Hydrol. Earth Syst. Sci. Discuss., 10, C1118–C1120, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C1118/2013/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Elevational dependence of climate change impacts on water resources in an Alpine catchment" *by* S. Fatichi et al.

Anonymous Referee #2

Received and published: 24 April 2013

This manuscript presents a study where a climate impacts on runoff in an alpine catchment have been investigated using a catchment model which had been extended to consider the effects of human (mainly hydropower) activities. This is certainly an important issue which the scope of HESS and the authors put obviously considerable efforts in their study to implement all details of anthropogenic impacts. However, as already discussed by previous comments there are major concerns with this manuscript. My main concern it remains unclear, what the main focus of this contribution is and what exactly the additional contribution to knowledge is. If the goal is the development of a model that includes anthropogenic impacts then these model extensions need to be much better described, discussed and, most importantly, analysed. In this case it is not enough to say, this is the best we could do with the available data, but it also

needs to be investigated what effects certain assumptions or simplifications have and what alternatives have been tested. Some assessment of uncertainties would also be necessary.

If the main focus should be the quantification of climate change impacts then several aspects of the study are not fully convincing:

1) The use of only one GCM and two RCM (and only one emission scenario) – this is in the days of ENSEMBLE and other ways to obtain GCM/RCM data simply not state-of-the art anymore. Several studies have highlighted the need for using ensembles of GCM/RCM and the argument that the use of a weather generator would compensate is not really true – systematic biases and random variations are not the same! 2) In this alpine catchments, the glaciers of course are crucial for the long-term changes in runoff. The area-thickness scaling approach used here to estimate ice-thicknesses seems a very crude assumption. Such equations might be useful at a larger scale, for which they usually have been derived but are in general not very satisfactory for smaller areas. 3) The assumption of unchanged operation rules in the future (p3767) seems unrealistic if we assume runoff changes in the future.

Looking at the conclusions, I am also wondering about the exact novel contribution of this manuscript. The first conclusion, namely that hydraulic infrastructure has had a larger effect than climate change might have is in general not surprising, although a detailed discussion could be useful, but this comparison actually can't be found in the manuscript! Also the conclusion on the importance of ice melt is not new and given the crude representations of glaciers I am not sure what new insights this study provides. My other concerns are the not fully satisfactory presentation and the lack of an uncertainty assessment. As already pointed out by referee #1 there are a number of small issues like places where the language needs to be improved or where the reader has to guess what actually has been done. For instance, it remains rather unclear how the factors of change have been derived (additive/multiplicative, seasonal variation, ...) how these change factors relate to the (which?) bias correction that apparently has been used. As a reviewer it is an awkward situation if you have to guess what might have done!

The authors argue against model calibration. Even if model parameters in TOP-KAPI have a physical meaning, many of them (such as the degree-day melt factor or the outflow coefficient of the linear groundwater reservoir) are not measurable at the catchment scale. The argument that automatic calibration would result in a poor parametrization might be valid if one would calibrate looking only on something like the RMSE of runoff. The advantage of manual calibration is that hydrological understanding can be considered, but this understanding can to some degree also be considered in automatic calibration if the objective function is formulated in a good way. Using automatic calibration would require to explicitly state the calibration criteria (which actually would be quite useful). A somehow automated calibration approach would also have the benefit to allow for an uncertainty assessment, which is largely missing so far.

To summarize, while the authors put great efforts into their model development, the manuscript in its present form is lacking a clear focus. As outlined above, there are possibilities to go into different directions and if the authors can address all the concerns expressed above and by the other more detailed comments, the manuscript might make an interesting contribution. In its current form, however, I have to admit that I do not really see, what exactly the scientific contribution of this manuscript is (i.e., what have we learnt by this study?).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3743, 2013.