

## **Reviewer #2**

*The authors have somewhat improved the manuscript but it is still far from possible acceptance. The level of statistical thoroughness is insufficient to judge the significance of the results and raises several major concerns. Accordingly, before this is clarified, the reliability of the claimed results cannot be assessed.*

**We thank the reviewer for the careful reading of the manuscript and thoughtful and improving comments.**

*1. Datasets. The authors ignored my previous requirement to show the annual  $B_z$  and  $B_y$  series which are not even described in how they were obtained. All the used datasets, including  $B_y$ ,  $B_z$ ,  $B||$ ,  $BT$ ,  $E$  must be shown and described. Without that, the results cannot be trusted.*

**We are sorry, we misunderstood your previous comment and thought you were interested in cross-correlation coefficients between the SSN and these quantities, which we put in Table 1 in the first revision.**

**Now, in the new revision, we show the time series of  $B_y$ ,  $B_z$ ,  $E_{||}$ , and  $E_T$  used for the construction of cross-correlation maps with lightning (Figure 5 and related text – lines 261-262 and 419-420). In addition, we also show the ENSO index.**

*2. The authors do not describe how they estimate the significance/confidence of the obtained correlation coefficients. This is particularly crucial for the smoothed data series since the significance/confidence of correlation between smoothed series cannot be estimated using the standard formula. The authors should explicitly describe how they evaluated the 95% conf. level for the smoothed series. I suspect that was done incorrectly leading to the overestimated results.*

**Thank you for this comment. Considering also the concern of the other reviewer on the effect of ENSO, we now provide cross-correlation maps with the ENSO index (Figure 10 and the related text – last paragraph of the Section 3) instead of smoothing lightning data in time. We completely removed the cross-correlation maps with the smoothed lightning data in the revised version**

**The statistical significance of correlation between data series (now only unsmoothed) is calculated by the `corrcoef` function in MATLAB. It is based on the use of the t-statistics with  $N-2$  degrees of freedom. This information is added in the text (Section 2, text after Eq. 2).**

*3. Some correlation maps do not look statistically significant. For example, the fraction of the shown coloured bins in Fig.2a is  $\sim 5\%$  which is fully consistent with the applied significance threshold ( $<0.05$ ), viz. 5% of the correlation values may appear seemingly significant just by random coincidence. This should be clarified.*

We now discuss a possibility of random coincidence in the end of the first paragraph of the last Section (Discussion and Conclusions). We also note that random coincidence is probable for the cross-correlation with  $B_z$  component.

4. In addition, there is a problem with the units of the analyzed data. This doesn't affect the results based on the standardized series but is quite annoying. For example, the authors discuss the number of all lightning strikes during the analyzed period (Fig.1) with the maximum values of  $\sim 3000$  strikes (per  $1 \times 1$  deg bin, as specified in line 208). However, this leads to  $0.02$  lightning/ $\text{km}^2/\text{yr}$  which is obviously too small. Is it the number of strokes given per  $\text{km}^2$  or  $1 \times 1$  deg bin? Fig.4c gives the values of  $3E5$  light strokes per bin which is inconsistent with Fig.1. Another example is Fig.4b which is declared as "Yearly NM counts" with the value of  $\sim 3E4$  counts/yr which leads to an obviously wrong count rate of  $\sim 3$  counts/hr. All the units must be verified.

Thank you for noting. Regarding Figure 1, we accidentally plotted map of simulated thunder day numbers. We now provide both map containing corrected number of lightning (Fig 1a) and the number of simulated thunder days (Figure 1b).

Concerning Figure 4b, we corrected the description/caption. It is correctly "Yearly averages of 1-min NM counts" and the related text on lines 244-245.

Other minor comments include:

- Please write "sunspot number" (not "Sun spot number")

#### Corrected

- Line 168-169: what does "have been studied primarily for simplicity" mean?

To make it clear, we reformulated to "...but since the relative changes in  $B_y$ ,  $B_z$  are much larger than the relative changes in the Earthward solar wind speed  $v_x$ , only the dependencies of the Earth's electric field on  $B_y$  or  $B_z$  have been often studied (e.g., Burns et al., 2008)"

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#### **Reviewer #1 (E. Williams)**

The authors have done a thorough job in addressing the detailed comments and suggestions by the reviewers, and in comparing their findings with other results in the literature, and on this basis, the manuscript is much improved. The statements of the problem have also been much more thoroughly addressed. But given that the results are so markedly different than in the first round, it would be valuable (at least for the reviewers' benefit) to know why the results are so different, and especially how confident the authors are in the end that there is a physical connection between the solar cycle and global lightning activity.

Summary: Consider for publication after minor revisions

Text edits: (Note that line numbering is based on the "tracked changes" version of the revision.)

**We thank the reviewer for the careful reading of the manuscript and helpful comments.**

*Line 43 “around zero” is not very quantitative*

**Specified, “(absolute value less than 0.2)”, on line 34**

*Line 67 “e.g., Markson (1981)”*

**corrected**

*Line 88 What exactly was questioned by Hale (1979)? This sentence is not clear.*

**reformulated “Hale (1979) suggested to look for effects more directly related to magnetospheric and auroral processes.”**

*Line 115 “using the World Wide Lightning Location Network”*

**corrected**

*Line 128 “that the satellite”*

**corrected**

*Line 131 “consistent” in what respect? The sentence is unclear.*

**Reformulated, “The WWLLN lightning counts in  $1^{\circ} \times 1^{\circ}$  bins are used in this study, but is also shown that similar results are obtained if larger bins ( $3^{\circ}$  latitude  $\times$   $6^{\circ}$  longitude) are used.”**

*Line 135-136 These will generally be CG strokes so the 30% estimate for all lightning is misleading and inflated. The WWLLN operators have never been straightforward about their detection efficiency estimates. We know what the mean CG peak current is in lightning, so why not give a detection efficiency for that particular value of peak current, instead of a substantially larger one (30 kA)?*

**Specified that the detection efficiency concerns the CG strokes (line 123)**

*Line 143 “sensors”*

**corrected**

*Line 148 Yes, WWLLN has been weak in Africa because few sensors are located there. The authors should also consult the paper by Virts et al. in BAMS which is perhaps the best paper to date on WWLLN performance in comparison with other optical detection systems.*

**Reference to Virts et al. added (lines 132-134)**

*Line 177 “logistic function” What is it? Why logistic?*

**Text around Eq (3) was reformulated/expanded. We use the logistic function to avoid a fixed threshold and to avoid a small uncertainty around it.**

*Line 207 "strokes"*

**corrected**

*Lines 212-213 You should have commented on this important aspect earlier in the manuscript.*

**Now mentioned also in section 2 (lines 136-137)**

*Line 217 "also be shown"*

**corrected**

*Line 231 "the South Atlantic Anomaly region"*

**corrected**

*Line 236 "also shows up over a part of ..."*

**The whole text and Figures related to smoothed data were removed. See the response to comments of the reviewer 2 please.**

*Line 325 "number of lightning strokes"*

**corrected**

*Line 420 What does the MJO have to do with the main goal of the study, which is the 11 year solar cycle?*

**Reformulated. MJO can affect rain intensity and lightning and there was a suggestion in the literature that it is linked with the ionospheric potential (line 355-356).**

*Lines 421-422 Positive variation? What does this mean when the MJO is a global wave and  $V_i$  is a DC phenomenon? Please clarify.*

**Reformulated. MJO can affect rain intensity and lightning and there was a suggestion in the literature that it is linked with the ionospheric potential (line 355-356).**

**Reformulated.**

*Line 422 "depend"*

**corrected**

*Line 423 "the ENSO"*

**corrected**

Line 424 "occurs"

**corrected**

Line 425 "Schumann"

**corrected**

Line 427 change "solar" to "a"

**changed**

*Line 428 These events were not Super El Ninos (ONI index > 2 C) so how "reasonable" is this conclusion has not been clarified by the authors. Instead, they seems to be hoping that ENSO aliasing is not a problem. (I am reviewing another paper on lightning trends in the South China Sea, and ENSO aliasing has been a problem in establishing a decadal trend.)*

**We changed the text (lines 366-371 and also 362-263) . In addition, we now provide the maps of significant cross-correlation coefficients between the ENSO index and lightning counts (Figure 10a) and thunder days (Figure 10b).**

*Line 442 This text line (as are many others in my copy) is gibberish.*

**We are sorry it was a problem of pdf file (version) containing track changes. We now provide text with highlighted changes (yellow).**

Line 443 "found a solar cycle"

**corrected**

Lines 445-446 Change "In contrary" to "In contrast"

**corrected**

Line 448 "over a non-negligible part"

**The whole text and Figures related to smoothed data were removed. See the response to comments of the reviewer 2 please.**

*Line 452-453 Do you mean to say "not uncorrelated"? (double negative). This sentence is not as intended and needs to be rewritten. It is not clear at present where the authors stand on Markson's (1981) claim that cosmic rays and ionospheric potential are anti-correlated.*

**Reformulated. "cosmic rays are uncorrelated with lightning activity over most of the globe."**

Line 461 "the By component"

**corrected**

*Line 465 "the South Atlantic anomaly"*

**corrected**

*Line 467 "Earth's"*

**corrected**

*Line 469 "a large flux"*

**corrected**

*Line 476 "The energy spectrum..."*

**corrected**

Line 483 Gibberish again (unintelligible sentence, needs rewriting)

**We are sorry it was a problem of pdf file (version) containing track changes. We now provide text with highlighted changes (yellow).**

Line 488 Sentence remains unfinished.

**We are sorry it was a problem of pdf file (version) containing track changes. We now provide text with highlighted changes (yellow).**

Lines 496-497 Another incomplete sentence

**We are sorry it was a problem of pdf file (version) containing track changes. We now provide text with highlighted changes (yellow).**

Line 501 Sentence is difficult to read/decipher.

**We are sorry it was a problem of pdf file (version) containing track changes. We now provide text with highlighted changes (yellow).**

Line 503 Sentence is incomplete.

**We are sorry it was a problem of pdf file (version) containing track changes. We now provide text with highlighted changes (yellow).**

I don't see a real conclusion to this work (though the last sentence is covered up in my copy and may be very important). Are the authors happy and content with their results? It seems a collection of results many of which bear little relation to other earlier findings. One of these is Pinto et al. (2013). Another is the strong Siberia result in Brooks (1934), as the present correlations in that region are mostly negative

(anti-correlations). The robust nature of positive correlations almost everywhere evident in the first round has largely disappeared. The possible connection with the South Atlantic Anomaly is interesting. What single additional effort do the authors view as being able to shed important new light on this problem?

**The larger regions of positive correlation in the first submitted version were based on the trend removal using quadratic polynomial and on smoothing lightning data over neighboring years. Based on the critical comments made by the reviewers, we do not use these methods any more. We now only use official correction coefficients to the WWLLN data and in the second revision, we also do not use smoothing of the lightning data. Instead of that cross-correlation with the ENSO index is provided.**

**Yes, we agree that our study does not confirm some of the previous works. Note that it is done over different time intervals and that also the results in the literature differ (see also the Discussion section). We agree that the most interesting results is the connection with the South Atlantic Anomaly. It does not depend whether lightning counts or thunder days are used. As we discuss, further investigations are needed.**