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Interactive comment on "Lightning characteristics observed by a VLF/LF lightning detection network (LINET) in Brazil, Australia, Africa and Germany" *by* H. Höller et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper presents an overview of lightning observations collected with the same lightning detection network in four different regions in the world (Germany, Brazil, Africa and Australia) during different field campaigns. Such documentation, performed with the same lightning instrument, is unique and should definitively be analyzed. I should point out that such work requires dedicated times and significant efforts during both the campaign phase in the field and during the analysis phase. The Authors, especially the first Author, should be acknowledged.

The paper is well organized and contains significant new material. It also opens doors for future studies and I believe that the lightning data presented here should be made available to the entire Atmospheric Science community.

AUTHOR COMMENT:

We whish to thank the referee for the general rating of the present work which meets exactly the point that such a summary of measurements obtained from different campaigns requires a big effort in field and analysis work and finally leads to a rather comprehensive paper. We are also grateful for the many useful hints making the presentation more clear for the reader.

The LINET lightning data are available from the different project data bases like TROCCINOX, AMMA or TWP-ICE subject to respective data protocols. Thus they are available also for the entire community.

I have one major concern: the Authors are using the ratio of the number of IC (Intra-Cloud) LINET events over the total of IC + CG (Cloud-to-Ground) LINET events. This ratio is, I believe, misleading because it does not represent the common IC:(IC+CG) ratio usually used in the literature. LINET observations, I believe, should be combined in flash components and flash components in flashes, in a similar way that VHF lightning mapper records are analyzed or that NLDN-like records are processed to form flashes. I would not ask the Authors to develop such algorithms but they should mention in the paper that i) the LINET-based IC:(IC+CG) ratio is different to the one discussed in the literature and ii) that it is envisaged to combine the LINET events in flashes in the future.

AUTHOR COMMENT:

We agree to the reviewer's comment that the usage of the event rates like IC/(IC+CG) should be explained in somewhat more detail at a more prominent place in the paper. Otherwise it could leave too many questions to the reader. We will do this in the revised manuscript.

There are several reasons supporting the presently proposed analysis:

1) One of the main intentions of the present analysis was to present a data comparison from the different regions making use of the same equipment and analysis procedure, thus it is a kind of comparison of system internal parameters. We did not use any other lightning data sources for this comparison. Thus there is no direct need to find a common base for data inter-comparison.

2) Another main goal of the present study is to infer lightning NOx production from the lightning measurements. For this purpose it is more appropriate to use the original stroke information rather than a combination into flashes. So from this point of view there is no direct need for a flash analysis.

3) For gaining more physical insight into the structure of lightning it is of course useful to perform a flash analysis. We have developed algorithms performing this automatically using temporal and spatial differences of the VLF events. Usage of these grouping algorithms still leaves open the choice of threshold parameters determining the flash counts. In agreement with the reviewer, we think that the flash analysis to be performed from the LINET data is such an important task that it requires a separate paper analysis. In this context it is intended to compare with the TRMM-LIS data optical lightning measurements from space.

The section dealing with the NO production is interesting. It provides new results on the participation of the different flash components in the NO production even if some assumptions are used such as extending Wang et al.'s relationship to higher/lower current ranges and assuming that the current measured by the lightning sensor is comparable to the one used in the Wang et al.'s relationship.

SPECIFIC COMMENTS

Page 6063, lines 24-26: in that phrase, you are mentioning the “total daily lightning rates”, are those rates deduced from the LINET event rates or from LINET flash rates?

From the LINET event rates. No flash rates used in this paper

Page 6067, line 19: I suspect that when you mention ‘horizontal’ you are discussing on LINET capability to provide 2D event location. Am I correct? If so, please add ‘(latitude-longitude)’ just after ‘horizontal’.

Yes, we add lat-lon

Page 6068, lines 1 -4: is the term “latter” related to the internal detection threshold? How do you select the proper threshold? Did you perform some preliminary survey on the noise level in the different locations you have deployed DLR-Linet? Did all DLR¬Linet stations be bset up with the same thresholds?

By 'latter' we mean the software settings which determine the internal detection threshold. The threshold was determined based on empirical (measurements performed) and theoretical (dependence of minimum detectable signal on range from a sensor) considerations. The empirical determination is equivalent to accepting some standard noise level introduced by non-lightning sources but still being able to detect peak currents of around 2 kA in the inner network area. We avoid too high thresholds, because the efficiency would go down and the signals would have a distorted shape. The threshold was kept constant for all stations during all experiments thus insuring comparability. The remaining noise sources are rejected from the analysis by the stroke location software. In case a site was too noisy (possibly due to very close power transmission lines or radio stations) it had to be replaced by a more quite one.

Page 6068, lines 4-5: when you mention 'typical' do you mean within the inner network? Please precise your statement.

'Typical network configuration' means 'a central station and 5 remote stations at around 100 km base line'. We will add this. As stated, the 1-2 kA strokes can only be detected in the inner part of the network.

Page 6070, lines 26-27: you are mentioning here the CG/IC ratio. As I wrote above, it is misleading because it does not represent the common CG/IC ratio usually used in the literature. You should state it in your paper. I actually wonder how you would compare LINET data with time-coincident LIS data and deduce for instance a detection efficiency between the two instruments. Would you combine LINET events in flashes and compare LINET flashes to LIS flashes or would you look at finer time scales and compare LINET events to the optical pulses sensed by LIS?

In this specific case CG/IC should read as 'CG-IC' or 'CG and IC' just indicating the source region of data used in the following analysis for both types of strokes. Concerning the problem of the ratios we add a general comment at the end this list.
A comparison of the LINET date with LIS data would be a highly interesting issue and is under investigation at the moment by (some of) the authors. One can compare both, flashes and their building components (strokes from LINET and groups from LIS). Preliminary results show for some examples good temporal correlation of LINET strokes and LIS groups.

Page 6072, lines 7-14: would it be possible to plotthe discrimination area (D) in Figures 5, 11, 18 and 24 as it is done in the other latitude-longitude Figures?

This might possibly be misleading as these Figures just demonstrate the temporal evolution without addressing the IC-CG discrimination.

Page 6072, line 22: does it lead always to an overestimation?

Not in principle. This question is still under investigation.

Page 6074, line 24: what do you mean by 'main lightning region'?

The center of the IC-stroke activity. The region where most IC strokes are located.

Page 6075, lines 1-4: what is % of IC events in Figure 7?

Nearly 90 % of all strokes shown in Fig. 7 are IC events. IC/(CG+IC)=0.88

Page 6075, lines 6-14: I wonder if you have any way to fix that large errors. Is it due to instrumental noise, high noise level at one or more station sites, or are they intrinsic to the signal emitted by the lightning flashes?

As explained in Chapter 2.1 the error is computed from a minimization procedure and thus depends on the geometry given by the horizontal and vertical location of the VLF event and the position of the stations recording that event. In an unfavorable case of a linear alignment of a larger number of stations with the lightning stroke increased errors do arise even in the horizontal plane. This is especially a problem in a limited and smaller network like the present one, in case of some of the stations not working, or outside of the network. Like in this latter case, an IC event which is happening well above the surface where the ground stations are located can also be looked upon as happening outside the network plane. Thus large errors may arise especially because the vertical dimension is not covered by any more sensors. We think that the absence of sensors in the vertical causes the vertical errors to be inherently larger than the horizontal one. This situation might be improved by additional sensors operating e.g. from an aircraft.

At present we can only speculate about the details of the extracted height distributions and, thus, we do not want to state too much that might turn out to be erroneous. See also our general comments, which include remarks on LDAR and LIS.

Do you see during any intensification of the convection the vertical distribution of the LINET sources changing in a similar manner as it has been shown with LDAR (eg Ushio et al., JAM, 42, 2003) or LMA observations?

In the context of this paper we did not investigate any individual storm development. As this is a subject of its own we would like this to investigate within another study.

I wonder about your statement on associating high altitude LINET strokes as potential sources of jets or developing channels of bolt-from-the-blue lightning. It is only an assumption for the moment. I do believe comparison with other lightning mapping sensors should provide some preliminary answer. For example comparing concurrent LIS observations with LINET deployed at the different locations you operated it would be a first step. I suggest to study LIS Science dataset (where the entire optical pulses are available). I won't be using LIS flash data because your need to look in detail the optical signal and to identify where LINET locates temporally and spatially the flashes relative to the optical radiation.

The statement about the high altitude strokes points out that it cannot be excluded from the present data that some of the strokes are due to activity above storm top. If so, then the mentioned stroke types as reported in the literature might be a possible explanation for these strokes.
Comparison with LIS data could certainly be of great value for the general data interpretation but it is not obvious that it could help discriminating flashes above from those below cloud top.

Page 6076, lines 9-10: did you apply any specific set up to avoid any human-related noise? Or did you keep the same detection configuration during the different field campaigns?

We tried to keep as close as possible to a similar set up configuration in all regions. Limitations were due to availability of possible sites or power supply at the envisaged locations.
In case of too much human-related noise the station location had to be changed as already explained above. On the other hand, human-related noise cannot affect our results because we determine a stroke location with a minimum of four sensor reports.

Page 6076, lines 23-27: I am wondering how your lightning measurements are representative relatively to long time series. How confident are you that your measurements are climatologically representative? It is not a criticism I am making here.

We know by comparison with the literature studies mentioned that some of the longer term climatologigal aspects are also represented in our data. This is the different phases of the monsoon in N-Australia which are appearing regularly even though subject to changes in their onset dates, the regular shift of the ITCZ in W-Africa and even the pattern of regional lightning occurrence in Brazil that is similar to our statistics despite the short observation period in this case.

Page 6077, lines 16-20: again I having difficulties to gauge your IC-fraction because it does not correspond to what we are used to.

See previous comments and general statement

Page 6078, lines 3-5: I had a hard time to understand your phrase the first time I read it. Could you rephrase it and even split in two parts?

There is a strong oceanic influence on the convection which was not extending that high up as in the build-up and break phases ~~and was oriented preferably in narrow lines.~~

We suggest to cancel the second part of the statement as the orientation of the lightning structures under such oceanic influence is not discussed further in the rest of the study.

Page 6079, lines 4-7: based on which criteria do you determine the intensity of the cells? Do you use LINET event rate, density map of LINET events?

We use LINET event rates

Page 6079, lines 10-11: when you mentioned the terms 'two major events', I guess you are using the LINET event rate to identify these two events. Right? I would mention somewhere in that statement that we are dealing here with multiple storms occurring at the same time.

Yes, we use LINET event rates. We will add the following statement:
Note from Fig. 11 that these bursts are made up by several cells occurring at the same time.

Page 6079, lines 15-16: Where are located that 14-17-km height bursts? In the inner region of LINET?

These events contributing to this bursts are taken from the discrimination area (D) as stated in the caption of Fig. 12.

Page 6079, line 29: I am surprised by this large vertical error of 6 km. Have you any idea about its origin? What is the % of LINET events having such large vertical error for the population of LINET events shown in Figure 13?

For this example we find the mean values of the error to amount to 6 km or roughly 30% of the cloud depth. As explained in Chapter 2.1 the error is computed from a minimization procedure and thus depends on the geometry given by the horizontal and vertical location of the VLF event and the position of the stations recording that event. In an unfavorable case of a linear alignment of a larger number of stations with the lightning stroke increased errors do arise even in the horizontal plane. This is especially a problem in a limited and smaller network like the present one, in case of some of the stations not working, or outside of the network. Like in this latter case, an IC event which is happening well above the surface where the ground stations are located can also be looked upon as happening outside the network plane. Thus large errors may arise especially because the vertical dimension is not covered by any more sensors. We think that the absence of sensors in the vertical causes the vertical errors to be inherently larger than the horizontal one. This situation might be improved by additional sensors operating e.g. from an aircraft.

Page 6080, lines 7-9: I suggest that you develop in the future some algorithms to combine the LINET events in LINET flashes.

This algorithm exists but we do not use it in this work because the number of strokes is more characteristic and gives more details than the number of flashes. One the other hand, a meaningful grouping is a subject of its own and will be addressed in separate paper. As explained already above, in the context of the present paper it is more appropriate to look at the strokes as they are more relevant for LNOx production.

Page 6081, lines 7-8: I suspect that when you mentioned “detection efficiencies” you were thinking about the network having all its stations fully operational. Am I correct?

In the context of the previous paragraph you are right. We refer to similar overall network geometry (all stations operative) and, thus, to similar detection efficiency. Station failures will produce problems in all of the regions. The special problems we were faced in Africa are also mentioned in the previous paragraph.

Page 6082, lines 12-13: what is the impact on your AMMA data collection relative to the other datasets in having added “an additional requirement for getting most reliable classification in the inner station”? Does it mean that the LINET network finally exhibits some differences between the AMMA setup and the other setups?

This means that those cases where the central station did not work were excluded from the analysis. This specially happened with the African network and not with the others.

In Figure 18, in the sector 7N-8.5N & 3E-4E, the lightning flashes seem to be distributed along a line. Is it real or is it due to some noise?

We think that this line-like feature is real as noise normally would cause signals extending closer to a station and originate closer in time than in the specific example.

Page 6083, lines 3-5: same question as for Page 6075, lines 6-14: do you see during any intensification of the convection the vertical distribution of the LINET sources changing in a similar manner as it has been shown with LDAR (e.g. Ushio et al., JAM, 42, 2003) or LMA observations?

Same answer as for page 6075. In the context of this paper we did not investigate any individual storm development. As this is a subject of its own we would like this to investigate within another study.

Page 6084, lines 12-19: I am wondering if the noise levels are the same between South of Germany and other locations where you deployed your LINET network. In other words, can you confirm that the LINET network used in Germany exhibits a similar setup to the ones used in the other regions?

Yes; the detection thresholds were the same as for the tropical networks. On the other hand, the noise is not a decisive point because – as said above – too noisy sites are not used.

Page 6085, lines 10-11: I think that the definitions of the parameters listed in that phrase are described earlier in your manuscript. Am I correct?

Yes.

Page 6085, lines 18-24: I wonder if your preliminary results are consistent with what was found during the EULINOX campaign. I wonder if during the EULINOX there has been some attempts to document IC/CG ratio.

Yes, during EULINOX there were measurements from the ONERA-IFT and Siemens LPATS data available. But LINET did not yet exist, thus the problem is to compare data from different years with special characteristics of storms observed and from different systems. So at the moment it would be too speculative to make comparisons without carefully considering the details of the classifications used by the different systems and the specific situations like cell types (EULINOX supercell) or so.

Page 6087, line 2: what is the % of LINET events plotted in Figure 25b having errors below 3.5 km?

These make up around 30% of all LINET IC-strokes

Page 6087, lines 17-18: on which assumption(s) do you base your statement “Both systems detect the majority of the VLF sources”?

This statement is not based on assumptions on the emission heights but on inspection of the vertical profiles of radar reflectivity and the VLF sources (see Shao et al., 2006, Fig. 11 and our vertical sections in the present paper Figs. 7b, 13b, 20b and 25b)

Page 6087, lines 19-23: in Figure 26, the vertical density of LINET events decreases as function of time. Is it due to the properties of the storms or is it due to better accuracy in locating the VLF signal along the vertical, the storms after 1500 UTC being located over the network?

The decrease in height and intensity of the IC activity reflects the decaying of the squall line activity in the evening, thus it is due to storm properties and not to network effects. You may also see that from the fact that a large part of the line is still within the network during the dissipating stage.

Page 6088, lines 18-19: the notion of flash is more relevant here. I am having some concerns on what really represents the daily LINET strokes. We are coming back here to my major concern on the use of LINET strokes and not LINET flashes. You should remind the Reader that the daily stroke rate does not necessarily represent the daily flash rate. For example CG flashes with multiple ground connections would be counted many times.

The charge transfer in a thunderstorm is accomplished by many processes like leaders, strokes or continuous currents. By LINET we measure the VLF emissions of impulsive stroke processes happening within the clouds or making connection to the ground. Thus the number and strength of these VLF events is some kind of measure of the electric power of a thunderstorm what is ultimately what we want to know and to compare for the different regions. This information gets lost when grouping these strokes into flashes and counting these flash numbers exclusively. In this view the stroke multiplicity of a flash is more a kind of desirable parameter for describing how often and how strong there were electrical currents happening. It makes sense to compare these numbers for areas of the same size in the different regions. (We will make such statement in the paper in order to make it clearer to the reader). Additionally, there are non trivial problems in deciding appropriate criteria on how to define this grouping. Conventionally, in the case of VLF stroke events, one can choose their separation in space and time as clustering criteria. But it is not a priori clear what thresholds should be used here and how to perform this clustering. Unless absolutely necessary, one should avoid dealing with flashes as they are more generalized entities and harder to define. Flash counting makes sense when comparing with other studies or measuring systems which only are able to provide flash information.

Page 6088, lines 22-23: here too, I am having a hard time to really sense what the ratio of strokes from one region to another one can represent because the number of storms is different from one region to the other one, and because the number of strokes per flash can also be different. I think it is misleading.

We think that the basic stroke numbers are a better representation of electrical activity of a region than are grouped numbers like flashes or even thunderstorm number. (See comments above). The higher the degree of grouping the more information gets lost. Assume you would come up with the same number of thunderstorms for all the regions you would come up with the result that there are no regional differences at all. In reality, thunderstorms might behave electrically very different (different number of flashes with different number of strokes). That is why the use of the lowest order of grouping, in our case the strokes instead of the flashes, is preferable. The use of flashes would be misleading.

Page 6089, line 22: please explain what you call –IC and +IC.

The polarity of a vertical IC stroke is defined exactly as the one of ground strokes. A stroke of negative polarity (no matter if CG or IC) results from a discharge between a negative charge center on top of a positive charge center (the vertical mirror image holds for positive strokes). We agree, though, that horizontal IC strokes would not allow a clear assignment of polarity.

Page 6090, lines 6-8: please provide the total number of samples in each category (-CG, +CG; -IC, +IC).

Total number of strokes contributing to the 'core' data set derived from observations within the inner analysis area (I) with 6 operative stations.

Total Stroke Numbers	Australia	Brazil	Benin	S-Germany
IC-	110 768	47 561	125 755	130 937
IC+	151 729	67 949	144 381	104 169
CG-	36 014	12 589	86 340	67 925
CG+	20 283	11 652	68 807	38 294

We will integrate this table into Table 6.

Page 6090, lines 10-16: I want to be sure that I understood correctly the plot in Figure 28, especially the “error” bars. The “error” bar is computed on the daily basis for a given range of current within 1 -kA bin. So for every region, for the 4 categories of LINET events (-CG, +CG, -IC, +IC), you have computed the ratio of each LINET event category on the daily basis. Then you computed a standard deviation of that ratio (for the different regions and types of LINET strokes). Am I correct?

Yes, exactly that was done. The motivation was to assess the variability of the distributions from one observation day to another because this might help to judge how representative special case studies are, e.g. on days where aircraft operations were performed.

By the way did you compute the ratios on the daily basis, ie from 00 UTC to 24 UTC, or from 00 LT to 24 LT, or from roughly the beginning time of the convection to the beginning time of the convection + 24 hours?

From 00 UTC to 24 UTC

You wrote that the mid-latitude statistics exhibits larger variability. It is an interesting result. I initially thought that it was due to statistical sampling issue, but looking at Figures 4, 10, 16 and 23, the number of core cases is the highest in the mid-latitudes. I guess Figure 29 deals with the same population of LINET data

Yes

but I am curious to see the plot in Figure 29 for the four regions separately and for the different IC and CG categories. Could you please add that additional plots in Figure 29 in addition to the plot already given in Figure 29?

This is already given in Fig. 28 with is computed with the same data. Just a linear scaling has been use to better see the rations, whereas Fig 29, using a logarithmic scale, better shows the behavior at large amplitudes.

Pages 6090-6091, lines 26-9: I think you need to remind the Reader that you are dealing here with strokes and NOT flashes.

We will add a statement about this issue somewhere at the beginning so that we do not have to come back to it so often. Again, we think that the usage of strokes instead of flashes is the better and most appropriate choice for describing and comparing the lightning properties in the different regions as it retains some kind of information of the intensity of the convection. Of coarse, you can only do that if you use the same kind of measuring equipment with a comparable sensitivity in all the different regions. If this is not the case and you compare data obtained from different types of measuring equipment having different sensitivity or measuring different properties of lightning then it might be necessary to compare countings of higher order clustering products like flashes.

Page 6091, lines 16-27: I found interesting that the Australian storms exhibit statically higher peak current for –CG strokes. I wonder about your distribution above 50 kA or below -50 kA. What is the highest current value you have recorded for the different regions and for the different LINET event types (-CG, +CG, -IC, +IC)?

Plotting of lower and higher current values results in a rather noisy figure so we did this only in Figs 33 and 34 for the NO-production. For the 'core' data we find the following extreme values:

Max Peak Current (kA)	Australia	Brazil	Benin	S-Germany
IC-	-289	-186	-189	-254
IC+	+112	+174	+161	+292
CG-	-312	-144	-280	-220
CG+	+134	+130	+139	+244

Page 6092, lines 4-1 1: I don't understand your statement "Consequently, high relative peak stroke frequencies imply a narrow vertical profile as can be noted from Fig. 30". Could you please clarify your statement?

Many strokes (high maximum) at the height of the maximum imply fewer strokes in the other levels if the integral is normalized to 100%.

What is the impact on the vertical error on that vertical distribution?

Including larger errors would further broaden the distributions by introducing more data points at larger heights, whereas the lower part of the profile remain basically similar, i.e. the absolute accuracy is better at lower heights.

What is also the impact on the current distribution on that vertical distribution? In other words, are those vertical distributions dependent of the current?

Not really in a clear way.

Page 6092, lines 19-23: I TOTALLY agree with you and it comes back to my main concern about not having combine LINET events in LINET flashes. I agree with you when you mention that some IC LINET events can be part of a CG flash. You need also to mention that IC flashes can also exhibit multiple IC LINET events as shown in Figure 2 in Betz et al. (GRL, 2008).

As already commented before, we will add a discussion of this issue in a more prominent place at the beginning of the paper. It obviously was kept too short and too much scattered within the present version.

Page 6093, lines 5-16: Could you confirm that the % you are computing is relative to the population of LINET events that were retrieved with a vertical location error < 3km and a current > 5 kA? The data you collected and presented encompass different stages of the storm, ie developing, mature and dissipating stages. So I am having some troubles that a single vertical distribution of the charge layers can fit the different stages of the storms. I guess the next paragraph (lines 17-23) should be relocated at the beginning of the current paragraph (lines 5-16). I am also wondering about possible tilted charge regions (as already documented in studies where LMA observations were available).

We will re-arrange this and also point out that tilted charge structures might lead to non-vertical stroke channels which in turn lead to uncertainties in polarity determination. We think that the presented results are only the beginning of an interesting observation to be followed more closely in the framework of a case study.

Page 6093-6094, lines 24-7: I am wondering about to what the LINET VLF sources are associated with. Are those events occurring at the beginning of the flashes as Betz et al. (GRL, 2008) have documented, or are they recorded later during the life of the flashes? I understand quite well that it is not easy answer and more investigations are needed.

Both cases occur in appreciable fractions. Since this is not clearly understood and has not yet been investigated quantitatively, further research is necessary.

Page 6094, lines 16-22: how applicable is Equation (6) to natural lightning flashes?

Wang et al. (1998) tried to keep the laboratory conditions as close as possible to atmospheric conditions under which natural flashes occur. The sparks produced resembled natural discharges in current waveform and amplitude and were investigated under different atmospheric pressure corresponding to heights up to 8 km.

Page 6095, lines 3-12: please indicate that the vertical “error” bars, associated with the cumulative distribution frequencies, are also cumulative, if I am not mistaking.

Yes, this is correct and we will add an explanation.

Page 6096, lines 1-4: I agree with you but for current below -20 kA. No?

We mean 'not too different' and will clarify this in the text.

Page 6096, lines 16-17: I don’t understand your statement.

We cancel this statement

Page 6098, lines 16-17: your statement is a bit misleading I believe without some detailed analysis of LINET data in synergy with VHF lightning mappers. One should really consider making comparison of concurrent observations of LINET and VHF mappers in order to determine to what different successive IC LINET sources correspond. It is not yet proved that successive IC LINET sources could be joined to form physically a lightning component on the opposite of what can be done with VHF interferometric or time-of-arrival VHF measurements. I would suggest to reword your phrase.

We reword. (Please consider the general comment.)
'Even though the complete lightning channel cannot be resolved in detail, the reporting of emission heights adds interesting information about the vertical structure of cloud discharges.'

Page 6098, line 27: I guess you are speaking about current distribution. No?

Yes, we will clarify.

Page 6099, line 3: what about the amplitudes above 100 kA??

In accordance with this broadening, all regions are characterized by a dominance of the negative ground stroke numbers over the positive ones except for large amplitudes larger than around 100 kA in the German case (not shown in detail here).

TECHNICAL CORRECTIONS

Will be included

<u>General Comment on VLF/LF and VHF reporting for flashes and strokes</u>

We comment the difference between different previous observations of “total lightning” and what is possible with the use of LINET.

As regards the LDAR and SAFIR observations, one must recognize that

- flashes can be detected with a probability of ~100%
- the discrimination of IC and CG remains critical (see below)
- it is not well known what is really measured.

There is no doubt that a flash produces enough signals so that LDAR and SAFIR type networks are able to catch enough in order to report activity.

VHF networks measure small discharge steps (mostly negative leader steps) that are abundantly produced during a flash or, more general, during a discharge process. It is not clear to what extent positive leader steps are also detected. Also, there are distinct signals (TOA) and pulse bursts (DF) in unknown ratio that are detected better by TOA and DF, respectively.

When one inspects modern high-speed photographs of flashes one sees a tremendous amount of leader activities (like a firework). Obviously, only a small fraction of this activity is detected and located by VHF. Still, this is no problem when one counts only flashes that last up to ~1.2 s.

The discrimination between IC and CG is highly problematic. The VHF networks cannot discriminate. Thus, they use an additional VLF/LF technique and assume that cloud activity measured by VHF is an IC flash when the VLF/LF network does not report a signal, which is labeled as CG. There are two problems:

1. The VLF/LF systems have limited detection efficiency. We do not know about LDAR, but we know that SAFIR use highly inefficient VLF/LF sensors that do hardly report strokes below ~15 kA (Poland, Hungary) and, thus, miss most of the CG.
2. The VLF/LF system itself must discriminate IC from CG. This is performed by means of the wave-form discrimination method. In the meantime, after some 20 years of experience, one concedes that this method fails for small strokes (below 15 kA), especially for positive strokes.

For these two reasons, the traditional counting of IC and CG is questionable.

Furthermore, it is not clear whether the stepped-leader activity, seen by VHF, must necessarily be connected with a stroke (either IC or CG). Can one rule out that there is such VHF activity without ever having a visible and auditable stroke?

In any case, the traditional method relates phenomena that are different for IC and CG. The IC part is VHF, and the possible CG part is a VLF/LF return stroke.

We agree that the traditional counting of IC-flashes (any activity without CG) is meaningful. Ratios IC vs CG though are more critical (see above).

Our technique differs in that it compares discharge events that are physically comparable, namely strokes that both occur (also) in the VLF/LF range.

We concede that we do not know whether each IC flash (defined by VHF observation) also produces any IC stroke (in the VLF/LF). This may not be the case. Still, the data comparisons between SAFIR and LINET suggest that there are enough IC stroke occurrences in the VLF/LF regime. When this is correct, then the comparison of strokes is certainly a better way to characterize electrical storm activity.

It is not our intent to criticize the traditional procedure, but it must be allowed to describe its functioning in the essential details. Our intention is to present an additional alternative to quantify electrical storm activity by reporting real strokes in both the clouds alone (IC) and down to ground (CG). We think that this is justified, because the multiplicities of both IC and CG strokes do characterize a flash and, thus, are qualified to be counted. In particular, when derived processes are considered such as NOx production, the number of strokes – even within an IC flash - are influential.

As regards VLF/LF comparisons with LIS, the problem is similar as with VHF. LIS is sensitive to optical sources that may or may not be associated with a VLF/LF stroke carrying large currents. The present paper does not focus on LIS observations; therefore we prefer not to deal with this independent issue.