Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-291-RC1, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Characterizing severe weather potential in synoptically weakly forced thunderstorm environments" *by* Paul W. Miller and Thomas L. Mote

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Received and published: 26 October 2017

Authors present an interesting research on one of the more challenging tasks for forecasters - how to recognize the potential for severe thunderstorms in weakly sheared / forced environments. They also use a novel approach trying to set thresholds of parameters to differentiate between nonsevere and severe events. Paper is well structured and after some minor revisions to the contents it should be ready for publication.

Introduction

Line 53: I am pretty sure that the criterion for large hail is 2.56 and not 0.56 cm.

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Discussion paper

Line 59: Authors use "signal to noise ratio" throughout the paper. Introduction to the term is made here. I am wondering, has anyone used this term in forecasting before? Or are authors introducing it? If not, references should be made.

Line 62: Should be represents instead of is represented?

Line 95: SWEAT - Severe WEAther Threat (without and)

Methods

Line 107: I understand that the dataset of weakly forced thunderstorms itself is now published somewhere else, but I strongly suggest that authors at least briefly introduce the definition of weakly forced environment. It would help the reader to better understand the paper.

Line 127: Are you sure you are always creating soundings of pre-storm environments and not soundings that may be contaminated by model simulated convection? While authors subsequently perform a check on the model vs observations performance, has there been any quality control of individual soundings?

Line 194: How exactly are the results aggregated daily and by the radar sites? Does it mean that you take the average values of parameters for particular day and radar site? This description should be expanded so that reader understands exactly what are the implications of such aggregation.

Line 211: Should be precedence, not precedent?

In general, why are authors even looking at the measures of vertical wind shear when they consider only the weakly sheared environments?

Results

Authors spend a lot of time trying to find the best "threshold" value for each parameter. Have they considered looking at this problem from probabilistic point of view?

Abbreviations and calculation of different parameters are stated in the Table 1 and then Appendix. However, I still advise authors to at least briefly introduce the mentioned, best discriminating, parameters (beyond their abbreviations) here.

Discussion

Line 359: How would lower freezing level and drier lower troposphere promote more efficient growth of hailstones? The main point here is that melting of hailstones will be less of an issue, which is important particularly for smaller hail sizes.

Same as authors, I was also suprised to see that measures of lapse rates (such as Vertical Totals) perform better than CAPE itself. Apart from possible model errors, I suspect two other reasons for that:

A/ CAPE is spatially variable, more so than the lapse rates. Could it be that the aggregation of soundings and events "smoothed" out CAPE too much?

B/ I presume that in this region of United States, it is easy to get substantial CAPE values owing to the high lower tropospheric moisture content. Then indeed, shape of CAPE (skinny vs fat) that is regulated by lapse rates makes a big difference, with "fatter" CAPE profiles involving stronger updrafts. It would be interesting to see if Normalized CAPE (NCAPE), which is CAPE divided by the depth of convective cloud (EL - LFC) would outperform CAPE by a large margin. I suggest trying out this parameter as well as authors actually have everything they need to calculate it.

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