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Atmospheric Measurement Techniques Discussions
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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.

Felix Erdmann et al.

erdmann.professional@gmx.de

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1 AUTHORS RESPONSE TO EDITOR COMMENTS

RC1: Anonymous Referee #3, 30 Jul 2019

The authors thank the referee #3 for the detailed and 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.]

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Discussion paper

1.1 Specific Comments

1. Sentence on lines 47-48: networks with “widely-spaced” VLF/LF sensors (like Meteorage and the U.S. NLDN) report far more cloud pulses than return strokes, because they are sensitive to vertically-oriented current-carrying channels in “larger” IC flashes (see Cummins and Murphy, 2009 or Nag et al., 2015). I would say that they have “... somewhat limited total lightning detection efficiency (DE).” Shorter-baseline VLF/LF systems like LINET and portions of the ENTLN (cited in this work) have very high total lightning flash DE.

The statement is shortened and implemented after the introduction and citation of Cummins and Murphy (2009) and Nag et al. (2015).
Comment: The comment is true nowadays looking at the overall numbers of detected strokes/pulses (although it is different for the DE), however, our Meteorage data include no IC pulses until 2008. The first year exhibiting more IC pulses than CG strokes in Meteorage data was 2015 (within the study region of this work). Meteorage has still a higher (spatial) accuracy and DE for CG strokes than for IC pulses. The signal strengths (currents) are also significantly stronger for CG strokes than IC pulses. The predominant vertical channels within the cloud (Nag et al. 2015) do not exist for all IC discharges, thus their detection can be difficult for (V)LF LLSs. Return strokes associated with CG strokes do indeed very often produce sufficient (V)LF radiation to be measured by those LLSs (attenuation with distance to the discharge).

We want to point out that the detection efficieny (DE) of CG strokes is in general high for VLF/LF networks. The IC DE depends on the baseline distance. In particular, Meteorage is not a "widely-spaced" network as stated in the comment. The baseline distance is similar to that of e.g. LINET in Germany. The overall DE of Meteorage, with high CG DE and somewhat lower IC DE, is sufficient for a comparison to optical satellite instruments like ISS-LIS (it likely exceeds the ISS-LIS DE in most cases).

[please see changes in lines 54-60]

2. Line 57: This is the first use of the term "relative DE", and (unfortunately) one of the referenced papers (Bitzer et al, 2016) use this term to mean the ratio of the conditional probabilities (see their equation (2)), but others do not use this definition. SO – you need to say that you use the common definition of relative DE used in many studies, which is the percentage of matched flashes divided by the number of flash in the other (reference) LLS (if that is what you are doing).

The relative DE is briefly introduced prior to the literature review.
[please see changes in lines 48-49]

3. Line 138: the sentence about "...within 330 ms and 5.5 km..." is not really correct. No group associated with these values will be included in the flash - these are normalizing parameters for a Euclidian distance measure (see Mach et al., 2007 - figure 2)

The weighted Euclidean distance concept is introduced in the paper to correct the sentence. **[please see changes in lines 154-156]**

4. Line 162: The Meteorage network only requires two sensors to report a lightning discharge (see Cummins and Murphy, 2009).

We confirm that two sensors are sufficient. They use the time and angle of arrival to create four variables for three target values (time, latitude, longitude).
[please see changes in line 182]

5. The description of LMA in lines 169-177 is not quite correct and should be reviewed by co-authors on your associated SAETTA paper. It reports leader development (associated with breakdown processes or fast leader propagation in established channels), serving to produce a spatial map of possible paths for later, high-current processes in the flash. The phrase “Fast CG discharges traveling between the cloud and ground” is not correct – you are probably referring to dart leaders that can occur in pre-established leader channels for earlier CG strokes, as well as in established channels within IC flashes.

We removed the phrase "Fast CG discharges travelling between the cloud and ground in time frames shorter than 80 μs might be missed." It should indicate instead that flash components with continuous VHF radiation would be hardly mapped with a LMA due to time-of-arrival technique and the sampling method.
[please see changes in line 194]

6. Lines 211-213: regarding the authors' discussion of the rationale for using events rather than groups in the flash algorithm: This reviewer (and many others in the references cited in this work) firmly believes that reports from mid- to long-range ground-based VLF/LF LLS networks match well with LIS groups. Long vertical channels during periods of high currents provide localized light sources, and most of this light is produced during periods of less than 2 ms. The timing and centroid location of a LIS group are a good match for such sources. Figures 8 and 9 in Bitzer et al. (2016) show the very tight time- and space-correlations for these discharges. However, this does NOT mean that ALL LIS groups are space- and time-correlated with VLF/LF strokes/pulses. The authors' accompanying rationale related to lightning mapping and comparisons with SAETTA are quite reasonable. I ask that the authors refine/revise the rationale on these lines under the light of these comments.

The formulation was too stringent. The authors agree that LIS groups often represent similar physical phenomena (discharge processes) as (V)LF pulses/strokes. The significantly higher number of LIS groups than (V)LF sources shows that LIS groups can indicate additional discharge mechanisms that are not seen in the (V)LF range. The rationale is adapted.
[please see changes in lines 243-244]

7. Lines 284-292: regarding flash matching: This text seems to indicate that flashes are matched if any elements in the two LLS meet the time constraint, and if any elements (the same or different that the ones meeting the time constraint) meet the spatial constraint. This seems problematic, when multiple flashes are closely spaced in time and space. I ask that the authors clarify this point, to be sure that the algorithm is described properly.

The matching algorithm uses a combined distance-time criterion. The same elements must meet both the spatial and the time constraint. Flashes are only matched if at least two elements (one per flash) meet both the spatial and the temporal criteria for a match.
Section 2.5 is revised in total.
[please see changes in lines 317-323]

8. Lines 397-403: The group:pulse distances should not necessarily be greater than the event:pulse distance, because the group locations are interpolated to sub-pixel spatial resolution by the radiance-weighting of the spatial centroid. Also the position differences reported by Bitzer et al. (2016) are much larger than the actual position differences because of +/- 5 km latitude location offsets associated with LIS yaw maneuvers (see Zhang et al., 2019). This issue is fixed in ISS-LIS. You may want to refine your analysis in the light of these issues.

It is true that group centroids can be located in any location while event locations are fixed to the pixel centers. Group centroids can in fact be closer or further away from VLF/LF pulses/strokes (which are not bound to pixel centers obviously). One should expect statistically about the same distance between VLF/LF pulses/strokes and LIS events and groups. We removed the statement and added a short discription of the difference between event and group locations.

Zhang et al. (2019) provide very interesting findings and it is a valuable reference to be cited here. The correction during TRMM yaw maneuvers results in slightly lower distances to ground-based LLS for TRMM-LIS groups (1-2 km in Zhang et al.) than for ISS-LIS events in this study (2-4 km).
[please see changes in lines 447-460]

9. Lines 469 and later: the units for the LIS radiance product is not correct, in terms of the spectral density. The units should be nm, not μm. See Zhang et al., 2019, which also shows that the TRMM-LIS minimum radiance is about 3 μJSr-1m-2nm-1, indicating that it has a lower threshold than ISS-LIS!

The unit issue was further examined in collaboration with D. Buechler (University of Alabama Huntsville, Huntsville, AL, USA). We found that the available version of ISS-LIS P0.2 had not yet included the calibrated radiance. The radiance variable in the data has the same values as (and is therefore identical to) the uncalibrated (raw) optical amplitude count. Hence, all analyses of optical signal strength ("radiance") are actually amplitude counts. The paper terminology is changed and the corresponding Figure labels are updated.
[please see changes in lines 162-166 and e.g. in lines 532-533]

1.2 Editorial Corrections and Suggestions

10. Introduction, first paragraph: it might be helpful for some readers to also know that total lightning flash rate within a storm is associated with storm intensity features such as ice flux and updraft volume and rate (see Deierling and Petersen (2008) and Deierling et al., 2008, among others)

Suggested references were cited with a short summary of their work.
[please see changes in lines 27-32]

11. Sentence starting on line 41: "SAETTA" and "mapping" have not been introduced yet, and this sentence is probably not required at this point in the paper.

The second part of the sentence was indeed not required here and is removed.
[please see changes in lines 46-47]

12. Line 50: the term "LIS groups" is used, but there has been no description of the LIS products (provided later in the manuscript). It might be helpful to do this in 2-3 sentences, or point the reader to section 2.1.

Groups are briefly explained.
[please see changes in line 64-65]

13. Line 90: suggest replacing "for GLM" to "as GLM"

ok.
[please see changes in line 105]

14. Line 94: “orbits” should be “orbit”, since there is only one orbit

ok.
[please see changes in lines 109]

15. Sentence starting on line 105 (“Among the...), does not seen to be a complete sentence.

The sentence was misleading and should be clear now.
[please see changes in lines 120-122]

16. Line 112: suggest changing “... chacteristics in lightning detection...” to “...lightning detection characteristics...”

ok.
[please see changes in lines 127]

17. Line 119: suggest changing “our paper” to “this work”

ok.
[please see changes in lines 134]

18. Lines 151-152: suggest changing “... from the lightning discharge on earth...” to be “... from the optical source at cloud-top...”

ok.
[please see changes in lines 171-172]

19. Line 196: The sentence starting on this line does not make sense to me. Two events in a flash can be much farther apart than 14.3 km. If you are referring to adjacent events then I do not see how they can be 11.9 km apart at nadir. Please clarify.

We tried to give a value for the distance constraint of the NASA ISS-LIS clustering algorithm to be compared to our in-house algorithm. We cannot really identify such a value between the events (additional communication to D. Mach [NASA MSFC, Huntsville, AL, USA]). The NASA ISS-LIS clustering algorithm uses a weighted Euclidian distance (space and time) between group centroids. The paper is corrected.
[please see changes in lines 225-229]

20. Line 204: suggest changing “group” to “collection”, given the LIS definition of group

ok.
[please see changes in line 235]

21. Line 222: Use of the words “initial element” suggests that all the sources must be within the ds limit of the first source. This can cause spatially propagating flashes to be broken up. This should be clarified.

The term "the initial element" is replaced by "any element of the flash". The algorithm can indeed treat spatially propagating flashes.
[please see changes in lines 255-259]

22. Line 299: change “onl yup” to “only up”

ok.
[please see changes in lines 342]

23. Section 3.2: Just a comment: It seems that the distance and timing offset distributions are produced by comparing all LIS events and all Meteorage pulse/strokes, so (for LIS) it mergers any underlying space:time correlations for individual matched pairs with the timeevolution of light that is observed by LIS, due to things like leader propagation and continuing current in long channels.

We do not completely understand this comment. We would appreciate if you explain your comment further.

24. Lines 444-445: a contributing factor could also be related to increasing length of the optical sources due to finite leader and return-stroke velocities.

Do you mean we need to consider the time of propagation of the leader or return stroke to explain the time difference between the optical signal and the Meteorage CG stroke?

25. Lines 516-518: does this paragraph belong before the previous paragraph?

Yes, the brief summary fits better directly after the comparison and before discussing the exceptionally long flashes.
[please see changes in lines 581-590]

26. Line 528: The vertical displacement of LMA sources in the xlma display at large distances from the center of the network have always troubled me, even after speaking to the developer of xlma. It seems that refraction is not being handled, so that distant source heights are not really useable. Additional insights would be nice but are not required.

Currently the SAETTA data processing is similar to the other LMA processing chains and does not include any refraction correction. As a matter of fact electromagnetic waves propagating in the clear sky are downward deflected because of the refractive index gradient that is most often downward directed. If refraction is not taken into account in the calculation of the VHF source position, the calculated altitudes may overestimate the true altitudes for distant events from the network. This is something that should be investigated in the future.

27. Line 543: suggest changing “It can be constituted that...” to “Overall, ...”

Ok.
[please see changes in line 618]

28. The term “average mean radiance” (and similar terms that reflect statistics of statistics) require the reader to think hard to interpret the variables. Possible better wording could be “the mean radiance averaged over all heights” or something like that

Okay, the wording is clarified.
[please see changes in e.g. lines 633-634]

29. Line 550: is it “maximum radiance per flash” or “maximum event radiance per flash”?

It is the highest amplitude count (former stated as radiance) observed for an event of a flash. The term maximum event amplitude count per flash is more precise here. **[please see changes in e.g. lines 633-634]**

30. Line 558: suggest changing “dark” to “darker”, since they are not really dark (they can be seen).

Ok.
[please see changes in line 636]

31. Line 583: A reference for the 10 kA value would be helpful

The value was inititally chosen to discuss the observed differences in the results. For example, Cummins and Murphy (2009) also use values of 10 kA and 20 kA to distinguish "small events" and "larger events", respectively, and to identify CG strokes and IC pulses.
[please see changes in lines 675-681]

32. 591-592: This seems to be an over-statement. The relationship between current, polarity, and height will vary with storm polarity and type, and falls apart for hybrid (IC+CG) flashes.

The possibility of a different relationship in inverted polarity storms (and in other regions) is added to the discussion in the paper. The relationship of the polarity of the maximum current and the flash maximum altitude might also be distorted for hybrid

flashes. The fact is added to the discussion in paper. We do not have data to prove this idea.
[please see changes in lines 692-695]

33. Conclusions section: Present tense should probably not be used, since the findings may not be universally applicable.

The use of present tense in the conclusions is revised and changed for study specific findings.
please see changes in section 4, page 23

34. Line 614: it would improve clarity if you replaced “to an overall equal proportion” with “when considering the complete dataset”

The author does not totally agree with the suggested wording. The study presents two distributions (for ISS-LIS and for Meteorage). The sentence provides the information that both distributions feature a similar ratio of negative values (indicating the match was detected prior to the source). The term “to an overall equal proportion” is replaced with "with similar probability".
[please see changes in line 716]

35. Line 621: Zhang et al. (2019) might be a good reference to add here

Ok.
[please see changes in line 723]

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36. Line 626: suggest replacing “dark” with “darker”

Ok.
[please see changes in line 728]

37. Lines 626:640: It might be useful to add that all of these height-related behaviors are likely driven by the range of heights associated with CG flashes vs. IC flashes.

A short sentence is added mentioning the flash types.
[please see changes in line 729-730]

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)