Hydrol. Earth Syst. Sci. Discuss., 7, C2980–C2988, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C2980/2010/

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 #2

Received and published: 15 October 2010

1. Major Comment about Contribution and suggested Restructuring of the Paper

The authors characterize the major contributions of the 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,

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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)) where runoff impact portrayal reflects how climate change affects PET. So given this context, I can't agree with the contribution statements.

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 sim-

HESSD

7, C2980–C2988, 2010

ulate PET and instead requires pre-simulation definition of potential ET (PET)) instead of a more contemporary hydrologic model that computes 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, rainfall-runoff 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

HESSD

7, C2980–C2988, 2010

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.)

2. Other Comments

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

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.

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

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 sce-

HESSD

7, C2980–C2988, 2010

narios 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.

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

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?

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 time-disaggregation performed? Citation?

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.

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.

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.

HESSD

7, C2980–C2988, 2010

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

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-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).

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

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?

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 hydroclimate statistics of this period. So that said, I'm concerned about the choice of this period as the runoff bias-correction 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-

HESSD

7, C2980–C2988, 2010

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).

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

Prior to Section 3: It would be useful to have information about the quality of the SacSMA 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.

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).

HESSD

7, C2980–C2988, 2010

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

p. 5591, lines 8-13, 15-19: Lines 8-13 would be better placed near the beginning of the methods section; lines 15-19 would be better placed in section 2.5.

p. 5591, section 3.2: Delete this section. I assume the intent is to describe the amount of bias in the SacSMA simulations under the historical period forced by BCSD-CMIP3 historical weather vs CBRFC historical weather. As it is, Table 1 doesn't indicate where this bias is situated. Suggested adding text to 2.5 that reports bias in simulated 1976-2005 mean-annnual runoff per basin (see earlier comment).

p. 5592, lines 6-8: This sentence reports a 150-year mean-annnual runoff. This doesn't seem like a relevant statistic given that the point of this paper is how shorter-period runoff statistics are changing with larger scale climate change. Same comment for other basin-specific sections.

(*)p. 5592, lines 16-19, "Of interest...": My initial hunch is that this is an anomalous result affected by the various steps of translating coarse spatial-scale GCM information to monthly BCSD information and ultimately to 6-hourly SacSMA MAT and MAP inputs. However, the authors do not offer enough diagnosis to support or refute this. Rather than spend time in this paper addressing this, I'd suggest that the authors not focus on single-year simulated streamflow extremes from single projections. If the authors

HESSD

7, C2980–C2988, 2010

wish to draw attention to range of variation, I'd suggest commenting on trends in time-series 10%-tile and 90%-tile magnitudes (or some other percentile choices defining a range of interest), and then discuss trend in the spread of the projections' envelope. I also wouldn't focus on emissions characteristics in this paragraph - results do not suggest much variation in runoff impact depending on emissions pathway among the pathways considered. (Related comments: p. 5592, Figure 6 - not sure much is going on in terms of runoff impact differences by emissions scenario. Suggest the authors consider omitting discussion of results by emissions scenario.)

(*) 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?

(*) 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.

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

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.

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

HESSD

7, C2980–C2988, 2010