Reviewer’s report

Title: To what extent is multi-morbidity associated with new onset depression in patients attending cardiac rehabilitation?

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Reviewer: Arash Harzand

Reviewer's report:

I applaud the authors for addressing what is a highly relevant and important contemporary issue regarding the role that depression plays among participants with heart disease attempting to participate in cardiac rehabilitation, and for their willingness to undertake an analysis of a very large and rich data set in order to answer their specific objective to characterize the comorbidity profile and characteristics associated with new onset depression in patients initially attending CR.

I have several concerns, however, primarily with their baseline analyses. Although the statistical methods appear to be well done, I believe their conclusions may be somewhat overstated. Although many of the variables assessed were more commonly seen in patients with a HADS &gt; 8 as evidenced by the P-values, with the exception of the HADS anxiety score, the clinical significance of these differences is somewhat unclear as the subjective differences don't appear that large (i.e. a mean age of 64 vs. 66, 2.6 vs. 2.3 comorbidities, BMI of 29 vs. 28). This is reflected in the mostly small effect sizes they report (Cohen's d are all &lt; 0.20 in Table 1, with exception of HADS Anxiety Score Measurement). Similar patterns are seen in Tables 2 and 3. Although their linear regression model sheds some additional light and highlights the importance of several factors (namely HADS anxiety score, exercise, male gender and diabetes all of which have an odds ratio of 1.2 or greater), the OR for total number of comorbidities remains subjectively small and clinically unclear despite it's significant p-vale.

Overall although I believe their manuscript sheds important light on the characteristics of patients with new onset depression who enroll in CR, I would reframe their ultimate conclusions to highlight that these findings are more likely thought provoking and hypothesis generating at this point. Their summary and conclusions could also benefit from a more open discussion around the small effect sizes and odds ratios seen, instead of simply generally stating that patients with new onset depression overall have a higher number of comorbid conditions because, although that may be true from a purely statistical standpoint, in a practical sense I'm not sure if that's the case.
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?  
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Yes

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I am a consultant and minority shareholder for Moving Analytics, Inc, a company which develops a digital platform for home-based cardiac rehab delivery.

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