Article details: 2015-0123	
Title	
Authors	
Reviewer 1	Ms. Maria C. Bennell
Institution	Sunnybrook Health Sciences Center, Cardiology
General comments (author response in bold)	This is an interesting and well-written study about HIBI patients. The authors present a convincing case as to how understudied this population is, and the need to better understand the needs of the population. They present important evidence that could be beneficial for rehabilitation researchers and clinical practitioners. My comments and questions: 1) Page 6, paragraph 1, line 10 "In the rare case of multiple episodes…" Can you give the n for this? Why would longest LOS be more appropriate than first LOS? We would like to thank the reviewer for bringing our attention to this unfortunately worded section. While our data cut strategy was, a priori, to prioritise longest LOS in cases of multiple DAD entries per individual over the study period, in actuality, we did not end up with any repeated episodes of care in the DAD meeting inclusion criteria for our "HIBI Extended" (and by extension, smaller "HIBI MRD" subset) study sample. As such, we feel that this point has been rendered moot and we have omitted this entire sentence (page 5, lines 11-12) 2) Case definition (page 6, paragraph 2). Why were both cohort definitions used in this paper? Seems like the second (broader) definition is better, as it is more sensitive. So why not just use the first, more conservative definition, as a sensitivity analysis? It would reduce the bulk of the paper and make data interpretation easier. Due to issues of reliability of coding for secondary diagnostic fields in the DAD, we felt the more restrictive/specific study sample, including only those with an most responsible diagnosis of G93.1, was a more likely to exclusively comprise patients hospitalized/treated for HIBI. As such, the authors feel more comfortable keeping the more restrictive "HIBI MRD" population as the main analytic sample. For more detail on this issue, please see our response to the major comment for the other reviewer (Mr. Dibatista) 3) Consider including the causes of HIBI (e.g. arrest, etc) as covariates in your models. It might give some additional insight into ALC days. Due to issues of reliability of coding for secondary diagnostic fields in the DAD, we did not feel confident in our ability to use this information as such, though we do agree that it would be interesting, if determined feasible, for future work. For more detail on this issue, please see our response to the major comment for the other reviewer (Mr. Dibatista) 4) Which is more important in this study? 1) to look at HIBI patients who have ALC days? Or 2) HIBI patients who do not die in hospital? If it is 1), you should include the 3% of patients who died but had ALC days, as these patients should be in the analyses. Based on the intro, it sounds like you used definition 2. If so, why was it important to only include patients who survived? This study, examining determinants of delayed discharge among the HIBI acute care survivor population, is part of a larger project tasked with investigating determinants of healthcare utilization and barriers to appropriate care for HIBI patients who survive their index HIBI. The HIBI acute care population is unique in that almost 80% of those admitted with HIBI die. As such, including these individuals in analyses would have skewed interpretation toward a different population of interest. Further, we maintain that individuals who survive their initial hospitalisation for HIBI stand to benefit more from interventions/policy aimed at minimizing delayed discharge, making the exclusive HIBI survivor population of greater interest. 5) Figure 1 footnotes. "2012 data excluded due to truncation…" Does this mean the 2012 data is excluded from the study, or are you referring to omitting that data from Figure 1? Please clarify. The data cut for this study was truncated at the first third of 2012 (i.e., end of fiscal year). We used data right up to this point for all analyses. Because we did not have a full year of data for 2012, we did not include in Figure 1 (now Figure 2) as this would have made the number of Length of Stay days

	appear artificially few compared to the remainder of the study period. 6) Page 8, paragraph 3, line 41 “of the HIBIH MD and HIBI extended cohorts, 47.1% and 41.6%…” Where is this ALC data coming from? I don’t see it in table 1. Why isn’t it in table 1? We reserved Table 1 to describe potential determinants of the main study outcome (ALC days. Further, as this the proportion of the study population experiencing ALC days is mentioned in the text of Results, we feel it may be somewhat redundant to also include in Table 1.). If the editors feel that the quantification of the outcome should also be described in Table 1, we would be happy to add this. 7) Page 8, line 51. What are the units for median LOS values? Units of LOS is days. This is mentioned in the first part of the sentence in question. 8) Figure 1. Mean % ALC days/total LOS days might be more informative. We feel that ALC days, by definition, is an indicator of acute care inefficiency. We felt that Figure 1 (now Figure 2) would be more impactful if it illustrated the year-by-year accumulation of ALC days (relative to length of stay days). A measure of central tendency would, alternatively, illustrate the average HIBI patient experience, which we deemed less impactful. 9) Does LOS included the ALC days? It is not clear to me. Length of Stay (LOS) includes the entire time spent in inpatient hospital for the captured episode, which includes ALC days. Please second last sentence of second paragraph of Results for clarification. 10) Results should be broken down into sub-sections that refer to the different models. This would help greatly when examining the results. We are uncertain what is meant by “different models”. The last paragraph of Results describes independent predictor effects across “HIBI MRD” and “HIBI Extended” populations from a single multivariable count regression model (zero-inflated negative binomial regression) which incorporates both count and binary outcome processes. Should the editors feel it clearer to divide the final Results paragraph into 2, each describing covariate effects on each outcome process, we would be happy to accommodate. If so, we advise starting the new paragraph with the sentence starting “Predictors of no alternate-level-of-care days…” (ie, 5th last sentence of last paragraph of Results) 11) Analyses for table 2. Rate ratios of ALC per LOS day – on page 9 you sometimes refer to a result as more or less ALC per LOS days, and other times you refer to the results as more or less ALC days. Can this be written either way? Or should it always be written as ALC per LOS days? We agree that, for the first part the final Results paragraph, it is more precise to consistently phrase predictor effects in terms of ALC days per LOS days. Please see changes made for clarification throughout first half of the last Results paragraph. 12) Page 9, lines 15-22. Aren’t these 2 sentences referring to the same thing? We agree that the wording might make the second sentence sound somewhat redundant. Please see changes for clarification on lines 10/11 of page 9 13) Why are there different predictors in tables 2 and 3? There are difference predictors in tables 2 and 3 because, as might be expected and as we encountered, count outcome (ie, number of ALC days per LOS) and binary outcome (any ALC days) are not necessarily associated with the same set of independent variables. We only retained variables in the final model that improved specification of either model process by a minimum set of criteria (i.e., minimum level of statistical significance). Only two determinants (Special Care Hours and Psychiatric/Behavioral Comorbidity) were retained as independent variables for both outcome processes. 14) Page 9, line 27 “only discharge disposition and psychiatric…” No SCU hours was significant too. SCU hours did not significantly add to the binary process portion of the zero-inflated negative binomial regression model for the HIBI extended sample, described in the latter half of the last

	paragraph of Results. This is indicated by the corresponding "N/A" across from SCU hours in Table 3. 15) Page 9, line 39 "lastly, having a psychiatric co-morbidity…" Why is this line here? It is part of the table 2 results, not the table 3 results. Psychiatric/Behavioral comorbidity is the final predictor listed in Table 3. Upon review, we feel, however, that this final sentence of Results may be redundant. We have also taken the opportunity to attempt to clarify by changing the label of this potential determinant of ALC days to "psychiatric/behavioral comorbidity" throughout the entire manuscript text, as well as in Tables 1, 2 and 3. 16) Tables don't have footnotes. We have added footnotes writing out abbreviations in tables and are happy to add further footnotes to the Tables should the editors feel they are warranted. 17) Page 10, line 18. "1 in 2 patients with HIBI had an ALC day." If this is noteworthy, then why wasn't this finding in the results somewhere? This finding of ~ 50% is much higher than in other studies. Can the authors speculate as to why this is? In the other studies, did they exclude patients who died when they came up with their values? We state that "nearly 1 in 2" HIBI survivors have at least one ALC day in the first paragraph of Interpretation that is referenced here. This is recapitulated from the 3rd sentence of the 2nd paragraph of Results where we state "Of the HIBI MRD and HIBI Extended cohorts, 47.1% and 41.6% of patients had at least one ALC day, respectively."
Reviewer 2	
Institution	
General comments (author response in bold)	