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.

Anonymous Referee #1

Received and published: 31 July 2019

General Comments

The authors provide a comprehensive overview of the relative performance of the Lightning Imaging Sensor and a traditional lightning location system (Meteorage), and use a research-grade VHF lightning mapping system (the SAETTA LMA) for reference. The authors apply a custom clustering methodology to all three datasets. The relative performance of these systems is of great interest beyond this specific project; the authors

note this fact and I agree with them.

Overall, the analysis method and results are clearly and comprehensively described, so this study only requires minor revisions. The study is relatively long, with dense prose summarizing each of the figures. The authors might consider trimming out some of the description of the figures in favor of (1) summarizing where things behave as expected, and (2) focusing on the most relevant or surprising findings.

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.

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.

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?

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).

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?

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.

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.

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?

9. Line 595: This statement is accurate for the authors' data, but I would predict storms

C3

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.

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

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

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-149, 2019.