**Hydrology and Earth System Sciences Discussions**

Interactive
Comment

# *Interactive comment on* "High-resolution projections of surface water availability for Tasmania, Australia" *by* J. C. Bennett et al.
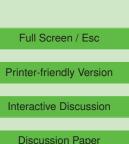
**Anonymous Referee #2**

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Referee report/comments on "High-resolution projections of surface water availability for Tasmania, Australia" by J. C. Bennett et al.

The paper deals with an important field of research and it is well written. But I have some major issues with the methodology/techniques used to get the current and future climate (rainfall and APET) projections through RCM downscaling.

As per my knowledge, CCAM itself is a GCM in which you can use a nested grid to get high resolution projections for specific areas. The authors need to explain clearly (in a paragraph or two) as to how they can force it with just SST as forcing data from 6 GCMs. It appears that the SST forcing from the GCMs will have minimal effect on the outputs from CCAM as all the other boundary conditions for the area of interest (Tasmania) will be from the base CCAM run.

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How big are the GCM SST biases? I do not have a problem with bias correcting the inputs (such as what is done here for SST) but the authors need to explain how they and the readers can be sure that the bias correction for historical data for SST (1961-2000/7) is valid in the future, especially when you are applying the same correction for 2070-2099 (about 100 years ahead in time). The authors need to do a validation test using part of the observed SST data (say 1961-1980) where they develop the correction coefficients/factors and then show that they work fine for the other part of historical SST data (say 1981-2000/7). This should be shown in the paper so that we can have some confidence in the results for the future.

I have major issues with bias correcting the outputs from RCM downscaling (rainfall and APET). It is not standard way to bias correct the outputs (there are a number of statistical downscaling papers which all bias correct the inputs but not the outputs). The authors can go one step further and just bias correct the RCM runoff so that they do not need to use any hydrological model? Also, the bias correction for rainfall and APET is done at a grid cell scale which means that the spatial correlation between different grid cells within catchments is not maintained. The authors have stated that they are under predicting rainfall/runoff using the bias corrected rain and APET. This is to be expected as the high runoff events in a catchment are associated with majority of the grid cells within the catchment getting high rainfall on the same day or two which can not be captured properly when you do cell by cell bias quantile-quantile mapping (it is done on ranked values and so timing is not considered). Again, as I said above for SST, the authors need to explain how they and the readers can be sure that the bias correction for historical data for rainfall and APET (1961-2000/7) is valid in the future, especially when you are applying the same correction for 2070-2099 (about 100 years ahead in time). The authors need to do a validation test using part of the observed rainfall and APET data (say 1961-1980) where they develop the correction coefficients/factors and then show that they work fine for the other part of historical rainfall and APET data (say 1981-2000/7). This should be shown in the paper so that we can have some confidence in the results for the future. The authors say in the discussion very briefly that when

they tested different periods for calibration and validation, they got very different results. If that is the case then any of the future rainfall and runoff projections are not reliable and valid. Another way of doing the bias correction may be to use all the odd years to get the factors and then test these factors for all the even years. This way you will overcome to problem of under/over estimation of rainfall from RCM in different periods.

The authors seem to put one reason for any issues with the runoff projection – problem/deficiencies in the hydrological models. This is not true, for example, the deficiencies in low streamflow that you are getting is not because of the problems with the hydrological models BUT because of the objective function that you have used to calibrate the hydrological models. The NSE-Bias objective function effectively calibrates the model for daily Nash-Sutcliffe efficiency (NSE) while making sure that the total bias is small. As such it is putting all the weight on the high and medium flows (with little to no weight for the low flows). The problem with large daily flows overestimation is again attributed to hydrological models. This is not correct.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1783, 2012.

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