The authors would like to thank anonymous referee #3 for the valuable comments on our manuscript.

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

Anonymous Referee #3, received and published: 23 June 2012

The authors present a 'classic', but still potentially interesting study on the effect of climate change and land-use / cover change on runoff. The preceding two reviews have already listed a number of important points. I agree with many points, especially those of reviewer #1. My own major concerns are the following:

1) Uncertainty of the hydrological model has not really been addressed. It might be true, that in comparison to climate scenarios/models, these uncertainties are smaller, but I doubt that this still holds when land-use effects are considered. Running a model like Prevah with different parameters for different land-use classes, implies that there might be many different solutions for current conditions (because of compensation effects), but these can have largely different consequences when a changed land-use distribution is considered. I therefore strongly recommend evaluating parameter uncertainty effects of the hydrological model!

We are aware that model parameter uncertainty is an important aspect, especially in climate impact studies. The assessment of parameter uncertainty was beyond the scope of the present study which we explained in the following paragraph that we added to the discussion section:

Another aspect of the hydrological model that should be critically reflected is parameter uncertainty. If the tuneable parameters of the hydrological model could be calibrated on runoff of future climate and land cover states, then the parameter set is likely to be different from the one calibrated on the control period conditions. This is why assessing model parameter uncertainty is crucial, especially when using the model for climate impact analysis and when studying land cover change. For several reasons this assessment was not included in the present study. For example, the study analysed 15 representative case studies taken from a set of 186 catchments in Switzerland (Köplin et al., 2012; cf. Sect. 2). The model parameters for those catchments were regionalized because most alpine catchments in the study domain could not be calibrated on measured natural runoff. The regionalization procedure, however, entails that the resulting hydrographs cannot be referred to one distinct parameter set (for details please see the description in Sect. 2 and Viviroli et al., 2009c), which hinders assessment of parameter uncertainty in general. A common way to sample parameter uncertainty is to generate 10 000 random parameter sets to run the model and to evaluate these so-called Monte Carlo runs for their goodness of fit. This goes far beyond the means, however, when using a semi-distributed hydrological model like PREVAH and when studying a range of different catchments rather than a single case study. Moreover, applying 10 or 100 equally good parameter sets does not at all guarantee that those parameter sets are better suited for climate and land cover change modelling. Other studies showed that model parameter uncertainty is less important than climate model uncertainty (e.g. Finger et al., 2012, Schaefli, 2005). In summary, although we are aware that we did not assess all sources of uncertainty in the present climate impact study, we are also confident that we assessed the most important source of uncertainty, i.e. the climate model and compared it to the relative importance of glacier and forest change.

2) The Penman Monteith approach is used for potential evaporation (p5993). However, it remains unclear how this has been done for the climate scenarios. If one looks at vegetation change effects and assumes that these are mainly caused by different evaporation rates (directly or indirectly through storage), as it is done in this study, evaporation seems to be central. If for the scenarios evaporation is assumed to be unchanged (which I assume is the case in this study), then this should at least be clearly stated. However, I also would like to recommend to consider a potentially changed potential evaporation for the scenarios.

The reviewer is partly right. Applying the Penman-Monteith equation to calculate potential and actual evapotranspiration allows us considering some changes in evapotranspiration, but not all necessary input is changed with the applied climate scenarios. The two variables that change are temperature and precipitation, the latter governing the amount of available soil moisture for evapotranspiration. The variables radiation, relative humidity and wind speed do not change with the applied climate scenarios, but they are used to calculate potential evapotranspiration. For the scenario, the respective time series from the control period are used. We agree with the referee that this is a rough simplification which should be stated more clearly. The actual evapotranspiration ETA is derived from the potential evapotranspiration ETP by adjusting ETA with plant-specific albedo, leaf area indices, vegetation height and minimum stomata resistance. Therefore, we think it is justifiable and reasonable to assess the effect of a changed land cover on the evapotranspiration and indirectly on the runoff. We integrated the following passage in Sect. 2.4 (p. 5993 of the discussion paper) to clarify the assumed simplifications:

It should be clearly stated that temperature and precipitation are the only variables that change in the climate scenario. Relative humidity, radiation and wind speed, which are also used to calculate potential evapotranspiration, do not change. For these variables the control period's time series are applied, which is a rough simplification that might affect the reliability of the results. Land cover-specific parameters like albedo, leaf area index, vegetation height and minimum stomata resistance do change with the land cover, though. As stated above, those variables are used to assess ETA, which is why it is reasonable to assess the effect of a changed land cover due to changes in climate with this modelling setup.

Minor comments:

The use of (only) a delta change approach for the scenarios is certainly a limitation. As the authors correctly state this means that mean values should be evaluated rather than extremes. For the present study using only the delta change approach might be ok, but for future studies I would, however, strongly recommend to consider also other approaches. It would be useful to state the limitation of a delta change approach even more clearly. The limitations of the delta change approach are well-known within the climate impact community, but should also be communicated clearly.

The reviewer is right and we included the following statement in Sect. 2.1, Climate scenarios, to address the limitations of the Delta Change approach more clearly:

[…] *This post-processing method does not account for changes in the variability of the climate variables, though, which might be a strong simplification.*

P5992, l23: where exactly comes this number of 10 cm from? And how do these 10 cm translate into changed values of the soil storage?

This value was actually chosen arbitrarily (but based on literature, p. 5992, l. 19f.) to represent a possible mean increase in soil depth in general. This was not described explicitly and we added the following text passage:

We mimic this in our scenarios with an *arbitrary* general increase of soil depth by 10 cm in 100 yr on forest covered areas, both new and existing forests. *This leads to increases in the mean soil depth of the catchments between 4 % where the previous mean soil depths was high (> 1 m) and 23 % where it was low before (< 0.25 m).*

The authors should be more careful with a consequent use of past tense for their own work. On page 5987, for instance, both past tense, present tense and future are used (We extended…. We only assess... we will analyze).

We corrected the tenses on page 5987, checked the whole manuscript for further wrong tense use and corrected it. We are confident that remaining mistakes will be corrected during copy-editing, in the case the paper will be accepted for publication.

Abstract, l20, catchments instead of catchment

We changed that.

P5994, l8: this sentence reads as if Bronstert (2004) stated something about Prevah, he, however, did not use Prevah at all. Please reformulate.

The referee is right. Actually, the reference to the paper of Bronstert (2004) is not essential, here, and we deleted the reference.