Evaluation of impact of climate change and anthropogenic change on regional hydrology

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Abstract: General circulation models (GCMs) have been widely used to simulate current and future climate at the global scale. However, the development of frameworks to apply GCMs to assess potential climate change impacts on regional hydrologic systems and compliance with water resource regulations is more recent. It is important to predict potential impacts of future climate change on streamflows and groundwater levels to reduce risks and increase resilience in water resources management and planning. This study evaluated future streamflows and groundwater levels in the Tampa Bay region in west-central Florida using an ensemble of different GCMs, reference evapotranspiration (ET0) methods, and water use scenarios to drive an integrated hydrologic model (IHM). Eight GCMs were bias-corrected and downscaled using the Bias Correction and Stochastic Analog (BCSA) downscaling method and then used, together with three ET0 methods, to drive the IHM for eight different human water use scenarios. Results showed that changes in projected streamflow were most sensitive to GCM selection, however, projections of groundwater level change were sensitive to both GCM and water use scenario. Projected changes in streamflow and groundwater level were relatively insensitive to the ET0 methods evaluated in this study. Six of eight GCMs projected a decrease in streamflow and groundwater level in the future regardless of water use scenario or ET method. These results indicate a high probability of a reduction in future water supply in the Tampa Bay region if environmental regulations intended to protect current aquatic ecosystems do not adapt to the changing climate.

General comments:

The authors have presented an evaluation of the relative sensitivity of a water system in western Florida to a variety of forcings: precip/temp (via GCMs), evapotranspiration calculation method, and human water use scenario. They find that the system is relatively insensitive to ET calculation method, as well as to water use scenario. The authors conclude that the system is most sensitive to GCM projection. The quality of writing is good, and the results figures are professional. However, I have methodological concerns with the work, as well as concerns with the presentation of results. I am not sure that the concerns can be addressed in a straightforward re-write, but maybe they can. Most importantly, I think that the authors need to: 1) better justify the claim that RCP doesn't particularly matter; 2) use a physically-based ET calculation method (and not a temperature based method like Hargreaves); 3) present model calibration/validation for the hydrologic model and the groundwater model, as well as the human water system model; 4) show where the water use scenarios come from and why the authors feel justified in not changing land use. If the authors were able to do all of these things, I would re-review, but if they cannot, I think it would be best not to publish the work in HESS.

Specific comments:

Line 45: the authors note that the GCMs have biases that prevent accurate reproduction of historical hydrological conditions, but do not address those biases. The bias correction and downscaling methods mentioned do not correct for problems with the large-scale synoptic forcing that results in the failure of GCMs to reproduce natural variability (e.g., precipitation timing, variance, low frequency oscillatory behavior), and therefore are not particularly useful for use in driving hydrologic models. They are especially poor at the precipitation extremes (flood and drought). I cannot agree that a climate change analysis should be driven with downscaled, bias-corrected GCM output.

Line 150: only RCP 8.5 was used because previous work showed choice of RCP to be less important than choice of GCM or ET estimation method. And yet you found choice of ET estimation method to be essentially unimportant here. I am suspicious of this claim. RCP 8.5 has very much more ET potential than does RCP 2.6. I would like to see it demonstrated that the difference between those two scenarios is insignificant for hydrology. That has not been my experience.

Line 107: you use HSPF and Modflow in something called IHM, but don't show calibration validation. Calibration/validation is essential for this work. How does the combined tool do with low flow versus high flow? What can you really know about groundwater contribution? A number of statistics are given in this paper (lines 155-160) about actual evapotranspiration over the historical period, but how is this really known? You know precip, and you know streamflow, but you don't know either groundwater infiltration or evapotranspiration, so you're just guessing at which portion is which, aren't you? I'd like to see your confidence in these numbers better justified.

Line 140: why is NLDAS-2 a good choice for bias-correction? What are the accuracy/biases of NLDAS-2?

Line 147: Your historical period is only 24 years. Are you confident that that is long enough to capture variability properly?

Line 200: Please provide calibration/validation results for the AFSIRS model. Is AFSIRS using Penman-Monteith for evapotranspiration? Can Hargreaves really substitute?

Line 243: what does item (7) in this list mean?

Line 247: why is irrigation assumed to be 85% efficient? That seems to me to be an important sensitivity.

Line 289: the only 2 equations presented are poorly described and confusing. Please put in terms of this study. It is not clear how the results are useful and interpretable. Is it a sensitivity in long-term average hydrology? A sensitivity in conditionality? What are the conditional relationships shown in (2)? Very difficult to make sense of how these relationships are applied in the results tables.

Line 331: Only Hargreaves was used. This is hugely problematic. Temperature-based evapotranspiration methods are empirical in nature, and have very high sensitivity to temperature that causes them to "overestimate ET in a way that is greatly at variance with the fundamental principle of conservation of energy at the land surface" (Lofgren and Rouhana (2016) "Physically Plausible Methods for Projecting Changes in Great Lakes Water Levels under Climate Change Scenarios", Journal of Hydrometeorolgy, 17, 2209-2223). You cannot perform a climate change assessment with pumped up temp values using a temp-only evapotranspiration calculation.

Line 474: I don't think the results "clearly show" this. Much hand-waving is done.

Figure 2: I'm not sure that boxplots are the best way to show this. There are trends that get obscured, aren't there?

Figure 3: Where are the historical baselines on these CDF's? How well does each GCM perform relative to the historical? Even after BCSD, probably big misses in retrospective relative to historical observed.

For sensitivity analysis, the ranges matter, don't they? So how were the ranges of change in pumping, ag, etc., determined? Local expert elicitation? Where do these projections/expectations come from?

Difficult to parse whether this paper si talking about changes in long term means or changes in variability. Where is the discussion of changes in variability/extremes? Hugely important for how much water will actually infiltrate versus evaporate. Changes in timing/duration/intensity/frequency of precipitation. And was surface storage modeled for its effect on evaporation and long-term infiltration? I didn't see that.