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Interactive comment on "Stochastic consideration of relationship between occurrences of earthquake and fluctuations in the radio wave propagation" *by* Kuniyuki Motojima et al.

Anonymous Referee #1

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In this (and previous) work the Authors attempt to establish a statistical relationship (validation) between earthquake occurrence and a particular type of purported earthquake precursor, namely the alteration of atmospheric refractivity in the VHF radio band. Unfortunately, I find myself to be very skeptical, if not apprehensive of their approach.

There's a persistent (and frequently perilous) problem with the study of earthquake precursors: The existence of most precursors has actually never been physically demonstrated beyond reasonable doubt! The most fundamental problem has always been the dearth of verifiable models and physical mechanisms that would consistently explain the "precursors" and, more importantly, allow for reasonable discrimination of "genuine

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precursors". In some cases, is has even been determined that laboratory-tested physical mechanisms proposed to explain certain types of precursors had little or nothing to do with the reality of crustal-scale Earth processes. In most cases, "precursors" have been postulated on the basis of a perceived association of earthquake occurrence with relevant observational evidence, although this evidence was often inadequately constrained, or circumstantial, or even anecdotal of nature. An outcome of this deficit has often been that wherever there were many earthquakes, many occurrences of other natural phenomena and some Idea (model) associating earthquakes and phenomena, researchers were also tempted to make the necessary leap of faith and suggest an association on the basis of statistics. The pitfall of this approach is that a viable null hypothesis on which to base statistical appraisal could generally not be clearly defined and in the end, all that the statistical approach could prove was that it is always possible to formulate a correlation of anything with anything! In some cases the postulated statistical association of earthquakes and the Idea developed into confirmation bias in favour of the Idea which, in turn, has become the basis of spectacular claims and vivid discourse that has generally ended up with the subject matter being spectacularly or silently laid to rest.

I think that some elements of the above narrative can be identified in the reviewed paper. Specifically: 1) A physical (causal) relationship between earthquake preparation processes and changes in atmospheric refractivity has actually never been demonstrated. Every theory or model attempting to establish such a relationship is at best conjectural! 2) Even if a physical mechanism that would change the refractivity of the atmosphere at the end-phase of some earthquake cycle exists, it is totally unknown whether this is universal or selective, i.e. depends on a quorum of as yet undetermined preconditions. It is therefore not possible, and in my opinion will hardly ever be, to appraise whether all earthquakes, some earthquakes, or no earthquake at all can affect the refractivity of the atmosphere... 3) To the best of my understanding a multitude of mechanisms other than earthquakes exists, that can affect atmospheric refractivity, and some of those are not yet completely understood.

I am afraid that the authors either do not take into account the limitations given above, or they appear not to. The way I see it is that they formulate statistical statements (probability gain, “hit” and “alarm” rates), based on the assertion that some earthquakes do indeed affect wave propagation in the atmosphere and the unspoken premise that the mere observation of “anomalous fluctuation” in wave propagation at some place along the epicenter – receiver line is sufficient to proclaim a causal effect between “earthquake” and “anomalous fluctuation”. Even more, they appear to do so irrespective of the earthquake’s source parameters (e.g. depth) and in spite of the prolific earthquake occurrence and industrial activity throughout the study area which, in my view, could generate a serious number of false detections. In addition, they base their inference on a statistically small number of cases!

At this point I feel I should point out that my apprehensive evaluation of the author’s statistical approach does not mean that I completely dismiss their methods and findings. Rather, my skepticism should also be understood in the context of the unknowns and uncertainties associated with our inadequate understanding of the physical mechanism(s) of earthquake-induced atmospheric refractivity modification. Accordingly, I do not suggest that the reviewed paper should be rejected – I am merely trying to emphasize that this line of research should be approached with due caution and extreme vigilance so as to prevent miscomprehension of the results by the less specialized or skeptical readership. Thus, although I believe that the paper is not suitable for publication in its present form, I also believe that it could be significantly improved so as to comprise a useful contribution, if the authors would acknowledge and specify the uncertainties and limitations associated with their work in a clear and unmistakable manner, so as to comprehensively inform their readership.

In a final comment, I also believe that the authors should also take particular care in order to improve the (technical as well as literary) English of their paper: in its present form it is definitely not up to standard!

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