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Hydrology and Earth System Sciences Discussions

Interactive comment on "Quantifying different sources of uncertainty in hydrological projections at the catchment scale" *by* C. Dobler et al.

T. Bosshard (Referee)

thomas.bosshard@env.ethz.ch

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1 Summary

This study uses an ensemble of hydrological climate-impact model chains to assess uncertainties in the projected impacts due to different elements of the model chain. Uncertainty sources considered are the GCM, the RCM, the bias-correction model and hydrological model parameter uncertainty. The study is conducted in the Lech catchment and for the scenario period 2070-2100 with respect to 1971-2000. Dobler et al. (2012) found the GCM and RCM to be the largest uncertainty sources in projections of mean monthly runoff changes. The bias-correction method is more important for

changes in runoff extremes while the uncertainty due to the hydrological model parameters is almost negligible except for the mean runoff changes in winter time.

2 General comments

The study is an important contribution to the field of climate-impact modeling in Alpine catchments, particularly addressing the issue which model chain element the uncertainties arise from. The literature review includes the most relevant publications, the paper is well-structured and the methods are mostly well explained. My main criticism is the way the uncertainties are quantified (see 9. comment below). Along with some other minor changes, I recommend acceptance for publication.

3 Detailed comments

1. Chapter 3.1, page 3179, line 8: It would be good to indicate that the HadCM3Q3 is the low sensitivity version of HadMC3Q0.

2. Chapter 3.1, page 8180, lines 15-17: You derive the lapse rate based on two temperature stations, only. Why did you not include other temperature stations? I guess within the search radius of 40 km, there should be other stations as well. I'm just a bit worried to derive a linear regression relationship based on two data points, only.

3. Chapter 3.4: HQsim only requires temperature and precipitation as input variable. So, how is the evapotranspiration derived? I might be described in the referenced papers, however, I couldn't access these papers. Therefore, it would be good to include a short statement about the way evapotranspiration is modelled.

4. Chapter 3.4, page 8184, lines 1-2: There should be more information about the

validation of the hydrological model. Fig. 3 is not very helpful as it only shows one year. Why did you chose this particular year? I think it is also necessary to include a validation of the target variables in the impact study, i.e. the mean annual cycle (Fig. 7) and the exceedance probability distribution (Fig. 8). Regarding the issue of the transferability of the model parameters to future climates: How does the performance change in the period 1971-2005? As we have already experienced some temperature increase during that period, it might be very interesting to see how the performance changes. I would like to state that I know that such a presentation is not standard practice yet. However, in my view, it would help the community to get a hand on the issue of the model transferability (e.g. Merz et al. (2011))

5. Chapter 4.1, page 8185, lines13-22: From Fig. 4.b), I have the impression that there is a pronounced tendency towards underestimated seasonal mean runoff (i. e. out of the 6x4 seasonal data points, most of them are lower than the 4 reference data points). Since the local scaling corrects for the mean, I would expect the water balance to be equal over the long run. Deviations should only be due to natural variability, thus, there should be a balanced set of over- and underestimated seasonal mean runoff set. It seems though that the statistical characteristics of the bias-corrected scenarios lead to a systematic underestimation. How come? Do you know something about it? Could it be related to a bias in the wet-day frequency, causing higher evapotranspiration? It is only a very small effect and most probably does not have any impact on the validity of the study, but it would be very interesting to know more about it. I leave it to the authors to decide how much they want to discuss this issue.

6. Chapter 4.2: The title sounds a bit misleading to me. Also in chapter 4.3, uncertainties related to climate models and downscaling are discussed.

7. Chapter 4.2, page 8186-8187, lines 25-5: This needs some more discussion of other studies. I would have expected that the RCMs have more freedom to develop there own atmospheric circulation during summer due to flat pressure fields. Thus, the uncertainty due to the RCMs should be higher in summer. Other studies (e.g.

Hingray et al., 2007) have shown that regional scaling relationships are more important in summer than in winter.

8. Chapter 4.2, page 8187, lines 11-12: According to chapter 3.3.3, the QQ mapping is done using a 31 day moving window. This is very much similar to a monthly calibration. Please clarify this inconsistency.

9. Chapters 4.2 and 4.3: The uncertainty measure is not defined. One can deduce that the range from minimum to the maximum ensemble member is used as a measure for the uncertainty, but this needs to be defined explicitly. Furthermore, the choice of the minimum-maximum range as a measure of the uncertainty needs to be discussed. While it is certainly an easy measure to interpret, there are some statistical disadvantages. The minimum-maximum range does not make use of the data from the ensemble members in between. For example, in the temperature panel of Fig. 5a), the months 10 and 11 have a similar uncertainty according to your definition, but in month 11, the third members is situated more in the middle of the uncertainty range than in month 10. If one estimated the uncertainty in terms of variances, this would lead to a different result. Another disadvantage is that the range is not normalized by the number of samples. While you use 3 samples for GCM, RCM and bias-correction, you have 20 samples for the hydrological model parameter uncertainty. This might distort the importance of the different uncertainty sources. And as a last point to discuss, the measure does not allow for an additive partitioning of the uncertainty into contributions of the 4 uncertainty sources, i.e. the sum of the 4 uncertainty ranges does not equal the range of the full ensemble. So far, a proper discussion of these limitations is missing. Maybe even a new sub-section in section 3 would be appropriate to cover this methodological aspect of the study.

10. Chapter 4.3.2, page 8188, lines 15-18: Shouldn't it be the other way around: Small precentage changes in summer translate in large absolute changes since discharge is higher in summer than in winter (Fig. 3)?

11. Chapter 4.3.2, page 8189, line 11: I would say, the largest uncertainty range due to hydrological model parameters amount to about 35

12. Chapter 4.3.2, page 8189, line 14: What do you mean by "certain condition"? Large biases of the hydrological model that potentially transfer in large projection uncertainty or rather large climate changes that force the hydrological model to simulate runoff regimes that it has not been calibrated to? Or anything else? I think the issue of the model parameterisation is an important one and merits some more detailed discussion for the reader. Otherwise, every reader will interpret it differently.

13. Chapter 4.3.3: Mean high flow is not defined. From Fig. 8, I understand that you discuss the whole exceedance probability distribution, thus you cover the whole range from low to high flows.

14. Chapter 5, page 8190, lines 17: Using local scaling, also biases in the variability (e.g. wet day frequency) are inherited from the climate models.

15. Chapter 5, page 8193, lines 9-15: I do not fully agree with the author's conclusion. Even with a 20 year period, the model cannot be calibrated on conditions in the climate future when the temperatures are considerably above present day conditions (Coron et al., 2012). Other studies with shorter calibration periods have found the same result that hydrological parameter uncertainty is a negligible source of uncertainty (Prudhomme and Davies, 2009; Schäfli et al., 2007). Also, from Fig. 3 I would say that the uncertainty band width of the hydrological simulations is too narrow since the observed runoff is often outside of the uncertainty range. Based on this result, I would rather speculate that the uncertainty due to the hydrological model parameters is underestimated. The issue of the model transferability to future climates is an important topic and in my view, probabilistic parameter sets do not fully solve the problem. At the same time, it is clear that one cannot blame the hydrological model alone as also all the other model chain components suffer from the same problem. Thus, I do not expect the author's to solve this problem. It is clearly out of scope of this paper, but I

would like to have a more diverse discussion of the issue.

16. Chapter 5, pager 8194, lines 16-21: I like it that you mention the issue of varying one uncertainty source while the others are kept constant. I suggest to inlcude the keyword "interactions" between the uncertainty sources in this text passage.

17. Figure 7: The suffixes DELTA and SCAL seem to be wrong when compared to the other figures.

18. Fig. 8a)-d): Please include a line at zero to enhance the readability.

4 References

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