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Hydrology and Earth System Sciences Discussions

Interactive comment on "The importance of glacier and forest change in hydrological climate-impact studies" *by* N. Köplin et al.

M. Zappa (Referee)

massimiliano.zappa@wsl.ch

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General remarks:

In the last year, this is the third paper addressing the topic of shrinking glaciers, hydrology and climate change I review, but the first one exploring other directions and trying to assess combined sensitivities of glacier extent scenarios and ad-hoc forest change scenarios in Alpine environment. In a first evaluation one might criticize that no information is given on the calibration, verification and uncertainty assessment of the hydrological model adopted. This is true, but it is also true, that this paper belongs to a suite of papers explaining the calibration strategy (Köplin et al., 2010), the parameter regionalization methodology (Viviroli et al., 2009) and the identification of

representative mesoscale areas, exhibiting similar sensitivity to climate change (Köplin et al., 2012). It is my opinion, that we should appreciate such kind of well embedded additive contributions, rather than reading papers repeating each time with other words the description of a particular basic setup leading to a calibrated model. Having said this, in this case the authors assume that the setup obtained from the previous studies can taken to explore the sensitivity of alpine areas to the change of two most important land cover elements (glacier extent and forest coverage). The setup a classical impact chain using currently available scenarios and glacier maps and introduce a conceptual framework to create forest change scenarios. In this respect I like the good links to specific literature in on the topic "forest change". The presented results are interesting and enlighten interesting aspects concerning relevance of different components of the hydrological cycle on the full response of the system to changed boundary conditions (i.e. climate and land cover). The general shape of the manuscript is of high quality. It is clearly written. The Figures tend to be loaded with huge amount of information. One must really take 3 minutes to explore the Figure before being able to interpret it. Beside this, I have some points that I want to be addressed by the authors in a revised version of the paper.

Issues to be addressed (Page(s) – Line(s)):

5984_5985 – bottom_top : The abstract end with saying that the impact modelling chin presented can give answers on the topic addressed. I would prefer to read here some of these answers.

5985 – 1,27: I agree with the other reviewer, that the authors should give more credit on current similar research on using glacier scenarios in hydrological impact studies (Jost et al., 2012, Horton et al., 2006, Huss et al., 2008; Finger et al., 2011).

5987 -26: Some of the areas are affected by hydropower. Please declare here that for all areas you present actually the impact on the natural hydrological regime.

5988 – 1,7 : It is obvious, that the study would largely profit from a estimation of parameter uncertainty. Ideally one should have a 4 dimension to test with ANOVA, where one applies some equifinal parameter sets with each of the other options concerning scenarios (climate, glacier, forest). In my opinion this would result in a very confusing cloud of results, where one could not discern about the weight of each scenario. What to do? You should declare here some sound reasons for keeping the model parameterization constant and discuss how different parameter sets would affect the results at monthly time scale.

5988 – 26,29 : Maybe specify here that the precipitation decrease is larger in southern Switzerland (Bosshard et al., 2011)

5990 – 11: Please clearly declare that the land use scenarios are conceptual and "ad hoc". Add in the conclusions, that possibly a coupling with landscape evolution model (e.g. Lischke et al., 2006) would allow to have a more physically based regional differentiation of forest scenarios.

5990 – 12,16: In lower altitude ranges (below 1200. m.a.s.l, e.g. Dobbertin et al., 2006) a decline in Scots pine density has been observed in the Wallis region. You should maybe discuss somewhere, that forest change is only a issue for the elevation ranges you considered, but that at lower ranges the opposite is behavior is predicted.

5994 – 15: Concerning SSM I think it would be useful to show the results as "deficit from the maximum storage capacity" in millimeters.

5994 – 17-22: The authors introduce how the runoff coefficient (RC) is derived from the model variables. They should comment why the use this variable as a climatic indicator. RC is generally adopted in (flash-)flood characterization. On a event-basis I would agree to call the variable defined by Eq. 1 "runoff coefficient". On a monthly and/or yearly average I would call this term "direct runoff coefficient". As an alternative I suggest to include also the base-flow component and call the index "runoff efficiency"

5996 – 14: I find the ANOVA idea very appealing and the results from such analy-

sis very interesting since it boosts a process relating critical discussion of the model results. Anyway I think, that your setup is rather tricky concerning the applicability of ANOVA. For instance the combinations CCx–GCTRL-Fx would result in unrealistic scenarios where you will have a large increase of ICE-MELT and generate additional discharge as soon as snow cover is melted away. This is in my opinion the reason why in Figure 8 you have such large contribution of glacier scenarios to the total variability in April and May (for basin 9) and throughout the year (for basin 8). I guess you should reduce the number of levels and realize a 2x2x2 sub-sample of your experiment and look at ANOVA only for that. I mean that you select the two more extremes climate scenarios (CCoptimistic and CCpessimistic) , two glacier scenarios (GC and GCNO) and two forest scenarios (FC1 and FC3). Furthermore, you could later make a random choice of two of the 10 CC scenarios, and evaluate how this affects the ANOVA analysis.

5998, 2 : In this section you describe the (very loaded) Figures 5 and 6. Concerning these 3 Figures I have 2 wishes: 1) SSM expressed as deficit, so that you can see more of the signal. 2) Do you really need to plot Psol stacked to Pliq and SME? Having the three variables stacked represents a column of water, that is not existing. Since you just need the solid precipitation for the First sentence of Section 4.1, then I would recommend you to support this finding with a table (change in portion of solid precipitation for each month) and show here only the stacked column of liquid precipitation and the snowmelt, e.g. the water amounts which are inputted to the soil and runoff generation modules of you model.

6002 – 19,23: The peak of "climate-scenario variance" in summer soil moisture changes is probably linked to the variance in expected summer precipitation decrease? Why you discuss only the temperature change? Why there is no "peak" for basin 7?

Minor comments:

5987-4 : Add already here that you extend also (Köplin et al., 2010)

5991 – 15,18: Cite some of the studies you are thinking at when formulating this sentence and discuss them later on after presenting your results.

5993 – 9: The parameters of PREVAH evapotranspiration module are well described in Gurtz et al., 1999.

6002 – 2,4: Any citation to support this statement?

Figure 2: Put bold basins with important hydro-power influence (7, 9, 10, 14)

Figure 5 and 6: Try to unload it.

Final considerations: The manuscript is innovative and deserve consideration by HESS. The largest the complexity of a modelling chain, the most difficult is to be state of the art in all the sections of the paper. I find this study a well balanced effort to consider most of the factors needed to address the targeted scientific questions. There is this neglecting of the uncertainty of the hydrological model. I am confident, that the authors will be able to provide in the reply sufficient argumentation to address this issue. I would be happy to re-consider this manuscript after moderate revisions.

Best regards

Massimiliano Zappa

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5983, 2012.