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Hydrology and Earth System Sciences Discussions

Interactive comment on "Impacts of climate change scenarios on runoff regimes in the southern Alps" *by* S. Barontini et al.

Anonymous Referee #2

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The paper by Barontini et al. presents a study in which the hydrological impacts of climate change scenarios on the runoff is studied for two catchments in the Alps. Such impacts might have significant consequences especially because of the role of the Alps as 'Water tower'. In this study scenarios of one GCM (PCM) are used to study both how well today's runoff is simulated and how runoff might change due to different scenarios of future climate. These are important questions and, thus, the paper is highly relevant. Having said this, I also have to say that I have major concerns with the paper as it is now. Referee #1 already had a long list of detailed (and good) comments, I won't repeat those here. My major concern is that the hydrological model application does not follow good modelling practises.

One issue is the use of GCM in an area of high topographic variability. There might be arguments to use GCM simulations instead of RCM simulations, e.g., one might argue that even with a RCM the topographic effects in the Alps are not realistically simulated. However, just ignoring the existence of RCMs is not appropriate. For whatever reason you decided to use GCMs, please discuss these!

The authors started using three GCMs but then decided using only one for most of the analysis. As reason it is stated that PCM performed best. From Fig 2 it is not obvious that PCM clearly is the best model. In any case, it would be interesting to perform the entire study with all three GCMs.

A major concern is the way the hydrological model is applied and evaluated. To me it remains rather unclear how the model has been parameterized. Was there any kind of calibration? If yes, how? If not, how were the parameter values chosen? To demonstrate the validity of the model the authors plotted observed and simulated runoff (based on observed climate) in Figure 5a. From this plot it is rather difficult to see how well the model performed. Why not providing any goodness of fit measure such as the model efficiency as it is standard in hydrological modelling studies? (p. 3117)

Figure 5b is rather surprising to me. Here the authors state to present the "historical simulation driven by PCM-based downscaled data". I have difficulties to see how one can compare observed runoff with runoff simulated based on a GCM simulation on a day-by-day basis. A GCM does not reproduce the weather for a certain day!!! (p. 3117)

The only quantitative model evaluation provided in the paper is the correlation coefficient on a long-term monthly basis. This is a poor evaluation for two reasons. By aggregating the data to long-term averages any model errors on shorter time steps or inter-annual values can be cancelled out. The only thing this evaluations can tell ist that the model gets the seasonal variation on average about right. In a snow-dominated this is no real challenge! And if one looks carefully there are actually some rather significant errors which are hardly discussed in the paper. Secondly, the correlation coefficient is no suitable objective function.You could get a perfect fit (i.e. value of 1) even if runoff is simulated very poor (e.g., always double runoff). (p. 3117)

The hydrological model is used for simulations with a changed land-cover (shift of tree line). It is a good point to include this change (which often is forgotten in hydrological climate impact studies), however, I would like to see some indication why we should believe that the effect of different land-covers is simulated correctly by WATFLOOD. (p.3112)

A general remark on the modelling: what about uncertainties? How sensitive are results to the actual parameterization of the hydrological model, or vice versa, how sure can we be that the used parameterisation is the 'correct' one?

Another problematic issue of the model is the treatment of glaciers. It remains unclear to me how the equilibrium line approach was used to consider the "actual disequilibrium". (p. 3113)

The downscaled PCM precipitation overestimates precipitation during fall (Fig 6, August-Oct). I wonder why the authors chose to use monthly correction factors for temperature but not for precipitation. To me, the problem of the GCMs to get the seasonal distribution of precip right would motivate monthly varying correction factors. (p. 3102 ff)

Please change the units in Eq 28 to SI units (i.e. no Fahrenheit) (p.3115)

The paper would greatly benefit from carefully copy-editing the language. Just as an example you might look at the last sentence: do the authors really mean these results are useful "because" of the uncertainties?

The structure also should be improved, including shortening the introduction by about 90% and providing more information on the methods used in this study.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 3089, 2009.