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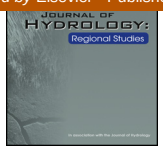

Peer review report

Peer review report 2 on "Changing climate increases discharge and attenuates its seasonal distribution in the northeastern United States"

1. Original Submission

1.1. Recommendation

Major Revision

2. Comments to Author:

Title: I would suggest changing in the northeastern United States to the Merrimack River Basin AND I do not think that the paper directly addresses the question of attenuation of seasonal distribution of discharge. And certainly not enough to warrant inclusion in the Title of the paper. If this were really addressed, as suggested in the title wouldn't this be highlighted in the paper results?

Line 29: you may want to say in the NE USA since Hayhoe et al (2007) is for this region (Merrimack)

Line 35: replace "highly regional" wit\h "highly variable by regions"

Line 39: should say "increases in annual precipitation"

Line 56: editing needed for grammar

Lines 60-61 The Hatcher and Jones 2013 paper concluded that responses at the larger basin scale are "obscured" by dam regulation and withdrawals so this is not a good citation here. Based on their titles, Gallart et al., 2011; Nayak et al., 2010; Reba et al., 2011; Viviroli et al., 2011 all appear to deal with headwaters only and are not compelling for the argument that headwaters are effective references for large basin hydrology. Whitfield et al., 2012 actually addresses the problem on pages 1572 and 1573 stating that ". . it is important to also consider the scalability of results; headwater catchments, while perhaps more sensitive to climatic variability, may not be representative of larger scales relevant for water management, so there is some argument for considering results from RHNs in the context of results from large catchment studies". I don't think you have cited evidence that small watersheds are necessarily the best effective references for impacts on large basin hydrology. After reading the paper I think it would be better if you left this concept out entirely - you don't address it with your data and it adds nothing.

Lines 61-65 For Campbell 2011 to be and "Example" whereby a headwater catchment was effective references for impacts on large basin hydrology Campbell 2011 would have to address the scale question and it does not.

Line 81: Do you mean "in or of reference catchments" rather than "to reference catchments". The use of "to" is confusing.

Line 82: Do you mean "developed" rather than "development"

Line 82: It is not clear what you mean by a "baseline to fill the gap". Are you saying that you developed a method to scale the responses of the sub-basins to the large basin? This needs clarification. After reading the entire paper I see that you did not develop such a method so I don't understand this "baseline to fill the gap".

Line 91: "water year" should be defined here.

Line 94: This sounds too low to me. Concord, NH which is at a latitude poleward of the basin centroid has an annual average minimum temperature of about 1.5 C. Is this the basin-wide average? Provide citation, and please check.

DOI of the original article:http://dx.doi.org/10.1016/j.ejrh.2015.12.057.

2214-5818/$ – see front matter
http://dx.doi.org/10.1016/j.ejrh.2016.01.029

Lines 103 to 105: For the purposes of this paper it would be useful to have an idea of the amount of impervious surfaces and consumptive water use withdrawals and perhaps an estimate of enhanced evaporation owing to reservoirs.

Line 157:There is a need to give a reference for “a double mass curve” and to explain what it is

Line 167: What does “opted” mean.

Line 162:Searcy 1960 is not in the ref list.

Line 162: I can find no mention of break points or the slope of a mass curve in Gao et al., 2011 so I don’t think this citation works here.

Lines 203 and 204: Grammar problem “experienced” is not correct. Do you mean “all multivariate statistical analysis”, since you just said that the Mann-Kendall trends were performed in Excel2007?

Line 217: Figure 4 should not be referenced before Figure 2. YOU NEED TO RENUMBER THE FIGURES In Fig. 4 it would be helpful to highlight the reference sub-basins in the Merrimack (13, 20 and 23) that are not Hubbard Brook basins - Perhaps with a different symbol noted in the caption. I think you need to explain that basins 20 and 23 fall well within the “mostly developed cluster and outside the HBEF cluster making it hard to see how you can say in lines 221 to 224 that you found no overall differences using multivariate statistical analyses among HBEF catchments and the Merrimack Watershed Reference sub-basins.

Line 274: Fig. 6 is referenced before Fig. 5, needs renumbering

Line 229 - Figure 2 is complicated to follow. First I would recommend discarding the top row”period of record“entirely. The differences between that and”average“are so subtle it is not worth retaining it. Second, I find the caption difficult to understand. The first sentence of the figure caption should probably say Flow duration curves (PLURAL) FDC comparing reference catchments with sub-basins that are regulated and developed (as it is it can be read to suggest that there are 3 curves ref, reg. and dev.) Second to the last line second use”variation is misspelled.

Lines 244 to 250 The grammar (English) needs wordsmithing.

Line 248: I believe the statement “In the average hydrologic flow class, high discharge quantity occurs in highly developed sub-basin” is incorrect. In fact in this case the highest discharge is also in the least developed class if I read the Figure correctly. It would be clearer if the authors labelled the figures in the panel as a, b, c, d,. . and referred to them that way than “left and right”

Lines 252 to 265 discussion should explain inverse behavior for 25% discharge timing between reference and regulated (left most column) and central column (ref and regulated. Lines 266 to 269 are more related to column 1 results with respect to the difference caused by development.

Lines 295 - 296. For ref basins 5 out of 12 had negative trends (42%) and in developed basins 7 out of 15 had negative trends (47%), those are pretty close.

Lines 303-306. I find this passage convoluted and requires multiple re-readings to make sense of. These “average” values certainly cannot be deduced from the cited Figure 6d. AND, when the authors say that these are averages for catchments with short periods of record - it is not clear which watersheds they are averaging - I don’t see how you can get an average of -17 out of the data shown in Table 3 for developed sub-basins with short records. I think I would revise this for clarity and be more qualitative rather than give average numbers for some subset (based on period of record) of the developed catchments. Here and in other places in the manuscript it is probably inappropriate to be averaging among watershed with substantially different periods of record.

Line 308: The use of the words “validate” and “foretelling” may be incorrect in this context. I think you are trying to convey that the Merrimack historical data showing decreasing discharge in dry years and increasing discharge in wet years in consistent with model projections. In this context, Karl and Knight (1998), Madsen and Wilcox (2012), Armstrong et al. (2012) and Collins (2009) do not address PROJECTIONS, they only address historic increases - THESE ARE INCORRECT CITATIONS. Climate projections clearly do suggest that precipitation is likely to increase in the northeast USA (See IPCC AR5 Working Group 1 2013), but the papers cited in your manuscript do not. This information is consistent with the concept of the wet getting wetter and dry getting drier (Trenberth, 2011).

IPCC, 2013: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 1535 pp.

See also

Wang, J., and X. Zhang, 2008: Downscaling and projection of winter extreme daily precipitation over North America. Journal of Climate, 21, 923-937, doi:10.1175/2007JCLI1671.1.

Trenberth, K.E., 2011. Changes in precipitation with climate change. Climate Research, 47(123-138).

Lines 347 - 352: No data are presented in the form of a figure and no identification of the principal components. I did not find this paragraph useful in any way.

Lines: 64-365: Actually it is more like 3 orders of magnitude difference, making one wonder how effective they can be in representing hydrologic responses of large basins.

Line 369-370: You need to state what time period this trend was for.

Lines 382 to 384: Revise, because high discharge events also occur in developed regions, especially under extreme events where flood control measures are overwhelmed.

Lines 379-380: this language “should be of interest” seems inappropriate in the discussion. If the assessment presented is of interest, then discuss how it is of interest. Who can use it? And How can they use it? Rather than say it “should be of interest”.

Lines 381 to 387: This seems like a reiteration of results rather than a discussion of results

Line 391: here and elsewhere in the paper, what does it mean to “remain intact”? How can an FDC “come apart”? it is a continuous function by definition. You need to explain this.

Line 396 English grammar problem “are as quite the same”

Lines 437-441: It is not clear how the authors come to this conclusion based on the data in table 3. The discharge trends for Watershed 19 are odd, how can discharge be decreasing at -1.7 mm/yr in dry years and -1.4 mm/yr in wet years and be increasing by only 0.2 mm/yr in average years but be increasing by 2.5 mm/yr over the entire period of record. This does not make sense. The increased impervious surface could mean more runoff than in the reference for some events, albeit reduced by increased evaporation losses from the reservoir and consumptive water use. Doubling of runoff in the developed basin is completely illogical.

Lines 444-445: AND temperature which regulates the timing of snowmelt and whether precipitation is occurring as snow or rain.

Lines 461-462: Authors should address the question of whether there are changes in reservoir storage that result in water released in one year having come from precip in another year that would complicate their interpretation.

Lines 465-466: Here the authors could cite two papers with evidence for increasing rates of ET in this region:

Kramer, R., Bounoua, L., Zhang, P., Wolfe, R., Huntington, T., Imhoff, M., Thome, K. and Noyce, G., 2015. Evapotranspiration trends over the eastern United States during the 20th Century. Hydrology, 2(2): 93-111.

Huntington, T.G. and Billmire, M., 2014. Trends in Precipitation, Runoff, and Evapotranspiration for Rivers Draining to the Gulf of Maine in the United States. Journal of Hydrometeorology, 15(2): 726-743.

Lines 472-473; I do not see how the left panels in Figure 3 demonstrate the impact of development versus regulation at all. This is not really addressed in the results that I recall either.

Lines 485- 486: this statement makes no sense to me.

Lines 501 - 503: this point has already been established - in the discussion it should be related to the findings of this study, which you have already done.

Lines 539 - 541: I do not think this belongs in conclusions. This analysis is consistent with the changes in temperature and timing of snowmelt, HOWEVER this paper did not analyze these variables so this is better left to the discussion. The conclusion should highlight the results of this study. Same problem with lines 546 and 547 re snow hydrology.

OVERALL To mention ranges of trends for watersheds with such variable lengths of records is not really appropriate, especially when as the authors noted the slope of some of the trends is much steeper in recent years. The ranges are really apples and oranges.

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