Author's response to reviews

Title: A predictive score to identify hospitalized patients requiring discharge to a post-acute care facility

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Author's response to reviews: see over
Dear Editors,

Thank you very much for your mail of October 25, 2007. We revised the original manuscript along the lines that you and the reviewers suggested, and performed the additional analyses requested. Please find enclosed detailed answers addressing the concerns and the questions that were raised by the reviewers.

Responses to reviewer Alan Tennant

1) There are some problems with the samples, viz. their adequacy for the analytical procedures (logistic regression); particularly that related to their day 1 analysis (where there are relatively few designated for PAC). Power calculations should be provided to demonstrated that their study has adequate power for each logistic regression.

We wonder if we correctly presented our results which, therefore, were misunderstood by the reviewer. Day 1 model was built on the whole cohort (349 patients) and aimed at identifying variables that were associated with a transfer to a post acute care facility occurring at the end of hospital stay and not between day 1 and day 3. Therefore, the total number of events is 104, and we agree that our study would have been greatly underpowered if the goal of the day 1 model was to predict a transfer to a post-acute care facility between day 1 and day 3, with only 9 events. Day 3 model was built on the patients who were still hospitalized on day 3 (299 patients) and it was aimed at identifying variables that were associated with a transfer to a post acute care facility occurring at the end of hospital stay. The total number of events was 95.

The rule of thumb for multivariate modeling is that there should be 5-10 observations per variable in the smaller group. Thus, we were in good position to model 10-20 variables, which was what we did.

Furthermore, a demonstration of sufficient power is necessary when a study yields a non-significant result and type 2 error must be considered. When significant associations are found, as was the case here, then the study obviously had the power to uncover them.

To avoid misunderstandings and misinterpretations of our results, we modified and clarified the presentation of the results of each model on the revised version of our manuscript under the results section (p 8-9).

2) The authors also fail to provide evidence that the logistic regression results are valid. Appropriate fit statistics should be reported, and possible the proportion correctly allocated by the model.

C-statistics (i.e., areas under the ROC curve) provide an indication of model fit. These statistics were high (>0.8) for both models. We also compared the ability of the 2 models to correctly allocate patients. The proportion of patients correctly allocated reached 78.5% for the Day-1 model and 77.9% for the Day-3 model. In addition, we performed the Hosmer-Lemeshow goodness of fit test with 8 degrees of freedom on both day 1 and day 3 models. P values were respectively 0.37 and 0.21, indicating good fit for each model. These values were added to the result section (p. 9 and 10).

3) My main concern is the failure of the authors to indicate the true extent of the misclassification of patients using their algorithm. They do state that the
proportion correctly classified is 71%, and also report sensitivity and specificity, but in a setting where large numbers of patients are passing through the system, then the number of false positives is of prime concern. If the authors had reported the positive predictive value of their screening test, they would have found it to be 53%. Consequently, for each person they identify as requiring PAC who subsequently required PAC, they would identify another person who subsequently would not require PAC. The number of false positive almost equals the number of true positives. This omission, and the failure to discuss the implication of this for discharge planning, represents a major weakness.

We agree with the reviewer that we omitted to indicate explicitly the true extent of the misclassification of patients using our algorithm, the reasons why we choose for our algorithm a cut-point of 8 or more, and its clinical implication in term of discharge planning policy.

At a cut-point of 8 or more, the score correctly classified 71% of the patients discharged to a PAC facility and predicted the risk of being discharged to a PAC facility with a sensitivity of 87%, a specificity of 63%, a positive predictive value of 53% and a negative predictive value of 91%. At a cut-point of 16 or more, the score correctly classified only 42% of the patients discharged to a PAC facility and predicted this outcome with a sensitivity of 42%, a specificity of 95%, a positive predictive value of 80% and a negative predictive value of 78%. Detailed predictive values (i.e., proportion transferred) at all levels of the score appear in table 4, first column. We chose a cut-point of 8 and more in an attempt to maximize both sensitivity and specificity. Indeed, our goal was to identify early patients at risk of not returning home in order to take appropriate actions for these patients, since the major cause of discharge delay in our service was waiting for a PAC facility. Therefore, we were more willing to accept a higher number of false positives (i.e patients identified as requiring PAC facility who subsequently would not require it) than a higher number of false negatives, (i.e. patients not identified as requiring PAC facility, who subsequently would require it). Early identification of those patients at risk of not returning home can act as an alarm and prompt the house staff to better assess patients' needs as well as discharge destination, and to engage specialized discharge planning services in a more timely and appropriate manner, potentially reducing the risk of subsequent transfer to a PAC facility. Applying such a discharge planning strategy could reduce patients' inappropriate hospital use without unduly increasing patient’s transfer to a PAC facility.

The true extent of the misclassifications of patients using our algorithm and the reasons for choosing this cut-point and are now presented in the method section (p 10-11) and a new paragraph concerning this problem has been added in the discussion (p 15).

We also analyzed in the derivation sample the characteristics of the patients misclassified by our algorithm and showed that false positive patients were younger and had less disabilities in ADLs and IADLs than true positive ones. Other variables were similar. This analysis has been now added to the results sections (p 11).

4) Finally, there are other studies with similar objectives, published in BMC, which the authors have failed to include and comment upon. They may wish to consider the relevance of such papers to their own work.

We thank the reviewer for this comment. We have now added and commented in our discussion (p 14) two recent studies with quite similar objectives, one published by Slade A et al, BMC Health Serv Res 2006;6:31 and the other one by Holland D et al, Nurs Res 2006,55:62-71.
Responses to reviewer Robert L Kane

1) The authors need to make clear the nature of their sample and its implications for the results. They are discriminating often among young and old patients.

Our sample included patients hospitalized for a medical problem, with a mean age of 65 years, who were cognitively intact, who had a comorbidity index of about 2, limitations in about 2 activities of daily living, and who were taking about 4-5 medications on average before admission to hospital. Our predictive model may reasonably be expected to perform well in similar samples, but not in patient populations of a different type, such as younger or very elderly patients, surgical or orthopaedic patients, or patients with more impaired cognition. In addition, our findings may differ from those of hospitals with different discharge planning decision-making or models. Because of these limitations, use of the score to other settings will require local validation.

The results section has now been modified in order to clarify the nature of our sample (p 8 and 9) and its implication for the results has been added in the discussion as a limitation (p 16).

2) The study does not distinguish post-acute care at home from in an institution.

We choose as our main outcome variable the patient’s discharge destination, i.e. whether the patient was discharged to home or to a PAC facility. In this study indeed, as the most frequent cause for discharge delays in our service was waiting for a bed in a PAC facility, we were concerned with discriminating patients unable to return home and requiring transfer to a PAC facility from those returning home with (or without) formal (or informal) help. Therefore, we did not consider referral to home care as a PAC destination. This explanation has been added now to the method section in order to better define our outcome. (p 5)

3) What was the basis for assigning points? (p8)

As seen in table 3, points were assigned to each significant element of the day 3 model in proportion of the value of the regression coefficients. For the variable number of medical active problem, a continuous one, the regression coefficient yield 0.24. Since all other regression coefficients have a value around 1, we decided to give them a value 4 times the value of the coefficient associated with active medical problems. Therefore 1 point was attributed for every additional active medical problem and 4 points for every other variable. The basis for assigning points has now been added in the method section, p 10.

4) The discussion should address why cognition was not more significant. Why does age drop out? Would the results differ if you used age groups instead of years.

Cognition and age were probably not significant because there was quite an important homogeneity concerning these two variables in our derivation sample. The mini mental state evaluation was performed on a fraction of our cohort i.e. patients aged 65 and older. This population was remarkably intact in terms of cognition (mean MMSe 24.9, median 26.0 and 25th percentile 22). This probably explains why weakened cognition was not associated with discharge to a PAC facility. This point is now underlined as a limitation of our study in the revised version of our manuscript (p 16).

Patients who leaved hospital between day 1 and day 3, and therefore not included in the day-3 model, were notably younger than the ones who stay longer. This phenomenon implied higher age homogeneity in the derivation cohort. This can explain why age was no
more a predictor of discharge to a PAC facility on day 3 model. In addition, when age groups (18-34 years, n=20 (6.7%); 35-49 years, n=32 (10.7%); 50-64 years, n=65 (20.7%); 65-79 years, n=121 (40.5%) and 80-93 years, n=61 (20.4%) were used in the day 3 model instead of continuous scale, we also found no significant association, (p=0.47). Age distribution and its absence of significant association are now presented in the results section p 9, and explanation about age drop is added in the discussion p 14.

Responses to reviewer Kathryn Bowles

1) Discharge to home with professional services is not the same as discharge to home with an informal caregiver. It is of concern that 104 of the 349 patients received professional home care. The characteristics of these patients are quite different than those returning home without care and they are in many ways similar to patients going to the PAC facilities identified in this study. Why was discharge to professional home care not analyzed as a PAC outcome destination or perhaps you should have excluded those patients from the sample. There may be patients within that cohort of 104 who should have or would have received PAC facility care but the clinicians sent them home again to resume previous services. I would feel better if the characteristics of this cohort of 104 were compared to the others to examine how they differ or not. I also suggest some discussion about this. The authors need to help us understand why they did not consider referral to home care as a PAC destination.

We agree that discharge to home with professional services is not the same as discharge to home with (or without) informal care giver. Since the most frequent cause for discharge delays in our service was waiting for a bed in a PAC facility, we were concerned with discriminating patients unable to return home and requiring transfer to a PAC facility from those returning home with (or without) formal (or informal) help. It is the reason why we did not consider discharge to professional home care as a PAC outcome. This explanation has been added to the method section in order to better define our outcome (p.5). As an independent variable, receiving professional home care before hospitalization was included in our predictive model and was found to be neither predictive nor protective for our outcome. As suggested, we compared patients receiving professional home care before hospitalization (n=104) and those not receiving such help (n=245). Not surprisingly, these patients were older (74 vs. 61 yrs; p<0.001) had more disability in ADLs (1.4 vs. 0.7; p<0.001) IADLs (2.4 vs. 1.2; p<0.001) and comorbidities assessed by the Charlson comorbidity index (2.4 vs. 1.7; p=0.008) were more likely to live alone (64% vs. 39%; p<0.001) and less likely to be helped by a partner (25% vs. 45%; p<0.001). In terms of sex, and active medical problems, both groups were similar. Among these 104 patients, 64 (62%) were discharged home and 40 (38%) to a PAC facility. Both groups were however similar except in terms of disability in IADLs (2.0 vs. 3.1; p=0.005).

A paragraph was added in the results section, p 8 and 9, summing up this observation.

2) The reader needs to understand the model of discharge planning and decision making at the study site. This effects the generalizability of the findings because the analysis is based on decisions made by clinicians at this study site. We need to understand how those decisions are made and by whom and how this compares to other comparable sites.

At our study site, patient’s physician and primary nurse manage routine or uncomplicated discharges. For complicated or non-routine discharges, we use a consultative discharge planning model in which physicians, ward nurses and social workers work together to
assess, coordinate and implement the patients’ discharge plan. In addition, a weekly formal multidisciplinary ward round is held for the assessment of all patients’ discharge plan. The model of discharge planning and decision making at our study site is now presented in the method section, p 4, making comparison with other sites possible.

3) The authors do not discuss the screening tool published by Dr. Diane Holland. Dr. Holland's tool is similar to the one reported in this study. It is meant to identify early those patients who need comprehensive discharge planning to assure early recognition of needs and timely discharge. Holland, DE, Harris, MR, Leibson, C, Pankratz, VS & Krichbaum, K. (2006). Development and validation of a screen for specialized discharge planning services. Nursing Research, 55(1), 62-71.

We thank the reviewer for this remark. The study by Holland et al has now been included in the discussion section, comparing our study with other published screening tools (p 14).

4) Please provide a definition or description of how medication management was Measured and.

8). How were orientation and behavioural disturbances measured? Overall the authors do not describe their data collection instruments in enough detail.

Medication self-management before admission (or medication use) was assessed by asking patients if they were able to prepare their medication at home and to take correct dose. Dependency in this self-reported instrumental activities of daily living (IADLs) was defined as partial or total need of assistance from another person and was rated by research nurses. Orientation and behavioural disturbances were recorded by asking the nursing staff if patients were adequate in their time and space orientation, if they could recognize their relatives, and if they were agitated, confused, wandering or aggressive.

In the revised version of our manuscript, we have now attempted to describe more precisely our data collection instruments under the method section, p 5 and 6.

5) The sample is surprisingly cognitively intact. 95% of the sample are ages 44-85. It would be helpful to know the distribution of ages in the study. I also think some discussion about age, which is a predictor in most other studies of this nature, and perhaps why age did not predict in this particular analysis.

Cognition and age were probably not significant because there was quite an important homogeneity concerning these two variables in our derivation sample. The mini mental state evaluation was performed on a fraction of our cohort i.e. patients aged 65 and older. This population was remarkably intact in terms of cognition (mean MMSe 24.9, median 26.0 and 25th percentile 22). This probably explains why weakened cognition was not associated with discharge to a PAC facility. This point is now underlined as a limitation of our study in the revised version of our manuscript, p 16.

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6) There is no information about the assessment tools used in this study. The AEP and Delay Tool are referenced but if not familiar with the tools, the reader is left to wonder if they are reliable, valid and appropriate.
A more detailed description of AEP and Delay Tool has now been added in the method section, p 5.

7) Were orthopedic patients included in the sample? Are they included in the rheumatic or neurological disease category? The predictors might be different if orthopaedic was separated out since they tend to be sent to rehabilitation more often than other diagnoses. There also may be collinearity with orthopaedic diagnosis and function. The sample came from a general internal medicine floor and therefore probably did not contain patients recovering from hip or knee replacement. The authors should make this clear and identify it as a limitation.

Neither orthopaedic nor surgical patients were included in our sample, since, as correctly underlined by the reviewer, our sample came from a general internal medicine floor. Only medical patients with an acute condition are admitted in these beds. This has been clarified in the method section (p 4), and identified as a limitation in the discussion section (p 16).

9) How was data collection completed if the patient was cognitively impaired? For the few patients who were cognitively impaired in our sample, data were completed by asking their family. This precision is now added under the method section, p 5.

10) On page 5 the type of admission is described as from emergency department versus internal hospital transfer). In table 2 it is described as hospital internal versus other provenance. Please be consistent.

We have now described type of admission similarly in the method section and in the table 2.

11) Please describe how the weights of 1-4 were assigned to the predictors.

The basis for assigning points has been now added in the method section, p 10.

12) Reference number 24 is from 1988. there are more recent studies that support this finding.

In the revised version of the manuscript, we suppressed this reference and added more recent studies (see ref. 28 to 31)

13) Page 11 discuss how you see this tool being used and by whom. How would they receive the information?

Patient’s resident and ward nurse calculate the score on the 3rd hospital day. They systematically report the score and its components into the patient’s medical chart, making the information easily available not only for the ward staff but also for the multidisciplinary clinical team who cares for the patient. This has been now more precisely described in the discussion section (p 15).

We hope that our revised manuscript will be deemed acceptable for publication in the BMC Health Services Research.

We thank you for your consideration,

Yours sincerely,

Martine Louis Simonet, MD
First and corresponding author