MANUSCRIPT ID PA-2018-043 ENTITLED "INFERENTIAL APPROACHES FOR NETWORK ANALYSIS: AMEN FOR LATENT FACTOR MODELS"

Dear Professor Gill,

Thank you for the opportunity to revise and resubmit our manuscript. We believe the manuscript has benefited from the Reviewers' helpful and thoughtful comments. We have revised the manuscript, taking seriously each individual point raised by the Reviewers. The revision memo is organized by first responding to your comments and then addressing the reviewers' points. Our comments and responses are shown in *BLUE* below each point.

We hope you agree that the manuscript has improved through this process and we are looking forward to your response.

Sincerely,

The Authors.

Date: May 29, 2018 Version 0.01.

1. Reviewer 1

1.1. **Major Comments.**

(1) More justification is needed for the particular empirical application used here. While the use of the same study employed by Cranmer et al (2017) provides nice continuity for readers interested in inferential approaches to statistical network analysis, the dataset suffers from two notable issues with regard to a fair comparison between ERGM, LSM, and the proposed AME approach. The first is that each node's latent position is likely to be correlated with the model terms of interest – a point raised by Cranmer et al. as well (pp. 248-9). In this case, key government players have lower Euclidean distances because they tend to be positioned in the center of the latent space. If this were not the case, would we see such a large divergence between the LSM and AME results? For example, would the AME perform much "better" than the LSM if the network data exhibited a less hierarchial/more egalitarian structure?

- *Insert great response.*

(2) The second is that the data exhibit high reciprocity such that the directed network is effectively a symmetric undirected network. How would the results differ for a more evenly-balanced directed network (i.e. one where the probability of a reciprocal tie is 50%)?

- *Insert great response.*

(3) To address both issues, the authors could either describe in detail the merits and demerits of this empirical application for comparing AME to existing frameworks, or could draw up a simple simulation to show the sensitivity of the model comparison results to the factors identified above. Something along the lines of constructing a simulated network with known determinants of tie formation based on nodal characteristics could be helpful here (and could certainly be relegated to the appendix to stay within the word limits).

- *Insert great response.*

1.2. **Minor Comments.**

(1) It would be helpful for the reader to compare AME to non-network models as well, so the authors could include a column in Table 3 for results from a logit model (or the like).

- *Insert great response.*

(2) Typo on page 13, first sentence should read: $y = B'x - |u_i u_j|$ instead of $y = B'x + |u_i - u_j|$.

- *Insert great response.*

2. Reviewer 2

2.1. **Major Comments.**

(1) Would it be appropriate to incorporate time dependence in your model given the empirical setup.
   - *Insert great response.*

(2) Talking about inference and using non-experimental data as you do for example on country diads may be confusing to those who may expect a treatment effects estimator.
   - *Insert great response.*

(3) it could be useful to compare your model to the current work going on in modelling network interdependence in statistics, engineering and epidemiology, That may make your model more relevant to the broader field.
   - *Insert great response.*

(4) I also recommend including the estimation of the basic logit model as that is still the starting point for many approaching this field.
   - *Insert great response.*