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Atmospheric Measurement Techniques Discussions

Interactive comment on "Analysis of the lightning production of convective cells" *by* J. Figueras i Ventura et al.

Anonymous Referee #2

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On 2017 during the Santis campaign, in Swiss, the unique chance for high resolution observations of convective cells and associated lightnings took place. For the first time in the Alps a LMA was set up allowing detection of intra-cloud (IC) and cloud-to-ground (CG) lightings in complex horography. The lightnings observations by LMA collected during the aforementioned campaign are analysed with EUCLID lightnings data and TRT severity rank. A general agreement between EUCLID and LMA observations is found but some relevant disagreements occurred.
TRT severity rank seems to be poorly correlated with lighting activity, while rimed particle columns is a better predictor of lighting activity. Related to their lightning activity, the detailed study of two convective cells confirmed different hydrometeor compositions between the two cells.

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Discussion paper

The paper is valuable, investigating one of the most relevant and still partially not well-understood process in clouds: the lighting activity in convective atmosphere. Nevertheless, the paper is largely unclear and improvements are needed to increase the paper clearness.
First of all, the language is sloppy with a lot of mis-spellings and unclear sentences (e.g. page 7 line 5 “The hypothesis for that is that. . .”, or same page line 17 “Moreover, when no RPC was retrieved, i.e. RPC height equals 0, The dominant type”) : a deep language review is mandatory.
General comments and recommendations. As anticipated by the generic paper’s title “Analysis of lighting production of convective cells”, the main focus of the paper is unclear: is it to find good predictors of lighting activity? Or, is it to relate lighting activity and TRT severity rank? Or, is it to compare EUCLID and LMA observing systems? Or, is it to assess lighting efficiency? Or all these points?
The introduction focuses on warnings based on lightning activity while it lacks of several references on lighting processes in clouds (Carey and Rutledge, 2000, Baker et al. 1995, Buiat et al. 2016, Adirosi et al. 2016 Mattos et al, 2016, Lund et al. 2008).
The analysis is quite shallow (the physical explanations of results are often missing), resulting quite confusing and hard to follow. For example at page 5 line 7 “The cells that spent their entire lifetime within the LMA domain boundaries tended to be shorter-lived and weaker. The highest rank of a cell generated and dissipating within the domain was moderate (2.1)”: why does LMA area show weaker storms?
At page 7 line 4 “The hypothesis for that is that the strong updraft characteristic of severe cells would lift the charge centers higher up (thus making it less likely for flashes to reach the ground) and prevent particles to grow and acquire charge at a given level (thus reducing the IC flashes likelihood)”. Here, the authors move from TRT severity rank to cell updraft. One difficulty is the relationship between TRT severity rank and updraft strength (not straightforward, indeed). Moreover, updraft–total lightning relationships of individual thunderstorms have been explored in several previous publications (Lang and Rutledge, 2002; Tessendorfet al., 2005; Wiens et al., 2005): authors

hypothesis should be framed in this contest that demonstrated the role of updraft area. The role of RPC columns is underlined by the authors. Several characteristic RPC heights derived by weather radar observations, are reported: heights error and uncertainties should be discussed and reported.
In the cells analysis time are expressed in local time?
The authors find very weak correlation between TRT severity rank and lighting activity: it is not clear why a correlation was expected. Moreover, is it the sampling statistically meaningful (only eight severe cells)?
At page 9 line 20 “Cells without lightning activity during their life cycle were mostly classified as weak but the rank of the convective cell is a poor indicator of its lightning activity, particularly considering CG flashes.” The authors should try to explain the reason for this result.
The authors conclude that different ice distribution within clouds is responsible for different lighting efficiency: however, this conclusion is supported by the analysis of only two cells. It is a clue, but for a reliable assessment more cases need to be studied.
Specific comments Page 9 line 32 It is not evident why LMA network is usefulness in complex terrain: could the same result be obtained by satellite observation?
Page 10 lines 1-4 Mosier et al 2010 and Seroka et al 2012 should be taken in account as pioneering works on this topic.
Figure 1 Quite unclear: please change basemap, add distance reference, North arrow and more contrasting colors.
Figure 2 please y-axis range equal to x-axis. 500 LMA observations sound very strange. Figure 9 RPC base height equal zero m ASL is quite confusing.
Figure 10 please insert a base map with horography in track plots. Please, make y-axis with same range.
Figure 11 It is not clear the need to multiply by ten the cell rank. Please, make y-axis with same range
Figure 12 The maximum values seem to be spikes (anomaly and isolated very high values)

Figure 13 which is the RPC base or top altitude estimations uncertainties? 50 meters?

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