Reviewer's report

Title: Does psychopathology at admission predict the length of inpatient stay in psychiatry? Implications for financing psychiatric services

Version: 2 Date: 4 May 2011

Reviewer: Shofiqul Islam

Reviewer's report:

Thank you for submitting the substantially revised version of the paper. Current version seems to be acceptable for publication with some minor revision. Please see my response below marked with ** after each section of authors response and few additional comment at the end.

Author's response to reviews

Title: Does psychopathology at admission predict the length of inpatient stay in psychiatry? Implications for financing psychiatric services

Authors:
Ingeborg Warnke (iwarnke@dgsp.uzh.ch)
Wulf Rössler (roessler@dgsp.uzh.ch)
Uwe Herwig (uwe.herwig@puk.zh.ch)

Version: 2 Date: 24 March 2011

Reviewer SI:

Comment: The author made a reasonable attempt in identifying the factors associated with the length of stay at hospital (LOS). They used a hospital based administrative database (psychiatric patient at admission) for this purpose. Length of stay in the hospital mainly depend on the assessment done at admission as well as successive assessment done during their stay in hospital. It is not very clear from the discussion if any of those factors used in the analysis includes assessment during the stay at hospital and the degree of severity in each.

Response: Thank you, this information is now presented more clearly in the methods section (p. 8) and discussion (p. 15). We performed an assessment of psychopathology and its
degree, of the severity of illness and of the global assessment of function at the beginning of the stay in hospital. All other variables (gender, age, work, housing etc. can be seen) as stable variables that were collected in the early period of inpatient stay. Treatment was coded at the end of hospital stay but we assume that physicians know about it in the early phase of treatment. Further, treatment was not our primary focus. We also mentioned severity of illness at discharge in table 2.

**Thank you.

Comment: I agree with the findings that selected factors couldn't explain much of variation in LOS. However, the conclusion based on this finding seems inappropriate as quoted below: “Therefore, classifying patients on the basis of those measures is obviously inappropriate with respect to financing inpatient psychiatry.” Even if we had high amount of variation explained by those factors, not sure how we could classify patients on the basis of those measures that could help us controlling resources use in terms of LOS. This need to be clearly justified in the methods section or this is beyond the scope of this paper?

Response: We agree. Indeed, because we did not analyse a classification procedure, we changed this conclusion (p. 2).

**Thank you.

Comment: Also data used in this analysis collected from a specific hospital in Zurich and need to be clearly described as a limitation in terms of generalizability of their findings.

Response: Again we agree and we consider and discuss this point in the limitations (see p. 16).

**Thank you. However, in the last sentence in page 16 the author stated that "Thus, it might be considered performing such investigations in other countries with other health care system". I am wondering if it would be more appropriate to say, "This result might not be appropriate to generalize for other countries or in different health care system". 
Comment: Since the distribution of the outcome (LOS) is much skewed, statistical approaches taken for modelling is appropriate (taking the logarithm and exponentiating them back for point estimate and confidence interval). However, summarizing relevant informations seems inadequate. Following suggestions need to be considered:

Comment: 1. Scoring system for different syndrome need to be clarified more in the methods section with at-least one clear example. As quoted from the paper, “The severity of symptoms ranged between 0 = no symptom to 3 = severe symptoms”, does this mean severity of symptom could have any values between 0 and 3 or simply 0, 1, 2, 3? On the other hand, symptoms clearly described as categorical. Adding and averaging them requires a ratio scale assumption and need to be justified for those scores. 2) Other option is to consider them as categorical variables (with three categories) or simplify them to binary variable (yes vs no) and include them in the model. However, this will require fully revised analysis of the paper.

Response: The psychopathology scoring represents a grading with ordinal variables from 0 to 3. Taking for instance “delusion” as an example, 0 would mean no delusional symptoms, 1 slight symptoms not influencing actions, 2 prominent symptoms sometimes influencing actions but with possible distancing, 3 strong symptoms that guide the person. We have now grouped 1 and 2 / 2 and 3 together and present this new categorical analysis.

**Thank you. Perhaps Odds Ratio ratios are easier to interpret while describing the association between LOS and other factors. However, R square presented from linear regression is more precise to identify the amount of variation in loss explained by other regressors.

Comment: 2. Inclusion of Figure 1 need to be clearly justified with sum of score vs LOS. This Figure is useful if we are fitting a model with LOS vs combined score. Specially since
the scatter plot showing a negative correlation but the regression line showing positive slope which is misleading. Need to be checked and reported a fitted regression coefficients with the plot.

Response: We agree and now changed this figure by demonstrating exemplarily the distribution of LOS concerning the apathetic syndrome, which was significant in the final multivariate models.

** Thank You.

Comment: 3. Statistical analysis section indicated the use of Chi-square test and Mann-Whitney U test. Please clarify where you used this approach.

Response: We have mentioned this now at the bottom of the respective tables (1 and 2).

**Thank You.

Comment: 4. The term “hierarchical stepwise linear regression (forward method)”, need to be replaced by “liner regression with forward selection procedure”.

Response: Thank you, we corrected this.

**Thank You.

Comment: 5. In the result section the total sample indicated as 5660 but in Table I it appears as 3220, please clarify and the reason. A comparison between those included in the analysis vs those not would be more useful.

Response: We differentiated between total, excluded and final sample. The total sample assessed for eligibility includes all hospital admissions between 2008 and 2009, n=5660 (respectively 5,224 because we first mistakenly considered episodes with stays between 1 and 180 days; now between 3 and 180 days), whereas the final sample of 3220 patients were considered for multivariate analyses on AMDP (due to complete data). Following the reviewers suggestion, we now compared both, the included and the excluded sample, in terms of basic patient characteristics. In
this course, we see that relatively more psycho-geriatric patients were excluded which is an important factor that leads to differences between these samples as described in the results section, whenever the proportion of diagnoses was still of a comparable magnitude. Of course, this might raise at first glance the question whether the sample included is representative. Certainly, it can be seen as representative for the case mix investigated. However, we consider the assessed question on psychopathology and LOS as a basic one that should be independent of the exact sample characteristics. Of course, the local treatment culture may influence general aspects of the LOS, but the question is here whether psychopathology, as a descriptive, basic feature of the individual patient within this systems explains the variance of LOS. Thus, aspects as for instance a different case mix within a certain range should not primarily influence the general relation between psychopathology and LOS. However, addressing this issue, we emphasize in the manuscript that our results are to be regarded as valid for a comparable case mix.

We also changed the sample size of the sample finally considered for multivariate analysis on several predictors because we only considered variables that were significant in the final models (and not those that were excluded). This led to a sample size of N = 2939 (vs. 2734 in the previous version of the manuscript).

**Thank you. Now the numbers are consistent with clear explanation.**

Comment: 6. Table 2, 3 and 4 need to be consistently used same variables with exactly same labels or combined into one tables for comparability in different models.

Response: Thank you, we revised the tables accordingly.

**Thank you, table looks much better and comparable now.**

Comment: 7. Table 4 and 5 can be combined together to save space.
Response: Thank you, this has been taken on board.

**Thank you.

Comment: 8. Table 5, row 7, “,” need to be replaced by “.”

Response: Thank you, this is done now.

**Thank you.

Comment: 9. It would be interesting to see the effect of gender in the model or else need to be justified.

Response: Gender had no significant effect. It did not remain in the final statistical models throughout the linear regression. We now mention this in the results section (p. 12).

**Thank you.

Comment: 10. Software used in this analysis need to be mentioned.

Response: We used the SPSS software package as now mentioned in the methods section on page 10.

**Thank you.

Comment: 11. It would be interesting to see those proportions in Table 1 by binary outcome categories (Below median LOS vs above median LOS). As well as fitting a logisting regression model and reporting Odds Ratios associated with each syndrome (binary: Yes vs No).

Response: We conducted the respective analyses and found results comparable to the linear regression analysis. They are now added to the manuscript in table 2 and table 4 as well as on p. 9/10.

**Thank you. Results are comparable and radars could interpret the coefficients of logistic regression easily with simplified binary LOS and its association with other exposures as an Appendix table. It would be useful to present the same for Model 5 and 6 as well (in Table 4).

Additional comment: The coefficient of determination (R square) can be interpreted as "the amount of variation in outcome explained by the set regressor" but this is not the "Variance". It would be more appropriate to change the word "Variance" throughout the text with "variation" while referring R square.

Thank you for the opportunity to review your manuscript. Good luck with your publication.
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests.