Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-307-RC1, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "An algorithm for estimating the detection efficiency of a lightning location system" *by* Haibo Hu and Xiya Zhang

Anonymous Referee #1

Received and published: 28 November 2017

The paper discusses detection efficiency of a lightning detection network in the Bejing area. It presents many interesting observations so that a publication can be envisaged. However, there are many shortcomings that must be remedied before publication. First, many procedures and evaluations are not described appropriately and remain diffuse. Second, the authors ignore the quality of now-a-days networks and remain oriented along very old networks. Thus, substantial revision is necessary.

In the following a list of obvious detail-problems is given.

p. 1, line 7 please define the detection efficiency DE; there are several possibilities to do so.

p. 1, line 14 the term "single-station acceptance" is not defined. Please clarify.

Printer-friendly version

Discussion paper

p. 1, line 15/16 it is not generally known what the authors understand by multi-detector location modes

p. 1, line 17/27 the term “lower” does not mean much; please quantify. Also, it is normal that DE varies in a network, especially versus and beyond the border lines; it should be said that the intrinsic network DE as regards sensors and baselines is not responsible for the observed variations of DE – if this is true.

p. 2, line 22 it is unclear what the authors mean by “performance”. Please specify.

p. 2, line 23 it is known that ground conductivity affects signal attenuation, but this is not a very dramatic effect, in any case not comparable with sensor and network features dominated e.g. by noise and thresholds. The authors should give the km-extension of the considered network area. It appears that all conclusions come from within some 600 km, where other networks operating elsewhere did not find large effects of the claimed type.

p. 3, line 10-20, 13ff this paragraph is not understandable. It is not clear what network or lightning parameters enter into the quoted statistical distribution and how this relates to the (undefined) single-station acceptance.

p. 3, line 21-30 this paragraph can be deleted or should be drastically updated because the given information is outdated and does not account for modern high-precision networks. In particular, it does not take into account the large difference of DF and TOA in terms of location accuracy (LA).

p. 4, line 5 the wording “TOA network appears more sensitive to land surface damping” is misleading. Signal damping has nothing to do with the kind of signal evaluation at a later time (DF, TOA, or both). There are many highly accurate networks that do not take into account damping and still obtain quite homogeneous DE and high LA.

p. 4, line 18 please specify the improvement

p. 4, line 24/25 please specify “optimal” and “upgrading”. What are the deficiencies

and what measures are taken to reach what kind of improvement?

p. 5, line 11 please specify “non-duplicate region”

p. 5, line 19/20 it seems quite strange to quote “0.08 kA to 995.9 kA and 0.258 kA to 992.6 kA” ! First, stroke currents as low as given hardly exist, and second, the high numbers are outside any verified range, and suggest a totally meaningless accuracy.

p. 5, line 22 what is the meaning of 90% DE? How is the definition for 100% ? In view of the reported extremely small CG currents of 0.08 kA (a CG stroke that small was never ever reported anywhere else) it can be virtually excluded that the quoted %-DE values have high credibility. There seems to be a big mix of definitions, observations, and interpretations. The entire data descriptions needs substantial revision.

p. 5, line 23 what is the reason for the statement “only the flash DE can be 90%” ? This statement is not understandable.

p. 5, line 24 the authors still follow the outdated claim that the first stroke of a flash is the most intensive one. This is not correct, because statistics show that flashes with high multiplicity exhibit the strongest stroke in the 2nd – 4th stroke order.

p. 6, Fig. 1 please define the scale “probability density”. Fig. 1 exhibits the most likely current around 24 kA. This is best proof that the network DE is extremely low. Modern high-quality networks have the maximum far below 10 kA, mostly between 4 and 6 kA. For this reason, all the %-DE values in the present paper are very suspicious.

p. 8, line 12 it may be best to give the explanation for the definition of “single-station acceptance“ in this chapter.

p. 11, line 4 how is acceptance confirmed? Why is an iteration needed?

p. 12, line 6/7 in 10 years 240,804 CG strokes are reported. What was the area? Is the resulting flash density reasonable?

p. 12, line 10 what is a “certain” threshold? Please quantify.

p. 12, Fig. 2 what is plotted? The x-axis (hardly to identify) seems to scale the signal strength (in a.u.?); the y-axis gives a “probability density"; please specify the unit: strokes/a.u. or strokes/kA or what else?

p. 13, line 7-12 other networks are not comparable and not relevant here, please delete

p. 13, line 24 please explain “LCLU”

Fig. 3-5: the km-scale must be larger, it is hard to read in the graph; may be shifted to the caption (?)

p. 16, line 13 as stated above a DE of 78% is not possible when the most likely stroke current is as high as 24 kA

p. 16, line 14-20 comparisons with other networks are not relevant, please delete

Table 2 The trend of the numbers of used sensors is highly suspicious and unreasonable. Why should it be easier to locate with 6 rather than 5 sensors? Please explain the remarkable discontinuity in the number of used sensors (e.g. 37.6% for 6 sensors)

p. 21, line 2 is there any quantitative result about the minimum threshold Smin ?

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-307, 2017.

Printer-friendly version

Discussion paper