Dear Marc and company,

Below are some comments I've sent to various reporters that have asked me my opinion about their study:

I have read through the new paper (and the Supplementary Information) by Elsner et al. being published today in Nature. As is usual for a study led by Elsner, the statistical methodology is excellent.

However, because of concerns with the data being utilized, I do not agree with the conclusion in the paper that "clearly show that the strongest tropical cyclones are getting stronger" around the world.

First off, I do not disagree with the finding that there has been a sizable increase in intensity for hurricanes in the Atlantic since the early 1980s. This is reconfirming work going back to conclusions that Goldenberg et al. initially showed in Science in 2001. My interpretation is that the Atlantic basin shows quasi-cyclic variations and that the current busy era (1995 onward) is quite similar to that which occurred from the late 1920s to the late 1960s (and the 1870s to the early 1900s). This current paper cannot address this cyclic variability for the Atlantic, as it starts with data in 1981.

Nor do I disagree with the findings that for the Northeast Pacific, the Northwest Pacific and the South Pacific there is either no change in intensity or a very minor, not statistically significant increase for up to 95% of all the tropical cyclones as is shown in the paper.

Where I believe that the study may not be reliable is in two key aspects: the treatment of the Indian Ocean tropical cyclone data and in the use of the data for the most extreme winds.

Readers of this paper and the earlier groundbreaking one by Kossin et al. in Geophysical Research Letters in 2007 would note a huge difference in the South Indian and North Indian tropical cyclone intensity trends. In Kossin et al.'s earlier paper, the Indian Ocean basins had no trend in the number of extremely intense tropical cyclones, while in the current paper the Indian Ocean basins show a dramatic trend in intensities for the strongest third of the storms.

Why the big change between the two papers? It is mentioned in the Supplementary Information that a correction was applied to the infrared cloud top temperatures measured for the Indian Ocean tropical cyclones because of the very large change in the satellite view angle that occurred in 1997 (with the launch of Meteosat-7 satellite). This alteration the authors introduced would make for a more realistic (warmer) cloud top temperature for pre-1997 tropical cyclones. However, while cloud top temperature is the most important factor for the average tropical cyclone, being able to accurate measure the eye temperature is crucial in correctly ascertaining the intensity of strong tropical cyclones. Because of the very oblique look-angle in pre-1997, one would not be able to see the warm eyes (and thus correctly identify the strongest cyclones). Correcting only for the cloud top temperatures but not taking into account unobserved warm eyes might well be fine for the average tropical cyclone, but it is not for the strongest storms. These big trends in the Indian Ocean tropical cyclone intensities may not be real.

The second key aspect is whether this dataset is appropriate for examining trends in the most intense tropical cyclones. Given the 8km resolution of the infrared satellite measurements, it is often the case that extremely intense tropical cyclones would have an eye that would barely - if at all - be resolved. Moreover, the second author had stated previously that there is "a caveat to our analyses that limits its usefulness in the discussion [of trends in extreme tropical cyclones]. This regression-toward-the-mean aspect makes it less suitable for capturing the most extreme cases (Cat 5 intensities)." It is curious that the second author would previously issue caution in using this dataset for examining extreme tropical cyclone intensity trends and yet now in the current paper that is exactly what the authors are doing with the dataset and that is the focus of

the paper: how have the extremely strong tropical cyclones changed? I would probably agree with what the second author said a few months ago, that this dataset is not very suitable for such analyses because of both the resolution and the regression methodology.

Lastly, just a comment about how the authors conclude that the new findings are "consistent with the theory". It's not. The latest numerical modeling and theoretical studies suggest a rather tiny 1-2% stronger tropical cyclones per degree C warming of the oceans (see, for example, Knutson et al. 2008 in Nature Geosciences or Emanuel et al. 2008 in the Bulletin of the American Meteorological Society). Instead here they find a huge increase of 5-12% stronger per degree C. This is not consistent at all, as Elsner et al. are getting a much bigger sensitivity than the extremely small increase suggested by all of the modeling and theoretical studies.

So overall, the paper has some elegantly calculated statistics, but these are generated on data that are not - in my opinion – reliable for examining how the strongest tropical cyclones have changed around the world.

Best regards,

Chris
P.S.: The opinions expressed above are mine alone, and do not represent any official position of the National Hurricane Center, the National Weather Service, or NOAA.
****

Chris Landsea
Science and Operations Officer
NOAA/NWS/National Hurricane Center
11691 S.W. 17th Street
Miami, Florida 33165-2149
Chris.Landsea@noaa.gov P:305-229-4446 F:305-553-1901