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

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General Comments This manuscript describes an algorithm and method for estimating the detection efficiency of a lightning location system for cloud-to-ground return strokes, and applies this method to assess the performance of the Advanced TOA and Direction System (ADTD) LLS in the vicinity of Beijing, China. The authors present their material in a logical way, with generally good organization. However, this reviewer has concerns about several aspects of the approach, and about the value of constraining the lightning signal strength distribution using a parametric probability distribution. The comments below reflect the key concerns that call into question the methods and results presented in this work. Other lesser but important comments will be provided if the authors are able to address these first issues.

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[Discussion paper]

To this reviewer, this work is primarily a re-statement of the earlier work by Schütte, using a more-general parametric probability distribution. It suffers from the same limitations as that earlier work, as expressed in the first two numbered comments in this review. I do not see new and/or novel concepts, ideas, tools, or methods. I would be happy to be convinced by the authors to change my opinion.

Specific Comments

1. My most fundamental concern about this work is what appears to be the use of a single "acceptance" function A(r) that is independent of the peak current of the underlying lightning discharge. This is not a problem for single-sensors flash counters, but it is a problem when evaluating detection that requires simultaneous measurements from more than one sensor. Consider a 2-sensor network where the sensor thresholds are the same. Then consider a lightning location that is close to one sensor and far from the other one. The close sensor will not report high-current discharges because their signal exceeds Smax (saturation), and the distant sensor will not report low-current discharges that fall below the minimum detection level (Smin). Under such a condition, one sensor would report only low current discharges not reported by the other sensor, but the A(r) values associated with that location, as defined in this work, will be non-zero for both sensors. This will result in a non-zero detection efficiency value for the two-sensors network. In the worst case, each sensor could have a DE of 0.50 at some distance r, with no overlap in the reported peak current. In this case, the model-computed DE for that location would be 0.25, but the actual DE would be zero. Some modern DE models avoid this problem by defining their equivalent to the A(r) values for narrow ranges of peak currents, and them summing their contributions after weighting by the probability of occurrence of discharges in each narrow range or currents (such as Naccarato and Pinto, cited in this work). Another approach is to partition the DE Model into three elements (peak current distribution, radio propagation/losses, and sensor threshold functions) and then computing DE from these physical characteristics, as described in an appendix in Pessi et al., 2009 and refined in an appendix to

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Nag. et al., 2015).

2. In the first key point in the Conclusions of this work, the authors state that "It is critical to identify a suitable probability distribution if the LISSs", and they go on to discuss various parametric distributions. I question both the need for, and value of, employing a parametric distribution (in this case, the generalized extreme value (GEV) function) to describe the distribution of "lightning impulse signal strengths" (LISS) values. Since the underlying LISS distribution is derived through measurement using a LLS, presumably in a region with very high DE, the necessary cumulative distribution is simply the integral of the measured distribution. I do see that the authors have employed the GEV formulation to produce formulas for A(r) and an effective radius, but neither of these are required to compute detection efficiency. I ask that the authors acknowledge that a parametric distribution is not ESSENTIAL for DE calculations, and then state the value that they fell comes from using one.

3. Equation 7 seems to have a problem, at least in terms of the range of valid values for the three expressions. It might just be that the meaning of the variable "c" is not correct, or that I simply do not understand it. The text refers to "c" as the "signal bias" (which needs to be defined), but then it says that it can be replaced by the "minimum values of the signal samples" (which is confusing, since there are two "signals" – the LISS signal, and the propagated signal received by the sensors). I would expect A(r) to have distinct behavior over 5 separate ranges of range r, as depicted in Schütte (JAOT, September 1987), Figure 1:

a. Small r (< 10 km), where signals from even the lowest peak current (LISS) will saturate the sensors so A(r) = 0;

b. Somewhat larger values of r ($\sim$10-50 km), where signals from the lower currents do not saturate the sensor, but where the large ones do;

c. Intermediate values of r (50-200 km for lamda = 0.3) where the signals from all currents are above Smin and below Smax. In this case, the acceptance is 1.0 (100%)

d. Large values of r (200-500 km for lamda = 0.3) were signals from progressively larger currents fall below the lower (Smin) threshold as the range r gets larger; and

e. Very large values of r (>500 km for lamda > 0.3) where signals from all currents fall below the lower threshold, and A(r) = 0.

It might be very helpful for the reader if the paper included a representative plot of A(r).

4. The authors' handling of Damping in section 2.4 and 3.2 is not very clear, and it may have technical errors. I am left with the following questions and problems:

a. What is the source reference (paper) for the damping function and parameters, and why were they selected?

b. Section 3.2 implies that more than 1 conductivity value was used, but equation 15 indicates a single value

c. If different conductivity values were used in different regions, how was this applied to the propagation equation? Is there a theoretical basis for how this was done?

d. There seems to be two different meanings for "sigma" in Equation 17 – one of for the ground conductivity, and one is a GEV parameter. This is a real problem.

e. In Section 3.2, the authors refer to values with units of Ohm-meters as conductivity, but they are actually resistivity.

5. Equation 19 for F(3,2) does not seem to be correct. The first term seems to be associated with all three sensors "accepting" the event. Also, the definition of A(r2) in the text that follows this equation does not make sensor – how is it the acceptance of detector r3 given the value of A(r2) when A(r2) is already used on equation 18 for a 2-sensor network?

NOTE: I will attempt to send the three new references mentioned in this review. Thus far, I was only able to attach one of the three references to this online review

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-307/nhess-2017-307-RC2-supplement.zip

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