Review report EK11761

The authors study effects of spacial embeddings on networks. Their contribution relies in proposing extensions of existing spatial or non-spatial network models and running several numerical studies to investigate their behavior. They also propose a spatial centrality measure as a candidate metric to measure the effect of spatial embedding in edge formation.

The authors fail to fully motivate me about the relevance of this contribution as there are no relevant examples on real data showcasing potential applications, which would make the paper stronger. However, the paper is clearly written and the problem is well presented and investigated, in particular in criticizing the model choices and assumptions that they make. I appreciate the honesty in showing that real networks show spatial centrality values that are higher than those obtained in their synthetic structures, thus highlighting that this measure, or the synthetic topologies, are missing some information that is contained in real data. It is thorough from their part not only to show this but also to point out several ideas to improve this measure (section V E).

Judging the scientific quality, I vote for accepting the paper.

There are minor corrections that I recommend:

- The spatial centrality as defined in (14) seems to reward hubs, i.e. nodes with many connections to, likely, small-degree nodes. In other words, $S(hub)$ should be big because of $K(hub)$ is small. In this case then, it seems to me that this measure is not capable of distinguishing spatial contributions, as the magnitude of S is due mainly to K. Perhaps add few lines in page 10 (where you discuss about neighbors of a hub) about this scenario, i.e. the S for a hub; in addition, for the hub-spoke network of figure 15, it would be helpful to see the values of S for the 3 hubs compared to the average values of the non-hub nodes, to see the interplay of these two contributions to the overall small S.

- The 'deterrence' function in the abstract was obscure to me the first time I read. It is a jargon only introduced later in the paper. Perhaps, for the abstract, think about another clearer name for that function.

- The explanation of why they consider Gaussian-distributed fitness (sec II B) it is not clear. They say that nodes have a variety of intrinsic factors that influences how they interact. Are you thinking about the central limit theorem or something similar? Please make a clearer statement.

- In Pag 8 end of sec IV A, they mention potential other types of spatial configuration models. For example, preserving A but randomizing the locations. I'm confused, isn't it this already part of their model? Meaning, that in their models it seems that locations are always chosen randomly anyway. Are you thinking about a case where locations are instead given a priori? (example as attributes). Please make this sentence more clear.

- Please add error bars on plots where necessary, e.g. when you run over 30 instances.

- Fig 12, why for the orange markers (config model SPA) there are less points? Is it because is more computationally intense? Please say it somewhere, perhaps in the caption.

Typos:

- Pag 1 beginning of introduction: In nature, such a space can be literal,..., or 'they' → 'it' can be ...;
- One line below: 'one can construe' → 'one can construct';
- Pag 8 left columns: 'One can also many' → 'One can also envision many' (or similar to envision);
- Same page and column but down below: 'the mean local ... and distance increases' → 'the mean local ... and distance increase';
- Pag 10 left columns: 'a peak in the mean ... strength reaches at' → 'a peak in the mean ... strength ~~reaches~~ at';
- Pag 12 right column: 'it sometimes is able to captures ... influencing' → 'it is sometime able to capture ... influence'.