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Comment

Interactive comment on “Lightning characteristics in Eastern Mediterranean thunderstorms during different synoptic systems” by Y. Ben Ami et al.

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Lightning characteristics in Eastern Mediterranean thunderstorms during different synoptic systems

by Ben Ami et al,

Submitted to Atmospheric Research

This paper presents an interesting analysis of lightning over the area of Israel, as a physical continuation of previous studies over the same region. The paper presents interesting results and deserves publication, there are however many aspects that have to be reconsidered before publication. I tried to provide detailed remarks that are listed

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in the following.

Major Remarks

- 1) Title: I believe that the reference to Eastern Mediterranean is rather misleading, since only a very small part of East Med. Sea is covered. I would suggest changing to “Israeli coasts” or “Crusade”.
- 2) Page 3656, line 10: why stroke density is provided per units of stroke per 25km²? Comparison with previous studies would be easier if the units were simply per km².
- 3) Page 3659, line 5: Are there any days without data during the analyzed period? Moreover how the authors believe that the results are influenced by the absence of one sensor for a 4-year period?
- 4) Page 3660, line 11: The limit of 20 strokes per day seems rather subjective. How this number was selected? Why these days are excluded? Do you believe that these few strokes are noise from the sensors? How many days are therefore excluded?
- 5) Page 3660, lines 19-23: First of all, I would like to point out that a resolution of 1x1 degree is rather poor and that many interesting meteorological patterns are filtered at such a coarse resolution. The authors should comment on that. Moreover, how many vertical levels are used? Since later on you calculate variations of temperature and wind on the grid column, it is necessary to know the vertical resolution of the used dataset. Finally, I cannot understand how you combine data at 00 and 12 UTC with lightning strokes, do you select a time-window (how many hours?) around each synoptic time for which GDAS data are available?
- 6) Page 3661, line 6: The 17% of the days omitted in the analysis correspond to what kind of synoptic settings?
- 7) Page 3663, lines 1-10: I have doubts about the correct use of CAPE on your analysis. First of all, you refer to “daily CAPE” which is a rather misleading term; CAPE is an instantaneous value at the time of sounding or model analysis. CAPE values are

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then compared against daily strokes (within the whole domain?). I believe that a more correct approach would be to combine CAPE values (at let say 1200 UTC) within each 1x1 GDAS grid with the number of strokes within each grid and for a short time-window around 1200 UTC. This approach assures that the thermodynamic conditions inferred by CAPE are correctly associated with strokes, both in space and time.

8) Page 3663, line 26 and Page 3670, lines 8-10: The explanation provided by the authors at both instants concerning IC flashes is a hypothetical assumption, that it is not supported by the available data. Can the authors confirm this assumption based on data?

9) Figure 3 and related discussion: Do the authors believe that the pattern seen on RST could be related to the small number of events? Looking into the days of RST (red crosses) a large dispersion of data is obvious and therefore the calculation of the centroid (red bold circle) is, at least, questionable. The dispersion can be related to the small number of events, but in any case, I do not believe that the results and the explanation provided by the authors can hold for such a small dataset showing such a large dispersion. Moreover, the equations shown in the upper-left part of the Figure are merely a statistical result with no practical physical meaning (e.g. they provide lightning with 0 CAPE) and therefore I suggest removing them. Taken into account my previous comment #7 on daily CAPE and this comment, I believe that the authors have to modify Figure 3, based on the aforementioned remarks.

Minor remarks

- 1) Page 3656, line 23: The sentence “timing of frontal system” is not clear.
- 2) Page 3657, line 21: Which is the meaning of a “low-level trough”?
- 3) Page 3661, line 20: A reference to MODIS data is necessary.
- 4) Page 3664, line 7: How the authors define a “thunderstorm day” and for which area? Moreover, since the authors exclude days with less than 20 strokes, how this exclusion

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can affect the correct calculation of thunderstorm days over the area?

5) Page 3665, lines 15-17: I agree that sea is source of moist and heat, but I believe that this effect is more pronounced during fall season, when SST reaches its yearly maximum, especially in September.

6) Page 3665, lines 25-26: It is of interest your statement for the position of trough axis, do you believe that you can further support this by using GDAS data?

7) Section 3.2.3: The authors use in some places the term “significant”, I wonder if this term is used from a statistical “point of view”, please clarify.

8) Page 3670, line 5: The reference to 515 ± 615 J/kg looks strange, it would be better to refer to the confidence interval of the values.

9) Figure 1: the center of ILLS shown with arrows on the Figure is not the same as the center written in page 3659, line 26.

10) Figure 5: only 7 sensors are shown on the Figure.

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