COMMENTS AND REPLIES

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Reply

REPLY to comment on "The geological legacy of Hurricane Irene: Implications for the fidelity of the paleo-storm record" by Scott P. Hippensteel, Matthew D. Eastin, and William J. Garcia

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We were pleased to receive a formal comment on our December 2013 *GSA Today* article from the paleotempestology group represented by Donnelly et al. (2014). After reviewing their comments, however, we were hard pressed to find a single sentence that doesn't contain a factual error or an egregious restatement of our position.

In the first paragraph, Donnelly et al. (2014) state that we "repeatedly make reference to significant controversy in the literature … but only cite the dialogue between Otvos and Liu and Fearn." Such a narrowly focused statement ignores our discussion of the second proxy method—microfossil derived paleotempest proxies—as well as six citations of other researchers. We clearly stated that there were *two* contentious proxies to be discussed, and we included 13 references that provide a *holistic* background for these two widely used proxies.

Donnelly et al. continue by criticizing our use of the term "sand layer counting," which they contend is "outdated" and then contend that we "lack knowledge of more recent methods (e.g., grain-size analysis, geochemical techniques, numerical modeling)." Our apparently "outdated" reference to sand layer counting originated from Donnelly's co-author Otvos's 2011 (p. 19) paper. Furthermore, we are criticized for not including "geochemical techniques" when our paper specifically mentions "geochemical anomalies" (with 2012 and 2013 references).

A more concerning error arises when they misleadingly paraphrase the basic purpose of our paper: "The premise of Hippensteel et al. (2013) is that hurricane proxy records somehow document all strikes." Rather, we specifically stated, "One of the *underlying goals* of *paleotempestological research* is the compilation of landfall record of storms of all intensities (Categories 1–5)." Our point was that for proxy records to be of optimal use to climate scientists and hurricane risk assessments the fidelity of the record must be better understood and the full spectrum of events must be well documented. It is recognized within the climate community that inferring a spectrum of return periods across a region by either extrapolating from a few extreme events located at the tail end of the intensity distribution (i.e., only documenting Category 4 and 5 hurricanes) or by extrapolating in space from a limited number of sites can be problematic and potentially misleading due to the sensitive assumptions inherent in each extrapolation (Emanuel and Jagger, 2010; Villarini et al., 2012). Our study simply documents the application of *one* such proxy method (displaced marine microfossils) to a more typical event (a weakening Category-1 Hurricane Irene), well away from the extreme tail of the intensity distribution.

After misstating our premise, Donnelly et al. argue that the relationship between storm deposition, magnitude, and preservation is "obvious" and "the literature provides little argument to the contrary." This ignores repeated reports of smaller or "peripheral hits" (our figure 1 included numerous sites where smaller paleostorms have recently been recorded). Furthermore, confounding any "obvious" relationships between storm deposition and intensity is the recognition that hurricane intensity is characterized solely by maximum wind speed, yet wind speed is only one of many characteristics that govern storm surge and overwash magnitudes at a given location (Resio and Westerink, 2008). A contemporary example of this dichotomy is Hurricane Sandy (2012), which made landfall with Category-1 maximum winds and Category-3+ storm surge (Blake et al. 2013). Such examples provide further support to our contention that documenting the full spectrum of events is critical for accurate hurricane risk assessments in a changing climate.

Donnelly et al. close with a statement that is disturbingly similar to one from our paper, suggesting a lack of careful reading: "Many factors must be considered when interpreting paleo-storm records, including the susceptibility to overwash, preservation potential, local geomorphic variability and the archive's fidelity." We attributed the lack of preserved storms to destruction from bioturbation (preservation potential) or the sensitivity of the sites to storm deposition (susceptibility to overwash/geomorphic variability). So, essentially, they are reprimanding us while agreeing with us. Nevertheless, their concluding paragraph misstates our conclusion: "Condemning all paleo-storm studies because the modest Hurricane Irene did not produce a discernable signal in Onslow Bay is misguided." Where, exactly, did we condemn all paleo-storm studies? We stated that "caution" should be used when interpreting paleo-storm records because the exact nature of the depositional mode is still not definitively known. Our goal was to see what might be left behind by a weaker hurricane before post-depositional alteration, and it is insincere to conflate this to a condemnation of all studies.

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Paleotempestology is an important, relatively young science that may prove even more significant as better records are developed. New proxy methods will certainly surpass those discussed in our paper. Unfortunately, the criticisms advanced by Donnelly et al. appear to have been prepared without proper consideration of our paper, are repeatedly based on inaccurate restatements of our premise, and add little to the discussion.

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