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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 #2

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GENERAL COMMENTS

The authors give a short description of the lightning detection system, however, more information is needed about the nature of the different types of lightning discharges detected by the system. This holds particularly for the characteristics of the detected intracloud lightning data.

AUTHOR COMMENT:

We will add some introductory remarks on what the LINET system detects in terms IC and CG lightning components and why and how the usage of strokes instead of flashes is meaningful for the interpretation and the comparison of the lightning data from the different continents.

SPECIFIC COMMENTS:

The LINET lightning detection system is able to detect both intracloud and cloud to ground lightning. Some additional explanation of the sources of the detected events would be helpful.

1. Interpretation of the IC and CG events.

How the single IC and CG events have to be interpreted? Is there a short time correlation (1sec) of the CG and IC flashes? Do the ground flashes consist of several return strokes? Are the IC events parts of one large cloud flash? If not, what point of the IC flash channel is detected and given as horizontal position? Is there a different interpretation for horizontally extended IC lightning? Are the CG flashes associated with cloud flashes?

If possible, provide numbers or ratios for these quantities, because this is important for the interpretation of the IC numbers and the IC/CG ratio.

The VLF events detected by LINET arise from a discharge process leading to relatively large currents between areas of opposite charge flowing over distances (larger than a few 100 m) larger than those typically involved in the stepwise propagations of leader processes. In case of a ground flash these are the return strokes which may happen between the ground and often negative (but also positive) charge regions within the cloud. These account for negative or positive CG strokes, respectively. In the case of an IC flash the events may arise from Kchanges or recoil streamers. They may be part of a discharge process happening entirely within the cloud or involving a ground connection. Another identified source of VLF emissions in the cloud is the so called narrow bipolar events (NBE) which is identified as a strong, well defined pulse (see Betz et al., 2008). All these different events may happen in combination as part of the same flash. In this context the definition of a flash is somewhat arbitrary and may be done by grouping all VLF events happening within a prescribed time interval and distance from each other (e.g. 1 sec and 10 km). But it is not clear a priori how to fix these parameters and if they apply to all possible thunderstorm situations. This is one reason why we prefer to use the original strokes in the context of the present paper and regard the grouping as an issue to be followed in a separate study. Another reason is the loss of information when simply counting flashes instead of the contributing components which give better information of the intensity of the discharge. Especially when comparing data from the same measuring system for different regions there is no need to look at flashes as long as we do not intend to compare to outputs from other lightning detection sensors providing different properties of lighting (like VHF or optical detection). In this case a flash definition might be helpful as common base for interpretation. Also the NOx production aspect favors the use of strokes instead of flashes as it is difficult to associate flashes with the component peak currents.

There are still many open questions concerning the processes happening during a discharge and what exactly is causing the VLF and other emissions. A next step for clarification will be further comparison with other lightning detection systems which are complementary and see different phases (VHF) or effects (optical) of a flash. Thus for the moment we cannot tell exactly which point along a lightning channel corresponds to the VLF source location in 3D. As the horizontal field components of the magnetic field are measured by LINET we expect that the vertical channel components are responsible for the measured effects. But it has not yet been shown where along the channel these arise from.

2. Amplitude distribution functions

Fig.28 and text:

The amplitude distribution functions differ from the figures usually reported in the literature. Mean values for the peak current amplitude are usually around 23kA, this paper reports much lower values. Moreover, the fraction of positive ground flashes seems to be larger than usually reported. The authors should address this issue and give a short discussion if and how the characteristics of the LINET produce this result? Can the differences between the presented picture and the 'usual' results be explained consistently by the higher sensitivity of the LINET to weaker lightning?

LINET with the chosen sensor configuration is much more efficient than most other networks. This has been referenced in many papers about LINET. Thus much smaller amplitude strokes can be detected as compared to other networks and, consequently, mean amplitude values are smaller. The weak positive CG strokes are thought to represent the involvement of the lower positive charge center in the discharge process.
The positive CG are relatively abundant only for weak amplitudes. Above some 15 kA we do not find differences to other networks; provided these are sufficiently sensitive in the range 10-15 kA. This holds for ALL stroke types (CG-, CG+, IC-, IC+).
We will add some more discussion on that in the manuscript.

3. Polarity of the IC lightning.

What part of the cloud discharge process is detected by LINET (leader or recoil processes)? How does LINET detect the polarity of IC flashes and how is this to be interpreted? VHF systems respond differently to leaders of different polarities. Is this also true for LINET?
Please, explain this statement. Why should the IC polarity depend on charge *configuration* of the cloud? Generally, leader processes of both polarity initiate the discharge process

between the cloud charge regions. The resulting recoil discharge will transport negative charge due to its higher mobility.
What is the base for the derivation of the charge structure from the polarity of the events detected by LINET?

LINET detects strokes, not leaders. LINET is a LF system and, thus, can detect only relatively strong currents that flow through a reasonably long channel (e.g. previously established by leaders). Therefore, LINET detects a totally different part of a discharge as compared to VHF systems. The VLF/LF events seen by LINET is are thought to arise from return strokes or preliminary breakdown processes of ground flashes, from IC strokes due to K-processes (or recoil streamers) occurring during IC-activity of ground flashes as well as for pure cloud flashes. A diagram illustrating the field changes to be observed in the different frequency ranges is shown in Uman (The Lightning Discharge, Dover Publ., 2001).

LINET detects and locates an IC stroke in exactly the same way as it detects and locates CG strokes. In fact, the measured signals are very similar for the two types of strokes. The polarity depends on the charge distribution, i.e. whether negative charge is lowered or raised during the discharge (just as for CG) and not whether the leader charge is positive or negative. Of course, these arguments hold only for vertical cloud discharges; it is assumed that there are dominantly vertical discharges. At this time, we can not derive charge structures very accurately.

LINET determines the stroke polarity from the peaks of the observed wave forms. This and the questions raised before have been discussed in detail in a recent paper by Betz et al. (JOLR, 2007).

An additional hint for the similarity of CG and IC strokes may be derived from the observation that conventional assignments of stroke type, as performed with the so-called wave-shape method, often mix up CG and IC. For example, it is reported that most of the small amplitude positive CG analysed by the NLDN are, in fact, positive IC. Given a suitable sensor geometry, LINET avoids this problem by means of a specifically employed 3D-technique.

The time series of the daily lightning totals are given in the Figures 4, 10, 16, 23, 27. These figures also show 'trend lines' which were calculated as polynomial fits of 3rd order.
What is the purpose of these lines?
Firstly, the terminus 'trend line' may be misleading here. These lines are rather a fit to the overall data set, which is arbitrarily calculated as a 3rd order polynom. I doubt, if an overall fit of this kind should be applied here.
Lightning data are highly intermittent and characterized by the time scales of the producing processes. The daily totals are dominated by the number of storms in the detection area, the intensity and lifetime of the storms. These storm characteristics are determined by the circulation pattern, atmospheric stability, etc. and by the time scales of these underlying processes.
Hence, for certain locations the time series is always determined by few days with highest activity and periods without or weak storm activity. The authors have observed this for Darwin and Germany.
Any 'trend line' will fail to describe such a behavior, since it will be always below the time series for days with expressed storm activity and above the time series for days with no lightning.

The fitting lines have been applied to the data in order to illustrate any intra-seasonal variability thus the purpose was to filter out the high variability introduced by high-activity days.

Additionally, the displayed 'trend lines' show some artifacts on the various figures:

- fig.4, p6071: The text notes a 5-10 day oscillation. This is in contradiction to the fit of the whole time series with one 3rd order polynomial. The trend line does not support the interpretation, particularly for the ALL data it seems to be strongly affected by the few days with maximum lightning numbers.

It was not intended to demonstrate this 5-10 day variability by the fitting line.

- fig16: The 'trend line' for CG and IC after mid of September is based on just 1 day. The trend line for these data should not go beyond September. The strong decline in November is supported by ALL data. The pulsation in the IC and CG time series is not reflected in the trend lines.

- p6085, fig23: The trend lines at the edges behave very mysteriously. The edge data points seem not to support the strong decline of the trend curve.

The 'trend lines' are interpreted and discussed with the necessary caution in the text. However I doubt, if this fitting line is necessary at all, and I recommend to omit it. The discussion of the pulsations and changes in storm activity can be given without the 'trend lines'.

We agree that the use of trend lines defined by polynomials might be too confusing for the reader and are probably not necessary here. Thus we will omit them and discuss these issues in the text.

p6095-1 pp:
The NOx-production per flash is calculated by the formula of Wang et al. This formula was derived as a fit to second order in current amplitude from laboratory measurements on sparks. The authors extrapolate the formula to larger current amplitudes. This can lead to an overestimation of the NOx production at the large amplitudes. The authors should justify the application of the formula (Nox proportional to the squared amplitude) by dimensional arguments or energy considerations.

As stated above by the reviewer the Wang-formula is simply an empirical fit of laboratory data. There is no direct line of argumentation by dimensional arguments or energy considerations that the quadratic dependence holds more generally. As pointed out by Wang et al., it results in a much better representation of their laboratory data than does the commonly used proportionality between specific spark energy and NO production. Moreover, they operated their experiments in a range of peak currents typical for natural lightning. We think that we can extrapolate the quadratic dependence claimed by Wang et al. because our peak current data show quite rapidly decreasing number frequency towards higher amplitudes. Thus especially the NO production values for positive peak currents larger than 30 kA remains small. As the drop-off of the spectra towards larger negative peak currents is much slower, the effect on NO production is more pronounced in this case. We think that, in general, the NO production at high peak currents as observed in atmospheric lightning is still insufficiently known and further research is needed.

p6097-2pp:
The channel length could not be taken into account in the NOx estimation. However, particularly the relation between the CG and the IC contributions may change completely if the lengths are introduced. While for ground strokes a mean length can be anticipated, this is much harder for the IC lightning.
In view of this open question, I doubt, that the relative importance of IC and CG can be estimated.

Because of the difficulties mentioned by the reviewer, we are just dealing with the NOx production per unit length of a stroke in this paper. Then we primarily compare the different regions with each other rather than the different types of strokes. On the other hand, even for CG-flashes the properties of their intra-cloud parts are much less well known as those of the visible ground channel.

We shall add a comment to the Figures comparing the NO production to give the reader a better background on what these values represent (see also next comment)

p6095pp and fig.33-34:
The discussion of the contribution of the NOx as a function of the peak current amplitude is instructive. However the presentation of the integrated curve starting from infinity is inconvenient and unnecessary.
The authors have to keep in mind the bimodal character of the current distribution. A more appropriate procedure would be a running integration for the *absolute value* of the current, starting from the strongest amplitudes of both polarities and extending to the weakest amplitude (or vice versa). This would be also consistent with Wang's formula, which doesn't account for the polarity.
This integration curve poses a lot of difficulties for interpretation. E.g.

- Intermediate values along the curve are to be interpreted as (e.g. at +20kA): NOx for all negative flashes and the lowest (<20kA) positive flashes. What is the value of this information for the reader?

We think that a representation of the NO production in the format chosen is especially intriguing as it summarizes all aspects of the results just in one Figure. With that it is not necessary to make separate Figures for positive or negative peak current contribution or to produce too many curves intersecting in one Figure (admitting that positive and negative strokes have different starting values and different order during the integration). The rate of change (increase) of the accumulated graphs does demonstrate the contribution from the respective categories. This is anyway given in the non-integrated lines included in the Figures but its effect upon the total amount can much easier be demonstrated by the accumulated curves. Moreover, we directly see the contributions from the overall negative and positive stroke population by comparing the values from the accumulated curves at zero and +200 kA peak power. Also the same Figure shows the final result which would not be so easily detectable by performing separate integrations for positive and negative strokes. We should certainly comment more on this in a revised version of the paper.

- How should the end value of the integral curves be interpreted and compared between the 4 regions? As total NOx per storm, per day? Since the basis was the normalized amplitude distribution (percentage of flashes), the NOx distribution is distorted. It would be appropriate to normalize the integral curves to a equal final (right) values. A mathematical formulation of the curves would clarify this.

Unfortunately, there is an error in the labeling of the ordinate units. It is not in % but should be labeled as
For the distribution functions (left)
10^{21} NO molecules m^{-1} kA^{-1}
For the accumulated curves
10^{21} NO molecules m^{-1}

The idea behind these diagrams will be made clearer in the text.
As the present data set does not represent a whole convective season in each of the regions (e.g. the Brazilian data just cover one month) we cannot compare absolute lightning frequencies in this paper. Thus we do not relate the numbers to a 'global' data set of al measurements lumped together. Instead, we can compare the NO yield from a stroke

sample of equal number (say 100 strokes from each region) and unit length for each of the regions. The samples differ in their peak current distribution and in the mean IC height observed for each region. Thus we want to study the effects of different current distributions and different IC and CG contributions on NO production, well knowing that the effects of channel length and regional thunderstorm occurrence etc. remain unresolved here.

As the basis is the normalized amplitude distribution (in %) for each region separately (providing the number of strokes per amplitude interval of 1 kA relative to the total stroke number of the representative sample of the core data) we compute the NO yield per meter and kA interval of 100 strokes by multiplying by the Wang formula. Integrating this over all amplitudes provides the total NO production per meter from 100 strokes for each region, thus some kind of efficiency of the lightning activity for NO production resulting from differences in stroke amplitudes and types (heights). These values from the different regions can be compared directly. Therefore normalization with total NO-production is not aimed at here. This could only demonstrate that CG and IC contributions add up to 100% for each region. The use of an overall 'global' NO production is not helpful here as the absolute flash numbers (or total thunderstorm occurrence) is not known sufficiently well for all regions (see arguments above).

• The error bars are intended to represent the variability in fig.33-34. However they increase monotonically towards large positive amplitudes due to the integration procedure. Again this may mislead the interpretation.
The integrated curves are misleading and hard to understand. I strongly recommend to omit these 'integration' lines. All the interpretation and discussion in the text can be made on the distribution functions alone. For a separation of the contribution from 4 types of lightning the totals over the complete amplitude ranges might be given. In order to characterize the variability the authors might consider to display the frequency distribution function of the daily total NOx production instead of the error bars.

The 'error bars' were introduced as the integration of the upper and lower bounds defined by the corresponding 'error' curves given in the distribution function. Thus they have to increase monotonically. One could also visualize this as a separate line but this would be confusing when adding too many lines to the Figure. Thus we chose to visualize this by the bars even though these are no randomly distributed errors. Maybe further explanation in the text will have to make these points clearer. The advantage of this kind of representation is that you unify in one Figure both, the spectral contributions as well as its total accumulated effect.

Minor Remarks

p6066 :23-24:
Explain please, what do this numbers relate to? Is this the contribution from all storms of this type, or for a certain part of the globe?

The indicated numbers of global LNOx production result from a scaling of airborne NOx measurements with LINET observed strokes - LIS observed flashes - OTD global flash rates. Thus they represent a kind of extrapolation from this type of observed storm to the globe. The numbers would result if all storms on the globe would be of this type.

p6069:21 pp, Fig.3b) and similar figures:
What quantity is displayed, is it a density measure? What is the unit, events per km2 per year?

As indicated in the Figure. Number of strokes > 10 kA per 0.1°x0.1° grid box for the complete observation period indicated in the Fig. caption.

p6073-1 5pp, Fig.6:
Why is the IC-fraction equal 1 during no-lightning periods? You might omit this parameter for these periods or increase the time base for calculation. Can you give an uncertainty measure for the IC ratio? I suppose it will fluctuate largely.

We will omit these data points. Was just set to 1 for missing data values to indicate the limiting line. You may note large fluctuations for low activity values from the figures.

p6074-28pp:
Can you explain the large difference in position error for CG (40m) and IC (340m) lightning in this case?

The horizontal and vertical errors of IC locations are generally somewhat larger than for CG as an extra dimension (height coordinate) is introduced in the analysis as compared to a 2D solution. A more detailed comment of error aspects is given in the answers to reviewer 1.

p6081 -26pp: Fig.1 5(trace), Fig.1 7:
The shift of the daily mean position with latitude is a striking feature and one is tempted to interpret this as the shift of the ITCZ. However, is the network area large enough for doing this conclusion? The 'mean position' might not correspond to a real center of the storms of the much larger ITCZ region.
A similar motion of a mean position might be found for most other locations on Earth. It reflects gradual shifts of storm activity areas influenced by geographical features and circulation pattern.

The shift shown is connected to the ITCZ movement but the lightning activity during July/August extended much further than the observation box. Thus, your conclusion is correct.

Fig.25b:
Typo, x-axis must be probably distance.

Yes

section 3.1, Fig.27:
The figure puts together (for comparison) the daily lightning totals for the 4 regions as a function of the observation days. This type of presentation makes no sense for me. What information should the reader get from this combined plot? The data should not be displayed along the 'time axis' of observation days, which is completely arbitrary and moreover of different length.
More instructive for the reader could be a frequency distribution of the daily totals for the different regions.

This seems to be a good idea. We will try to change this.

Fig.28:
Explain, what quantity was averaged? The percentage curves for each day?

yes

How are the mean values calculated: for the whole data or from the daily percentages?

For the whole data

The choice of the method affects the result mostly for regions with strong variations in storm activity. What is the bin size of the classes?

1 kA

p6099-9pp:
"..The Brazil and Benin lightning was found to behave very similar to each other in terms of total LNOx production which was found nearly a factor of two less efficient." What does this mean? Does a storm in Brazil, Benin produce less NOx? I suppose, the authors want to express that the normalized lightning distribution is a factor 2 less efficient due to the IC/CG ratio and the discharge altitude. For total NOx production the total lightning number has to be multiplied. The authors should clarify this point.

This is correct. The normalized distribution is a factor of 2 less efficient. But not only due to different emission heights but also due to the shape of the peak current distribution. See above comments on the general idea behind the comparison of LNOx production.