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Hydrology and Earth System Sciences Discussions

Interactive comment on "Development of streamflow projections under changing climate conditions over Colorado River Basin headwaters" *by* W. P. Miller et al.

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Dear colleagues,

First can I thank everyone for a set of comprehensive reviews and author responses and the effort that the authors have put into these responses and the improvement in the current iteration of the paper. I have taken some time to think about these issues and I hope what i provide is a balanced assessment. What was clear from the

reviewers that there was work to do on this paper and many good discussion points in my opinion were provided. Certainly there was a need for a greater link to previous work in this area and a better clarification of some of the techniques. What is clear for all the hydrological community is that dealing with climate change impacts is not easy and indeed it is easy to criticise techniques because of the limitations and uncertainties in the driving (climate change) data. However I consider the fact that this paper tries to resolve water resource issues at large scales for operation purposes and the need for evaluations to be considered by this branch of the forecasting community puts into context the methods chosen. They have tried to deal with the uncertainties in these forecasts as far as the ensemble of GCM simulations within their forecasting system (many authors have not done this). I read the current version of the paper in some detail and also read the responses and comments fully, these are my main conclusions:

1) The authors still fail to make as clear as it could be the interaction between the VIC model and the evaporation outputs to the NWS RFS forecasting model and how these all combine into a post bias corrected output. This section is still not clear to read and understand and it should be. I personally suggest a series of concise steps are needed in a section to clearly explain the rationale and the cascaded modelling approach taken.

2) The authors have done a good job on linking to previous work in this area, I am happy with what they have done. What is a pity that they have failed to do is bring that into the final discussion to compare findings in a more interesting discussion. I still believe this has value to round off the paper and put it's findings in context

3) There were consistent comments from the reviewers about the need to understand the quality of the outputs of these models used to predict the climatic change impacts and indeed the methodologies used to ensure they have good model skill. The authors have counteracted suggesting the metrics to determine this are not available. First in a scientific journal this is not a satisfactory response in my opinion given how the rest of the paper has been improved. If such metrics are unavailable then the authors at

the very least have to ask themselves some hard questions in the evaluation of the results and how they can be confident about the results. This is important throughout the paper, we have to remember results are being biased corrected from the GCM cascaded inputs to make them exactly the same as the forecast results, in this case there needs to be a strong basis of why this should be the case as they completely determine the range of climate change behaviours into the future

4) Following on from above it is interesting why the authors force the bias corrections to perfectly match the mean CBRFC behaviour given the confidence intervals shown, isn't this in a sense mixing a deterministic point of view with something that is clearly quite uncertain? I think given the reviewers comments more need to be discussed about the limitations of the bias correction and that if the authors stand by their assessment then more critical discussion of the assumptions of doing this must be made (if I have this right they should reflect on why 112 GCM scenarios are used but only one VIC model simulation to quantify evapotranspiration changes and without knowedge of the model skill / performance). I really think this final discussion is key to the paper and gives the authors the opportunity to think what has been possible here and therefore what needs to be done to improve this assessment making sure they provide the level of confidence (qualitative or quantitative) they would consider their results to have for policy making decisions and long term evaluations of change. What are the clear assumptions and what are the challenges to improve such assessments. I would also quickly note that the results in Table 3 suggest to bias correct to the mean suggests the spread of simulations into the future may have become unduly wide if the skew and maximum streamflow metrics are considered but no comments seems to have been made of this characteristic of the results, should this be considered?

5) Finally a quick couple of points. There is a reference to O et al. on page 22 and at least one other place. I would also say the title for Table 4 is not at all clear and needs to be improved.

So I think the paper will be fit for publication in HESS if these points above are consid-

ered by the authors. I suggest the authors can easily alter the manuscript to complete these comments I have made above about their current iteration (but they will need to take time to develop an effective discussion of their results). I would then like to send this paper out for a final review due to the considerable number of changes in the manuscript to complete this process, kind regards, Jim freer

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5577, 2010.