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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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RESPONSE TO ANONYMOUS REFEREE #2 COMMENTS Streamflow Projections Under Changing Climate Conditions over Colorado River Basin Headwaters

Responses to Anonymous Referee #2 Comments

âĂć Major Comment 1 (Part 1): The authors characterize the major contributions of the



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article in the introduction and summary discussion: – p. 5581, lines 6-9: "The development of a methodology to develop streamflow projections for use in Reclamation river and reservoir management models is described. An important contribution of this work is the evaluation of the impact of changing climate based on changing evapotranspiration rates." – p. 5598, lines 12-14: "A major contribution of this study is that by adjusting evapotranspiration with temperature, catchment streamflow projections better reflect the potential impacts of climate change." Based on my review, I cannot agree with the authors about these contributions. – Much of the authors methodology appears to follow Wood et al. 2002 and Christensen and Lettenmaier 2007 (C&L2007). The only departures from the latter is the need to pre-process an ET-demand adjustment (which is triggered by the decision to use SacSMA rather than VIC) and the need to temporally disaggregate monthly BCSD T and P data to sub-daily forcings required by SacSMA (C&L2007 used a daily VIC application). – Many studies have illustrated the potential non-stationarity of runoff under projected climate change over the Colorado River basin. Of these studies, most feature simulated runoff impacts under projected climate conditions, which involves models that also simulate actual evapotranspirtation (AET) constrained by a potential ET (PET) demand. A subset of those studies featured use of a hydrologic simulation model (e.g., Christensen et al. 2004 (C2004) and Christensen and Lettenmaier 2007 (C&L2007)) or GCM-output analysis (e.g., Milly et al. 2005 (M2005)) here runoff impact portrayal reflects how climate change affects PET. So given this context, I can't agree with the contribution statements.

Major Comment Response 1: The authors appreciate this, and all, of Anonymous Referee #2's comments. We attribute most of the reviewer's concern that our initial draft of this article did not stress the importance of the NWS RFS to operations within the Colorado River Basin. We have modified our manuscript to read on page 10, lines 8-10 of the revised manuscript:

Reclamation is required to use streamflow forecasts by the CBRFC for input into operational and policy models; as such it is important for Reclamation, or any water manager,



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to evaluate potential impacts of climate change to streamflow forecasting tools.

Furthermore, we recognized and greatly appreciate the contributions that Wood et al. (2002) and Christensen and Lettenmaier (2007) have made. We feel that whereas Wood et al. (2000) and Christensen and Lettenmaier (2007) presented valuable research regarding the use of downscaled climate data to project streamflow using the distributed VIC model, we have presented a methodology to incorporate distributed climate data into a lumped model environment. We have modified our contribution statements to read on page 5, lines 12-14 of the revised manuscript:

The development of a methodology to develop streamflow projections for use in Reclamation river and reservoir management models is described. An important contribution of this work is the evaluation of changing climate to changing evapotranspiration rates.

and to read on page 28, line 22 through page 29, line 1 of the revised manuscript:

A major contribution of this study is that by adjusting static evapotranspiration demand with temperature within a lumped model, catchment streamflow projections better reflect the potential impacts of climate change.

âĂć Major Comment 1 (Part 2): I do feel that the authors have done a significant amount of work and that with some restructuring of the results analysis, a paper can still be generated from this effort. In the restructuring, I would suggest eliminating any focus on nonstationarity since this has been illustrated in previous research contributions (C&L2007, M2005) and is largely a matter of assessment at this point). Instead, the analyses could address the potential objectives of (1) attributing projected runoff impacts to respective changes in AET and PET, (2) geographic variations in this attribution, and (3) uncertainties about this attribution particularly with respect to PET sensitivity to temperature change. Regardless of this restructuring recommendation, the authors need to explain their choice of using a legacy hydrologic model like SacSMA (which does not internally simulate PET and instead requires pre-simulation definition of potential ET (PET)) instead of a more contemporary hydrologic model that computes



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PET innternally (like VIC, which was used in the C2004 and C&L2007 efforts). Under the restruction recommendation, the potential objective (1) sets up a reason for using SacSMA in that SacSMA permits defining PET sensitivity to warming outside of the simulation, which allows the authors to explore the PET attribution question. On potential objective (1), the authors should clearly separate attribution of runoff impacts to changes in AET and PET. Also be aware that the SacSMA simulations will simulate different AET under changed climate relative to historical, even if PET is kept unchanged (due to change in precipitation regime and how that subsequently affects hydrologic processes). On potential objective (2), I'm reacting primarily to figure 4, 8, 10, which are all nice displays. However, I'd like to see the authors offer more interpretation on geographic attributes of runoff impact sensitivity to ET adjustment (see part (2) of the restruction recommendation under major comment). For example, for their three large basins, did their sub-basin assessment typically show runoff impact sensitivity to be greater in certain types of subbasins? I'd suspect this possibility for low-elevation, arid, rainfallrunoff dominant basis... regardless, I'd like to see more discussion on why results geographically varied. Are these interpretations common across the three basins? Also on Figures 8 and 10 - suggest making like Figure 4 and showing maps based on simulated runoff with and without ET adjustment. On potential objective (3) and characterizing attribution uncertainty, I'd recommend that the authors add an empirical temperature-based method of PET adjustment (Haman? Hargreaves?) and adjust SacSMA PET inputs based on that method. Such SacSMA simulations would complement the results where SacSMA PET inputs have been adjusted based on the projected T and VIC-simulated PET(**) sensitivity to T change (which is essenstially a Penman-Monteith PET sensitivity). (** - Note, it was unclear from the manuscript whether the authors used VIC simulation to assess AET sensitivity to 1degC warming or PET sensitivity to 1degC warming. It should be the latter since they are using those sensitivities to adjust the ET demand inputs for SacSMA (ET demand _= PET). Also, I wasn't sure whether the authors meant to discuss AET or PET when they introduced equation 1.)



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Major Comment Response 2: Thank you for your comment. We appreciate your revised restructuring suggestions. In fact, increased investigation into changes in both potential and actual evapotranspiration rates due to climate change seems very interesting and one worth pursuing. We think that research is better done with a distributed model, such as VIC, since the lumped aspects of the NWS RFS would limit analysis of topographic impacts. We have clarified ET demand within the revised manuscript.

Other Comment 1: p. 5580, line 5: acronyms AMO, PDO, SOI should be written out

Other Comment Response 1: We have revised the manuscript to spell out the acronyms.

Other Comment 2: p. 5580, lines 5-8: "Drier conditions in the American West have persisted since 1999. In contrast, 6 of the 10 warmest years occurred between 1986 and 2000 and have continued to persist throughout the southwest." I'm not following how these two sentences are comparable. Also, the phrase "6 of the 10 warmest years" doesn't have context -say during what period.

Other Comment Response 2: Thank you for the comment. We have revised the manuscript to read ong page 4, lines 2-4:

Drier conditions in the American West have persisted since 1999. Timilsena et al. {{75 Timilsena,J. 2007/a;}} identifies the 2000 through 2004 period within the Colorado River Basin as one of the most severe over the past 500 years.

Other Comment 3: p. 5580, line 22 - p. 5581 line 5: It would be more appropriate to say that this study follows C&L2007 and Raff et al. 2009. It follows C&L2007 on the development of transient runoff projections for the Colorado River Basin, consistent with BCSD monthly climate projections that have been time-disaggregated to a sub-monthly time step using an historical resampling technique (Wood et al. 2004). It departs from C&L2007 and follows Raff et al 2009 on the matter of model choice (SacSMA apps from the NWS CBRFC rather than VIC), which necessitates external



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adjustment potential evapotranspiration (input to SacSMA versus internally computed in VIC).

Other Comment Response 3: Thank you for the comment. We have revised the manuscript to read on page 5 lines 5 -11 of the revised manuscript:

Other recent studies have developed alternative methodologies for incorporating temperature and precipitation patterns over the Upper Colorado River Basin (Matter et al. 2010). Christensen and Lettenmaier {{73 Christensen,N.S. 2007/a;}} has previously used downscaled projections of precipitation and temperature to develop transient projections of runoff over the entire Colorado River Basin using the distributed Variable Infiltration and Capacity (VIC) model. Although this study utilized information from the VIC model, the models and data sources presented in Raff et al. (2009) are very similar to the models and data sources utilized in this focus of the study.

Other Comment 4: p. 5582: Section 1.2 ... – Suggest rethinking the organization of this section. , –1.2.1 This might be the only section you keep in 1.2. Define the period of the projections (1950-2099?)Suggest using Maurer et al. 2007 to reference the archive (Maurer, E.P., L. Brekke, T. Pruitt, and P.B. Duffy (2007). "Fine-resolution climate projections enhance regional climate change impact studies," Eos Trans. AGU, 88(47), 504." ... Also suggest referring to this information as BCSD CMIP3 projections (rather than data and rather than WCRP CMIP3... just to distinguish these projections from the actual coarse-scale WCRP CMIP3 projections). – 1.2.2 I'd omit description of emissions scenarios and only briefly reference them when you introduce the monthly BCSD CMIP3 projections. – 1.2.3 Discussion of evapotranspiration projections should be moved after discussion of the two models used in the effort, SacSMA and VIC (section 2.1). Suggest contrasting the two models, particularly on their handling of AET and PET, explain interest in using SacSMA for your attribution interests, and then how VIC is used to provide one basis for adjusting PET inputs to SacSMA.

Other Comment Response 4: Thank you for the helpful comments. We have refer-



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enced Maurer et al. (2007) and have changed references to the WCRP CMIP3 dataset to BCSD CMIP3 dataset where appropriate. Thank you for your suggestion regarding the reorganization of section 1.2.3. We have relocated it such that it is now section 2.2.

With regards to section 1.2.2, we have found that a brief description of emissions scenarios helps to put the results displayed in figure 6 into perspective.

With regards to comparison between the VIC and RFS models, we hope the following paragraph inserted into section 2.2, lines 4 -14 of the revised manuscript meets your concerns satisfactorily to read:

Evapotranspiration within the VIC model has been extensively studied (e.g., Christensen and Lettenmaier 2007, Hamlet et al. 2007, Hurkmans et al. 2008, Hurkmans et al. 2009, Lakshmi and Wood 1998, Nijssen et al. 1997). Of particular importance to this study Hamlet et al. (2007), indicated that evapotranspiration trends within VIC were driven by trends in precipitation and temperature; concurrent work indicated that evapotranspiration significantly influenced projected streamflow response within the VIC model (Christensen and Lettenmaier 2007). An advantage of the VIC model, and other hydrologic models discussed, over the NWS RFS utilized by the CBRFC is that these models allow for the user to account for evapotranspiration as a function of changing conditions within the model. The NWS RFS utilized by the CBRFC is dependent on user-defined evapotranspiration demand that is unique to each month; that is, evapotranspiration demand in any given month is identical throughout the length of the model run.

Other Comment 5: p. 5582, line 16, "As previously described..." This sentence and the following sentence can be combined - sounds redundant.

Other Comment Response 5: Thank you for the helpful comments. We have revised the manuscript to read on page 7 line 21 though page 8 line 1:

This climate data has been downscaled to 1/8th degree (approximately 12 kilometers



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or 7.5 miles) grid cell resolution, making it more useful for regional hydrologic analysis. This data have been downscaled using the BCSD technique described in Wood et al. (2004) and is available at a monthly timestep.

Other Comment 6: p. 5583, line 1-14: what is this effort being mentioned? how should the reader regard this ongoing VIC effort relative to the SacSMA effort in this paper?

Other Comment Response 6: Thank you for the helpful comments. We have revised the manuscript on page 8 lines 11 through page 9 line 3 to more explicitly define the role of the VIC model.

Currently, Reclamation is developing streamflow projections over the Upper Colorado River Basin using the VIC model and the BCSD CMIP3 dataset described in this study for the Colorado River Basin Water Supply and Demand Study (Basin Study) to examine the impacts of changing water supply and demand conditions over the Colorado River Basin (U.S. Department of the Interior, Bureau of Reclamation, Lower Colorado Region 2009). The VIC model is run at a daily timestep; as such, temporal disaggregation of data from the monthly BCSD CMIP3 dataset over the Colorado River Basin is required. Temporal disaggregation of the monthly BCSD CMIP3 data was accomplished by scaling historical daily precipitation or shifting historical daily temperature data to match monthly time series data (Wood et al. 2004). Daily precipitation and temperature time series have been derived for the entire spatial and temporal extent of the monthly Reclamation, LLNL, SCU dataset, and are archived at the Department of Energy (DOE) National Energy Research Scientific Computing (NERSC) Center.

To facilitate future research comparing streamflow projections from the Basin Study and streamflow projections developed herein, daily climate data utilized in the Basin Study is also utilized in this effort.

Other Comment 7: p. 5583, lines 10-14: Later the methodology talks more about using time-disaggregated (monthly to daily) versions of the monthly BCSD CMIP3 projections. How was this timedisaggregation performed? Citation?



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Other Comment Response 7: Unfortunately, this data has not been cited as of yet in the literature, though it was obtained directly from Andy Wood and he provided guidance regarding the citation of the data. The temporal disaggregation was done as described in Wood et al. (2004), with adjustment for the smaller timestep.

Other Comment 8: p. 5584, lines 14-16: Not really true - there have been hydrologic impacts assessments where potential and actual ET are computed internal to the model (e.g., using VIC and other land surface models). I think it's more appropriate to suggest that past studies have not focused on how warming-related changes in AET and PET relatively contribute to runoff change, and that annual runoff changes are not just a function of precipitation and snowpack changes.

Other Comment Response 8: Thank you for the comment. We have revised the manuscript to read on page 13 line 16 - 18:

Changes to evapotranspiration demand with changing climate have seldom been considered when using hydrologic models and projections of climate data to develop projections of streamflow (Brekke and Prairie 2009).

Other Comment 9: p. 5585, lines 21-23: "Results were then averaged..." This sentence speaks to limitations of PET portrayal in SacSMA and calibration of CBRFC's SacSMA apps. This should be introduced before introducing the VIC model and discussing the need to do VIC simulations to characterize PET sensitivities to T change.

Other Comment Response 9: Thank you for the comment. We have revised the manuscript to read on page 13 lines 4-14 of the revised manuscript:

Evapotranspiration within the VIC model has been extensively studied (e.g., Christensen and Lettenmaier 2007, Hamlet et al. 2007, Hurkmans et al. 2008, Hurkmans et al. 2009, Lakshmi and Wood 1998, Nijssen et al. 1997). Of particular importance to this study Hamlet et al. (2007), indicated that evapotranspiration trends within VIC were driven by trends in precipitation and temperature; concurrent work indicated



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that evapotranspiration significantly influenced projected streamflow response within the VIC model (Christensen and Lettenmaier 2007). An advantage of the VIC model, and other hydrologic models discussed, over the NWS RFS utilized by the CBRFC is that these models allow for the user to account for evapotranspiration as a function of changing conditions within the model. The NWS RFS utilized by the CBRFC is dependent on user-defined evapotranspiration demand that is unique to each month; that is, evapotranspiration demand in any given month is identical throughout the length of the model run.

Other Comment 10: p. 5585, lines 23-26: "Although..." This sentence is about the subject of simulated runoff bias. It's a unique aspect of the methodology and should be separated from the discussion on ET sensitivity.

Other Comment Response 10: Thank you for the comment. We have revised the manuscript to read on page 15 lines 7-12:

2.2.1 Calibration of Evapotranspiration Demand In practice, evapotranspiration demands, as well as other parameters, are adjusted by the CBRFC through the use of an external calibration model to more accurately represent observed streamflow conditions. Although this study was unable to use the calibration model used by the CBRFC, calibration of streamflow projections was achieved through the use of a ratio method in post-processing of streamflow output (see Section 2.6 and 3.2).

Other Comment 11: p. 5586, lines 10-14: Is this critical information? If not, omit.

Other Comment Response 11: Thank you for the comment. We believe it is critical information as it relates directly to limitations involved with this study.

Other Comment 12: p. 5586, line 24: Define how "evapotranspiration demand" in the CBRFC practice is similar/different from Penman-Monteith's PET... It seems fair to say that these are artificial PET estimates, greater than the AET simulated in SacSMA, but potentially not equal to PET that might have otherwise been computed using Penman-



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Monteith (PM). This is also a source of uncertainty in the methodology (i.e. using PM-based PET sensitivity from VIC to adjust non-PM PET inputs to SacSMA).

Other Comment Response 12: We appreciate the reviewer's concern regarding differences between the Sac-SMA model and the Penman-Monteith method. We hope that our response to Other Comments 9 and 11 addresses this concern satisfactorily.

Other Comment 13: p. 5587, lines 4-10: This paragraph should be moved before preceding paragraph (following second paragraph in section that introduces MAT and MAP).

Other Comment Response 13: Thank you for your comment, we have incorporated your suggestion.

Other Comment 14: p. 5589, line 16: "average monthly temperature" - averaged over what period in the projection time series? Are you making one set of mean-monthly ETt values for each climate projection, or are you making a time-series of running mean-monthly ETt values for each climate projection?

Other Comment Response 14: Thank you for your comment, we have clarified the manuscript to read on page 17 line 21 through page 18 line 2 of the revised manuscript to read:

In addition, 12 base average temperatures were derived for each month for each of the 112 climate scenarios using the 30-year calibration period.

Other Comment 15: p. 5590, lines 7-9: "Additionally, twelve monthly average streamflow projections over the 30-yr calibration period were derived using data from the BCSD, temporally disaggregated WCRP CMIP3 dataset." I'm not sure what's being done here. I understand that the historical sequences in the BCSD CMIP3 projections are statistically consistent with observed T and P during 1950-1999. But the runoff bias-correction period is 1976-2005. This means that the simulated 1976-2005 hydroclimate statistics for each BCSD CMIP3 projection may differ from that of observed



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hydroclimate statistics of this period. So that said, I'm concerned about the choice of this period as the runoff biascorrection period. Second, I'm confused by the statement above. Did you compute a single set of ratio differences in mean-monthly runoff (obs vs. ensemble means), or did you compute projection-specific ratios? Then, after identifying these ratio-corrections, did you apply these ratios to 21st century projected runoff also? Sidebar comment: – Rather than this ratio method, the authors might consider implementing the runoff bias-correction from Snover et al. 2003, which is much like the quantile-mapping bias-correction used to develop the BCSD CMIP3 T and P projections (website has details). The latter approach bias-corrects all period statistics (bias-correction of the distribution) whereas the ratio method only reflects bias-correction in the mean. If the authors opt to not implement Snover et al. 2003, that's fine - but it would be good to acknowledge that option. One caution if Snover et al. 2003 method is implemented and if authors wish to focus on runoff extremes, be sure to consider how the quantile maps must be extrapolated to correct runoff magnitudes from the 21st century that did not occur during the historical period when the quantile maps were defined (i.e. if the map's distribution tails cross, then then the extrapolated correction results in reducing max or increasing min extremes; if the tails diverge, the opposite occurs). For these cases, the authors might consider modifying Snover et al. 2003 to fix ratio changes for runoff magnitudes beyond the range defining the quantile maps (i.e. ratio change at the max quantile is used for all future magnigutes greater than historical max, and ratio change at the min quantile is used for all future magnitudes less than the historical min).

Other Comment Response 15: Thank you for your comment, we have clarified the manuscript to read on page 19 line 5 through 11:

Additionally, twelve monthly average streamflow projections for each of the 112 climate scenarios over the 30-year calibration period were derived using data from the temporally disaggregated BCSD CMIP3 dataset. The ratio of these two values was computed and applied to entire period of record (1950 through 2099) streamflow projection de-



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rived using the temporally disaggregated BCSD CMIP3 dataset. This result ensures that the derived long-term (30-year) mean streamflow from 1976 through 2005 is equal between the original dataset provided by the CBRFC and the BCSD CMIP3 dataset.

The authors appreciate the discussion regarding Snover et al. 2003 and will investigate further in future studies.

Other Comment 16: p. 5590, lines 12-15: delete, or move to beginning of methods section (it's a preview comment).

Other Comment Response 16: Thank you for your comment, we have removed the selection.

Other Comment 17: Prior to Section 3: It would be useful to have information about the quality of the Sac-SMA applicqtion before judging biases like those reported in Table 1. At some point in the paper, suggest characterize the SacSMA bias in mean-annual or mean-monthly runoff when comparing (1) simulated runoff forced by observed historical weather to (2) estimated observed runoff. List bias for at least the outlet locations for the three case study basins.

Other Comment Response 17: Thank you for your comment, unfortunately, due to the inaccessibility of the NWS database, we are unable to do an in-depth analysis in a timely manner. However, we have indicated the CBRFC's streamflow results in Figures 5, 7, and 9 and assume they are close to observed streamflow conditions.

Other Comment 18: Prior to presenting Runoff results in Section 3: Authors should first summarize climate projections over the basin (T and P). This will help interpretation of runoff projections later in Section 3. Next, the authors should summarize the sentivity of the VIC-simulated ET (PET? AET?) to 1degC increase in T (and if a second T-based PET adjustment method is added to the study per recommendation above, summarize those sensitivities as well).

Other Comment Response 18: We appreciate your comment. While this is a valid and



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good request, it is a fairly intensive request that would be difficult to present succinctly in this study. There are multiple considerations to account for when presenting this data which include, but are not limited to:

1. The raw BCSD CMIP3 data distributed at the 1/8th degree interval over the extent of the Colorado River Basin can be presented over multiple time frames. The data of interest in this study is at the daily time step; while this could be aggregated to a more appropriate time step it may be misleading as to the true input data used here.

2. Because the Sac-SMA model is a lumped model, temperature and precipitation characteristics are aggregated based on elevation bands within catchments. The spatial distribution of this data may mislead the audience, since some cells may not lie entirely over a particular elevation range.

We would invite the reviewer and others interested to investigate both the raw BCSD CMIP3 data and the raw timeseries data used in this study. Future work, which focuses solely on the VIC model, would greatly benefit from this presentation.

Other Comment 19: p. 5590, lines 18-24 and Figure 3... nice display, questions/comments: (a) What's a "modified" boxplot? (b) Is the red boxplot showing results from simulated runoff forced by observed weather, or estimated-observed runoff? It would be best to have both. (c) Axes labels are hard to read. (d) Sentence on lines 20-24 ... Are you talking about reduction in some ensemble-median runoff statistic? Which statistic? Is the reduction relative to historical, or is it showing reduction by 2010-2039 with ET adjustment compared to reductionn without adjustment? It would be helpful to just quote reductions by 2010-2039 under both adjustment choices. One remedy might be to modify Table 3, showing historical stats as listed in columns 2-4, but then percentage change relative to historical in the remaining columns

Other Comment Response 19: We appreciate your comment. In response to your comment (a), a modified boxplot is one in which the outer whiskers are defined at the 10% and 90% exceedance values, and the outer edges of the box are defined at the



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25% and 75% exceedance values. This is slightly different from a traditional boxplot. We have removed the term “modified” and instead just describe the bounds of the boxplot to avoid confusion in the revised manuscript.

In response to comment (b), the red boxplot is showing runoff produced from climate (temperature and precipitation) input data files used by the CBRFC which are based on observed values.

In response to comment (c), we have re-generated to the plot to make the axis easier to read.

In response to commend (d), the reduction is to the mean annual runoff. We have clarified this in the manuscript to read on page 19 line 16-20:

Over the 2010 – 2039 time period, adjusting evapotranspiration in response to temperature change results in a decrease of approximately 149 mcm (121,000 acre-feet or approximately 6%) to the mean annual runoff than projections of mean annual runoff made without an adjustment to temperature.

Other Comment 20: (*) p. 5592 - Figure 5 - nice display, but suggest making this plot and others like it a two-panel figure, with one panel showing simulated runoff without ET adjustment and the other showing simulated runoff with ET adjustment. Also, what’s the timestep of runoff on this plot? Annual?

Other Comment Response 20: Thank you for your comment. With some additional time, we can create some plots showing simulated runoff without ET adjustment. We will attempt to do this. The timestep for this plot is annual water year. We have revised the captions below each figure to indicate this.

Other Comment 21: (*) p. 5595, re: section 4 on Stationarity: If the authors follow the restructuring recommendation, I would suggest omitting this section to make room for more discussion on the three suggested objectives listed in Major Comments.

Other Comment Response 21: Thank you for your comment. However, we feel that



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the topic of stationarity is important for water managers, particularly those that rely on historical observations to forecast future operations. We feel that our target audience benefits from this discussion.

Other Comment 22: p. 5595: Table 3 - What is meant by first or third Quantile? Do you mean Quartile? Or 25 and 75 percentiles?

Other Comment Response 22: Thank you for your comment. Yes, quartile is a more accurate term. We have revised Table 3 in our manuscript to reflect this change.

Other Comment 23: p. 5596, lines 8-9: Please clarify... do you mean that KS tests were applied to distributions of BCSD-CMIP3 SacSMA-simulated runoff, where the first distribution is 1976-2005 and the second distribution was one of three 30-year future periods? Also, it seems like lines 6-11 can be deleted and you could just jump to lines 12-20... or vice versa. It doesn't seem like results are too sensitive to emissions path.

Other Comment Response 23: The KS-Test was applied first to compare CBRFC results to the entire period over 2010-1099. It was then applied to each 30-year period separated by emissions scenario. We have tried to clarify this by revising the manuscript to read on page 25 line 18-20:

The KS – Test was then applied between streamflow projections derived by the CBRFC over the calibration period and streamflow projections derived using climate data from the 112 temporally downscaled BCSD dataset over the entire period from 2010 to 2099.

With this distinction, lines 6-11 and 12-20 discuss two separate comparisons, though the difference is admittedly a bit slight.

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