

## Female- and male-named hurricanes remain equally deadly

In an article recently published in the *Proceedings of the National Academy of Sciences of the United States of America*, Jung, Shavitt, Viswanathan, and Hilbe (1) claim that female-named hurricanes that made landfall in the United States were deadlier than male-named hurricanes. In a letter to the editors (2) I criticized that the results of their archival study were not robust to the inclusion of an interaction effect that they had arbitrarily omitted from their analyses. My letter marks this clearly as its main criticism. To quote: “[t]he criticism of this letter is a different one: the results of their archival study are a function of the selective inclusion of regressors. . . . [T]heir results are not robust to the inclusion of the one two-way interaction that they omitted from their analysis.” In other words, I indicated that their results were likely driven by spurious correlation and missing variable bias. Jung et al.’s (3) reply to my letter is disappointing because they fail to engage this criticism. Sadly, the *Proceedings* only allow for one interaction between authors and letter writers irrespective of the quality of the authors’ work, the seriousness of the criticism, or the quality of the authors’ response to a criticism.

In the four opening paragraphs of their reply Jung et al. rebut one of three criticisms that had been made in online commentaries and that I cited in the opening paragraph of my letter. I cited these criticisms for two reasons. First, I wanted to give the reader a sense that the criticisms of Jung et al.’s paper were far more numerous and extensive than the one criticism that I would be able to discuss in the letter. Second, I wanted the reader to know that the criticism I was about to raise was different from the criticisms that had already been raised in other places.

One of the criticisms raised elsewhere was that Jung et al. included hurricanes from the era before they were given male names (before 1979). Unfortunately, by citing Malter (2) and devoting their first four paragraphs to the discussion of this issue, Jung et al. give the impression that the inclusion of the pre-1979 sample was the main criticism of my letter. It was not. Their discussion of this and another cited issue is thus likely to leave the reader of their reply under the impression that they were able to defend their results against the main criticism of my letter. They were not.

The main criticism of my letter was the arbitrary exclusion of the interaction effect of a hurricane’s barometric pressure and its damage toll on its death toll. Jung et al. dismiss this criticism with a single sentence: “. . . Malter (2) argues for the addition of an interaction term for which there is no conceptual rationale.” That is all they have to say about the criticism to which I devoted all but the opening paragraph of my letter.

To say that there is “no conceptual rationale” either means that there isn’t one or, in a less precise interpretation, that I did not provide one. However, to claim that there is no conceptual rationale for an interaction effect between the barometric pressure of a storm and its damage toll on the death toll is a similarly strong claim as would be claiming that there is no conceptual rationale for an interaction effect of body height and body circumference on body weight. Simply because they cannot see a conceptual rationale does not mean that there isn’t one. And simply declaring a strong relationship in the data void because they cannot see the conceptual rationale for it does not mean it isn’t there. It would at least require a careful engagement of the criticism to determine where the truth lies.

The claim that there is no conceptual rationale for this interaction effect is all the more surprising because I articulated one in my letter. The specific rationale I provided was that the damage toll is most strongly reflective of the safety infrastructure in an area, which should reduce the death toll more during stronger storms. The last two of the four paragraphs of my letter are devoted to the discussion of this rationale in more depth. Hence, the claim that there is no rationale for this interaction effect, or that I did not provide one, is theoretically and factually false.

What makes it difficult to provide a single unifying rationale for this interaction effect is not that there isn't one but that there are too many because "the damage toll is a simultaneous outcome of the storm and [...] merely reflects other underlying characteristics [...] of the hurricane or its area of effect" (2). The authors could have disentangled many of the underlying characteristics embodied in the damage toll with freely available and much more fine-grained data from the NOAA's storm database in combination with Census data. Jung, Shavitt, Viswanathan, and Hilbe should thus not be surprised that I am baffled to see them celebrate the ambiguity of the damage toll variable, for which it is difficult to find a unifying conceptual rationale in and of itself, let alone its interaction effects, as a sort of defense of their theoretical rationale and modeling strategy.

Jung et al.'s reply (3) leaves my main criticism unaddressed. None of the results of their archival study survive the inclusion of the interaction effect between the barometric pressure and the normalized damage toll. That is, a more parsimonious model for which a plausible conceptual rationale was provided explains the patterns in these data better than their theory and models do. Until the authors engage the central criticism of my letter and present better evidence for their claim we should maintain the null hypothesis that female- and male-named hurricanes that made landfall in the United States were equally deadly.

#### References:

1. Jung K, Shavitt S, Viswanathan M, Hilbe JM (2014) Female hurricanes are deadlier than male hurricanes. *Proc Natl Acad Sci USA* 111(24):8782–8287
2. Malter D (2014) Female hurricanes are not deadlier than male hurricanes. *Proc Natl Acad Sci USA*, 10.1073/pnas.1411428111.
3. Jung K, Shavitt S, Viswanathan M, Hilbe JM (2014) Reply to Christensen and Christensen and to Malter: Pitfalls of erroneous analyses of hurricanes names. *Proc Natl Acad Sci USA*, doi:10.1073/pnas.1411652111

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