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Hydrology and Earth System Sciences Discussions

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Interactive comment on "Evaluation of impact of climate change and anthropogenic change on regional hydrology" *by* Seungwoo Chang et al.

Anonymous Referee #1

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General Comments:

Overall, I found this work to be a well-written case study that connected climate change (in the form of general circulation model data) and water use (a number of demand scenarios) to surface and ground water availability in the Tampa Bay area in Florida, USA. In its current form this work represents a small but necessary contribution to the literature; many works have forced regional hydrologic models with downscaled GCM data, but fewer have done so under different future demand scenarios and this work furthers our understanding of climatic influence on hydrologic processes of importance to humans.

I have some concerns that the chief result of this paper – climate change will have stronger influence over water availability than human use/demand and likely result in

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reduced water availability regionally – was not strongly demonstrated. The authors use high sensitivity of projected water levels in the study region to downscaled GCM data as evidence of climate change's stronger influence over water availability than other factors. However, the paper references past impact analyses that claim GCM selection can have "unacceptable influence" over results, but have not provided any evidence that the eight GCMs used in this analysis do not have the same effect. Furthermore, sensitivity of water levels is based on relative variance of the contributing factors, two of the three factors (reference ET, water use scenarios) being deterministic and limited in scope and the third (downscaled GCM precipitation) randomly sampled from thousands of data points; I question whether the forms of data driving this analysis combined with the use of relative variance to determine sensitivity does not overly influence the study conclusions that GCMs have the most influence on water levels.

Beyond these larger worries, I would like to see a discussion of the results, which was largely missing from the manuscript. For two study limitations in particular, potential influence of land use and landcover change on hydrologic interactions related to water availability and the ramifications of measuring water availability without accounting for concurrent water demands, it would be good to spend more time on.

Specific Comments:

Lines 83-84: you mention in the introduction that analyses of hydrologic impacts at a catchment scale relying on a limited number of GCM projections are overly influenced by the choice of projection (Line 72) but drive your results with only 8 GCMs selected based on their provision of evapotranspiration parameters; how have you accounted for any bias in your study results from using this small subset of GCMs?

Lines 99-100: can you elaborate here? In what manner do surface waters and groundwater interact?

Lines 118-119: do you anticipate significant land use/landcover change in the Northern Tampa Bay domain relative to 1989-2006 conditions used to calibrate and validate the

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INTB model? Do the hydrologic responses presented in this work reflect expectations of future landcover change or a maintenance of current landcover? If the answer is the latter, as explained in lines 211-213, how do you think your results would differ if future landcover change projections were included?

Lines 154-156, Table 1: aside from citations and resolution, this table has very little information about each GCM used. Perhaps you could include the statistics on lines 157-161 in this table rather than the body of the manuscript? Are there other relevant factors that differentiate each GCM, some that may help explain the wide ranges in precipitation and evapotranspiration?

Line 163: a paragraph of the introduction was devoted to the limitations of using GCMs to drive regional hydrologic models, and various downscaling techniques have been developed to address this. How does the downscaling and bias correction approach used here compare with other methods?

Lines 227-229: I am confused between this statement and the demand scenarios laid out on the following page. Do these lines mean that 2003-2009 historical averages of reservoir withdrawal rate for Tampa and TB Water use are consistent through each simulation period? Should these vary with demand scenario as demand increases or decreases? Is the daily average rate referenced here sensitive to seasonal trends, or a flat average year-round?

Line 248: what is the basis for this ratio assumption?

Lines 253-259: are scenarios 1-3 included to isolate regional factors of water availability besides pumping? Otherwise, these scenarios do not seem plausible/necessary. Some justification for their inclusion or utility in this paragraph would be helpful.

Lines 260-271, Table 2: for scenarios with changes in pumping, where do these increases/decreases come from? Are these tied to regional planning reports or other projections of regional use? It would be good to have a citation or explanation of each.

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Lines 312-314, Table 4: why is there such a large difference in sensitivity to water use scenario between NWH-RMP-13s and NWH-RMP-08s? You say here that the 13s monitoring well is furthest from well fields, but it appears from Figure 1 the two NWH-RMP stations are relatively close together and both far from the nearest consolidated well field.

Lines 178-180: based on my understanding each of the eight water use scenarios projections, based on fixed historic pumping rates, and the three evapotranspiration methods, calculated for each GCM used but are not downscaled, are deterministic and limited in number, while the downscaled GCM precipitation is randomly sampled monthly from thousands of realizations with more spatial and temporal variation than either water use or ET. As a result, I would assume that the variation contributed to streamflow and groundwater levels from precipitation (GCMs) is much greater than that from evapotranspiration (ET0) or water use scenarios. Given that your method for assessing water availability sensitivity to each of these factors is based on what amounts to a normalized summation of total variance, how much variance is inherent in the GCM precipitation, ET, and water use scenarios used? Will your sensitivity calculation be biased toward identifying GCMs/precipitation as the largest contributor of variance in water availability because of the relatively large variance within the downscaled data? If this study was driven using randomly-sampled monthly water demands and fixed precipitation projections, would GCMs still be the largest driver of uncertainty?

Lines 321-334: much of this paragraph belongs in the methods section when the evapotranspiration methods are introduced. There are no actual results in this paragraph. I am curious, however, what differences are there between the ET methods this study retained and Chang et al. (2016b) used that would result in such large differences in sensitivity? Furthermore, if you only plan to elaborate on results related to a single ET method (lines 330-334), and future streamflow and groundwater levels are collectively insensitive to the three methods you selected, why is not only one ET method used

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for the entire work? Perhaps the surface flow/GW sensitivity analysis related to ET methods is best left for an appendix, with a single method used as a focus in the body of the paper.

Lines 349-350, Figure 2: why are future 2 (should consider referring to this as the 2070-2100 future period; were future 1 and future 2 periods explicitly defined in the methods?) streamflows on average lower than future 1 predictions, after future 1 mean daily streamflows in Figure 2 appear to be greater than observed mean flows? It might be more informative to condense the eight sub-plots of Figure 2 in a way that better communicates the differences in monthly flow averages between water use scenarios. Could you, for instance, aggregate these plots seasonally and then have four plots, one per season, that each has eight boxplot ranges, one per scenario. It would be much easier to see the differences, I think.

Lines 360-364: how does groundwater pumping result in lower streamflows? This relates to my previous comment about lines 99-100.

Lines 403-409: I would move these lines into the methods section; I cannot find a substantial mention of this analysis angle before this section.

Lines 406-409, Figures 6 and 7: an evaluation of water availability under these criteria may not account for the seasonal patterns of demand or timely needs for water supply. Does municipal water demand fluctuate seasonally in this region, and, like other regions in the Southeastern US, do peak months of demand (summer) correspond with times with lower streamflows? Evaluating water availability for urban supply solely based on in-stream water availability and capacity constraints of surface water intakes will tell you that water is constantly available during winter and spring (high flows), but this is also when demand is down and so availability is not as crucial. Aggregating availability without accounting for concurrent demand at an annual scale as done in Figures 6 and 7 will (1) obscure the seasonal differences of availability and (2) not offer a sense of water availability when it is most needed.

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Lines 452-453: to my understanding, your results show that the choice of GCM used to project water availability is the dominant cause of projection uncertainty, much more so than different water use scenarios. This is not the same as saying that climate change will drive water availability more than human use. As mentioned in the following lines (454-457), this work has shown the large uncertainty associated with GCM selection but in doing so has not shown that climate change is clearly more influential than human water demand in determining water availability in rivers, just that it is uncertain. This is especially true given that not all anthropogenic influences on water availability have been projected here (land use change, as an example), and that you have instances within your results (Table 4) where groundwater availability was more sensitive to the use scenario than to the GCM used.

Technical Corrections:

Introduction: parse out citations to show which studies emphasize which results, rather than blanket statements with many citations at the end and no citations for more specific findings or contributions

Line 78: consider adding "Furthermore, the effects of climate change..." to help the connectivity of this paragraph

Figure 1: it is difficult to read the well and gage labels. Can they be called out more effectively? Also, you mentioned this region contained multiple large municipalities – can you include municipal extents to illuminate what fraction of the region is more likely urban cover?

Line 185: add a comma and remove "and" from "Warming temperatures and reduce precipitation..." ("warming temps, reduced precip...")

Line 186: remove the comma

Lines 195 and 196: use of "lumped" is colloquial, replace with "included" or "combined"

Line 227: diversion not diversions

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Tables 3 and 4: replace “Fut1” and “Fut2” with the time periods of each future simulation.

Figure 2: differences on each sub-plot are so small I had to zoom in several levels before it was noticeable. It may be more effective to convert this graph so that it fits an entire page, and remove the top portion of each graph window (y-axis values of 35-45)

Figures 6-8: increase fonts and boxplot sizes

Line 395: what streamflow projections? Is this a reference to GCM projections of streamflow in futures 1 and 2?

Lines 419-420: Abstract says 6 of 8 GCMs project less water availability, here it says 5 of 8 and Figures 6 and 8 appear to confirm that. Adjust as necessary. After reading further I understand these ratios change between groundwater and surface water availability, but this was still confusing to read to me.

Lines 445-446: The sentence “For both gages more GCMs in future period 2 were significantly different from the retrospective period than future period 1” is confusing. Consider adjusting.

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