Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2016-172-RC1, 2016

Natural Hazards and Earth System Sciences Discussions

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Interactive comment on "Atmospheric and ionospheric coupling phenomena related to large earthquakes" *by* M. Parrot et al.

Anonymous Referee #1

Received and published: 15 July 2016

The manuscript submitted by Parrot et al. addresses an important topic, and clearly is the result of a lot of work, analyzing a broad range of different data.

It is hard for me to evaluate it since it is outside my main fields of interests, but I believe that some criticism can be made without addressing the technical details.

In general, it seems to me that this study suffers from a lack of statistics-based quantification of the "significance" of the "anomalies" that are discussed. In some cases, I frankly do not see anything anomalous in graphs that the authors claim to show significant changes in some observables. This is the case, for example, of Figs. 2 and 3 and other analogous plots. In some other figures, differences can be seen visually, but there is nothing indicating that they should be considered "significant"–at least for a non-specialist as I am. Take for example Fig. 4: we are shown three curves, each

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Discussion paper

recorded on a different day. We are told that one curve represents "undisturbed conditions", and the other two, recorded on two consecutive days, are characterized by an "anomaly". But why should we call it an "anomaly"? What are the typical differences between observations of this kind that are made in the absence of large seismic events? Or does the "GPS TEC" indeed follow precisely the green curve on any day except for 4 and 5 days before the earthquake in question?

Another example is, to me, Fig. 16, where we are shown the behaviour of electron density during a month and a half, around the time of a seismic event. The curve shows a peak about a month before the event, but how does it look like over the rest of the year? and how do similar data look like in other areas, in the absence of seismic events? I am not saying that there is no relationship between this observable and the seismic event, but, rather, that this curve and the authors' discussion of it are not a convincing indication of such a relationship.

Somewhat more convincing, or at least more suggestive, are Figs. 17 and 25. In both cases, "outgoing longwave radiation" is shown over one entire year, and the largest peak in its "anomalies" happens shortly before the large earthquake under consideration. I, however, did not understand what the authors mean by "average", "daily values" and "anomalies", i.e. what the difference between the three curves shown in these figures is. As a result, I do not really know what to make of these figures. Also, the red curves show many other peaks: is there a statistically significant correlation between these and seismicity?

Finally, the paper is written in clear English, but contains many typos ("witch" for "which", "reviled" for "revealed") and some non-quite-English phrasing. The Figures are small and hard to read, at least in the pdf file I have downloaded. Often colorscales and axis labels are given withouth specifying the units of measurement. All this does not help the reader.

In summary, I think that this is an interesting paper, but needs at least a very thorough revision. I am not quite sure what the benefit of such a contribution would be in the absence of a quantiative (statistics?) evaluation of the "significance" of the observed anomalies. To be honest, I am hesitating between recommending rejection and major revision, and I am opting for the latter on account of my lack of specific competence in this field.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-172, 2016.