Reviewer's report

Title: Physical and mental health comorbidity in adults with intellectual disabilities: population-based cross-sectional analysis

Version: 1  Date: 8 March 2015

Reviewer: Mary McCarron

Reviewer's report:

This is a very important and indeed timely paper which address some critical issues in the field of ID.

Overall, the analysis the paper has done seems appropriate and correct. They refer to two papers (Smith et al. 2013 and Court et al. 2014) both of which have used very similar methodology. The sample size is very large which is great and this is reflected by the low standard errors and narrow CIs for the odds ratios. The following comments may help to improve the paper further.

1. Propensity score matching (matching on age, gender and deprivation levels) perhaps could have been used? Rosenbaum and Rubin (1983) showed that treated and untreated subjects with the same propensity scores have identical distributions for all baseline variables. This would result in a lower overall sample size but would address the issue of imbalance in these key identified demographics. Interested in at might be gained or lost from this,? This maybe useful but not necessarily essential.

2. Very short paragraphs were used including single sentence paragraphs (page 7 for example). In several places no comma was placed between the ORs and the CIs making it harder for the reader (e.g. OR 1.48 95% CI 1.41-1.55 (page 7)).

3. Personally I wasn't a fan of how the p-values in tables 2-4 were presented (although this is consistent with Court et al 2014). Either use a format such as * for P # 0.05, ** for P # 0.01 and *** for P # 0.001 or report all p-value for consistency.

4. Also include a foot note at the end of these tables indicating when the t-test or ANOVA were used etc. Tables should be able to be interpreted as stand alone pieces but these don't give any indication of what test was used etc.

5. The discussion should address what this study confirms from prior reports (e.g., Heslop, McCarron, Evenhuis) on morbidity differences and contributions to earlier death. It would be helpful to offer some insights into the level of assessment to establish both level of ID and of the different conditions and any data on the validity and reliability of the diagnoses reported in this dataset

6. The measure of deprivation poses a number of concerns. Certainly if people with ID are living independently or with family there postcode deprivation is possible but those living in supervised setting regardless of address likely have a similar and somewhat affluent lifestyle. Perhaps this cannot be ascertained from
the dataset and this issue should be discussed more fully as a limitation.

7. It would be helpful to state earlier in the manuscript that the prevalence rate of ID in this sample is similar to other Scottish reports.

8. The references also need to be checked. For example Court et al 2014 [15] the title of the paper is incorrect.

Recommendation: accept with minor revision.

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I do not have any competing interests.