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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.

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ATMOSPHERIC MEASUREMENT TECHNIQUES DISCUSSIONS

Manuscript amt-2019-192

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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Discussion paper

William Rison

Dear Associate Editor,

We are very grateful to Referee #2 for his criticisms and suggestions that we tried to take into account to improve the manuscript significantly. We also thank Referee #2 for the comprehensive corrections he provided in the PDF scanned copy he sent. You will find below our item-by-item response (indicated by *) to the comments and recommendations of Referee #2. The proposed modifications appear in red font in the revised manuscript in order to readily identify them.

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.

* We agree with Referee #2. We changed lines 21-26 in page 25 of the initial manuscript and also the abstract accordingly.

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 (shortterm and long-term failure), (2) the effect of localized high-rate storms on a sensor’s contri-

bution 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.

* Referee #2 is right on the 2 issues raised. It is well worth noticing most of the advantages arising from a 12-station network. We added the advantages proposed by Referee #2 and somewhat changed the initial sentence. We agree with Referee #2 about the second issue. We accordingly added some comments at the end of the 2nd paragraph of Section 2.3.

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.

* We added a sentence at the beginning of the first paragraph in Section 3.1 in order to describe the XLMA plots (see also the last sentence of the introduction about the XLMA tool and its attribution to Ron Thomas). As far as scales are considered, all XLMA plots are in Cartesian coordinates except Figure 5 left. As a matter of fact, Figure 5 displays 2 graphs. The left one is in lat-lon coordinates, the second one is in km-km coordinates. This allows comparing both sets of coordinates at least one time in the paper and identifying the latitude and longitude of the domain. We prefer to keep this information.

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.

* We enlarged all the figures including XLMA plots, enlarged graphs in Figures 7 and 8 (and also in Figures 12 and 14), and changed colors and font size in Figure 9. We prefer to keep the whole XLMA panels in Figures 11 and 13 because they allow having good landmarks for example when analyzing Figure 14 left with the help of bottom left panel in top right Figure 13. As far as Figure 10 is considered, it is difficult to obtain a good quality image of its height-versus-time panel because the event is very long so that, for instance, overshooting-tops (or convective surges) cannot be correctly detailed. This is why we added information in the caption of Figure 11, which exhibits the detail of one of the convective surges (#5). If you think that the font size of all XLMA plots should be enlarged for the edition of the paper, we can provide new figures.

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.

* For simplification, the atmospheric refraction is not taken into account in the calculation. This is now clearly stated in the text (Section 2.2) and in Appendix A, as rightly suggested by Referee #2. Inferences of this simplification on the results are drawn at the end of Section 2.2 (i.e. VHF sources can be detected even below the limits indicated in Figure 2).

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.

* Only data from 2014 to 2016 have been fully processed. However, as it was easy and simple to get the information about the dates of the 2017 events (from quicklooks), we choose to add this information in Figure 9 in order to enrich the statistics on this point. This is now properly mentioned in the text.

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

* Yes, we identified them and removed them.

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.

* We carefully corrected the manuscript according to the annotations of Referee #2. Two of them were somewhat difficult to read and/or understand (page 2 between lines 14 and 15; and page 13 in the right margin between lines 15 and the bottom of the page) so we did not consider them. Here are some comments about the corrections made, according to the pages of the pdf copy provided by Referee #2.

* Page 2: We kept the separation between "convection" and "latent heat release" because we describe the chain of processes one after the other (literally, convection is the vertical macroscopic motion, at the origin of the cooling of air, itself at the origin of water vapor condensation, itself at the origin of latent heat release. . .). OK for all other corrections.

* Page 3: OK for all corrections.

* Page 4: We added a comment about the time accuracy in order to clarify the purpose. OK for all other corrections.

* Pages 5 and 6: OK for the corrections.

* Page 8: We added a reference to Figure 13 in Thomas et al (2004) in order to clarify the evolution of the altitude z uncertainty versus r2. OK for all other corrections.

* Page 9: OK for the correction.

* Page 10: OK for the corrections. Data for the year 2017 are not considered because they have not yet been fully processed. Simple information such the number of lightning days is though available (and therefore reported in the paper) since it is issued from the daily quicklooks and not from the data processing. This is explained in Section 3.2.

* Page 11: OK for the corrections. We did not add the CG strokes data from the Euclid/Meteorage network because the display in Figure 6 left is not ambiguous about the cloud-to-ground nature of the lightning.

* Page 12: At the line 15 we did not mention the simultaneous propagation in the main negative charge region because it does not appears in the altitude versus East-West distance display in Figure 6 right (almost no red points distributed along the corresponding horizontal channel). Figure 6, as many other figures in the manuscript have been enlarged. OK for the corrections.

* Page 13: OK for the corrections (except the comments in the right margin as indicated above). We deleted the sentences relative to the aircraft trajectories in order to avoid the addition of new figures since the paper is already long. In fact this point does not really matter in this article. It will be later if we can get the flight data to evaluate the location efficiency of SAETTA. For information, the flight we were talking about produced an exceptionally huge amount of VHF sources (406210 sources, see figure here after).

* Page 14: OK for the corrections. We deleted the mention to the "secondary relief". The graphs in Figure7 have been enlarged.

* Page 15: We added a sentence to clarify the point raised about 2017 data. OK for the corrections.

* Page 16: OK for the corrections.

* Page 17: OK for the corrections. The high altitude discharges are said high but not very high. Referee #2 is right in his comment when saying that the considered discharges are not so high since they can appear at much higher altitudes as reported for instance by Krehbiel et al (2002). This is why we call them high altitude discharges rather than very high altitude discharges. As suggested, we added a sentence to describe the convective surges, with a reference to the paper of Krehbiel et al. (2002). We also changed the end of the 1st paragraph of Section 4.1 in order to clarify the comparison between present MCS and usual MCSs described in the literature.

* Page 18: OK for the corrections. The use of the XLMA tool is now mentioned at the end of the introduction.

* Page 19: Figures now enlarged, color scale not changed.

* Page 20: OK for the corrections. We modified the end of the sentence in line 9.

* Page 21: OK for the corrections. We enlarged the figures and also added some comments to help the reader. Details on the maxima were given because it is precisely difficult to correctly identify them in Figure 12. We simplified the corresponding sentences.

* Page 22: OK for the correction. We enlarged the graphs in Figure 13 instead of deleting some panels.

* Pages 23 and 24: OK for the corrections.

* Page 25: OK for the corrections. We modified the discussion about the conceptual model of Dye and Bansemer (2018, 2019) as suggested by Referee #2.

* Pages 26, 27, and 28: OK for the corrections.

* Page 29: Referee #2 is right about the atmospheric refraction. We neglected its effects in the calculation. It is now indicated in the text with a short analysis on the

consequences on the altitudes calculated.

* Page 31: OK for the corrections.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2019-192/amt-2019-192-AC2-supplement.pdf

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Discussion paper

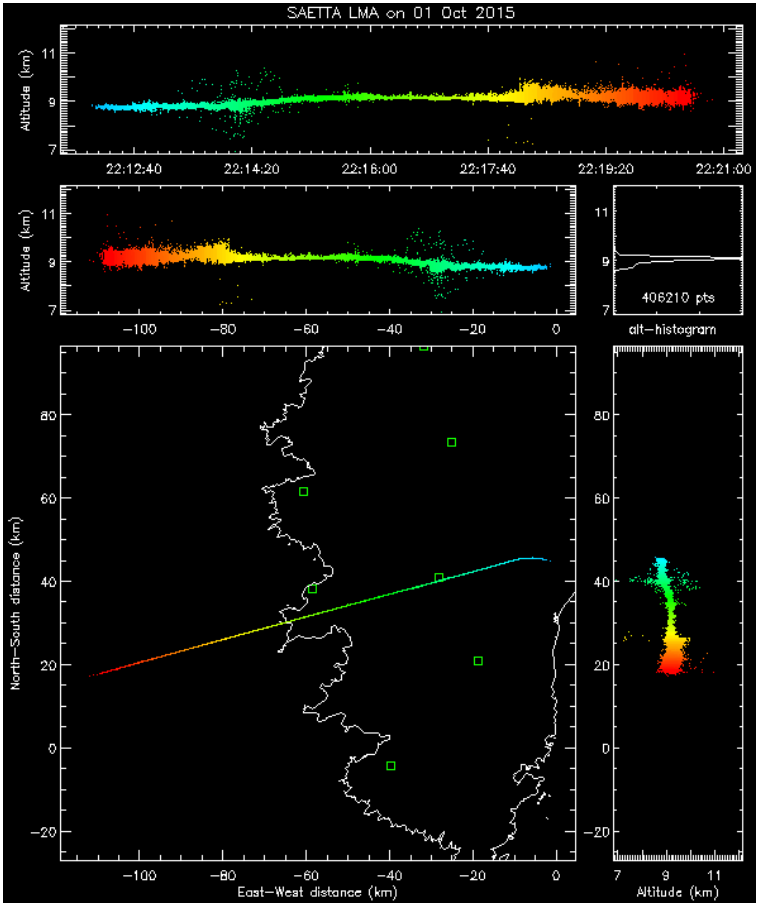

Fig. 1. Track of a commercial aircraft flying at about 9 km altitude that produced an exceptionally huge stream of sparks, which may be distinguished in the cumulative number of filtered VHF sources in the to