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Atmospheric Measurement Techniques Discussions

Interactive comment on "Concurrent Satellite and ground-based Lightning Observations from the Optical Lightning Imaging Sensor (ISS-LIS), the LF network Meteorage and the SAETTA LMA in the northwestern Mediterranean region" *by* Felix Erdmann et al.

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1 AUTHORS RESPONSE TO EDITOR COMMENTS

RC2: Anonymous Referee #1, 31 Jul 2019

The authors thank the referee #1 for his constructive comments. We included the general and specific comments in the updated paper manuscript. Based on the general comments, the results section is revised in total. Two Figures (former 14 and 15) are removed and we introduced two tables summarizing the statistics of investigated flash characteristics. The specific comments are addressed in the following.
[We indicate the line number in the document showing track changes for each comment.]

1.1 Specific Comments

1. Check the order of introduction of instruments in Section 1 (Introduction) to make sure each instrument is described before being referred to by its acronym. There are also cross-references between instruments in odd places, such a mention of LF/VLF systems in the paragraph (line 80) discussing VHF systems. Overall, I thought the introduction could be shortened somewhat to focus more on the aims of the study, with less context about the lightning detection problem as a whole.

The introduction was revised and refined. It should be clearer in terms of structure and the references. For example, papers of Cummins and Murphy (2009) and Nag et al. (2015) were added that provide summaries of the different LLSs. The introduction focuses more on the former studies and less on sensor characteristics than before.
[please see changes in section 1, pages 2-5]

2. Line 84: WWLLN relies on ionospheric reflections but operates at VLF. As noted on line 164, the LF measurements in this study do not include ionospheric reflection.

This technical paragraph was shortened in order to focus on the literature review.
[please see changes in line 93-99]

3. Fig. 1c may be two lightning flashes in the VHF data, and if it was automatically identified by an algorithm illustrates the challenges in flash classification. There is a large gap in the channels to the SE, and if this were one flash I would expect to see that gap filled given the otherwise very well-resolved channels. Was there evidence of a new channel developing and exhibiting bidirectional development in the flash to the SE at $\sim$0.1 elapsed seconds?

As a general comment the automatic algorithms to identify flashes from signals like VHF sources, LF pulses/strokes or optical events work in a statistical sense, however, neither the algorithms nor an observer can always distinguish individual flashes perfectly.
The flash in Figure 1c was identified as one flash by three different algorithms. Branches to both sides of the mentioned gap to the SE propagate towards each other. We looked at the flash development through a series of animations using SAETTA observations. We also looked in the raw LMA data to evaluate the idea given in the comment. In fact, there was a significant amount of VHF records and we could not clearly identify the single branches. Conclusively, we think that Figure 1c shows one flash that originated as two flashes whose channels connected. The connection is not directly visible in the data, as it is likely masked by the lack of reconstruction.
[please see changes in Figure 1, page 28]

4. Line 180: Recent studies by Chmielewski and Bruning (2016, 10.1002 / 2016jd025159) and Koshak, Mach and Bitzer (2018, 10.1175 / JTECH-D-17-0041.1) show that changes to the network geometry can have a significant influence on detection efficiency and location precision. This effect may be important for the SAETTA network which has a long N-S baseline, in contrast to the somewhat more circular and compact network in Thomas et al. (2004).

We agree with Referee #1 about the influence of the network geometry on the location precision. Coquillat et al. (2019) show that the displacement of 2 stations located in the south-west and south-east of Corsica towards 2 positions located in the extreme south and the extreme north of the island in 2016 led to a modification of the errors (Figure 3 in Coquillat et al., 2019). The radial error was markedly reduced except in the north of the studied domain meanwhile the altitude error increased almost everywhere at larger distances from the network. Therefore when different sets of at least 6 stations are involved in the calculation of the VHF source position one would expect a different geometry of the network, which influences the location precision. Nevertheless given the maximum SAETTA location errors (and errors within 50 km of the LMA) that are given in section 2.1, the location accuracy is sufficient for an intercomparison to both Meteorage and ISS-LIS as the maximum SAETTA location errors are not greater than the location errors of the LSSs in the comparison.

As far as detection efficiency is considered, Chmielewski and Bruning (2016) found in general high DE (>90 $\%$) for all networks with different receiver thresholds up to a range of 150 km from the LMA center. Within a distance of 200 km, the modeled DE is still greater than 85 $\%$ in their study. The actual DE of SAETTA is difficult to address as the region and conditions are different from the regions used by Chmielewski and Bruning. One study is currently underway to assess SAETTA location accuracy through a comparison between SAETTA records and aircraft tracks in ice clouds.
The Koshak et al. (2018) paper is an interesting supplement and it is now given as a

reference in section 2.1.
[please see changes in line 200-208]

5. Lines 182-183: Does the study domain refer to the 350 km max range of detection or the somewhat smaller lat/lon box at the beginning of section 3?

The study domain is limited to the region shown in Figure 1(a). It is introduced in the opening paragraph of section 2 to be within 40.5 N to 44.0N and 7.0E to 11.0E. The maximum distance to SAETTA's center approximates 271 km (the SW corner of the domain) and 238 km (the NE corner of the domain).
The reference to Figure 1(a) was added to clarify the meaning of study domain.
[please see changes in line 211]

6. Line 211: I don't have a concern here, so much as I wish to highlight that the authors raise an essential question about the measurements: "it is questionable whether LIS groups really correspond to (V)LF pulses/strokes." I agree with prior studies that the group is the fundamental physical measurable from the instrument - it is an ∼instantaneous light emission tied to heating by a "large" current flow along a channel, and the events register the extent of the light scattered by that process. However, the authors are also right to point out that not all (V)LF pulses/strokes have a corresponding group, which suggests that either the instruments are sensitive to different physics, or the (V)LF and optical measurements lack the necessary sensitivity to see what is actually the same physics. In the end, the authors' approach of clustering using the events instead of the group centroids is a good choice, since considering events will help them align with LIS data and better identify coincident ground strokes that might happen at some distance from the centroid of the light emission as observed at cloud top, but I would disagree that the events are the fundamental physical detection.

Yes, it is difficult to find a physical process representing the optical LIS events. Groups are likely the best representation of fundamental discharge processes in optical lightning data. Events should be seen as smallest, elementary measured units of LIS-like instruments. The rationale on these lines was refined (e.g., the word "fundamental" in line 234 was replaced by "elementary" to be consisent with the further use of the term *element* for LIS events and LF pulses/strokes).
[please see changes in lines 237, 243-244]

7. In the paragraph beginning line 325, it is ambiguous whether the authors think the missed flashes nearest SAETTA were truly missed or if station downtime were to blame. This is especially interesting because the authors close the paragraph by stating that SAETTA is not an absolutely reliable DE reference. Pédeboy et al. (2018) is probably more explicit about the details, but it is not in a peer reviewed article; regardless it would be helpful to clarify here what the authors mean.

We looked in detail in our raw SAETTA data, with information from individual stations. It is found that VHF signals were observed for most of the missed (by SAETTA) flashes (114 of 120). The most important reasons for the classification as missed were signals at less than 6 stations and thus no reconstructed VHF source (77 of 120) and locating/timing differences just exceeding the algorithm criteria of 0.2 /0.3 s between observed flashes and VHF sources (34 of 120) or reconstructed VHF sources had too high reduced chi^2 values and were disregarded by our algorithm. As we do not know the origin of the signal in the raw data, we cannot assign raw data signals at individual stations to single flashes. This needs the reconstruction of VHF sources using at least 6 stations.

The missed flashes nearest SAETTA were not detected by SAETTA due to a reduced number of active stations (5 of 15), reconstructed SAETTA sources showed a slightly too high chi^2 uncertainty (5 of 15) or SAETTA reconstructed sources are found slightly shifted in space/time to a Meteorage flash (3 of 15). Only 2 Meteorage flashes in the proximity of SAETTA were really not detected.
[please see changes in lines 368-383]

8. Given the predominance of flashes that occur outside SAETTA, altitude errors will be large and the total number of sources detected per flash will be small. How does this affect the results that depend on altitude retrieval from SAETTA in section 3.3?

SAETTA detection range allows in general for many (order 100) sources per flash up to distances of about 350 km. The issue regarding the detection of low altitude sources in large distance to the LMA was added in the section 2.3. SAETTA is even capable of detecting sources lower than the theoretical values added in section 2.3, because most stations are well above sea level and the highest station is located at an altitude of 1950 mASL. In theory, this station has a direct line of sight to sources at about 1.6 kmASL altitude in a distance of 300 km to the station (which would be outside the study domain).

The altitude errors are given in section 2.3 and are expected to be less than 500 m within the entire domain (maximum distance to the LMA center of 270 km in the SW corner, significantly lower altitude errors closer to the network). The results in section 3.3 are discussed taking the theoretical altitude errors into account, however, actual errors also depend highly on the set of stations used for the reconstruction of the VHF source and the source altitude. This is especially true for each individual VHF source. The overall error of a sample of VHF sources should be more consistent and closer to the theoretical model than individual source errors. Flash altitudes in section 3.3 use statistical values of concurrent SAETTA sources (i.e. the mean, 10th and 90th percentile) and should represent the statistical flash altitudes within the given range of uncertainty.

9. Line 595: This statement is accurate for the authors' data, but I would predict storms with inverted polarity would have the opposite expectation for detectability as function of polarity. This context would be helpful if another region were to be studied with the same methodology.

It will need definitely further research to investigate how this relationship between polarity of the maximum (LF) current and the flash altitude behaves in different regions and different storm types. The suggested context is added to discussion in the paper.
[please see changes in line 692-695]

1.2 Technical Corrections

After revisions are completed I recommend an additional read for flow and a few missing words. For instance, on line 80: "uses very high frequency (VHF)" needs "radio signals" or some other noun at the end of the sentence.

The entire paper is revised.

The colon at the end of line 46 seems like it was from an earlier revision where the instruments were introduced in a different way?

Yes, it was.
[please see changes in line 53]

Lines 609-13: “peak” here could be misinterpreted as “maximum.” I suggest “mode” or “most frequent.”

Ok.
[please see changes in line 712]

2 ADDITIONAL TRACK CHANGES

Instance	Description
Figure 1	Time series added (d)
Section 2.1	Raw amplitude count introduced as optical signal measure
Section 2.5	Revised and slightly shortened
Section 3.3	Revised
Table 2	New table added (section 3.3)
Table 3	New table added (section 3.3)
Figure 13	Labels updated (Radiance -> Amplitude count)
Figure 14	Removed (information gain after Figure 13 is low)
Figure 15	Removed (results in Table 3 and Figure 16)