

Authors' Response to the comments on the manuscript by RC2:
Graupel and increased turbulence observed near small-scale
intermittent lightning discharges at the top of intense thunderstorms

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#. Reviewer's comment. AR: Authors response. CH: Changes to manuscript with new text and **removed text**

Note: Line numbers correspond to the document created with latexdiff, not the manuscript from first submission.

We thank the reviewer for their constructive comments and suggestions that will benefit the quality of the manuscript. The manuscript has undergone thorough revisions. Responses to the comments are listed below.

- 1. Section 2.2 describes the sparkle classification algorithm, which seems reasonable, and per the LOFAR images in the manuscript and appendix seems to have been conservative in not classifying every sparse VHF source as a sparkle. However, to better understand what LOFAR is detecting, I wanted to ask about the nature of these sparse sources, both sparkle and non-sparkle. The clarifications fall into three categories.**

(1) radial location errors along an azimuth (in LMA data, these usually point back to the network centroid and are more common at longer ranges): green sources in Fig. C1(c) and C2(c). These seem strange since the other nearby flashes don't show this effect.

(2) potentially mislocated sources, such as the two sources west of the main orange cluster in the center-right of Fig. C2c; perhaps some of the sparkles and other single sources are similarly mislocated sources throughout the dataset, and in reality occurred along one of the better-imaged main channels?

(3) In Fig. C2, a semi-circular area of sparkles and a few other sparse sources, mostly in the absence of other extensive lightning, between 6.7 and 6.8 east and 53.2. and 53.3 north. Is this indicative of an electrified updraft region where otherwise large discharges were unluckily not detected in the short (<2 s) LOFAR imaging window?

Could the authors confirm my interpretation or speculate as to other causes for these sparse sources?

AR: Indeed, there are imaging errors present in the LOFAR data. This is the reason that we have been conservative to classify isolated VHF sources as sparkles. Selecting

only small clusters (at least two nearby VHF sources), eliminates the majority of imaging errors. Also note that sparkles get precedence over the other VHF sources when selecting radar data (within 2 km distance of VHF sources).

All of your hypotheses could be correct, but the truth is that we cannot verify this. However, our statistics (Fig. 6-7) show significantly different Z_h and W_{rad} distributions despite possible errors. This supports the conclusion that sparkles are a distinct entity from other lightning structures and that they live in distinct atmospheric conditions.

To respond to your hypotheses:

- 1) Indeed, these green sources could be a radial location errors. Our two-stage method for sparkle selection seems to deal well with this potential artefact. After all, few or none of these possible radial location errors are selected as sparkles.
- 2) Indeed, there may be mislocated sources that belong to main channels. However, it could also be sparse imaging along the main channel. Some part (mostly the positive leader channels) of the main channel may be invisible in the LOFAR data.
We believe our clustering algorithm eliminates most mislocated sources. When the mislocated sources are still near the main channel, they will be caught in the first stage of the algorithm (finding large clusters). When mislocated sources are far from the main channel, they are likely isolated and will be eliminated from the sparkles subset (small clusters).
- 3) We can neither confirm nor reject this hypothesis, but it seems plausible considering the radar images.
We note that the time span of this patch of sparkles (~1000 ms) is more than a magnitude larger than large lightning structures. Therefore, the observation that these sparkles exist in absence of large lightning structures supports our conclusion that sparkles are a separate entity from large lightning structures.

CH: No changes. Already touched upon in line ...: "However, given ... the results."

2. ***Line 62: Another relevant point of comparison is to Bruning et al. (2010), which isolated lightning associated with overshooting surges, identified charge where possible, and placed it in the context of radar data. For example, see the discussion on p. 3747, third paragraph in section 4b. Admittedly, they did not go as far as your other references in quantifying the radar statistics of these overshooting lightning surges. Also, the***

correlation of large upper-level reflectivity values to bursts of small VHF sources is known as far back as Lhermitte + Krehbiel (1979). Ushio et al. (2003) also made some early observations of the overshooting top lightning.

AR: Thank you for pointing out this literature. We have read the studies and incorporated it into our manuscript.

CH: Sect. 1, line 43-53 : **“Observations similar ... tops overshoot.”**

Sect. 1, line 51-57: “Numerous studies ... cloud tops.”

3. Line 45-47: The authors seem critical of the possibility that heterogeneous charging and turbulence in the updraft core (independently of screening layer mixing) might be responsible for the small-scale structure of charge, and they use evidence from Calhoun et al. (2014) to support their point. However, the charge pockets illustrated in Calhoun et al 2014 are fundamentally limited in size by the grid spacing of the model used, which was $dx=1$ km in the horizontal. As Bryan et al. (2003) show, the resolved structures in cloud models are filtered below $6*dx$, i.e., 6 km in Calhoun et al. With $dx=125$ m, Brothers et al. (2018, Fig. 4) show charge structures that are much closer to the scale of the sparkles illustrated herein. That figure also showed that the charge in and near the overshooting top tends to remain smoother than deep in the updraft. Later, in the discussion section, leading up to line 382-3, the alternatives are discussed more completely, and in a way that seems fair to the observations and all of the possibilities, so my recommendation here is to adjust the wording to be more neutral to the various hypotheses.

AR: Thanks for the in-depth insights. The coarse grid of Calhoun et al. (2014) would indeed explain the difference in scales between sparkles and the simulated charge pockets. We have read the work of Brothers et al. with interest and have incorporated their findings into our introduction and discussion. Also, we refrain from drawing conclusions based on only the simulations in Calhoun et al. (2014).

CH: Sect. 1, line 63-64: **“However, the ... of thunderclouds.”**

Sect 1, line 64-66: “Brothers and ... of sparkles.”

Sect 4.3, line 443-448: “The size of ... Brothers et. al (2018).”

3a. Before moving on from this topic: another factor that is less favorable toward a screening layer mechanism is that the depth of the sparkles extends several km below cloud top.

AR: Because of the coarse spatial resolution of the radar and time difference between radar and LOFAR data (somewhat mitigated by advecting radar data), it is hard to say where the cloud tops were located in terms of horizontal, and vertical extent. Therefore, we cannot conclude that the sparkles are actually located well below the cloud top.

Through advection, it is also conceivable that charge pockets that are created by folding of the screening layer, get transported further into the clouds.

We think our abstract and discussion could indeed be more conservative about the hypothesis of the interaction with a charged screening layer. And have made the following changes:

CH: Abstract, line 11-12: "... near sparkles. **Given the ... screening layer.** Near ..."
Sect. 4.3, line 464-465: "However, with ... screening layer."

4. *An overview of the basic meteorological parameters used in severe thunderstorm forecasting would increase the value of this study for those interested in the environments that make lightning in the Netherlands, and would also help in understanding the storm modes observed. A thermodynamic sounding (skew-T) including a hodograph, plus calculation of the 0-1, 0-3, and 0-6 km vertical wind shear and CAPE would be especially helpful; the MetPy or SharpPy packages are among the readily available tools that can do this. A quick examination (from the University of Wyoming sounding archive) of 12 UTC data from Meppen, DE (very near the LOFAR site) showed there was probably at least moderate CAPE (especially if the sounding profile were modified to account for afternoon warming near the surface) and a fair amount of low-level directional wind shear, perhaps consistent with a supercell environment. An ERA5 reanalysis sounding from 18 UTC at the same site could also be useful. The hodograph and shear measures would be useful in diagnosing whether bowing linear or supercellular modes were more favorable.*

AR: In line with your suggestion, we have added a skew-T diagram with hodograph, and some relevant thunderstorm parameters. A description can be found in Sect 3.2.

CH: Section 3.2 line 205-215: "On June ... mesoscale structures."

5. *Line 185 and following, convective system B and C: from the presentation in Fig. 5 (Fig. 3 is too zoomed out for me to say more) these do not look to me like bow echo*

structures, since those tend to be symmetric about their axis of propagation, and often feature counter-rotating bookend vortices. This looks to me more like a complex multicellular setup that develops supercellular characteristics around the time the B and C regions merge with one another. The discussion of Fig. 10 in section 3 suggests there mid-level rotation consistent with a mesocyclone at these two times, further favoring a transient supercellular structure. As such, I disagree with the conclusion (“first known case of a bow echo”) on line 409-410. The semantics might not much matter; as the authors show, it is the presence of a deep, turbulent updraft that matters, and such updrafts are likely in a variety of convective modes.

AR: Thank you for the critical remark. We follow up on your advice and conclude it is a "multicellular structure that develops supercellular characteristics" .

CH: Sect 3.2, line 257-258: “Reflectivity images ... multicellular systems.”

- 6. Second paragraph of section 3.3: I found the discussion of the convex shapes in the graphs hard to understand. I think the authors’ conclusion is that sparkles have a systematically larger value of reflectivity and spectrum width, and that this is true at all values of moderate to high reflectivity. A more direct way to confirm this finding would be with a formal test to the shift of the maxima of the 2D histograms, which could be shown by a simple 2D Kolmogorov-Smirnov test. A 1D test could also be conducted within each normalized bin.**

AR: Thank you for pointing out the unclear storyline. We have rewritten the paragraph and backed out claims with a KS test statistic and Cliff's Delta effect size.

CH: Sect 3.3, line 289-300: **“Figure 7 ... are much lower.”**

Sect. 3.4, line 289-300: “Figure 8 ... in Appendix C.”

- 7. 20: suggest, “of point sources in the VHF radio band”**

AR: Agreed.

CH: Sect. 1, line 22: “of point sources in the VHF (very high frequency) radio band”

- 1. Fig. 1, annotation 1, and text near line 30: Is there a negative leader corresponding to the positive leader segment? I would expect a bidirectional tree.**

AR: After thorough inspection and some discussion, we actually do not think that the lighting structure near annotation 1 is on a positive leader channel. It is more likely to be a high-altitude negative leader (Scholten et al., 2021), followed by a dart leader. We apologize for the mistake and thank you for your sharp remark.

In general, positive leader growth is not or barely visible in LOFAR images. Although, in some cases, needles or other lighting activity are visible on previously established plasma channels. In this case, we do not expect to see an oppositely charged propagation. The charge could be conducted along to the plasma channel to a location possibly 10s or kilometers away.

We do note that in this LOFAR image, there is barely any VHF activity between 7 to 9 km altitude. There are many negative leaders propagating downward, starting from ~7 km altitude, and negative leaders propagating upward from ~9 km altitude. As such, we believe that this must have been a patch of negative charge, with unimaged positive leaders.

CH: Sect. 1, line 35-37: “In Fig. 1, ... 350 ms.”

Sect. 1 line 29-30: “Annotation 1 ... relatively slow.”

Sect. 1, line 35-40: “In Fig. 1 ... charged layer.”

8. Section 2.4 The radar processing methods are well-described in this and previous sections and take the required care with the processing of the polarimetric variables, storm advection, etc. However, the vertical resolution limitations described in appendix B should be highlighted in this section. There are clearly unresolved vertical gradients that might bias the radar statistics at the lightning locations.

AR: We agree that this could be more prominently stated in the methods. On the other hand, we do not expect much bias from the coarse radar resolution. This will be mitigated by the 2 km radius to select radar data.

CH: Sect. 2.4, line 171-175: “We acknowledge ... different outcomes.”

9. Section 2.4 and related radar figures: In addition to altitude, the temperature profile of the atmosphere is also a key parameter. It would be useful to know the altitudes of the 0°C and -40°C isotherms, which constrain the altitudes of active primary noninductive charge separation, and to indicate these on vertical cross section figures where appropriate.

AR: This is indeed interesting information. We have added the 0°C and -40°C temperature lines in the cross section of Fig. 4. Furthermore, these temperature thresholds can be read from the skewT diagrams in Fig. ...

CH: Fig. 5: Added isotherms

Fig. 5 caption: "0° and ... lines respectively."

10. 221: "*convergence (upward motion on the ground)*" would be more precisely stated as "*near-surface convergence forcing upward motion*".

AR: Agreed.

CH: Sect. 3.2.1, line 277 : "... by **line of convergence (upward motion on the ground)**"
Sect. 3.2.1, line 277-278 : "... by near-surface convergence forcing upward motion."