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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.

Anonymous Referee #2

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The manuscript by Köplin et al. investigates and compares the influence of climate and additional land cover (here: glacier cover and forest cover) changes on the hydrology of 15 catchments in the Swiss Alps. The hydrological model PREVAH is applied to each catchment for a control period and for several scenarios, successively incorporating climate, glacier and forest changes. The effects on water balance components are analyzed. The relative importance of uncertainties in the different types of scenarios are assessed by an ANOVA.

The authors obviously put great efforts into their work. The study is well organized, the methods are mostly well described, and the results are presented in a clear and well-organized way. Most of the conclusions are justified by the results presented. To my

knowledge, this is the first study that investigates the joint effects of climate and land cover changes in such a systematic and comprehensive manner. In general, I consider the study very relevant for the hydrological climate impact community. It puts different kinds of scenario assumptions into a mutual perspective and allows to draw important conclusions (at least for the sample of investigated alpine catchments). Weaknesses of the study mainly relate to the interpretation of the results of the ANOVA analysis. Some figures are rather overloaden and could be simplified. A literature review on climate-land cover interactions (both directions) is missing but should be included. Possible implications of considering additional precipitation changes for the land cover scenarios are not discussed. A number of further issues should be improved as well, see the details below. After accounting for these points, which I'd classify as minor revisions, I consider the study as well-suited for publication. I congratulate the authors for their very nice piece of work.

MAIN POINTS

- Consideration of precipitation changes: When deriving the glacier and forest scenarios, only the projected temperature changes are taken into account, precipitation changes are not considered. I agree with the authors that temperature changes are probably the most important factor for both glacier retreat and forest cover change in alpine environments. But a more explicit mentioning of this simplification and a brief qualitative discussion about possible implications would be valuable. The projected increase of winter precipitation might lead to a somewhat less pronounced reduction of glacier area (increased accumulation), whereas a decrease of summer precipitation might limit forest growth in dry inner-alpine environments. Furthermore, additionally accounting for precipitation changes (which differ between the climate scenarios) might increase the importance of the CC factor in the ANOVA (if the GC and FC scenarios are indeed calculated separately or each CC scenario; see below). I leave it completely up to the authors to decide whether incorporating such a qualitative discussion or not. It is not absolutely necessary, but might enhance the quality of the manuscript.

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- page 5989, lines 17-18: "... with scenario-specific temperature changes": It rather seems that the glacier retreat scenarios are not "scenario-specific" (i.e. different retreat scenarios for each climate change scenario), but that some mean temperature change is assumed for GC (see e.g. Figure 4). Please correct me if I'm wrong. The same applies to the FC scenarios (section 2.3). Are forest cover changes calculated separately for each climate change scenario? If so, it might be helpful to mention this fact more explicitly and to modify Figure 2 (which suggests that the same GC and FC scenarios are applied to each CC scenario).

- Ensemble mean analysis (descriptive analysis) versus ANOVA: I assume that the authors are aware of the different nature of their two analyses (descriptive vs. ANOVA), but these differences are somewhat hidden in the manuscript and should be pointed out much more clearly. The ANOVA is not just a more quantitative version of the descriptive analysis (as it could easily be understood from the text), but produces an entirely different kind of information. While the descriptive analysis of water balance components is based on ensemble mean values and allows to compare the average effects of CC alone, CC+GC and CC+GC+FC on the water balance, the ANOVA assesses the importance of variations (here: uncertainties) WITHIN the different scenarios (CC, GC, FC) for changes in the investigated target variables. The descriptive analysis, for instance, shows that CC is in many cases the most important factor and that considering additional effects (GC and FC) has only little influence. This does not mean, however, that CC needs to be the most important factor in the ANOVA. If the ten CC scenarios were very close to each other, the influence of variations in GC and/or FC could still be dominant (this is not the case, however). Of course, if the descriptive analysis shows that a certain scenario/factor has only little influence in the ensemble mean, it is very unlikely that variations in this factor will have a strong effect. Still, the different kind of information the two analyses provide needs to be better emphasized in the manuscript. In this respect, it also very important to point out that the then CC scenarios do not sample the full uncertainty range of future climate change. For instance, only the A1B emission scenario is considered. See e.g. Bosshard et al. (2011)

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and CH2011 (2011). GC, on the other hand, possibly samples the full range of uncertainties as it ranges from GC_CTRL to GC_NO. Hence, the importance of CC in the ANOVA is very probably underestimated. The fact that the ANOVA focuses on uncertainties within the individual scenarios/factors can partly be clarified by incorporating the following modifications: Page 5996, line 15: Change to "... the relative impacts of uncertainties in ...". Page 5996, line 27: Change to "... the relative importance that uncertainties in the scenarios ...". page 5997, line 2: "variations" instead of "changes".

- climate-land cover interactions: The author's correctly highlight that climate and land cover are linked to each other by various interactions. These, however, are not investigated in their study and should not be confused with the interaction terms of the ANOVA. The study addresses the separate and combined effects of climate and land cover changes on catchment hydrology, but climate-land cover interactions are basically neglected. None of the ten RCMs applied includes a dynamic vegetation scheme or a dynamic glacier scheme, the land cover characteristics are fixed. Assuming a vegetation or glacier response in a fully interactive manner would involve RCM experiments that make use of such schemes and that could account for land cover feedbacks onto the climate. In other words: If a forest change scenario other than F_CTRL (i.e., F_1, F_2, or F_3) is linked to a certain CC scenario, there is some inconsistency as the climate change signal would be different if the climate model would interactively account for forest changes. The same applies for the glacier change scenarios. It is surely beyond the scope of the present study to investigate these effects in detail, but the authors should at least mention this simplification. There's quite some literature available on climate-vegetation interactions (e.g. on recent GCMs and RCMs involving dynamic vegetation modules), and also a few studies on glacier-climate interactions. Some of these could be cited. In this respect, also the statement on page 6005, lines 5-6 is very misleading, as these interactions have not been investigated at all. The authors refer to the interaction terms in the ANOVA, which should be clearly stated.

- Figures 5 and 6: These two figures are very complex and not easily accessible. It

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requires quite some effort to get the essential points out of them. The figures contain a lot of information, some of it very useful, other not immediately relevant. The authors should consider simplifying these figures. For instance, the right hand side panels (JJA, DJF and annual means) could be skipped in my opinion. The same is true for the mean annual cycles of temperature (red symbols in the uppermost panel). In any case, the two yellowish colors for ETP and ETA should be better separated (chose two different colors if possible). Concerning the "input into the simulated water balance" (grey and green bars) it is not clear to me why P_sol and SME are both represented. Isn't that a double accounting and shouldn't it just be SME? (P_sol is stored for some time after it appears again as SME).

FURTHER ISSUES

- abstract, line 8: "were derived from" instead of "consist".

- abstract, line 9: I'd suggest to use "changes" here instead of "deltas", as the delta change methodology has not yet been introduced.

- abstract, line 16: "as changes in evaporation . . ." instead of "as evaporation . . ."; "are concerned" instead of "is concerned".

- abstract, line 22: This statement, referring to hydrological climate impacts studies in general, is too strong. A generalization of the findings is not possible at this point, as correctly mentioned in the following sentence. Better replace "in hydrological climate impact studies" by "in this study".

- page 5985, line 7: "runoff generation and concentration processes" would be more appropriate.

- page 5985, lines 9-11: Phenological changes (for a given species) might be another aspect, that could be mentioned here.

- page 5985, line 23: "factors" instead of "conditions".

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- page 5986, line 22: “might change” instead of “will change”.

- page 5986, line 23: “. . . hydrological change when considering only changes in atmospheric conditions?”

- page 5987, line 6: “. . . the output of then regional climate models” instead of “. . . then regional climate models”.

- page 5987, line 20: Figure 1 also shows the clusters C1 and C7. Does this mean that C1 and C7 are not sensitive to climate change? If yes, why? It might be helpful to clarify this point.

- page 5988, lines 1-2: I’d suggest to rephrase this sentence. Not the catchments were parameterized, but the parameters of the hydrological model were regionalized.

- page 5988, line 6: I’d suggest to add “. . . neglecting the influence of hydropower production” after “.. land cover change . . .”.

- section 2.1: It is not entirely clear, how the climate scenarios were applied to the PREVAH model. Some more information might be helpful. Did the authors apply areal mean changes of T and P at daily resolution for each catchment? Or was the climate change information applied to individual stations within the catchments and then interpolated to the catchment area? Furthermore, the control period should be mentioned in this section as well as in the abstract. Currently, it is only contained in the caption of Table 1.

- page 5988, line 13: Change to “. . . all of them assuming the A1B emission scenario and driven by 5 different GCMs.”

- page 5988, line 15: Change to “. . . of daily changes, yielding a continuous . . .”.

- page 5988, lines 16-17: Change to “The mean annual cycles of temperature and precipitation for the control period XXX to XXX were provided . . .”.

- page 5988, lines 19-22: Please mention explicitly, that the delta change approach

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does not account for changes in variability, which might be a strong simplification.

- page 5988, lines 23-28: A further reference to Bosshard et al. (2011) or the CH2011 report might be helpful here, since more details on the projected temperature and precipitation changes are given therein.

- page 5989, line 3: “changes” instead of “deltas”.

- page 5989, line 7: The correct number is probably 150 m/K, not 100 m/K. Please check.

- page 5989, line 19: Change to “In PREVAH, the surface that is . . .”.

- page 5990, lines 5-6: Change to “. . . if no specific retreat scenarios were available.”.

- page 5992, line 3: “reduced” instead of “changed”.

- page 5992, lines 5-14: Even after having read this section several times, it is still not entirely clear to me how FC1 and FC2 differ. Does FC2 assume that areas that are used for alpine farming within the limits of the tree line today are additionally converted to forests? The authors might think about rephrasing this paragraph to make it clearer.

- page 5993, line 20: Change to “. . . the increased soil depth under FC3. An increased . . .”.

- page 5994, lines 21-24: Not clear.

- page 5995, line 15: Strongest increase of WHAT?

- Section 3.2: Does the ANOVA applied here account for the fact that the three factors contain different levels? Please specify. If this is not the case, one would expect CC to be dominant simply due to the fact that it contains 10 levels as opposed to 3 levels for GC and 4 for FC.

- page 5998, line 3: I’d suggest to introduce the (very complex) Figures 5 and 6 already at the beginning of this section.

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- page 5999, line 15: “exactly superimposed”: is this really true? Are the values identical?

- page 6000, lines 5-6: “the degree of forest cover CHANGE”!

- page 6000, lines 10-12: No, probably not. As the shown changes are changes w.r.t. the previous scenario, the overall change of ETA is probably still negative (but less negative than for CC alone).

- page 6000, lines 12-15: Section 4.1 investigated two different catchments (5 and 9), which partly showed an increase of summer ETA due to CC alone (as derived from Figures 5 and 6). Only in catchment no. 5 summer ETA partly decreased (in July and August). I’d therefore suggest to remove this sentence.

- page 6001, line 13: Change to “. . . the present-day forest extents. . .”.

- page 6001, lines 14-17: Not really. For the ANOVA, the degree of forest cover CHANGE is probably most important.

- page 6001, lines 21-23: Similar as above. I guess both the effects of present-day forest cover and forest cover CHANGE can be seen here (which go hand in hand as catchments with a large forest cover fraction also experience the largest forest cover changes).

- page 6002, line 2: Change to “. . . of evaporation in high-altitude catchments”.

- page 6002, lines 14-18: The authors should consider to remove the target variable RC entirely from the analysis. In my opinion, not much insight is gained by its consideration. Figure 8 would be streamlined.

- page 6003, line 13: One or two introductory sentences for the discussion section would facilitate the reading. Also, I’d suggest to replace the first sentence of the discussion by “We demonstrated the importance of forest cover changes on the projected evapotranspiration.” (see comments above).

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- page 6003, line 14: “of more than” instead of “on more than”.

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