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Comment on nhess-2022-255

Anonymous Referee #1

Referee comment on "Characteristics of cloud-to-ground lightning (CG) and differences between +CG and –CG strokes in China regarding the China National Lightning Detection Network" by Ruijiao Jiang et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-255-RC1>, 2023

Review of the paper "Comparison of two nationwide lightning location systems and

characteristics of cloud-to-ground lightning in China" by Ruijiao Jianga and co-authors.

The paper analyses lightning data from two Lightning Location Systems (LLS) in China recorded during one year (2020), in a first part, and the characteristics of the CG lightning flashes by using data from one LLS recorded during 6 years (2016–2021), in a second part. The link between the two parts is not obvious since the choice of the LLS for the second part was apparently due to the dataset availability. There is no clear conclusion in the first part which could guide the choice for a set of data from an LLS to achieve the second part of the study. This first part, if it is included, must be more justified and shown as more relevant. The second part is consistent enough to make interesting the study, but many ambiguities do not allow to understand and follow the thread of the analysis to make an evaluation. The main ambiguities are related to the difference between flash and strokes which is not clearly announced, to what is provided in the datasets (flashes or strokes), to the use of two terms for the same parameter (or not, we cannot know) rate and proportion of +CG, to the current and the discharge intensity

The paper must be deeply revised to clarify many questions of terminology and several missing information and justification.

I list the main comments in the following:

- The authors talk about detection efficiency (DE) in the abstract at line 22 (with values of 24.5% and 50.5%) but they do not indicate for which lightning entity it is applied: flashes or strokes? It is important to know at many steps in the study what is considered. Indeed, the DE is larger for flashes than for strokes, at least for -CG flashes. Very often in the paper, I was confronted to this problem (another example at line 26 in the abstract, another at line 103). A complete review (text, figures) is necessary for the authors to clear up any ambiguity. For the strategy of the study, it seems the DE for ATDT is only 24,5 %, it is very low for an LLS and for CG lightning (especially if it is applied to flashes). I think this point is not well commented for the following choice on LLS data and of course it should be interesting to well know if it was values for flashes or strokes, furthermore to make comparison by discussion with other LLS systems in the world.

At line 132, I read "Time of occurrence, latitude, longitude, current peak value, number of located stations, (type of lightning) for each flash was obtained." Does it mean the strokes were not available? In figure 3 caption, it is indicated "strokes". Clarify the whole paper with that.

- The problems of terminology can be grouped in a same comment, many times I noted fluctuant terms for an apparently same parameter:

(i) for the +CG two words are used, ratio and proportion. The first example is at line 34 where "ratio" is used and a value is given in %. We can logically understand it is the ratio between +CG and -CG (but strokes or flashes we do not know). But, in the paper "proportion" is also used (first at line 372) and logically the proportion is calculated by +CG / CG and not +CG / -CG. It is necessary to use the same word (and the same parameter) everywhere to understand. They have to make a clear choice.

(ii) the second word to be corrected (and clarified) is for the current. The parameter provided by the LLS is the "peak current" for the strokes identified. Thus, the authors could use this word "peak current" (and to say for which stroke it is used). In a flash there are often several strokes and therefore several peak current values. Which one is used when the authors consider the flashes in the figures. Example at line 35: "The discharge intensity of +CG and -CG on the Tibetan Plateau is approximate, while the +CG always

has a larger current than -CG on the plains” two words for the peak current and CG?
which stroke is considered in the flash?

- About the first part of the paper, the comparison of two systems in China. It can be interesting for the community if general rules are pointed out. The first remark which challenged me is at line 95: “In reviewing the literature, comparative evaluation of these two networks is lacking and mainly aimed at localized areas.” For who it is lacking? I am not sure these two specific networks (distribution and location of sensors, type of sensors, treatment of data) allow to generalize some results, and do the author reach information with a certain degree of universality in the study?
- At line there is a sentence about the selection of data: “As 3D-LLS only retained lightning detected by five or more stations simultaneously this year, accordingly, this study did the same for ADTD data” Is it relevant? It means some flashes can be eliminated on one sensor and not on the other because the distribution of sensors is different for a system and for the other? I do not understand this criterion, it seems not relevant at all. Furthermore, is it applied for the second part of the paper when the characteristics of the CG lightning activity is analyzed for 2016-2020?
- For the different maps represented in the figures, an information of distance scale could be given, longitude and latitude on the edges?
- At line 173: “The DE difference between the two systems can be up to a hundred times.” It is a huge difference! It would mean (for example) one is 5% and the other 50%! Is it significant? Is it calculated within an area large enough? For one pixel it is not significant. Does it mean the area is not covered? For which LLS is it a hundred larger?
- Figure 2 is the distribution of flashes versus peak current values. On the vertical axis it is ambiguous to note ratio. It is a proportion. There is no flash at low values of peak current (for both polarities) for ATDT. Is it already filtered and not for the 3D-LLS?

At line 184 why to say the distribution is the same for both systems? The values are low but in proportion the difference seems large above 59 kA?

At line 186, “of outliers in the 0-30 kA range” why between 0 and +30 kA and not between 0 and -20 kA?

At line 190-191: “direction is much larger than that in the horizontal direction, so a

significant number of misjudgment cases appeared” At which value of height a detected source is considered to belong to a CG flash?

- Section 2.3. The references of studies given at lines 199-204 seem to use very different criterion values, probably because they do not consider the same matching, maybe strokes for some and flashes for other? It depends which systems are compared. This information is not discussed. The authors choose 1 s for the time criterion, it can be justified for flashes but it is not indicated.

It is not possible to understand the comment at lines 210 and 212 with the figure 3a. A proportion (clarify ratio in the figure) < 0.012 is not large. But the figure 3a displays the interval of time 0-50 μs , where are the other values? This distribution is difficult to analyze! Make another distribution and express the proportion in % it is easier to understand. Now in Figure 3 caption, I see “strokes”! Ratio is not clear in Fig. 3a,b

Finally, at line 227, we understand that the strokes are considered for matching. In these conditions, the Δt (delta t) cannot be as large as it is considered (0-1 s, line 210). If Fig. 3c include the common strokes from both systems, a time difference close to 1 s cannot be considered, such time intervals are not consistent with common strokes (not physically consistent). It can explain many dots largely out of the main cloud that follows a line.

- For the lines 245-248, it seems the negative CG flashes are also filtered according to the figure 2: no CG (flash or stroke) between -10 and 0 kA. It is not clear. Again, a lot of clarifications are necessary. I do not see the utility of Figure 6; a ranking of the provinces is not scientific informative. The density distribution in Fig. 5 is much more informative.
- Lines 374-375 and figure 7: the authors use ratio and proportion for +CG, it is not the same, ratio can be +CG/-CG or +CG/CG and proportion is +CG/CG. Clarify and use proportion (I think) in all figures and text. The comment on “peak currents” at lines 382-383 is not relevant. The peak current values have to be explicit: average, median, others? And for -CG it also varies during the months between January and December.
- For the hour-by-hour frequency and intensity variations, the time is not clear: define time CST. Normally CST is central time in US/Canada. The problem for China is the size, how many time zones and how to consider the same solar time or same conditions in solar influence for the figure 8? Figure 8: CST not defined, the curve is not defined. For panel a, the ratio values could be better clear with an interval between 0 and 0.16 to well show the variation amplitude.

At line 403, I do not see the same value for “lowest at 15:00, only 2%,”: according to figure 8: the value seems to be between 0.09 and 0.1 (between 9 and 10 %). Again ratio

is not clear.

- Section 3.2.2 is concerned by the ambiguity between ratio and proportion (line 411 "ratio" , line 416 "proportion"). What is plotted in Figure 9?

Lines 453-456: the sentence is not relevant. What is the idea there? LIS on TRMM could not estimate the discharge intensity (if you consider that as the peak current). LIS is an imager and the light from the flash that reaches the sensor is scattered by the cloud, the magnitude (optical) depends also on the location of the flash within the cloud. Anyway, LIS data does not provide discharge intensity, at least directly.

- Conclusion. At line 476, it is indicated: "it is found that their detection time difference for the same return stroke is no more than 10 μ s", is it consistent with Figure 3a? When you look at the values of the vertical axis, you do not see that.

At line 481: "the ratio of +CG flashes up to 21.4%, much higher than the ratio (8.5%) of ADTD". The values are averaged about the whole area and the whole year 2020 probably. Is the value 8.5 % consistent with Figures 7 and 8? In Figures 7 and 8, the minimum value of the ratio is between 0.09 and 0.1 (closer to 0.1). Is it the same parameter? What happens to have only 8.5 %? In both cases it is called ratio of CG+.

Minor comments:

- In the title "cloud" instead of "could"

- line 28: Thus,

- line 45: Therefore,

- line 144: "than for CG"

- line 157: To write "Fig. 1(a,b)" I think it is better "Fig. 1a,b"

- line 159: The colored scale (typical colors) is displayed in the figure, not useful to write that. Better to comment with maximum values reached in the figure.

- line 173: Is the sentence correctly written?

- line 179: "The peak current values of CG flashes detected by the two networks are compared (removing outliers above ± 300 kA),"

- line 181: rewrite the sentence with a value. Most is vague and rephrase with lightning flash and peak current.

- line 250: "shows" It is present, the figure is in the paper.

- line 251: "Earth"

- line 270: What is the resolution for the density calculation?

- line 295: The shape of trumpet is not useful,

- line 345: It is a little strange to announce that now, the -CG and +CG have been already discussed before in the paper.

- line 350: reference is not correct.

- line 447: check the reference, the name must be written, not the first name for Qie I suppose.

- line 450: What does "that excites the positive charge region" mean? Why not the reverse? Which charge region excites the other? It is not really like that in the cloud physics. The maximum electric field region is generally between both regions (when they

are extended).