove “at high elevation” and “at low elevation” – they are both not

necessary.

Reply: Done

P3746 L21: “particularly” instead of “specifically”.

Reply: Done

P3746 L24: Add “,” after “catchments”.

Reply: Done

P3747 L8: Move “for an exception” after “2011”.

Reply: This part of the text has been modified.

P3747 L10: Remove “engineering” (what is the added value in terms of clarity of this word?)

Reply: We prefer to keep the term since it explicitly refer to the problem of giving solutions in conditions of scarce data availability, at least in hydrologic engineering.

P3747 L24: Remove “in detail” – it is in contradiction to what you state at L9-11.

Reply: Done

P3748 L10: Add “to be” before “stationary”.

Reply: Done

P3748 L21-23: Replace “the underestimation of uncertainty induced by neglecting realizations from additional climate models” with “the problem”.

Reply: We modified the sentence.

P3748 L24: “due to” or “introduced by” instead of “imposed by”.

Reply: Done

P3749 L8 (and throughout the manuscript): Check your use of “significantly”, and use it only where appropriate. How did you tested the “significance” of the “enhancement”? (The question is ironic)

Reply: Done

P3749 L20: “calculated based on” or “modulated by” instead of “mediated by”.

Reply: Done

P3749 L21-22: Remove the sentence – it is too general.

Reply: Done

P3750 L1: Replace “an” with “the” or remove “enhanced” (there are not many “enhanced temperature-index models” in the literature: : :)

Reply: Done

P3750 L5: Add “,” before “and”.

Reply: Done

P3750 L16: Don't begin a new paragraph.

Reply: Done

P3750 L18: Remove “significantly”

Reply: Done

P3750 L19-20: Remove “and that are essential for providing simulations in the regions of major interest for the society. Specifically” - the information content of the statement is negligible.

Reply: Done

P3750 L20-24: Replace these lines with “Lakes and reservoirs are described using all of the major technical information (e.g. spillway, turbine and outlet capacity, volume-level curves, maximum and minimum regulation levels, environmental flows), and reservoirs are simulated by using a “target-level rule” (Sect. 2.2.)”.

Reply: We prefer to keep the original sentence.

P3751 L9: “part” (or something similar), not “corner”.

Reply: Done

P3751 L10-11: Check your wording! The upper Rhone basin (which is subject of your sentence) certainly doesn't “flows” anywhere: : :

Reply: Changed.

P3751 L16: Add “and” after “precipitation,”

Reply: Done

P3751 L24: “downscaled” instead of “disaggregated”

Reply: Disaggregation is more appropriate than downscaling here, which otherwise can confuse the reader mixing it with “stochastic downscaling”.

P3752 L17: Remove “resolution”.

Reply: Done

P3752 L18: “grid cells” instead of “computational elements” (the information about size should be given earlier!)

Reply: We think computational elements is more correct than grid cells, although equivalent and that this is the correct place to give information about the size.

P3752 L21: What is “Rhone” in this case? The catchment? The river? The glacier?

Reply: Basin, this has been clarified.

P3754 L5-7: Replace “complex reality and it represents our best effort to consider anthropogenic alterations of the natural discharge of the upper Rhone river basin given the limited availability of public data.” with “reality, which is difficult to describe better given the limited availability of public data.”.

Reply: We re-wrote the sentence to account for some of the reviewer suggestions.

P3754 L8: Add “,” after “diversions”.

Reply: Done

P3754 L11: Add “hydropower” after “largest”

Reply: Done

P3754 L11-14: Reword into “One of the reservoir, Arnensee, is not physically located in the Rhone catchment but was included in the simulations since water from the Rhone tributaries is pumped back and forth from it”.

Reply: Done

P3754 L15: Reword “that represents 20% of the total annual upper Rhone flow” with “corresponding to 20% of the annual discharge of the considered basin”.

Reply: Done

P3754 L16: Remove “fundamental”

Reply: Done

P3754 L20-25: Avoid the wording “volume-level curves” 4 times in 6 lines.

Reply: We re-phrased this part.

P3754 L21: (A) Name the 7 reservoirs instead of only saying “including Mattmarksee: : :”. (B) What do you mean with “broader Alpine region”? That they are not inside the catchment??

Reply: Yes, 5 reservoirs were not located in the catchment but in the nearby catchment of Valle d’Aosta, we don’t think it is necessary to mention their names. We clarified better this point.

P3754 L22-29: Reword into “These curves were normalized using the maximum and minimum regulation levels, and averaged to obtain a reference volume-level curve. Application to other reservoirs implicitly assumes similar bathymetry. In the model, dams were operated according to a “target-level policy”. This means that for each reservoir and each day of the year, a target level was assigned (EXPLAIN HOW!), and the water released in the case the simulated reservoir level exceeded the target level. Water release rate was chosen to be lower or equal (WHEN IS IT “LOWER”? WHEN “EQUAL”?) to the [: : :]”

Reply: We re-phrased this part.

P3755 L2: What is the “environmental flow”? Probably you mean the flow outside the catchment of the reservoir?

Reply: Environmental flow is a well known concept in hydrology, it does not need any explanation, see e.g. http://en.wikipedia.org/wiki/Environmental_flow

P3755 L5-7: Remove this sentence. You said that before.

Reply: We did not mention it before. This explains how we calculated the target levels, not the volume-level curves. The reviewer is actually asking to explain this in his comment P3754 L22-29, ignoring that an explanation was provided few lines later.

P3755 L19: (A) Remove “of the actual irrigated area”. (B) “14” instead of “fourteen”. (C) What are “irrigation districts”?

Reply: We do not think that removing “actual irrigated area” will help in understanding the text. We decided to keep “fourteen”, as it is normal to write number in letters in the text body. Irrigation districts are arbitrary selected irrigation areas with a geographical proximity (see also our reply further above). This has been clarified and moved to Supp. Material.

P3755 L23: Remove “ in reality rather”

Reply: Done

P3755 L24: Remove “called Suonen”

Reply: No.

P3755 L25: Move “,” after “model”

Reply: Done

P3756 L3: “corresponds to” instead of “is indeed less”

Reply: Modified.

*P3756 L6 (and elsewhere): be consistent with the given units! (Here “0.3 m³ (person * day)⁻¹” instead of “300 L (person*day)⁻¹”)*

Reply: It is common practice in water supply literature to define the water allocation in [l /person day]. We prefer to keep the units as conventionally used in water engineering.

P3756 L7: “use”, not “uses”

Reply: Done

P 3756 L10: “apart” instead of “distant”

Reply: Done

P 3756 L11: “.” After “identified”.

Reply: Done

P3757 L18-20: (A) Replace “. The downscaling methodology” with “, which”. (B) Add “to be” after “assumed”.

Reply: Done

P3759 L5: Replace “multisite Neyman-Scott Rectangular Pulses and the multivariate Markovian” with “according”.

Reply: We preferred to keep the full text for clarity.

P3759 L9-10: Did you say that earlier? Or why “since”?

Reply: We removed since.

P3759 L17 (and may cases after this point): Do you mean “validation” or something similar instead of “confirmation”?

Reply: We decided to use the word “confirmation” rather than “validation” for a series of reasons that are well explained in Oreskes et al., (1994) *Science*, 263(5147), 641–646, doi:10.1126/science.263.5147.641

P3759 L20: “from” instead of “as elaboration of”

Reply: Done

P3760 L3: “Merics such as” instead of “Validation metrics in terms of”.

Reply: Changed.

P3760 L17: “dramatically”? Change the wording.

Reply: Changed.

P3761 L5-6: Reword “we were able to reproduce well the discharge interannual variability for a 18 yr period” into “ the interannual variability of discharge is well represented”.

Reply: Done

P3761 L7-12: Reword into “The simulated and observed mean discharge are in good agreement for all the examined stations (Table 1), further supporting the plausibility of the hydrological simulations, since it is very unlikely that the streamflow in 15 different stations has been simulated correctly “by chance”.”

Reply: We modified the sentence but not exactly as suggested by the reviewer.

P3761 L25: “Validated”, not “tested”.

Reply: We preferred to keep “tested”.

P3761 L26: Replace “multi-annual estimates of these quantities” with “consistent estimates”.

Reply: Done

P3762 L4-5: Remove the sentence. Begin the section with “For six of the considered stations, observations of discharge: : :”.

Reply: We preferred to keep this introductory sentence.

P3762 L15: Remove “significant”

Reply: Done

P3762 L27: “. This is most probably due to: : :”

Reply: Done

P3763 L11-13: Rephrase to “Averaged over the catchment, the simulations directly driven by the GCM ECHAM5 predicte a decrease of about 100mm yr⁻¹”

Reply: Done

P3764 L5: Remove “to drive the stochastic downscaling”

Reply: We preferred to keep to it in order to be very clear.

P3764 L11-13: Rephrase to “Reduction in ice melt has a particular influence on the runoff of August and September (Fig. 5). Glacierized area and volume are : : :”

Reply: Done

P3765 L4: Remove “downscaling”

Reply: We preferred to keep the word in order to be very clear.

P3765 L10: Remove “season”

Reply: Done

P3765 L14: “pronounced” instead of “remarkable”

Reply: Done

P3766 L9: Reword to “, which has the lowest elevation”.

Reply: Done

P3766 L11: “annual discharge”, not “discharge mm yr⁻¹”.

Reply: Done

P3766 L12: Remove “for the stochastic downscaling driven by RegCM3”. Why would this be true only for this particular RCM?

Reply: We wrote “shown for the stochastic downscaling driven by RegCM3” because we indeed showed in the former Figure 10a only for this specific driving climate model.

P3766 L14: What are “river reaches”?

Reply: River reach is a well known concept in geosciences, it does not need any explanation (see e.g. [http://en.wikipedia.org/wiki/Reach_\(geography\)](http://en.wikipedia.org/wiki/Reach_(geography)))

P3767 L12: Remove “Rhone”.

Reply: Done

P3767 L28: “differences” between what and what?

Reply: We meant between water levels. This has been now clarified.

P3768 L1-2: Remove the first sentence. Start with “In all simulations, irrigation and water consumption: : :”

Reply: Modified.

P3768 L7: Reword into “We provided for the first time simulations including present day: : :”

Reply: Done

P3768 L24-26: Check your wording! This is not a property of the catchments! It’s a property of your model!

Reply: We re-phrased the sentence.

P3769 L9-10: Add “used” after “of the”, and remove “driving the stochastic downscaling” (that’s clear from the context).

Reply: Done

P3769 L11-12: (A) “ice melt”, not “ice melted water”, (B) remove “downscaled” and “driven by the different climate models” (again, that’s clear from the context).

Reply: We corrected accordingly, except for downscaled, which we prefer to keep for clarity.

P3769 L12-14: Remove or reformulate this sentence. At the moment it is not understandable.

Reply: We re-formulated the sentence.

P3769 L15: Remove “the entire upper Rhone basin” (clear from the context).

Reply: We modified the sentence.

P3769 L16: Remove “probably” – that’s pretty obvious in my opinion: : :

Reply: We substitute “probably” with “very likely”. There is not counter-proof that a perfect initialization of ice thickness would lead to a dramatically different result.

P3769 L26: Remove “rather”.

Reply: Done

P3770 L6: Remove “fed by glacier sources”.

Reply: We preferred to keep to it

P3770 L7: Remove “at lower elevations”.

Reply: We preferred to keep to it

P3770 L11: Remove “out of the interannual variability”.

Reply: We preferred to keep to it

P3770 L12: “less important” than what?

Reply: Less important than changes in precipitation and ice melt. This has been now clarified.

P3770 L15-16: Remove “to drive the downscaling”.

Reply: We preferred to keep the original text, since the signal of the GCM/RCM or the signal of the stochastic downscaling driven by a specific GCM/RCM can be rather different.

P3770 L17-19: Reformulate the sentence (you never introduced the concept of “stochastic trajectory”).

Reply: We modified the sentence.

P3770 L22: What is an “autumn storm”? You never talked about.

Reply: It is a storm occurring in Autumn, there is no need of dedicating to it a specific introduction.

P3770 L27: “affected” by what?

Reply: by climate change. This has been now modified.

— COMMENTS TO FIGURES AND TABLES —

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!

Reply: See our response in the online document “First Response to the reviewer comments of Referee #1”.

Table 2: (A) Where is the temperature drop for the decade 2021-2030 coming from? I’ve never seen something like that in other climate projections, whilst you show it as consistent feature throughout the three models you consider. This requires at least a short discussion. (B) A reduction in ice melt of 65% within one decade?! That’s neither realistic nor nearly in line with other published estimates! My guess is that since you are melting glacier ice “in place”, your lower elevations run out of ice very quickly, basically stopping the computed ice melt. The only way for avoiding this is taking into account in some way the ice dynamics. Have a chat with your colleagues at the glaciological institute, they certainly can give you some advice.

Reply: The temperature drop is coming from the signal in the GCM. Being the used RCMs all driven by ECHAM5 is not surprising that this signal is consistent among the three scenarios. The discussion of such a behavior belongs more to the climate community, especially because it does not influence too much our results. We discussed in the manuscript that the abrupt transition in ice melting can

be an issue of the initialization of glacier thickness, as well as of the used spatial resolution and we now discussed extensively this issue in Section 4.

Fig. 2: (A) Remove the kilometer-grid around your catchment (as you did in Fig.1) or (better) use the CH1908 coordinate system. (B) In the caption, replace with an identifying number” with “with the identifying numbers given in Table 1”.

Reply: We preferred to keep the figure in its actual format, because we think it is very easy to read anyhow. We modified the caption.

*Fig. 3: (A) Additionally to panel “(a)”, show a scatterplot of simulated against observed snow cover. (B) In panel “(c)”, show the difference with respect to the observed values! At the moment, it is very difficult to see any difference between “(b)” and “(c)”. And that’s not because to model is so good ;-)
Moreover, create a plot in which you show the mean difference versus average altitude (build a running window over a given altitude band, or something similar), and show let say four lines for the four different season (or similar; for catching some of the temporal variability). (C) Remove the first sentence of the caption (it refers only to panel “(b)” and “(c)”.*

Reply: We completely modified former Figure 3 (now Figure 5) and we present a comparison for different elevation bands, as well as a map of differences. We also removed the first sentence from the caption.

Fig. 4: I’m not completely sure what you mean with the last sentence in the caption. Check the meaning, but probably, adding “simulated” after “and 15” would help to understand.

Reply: The sentence has been modified.

Fig. 5: (A) I cannot see any results that refer to “REMO”. Change the order of the models. (B) Why is ice melt starting later in the period 2041-2050 (around DoY=150) than it is in the reference period (around DoY=125)? Wouldn’t one expect the opposite (since snow is melting earlier)? This needs to be discussed!

Reply: The ensemble plotted by REMO is giving the “greenish” color because the yellow overlaps with blues. Ice melt starts later because the “low elevation” glaciers are totally melted by the 2041-2050 in our simulations.

Fig. 6: Define what your box-plots are showing (100%, 97.5%, 75%, 25%, 2.5%, 0% quantiles and mean I guess?). Why are the “+” missing for some box-blots?

Reply: On each box, the central mark is the median, the edges of the box are the 25th and 75th percentiles, the whiskers extend to the most extreme data points not considered outliers, outliers are plotted individually (+). This has been clarified in the figure captions.

Fig. 7: (A) In the caption, remove “river sections corresponding to the location of the”. (B) In the plots, add a number giving the glacierization (e.g. as a percentage) of the according catchment.

Reply: We modified the caption and we included in the supplementary material a table (Table S2) to describe the characteristics (including glacierized area) of the 15 catchments of Table 1.

Fig. 8: (A) Caption, line 2: “between”, not “among”; remove “scenario”. (B) Caption, line 4: Remove all “subplot”.

Reply: We modified the caption.

Fig. 9: (A) This plot is messy. Use the same box-plot style as you did in Fig. 6. (B) In the caption, remove al “subfigure”.

Reply: We do not think the plot is messy. It actually conveys the information in a rather compact way.

Fig. 10: Increase the line thickness of the individual river branches – it is difficult to discern between individual colors: : :

Reply: We modified the figures to increase the thickness of the different river branches.

Fig. 11: (A) This plot is messy again. Increase the horizontal distance between the individual box-plots. Use the same style as in Fig. 6. (B) In the caption, replace “difference for the stochastic downscaling driven by RegCM3” with “variations”, and add “ Results refer to the RegCM3-driven model run.” before “The differences are [: : :]” (an “s” is missing after “difference”).

Reply: We don't think the plot is messy. We actually think it conveys the information in a rather compact way. The reader is not interested in the single catchment as in former Figure 6 (Now Figure 8) but in the global vision. We modified the caption as suggested.