

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

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I think we have had two excellent and comprehensive reviews and I thank the reviewers for their timer in evaluating this manuscript. Both raise some substantial issues and the feeling is on the whole that this is a paper that needs considerable improvement in a number of key areas, which must be addressed within the revised manuscript, else the paper should be considered for rejection to HESS. However the reviewers recognise the contribution and it's topic is very relevant for HESS. Both reviewers comment on the need to justify a CGM approach to assessing and I would say more importantly the difficulties of downscaled values to be meaningful at the scales applied and the nature of the heterogeneous landforms this area is based in. This is not critically discussed

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and should be. The nature of only using PCM is well made by reviewer #1 and I don't think the response adequately resolves this issue, table 3 hardly provides compelling evidence that PCM is so much better than ECHAM (for example). In a similar form I think reviewer #1 is correct about the application of the model. Similar comments could be made about the quality of the hydrological model and the uncertainties in the modelling process, again these issues are brought out by the reviewers and they are important I should note a this point a number of the comments generated by the reviewers followed my own initial rejection of the manuscript in first submission. I did not wish with the second submission to be the only person assessing the paper and hence it went out for review, but that is not to say there are some serious concerns with the paper which the review process has now also highlighted. I refer the authors to my initial evaluation also much of which is still relevant to the current manuscript. My suggestions are the following for the manuscript to be considered for HESS. 1) Major changes need to be made, this relates primarily to a more critical analysis of the procedure used and some better evaluation of the quality of the simulations (GCM and hydrological model) and how this type of modelling chain can be used to understand climate change within such landscapes and at such scales. The authors have to fully justify (or change their methodology) of using such a limited combination of models to benchmark a climate change and it's impact on the hydrological regime. They suggest they provide a 'reference' for such studies (near the end of the discussion section), I am not sure as yet they can say a reference is proven within the methodology they use because I and the reviewers cannot see how you can benchmark such uncertain results without dealing with the uncertainty in the predictive capability of the GCM modelling, the downscaling approach and the hydrological predictions. 2) I would suggest the downscaling techniques could be moved (with reduced text in between) to a appendix section, this is too detailed for this part compared to the main issues the paper should address about doing such modelling. Similarly detail such as describing the A1, B1 etc. scenarios is not really required and this needs to be referenced but not written in detail as current. The introduction again could be shortened in numerous areas to

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focus correctly on the more critical aspects of the paper. 3) The authors either need to do or critically discuss the lack of uncertainty within their methodology, is this realistic?, is this appropriate?, how much are the results flawed by not taking into account the general quality of the simulations and how well these quantify the observations? Can we really drive the imperfect results of one deterministic hydrological model from one downscaled GCM (which from figure 5 is hardly showing perfect simulations) and say something meaningful for the future? These issues have to be dealt with effectively and they are not at present in the manuscript.

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

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