From: grlonline@agu.org [mailto:grlonline@agu.org]
Sent: Monday, March 05, 2007 8:03 AM
To: xxxxxx@cires.colorado.edu
Subject: 2007GL029666 (Editor - James Famiglietti): Decision Letter

Dear Dr. Pielke, Jr.:

We have had your manuscript, 2007GL029666, "Decreased Proportions of Tropical Cyclone Landfalls in the United States: Data Artifact, Blind Luck, Natural Variability, and/or Global Warming?," reviewed for both scientific content and GRL-specific criteria. Based on this evaluation, I cannot consider your manuscript further for publication in Geophysical Research Letters. Attached below are the review comments, which you may find helpful if you decide to revise the paper and submit it to another journal. I am sorry I cannot be more encouraging at this time.

Thank you for your interest in GRL.

Sincerely,

James Famiglietti
Editor
Geophysical Research Letters

Reviewer #1 Evaluations:
Science Category: Science Category 4
Presentation Category: Presentation Category B

Reviewer #1(Formal Review):

General comments

This article attempts to throw some light on the problem of changing tropical cyclones over the North Atlantic and the landfalling component. It falls short is several ways. There are many major reasons why this paper should not be published in anything close to current form.

1) It is vague on some of what is done, but it appears to deal with all "tropical cyclones" as given in their Figure 2, including many that are at 60?N! Indeed many of the locations are north of 30?N and can hardly be considered tropical. Those storms are in the westerlies and moving away from the U.S. and not relevant to landfalling (except for ones right at the coast). Quite aside from these physical reasons, there were no records kept of "tropical cyclone" developments poleward of 25?N prior to 1970, and so the record at these higher latitudes is not homogeneous by design! [See Simpson and Pelissier 1971 Mon. Wea. Rev. for instance]. At the very least, the analysis in the paper must be stratified by latitude and with discrimination of what are really tropical cyclones and allowance for the change in 1970 polewards of 25?N.
2) The paper concludes that there may be problems with the data base in the early years. This is indeed well established (or so claimed by Landsea as referenced), prior to 1944 when aircraft surveillance began, and even then the far western Atlantic may not be sampled. See also above for regions poleward of 25?N. The paper does nothing other than substantiate these.
3) The paper deals with trends over arbitrary periods that are not statistically significant (as is easily seen when the longer record plotted is seen). The trend lines are irrelevant.
4) Also it is well established that there is multidecadal variability and the Atlantic Multidecadal Oscillation (AMO) in SSTs and hurricanes that are strongly related to each other. There is no discussion here of SSTs or the AMO to provide context for the variations. The increase after 1994 in Fig 3 is not picked up by the straight line fit, for instance. Nor is the obvious increase in 1944 when surveillance by aircraft began.
5) It is well established that tracks of TCs depend mostly on the detailed synoptic situation at the time and place of the storm, and thus it is largely random as to just where a storm goes (and makes landfall). This fundamental point is lost in this paper.
6) The statistical analysis is fraudulent in taking what amounts to a zero trend for landfalling storms, and combining it with increasing trend in east Atlantic storms to come up with a decreasing proportion making landfall (Figs. 5 and 6) that says nothing whatsoever about landfalling storms. The conclusion that "it is possible to rule out a hypothesis of randomness as a basis for the discrepancy between lack of trend in landfall data and the seemingly significant trends in the other overall basin indices of hurricane activity" is not justified. Nor is the statement in the last sentence of the abstract.
7) The paper makes use of Emanuel's (2005a) filter when it is well established that this was not the way to do things (as noted in this paper). Such a procedure is outlandish.
8) The procedures of what was done and what data are being used etc are often not clear throughout the manuscript. Are these data every 6 hours? What were the

criteria? How sensitive are the results to changes in thresholds: tropical storms, vs named storms vs hurricanes vs strong hurricanes, etc?
9) As the ocean warms, as it has, then presumably tropical storms may occur in places never before seen, such as Vince in 2005 in the east Atlantic that ended up in Spain and Portugal, or Catarina in the South Atlantic in 2004. Is this relevant for landfalling storms in the U.S.? By focusing on the east Atlantic the implication here is that it is, but it should not be at all.

Some other comments
What is a quintile as used: is that equal fraction in each domain?

Reviewer #2 Evaluations:
Science Category: Science Category 4
Presentation Category: Presentation Category A

Reviewer #2(Formal Review):

Review of Pielke and McIntyre: "Decreased Proportions of Tropical Cyclone Landfalls in the United States: Data Artifact, Blind Luck, Natural Variability, and/or Global Warming?"

The paper examines trends in various hurricane parameters within quintile longitudinal bands across the North Atlantic. The conclusion is that there is an "easterly" (sic) trend in median longitude and that the "entire" increase of Atlantic cyclone activity took place east of 63oW. This conclusion then leads to the supposition that the observed changes in storm activity are either an artifact of changing observing practices, or due to some as yet unknown climate process, or both.

The paper should be rejected. The eastward extension of hurricane activity is well known, the methodology is at best simplistic, and, aside from some unsubstantiated supposition, the authors do not address any of the "Data Artifact, Natural Variability, or Global Warming" issues. Indeed the paper reads more like a poorly constructed commentary on Mann and Emanuel (2006) and Holland and Webster (2007a) than a piece of original scientific work. Unfortunately, and unusual for my reviews, I am not even able to recommend resubmission to focus on a specific point of merit, as there is not one. Some specific comments follow:

The article commences with an uncritical summary of some of the recent literature on hurricane trends, which has several misinterpretations: for example, the Landsea et al (1996) study covered only a change from a previously active to and inactive period (see Landsea et al 1997 reply to a comment by Wilson 1997). Two quotes are inserted from Mann and Emanuel (2006) and Holland and Webster (2007a), which are taken completely out of the context of the very careful, indeed laborious, consideration of all relevant factors that were examined in arriving at those conclusions. The authors also fail to note that both studies focussed much of their attention on the remarkably close relationship of cyclone characteristics with independently observed SSTs in arriving at their conclusions of statistical significance, and that there was not a general trend in storm numbers, but two short-period jumps, each of around 50% increases, that cannot be tied to any known observing system changes.

The claimed "new" empirical results in the paper (lines 39-44) include:
(1) A trend to the east in median annual longitude of NATL tropical cyclone tracks.
(2) A lack of trend in the western 60% of the NATL basin in all relevant integrals.....the entire NATL increase in relevant integrals is derived from the eastern 40%....
(3) A corresponding decrease in the proportion of storms that make landfall in the United States.

I address each of these in turn.

The trend to the east is not a new result. It has been debated vigorously in a number of international meetings over the past year (e.g., Landsea 2006, Landsea et al 2007, Holland and Webster 2007b) and is the subject of several studies that are examining the underlying physical causes. It also is referred to directly by, e.g., Hess and Elsner (1994), Kimberlain and Elsner (1996) and Holland and Webster (2007a). These studies focus on the changes between subtropical and equatorial developments but refer directly to the eastward extension associated with tropical systems.

The purported lack of trend in the western 60% of the NATL basin arises somewhat from a combination of the methodology used and the definition of "all relevant integrals," which are dominated by storm days. I concur that there has been an increase in storm formation in the eastern portion of the domain, and this is reflected in storm days and genesis frequency. However, it also has been shown that there has been an increase in genesis frequency in all parts of the North Atlantic except the far western Caribbean and the southern Gulf of Mexico (Holland and Webster 2007b). And there are good physical reasons for why this should occur. The lack of detectable trend in the parameters used in this study arises as much from their selection of such narrow longitudinal bands and focus on storm days as being a

real result.

It is notable that the HURDAT landfall set does not contain Caribbean landfalls, especially in the east. When these landfalls are included there is a marked trend in landfall activity throughout the entire ocean basin.

The "corresponding decrease in proportion of storms making landfall" is also not a new result. For example, it has been noted by Solow and co-authors (as referenced in the paper), by Landsea (2006) Landsea et al (2007), and by Holland and Webster (2007b, who also proposed possible explanations).

Lines 60-67: It is remarkable that given the (quite reasonable) criticism by Landsea (2005) of Emanuel's initial approach of pinning the data to the end points, the authors then adopt the same approach and also fail to note Emanuel's response to Landsea and subsequent work that showed his conclusions remain firm after the pinning is removed. One is again led to the view that the major purpose of this section is to present a biased criticism of Emanuel rather than a well argued scientific discourse.

Lines 68-127: I could continue on with specific criticisms here, but quite frankly the paper is not worth the effort. It is notable that there are many references to Holland and Webster (2007) in the context of an analysis that relies entirely on cyclone days, a parameter that Holland and Webster deliberately did not use because of the potential errors associated with observing system changes.

So I will move directly to a consideration of the major conclusions.

Lines 130 and 131: "The entire increase in Atlantic cyclone (sic) took place east of 63W" is demonstrably incorrect, as noted above.

Lines 137-142, and the implication that the trend is entirely due to changing observing methodologies: This ties back to earlier general statements on ship traffic, etc, without any quantification or analysis or even adequate reasoning. The authors seem to have not noticed that more than 50% of the increase in basin wide activity, and the associated substantial increase in equatorial (including eastern equatorial) genesis, have occurred in the satellite era, for which it is extremely unlikely that there were more than a minimal number of missed storms. They also fail to note the published very close relationship with independently observed SSTs, which provides both statistical and physical support for a real increase in activity. Fortunately, there are studies currently at the submission stage that have shown from a proper analysis of the observing systems that there remains a statistically significant trend since 1906 in cyclone frequency

even after the worst case scenario of potentially missed systems is accounted for.

Lines 146-147 on the lack of relationship between SSTs and landfall proportions (and related figure 6): This figure and conclusion serves no purpose whatsoever.

Cited References (not in Pielke and McIntyre list):

Hess, J. C., and J. B. Elsner, 1994: Extended-range seasonal hindcasts of easterly-wave origin Atlantic hurricane activity. Geophys. Res. Lett., 21, 365-368.

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Kimberlain, T.B. and J.B. Elsner 1998 The 1995 and 1996 North Atlantic Hurricane Seasons: A Return of the tropical-only hurricane. J. Climat., 11, 2062-2069.

Landsea, C.W., 2006: Chairman's report on Topic 4 to the IWTC. http://severe.worldweather.org/iwtc/

Landsea, C. W., N. Nicholls, W. M. Gray, L. A. Avila, 1997: Reply, Geophys. Res. Lett., 24(17), 2205-2206,

Landsea, C.W. et al., 2007: Can we detect climate trends in extreme tropical cyclones? AMS Forum: Climate Change Manifested by Changes in Weather.