

Answers to reviewer 1:

General Comments This manuscript describes the EUCLID Lightning Location System and the results of efforts over the last seven years to evaluating its performance in terms of Location Accuracy (LA), Detection Efficiency (DE), and peak current estimation. The authors indicate that the results presented in this paper can be used to estimate the performance of the EUCLID network for regions with similar sensor baselines and sensor technology. This manuscript is used to provide important background material for a companion paper addressing climatological observations derived from long-term EUCLID data.

The manuscript is clearly written and logically organized. The content is technically sound, reflecting best practices in this area of study. This reviewer has a small number of specific comments (concerns and recommendations) that need to be addressed by the authors, as well as larger number of minor editorial comments and technical corrections.

At this point in the (interactive) review process, I have chosen to post four comments that might impact the scope of the final paper. Lesser issues and suggestions will follow over the next few weeks. If requested by the editor, all comments can be provided in the next few days.

Specific Comments 1. The manuscript includes a map of the expected location accuracy (Figure 5), and this is compared to the independent validation studies reported in this paper. It would be very beneficial if a map of flash detection efficiency were also included. The authors have likely thought about this issue, and decided that this was not practical, possibly leading to the nature of the last sentence in the abstract. This issue deserves discussion.

In fact this is an important issue to be discussed. Or focus in this paper is to show the performance analysis we did based on ground truth data. We decided to include the spatial LA based on confidence ellipses and compare the ground truth measurements because

- The LA analysis based on confidence ellipses is well accepted – see references of Nag et al. and Diendorfer et al. in paper
- It gives the reader an idea about the spatial variation of the LA

Regarding flash DE there exists no way to determine from the network data a spatial variation of the flash DE without using a so called DE model. Because of our focus on ground truth data we decided not to include this type of DE representation – nevertheless the DE will vary spatially.

Model based DE is still idealized as it does not consider any temporary sensor outage due to communication or hardware problems.

2. This paper might be more valuable if it could be viewed as a “one-stop shopping” for a contemporary review of recent publically-available efforts to validate performance throughout the EUCLID network. The specific changes to the manuscript might be an expansion of the introduction and a table in the discussion where performance characteristics reported by others can be “tabulated” and commented on. A LA map (comment 1 above) might be helpful for these discussions.

It is meant as a one stop shopping because we have included all the recent publications and may have added additional data to them. We are not clear about the intention of this comment.

3. The authors have chosen to include the requirement of proper type classification (CG vs. IC) as part of detection efficiency assessment. This strict definition causes the issue of type classification accuracy to be “hidden” within the reported DE values. The authors should consider separating these effects, or at least providing a contingency table for type classification (when known). Additionally, polarity errors (when known) should be noted. I do note that type classification errors for positive CG discharges are discussed in the current manuscript.

For the video data – CG/IC classification is taken into account and for the Gaisberg we do not take it into account – therefore we should mention and discuss this in the paper. We do not take it into account for the Gaisberg validation because it is well known that the measured field widths measured at towers are smaller. These field widths are used for classification and therefore the results would be biased also. This is a topic of ongoing research and should be mentioned in the paper.

We have never seen a polarity error with our independent measurements. We will mention that also in the final document.

4. The authors bring up the issue of flash multiplicity and its impact on flash DE, in the context of qualifying the relevance of flash DE derived from upward lightning observations. The data are available in their video observations to report video-derived multiplicity, which would strengthen or at least refine this discussion. Regional variations in “true” negative multiplicity may well be important in the interpretation of flash DE values. This is a suggestion and not a requirement.

Interesting point – we agree that regional multiplicity variations will impact the flash DE. On the other hand we observe high variability of multiplicity on a daily base in the same region.