Second review of: "Solar cycle signatures in lightning activity" by J. Chum, R. Langer, I. Kolmasova. O. Lhotka, J. Rusz and I. Strharsky

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 statistics 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.)

Line 43 "around zero" is not very quantitative

Line 67 "e.g., Markson (1981)"

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

Line 115 "using the World Wide Lightning Location Network"

Line 128 "that the satellite"

Line 131 "consistent" in what respect? The sentence is unclear.

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)?

Line 143 "sensors"

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.

Line 177 "logistic function" What is it? Why logisitic?

Line 207 "strokes"

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

Line 217 "also be shown"

Line 231 "the South Atlantic Anomaly region"

Line 236 "also shows up over a part of …"

Line 325 "number of lightning strokes"

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

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

Line 422 "depend"

Line 423 "the ENSO"

Line 424 "occurs"

Line 425 "Schumann"

Line 427 change "solar" to "a"

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.)

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

Line 443 "found a solar cycle"

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

Line 448 "over a non-negligible part"

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.

Line 461 "the By component"

Line 465 "the South Atlantic anomaly"

Line 467 "Earth's"

Line 469 "a large flux"

Line 476 "The energy spectrum…"

Line 483 Gibberish again (unintelligible sentence, needs rewriting)

Line 488 Sentence remains unfinished.

Lines 496-497 Another incomplete sentence

Line 501 Sentence is difficult to read/decipher.

Line 503 Sentence is incomplete.

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?

End review

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