Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-105-RC2, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Comparison of lightning activity in the two most active areas of the Congo Basin" *by* Jean K. Kigotsi et al.

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Received and published: 12 September 2017

This paper takes a look at lightning activity in the "Dark Continent" that also happens to be (often) the leading contributor to global lightning. Accordingly, shedding light on the darkness is a valuable endeavor, and eventually this paper deserves to be published. The most important single need is to identify up front the reasons for the analyses selected, and then to make more detailed physical interpretation of what emerges from the analysis. Several additional areas are identified where improvements can be made below. These substantive issues are followed by detailed comments on the text.

Summary: Consider for publication after major revision

Substantive Issues:

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(1) WWLLN documentation

The WWLLN information is the mainstay of this study. Accordingly, more details about WWLLN are needed in the context of the two years selected for study. If differences are documented in selected parameters (Table 1), one would like to know how much of the differenced comes from the detection system and how much is real interannual variability. (That influences the physical interpretation.) So information on the number of receiving stations operating in both years during the period of interest would be helpful. It is widely believed that Africa is generally in third place in the ranking of tropical lightning "chimneys" and that is simply because WWLLN has rather few receiving stations in that part of the world. (In contrast, the other global VLF network, GLD360, is getting Africa much more prominently, but unfortunately Vaisala keeps its information about station numbers and locations secret.) See additional info on this aspect in Williams and Mareev (Atmos. Res., 2009). And toward justifying the scale for gridding of the data, estimates of the accuracy of stroke location are also appropriate. A mention of "continuous increase in detection efficiency" appears in line 77 but without further details. In the Franklin Lecture on "Lightning and Climate" (2012, AGU website), Williams has addressed the problem of the changing detection efficiency in using WWLLN and GLD360 observations as a diagnostic for climate change.

(2) Surface temperature documentation

In other studies, tropical lightning activity has been shown to vary with surface air temperature, also related to CAPE (instability). D. Romps has also shown recently that tropical CAPE may be scaling with the Clausius-Clapeyron relationship, and so there one has a predicted temperature dependence of CAPE. The reviewer has already made inquiry with the second author about this thermodynamic aspect, but the same question is appropriate here. Are surface meteorological observations available at any location in the DRC and in particular for the two areas targeted in this study? That would be a most welcome addition to the physical analysis and interpretation in this paper. The authors need to consider that virtually no additional information is provided

about the surface conditions of the two areas they have selected for study.

(3) Expectations for seasonal variations

The semiannual variation of temperature, rainfall and lightning activity in the climatology of the Congo is well recognized (Christian et al., 2003; Williams and Satori, 2004) but is not mentioned in the interpretation of the Figure 6. In a single year of lightning observations with a detection system that is decidedly inefficient, the semiannual variation may not be so robust, but there are hints of this in Figure 6 already. For example, note the maxima in April in Figure 6b and the local maxima in Figure 6a for October. Also, since the two selected areas are displaced south of the equator, one expects to have an annual phase with maximum in NH winter, also consistent with Figure 6. Place what has been found for localized areas in the broader context of knowledge about Africa.

(4) Positive correlation between lightning areas

My understanding of developments in the Congo is that often convection in the elevated terrain on the eastern boundary develops cold outflows that then go out to the west to stimulate/initiate new convection there. This could be a basis for correlation. Ground conditions are cited, but better would be to cite antecedent rainfall conditions over a large domain that will influence the nature of the convection on subsequent days. I would also strongly recommend another correlation calculation with an area that is immediately adjacent to the primary area, as presently the two selected areas are separate. It would be helpful to show that you have greater correlation when an area immediately adjacent is analyzed.

(5) Role of Lake Kivu

The lake effect is mentioned only briefly (lines 120 and 249) and may deserve some expansion. It is now known that Lake Victoria in Uganda (near to the region of interest) and Lake Maracaibo in Venezuela have dramatic effects on lightning activity. (See

for example the recent work by Albrecht et al. on tropical lightning hot spots, already mentioned.) So more should be said about the physical role of this lake, with possible inclusion of information on its size and about other studies of that role.

(6) The MCS issue

In the last paragraph of the Discussion section, the contribution of mesoscale convective systems is invoked. My big problem with this suggestion is that the authors have already documented the traditional 4 pm maxima in the lightning activity, and that is strongly suggestive of local (solar-stimulated) convection (assisted by cold outflow boundaries) rather than MCS activity that generally maximizes later in the diurnal cycle (and hence the greater prevalence of sprites later in the diurnal cycle, about which the second author is well aware, plus the fact that Africa is the leading "chimney" for sprite activity globally according to ISUAL satellite observations). So I am inclined to agree with what is stated in line 279. But expanded discussion on this aspect is needed. The authors should also consult TRMM work by Karen Mohr on African convection. And given that Zipser et al. (2006) is invoked, the diurnal phase of superlative activity in that study should also be examined and reported here.

(7) Observations with little if any interpretation

The paper has many analyses and observations that are not accompanied by physical interpretation. The Abstract for example contains no physical interpretations at all. Table 3, Figures 3, 4 and 5 are in a similar category. This aspect needs major improvement. It is helpful if every proposed analysis has a specific scientific purpose, and so also warrants an interpretation.

Detailed comments/edits on the text:

The authors are not native English speakers and so there are many edits needed to clean up the text:

Line 18 Suggest dropping "very"

Line 20 “days”

Line 27 Suggest adding Williams and Satori (2004)

Line 28 suggest changing “space” to “spatial”

Line 30 change “instance” to “example”; delete “the”; “from the Lightning. . .”

Line 31 “resolution”

Page 2

Line 32 “larger dynamic”? Meaning?

Line 34 “maxima”

Line 35 change “both” to “neither”

Line 36 change to “maxima remains throughout the year in considering the lightning activity with 3-month seasons”

Line 37 what is physical interpretation of “very sharp and localized maximum”

Line 38 “in the eastern Democratic. . .”

Line 42 “scattered over a large area”

Line 43 “maximum activity could. . .”

Line 43 “linear scale for flash density was. . .”

Line 46 “maximum activity”

Line 48 change “whole” to “entire”

Line 49 “most of them quantified”

Line 51 “maximum in flash density”

Line 53 “The geographical extent of this region”

Line 57 “high spatial resolution”; “allowed a better localization and specification of its shape”

Line 62 “contrasting from year to year”

Line 63 “extends roughly”

Page 3

Line 66 “maximum activity”

Line 70 change “dimension” to “area”

Line 77 Attributable to what? (see earlier discussion)

Line 78 “the last two years”

Line 81 “radiation”

Line 84 delete the first “the”

Line 85 quantify “very little attenuation”; it is not small and for this reason large numbers of sensors are needed for global surveillance

Line 87 Why report this for 2014 when it is 2012 and 2013 that are used for analysis?

line 91 This would be 5 km resolution. You should justify that in terms of the accuracy of the stroke location in Africa.

line 94 “with the same”

Page 4

Line 95 “the flash count”

Line 96 “the maximum flash density for both areas and for each year”

Line 97 “exhibit total flash counts”

Line 97-98 “indicates a stable situation from one year to the next. In contrast, the ratio. . .”

Line 99 “one year to the next” 4 digits here is overkill on precision

Line 101 “localized”; “one year to the next”; “Furthermore, the spatial density. . .”

Line 103 “depends on the spatial resolution”

Line 104 “at a resolution”

Line 105 “maximum of flash density”

Line 109 “clearly appears”

Line 114 “thunderstorms, which means that the number of flashes per day is larger. . .”

Lines 115-116 These two factors could be distinguished with WWLLN observations but you need to check the temporal development.

Line 117 I hope the authors disclose “specific and local conditions”

Line 119 Which side and why?

Line 120 “increases markedly”

Line 124 “daily cycle of flashes detected by the WWLLN”

Line 125 “These flash counts are calculated. . .”

Line 126 “so that the flashes are associated with the . . .”

Page 5

Line 129 “for the minima in the morning. . .”

Line 130 “and for the maxima in the afternoon. . .”

Line 131 “contrast in flash counts between. . .”

Line 134 Add comma after “day”

Lines 133 to 136 What is your interpretation?

Line 138 “distribution of flashes”

Line 139 “year of reference”? Only one year?

Line 142 How were the various classes selected?

Line 145 “also plotted in Figure 3” (reduce redundancy)

Line 148 “number of days”; “about twice that of Area. . .”; “157 versus 84”

Line 155 “Variability of flash counts during. . .”

Line 157 “a clear minimum activity”

Page 6

Line 160 “defined as the high activity” But you haven’t quantified HAP and LAP.

Line 165 change to “and also in roughly the same proportion. . .”

Line 166 “with number of flashes exceeding 5000 (CL6-CL111)”

Line 167 “during the LAP”

Line 169 “during the HAP and the LAP”

Line 170 “During the HAP”

Line 171 “of days”

Line 172 “number less than 5000”; “whereas during the LAP”

Line 174 You don’t have a real motivation here. Tell why you might expect correlated behavior.

Line 178 For this you should be giving local times, not UT times. Otherwise you lose

the physical interpretation.

Line 180 You should be reporting correlation coefficients in the text in the same form as in the figures. Otherwise this is potentially confusing.

Line 182 “it also increases for the other”; “first glance” of what?

Line 190 “is more widely distributed during the day”

Line 192 I don’t understand the meaning here? Clear-cut?

Page 7

Line 193 Shouldn’t this section be merged with 3.3 Annual variability. It is the same topic.

No discussion of the important semiannual variation in this section.

Line 194 “proportion”

Line 206 suggest adding text: “based on satellite optical observations of lightning” to distinguish from the approach taken here with VLF data. You should also define “hotspot”

Lines 210-211 What did A. Laing say in there about MCSs?

Line 214 Considered by whom? These are not the times considered in Section 3.2.

Line 216 This is yet another time interval.

Line 217 This is not what you reported in lines 128-130.

Line 219 “locations than our two areas”

Line 224 “result for 2011” on WWLLN ? Please clarify.

Page 8

Line 225 What is the meaning of “minimum proportion”?

Line 228 The authors need to articulate their views on the ITCZ in the lightning context. In my experience, the activity lightning is usually adjacent to the ITCZ because one needs subsidence to eliminate the widespread cloudiness that is shuts off the destabilizing influence of sunlight.

236-240 Nothing is included in here about antecedent conditions of rainfall, that can influence the Bowen ratio. See also Williams and Stanfill (2002; Comptes Rendues).

Line 249 Need more discussion on the role of “great lakes” in the lightning context

Line 250 “for the development”

Line 251 “at the planetary scale”; when do “the most intense storms” max out in the diurnal cycle? Are they isolated, or are they parts of MCSs?

Line 256 “spread from the east to the western Congo basin”

Line 257 Only if MCS status. But don’t forget role of cold outflow toward the west.

Page 9

Line 259 And antecedent rainfall. In any case, more should be said about the nature of the surface in the areas selected. In this context, Williams and Satori (2004) should be consulted.

Line 263 “regions of strong coupling between the atmosphere. . .”

Lines 264-265 One does not want a contrast if one is seeking to explain correlated behavior.

Line 266 “mesoscale convective systems”

Line 269 “in the Congo basin”

Line 270

“frequently overshoots the tropopause. The climatology. . .”

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Line 272 “From a five-year series of data. . .”

Line 273 “to the western side of the high mountains”

Line 275 “maxima in the number”

Line 279 I tend to agree with this statement but the discussion on MCSs needs to be elaborated on here.

Conclusions, like Abstract, is lacking in physical interpretation.

Line 282 “The spatial and temporal characteristics of the lightning. . .”

Line 282 “strongest thunderstorm activity”

Line 283-284 change to “with a secondary maximum”; “concentrated in the same part”

Line 287 to 288 “is similar in both areas

References

Suggest adding Williams and Satori (2004)

Williams et al. (2000, JAM) considers variations of tropical flash rates and diurnal cycles of flash rates and storm counts.

Williams (2012, Franklin Lecture) considers impact of changes in WWLLN detection efficiency over time.

Table 1 Two significant figures is probably more appropriate. In some places the authors use four!

Figure 1 The hotspot areas straddle the equator. Some discussion is needed about that aspect alone in driving the lightning counts up high. These zones are visited at least twice per year by the zone of instability. Caption could also mention location accuracy of individual strokes.

Figure 2 Suggest changing “amounts” to “counts” Please compare this variation with

those documented in Williams et al. (JAM, 2000). 4 pm is very consistent, and with Schumann resonance observations of “background”

Figure 4 Better to show flash counts that CLi classes, which require going elsewhere to check on definition/motivation. What is the thermodynamic situation on days with > CL10? Curious minds want to know.

Figure 5 If Rˆ2 value are used here, same values should be discussed in the text.

Figure 6 Need more discussion on semiannual and annual variations in general. (See earlier remarks.)

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