Author's response to reviews

Title: Physicians' propensity to collaborate and their attitude towards EBM: A cross-sectional study

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Author's response to reviews: see over
Dear Editor,

thank you for the invitation to revise and resubmit our paper titled “PHYSICIANS’ PROPENSITY TO COLLABORATE AND THEIR ATTITUDE TOWARDS EBM: A CROSS-SECTIONAL STUDY”. We believe that we have addressed the reviewers concerns and the editorial requests. Detailed point-to-point responses follow. All the requested documents are been accompanied electronically.

Yours sincerely,

Daniele Mascia

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Response to reviewers

Referee #1 (Phil J.M. Heiligers)

The subject of this study adds an interesting point in the discussion on organizational models in hospitals. The question is focused on the basic mechanism which directs the choice of formal and informal interpersonal ties: heterogeneity or homophily.

** We thank the referee for these comments.

Minor essential revisions

In general this is a thorough study, but on some points I have comments. In the section Research Methods under Variables

-you mentioned that you added variables based upon prior research. This argument is not appropriate, please argue about the relevance of those extra variables in your study;

** As suggested, we provided additional arguments for the inclusion of control variables in the present study (see Page 5, Lines 19-27).

-you present the variable ‘physician’s attitude towards EBM’ with a four-answer format: never – rarely-sometimes -often. Why is chosen for this subjective oriented answer, because you could have asked for the number of consulted publications.

** We agree with your attentive comment. We however preferred to include a specific issue in the survey questionnaire because the self-reported number of consulted publications could have been biased by the effect of alerting systems available within the surveyed organization. On the basis of single areas of specialty, staffed physicians receive periodically articles and other published materials about relevant evidences. Hence, the number of consulted publications would have not allowed us to fully grasp the physicians’ attitude towards EBM, who largely use the above mentioned system.

In the section Results your explanation and interpretation of the findings in Table 3 is very clear, but Table 3 is not clear at all:

-the table has no title in which the tested relationship is expressed;

-the difference between the dependent variable and the binarized dependent variable is not clear. Please, give in a note beneath the table some explanation.

** We thank you for these comments. We modified the Table 3 as requested.

In the section Discussion one point could be added:
The question whether homophily mechanisms are more likely to predict exchange of advice and information was conformed. But, the contribution of organizational affiliations is higher than the likelihood of a similar attitude towards EBM (see # and p-values). I think this point should be stressed in the discussion.

** As suggested, we stressed the importance of organizational affiliations of physicians in our study (see Page 11, Lines 2-4).
Mascia et al. present an analysis of physicians’ attitudes towards EBM and the presence of homophily in a social network comprising these physicians. The article is valuable because the authors seek to understand how relationships between physicians may influence their practice. In addition, the network size (297) is large relative to existing studies in the domain and the compliance rate (90%) suggests high reliability.

** We thank the referee for these comments.

Major Compulsory Revisions

1. There is currently not enough information presented about the construction of the network to allow for replication of the approach or detailed evaluation of the results. 87912 dyadic observations do not indicate the density (they represent the number of possible connections) and a reader will be unable to determine from what is written how weights were assigned to the relationships.

From visual inspection of Figure 1, it appears as though unreciprocated relationships were included in the construction of the adjacency matrix, which may possibly explain the abnormally high density. Homophily, like most other network metrics, is influenced by density and by degree distribution.

** To increase the possibility to conduct future replication of the approach used and a more detailed evaluation of the results, in the revised manuscript we provide additional information about the professional network. We produced a new Table which includes additional information such as density, % of reciprocated ties and % of symmetric pairs, etc. We used the “Univariate statistics” command in UCINET 6.232 to obtain such information for both the valued and binarized matrices. This Table (TABLE 2), shown below, is included in the revised version of the manuscript.

Table 2 – Univariate Statistics of the Professional network

<table>
<thead>
<tr>
<th></th>
<th>Professional Network</th>
<th>Professional Network (Binary) •</th>
</tr>
</thead>
<tbody>
<tr>
<td>Density (Mean) (%)</td>
<td>5.74</td>
<td>1.72</td>
</tr>
<tr>
<td>Std Dev</td>
<td>0.4685</td>
<td>0.1301</td>
</tr>
<tr>
<td>Sum</td>
<td>5,046</td>
<td>1,512</td>
</tr>
<tr>
<td>Variance</td>
<td>0.2195</td>
<td>0.0169</td>
</tr>
<tr>
<td>Minimum</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Maximum</td>
<td>5</td>
<td>1</td>
</tr>
<tr>
<td>Reciprocated ties (%)</td>
<td>61.43</td>
<td>61.43</td>
</tr>
<tr>
<td>Symmetric Pairs (%)</td>
<td>97.34</td>
<td>97.77</td>
</tr>
<tr>
<td>No of dyads</td>
<td>87,912</td>
<td>87,912</td>
</tr>
</tbody>
</table>

• Binarized network where $x'_{ij} = 1$ se $x_{ij} \geq 1$, and $x'_{ij} = 0$ otherwise.

On what concern reciprocity, the percentage of reciprocated ties was 61.43%. We conducted additional analyses to test whether our findings were conditioned by the asymmetric nature of the data used in the analysis. We symmetrized the professional network matrix on the basis of three different symmetrizing methods:
- the maximum method, which replaces $x_{ij}$ and $x_{ji}$ by $\max(x_{ij}, x_{ji}), i < j$;
- the minimum method, which replaces $x_{ij}$ and $x_{ji}$ by $\min(x_{ij}, x_{ji}), i < j$;
- the average method, which replaces $x_{ij}$ and $x_{ji}$ by $(x_{ij} + x_{ji})/2, i < j$.

This symmetrization was performed by using the UCINET 6.232 software package. Table 1R (not included in the paper) reports the density of these new matrices, labeled respectively SYMM_MAX, SYMM_MIN and SYMM_AVERAGE. We also computed, throughout “Degree centrality”, the degree distribution of all network nodes (not reported). Further analyses were conducted (see below). Overall, our findings are not significantly altered by the marginal difference in density and degree distribution.

Table 1R – Comparison of network univariate statistics

<table>
<thead>
<tr>
<th></th>
<th>Valued</th>
<th>Dichomized</th>
<th>SYMM - MAX</th>
<th>SYMM - MIN</th>
<th>SYMM - AVERAGE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Density (Mean)</td>
<td>0.0574</td>
<td>0.0172</td>
<td>0.0975</td>
<td>0.0173</td>
<td>0.0574</td>
</tr>
<tr>
<td>Std Dev</td>
<td>0.4685</td>
<td>0.1301</td>
<td>0.6142</td>
<td>0.2419</td>
<td>0.3822</td>
</tr>
<tr>
<td>Variance</td>
<td>0.2195</td>
<td>0.0169</td>
<td>0.3773</td>
<td>0.0585</td>
<td>0.1461</td>
</tr>
<tr>
<td>Euc Norm</td>
<td>138.0580</td>
<td>38.3797</td>
<td>181.9066</td>
<td>70.9225</td>
<td>113.0354</td>
</tr>
<tr>
<td>Minimum</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Maximum</td>
<td>5</td>
<td>1</td>
<td>5</td>
<td>5</td>
<td>3.5</td>
</tr>
<tr>
<td>N of Obs</td>
<td>87912</td>
<td>87912</td>
<td>87912</td>
<td>87912</td>
<td>87912</td>
</tr>
</tbody>
</table>

Minor Essential Revisions

1. Account for the choice of an undirected (symmetric adjacency matrix), which does not reflect the nature of the responses that were collected. In the survey, responders provided ordinal information about their relationships, and those relationships may not have been reciprocated.

** In the network surveyed the percentage of symmetric pairs was 97.34%. We performed additional MRQAP analyses taking into account as dependent variables the above described three different symmetrized networks. The results obtained were qualitatively similar to those reported in the manuscript. We do not want to overburden the referee with too many tables. These findings are available from the authors upon request.

2. Account for the potential for individual variation in answers amongst survey responders. Respondents view qualitative relationship answers in different ways and this is likely to influence their response.

** We agree with the referee. We have reported this limitation in the revised manuscript (see Page 11, Lines 30-32).

3. The opposite of homophily is heterophily, not heterogeneity (Page 11). Heterogeneity generally refers to the differences in personal characteristics within a group, whereas heterophily refers to the preference for creating links between individuals that are different.
4. Careful proof-reading is required. The writing is acceptable but the manuscript includes several errors. Instances of typographical errors include Page 5, Paragraph 2, Line 2; Page 5, Paragraph 2, Line 14, Page 5, Paragraph 3, Line 5, Page 6, Paragraph 1, Line 26 (adjacency matrix), Page 11, Paragraph 3, Line 1 (replace binarized).

** We modified the paper according to what suggested.

** A careful proof-reading has been conducted and all highlighted errors have been removed.

5. “A raft of papers ... [17]” “Although the literature has widely investigated the outcome of the formation of such communities (e.g. [8])”. These are unusual choices for citing what the authors suggest is a wide corpus of information on collaborative ties in healthcare organisations and their outcomes. There is a wealth of information about how communities are formed (in the vast literature on methods for modelling the growth of networks, outside of the domain of healthcare), and many recent examples of social network analysis being applied to communities within healthcare organisations. [17], although high-impact, is now 7 years old, and [8] is not representative of the others.

** We modified the paper accordingly (see Page 10, Lines 4-10).

Discretionary Revisions

1. What does the low value for the coefficient of determination imply? Would a different choice of construction (say, creating/changing a threshold in what determines a relationship in the binary estimation of the network) produce a higher value?

** In our opinion, the low value of the R square might be related to the omission of some variables such as individual attitudes, predispositions etc. that we have not investigated in our survey. We however performed separate analyses by using different thresholds to dichotomize our valued matrix. The coefficient of determination in all cases did not vary that much from those reported in the paper.

2. I am unable to determine how several confounders may have contributed to the homophily. For example, if a positive attitude towards EBM is closely correlated to the number of years since graduation, number of publications, or field of specialisation, then perhaps EBM is not causal in the formation of relationships. The authors have mentioned causality in the limitations section but it may be worth being more explicit about this issue.

** Thank you for this comment. We agree and in further analyses we will pay more attention to this issue, which might represent a topic of our interest for future studies. This issue is mentioned at the very end of our Discussion section.

3. I understand that quantitative literature on homophily is sparse but I feel that the authors have not done justice to the field of social network analysis and homophily. Perhaps identifying seminal examples of
homophily studies in the SNA domain (more recent than the McPherson et al. 2001 review) may provide context and provide guidance on the network properties that are essential in such a study.

** Taking into account what suggested, we added the following additional references:


This article is well written and of interest. The design of the study is well done. The introduction is concise and clearly laid out to the importance of the current study. The aim of the study is to assess whether physicians’ self-reported attitude towards EBM is related to the formation of inter-physician collaborative network ties. The use of SNA is appropriate.

**We thank the referee for these comments.**

However, I have several comments on this manuscript.

1/ Major Compulsory Revisions
- Although the method and result sections are well written, these sections should be shortened, especially the section on survey instrument (1 page) / variables (2 pages) and statistical analysis (1 page). An effort of synthesis would be appreciated. I also suggest grouping the survey instrument and variables sections. In the same way, the result section is frequently redundant between the text and the tables.

**We modified the text according to what the referee has suggested. We have substantially reworked the “Survey instruments and variables” section (see Pages 4-6) and the “Results” section (see Pages 7-9).**

I am not sure that the following sentences are results:
"In the first column of the table estimates are produced for the dependent variable represented by the valued professional network consisting of the complex web of collaborative ties among physicians. In the second column of the table the results of the MR-QAP in which the dependent variable was the binarized professional network are reported. A binary version of the professional network matrix was used in order to learn whether homophily in terms of physicians’ attitude towards EBM also has an effect on the existence of an inter-physician relation. Table 3 reports standardized coefficients for the parameter estimates, which allows us to answer the question which of the independent variables have a greater effect on the dependent variable.”

**Taking into account the referee’s suggestions, we moved almost all the sentences above reported in the methodological section.**

- Response rate for the social network analysis is potentially an important bias in this study. If the global response rate is high, this is not the case for the oncology clinical directorates. Can the authors discuss this point (may be to add in the limitation section).

**As suggested, we added a caveat in the limitation section (see Page 12, Lines 1-5).**

- The seniority in the hospital is also an important characteristic that should be included in the analysis.

**In the revised manuscript, we further clarify that seniority in our study is represented by two variables, namely “Tenure NHS” and “Tenure LHA”. We did not discussed thoroughly this issue since both the variables were not significantly associated with the dependent variable.**

- The authors should indicate in the statistical analysis the cut-off for the significance (p < 0.10 or p<0.05). It looks strange for me to choose p=0.10.
**In the revised text, in the methodology section, we clarified that the cut-off significant value is p <0.05. Coherently, we modified Table 3 (which in the revised manuscript is labeled Table 4).**

- This sentence is not clear: as shown in Table 3, results were significant when the binarized network, rather than the valued professional network, was considered as dependent variable.” The results are relatively concordant?

**We thank you for this suggestion. We modified the text to increase clarity and readability (see Page 9, Lines 29-31).

- It is not clear for me why “The negative sign of the parameters coupled with these covariates should be interpreted as a tendency toward homophily on the specific characteristic represented by the covariate. First, these results indicate that physicians who are similar with respect to the number of years since their degree are more likely to cooperate (# = -0.0318; p<0.01). Specifically, those who publish in a similar manner, defined in terms of number of papers published in peer-reviewed journals, are more likely to exchange information and advice (# = -0.0167; p<0.05).” Concerning age since graduation, I would say that professional network is less important when years since graduation are higher. Can we conclude from this result that physicians who are similar with respect to the number of years since their degree are more likely to cooperate? I am not sure. Complementary analysis would be necessary to confirm the hypothesis of the authors.

In the same way, concerning the number of publication, I would say that professional network is less important when the number of publications is higher. Complementary analysis would be necessary to confirm the hypothesis of the authors.

Same comment for the managerial role.

Considering this last comment, several part of the discussion/hypothesis on these results should be rewritten.

**The above comments were grouped since they concern the same, methodological topic of the paper. Probably the above interpretation of the findings reported by the referee is related to a lack of clarity in the presentation of the methodology.**

In the revised manuscript we provided better clarification of the procedures we adopted to transform attributional covariates (age, gender, affiliation to hospitals etc.) into dyadic variables. We reworked the methodology section of the paper. We clarified that, in light of the relational nature of the dependent variable, an appropriate transformation of all independent variables was needed (see Pages 6-7).

We also added the words “difference in” and “same category/affiliation” besides the labels of the different variables in Table 4 to facilitate the interpretation of our findings. The word “difference in” was used to recall that, for all continuous variables, a transformational procedure was adopted to convert the attributional covariates in dyadic variables using the absolute difference criterion. The label “same category/affiliation” represents the transformational procedure that we instead used to transform covariates representing binary/categorical attributes into dyadic covariates.

In light of your suggestions we revised our comments in the Results (see Pages 8-9, Lines) and Discussion (see Page 11, Lines 4-21) sections.
We hope that we have correctly interpreted the reviewer’s request. Should this not be the case, we ask that the reviewer re-formulates this particular comment.

2/ Minor Essential Revisions

- The study was conducted from February to November (10 months). Can the authors justify the length of the study?

** We clarified this issue in the methodology section (see Page 3, Lines 15-17).

- I don’t understand why clinical directorates which concerned Neuroscience, Oncology and maternal health included all the physicians. These 3 directorates don’t concern all specialities.

** Interdisciplinary units or directorates represent adequate contexts to investigate how either homophily or heterophily might influence the propensity of clinicians to collaborate. All physicians staffed within multidisciplinary directorates have been studied in this paper.