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Interactive Comment

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

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Letter to the Editor

Dear Sir,

We appreciate the referee's detailed and useful comments and suggestions. The point-by-point answers to comments and suggestions are listed as below.

Q1. 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 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.

Answer1: The particle burst (PB) frequency fluctuation is defined as the difference between two numbers of occurrence, which are counted in the same time window centered at the time of two different seismic events. Here the time windows we adopted are ± 0.5 and ± 4 days, respectively. Shorter time window will result in seldom PBs included, while longer time window will increase the possibility of overlapping for two successive seismic events.

The earthquakes are worldwide distributed, which magnitudes are greater than or equal to 5.0. This was illustrated in figure 3a.

Q2. 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.

Answer2: The energy range we chose (0.97∼2.3 MeV) is the high energy band of the DEMETER's particle detector-IDP. The reason for our choice is that there were several space experiments (Aleksandrin et al., 2003, Sgrigna et al., 2005) focused on possible correlation between earthquakes and high energy particle fluxes. These experiments were all carried out in the high energy band (above 4 MeV). As a first step for our study, we presently focus on the analysis of high energy particles.

McIlwain L-parameter with L=1.3 is near the lower boundary of inner radiation belt. For lower L (L < 1.3), collisions of electrons with atmospheric atoms dominate the loss of trapped electrons (Walt, 2005). For L>1.4, rarely counting rates are recorded with energy within this range by the DEMETER with exception of high latitude region.

Q3. 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 distributions 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.

Answer3: Both earthquake events and particle fluxes were selected from the same L range. Earthquakes are global and with magnitude greater than or equal to 5.0. So that earthquakes data sets are fixed with events arranged in time order. It is helpful to use time window as one variable to describe the time correlation between PBs and earthquakes. We compared the distributions of PBs occurrence fluctuation in two different time windows, i.e., ± 0.5 and ± 4 days, in figure 2. This figure shows evident difference between them, and suggests that the distribution of PBs occurrence fluctuation is time window dependent.

We also computed the P value with Szekely energy method for data sets of earthquakes and PBs. The result is P=0.498, which means that the possibility that the two data sets have the same distribution is not rejected at the 5 percent level based on this test.

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.

Answer: In this paper, exp(M) is a quantity related to the energy dissipated in an earthquake with magnitude M. This is consistent with the discussion by Caruso et al.

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