Author's response to reviews

Title: The utility of the Historical Clinical Risk -20 Scale as a predictor of outcomes in decisions to transfer patients from high to lower levels of security-A UK perspective.

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Author's response to reviews: see over
Re: The utility of the Historical Clinical Risk -20 Scale as a predictor of outcomes in decisions to transfer patients from high to lower levels of security-A UK perspective.

Dear Sir/Madam

Please find attached the final revised version of the above manuscript for publication in *BMC Psychiatry*. As suggested in your email of 10th August 2010, we have read through the text carefully and corrected any mistakes.

Yours Sincerely

[Signature]

Professor Mairead Dolan
Review 1: Major essential revisions
1. The introduction is under-developed. The authors should provide some notes on the scope of the problem. Why is this study interesting? What is the contribution of mental illness to crime?
   Each of these points have now been addressed
   2. The introduction does not present the concepts around violence prediction, and presents only the few England/Wales studies. Some mention should be made of studies from around the world, including other countries in Europe and the USA.
   This has now been addressed
   3. In the discussion, the authors should go beyond the local interest, and consider how the findings may be of interest internationally, and compare with other risk measures, such as psychopathy, and other types of risk indicators.
   This has now been addressed

Review 1: Minor essential revisions:
4. The group of patients is referred to as a sample. However, in my understanding of the term, a "sample" refers to a subset of a population that is deliberately sampled to represent the population. This group is better referred to as the study group or cohort.
   This has been changed
5. A case in point is that the study group differs from other study groups from England & Wales as described in the discussion.
   This statement is actually not correct- we point out that they are similar to patients in other services.
6. While I understand from the methods section that the patients in the unit are all there as a result of a criminal justice process, I would like to know of the process in England and Wales, at least in brief terms. Is the hospital stay a criminal sanction? What kind of legislation supports the hospitalization of these patients? What kind of criteria are endorsed to decide if a person is a high-risk psychiatric patient or a criminal who can be sent to prison? This section can well be concise, but it would make my understanding of the patients clearer.
   This has now been addressed as suggested.
7. The description of the HCR-20 could be expanded somewhat, given that there is no word-limit in this journal.
   We have now included the item content
8. No description is given of the process to diagnose mental illness at the hospital. I take it that the patients were diagnosed without a standardized procedure, and that the reliability and validity of the diagnoses has not been studied for this particular hospital. While this is not essential for the validity of this study, it is never the less and important limitation.
   We have now addressed this issue in the limitations
9. A case in point is that a maximum of 62% had a history of alcohol or drug problems according to table 1 (31%+31%). With my limited experience with the kind of patients described in the study, I suspect that this may be an under-estimation.
   We have commented on this issue now
10. Also, the term "primary personality disorder" is not logical given the multi-axial system, used in DSM-IV. The clinical judgment that is the basis of this diagnosis should be described.
   The authors should describe whatever can be said of the diagnostic procedure at the hospital. This has now been stated.
11. The HCR-20 may be confounded with, for instance, age. While regression analysis with such a small sample is likely to lead to a high risk of type II error, the authors should correlates of the HCR-20 that are also likely to be indicators of risk.
   We were unclear what they meant by this but there is no relationship between HCR-20 score and age in this cohort.
12. The limitations section must include a number of other issues, including representativity and the lack of other potential predictors for comparison.
   We have now expanded the limitations section to address this issue.

Reviewer 2
13. Discussion and conclusion need revision and rewriting. The discussion starts off with a rather long introduction, before the actual discussion begins. It is advised to look at the beginning, and see what is necessary, and what could maybe be placed somewhere else.
   We have now edited this section and changed the order as suggested. We have also cut out elements that are less essential.
14. Also the conclusion needs rewriting. The start is unclear, partly because of writing mistakes, and needs clarification. It is not clear what the co-morbidity concerns in the actual article, neither in table 1.
   This has now been addressed.
15. Table 2: include percentage beside description of numbers.
   This has now been added.