

Dear Michael and tropical storm folks,

I have some additional concerns about this new paper. As you know, I was one of the reviewers for your Nature paper and, as usual, I "signed" my review. Unfortunately, some very large concerns of mine about the paper were not addressed. The two gravest issues are the paper's use of the Atlantic basin tropical storm frequency data without consideration of new studies and the merger of the paleo-tempestology record to the historical storm data. Perhaps these could be addressed here.

The first point is that the paper disregarded (and not even discussed) crucial new work by Vecchi and Knutson (Journal of Climate, 2008) and Landsea, Vecchi, Bengtsson and Knutson (Journal of Climate, 2009). The first paper showed that about 3-4 tropical storms per year were likely "missed" in the late 19th Century down to less than 1 per year by the 1960s. The second paper (provided to you all in the review process as an accepted paper) shows that two-thirds of the massive doubling trend is simply due to very short-lived (< 2 days duration) tropical storms.

Taking out these "shorties" (very likely due just to our vastly improved observational capabilities) from the record and adding in the estimated number of "missed" medium to long-lived tropical storms causes the long-term trend to completely disappear.

Your paper starts with a premise that "Atlantic TC activity, as measured by annual storm counts, reached anomalous levels over the past decade", which is simply not correct based upon the new research. Instead of finding that the medieval peak in TC activity "rivals" current levels of TC numbers, wouldn't your conclusions have been substantially different? This isn't a small quibble: it's the difference between a massive trend with doubling in the last 100 years, versus no trend with only multidecadal variability remaining. This new peer-reviewed research should not have been ignored completely.

Secondly, I have no expertise on paleo-tempestology data and thus cannot provide commentary about that portion of the analysis in the paper. However, the merging of the paleo record with the historical all basin tropical storm counts is very problematic. Instead of trying to upscale the paleo-tempestology data (Category 2 and stronger) to all basin tropical cyclone activity (including all tropical storms and hurricanes), an apples-to-apples comparison directly of paleo landfall data to historical (1851 to today) hurricane-only landfall data should have been performed. Not surprisingly, the historical landfall record for those five sites shows no trend. How could this be considered a homogenous comparison: landfall of (primarily) major hurricanes at five locations versus all basin tropical storm and hurricane numbers whose trend is mainly due to very short-lived, weak tropical storms that we simply couldn't observe decades ago?

What is curious, too, is the press release (link below) issued at Penn State that concluded: "It seems that the paleodata support the contention that greenhouse warming may increase the frequency of Atlantic tropical storms," said Mann. "It may not be just that the storms are stronger, but that there are there may be more of them as well."

Why would such a strong statement be included in a press release that isn't even discussed in the paper? Would the paper's co-authors agree with [this very public pronouncement](#) about the implications of the work?

The bottom line is that the paper comes to very erroneous conclusions because of using improper data and illogical techniques. In my opinion, this work is, unfortunately, a step backwards in helping climate researchers understand how hurricanes have changed over the last several centuries.

Sincerely,
Chris Landsea

P.S.: The opinions expressed above are mine alone and do not represent any official view of the National Hurricane Center, the National Weather Service, the National Oceanic and Atmospheric Administration, or the Department of Commerce.

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