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Interactive comment on "Defining scale thresholds for geomagnetic storms through statistics" *by* Judith Palacios et al.

Anonymous Referee #1

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The authors (Palacios et al.) fit model statistical distributions to data from geomagnetic index time series. The authors find (section 3.2) that many of these model distributions fit the "bulk" of the data, but do not (simultaneously) also fit the tail of the distributions. They suggest that the intersection of the cumulative of the model distribution and the data distribution might be interesting as a threshold of sorts that might useful for characterizing risk (abstract and other places).

I have some significant concerns about this analysis, and I don't see how it can be fixed. The manuscript should be rejected.

1. First of all, it is important to recognize that autocorrelated time series (such as geomagnetic indices) are not statistical data (which are assumed to be "independent"). In this respect time series analysis and statistical analysis are fundamentally different. It

is, therefore, a serious mistake to simply lump all values of a time series into a statistical analysis and fit distribution functions to them, and, even, apply tests of significance (as the authors do with Kolmogorov tests). I refer the authors to the following reference material:

Priestley, M. B., 1981. Spectral Analysis of Time Series, Academic Press, London, UK, Chapter 5.3.2.

Thiebaux, H. J., and F. W. Zwiers, The interpretation and estimation of effective sample size, J. Climate Appl. Meteorol., 23, 800-811, 1984.

von Storch, H., Misuses of statistical analysis in climate research, in Analysis of Climate Variability: Applications and Statistical Techniques, edited by H. von Storch and A. Navarra, pp.11-25, Springer-Verlag, New York NY, 1995.

If the authors simply focus on the largest (most extreme) maximum values for each magnetic storms, this will remove almost all of the autocorrelation in the data, and they could, then, consider a statistical treatment. Note that examining the largest maximum values is not the same as simply examining the largest values of a time series, since large values tend to be autocorrelated within a few storms. Nope. Instead, find the maximum of each storm, and then treat the largest of those maximum values. This requires a special algorithm that can be rather tedious to implement.

If the authors take this approach, then the data will only be in a "tail" and the authors won't have to worry about the "bulk" of the data (as they presently are). Fits will only be made to these "tail" data and tests of significance can be performed.

2. Next, the authors seem to find it interesting to compare a whole bunch of different statistical distributions (e.g. Table 2 and other tables), and then take the one that seems to fit best (section 3.2). This is not how statistical investigations are normally performed. One usually starts with a hypothetical process (based on physics) that gives rise to a hypothetical distribution (normal, lognormal, etc.), and then one tests

this distribution against data. If the data have a high probability of being realized from the hypothetical distribution, then the hypothesis cannot be rejected. This is not the same as “accepting” the hypothesis, however. This issue is discussed in many books on statistical hypothesis testing.

So, the authors need to think about what the physical process they are testing, and focus on the one distribution that makes sense. Shopping for other distributions (among a constellation of many) is not the way to go.

3. The authors suggest that they are providing something useful for “risk” analysis. I do not see how this applies. Note, risk is normally defined as the probability that one will lose a certain amount of an asset over a given window of time. As I’ve already noted, the authors are not really performing a proper statistical analysis, so it won’t apply for a risk analysis either. And, then, the authors haven’t explained how their analysis actually fits into a risk analysis. Really, they should just drop this word, and concentrate on their geophysical investigation.

4. I think it is reasonable to ask if this analysis (by Palacios et al.) is actually all that relevant to the journal Natural Hazards and Earth System Science. Perhaps by starting over and performing a proper statistical analysis, and showing how it relates to hazard analysis we might have something of relevance. But right now, this manuscript is very seriously flawed.

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