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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Statistical similarity between high energy charged particle fluxes in near-earth space and earthquakes" *by* P. Wang et al.

Anonymous Referee #1

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I find this manuscript (1) very difficult to follow, (2) parameters seem to be chosen without explanation, and (3) I note that standard statistical methods are not used and, in particular, statistical significance is not considered.

1. As an example of how it is difficult to follow the exposition, consider the first paragraph of section 2 (lines 15-20): Here the particle burst (PB) frequency fluctuation is defined as the number of occurrences per time window. It is not clear how this time window is defined, how long it is, etc. It appears that it is related the time between earthquakes. What earthquakes are being considered (global? above a certain magnitude?) is not explained. If there is a long duration between earthquakes, are we to

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understand that the length of the time window is changed? There are similar problems with exposition at other points in the manuscript, which is very difficult to understand.

2. In section 2 it appears that several key parameters are being chosen. Why they are chosen is not explained, and one has to wonder whether or not they are simply being chosen so as to maximize reported correlations (which would represent a serious problem with objectivity). So, for example, why on line 24 do the authors choose the particular electron energy range? Why on line 27 do they choose a particular and very limited L range? I would like to emphasize that these choices should not be made to maximize reported correlations. I have no way of telling, from the material presented here, whether or not this is what has actually been done, but the specificity of chosen parameters raises questions which, at the very least, need to be explained and explained clearly.

3. While it is difficult to tell, it appears that the entire statistical analysis of PB events is conditioned on the occurrence of an earthquake (what size earthquakes, we don't know). In general, however, to establish correlation between two data sets, one should NOT condition the counting of one data set on the properties of the other. Each data set should be treated independently of the other. Anything else can result in biased results. But, as before, the exposition is so difficult to follow, I can't actually tell if this error is being made.

Furthermore, the most important plot seems to be Figure 3b, for which it is asserted (section 3) that the statistical distribution of P(z) for earthquakes is like that for particle bursts (PB). Honestly, when I look at this plot (comparing open symbols for PBs with closed symbols for earthquakes) it looks to me like the two distributions are very different. Note, for example, that the open symbols have a distribution that has broad shoulders, while the closed symbols are sharply peaked at the center of the distribution.

The authors have provided no objective measure of the statistical similarity of the dis-

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tributions for PBs and earthquakes (whether chi-squared, Kolmogorov, or otherwise), and, unfortunately, it appears that they have not tried to analyze the statistical significance of their results against data that were not part of the original formation of the hypothetical similarity of the two distributions. In general, significance is established by comparison against a second data set. Most objectively, this second data set is obtained AFTER the hypothesis is clearly and quantitatively stated. Simply saying that one distribution looks like another is not sufficient, and, indeed, in this case the two distributions (as I've noted) don't really look similar. Since this seems to be the main point of this manuscript, I have no alternative but to recommend that the manuscript be rejected.

Smaller points:

1. Note that earthquakes are generally defined in terms of base 10 statistics, not base e, so all discussion of magnitude in terms of exp(M) needs to be revised.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 3183, 2014.