October 2, 2014

Dear Dr. Knipp:

I am puzzled by your Sept. 30 letter which seems to reject my Comment paper submission 2014SW001123 without any meaningful review. There are aspects of the situation which I lack the information to understand, and I am hoping that you can clarify them.

1. You indicate that regardless of the quality of the submission, acceptance would violate American Geophysical Union publication standards to which Space Weather adheres. You write:

> "Space Weather adheres to the publications standards of the American Geophysical Union (AGU). AGU prohibits the submission of material for publication that has been previously published in any form that constitutes public distribution. See http://publications.agu.org/author-resource-center/publicationpolicies/dual-publication-policy/

> Since both your comment and the supplement appear on the web page http://www.math.umb.edu/ sp/papers.html you have publicly distributed the material prior to submission to Space Weather."

The link you quoted from the AGU standards does not say that at all. In fact, it says the exact opposite:

"... AGU prohibits the submission of material for publication that has been previously published in any form that constitutes public distribution. ..."

"Posting of a preprint of an article via electronic media does *not* [emphasis mine] constitute prior publication"

Moreover, a few days before, Chief Editor Dr. Lanzerotti asked me to resubmit the manuscript with two short paragraphs removed, the second of which directed the reader to my website www.math.umb.edu/ \sim sp for further details and discussion. Why would he do this if Space Weather policy clearly precluded consideration of the manuscript because it had been posted on my web page? (I spent about half a day on the resubmission, due to bugs in the byzantine automated submission system.)

2. Next you write:

"Additionally, I have examined the Riley manuscript and find that his work stated that applications of a power law distribution for any of the severe space weather classifications were assumptions. Other assumptions were also clearly stated. There are adequate cautionary statements and caveats in the work to cause any serious reader to understand that the work was presenting estimates based on sparse data. An independent review of the Riley paper by a scientist with expertise in applications of power laws reached a similar conclusion."

I agree that there are many caveats in Riley's paper, caveats which are generally omitted in extensively reported summaries in the popular media. The Comment does not criticize a lack of caveats so much as it focuses on one key omission: the paper states a mathematically incorrect definition of "power law" which omits a parameter x_{min} whose value is crucial to checking the paper's subsequent arithmetic. Without knowing this parameter, it is *impossible* to obtain the probability estimates which the paper presents. For no data set does the paper state this parameter.

In many but not all of his case studies, the parameter x_{min} can be reasonably guessed (e.g., from the figures). When the arithmetic yielding Riley's probability estimates is performed using this reasonable guess, the probabilities reported in the paper typically differ by an order of magnitude from those which I obtain using algebraic expressions identical to those of the paper.

I maintain that the paper's arithmetic is often wrong, resulting in probability estimates which differ by an order of magnitude from the estimates which would follow from the paper's power law assumption. Naturally, Space Weather cannot be expected to take my word for this. But I think that any observer with faith in Space Weather's integrity *would* expect that Space Weather would seriously investigate the allegation. There is no hint in your letter that anyone has even looked at the substance of the allegation.

Your letter concludes:

"Thus, even if the dual publication issue had not arisen, I see little reason to begin a comment-reply cycle with referees, who would most likely report that: 1) the Riley paper provided adequate cautionary statements, and 2) an extreme event has already been realized in a near-miss scenario."

This completely ignores the main issue of whether the probability estimates presented in the Riley paper are correct *under its assumptions*.

In principle, this could be easily and quickly determined by simply asking the author if he agrees with my arithmetic, which is presented in unusually complete detail in the submitted Supplementary material. I cannot imagine that we could fail to agree on the arithmetic. Of course, there is the possibility that I could have made a mistake as well as he, but a normal presumption of competence and integrity on both sides would predict almost immediate agreement on the facts of the case. If I am wrong, I would appreciate the opportunity to gracefully withdraw the Comment. If the author is wrong then he should submit an erratum and if he does not, Space Weather should unilaterally retract the paper.

Of course, we can imagine scenarios in which the author refuses to respond, in which case external referees would be burdened with the arithmetic, but why worry about such hypotheticals when they might not occur? I have not been informed if the author has been consulted, but if he has not, I cannot imagine why this commonsense first step has not already been taken.

I have sent the author four courteously worded inquiries over a two month period trying to resolve privately the issues of the arithmetic. Since he has not replied nor even given any acknowledgement, I have to assume that he is deliberately ignoring all of my inquiries. I submitted the Comment only in the hope that an inquiry from Space Weather would not be ignored.

I am retired and have no need to publish. My assumption has been that the author would eventually see the need to address the issues of the arithmetic and either submit an erratum or retract the paper, in which case the Comment would never be published. I assumed that Space Weather would recognize that the integrity of the journal is at stake and would take reasonable steps to investigate the matter. That is still my hope.

In closing, I have the following questions.

- i. Has Space Weather made any attempt to obtain the author's reaction to my Comment? If so, and if there was a written response, may I have a copy?
- ii. It is customary for scientific journals to furnish copies of referees' reports to the author, particularly in case of a rejection. Have any referees been consulted concerning my Comment? If they have made any written observations, may I have a copy?

This is important to me because my reputation is important to me. If I have made any mistake, I want to correct it on my website or wherever appropriate.

iii. Your letter states :

"An independent review of the Riley paper by a scientist with expertise in applications of power laws reached a similar conclusion [that Riley's paper contains adequate cautionary statements and careats]."

This is too vague to evaluate. Was his "review" taken in conjunction with my Comment, or in some other way such as a referee's report on Riley's original submission?

Did the expert address the question of whether it was proper for Riley to omit for all data sets the information of which minimum value x_{min} was used to obtain his probability estimates, making it difficult (in some cases) or impossible (in others) to check the paper's arithmetic?

Did he or she address the question of whether it was proper for the paper to omit the information that its 12% probability estimate (for the probability of a Carrington-class event in the next decade) was based on only about 20 Coronal Mass Ejection events, while expressing reservations about the very different estimate of 3% (from nitrates in ice core samples) because it was based on a data set of size only about 70? (This is one unequivocal example of a caveat which should have been given but was not.)

If the recommendation was written, may I have a copy?

Sincerely yours,

Stephen Parrott