Reviewer’s report

Title: Clinicians Can Independently Predict 30-day Hospital Readmissions as Well as the LACE index

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Reviewer: Maria Pia Fantini

Reviewer’s report:

The study by Miller et al. aims to assess providers' ability to predict 30-day hospital readmissions, and to compare their predictive ability with that of the LACE index, one of the most recognized and utilized prediction tools of hospital readmissions. The study, similar to Allaudeen’s 2011 paper, is in some ways "rough" and needs further confirmatory analyses, but the topic is interesting—is the LACE index a valid predictive tool? Does providers' experience "defeat" the performance of standard prediction models? However, the authors reach a conclusion that I only partially agree with. Also, there are some methodological issues that need to be fixed.

Major Compulsory Revisions

1. Lines 55-59: Could you please specify the range of LACE scores? In some countries, especially in Europe, this index is still not widely known nor utilized.

2. Lines 86-88: Using a different scale from that of LACE to assess the providers' predictive ability could have partially biased the results. I think it would have been more sensible to ensure an identical evaluation scale—different survey scales lead to different answers. Maybe the authors opted for a continuous percentage scale in order to facilitate creating ROC curves, but this choice might have "penalized" the LACE index, as it is less detailed (0 to 19/20 vs. 0 to 100 score). These aspects should be at least acknowledged among the limitations of the study.

3. Lines 110-111: Ideally, to ensure independence between evaluations the authors should have interviewed 377×4 providers (377 residents, 377 nurses, 377 attendings and 377 case managers, one for each patient), but I am well aware that this is impossible. Still, it is worth noticing that the authors have assessed the predictive ability of a restricted number of attendings, residents and, most importantly, case managers. I would not rely on evaluations made by such a little bunch of individuals: six case managers cannot be representative of the category of case managers overall, and results may be heavily unbalanced because of the answers of one or two individuals. Thus, I suggest that the authors delete all analyses related to case managers.
4. Lines 123-124: The authors state that they estimated 95% confidence intervals (CIs) for AUCs, but there is no sign of them in the paper. As the sample is not large, I recommend computing exact binomial CIs, rather than asymptotic normal CIs.

5. Lines 127-129: Please specify what subgroups you are referring to (poor understanding of disease, poor adherence to therapy, etc.).

6. Line 136: Could you please summarize patients' diagnoses and, if any, interventions? This would ease reproducibility and comparability of your results.

7. Lines 152-154: It is risky to make considerations on power and sample size after analyses are concluded. The authors should have determined the sample size during the study design process. I am prone to think that all tests failed to achieve statistical significance because there was no difference in predictive power between LACE index and providers as well as among providers themselves. Let's consider, for example, the AUCs of LACE index (0.620) and nurses (0.628): the difference is really negligible. Even the difference between LACE and attendants is very low (~0.07). To me, providers and LACE index perform equally.

8. Table 3: Could you please provide the number of patients belonging to each subgroup?

9. Lines 184-186, 197-202: The authors' statement is not supported by results. Both providers and LACE index exhibited a poor ability in predicting 30-day unplanned readmissions, as the AUCs are always below 0.7 (except for the subgroup of patients with poor understanding of disease). The authors can assert that providers perform better than chance only if 95% CIs of AUCs (not presented in the paper) do not cross 0.5. Results are undoubtedly better than Allaudeen's, but more inferences are needed. Moreover, saying that providers perform "possibly better than the LACE index" is misleading: providers' AUCs are higher than LACE's, but not significantly higher.

10. Lines 228-232: This statement too is not supported by results. How can clinician expertise be paramount, if the LACE index performs as well as (actually, as bad as) providers in assessing the readmission risk of patients? I interpret all the results in a different way: the LACE index performs badly and providers do not seem to do better. In order to make readmission models and tools more useful, efforts are needed to identify variables that may play an important role in predicting this outcome. These variables may include understanding of disease and drug adherence, as also suggested by the authors, as well as quality of care and organizational structure and processes that were not evaluated in this study. Some authors (Lenzi et al. BMC Health Serv Res 2016, 16:473) also found that low systolic blood pressure levels are highly predictive of 30-day readmission in patients with heart failure.

In conclusion, I suggest that the authors rearrange the discussion section in light of these thoughts. As it is, this paper draw a conclusion that is not consistent with the results: 30-day readmissions are still difficult to predict for both providers and published algorithms. Efforts
should be done to increase the accuracy and usefulness of readmission prediction tools to help and guide providers adopt tailored strategies for patients at high risk of readmission.

Minor Essential Revisions

1. Lines 118-131: I suggest that the authors carry out a sub-analysis whose results might be very useful and informative. I would investigate the association between readmission and the eight risk factors (poor understanding, poor adherence, etc.) using logistic regression models. Given the small sample size, each of the eight regressor could be analyzed separately. Result of this sub-analysis would help understand which factors determine hospital readmissions, guide further research and implement new prediction tools.

2. Lines 267-287: The study might have another limitation. The "absence" of a specific risk factor, such as poor social support, may reflect two situations: (1) the patient does not have that risk factor or (2) the provider does not know whether the patient has that risk factor. This could have partially biased the results, because answering "No" could mean both "No" and "Unknown".

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

Yes

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

Yes

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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I am able to assess the statistics

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