Reviewer #3 Evaluations:

Science Category: Science Category 3

Presentation Category: Presentation Category B

Reviewer #3 (Comments to Author):

16 July 2011

Review by C. Doswell of GRL Online Manuscript # 2011GL048583 Is there an optimal ENSO pattern that increases U.S. tornado activity? By S.K. Lee, D.E. Enfield, H. Liu, C. Wang, R. Atlas, and B. Mapes.

General Remarks:

Overall, this is not a bad effort at using the existing information to answer the titular question. However, it has a number of annoying problems that need to be addressed before it can be considered ready for publication. I've listed these in the "Specific Comments" to follow.

We would like to thank the reviewer for the extremely useful comments and suggestions. The manuscript is now revised substantially based on these comments. Please find below our reply to each comment from the reviewer.

Specific Comments:

1. *p. 2, lines 5 and 6: "Questions were raised almost immediately as to whether the series of extreme tornado outbreaks in 2011 could be linked to long-term climate changes."*

Who asked these questions? It seems to me that these questions were raised in the media, for the most part. Such a question is not likely to be attributable to anyone who knows the difference between climate and weather.

These questions were indeed raised mainly by nonscientists and largely involved the anthropogenic global warming effect. NOAA issued preliminary report that a change in the mean climate properties that are relevant to major destructive tornado events could not be detected during the last 30 years [http://www.esrl.noaa.gov/psd/csi/events/2011/tornadoes/index.html]. In any case, since this study is not particularly on the issue of climate change, the text is now changed to "….. could be linked to long-term climate variability".

2. *p. 2, lines 10-12: "However, due to the improvements in tornado detection technology with time, one must be cautious in attributing this secular increase in the number of U.S. tornadoes to a specific long-term climate signal [Brooks and Doswell, 2001]"*

It's something of a misstatement to interpret our paper to imply that we believed the changes in tornado reporting over this period of 61 years is attributable wholly or even mostly to technological change. Many of the changes in reporting frequency have been the result of a variety of policy decisions made by the National Weather Service - nationally and in the local offices.

This sentence is now changed to "However, due to numerous known deficiencies in the SWD, including improvements in tornado detection technology and changes in damage survey procedure over time, one must be cautious in attributing… [Brooks and Doswell, 2011]".

3. *p.2 lines 12-15: "Since intense and long-lived tornadoes are much more likely to be detected and reported even before a national network of Doppler radar was build [sic] in the 1990s, the number of intense U.S. tornadoes (i.e., from F3 to F5 in the Fujita-Pearson scale) in AM during 1950-2010 is obtained from the SWD (Figure S1b) and used, after detrending, as the primary diagnostic index in this study".*

Of course, the trade-off in limiting attention to F3+ tornadoes is that the sample size is correspondingly reduced. I have discussed this issue at some length in:

Doswell, C.A. III, 2007: Small sample size and data quality issues illustrated using tornado occurrence data. Electronic J. Severe Storms Meteor., 2 (5), 1-16.

which can be downloaded freely from :

http://www.flame.org/~cdoswell/publications/Scientific_Publications.html

A primary thesis of that paper is that it is challenging to use the existing tornado base to infer much about connections to various multi-year, quasi-periodic processes, such as ENSO. The vast majority of the large number of F3+ tornadoes in 1974 (the "most active year") are due to the events of a single day that year. Just how meaningful can this number be for that year? This is just one of many characteristics of the SWD that make it problematic for purposes of establishing statistical (and/or causal) relationships between tornadoes and other processes.

There is a slight trend in the data shown in Fig. S1b, but the most important characteristic is the major drop-off after 1974. This change has been explained, at least in the main in:

Verbout, S. M., H. E. Brooks, L. M. Leslie, and D. M. Schultz, 2006: Evolution of the U.S. tornado database: 1954-2003. Wea. Forecasting, 21, 86-93.

as an artifact of how tornado intensities were estimated in the period from 1950-1976. How influential is this artifact in any calculation purporting to relate the number of F3+ tornadoes to ENSO?

We acknowledge the overrating issue of F3-and-greater time series after 1974 as discussed in Verbout et al. [2006] and the references therein. In this study, to remove such long-term changes in the data, regardless of whether they are real or spurious, we detrended the number of intense tornadoes using a simple least squares linear regression. Obviously, as the reviewer points out, linear regression is not a very effective way to remove abrupt changes in a time series. Therefore, we attempt to remove a sudden drop in F3-and-greater time series after 1974, which is arguably due to the overrating issue after around that time, by assuming that the long-term averaged number of F3-and-greater tornadoes during 1975-2010 is the same as that during 1950-1974. This is achieved by performing the following simple procedure to the 1975-2010 portion of the dataset:

$$F3+(1975\text{-}2010) = F3+(1975\text{-}2010) - \mathrm{Avg}[F3+(1975\text{-}2010)] + \mathrm{Avg}[F3+(1950\text{-}1974)], \quad (1)$$

where F3+ is the number of F3-and-greater tornadoes in Apr-May, and Avg[F] represents a time average of the function F. Table 1 is reproduced using this new tornado dataset:

Index	DJF	FMA	AM
Gulf-to-U.S. moisture transport	0.08	0.19	**0.39**
Lower-level vertical wind shear	0.05	0.15	**0.33**
GoM SST	0.14	0.20	0.20
Niño-4	-0.21	-0.19	-0.18
Niño-3.4	-0.12	-0.13	-0.11
Niño-1+2	0.02	0.10	0.15
TNI	**0.27**	**0.28**	**0.32**
PNA	-0.05	-0.10	-0.20
PDO	-0.12	-0.09	-0.14
NAO	-0.01	-0.09	-0.17

As shown above, the TNI is still significantly (above 95% significance level) correlated with the revised tornado index.

Additionally, we performed a quadratic regression to remove nonlinear long-term trend in the tornado dataset, if there is any. As shown in the following table, the TNI is still significantly correlated with the number of intense tornadoes in Apr-May:

Index	DJF	FMA	AM
Gulf-to-U.S. moisture transport	0.16	0.26	**0.46**
Lower-level vertical wind shear	0.04	0.17	**0.33**
GoM SST	0.20	**0.27**	**0.26**
Niño-4	-0.23	-0.21	-0.19
Niño-3.4	-0.15	-0.15	-0.13
Niño-1+2	0.01	0.10	0.14
TNI	**0.27**	**0.30**	**0.31**
PNA	-0.07	-0.12	-0.21
PDO	-0.18	-0.18	-0.22
NAO	-0.02	-0.15	-0.20

As the reviewer points out, the number of intense tornadoes used in this study may not be the most objective metric for representing tornado years. For instance, 60 out of the 85 intense (F3 and above) U.S. tornadoes in AM of 1974 occurred on one convective day. Thus, some years with a large number of tornadoes are not qualified as outbreak years if the single day with the largest number of tornadoes in each year is taken out. Due to this limitation in the tornado metric used in this study, we test our main conclusions using different tornado indices. Another widely used metric is the intense U.S. tornado-days, which is obtained by counting the number of days in which more than a threshold number of intense (F3 and above) tornadoes occurred [e.g., Verbout et al. 2006]. The threshold number selected in this case is three and above, which roughly represents the upper 25% in the number of intense U.S. tornadoes in a given day of AM during 1950-2010. The time series of intense U.S. tornado-days in AM for 1950-2010 is shown in Figure S10. Table 1 is reproduced using the new metric as shown in Table S4. As shown, the TNI is still significantly correlated (above 95% significance level) with the intense U.S. tornado-days in AM, supporting the overall conclusions of this study.

4. *p. 2, lines 17-21. "air masses converging at different levels"*

This and the rest of the text here is a simply awful description of how steep lapse rates in the US plains come to be superimposed on low-level moisture to produce high CAPE environments.

This and the following sentences are revised to "….. Due to this so-called large-scale differential advection (i.e., any vertical variation of the horizontal advection of heat and moisture that decreases the vertical stability of the air column [Whitney and Miller, 1956]), conditionally unstable atmosphere with high convective available potential energy is formed."

5. *p. 2, lines 22-23. "a triggering mechanism, such as the horizontal spinning effect provided by the lower-level wind shear"*

"Trigger" for what? The storm? The storm's rotation? This is another terrible description of the processes involved. I suggest the authors review the literature before they try to introduce these basic background topics - for example:

Doswell, C. A. III, and L. F. Bosart, 2001: Extratropical synoptic-scale processes and severe convection. Severe Convective Storms, Meteor. Monogr ., 28, no. 50, Amer. Meteor. Soc., 27-69.

We thank the reviewer for pointing out this. As the reviewer states, the vertical wind shear effect is not a triggering mechanism, but one of the most important environmental conditions (or ingredients) needed for tornado formation. We have completely revised the manuscript to correct this and referenced Doswell and Bostart [2001]. In the revised manuscript, both the differential advection and lower-level vertical wind shear are discussed and analyzed important environmental factors for tornado activity. Therefore, Table 1, Figure 1, 3, 4, S2, S6, S7, S8 and the related discussions are all revised. The model results (EXP_TNI) indeed show that the lower-level vertical wind shear over the central and eastern U.S. is increased during a positive phase of TNI (Figure 3c in the revised manuscript), thus strengthening the overall conclusion of this study.

6. *p. 3, lines 1-2: "Consistently, the moisture transport from the GoM to the central U.S. is significantly correlated with the number of intense U.S. tornadoes in AM (see Table 1)."*

The correlation coefficients shown in this table hardly constitute a compelling demonstration of "significant" correlation. Most of the values apparently are not significant at or above the 95% confidence level, and the few that are don't exactly overwhelm me with their values.

The correlation coefficient is 0.4. Although this is statistically significant (above 95% confidence level) based on student-t test, it is not very high as the reviewer points out. Therefore, this sentence is now revised to “Consistently, the moisture transport from the GoM to the central U.S. is positively correlated (above 95% significance level) with the number of intense U.S. tornadoes in AM (see Table 1).”

7. *p. 14, Caption for Table 1; Explain the meaning of DJF, FMA, AM in this caption. I figured them out, but captions should describe the contents, including acronyms, abbreviations, etc. The sentence "Any correlation value with above the 95% significance is in bold" does not parse and needs a period at the end.*

The caption for Table 1 is now changed to “Correlation coefficients of various long-term climate patterns in December-February (DJF), February-April (FMA), and April and May (AM) with the number of intense (F3 - F5) tornados in AM during 1950-2010.…….. Correlation coefficients above the 95% significance are in bold[a].”.

8. *p. 6, line 2: Figure 1 shows that the most active years had dominant troughs in AM over the western US with low-level flow from the GoM into the plains, whereas the least active years in AM had dominant ridges over the whole US, with low-level flow into the GoM from the plains. I assume this isn't a surprise.*

Figure 1 is shown to establish that the increased large-scale differential advection and low-level vertical wind shear over the central and eastern U.S. are conductive to extreme tornado outbreaks over the U.S. As the reviewer points out, this is not a new finding. Nevertheless, it serves as a useful reference for understanding the results from the model experiments. Therefore, we still would like to keep this figure.

9. *p. 6, line 15. It's rather unclear from what is shown in this paper just what initial conditions in the atmosphere were used in the model simulations. How were the perturbations in the ensemble determined?*

We have performed 30-year control simulation using climatological global SST. For each of the 10 ensemble experiments, the initial condition is taken from the last 10 years of the control simulation. Therefore, the spread in the initial conditions largely represent internal variability of the atmosphere in this model, which is CAM3.

10. *p. 7. Line 10. "Fields" is misspelled.*

This is corrected.

11. *p. 7, lines 18-22: "these model results fully support the hypothesis that a positive phase of the TNI with cooling in the central tropical Pacific (CP) and warming in the eastern tropical Pacific (EP) enhances the large-scale differential advection in the central U.S. advecting more cold and dry upper-level air from the high-latitudes and more warm and moist lower-level from the GoM, and thus increasing U.S. tornado activity in spring."*

An interpretation regarding the phase of the TNI on various large-scale aspects of general circulation is acceptable, but the step to implying a change in F3+ tornado frequency is unwarranted by anything I've seen in this paper. I agree that these general circulation conditions might have an effect on F3+ tornado frequency, since they certainly are more favorable, but there are many causal links, including some currently unknown to science, between such features that remain to be demonstrated in this paper. The final phrase in this long sentence is far too strong a statement.

Now, we realize that this is indeed an overstatement. This sentence is now changed to “these model results support the hypothesis that a positive phase of the TNI with cooling in the central tropical Pacific (CP) and warming in the eastern tropical Pacific (EP) enhances the large-scale differential advection in the central and eastern U.S. advecting more cold and dry upper-level air from the high latitudes and more warm and moist lower-level from the GoM, and increases the lower-level vertical wind shear therein, thus providing large-scale atmospheric conditions conducive to intense tornado outbreaks over the U.S.

12. *p. 8, line 14. Should be "Figure 4 shows ..."*

For the record, this material in the body of the text simply repeats the content of the figure caption. As such, it is deadwood.

Thank you for pointing out this. This sentence is now deleted.

13. *p. 9, line 14: This phrase should read "... wind shear is one of the two critical factors required for tropical ..."*

This is now corrected.

14. *p. 10, lines 9-10. "Figure S5b shows the difference in moisture transport between these two groups." is another example of main body textual deadwood. Another example is in lines 12-13.*

These sentences are now deleted.

15. *p. 10, line 22 - p. 11, line 2: "the associated predictability of U.S. tornado activity, which can be defined as a ratio of the climate signal (the TNI index in this case) relative to the climate noise, is low."*

Exactly. This is a quite reasonable statement and simply underscores the inconsistency between reality and the statement eliciting my #11 comment.

In the revised manuscript, we tried to make this point very clear by changing the title of the manuscript to "Is there an optimal ENSO pattern that enhances large-scale atmospheric processes conducive to major tornado outbreaks in the U.S.?", and by revising the main text.

16. *p. 11, line 7: This phrase should read "... can be attributed to this year's positive phase of the TNI ..."*

This sentence is changed to "…. can be attributed to this positive phase of the TNI …".

17. *p. 11, line 14: This phrase should read "...anomalous Gulf-to-US moisture transport is weaker ..."*

This is now corrected.

18. *"Discussion" section. It seems to me that if a serious effort is going to be undertaken to connect tornado frequency to the various multi-year processes (known and yet to be discovered) that affect the general circulation, an important issue is the terrible situation regarding the predictand: observed tornado frequency. All of the efforts to use these data for this purpose have to be considered in light of its numerous known deficiencies at providing an accurate data base for this (or any other) purpose. Until that problem can be solved or some other method can be*

found to estimate the true tornado frequency, all this sort of research must include many strong caveats about the results. Moreover, the causal linkages to which I referred in an earlier comment need to be understood more thoroughly than is presently the case.

We absolutely agree with the reviewer on his concern on the quality of the tornado data. As discussed in Verbout et al. [2006] and Dowell et al. [2009], there are numerous known deficiencies in the SWD. These limitations include inherent errors in the structural damage-wind speed relationship and the associated tornado ratings, changes in damage survey procedure, and population increase. These issues with the tornado dataset are now discussed in the discussion section referencing Verbout et al. [2006] and Dowell et al. [2009]. We also clearly state that until such issues in the SWD are resolved, any tornado related climate research is subject to strong caveats.