

Interactive comment on “Assessing the influence of an extended hurricane season on inland flooding potential in the Southeast United States” by Monica H. Stone and Sagy Cohen

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Received and published: 17 November 2016

We thank the reviewer for the insightful comments. We have addressed all of them in the revised manuscript. Below we provide a point-by-point response.

"The premise of the paper is simple and the methods are straightforward, and the topic is interesting. The authors quantify the change in flood risk in four southeastern U.S. drainage basins under the assumption that the Atlantic hurricane season would increase by one month at the beginning and one month at the end of the currently delineated tropical cyclone season. Such research is placed in the proper theoretical context, as the expected continued warming would leave ocean temperatures warm enough to sustain a tropical system for a larger number of months per year. Fur-

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thermore, the authors do a proper job, without getting too bogged down in tangential points, of introducing the reader to the somewhat conflicted literature on whether it would be the frequency and/or intensity of tropical cyclones that would increase under such warming."

Thank you.

"A minor point: I think that the wording is a bit strong on Page 3, Line 13, where the authors say, 'This hypothesis was refuted by...' – at a minimum, a hypothesis can't be refuted when one of the papers doing the refuting was written before the opposing papers, but more importantly, I think the jury is still out on which hypothesis is correct. That point notwithstanding, I like the theoretical set-up for the paper."

The sentences in question were amended as follows:

Major hurricanes, those that are Category 3 or higher on the Saffir-Simpson scale, are the most likely to intensify (Frey et al., 2010; Mousavi et al., 2011), but there is some debate about changes in tropical cyclone frequency. Some research predicts that tropical cyclone frequency will increase (e.g. Greenough et al., 2001; Ouellet et al., 2012), while other research suggests that tropical cyclones are likely to intensify with global climate warming, but occur less frequently (e.g. Irish and Resio, 2013; Kostaschuk et al., 2001).

"The primary theoretical/methodological weakness of the paper is the failure to account for synergistic effects of interactions between May or December tropical cyclones with extratropical systems. We all saw in 2012 (i.e., Sandy) how such interactions can cause greatly increased precipitation totals. At a bare minimum, the authors need to acknowledge this as a major weakness of the study."

We added this as a weakness of our paper (page 8, starting in line 18):

Further, this research does not consider synergistic effects due to the potential inter-play between May and/or December tropical cyclones and mid-latitude cyclones, which

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could increase precipitation and flooding risk even further.

"The chief non-theoretical/methodological weakness is that the paper could have delivered the same message in perhaps 60% of the words. Even though I generally enjoyed reading the manuscript, I continually found myself a bit frustrated and thinking, 'not again?!?' when I read repetitious text or text that was unnecessary. If the text were tightened fairly significantly, I'm sure that I am many others would find the paper to be a nice contribution to the literature. I attach a marked-up version of the manuscript in the hope that this will assist the authors as they tighten the manuscript."

Thank you. We incorporated all of the suggestions in the marked-up version in the revised manuscript.

"One other comment: Please insert the word 'Atlantic' in the title and elsewhere in the text, to show that your study only considers one of the world's tropical cyclone-vulnerable areas."

Done!

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-320/nhess-2016-320-AC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-320, 2016.