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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Foreshocks and Short-Term Hazard Assessment to Large Earthquakes using Complex Networks: the Case of the 2009 L'Aquila Earthquake" *by* E. Daskalaki et al.

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In the last years, complex network theory has been applied in a variety of studies on the spatio-temporal organization of seismicity. The present manuscript by Daskalaki et al. adds to this increasing body of literature by reporting efforts to utilize network approaches for characterizing the foreshock activity associated with the 2009 L'Aquila earthquake. Similar evolving network analyses are still quite rare in the natural hazards community, and the present work could serve as an interesting case study demonstrating the potentials of this approach. However, in its present shape, the manuscript by

Daskalaki et al. leaves some questions open, which need to be addressed in a careful revision before this work can be recommended for final publication.

Major comments:

1. From the description of the utilized approach for earthquake network construction, it is not clear if the resulting network is considered directed or undirected. Since the construction is based on a temporal succession of events in some well-defined direction, a directed network representation appears reasonable. However, in such case, the definition of ACC would not be unique, since different motifs of three nodes would be accounted for. This aspect should be clarified.

2. The information provided by the evolving earthquake network analysis in terms of ACC and small-world index can hardly be interpreted without knowledge of the associated link density (or, alternatively, mean degree) and its variation with time. This information needs to be added. Notably, the path length of a network shows an ultimate relation with the link density, which would be reflected in the small-world index. A similar statement applies to the ACC: if we have a sparse network with low mean degree, the fraction of nodes with degree smaller than 2 can be expected to be larger than for networks with more edges. Such nodes contribute with a zero term to the calculation of the ACC. Hence, the temporal signatures of ACC reported by the authors could also trivially reflect different link densities during different time windows. A way to circumvent this problem would be replacing the ACC by the "network transitivity" or clustering coefficient as defined by Barrat and Weigt, Eur. Phys. J. B, 2000. A comparative discussion of both measures in terms of evolving networks can be found in Radebach et al., Phys. Rev. E, 2013.

3. The authors relate the "more clustered seismicity pattern" identified by ACC to "the emergence of few nodes with higher centrality [supposedly betweenness centrality?], which act as hubs" (p.6, ll.5-6). This is not clear, which can already be seen from the previous comment.

4. It is not clear why network measures are necessary to identify the strong spatial clustering prior to the L'Aquila mainshock. Couldn't standard methods of spatial statistics serve the same purpose?

5. The authors claim that "the topological measures appear to outperform other observables reported in previous statistical work" (p.7, ll.23-24) without clarifying which previous variables are meant. No corresponding references are given, nor does the manuscript contain a detailed comparative study for the considered foreshock sequence. Since the performance statement is repeated twice on p.8, the least to be expected is further detailed information on this aspect. I also don't think that a comparative performance assessment is possible based on just a single case study like the one reported here.

Minor comments:

6. On p.3, l.11, I would speak of "hindcasting" rather than "forecasting", since the corresponding analysis has been made a posteriori after the event occurred.

7. In order to better understand the meaning of the parameters b and r, please give the Gutenberg-Richter relationship explicitly in the text.

8. In what sense has the foreshock sequence been produced by a physical process "dominated by a strong chaotic component" (p.3, l.24)?

9. On p.4, ll.25-28, the symbols N_a etc. are used to denote event indices, but rather resemble window sizes. Using different symbols might help avoiding possible confusion with standard notions of other papers.

10. The motivation for the selection of ACC and small-world index is not clear. Instead of the small-world index (which should be accompanied by the original reference), it would make more sense to study ACC and APL, since both are independent while ACC and small-world index are not.

11. The statement that the network properties are obtained "by averaging the corresponding properties over all the nodes of the network" (p.5, l.1) is not quite obvious for

the small-world index.

12. The term "degree" should be briefly explained at its first appearance in the text. Network scientists know this term very well, but this is not necessarily the case for seismologists.

13. The term "random regular graph" (p.5, l.30) does not exist – you have either a random or a regular graph.

14. Can one motivate the idea that "hubs that could serve as potential epicenter locators" (p.6, ll.14-15) from known seismological principles?

15. The authors state that "the recognition of the seismicity anomaly by topological measures does not discriminate the seismicity style, i.e., foreshocks, swarms or aftershocks" (p.8, ll.18-19). This is surely correct for the present network construction approach. In turn, other recent types of construction mechanisms have been used for declustering earthquake catalogs and, thus, identifying fore- and aftershock sequences (Jimenez et al., EPJST, 2009). It appears reasonable to add a corresponding comment.

16. For the definition of the small-world effect in complex networks, both ACC and APL are commonly taken into account together. What the authors report on p.9, l.17, for the behavior of APL seems not to fully comply with the common view. I recommend consulting the seminal work by Watts and Strogatz (Nature, 1998) for details.

17. It is not clear what the authors mean by "local [network] property" (p.9, l.24). BC is clearly a node property, however, its computation requires global linkage information on the entire network. In this regard, the term "local property" might be misleading.

18. p.9, ll.24-25: "BC(i) indicates that the i[-th] node acts as a central node influencing most of the other nodes" – how has this influence to be understood in the context of the considered earthquake networks?

19. In Fig. 2c, an additional horizontal line at b=1 might help visualizing the differences

for different time windows as discussed by the authors. In the caption, the right panels of Fig. 2b should be mentioned (even though they only represent a zoom of parts of the right panels).

20. The cumulated BC (CBC*) is not clearly defined in the text, and its definition and meaning are not clear from the text. This aspect needs to be clarified.

21. I recommend including Fig. S1 in the main text. A supplementary information for just a single figure does not seem necessary.

22. Throughout the manuscript, there seem to be numerous (yet minor) language issues like confusion of singular and plural forms, use of articles, word order, etc. Careful proofreading is recommended prior to resubmission.

Despite this number of comments, I think that the present work is interesting and might provide relevant insights into the spatio-temporal organization of seismicity preceding the 2009 L'Aquila earthquake. I am not yet convinced that similar observations also apply to other major earthquakes, but corresponding follow-up studies appear reasonable given the promising results of the present work. In this regard, I would warmly welcome a revised version of this manuscript that has addressed all points raised above.

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