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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.

Anonymous Referee #1

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This paper uses downscale climate change projections to estimate changes in streamflow in the Colorado river basin. In recent years there have been a number of papers that have used of downscale GCM data to drive hydrologic model projections has been used to predict hydrologic responses to climate change. The authors in this paper focus specifically on changes in evapotranspiration rates and an agency forecasting model. Other hydrologic models that have been used in the Western US include changes in evapotranspiration rates in their approach (eg. VIC, DHSVM, RHESSys) although none

explicitly interpret results in the context of reservoir management using a forecasting model. This is an interesting idea - but it is not developed enough to really explain the value of this paper to the general hydrologic community. The authors should cite (and compare their paper to) examples of previous papers that examine changes in evapotranspiration with warming in the Western US (using both empirical and modelling approaches). The key difference between these other papers and this paper is the methodology - it would be helpful to explain to the readers how this approach compares with other hydrology models that more directly include mechanistic representations (such as Penman Monteith) of ET.

In general the paper assumes familiarity with US institutions and policies, agency modelling approaches and current agency approaches to climate change assessment in the West - this is problematic for an international journal - The methodology and results from this paper do say something interesting about climate change impacts and modelling climate change impacts from an agency perspective that would be of interest to many - but the authors need to do a better job of providing this contextual information and removing jargon associated with US water management.

I also found the method section difficult to follow and feel that it needs more development. As written, it was not clear to me why a) the authors did not simply use VIC predictions of streamflow - I suspect this has to do with how the RFS model is used but for those not familiar with RFS this is confusing b) if RFS is used, why it was not recalibrated with the VIC-derived ET incorporated for historic periods - why is the ratio method used? Re-calibrating with the “improved model” for historic periods would presumably improve calibrations and make the model more robust in a changing climate. Perhaps I do not understand the RFS calibration process - but that should be clear from the paper. I suspect that the reason why the authors chose their approach has to do with the use of RFS as a forecasting model - OK - but this needs to be presented to the reader - what is different about forecasting models - how is this model calibrated, etc. The paper needs a substantial rewrite to explain the RFS modelling

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approach, its calibration, why ET needs to be incorporated etc. There are also a lack of presentation of model assessment statistics that would build confidence in the paper results. It is not clear whether relatively small changes in predictions that occur when ET is included are really significant given model uncertainty (that might be estimated by looking at performance during historic periods). Similarly additional justification for some of the choices made is needed e.g how to vary precipitation and temperature in space is needed.

Some detailed comments/suggestions Pg 5579 - in the introduction it would be helpful to be more specific about previous studies in the Western US and projected changes - for example - line 15 states that previous research indicates “warming trends” - clarify the magnitude (or ranges in magnitude/direction) of these changes? - similarly the introduction notes “changes in timing of streamflow” - but does not explain how the timing of streamflow has changed.

Pg 5582 line 5 - “Research on the impacts of teleconnection events on drought and streamflow conditions in the Green River Basin have provided some insight as to the role of climate variability 5 over the Colorado River Basin (Tootle and Piechota, 2003)” - This sentence is vague - it would be helpful to say more of what this insight is

“Pursuant to the National Environmental Protection Act (NEPA) of 1969, an Environmental Impact Statement (EIS) and Record of Decision (ROD) were published in 2006 defining the operations of the Navajo Reservoir within the San Juan River Basin to aid in the conservation of endangered fish species, habitat, and continue to meet ReclamationÊijs obligations to 10 water delivery requirements and Native American water rights (US Department of the Interior, Bureau of Reclamation, Upper Colorado Region, 2006).” - While this may be true it is not clear how this statement contributes to a paper for a general hydrology audience - either remove or link these policy statements with the specific goals of the paper

In the “study area” section, some general information on the hydro-climatology of the

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basin should be provided, particularly given that this is an international journal. Provide information on mean annual precipitation, streamflow, seasonality, elevation ranges, snow versus rain etc.

Again in the study area section (pg 5582 line 1-5), statements are often vague and it would be helpful to provide more information on what previous studies have shown regarding climate change in the study basin eg. what did the studies of using "down-scaled climate projections" show?

Pg 5580 - how specifically does the Colorado River exhibit non-stationarity, line 14

Pg 5580, line 18 - again be a little more clear about how runoff event impact the operation of reservoirs - a key issue is what level of error in prediction is likely to alter decisions - providing more specific information on types of decision made would help reader to interpret the relevances of changes in prediction shown here.

Pg 5580 - provide some additional information on the RFS - in particular information on the type of hydrologic model (so it is clear to the reader why information on evapotranspiration is needed) - Providing performance statistics for prior uses of this model in the study basin would also be helpful if available.

Pg 5584 line 13 - again I disagree that changes to evapotranspiration rates have not been considered in hydrologic models - there are published examples from the western us that should be cited and compared with results here

Pg 5584 - line 18 - again here it is not clear to the reader why VIC is not used directly to predict streamflow (since it already incorporates ET)- additional information on how different models are used by the agencies involved is needed.

Pg 5585 - line 8 - explain how potential ET is reduced when area is not saturated - this is critical since in many cases warming will reduce area saturated, increase drought stress and reduce actual ET - so it is important to clarify how AET/PET is determined

Pg 5585 - line 20 - this section needs a clearer description of how ET from VIC is used

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in RFS - as presented here it sounds like evapotranspiration rates are assumed to be those predicted by VIC given a 1C warming - but then why not use VIC-ET estimates from downscaled climate data.

Pg 5585 - line 22 - why is this study not able to recalibrate - it seems to me that recalibrating for historic period where ET is incorporated would be appropriate! Pg 5586 - line 1-10 - some of this information would be helpful earlier so that the reader can better understand how ET predictions discussed in the previous section will be used

"The NWS RFS model used here was provided by the CBRFC and is run in calibration mode; that is, the model is run without the calibration model that is typically run in parallel with the model at the CBRFC. This calibration model is run to calibrate streamflow output from the RFS to observed streamflow from gage records." This sentence is very hard to understand if you are not familiar with their calibration approach - what does it mean to run a calibration model in parallel?

Pg 5586 - line 15-20 - mean area temperature (at what time scales?) - these are derived from gages? how?

"The NWS RFS model provided by the CBRFC relied on values of evapotranspiration demand unique to each month; that is, evapotranspiration demand in any given 25 month is identical throughout the length of the model run." This is a KEY statement - and the reader need to know this much earlier in the paper -

Pg 5588 - Recent studies (eg.Linquist et al., 2009) have shown that accounting for spatially variable temperature lapse rates can be critical in predicting snow accumulation and melt / streamflow - how are lapse rates with elevation determined here to downscale from 1/8th degree cell to elevation bands within catchment? are 3 elevation bands sufficient?) -(Note if statistics on prior model performance were given this would help convince the reader that their approach is reasonable). Similarly a key challenge for hydrologic modellers in mountain environments is interpolating precipitaiton data

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over space - it is not clear how the authors have addressed this issue.

Pg 5588 -give ranges of elevation bands - why 3? is that sufficient?

Pg 5598 - line 15-20 - I agree with the authors the ET is a sensitive and important parameter - but I think there are other sources of uncertainty in ET predictions that should at least be mentioned that are not accounted for by their approach. Consider for example the potential impact of increased water use efficiency with elevated CO2, or changes in land use/land cover (see paper by Cuo et al., 2009) as an example.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/7/C2702/2010/hessd-7-C2702-2010-supplement.pdf

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