The authors would like to thank Massimiliano Zappa for his very detailed and constructive comments on the paper. We think that the quality of the article could be improved incorporating his suggestions.

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, received and published: 25 June 2012

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 is 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 ends with saying that the impact modelling chain presented can give answers on the topic addressed. I would prefer to read here some of these answers.

The answers, i.e. the main results of the study are given in the middle part of the abstract. We closed the abstract with this statement to highlight the general aim or purpose of our study. We think this is one possible and common way to end an abstract and therefore decided not to change it.

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).

We added this passage in the introduction:

While there are a number of hydrological climate impact studies that account for glacier retreat (e.g., Hänggi, 2011; Horton et al., 2006; Schaefli et al., 2007), there are few that particularly assess its effect on the projected runoff (Finger et al., 2012; Huss et al., 2008; Jost et al., 2012; Stahl et al., 2008).

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.

In the paragraph (p. 5988, l. 1–7) that follows the stated text passage we said:

"[...]. A regionalisation procedure was applied because most of the catchments in the high alpine area are used for hydropower production and can therefore not be calibrated on measured natural runoff data. That is, we study the natural runoff behaviour of the catchments under scenarios of climate and land cover change, which should be kept in mind when interpreting the results."

5988 – 1,7 : It is obvious, that the study would largely profit from a estimation of parameter uncertainty. Ideally one should have a 4th 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.

We added the following paragraph in 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.*

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

At this part of the text we summarize the "[…] projected climate change signals *for the case study catchments* […]" (p. 5988, l. 23) and not the climate change in Switzerland. We decided therefore not to mention the stronger decrease in southern Switzerland, although the referee is right, of course.

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.

The referee is right, and the same applies to the glacier scenarios. We added the following passage at the end of Sect. 2.3. "Glacier retreat":

It is worth noting that the glacier scenarios are ad-hoc as well as they are static. That is, for the scenario simulations the glacier area is changed from the beginning, and there is no gradual glacier retreat during the modelling period.

And at the end of Sect. 2.4 "Forest scenarios" we now state:

As for the glacier scenarios (Sect. 2.3), all forest scenarios are applied instantaneously and they are static.

In the discussion section we added the following passage as suggested (after l. 23 p. 6004 of the discussion paper):

Another possible improvement would be to couple a landscape evolution model (e.g. Lischke et al., 2006) to the hydrological model which would allow a more physically based distribution of forested area under a future climate.

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 behavior is predicted.

We added the following sentence in the discussion where we critically reflected the derived forest scenarios:

Enhanced drought stress in summer caused by decreasing precipitation, however, could lead to a decline of forests, especially in the dry inneralpine valleys (Dobbertin et al., 2006).

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

We agree with the referee that the soil moisture storage SSM could also be displayed as a deficit. However, the maximum storage capacity of SSM is altered in the forest scenarios (through a changed root depth or increased soil depth, for example), which complicates direct comparison and interpretation. For example, the deficit might be larger under the most extreme forest scenario (FC_3) compared to the control, but the water column stored in the soil might be larger than under control period conditions, too, simply because the storage capacity is larger. The larger deficit volume under FC_3 would then be an artefact instead of a real difference and hinder interpretation or even lead to misinterpretation. Therefore, we display SSM as the absolute stored volume of water.

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"

We agree with the referee that "direct runoff coefficient" would be a better term. However, we decided to remove RC from the analysis, anyway, following the suggestion of referee #2. This decision is based on the limited insight gained from the analysis of RC (please refer to the reply to referee #2 for more details). Removing RC from the analysis and, therefore, from the figures, furthermore helps to unload the figures, which was a general issue raised by three of the four reviewers.

5996 – 14: I find the ANOVA idea very appealing and the results from such analysis 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.

We agree with the referee, that subsamples of 2x2x2 are likely to give a more realistic picture. We followed the suggestion and tested different combinations of two of the ten climate scenarios with GC/GC_{NO} and FC_1/FC_3. For a combination of CCoptimistic (low T increase) and CCpessimistic (high T increase, cf. Fig. A on p. 8 of this document) the contribution of the glacier scenario to the total variability of the runoff is very small for catchment 9, which the referee mentioned. However, it is very high (March/April) for catchment 8. For another 2x2x2 combination (low and moderate T increase), the climate scenarios contribute less to the overall uncertainty, which is particularly obvious for evapotranspiration. A systematic testing of all possible 2x2x2 combinations (i.e. always GC/GC_{NO} and FC_1/FC_3 combined with 2 of the 10 climate scenarios) would be certainly interesting. We decided, however, to apply a 3x3x3 setup instead (see Fig. B on p. 9). Our aim was to assess the uncertainty that is introduced to the hydrological projection when not accounting for glacier retreat (G_{CTRL}) or only simplistically accounting for the relative impact (G_{NO}). The same applies to the forest change: How much uncertainty is introduced if one neglects forest change (F_{CTRL}), if one accounts of an increase of the tree line alone (FC_1) or if one tries to reflect all possible changes (increase, ingrowth, soil genesis, FC_3) that are possible to occur in the extreme case (under perfectly favourable conditions). This then entails that the tested scenarios in the ANOVA setup are somewhat artificial or synthetic. In our opinion, this does not prohibit conducting an analysis of variance, but this should be stated clearly. We included the following text in methods Sect. 3.2 "Analysis of variance (ANOVA)":

[…]*The fully cross factored model also entails that physically unrealistic scenario combinations are assessed, for example a high temperature increase combined with the glacier extent of the control period and the most extreme forest scenario. Still, we decided on this setup to assess the uncertainty that is introduced to the hydrological projections when not accounting for glacier retreat at all (GCTRL) or only simplistically accounting for the relative impact (GNO). The same applies to the forest change: How much uncertainty is introduced if one neglects forest change (FCTRL), if one accounts of an increase of the tree line alone (FC1) or if one tries to reflect all possible changes (increase, ingrowth, soil genesis) that are possible to occur FC3.*

5998, 2: In this section you describe the (very loaded) Figures 5 and 6. Concerning these 2 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 your model.

We decided to display SSM as an absolute storage volume and not as a deficit for the reason explained above. We are confident that the signal is now – in the revised version of the figure (see attachment) – well perceptible. We followed the suggestion of the referee and included a table (see below) showing the seasonal and annual changes of all variables previously displayed in Figs. 5 and 6. We excluded solid precipitation from the revised figure.

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?

The referee is surely right and we discussed this already. The sentence (p. 6002, l. 23–26) that follows the mentioned passage reads:

This clear signal can be attributed to the pronounced temperature increase in summer and the associated depletion of SSM through evapotranspiration, but also to the decreasing summer precipitation and, therefore, a reduced input into the storage.

We analysed the monthly temperature and precipitation changes for the studied catchments, to see if there are any deviances or peculiarities for catchment 7 (the one that lacks the climate scenario variance-peak). The annual cycles of Delta *T* and Delta *P* of this catchment do not differ from the other catchments.

Minor comments:

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

We included this reference.

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.

We deleted the sentence, because it does not make sense (we do not compare our study to studies that do not account for glacier nor forest change). In the revised discussion section, we do compare our results to studies by Zierl and Bugmann (2005) and Gasser et al. (2003; forest change), to studies by Huss et al. (2008), Jost et al. (2012), Stahl et al. (2008; glacier change) and to studies by Bosshard et al. (2012), Finger et al. (2012), Horton et al. (2006) and Schaefli (2005; climate change). Please see the discussion section of the revised manuscript for the updated version.

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

We added this reference at the end of this paragraph, where we talk about evaporation in PREVAH.

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

The mentioned sentence now reads:

[...] Moreover, *the forest change and, therefore,* the relative importance of the forest scenarios is small in the highly glaciated catchments *(cf. also catchments 8 and 9 in Fig. 7)*, which *might additionally raise* the variance fraction explained through glacier change.

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

We marked catchments 1, 2 and 6 instead and stated in the caption of Fig. 2:

[...]*The catchments highlighted in bold font are not influenced, whereas the discharge of all other catchments is more or less strongly affected by hydropower production.*

Figure 5 and 6: Try to unload it.

We reduced the amount of information in Figs. 5 and 6 and rearranged the displayed data. Please see the revised Figure 5 that combines the two previous figures into one (p. 11 of this document).

Final considerations:

The manuscript is innovative and deserves 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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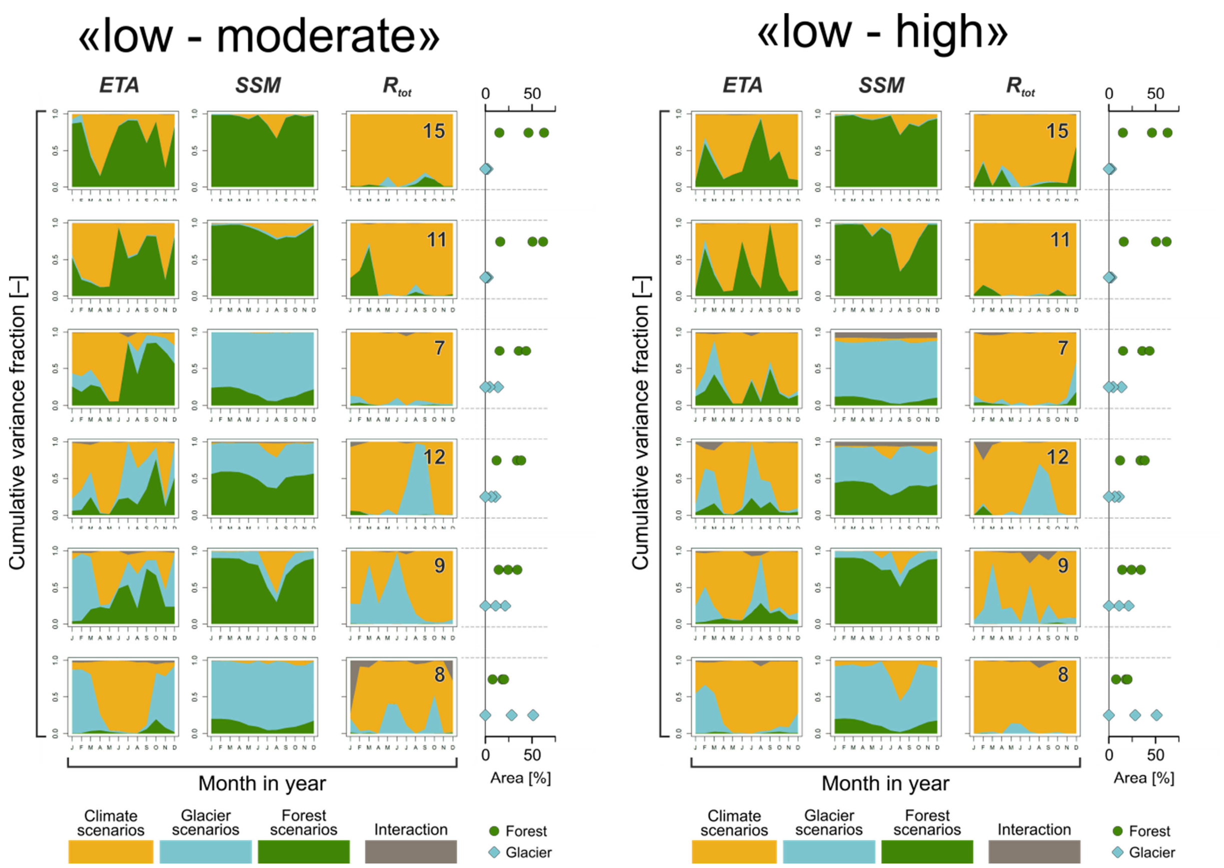

Fig. A. ANOVA results for different 2x2x2 combination (2 CC – 2 GC – 2 FC). Left panel: combination of low T increase and moderate T increase. Right panel: Combination of low and high T increase (CCoptimistic and CCpessimistic).

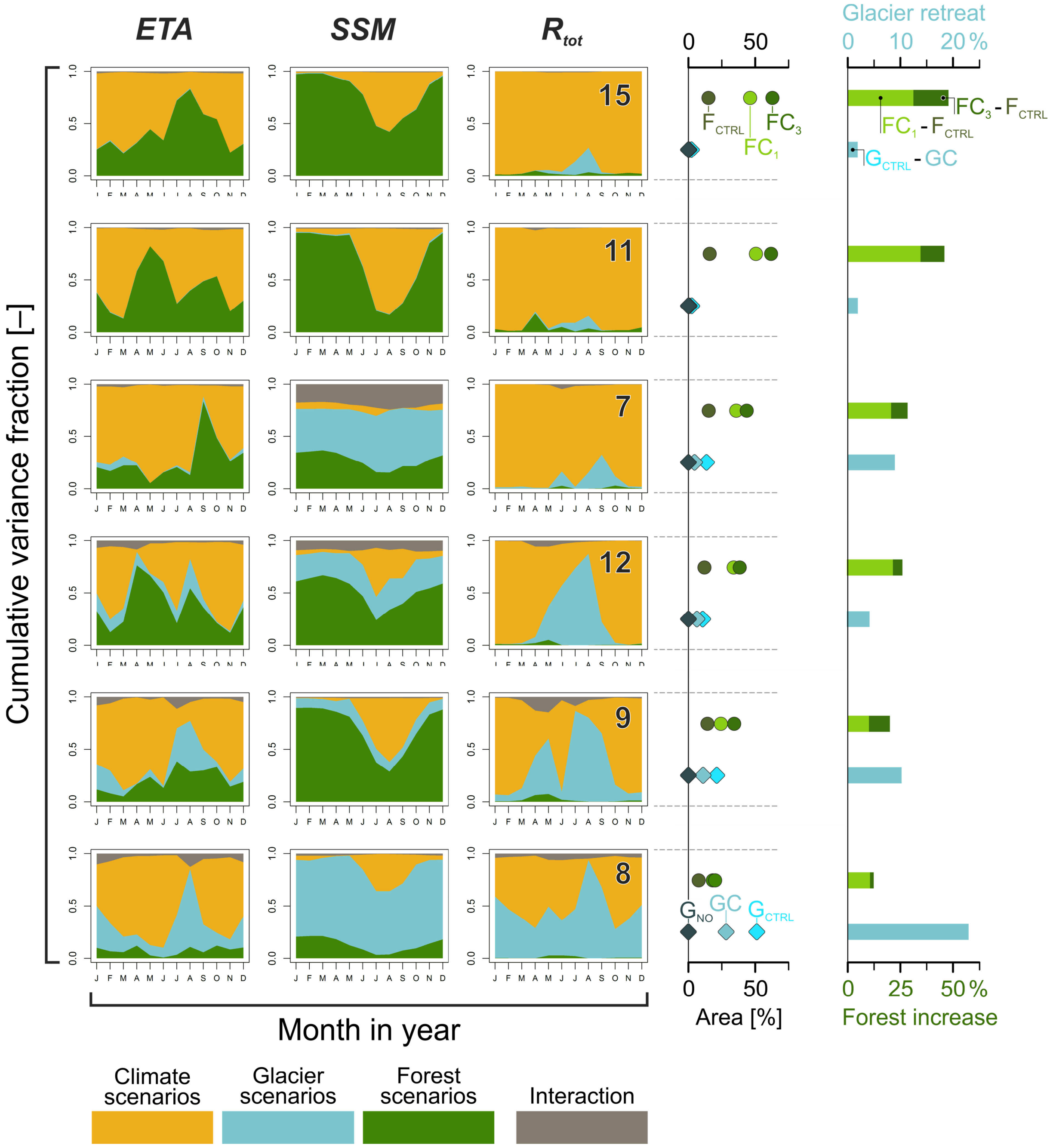

Fig . B. ANOVA results for the combination of three climate models (low, moderate and high temperature increase, cf. Table 1, p. 10) with three glacier scenarios (G_{CTRL}, GC, G_{NO}) and three forest scenarios (F_{CTRL}, FC_1, FC_3).

Table 1. Annual delta *T* and *P* for every climate model chain. The mean (MEAN), minimum (MIN) and maximum (MAX) values characterize the distribution of the catchment-specific annual change values. The precipitation change signal is given for additional information, but the grouping of climate model chains to the three different classes of mean annual temperature increase (low, moderate, high) is solely based on delta *T*.

Climate model name	Delta *T*			Delta *P*			*T*-increase
(Institution_GCM_RCM)	MEAN	MIN	MAX	MEAN	MIN	MAX	grouping
SMHI_BCM_RCA*	2.3	1.9	2.6	0.98	0.89	1.07	low*
DMI_ECHAM5_HIRHAM	2.6	2.0	2.9	1.00	0.94	1.04	low
ICTP_ECHAM_REGCM	2.9	2.8	3.0	1.03	0.98	1.08	low
CNRM_ARPEGE_ALADIN	3.0	2.7	3.4	0.91	0.88	0.95	moderate
SMHI_HadCM3Q3_RCA	3.3	2.9	3.6	1.07	1.03	1.13	moderate
KNMI_ECHAM_RACMO*	3.3	3.1	3.5	1.04	0.93	1.12	moderate*
MPI_ECHAM_REMO	3.4	3.1	3.9	1.03	1.00	1.06	moderate
SMHI_ECHAM_RCA	3.4	2.9	3.8	0.99	0.94	1.07	moderate
ETHZ_HadCM3Q0_CLM	3.9	3.7	4.0	0.96	0.89	1.08	high
HC_HadCM3Q0_HadRM3Q0*	4.2	4.0	4.4	0.95	0.93	0.99	high*

* These three climate model chains are applied in the ANOVA.

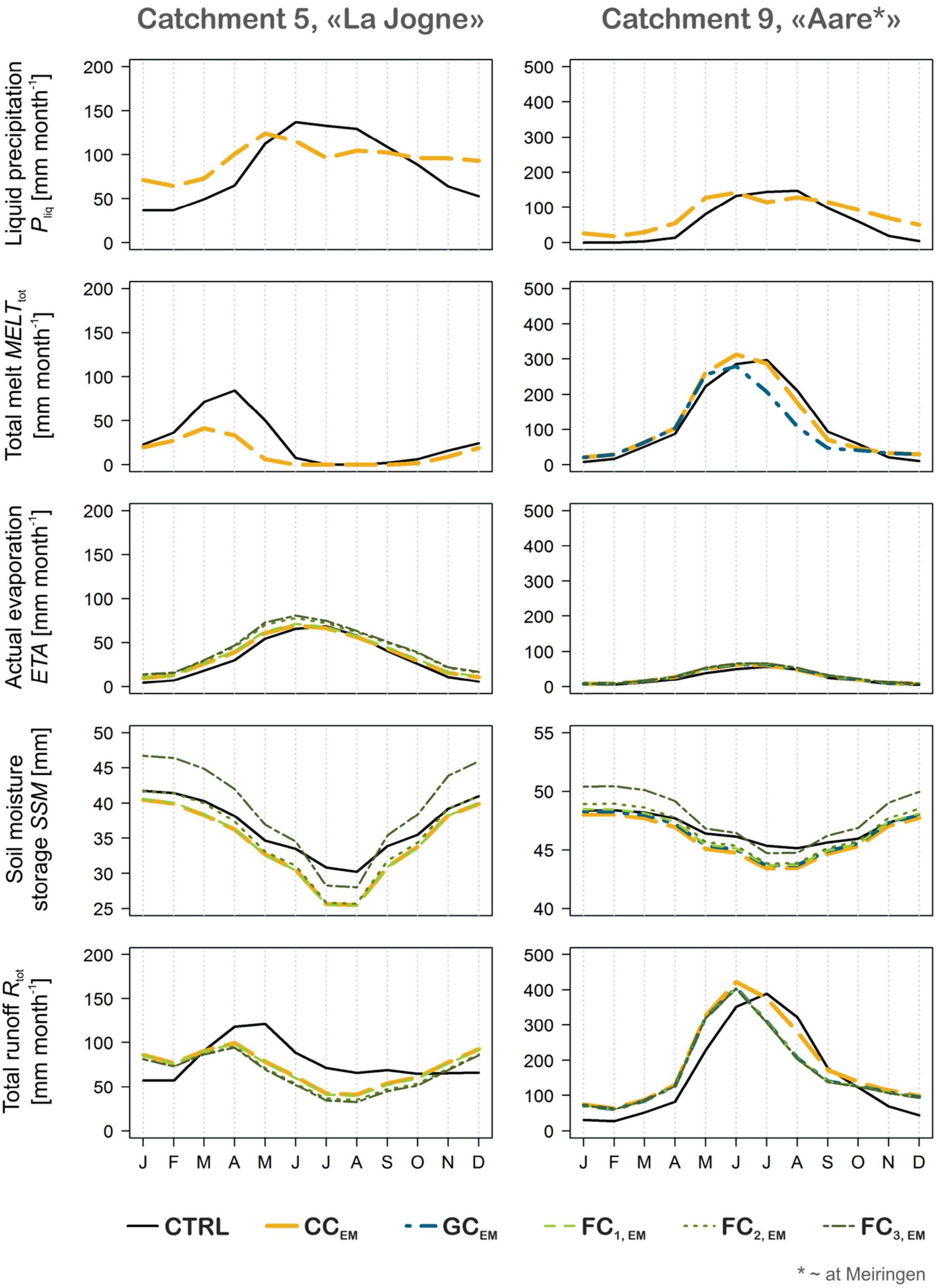
Catchment 5, «La Jogne»
Catchment 9, «Aare*»
Liquid precipitation P_{liq} [mm month^{-1}]
Total melt $MELT_{tot}$ [mm month^{-1}]
Actual evaporation ETA [mm month^{-1}]
Soil moisture storage SSM [mm]
Total runoff R_{tot} [mm month^{-1}]
J F M A M J J A S O N D
CTRL
CC_{EM}
GC_{EM}
$FC_{1, EM}$
$FC_{2, EM}$
$FC_{3, EM}$
* ~ at Meiringen

Fig. 5.