Author's response to reviews

Title: Schizophrenia and potentially preventable medical and surgical hospitalizations in the United States: a retrospective cross-sectional Study

Authors:

Elizabeth Khaykin Cahoon (elizabeth.khaykin@nih.gov)
Emma E McGinty (emcginty@jhsph.edu)
Daniel E Ford (dford@jhmi.edu)
Gail L Daumit (gdaumit@jhmi.edu)

Version: 2 Date: 17 January 2013

Author's response to reviews: see over
Reviewer 1

• Table 2. It would be more informative if a comparison can be conducted about the difference of each indicator between the two study groups, and report in the Result section only the significantly different indicators.

Thank you for the suggestion. We have added unadjusted ORs to Table 2 and have noted the significant differences in the results (page 8, paragraph 2).

“Unadjusted ORs of overall, acute, and chronic ACS hospitalizations were higher for admissions with secondary diagnosis of schizophrenia compared to those without. Unadjusted ORs of all four individual acute and six of the nine chronic ACS conditions measured, including CHF, COPD, asthma, diabetes mellitus long and short-term complications, and uncontrolled diabetes were higher for hospitalizations with versus without secondary diagnosis of schizophrenia.”

• Page 10, last paragraph: Table 1 and 2 showed that the “with Schizophrenia” group had higher proportion of CHF diagnosis respectively. This information should be included in your discussion about the decreased odds of hospitalization for CHF for the schizophrenia group in the last paragraph of page 10.

Thank you for pointing this out. We have double checked our output and code and this is correct. The direction of the schizophrenia-CHF relationship was affected by adjustment for confounding variables. We now note this in the discussion (page 11, paragraph 2)

“Schizophrenia was also associated with decreased odds of hospitalization for CHF and angina after adjustment.”

• Page 5, last paragraph: provide a citation after “Billings, ”.

Thank you for catching this missing reference. We have updated the manuscript with this reference. (Page 5, paragraph 3)

• Page 9, 2nd paragraph: provide a citation for “Li’s study....”

Thank you. We have updated the manuscript with this reference as well (Page 9, paragraph 2).
• Page 11, 2nd paragraph, 4th line: consider replace “only” by a different adjective word.

Thank you. We have reworded this sentence to state: “…this is only one possible cause of ACS hospitalizations.”

• Page 13, Table 1: the male, female percentage for the “with schizophrenia” group does not add up to 100%.

Thank you for noticing this in your draft. In our version, the column percent does add up to 100%. If we can clarify further, please let us know.

Reviewer 2

• I would like to know the detailed procedure of stratified sampling in the data source subsection.

The NIS is stratified by geographic region (Northeast, Midwest, West, or South) as defined by the U.S Census Bureau), location (urban or rural), teaching status (teaching or non-teaching), ownership (public, private not-for-profit, or private investor-owned), and bed size (small, medium, or large) (http://www.hcup-us.ahrq.gov/reports/methods/2003_2.jsp)

In order to better describe the stratified sampling procedure in the text, we have included the following sentence in the manuscript: (page 5, paragraph 3):

“The NIS is stratified by geographic region (Northeast, Midwest, West, or South), location (urban or rural), teaching status (teaching or non-teaching), ownership (public, private not-for-profit, or private investor-owned), and bed size (small, medium, or large).”

2. In the statistical analysis subsection, the authors should more clearly describe the statistical model to account for correlated structure.

Thank you, we are happy to elaborate. This is a complex dataset, but fortunately there is substantial guidance provided by HCUP which carefully advise on how to analyze this complex dataset. We have followed this advice carefully and contacted HCUP to confirm our model specifications will account for this design. For more information on how we computed statistics and variances accounting for sample design and weights please see:
For our primary analysis which had a dichotomous outcome, we used the rlogistic procedure of SUDAAN which is the only standard software to get stratified weighted ORs (until very recently SAS has developed a way). For example, in analyzing subpopulations we included all individuals outside the subpopulation to create the correct standard errors. We nested within NIS stratum and hospital and used discharge weights. We assumed sampling with replacement as suggested by the documentation.

We now elaborate our analytic method in the Methods section (page 7, paragraph 3):

“Analyses accounted for the NIS sampling scheme by nesting within stratum and hospital and incorporated discharge weights. While analyzing subpopulations, we included all individuals outside the subpopulation to create the correct standard errors.”

3. In the bivariate analysis of Table 1, the authors should report effect size rather than statistical significance testing.

Thank you for this suggestion. As the primary goal of Table 1 is to describe the study population, we respectfully suggest that Table 1 remain as presented in our original manuscript. We are concerned that presenting effect sizes may confuse readers, as the comparisons presented in table 1 are not the primary comparisons under investigation in the study. Rather, we prefer that our results focus on the main outcome of interest, potentially preventable hospitalizations in persons with schizophrenia. We do not present statistical significance testing for the bivariate comparisons in Table 1 as the sample sizes in this dataset are large, potentially leading to statistically significant differences that are not clinically meaningful. Descriptive comparisons for the Nationwide Inpatient Sample are commonly presented using this method (see Khaykin et al, General Hospital Psychiatry 2010).

4. According to logistic regression analysis, authors should adhere to some reporting guidelines. For example, they should report the results of testing for collinearity and the conformity of a linear gradient. cf. Bagley et al: Logistic regression in the medical literature: standards for use and reporting, with particular attention to one medical domain (J Clin Epidemiol 54: 979-985, 2001); Moss et al: An appraisal of multivariable logistic models in the pulmonary and critical care literature (Chest 123: 923-928, 2003); Ottenbacher et al: A review of two journals found that articles using multivariable logistic regression frequently did not report commonly recommended assumptions (J Clin Epidemiol 57: 1147-1152, 2004); Mikolajczyk et al: Evaluation of logistic regression reporting in current obstetrics and gynecology literature (Obstet Gynecol 111: 413-419, 2008)

The reviewer makes an important point. We do report significance testing, confidence intervals, sample size, and tests for interaction. Regarding collinearity, SUDAAN does not control for collinearity directly, but when betas get too big or too small SUDAAN will issue a warning message about the possible existence of collinear variables. No such warning was issued in the analyses for this manuscript. The assessment of a linear gradient is appropriate for continuous covariates, while our primary exposure of
interest is dichotomous (schizophrenia, no schizophrenia). In addition, to our knowledge, SUDAAN does not provide the ability to assess conformity with a linear gradient. Tests such as the goodness-of-fit assess whether or not the observed event rates match expected event rates in subgroups of the model population are used primarily for creating risk prediction models. In this study, we aim to simply compare people with and without schizophrenia, with regard to preventable hospitalizations, controlling for other differences that may potentially confound the observed association. To clarify this we have explicitly stated why predictors were included in the model (page 6, paragraph 2):

“Potential confounders were included in the model based on previous research in this field, differences found in Table 1, and so those that remained significant after adjusting for other covariates for the vast majority of PQI outcomes.”