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Interactive comment on "SAETTA: high resolution 3D mapping of the total lightning activity in the Mediterranean basin over Corsica, with a focus on a MCS event" *by* Sylvain Coquillat et al.

Anonymous Referee #2

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Title: SAETTA: high resolution 3D mapping of the total lightning activity in the Mediterranean basin over Corsica, with a focus on a MCS event

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The material presented in this manuscript provides (1) a detailed technical background and performance summary for the SAETTA LMA network; (2) a climatological summary of total lightning observations for its early years of operation in its mixed moun-

tainous/maritime operating region; and (3) a case study of an MCS that occurred in 2016 and exhibited interesting/unique flash-level behavior. This is a wonderful collection of materials that will likely make this paper the essential background reference for future users of this 3-dimenstional total lightning mapping network. The tri-level flashes discussed in Section 4.2 are unique (in the eyes of this reviewer). This material clearly deserves to be published, and is probably already being referenced in manuscripts in-preparation.

When reading this manuscript, I struggled with two competing reactions: the breadth and depth of material that made it a bit "heavy" to this reader, which was at odds with several of my annotations that implied additional work and refinement of presentation. I am sure that the authors had similar struggles when preparing the manuscript. The review comments that follow do not reconcile this issue. Sorry. However, I leave these comments in the good hand of the authors to pick-through and decide what works for them. I do not need to review or accept the changes that they make.

Broad Issues:

1. The "tri-level" (stacked?) flashes that were found to develop and then propagate in the trailing stratiform region may not be well-explained by conditions leading to the ice-based charging mechanism suggested by Dye and Bansemer (2019). The authors' discussion between line 25 on page 25 and line 7 on the following page describe other factors that require more-detailed analyses before interactions between separate-but-overlapping storms and upper-level screening can be excluded. The vertical separation of the two upper charge regions associated with the upper-level flashes (I think that I see about 4 km in Figure 13 – not the "2-3 km" that they indicate on Page 25, line 22) seems rather large for screening, and rather and high for charge separation by mesoscale updrafts. Overall – my only suggestion is that the authors "soften" the statements in the abstract (lines 26-27 in page 1) and conclusions (lines 8-10 on page 25), instead of the current rather-direct attribution to this effect.

2. The description and performance characterization of the LMA network in Section 2 is excellent, but I have two issues that are worth mentioning. First, on lines 1-2 on page 4, the authors state a single benefit of having 12 stations rather than fewer stations. There are other good reasons worth mentioning, such as (1) redundancy/reliability (short-term and long-term failure), (2) the effect of localized high-rate storms on a sensor's contribution to more-distant activity, and (3) the improved geometry for geo-location of distant lighting while maintaining height accuracy for nearby low-altitude lightning channels. I am sure that the authors can think of other benefits. The second issue relates to the depiction of vertical accuracy only at 10 km altitude. The vertical accuracy will be worse at about 3 km height (MSL) – above all the mountains. Users of these data would benefit from understanding this issue, either through additional figures or (at least) some words by the authors.

3. It would be nice if the first use of the xlma plots (Figure 5 for density and figure 6 for sources) were described in a bit more detail, including the distance (rather than lat-lon) scales, and then attributed to Ron Thomas and his xlma program.

4. Most of the figures will be difficult for the reader to interpret. The fonts are too small to be read; there is wasted space (large separation between panels) that should be filled with real content (e.g., Figures 7 and 8, among others) ; It is difficult to see the overshooting-tops in Figure 10; the color separation in Figure 9 makes it difficult to see the different years; and the "full xlma figures" maintain a lot of content (wasted visual space) that is not central to that points discussed by the authors (Figures 11 and 13). Maybe all figures should be reviewed by an author or colleague that never got the chance to manually zoom-in on the key features, so that they have the same disadvantages as the future readers.

5. The terrain blockage analysis is an important element of this work. The technical discussion in the appendix does not mention how the authors handled refraction at VHF. Was it ignored, or was the radius of the earth adjusted (increased) to provide a simple correction for this? It would be interesting to know if sources were actually

located at lower-than-expected heights.

Lesser Issues:

6. The end date for the climatology (2016) seems odd, given the availability of data for 2017 and 2018. A sentence providing a rationale would be helpful. It might help explain the awkward statement on line 15 of page 15 ("easy to extract") regarding the 2017 storm-day data.

7. There are several papers in the reference list that are not cited in the paper

8. I have a number of lesser comments, minor corrections, and editorial suggestions in a hand-annotated version of the manuscript. So that this review can be timely, a PDF scanned copy of this annotated version will be provided separately.

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