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

Reviewer #2 (Comments to Author):

Summary
This paper contains an interesting, simple study that looks at the role alternative El Niño patterns on hurricane activity in the Atlantic. They find little connection - at odds with the study of Kim et al. (2009). The paper would make an interesting contribution that would be important for the rapidly-moving field of non-canonical El Niño. Although I recommend publication, several further clarifications are needed to justify their findings. I would expect these should not take too long to complete, however.

We greatly appreciate thoughtful comments and suggestions. We have revised the manuscript following these suggestions as discussed below.

Major Comments
This paper presents itself as a counterpoint to the Kim et al. (2009) study in the introduction, yet there is little subsequent discussion of it. Please explain the reasons for the differing conclusions more thoroughly.

Kim et al. [2009] argued that CPW events are associated with an increasing frequency of cyclone activity in the Gulf of Mexico and Caribbean Sea. However, Kim et al. [2009] used only 5 CPW events to arrive at that conclusion and only two of those five years were characterized with increased Atlantic TC activity (1969 and 2004) as shown in Table 1 and Table S1 in Lee et al. [2010]. The other three years (1991, 1994 and 2002) were under normal or below normal TC activity. Lee et al. [2010] further showed that the tropical North Atlantic was warmer than normal (or the Atlantic warm pool was larger than normal) during those two active years (1969 and 2004). They performed model experiments to argue that the increased tropical storm frequency in 1969 and 2004 could be readily explained by a large Atlantic warm pool and the associated reduction of MDR wind shear, without invoking a remote influence from the tropical Pacific. Therefore, Lee et al. [2010] concluded that it was premature to associate CPW events with an increasing frequency of tropical cyclone activity in the Gulf of Mexico and Caribbean Sea.

In summary, the composite analysis of Kim et al. [2009] was not a robust one because it had two critical problems. First, the number of samples used was not large enough to make a statistically robust case to support their conclusion. Second, they did not remove the influence of the tropical North Atlantic SSTs.

These points were already discussed in Lee et al. [2010]. So, the basic idea of the current study is to perform a new composite analysis using more cases and other definitions (i.e., EMI, TNI and PMM) of the non-canonical El Niño.

Nevertheless, we do understand reviewer's point that more in-depth comparison of this study with Kim et al. [2009] and Lee et al. [2010] is required (reviewer #1 also suggested this). Therefore, a new section (section 5) is added in the revised manuscript. In this new section, we attempt to compare our results with those of Kim et al. [2009] and Lee et al. [2010]. Table S9 shows the Atlantic TC indices and MDR VWS for the five strong CPW years (1969, 1991, 1994, 2002 and 2004) identified in Kim et al. [2009]. As shown in that table, the five-year averaged number of TS (10) is slightly increased from that of a normal year (8). But it is certainly not significantly different (at the 90% significance level) from the climatological value. Note that Kim et al. [2009] used this statistically insignificant difference to argue that CPW events are associated with increased Atlantic TC activity.

In the new section, we also discuss why 1991 and 1969 are not identified in the list of the eight strongest CPW years in our study. In particular, we find that ASO of 1969 was a weak-to-moderate canonical El Niño season because NINO3 was only 0.63^{o}C and greater than NINO4 (0.58^{o}C).

Do you think that the seasonal progression shown in Fig 1 of Kim et al could also play a role? In other words, does their reduced activity in June and July cancel with the increased activity in August/September to show nothing on the JJASON average you're using?

This question was raised during the review process of Lee et al. [2010]. Therefore, in Lee et al. [2010], the number of Atlantic TCs (linearly detrended) was shown for both JJASON (Table 1 in Lee et al. [2010]) and ASO (Table S1 in Lee et al. [2010]) of the five CPW years identified in Kim et al. [2009]. If the number of Atlantic TCs is averaged for ASO, the five-year averaged number of TS (10) is indeed slightly increased from that of a normal year (8). But, it is certainly not significantly different (at the 90% significance level) from the climatological value. Kim et al. [2009] used this statistically insignificant difference to argue that CPW events are associated with increased Atlantic TC activity.

Kim et al. [2009] provided numerical values for ACE (linearly detrended) in JJASON in their Table S1. Their values were 101.9 for climatology and 102.8 for the five-CPW-year-average. Based on this result, they stated (in page 78, line 10) "*The accumulated cyclone energy (ACE) also shows that the overall cyclone activity is larger in CPW events than in EPW events (table S1)*". This statement is not correct because 101.9 and 102.8 are surely not different in statistical sense. Note that the detrended ACE ranges between 29.8 and 220.4 in the five CPW years indentified in Kim et al. [2009].

I was left uncertain how useful the linear regression is for removing the SST impacts on the tropical cyclone indices. As this is a crucial element of the analysis, I feel you must justify it better. Does vertical wind shear really depend linearly on SST? A scatterplot would help convince me, but you need to at least state what a is and a statistical significance for it. I would also have appreciated it, if you had shown a before and after time series along with the SST time series in the main development region.

Figure S1 is now added to the supplementary material. In this new figure, a scatter plot of MDR SST versus MDR VWS is shown along with the linear regression line. The regression coefficient

(a = -1.96 $m\ s^{-1}\ {}^{o}C^{-1}$) is above 99% significance level. This point is now added in the caption for Table 1 and in the text (after equation 1). Figure S1c shows that the standard error in the linearly regressed projection of MDR VWS using NINO3 is reduced when the influence of MDR SST is removed from MDR VWS.

Both Kim et al. (2009) and Lee et al. (2010) use five events of the non-canonical El Niño, with the implication that there are only five events strong enough to warrant analysis. Why do you then pick 8 events? Discuss whether you risk including such weak events that signal will not detectable.

Lee et al. [2010] pointed out that the sample size (5 samples) used in Kim et al. [2005] was too small to study the influence of CPW events on Atlantic TC activity. Therefore, we increased the sample size from 5 to 8 and also considered other definitions (i.e., EMI, TNI and PMM) of the non-canonical El Niño. To test if our main conclusion is affected by the sample size, Table 1 is reproduced by using only the five strong positive phase years for each ENSO index (Table S8). As shown in the new table, the Atlantic TC indices and MDR VWS are significantly affected (at the 90% significance level) only by the canonical El Niño (EPW), consistent with our main conclusion. These points are now discussed in the new section 5.

You should mark the events chosen on the time series shown in Fig S1. I also wonder if this should be elevated out of the supplement (possibly replacing the current fig 1).

Instead of marking the ENSO years in the time series plot, the eight strongest positive phase years for CPW, EMI, TNI, PMM, and EPW are now listed in Table S2, S3, S4, S5, and S6, respectively in the supplementary material.

Although we agree that revised Figure S3 (old Figure S1) is an informative plot, we feel that it is better to keep this figure in the supplementary material.

Minor Comments

I found the surfeit of acronyms detracted from the readability of the piece. Some could be easily removed; for example, "vertical wind shear" could be shortened to "shear" instead of VWS.

In the revised manuscript, VWS is still defined for equation (1) and Table 1. But, its usage is minimized throughout the manuscript. For instance, “VWS” is changed to “wind shear” in the abstract and in the main text.

Several of the sentences had multiple clauses. Shorter, sharper sentences would help the reader.

Some editorial changes are made to improve readability of the manuscript.

In section 2, mention that you will perform the analysis in section 4 with and without this approach.

We added a new sentence “All of our analyses in section 4 are performed both with and without this approach” at the end of the section 2 (page 5, lines 119-120)”

The notation with the <> for normalization confused me a little when you started interspersing greater than conditions at the top of p7.

The notation for normalization is changed from < > to [ ] throughout the manuscript.

Wallace is misspelled on p2, line 5

This is now fixed.

Diabatic is misspelled on p2, line 9

This is now fixed.

The sentence starting on p2, line 18 is far too long and needs to be subdivided.

This is now fixed.