Reviewer’s report

Title: How the Study of Online Collaborative Learning Can Guide Teachers and Predict Students' Performance in A Medical Course

Version: 0 Date: 25 Mar 2017

Reviewer: Robert A. Hanneman

Reviewer's report:

MEED-D-16-00778

How the Study of Social Structure Can Guide Course Design and Predict Student's Performance in a Medical Course

This paper shows that certain aspects of the structural location of individual medical students in on-line discussions are correlated with course performance, and that a combination of structural factors may predict course performance with a degree of accuracy that makes it, potentially, useful to monitor and perhaps manipulate structural locations to improve performance. An interesting and relevant data set is used, and the literature cited on structural effects on performance, and on network analysis more generally are solid and useful.

There are, however, a number of areas in which the paper might be stronger.

As currently written, the paper may be attempting too much. On one hand, a goal is to demonstrate a causal linkage between structural location in networks and individual performance. The article, however, also seeks to explain many network concepts and how/why these factors might be connected to performance. The article also seeks to provide a survey of the general uses of network analysis for description, prediction (perhaps to guide intervention), simulation, and social-structural engineering. Because it attempts so much, it is not (to this reader) sufficient in meeting any one of the goals.

I would suggest that much of the material explaining specific network concepts (e.g. the many variations of centrality measures) be simply dropped - relying on available good reference works.

The possible use of network analysis to identify at-risk students, design curriculum, and intervene/experiment are mentioned, but not developed. These ideas might have great value to the journal readers. They are not supported by data in the current experiment, but they might be discussed at greater length in the discussion/conclusion - so that readers might be stimulated to generate or monitor network data on their courses, and use it to improve teaching effectiveness.

There are a number of significant problems with the effort of the paper to effectively demonstrate that individual's position in the network matter in determining performance. At
present, the paper (may) succeed in demonstrating a correlation between position and performance - an important caveat to this, though, is that the statistical significance testing method is not explained, and may be incorrect. There are five main issues that need to be addressed in the effort to demonstrate a significant causal link between structural position in social networks and individual outcomes.

Endogeneity: it is plausible that a student's performance in the course is a cause of structural position, as well as the inverse. Students who perform very well at the beginning of the course (or who have a strong prior track record) may enjoy a "preferential attachment bias" (or "Matthew effect") such that they are likely to attract more social ties and become increasingly central as the network develops.

A dynamic simultaneous equation model of performance and outcomes would be called for to fully address this. Not possible in the current case, a control for performance at the beginning of the course (prior class standing, prior grades or test scores) might help. One could also see whether structural position at an earlier point in the development of the network was predictive of later position in the network.

Selection: Related to the above, actors with fixed attributes (e.g. gender) or varying scores on time-varying attributes (e.g. number of messages posted), may tend to form networks in different ways. Most notably, actors may differ (for both measured and unmeasured reasons) in their desire to seek ties, seek ties with central actors, or be seen as attractive targets of ties by others. These factors act as "network selection" biases. Notably, actors who are more likely to perform well because of differences in talent, prior learning, or social skills are likely to both form bigger networks and perform better in this course - independent of any additional effect of network structural position.

Diffusion/social influence: The current analysis looks at the structural location of actors in their ego networks and the total network. In addition, though, the attributes of the alters to whom an ego is connected may matter. Suppose two students are each connected to five others. Student A is connected to five high performing students; student B is connected to five poorly performing students. The performance of one's alters may set expectations and enhance or limit learning resources for ego. The point is, the author's might want to consider adding attributes of each ego's alters as predictive factors.

Lack of theory/hypotheses: The article asserts that there are benefits to enhancing performance of structural position in networks. The article does a good job of citing some relevant literature. But the article does not do much to provide theory of the supposed processes by which such effects may occur. Social influence, social support, access to resources, ability to broker and compete effectively, etc. could be developed more to make a more compelling case for why we should care about structural position and hence use it in designing curriculum and intervention.
Failure to control: Most literatures on variation in individual performance in a wide variety of contexts suggests that variation in individual attributes matter. They probably matter indirectly, by influencing network selection. They probably also matter directly, regardless of the structural position of actors in networks. The current analysis does not control for likely attribute effects (e.g. age, gender, prior experience, appearance, general sociability, and prior academic performance - and likely other things). As such, the failure to provide controls makes the case for structural effects suspect.

Some particular details:

Purpose of the paper (currently page 11) needs to be put right up front.

Both the literature review and the discussion of networks need to focus on predicting, and potentially intervening, to improve student performance. At present, there is little theory or hypothesis explicit. There need to be compelling logical arguments (theories) that suggest why network position (and what exactly about it) and network influences (and how they work) matter for performance above and beyond individual attributes factors.

7: 9-14. This claim is too broad. Most SNA perspectives argue that attributes and structural position are co-evolutionary and also somewhat independent. While fixed attributes may affect position in the network; dynamic attributes (e.g. behavior, attitudes) may be the result of position. The effects of fixed attributes (e.g. gender, age) may be both direct and indirect (by way of network selection).

7:42 among not between; node size proportional to "degree"; centrality has not been defined.

Centrality should be linked to the current problem: why or how does being central in various ways affect performance?

10: 18 "A network" not network. Weak discussion of degree distribution - needs precise definition and brief explanation of why one would care in the current problem context.

10 last paragraph of section identifies several additional important network properties, but gives only one word definitions and doesn't link these to the current problem.

Section 3 on simulation does not have a very clear definition of the processes and purposes of network simulation (simulations of action on the network? Or of the dynamics of the network?). Is simulation used in the current work? If not, perhaps drop this, too brief, section. If used, how is it applied to answer questions about variation in performance?

Section 4 on intervention is useful, but too brief. Several new implicit hypotheses are introduced here - suggesting things like poorly connected actors may be at higher risk of poor performance, and - if identified early - might have their networks re-engineered to reduce risk. But, the
hypotheses must be made explicit. Perhaps recommendations for interventions in the teaching context should be in the discussion/conclusion, and based on the findings.

11:20
12: 55 "moodle" and "LMS" are not clear.

13:42 insufficient description of the graphics algorithm. Is this similar to 2D MDS on a valued matrix, with spring embedding? What does it maximize? What are it's limitations for the current application?

13 bottom 14 top. Not all of these metrics have been explained or mentioned in prior discussions. Hypotheses should guide which metrics are examined, and the metrics should be given formal definitions either here, or in earlier sections.

14: The discussion thread level is interesting. Why was it used, in addition to individual and whole-network? Perhaps thread should be treated as a replication or blocking factor? Did discussions occur sequentially? If so, they might show how earlier network patterns affect later developments.

Throughout the whole paper, there is a tension between explaining the network analysis and its relevance at the general level, and presenting the current case. Perhaps the writing could, in each section, first give the general discussion, and then present the current case example as an illustration. Or, one could do a hypothesis testing exercise on the current data, and then use it to draw general lessons in discussion/conclusion.

P 15 and elsewhere: "total volume" What data were actually analyzed? Presence/absence of a directed/undirected posting? Were the number of words counted and a valued network used? Not clear. Needs to be clarified, and a rationale given.

Since the instructor plays a role in structuring the interaction, it might be useful to examine the data both with, and without the moderator. If the goal is to generate peer-to-peer interaction, the instructor should probably be removed.

P18

P27 26 The assertion is made that structural position has greater effect on outcomes than do individual attributes. This is not tested or demonstrated in the current paper. It would be nice to have node attributes such as demographics, academic record and experience, attitudes, etc. If such controls are available they should be used in the analysis. If not available, the claims here must be softened to only assert that network structural effects may provide additional insights.
Don't know what automatic linear regression is. If it is an automated variable selection technique, it is highly suspect, unless the results are validated in additional samples. Better to use simpler and less automated modeling techniques.

The significance testing of both correlations and regression coefficients is not explained. This is not an independent replications sample, so conventional estimations of standard errors cannot be used. Permutation methods for both correlation and regression effects testing are essential.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

No

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No

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