Dear editors and reviewers of the Journal of Hospital Medicine:

Thank you for your thoughtful comments and suggestions. We were excited to improve the quality of this paper and believe it makes a significant contribution to our understanding of health care policy and carries important implications for hospitalists and hospital leaders.

A line by line response to the editors are included below. Thank you for your time and consideration.

Best,

Chris Chen

*Summary comments*  
  
Comments to the Author:  
The intense interest in this manuscript was reflected by the number of reviewers who chose to review it and the high quality of those reviews. Although there is substantially more reviewer input than is typical, we believe this input is high-value, and since the current manuscript is relatively short, there is room for expansion of the methods, results, and discussion to address the concerns that have been raised and flesh out the analytic methods, assumptions, sensitivity analyses, and discussion.

Particular areas of concern that should be addressed:  
1)      Absence of risk adjustment: We know that patient factors have a major impact on readmissions; are there potential mechanisms that might explain sicker patients migrating to the “intervention” quartile after the intervention? Is there any way you can convince the reader that patient populations were relatively constant (except for access to insurance) across the time periods? Did the overall readmission rate in Massachusetts go up relative to other states in the wake of their reform efforts?

**Thank you for this suggestion. We have now incorporated risk adjustment measures into our results, and revised our paper accordingly.**

2)      CMS has reported that hospitals serving more impoverished communities (in aggregate) have slightly higher readmission rates than those serving more affluent communities; this observation is also supported by your observation that patients with higher percentages of uninsured patients had higher readmission rates at baseline.  How do you explain the observation that higher baseline uninsurance was associated with higher baseline readmission rates?  If your hypothesis is true that that insurance access can increase readmissions, then why in the baseline state was this relationship not observed?  Do you think there is a catch-up phenomenon (whereby those who suddenly get access to insurance get a lot of medical care over the subsequent 1-2 years as pre-existing but undiagnosed medical problems are diagnosed and treated?).

**You are right to point out that at first glance, our finding that gaining insurance is associated with increased readmissions appears to run counter to our finding that overall, high uninsured hospitals have higher readmission rates. We have explain this apparent incongruity in the discussion section of the paper**

**In brief, our argument is that hospitals with a greater percentage of uninsured patients had higher baseline readmission rates because these hospitals were typically safety-net hospitals treating patients of lower socioeconomic status. Patients of lower socioeconomic status are more likely to be re-admitted.**

**As health reform came into effect, we found that these safety-net hospitals subsequently experienced increases to their unadjusted readmission rate but no changes to their risk-adjusted readmissions rate. The risk-adjustment algorithm is driven by patient co-morbidities. Thus, these results are consistent with a mechanism in which safety-net hospitals took on a sicker patient population as reform progressed, which drove up their unadjusted readmissions rate but not when the readmissions rate was adjusted for clinical status. By expanding insurance coverage to a poorer, sicker population, the Massachusetts law resulted in the state’s population of insured patients becoming, on average, sicker and thus more likely to be readmitted.**

3)      If you had individual patient-level data, then why were readmissions attributed to the hospital? Would it not be more direct to determine whether individual patients had higher readmission rates after getting insurance than before via a pre-post comparison at the patient level?

**Because Medicare and other private payers measure readmissions at the hospital level, we felt measuring hospital-level readmissions was of great interest.**

4)      A particularly ominous source of potential bias in your study is ascertainment bias. Perhaps the healthcare reform led to more accurate labeling of patients across institutions (via a more robust and maintained patient identifiers) and better readmission case-finding. For instance, in the pre-reform period, a given patient might have not been recognized as the same patient when re-presenting to a second hospital after discharge from the first, whereas with more accurate unique identifiers in the post-reform period, the readmission would be more apparent and more likely to be correctly labeled as such.  Would an appropriate sensitivity analysis include looking at same-hospital readmissions, to limit the risk of ascertainment bias?  Inter-hospital readmissions are much easier to capture when you have access to payer data since unique identifiers are reliably carried throughout the healthcare system, and the reform led to having more payer data. If you showed that same-hospital readmissions also went up for patients in the least insured quartile (to the same degree that you observed with all-hospital readmissions), the analysis would be more compelling.

**We cannot rule out that the observed increase in the unadjusted readmission rate is due to ascertainment bias, and unfortunately, we cannot specifically track intra-hospital readmissions. That being said, the fact that the risk-adjusted rate does not change appears to indicate that the patient population has become sicker. We address this in our discussion section.**

**In brief, we cannot definitively rule out this possibility. However, there are a couple reasons to think that is not what is driving our results. First, Massachusetts’ state reporting system is robust; hospitals are legally required to report timely, accurate data, and the state agency conducts a quality control process that rejects each hospital’s data submission if it exceeds a one percent error rate. Second, the fact that risk-adjusted readmission rates do not increase casts doubt on this hypothesis, since if the increase in the unadjusted readmission rate were due to ascertainment bias, the risk-adjusted rate would not exhibit a similar increase. The divergence between the unadjusted and risk-adjusted rates appears to suggest that the patient population has actually become sicker.**

Minor issues:  
1)      Please treat the word “data” as plural throughout

**We have made this change.**

2)      It might be helpful to quantify readmission changes based on both relative changes and absolute changes. (a 0.6% difference may appear trivial at first glance)

**Thank you for the suggestion. We have done so.**  
  
*Reviewer's Comments:  
  
Reviewing: 1*  
  
Comments to the Author:  
General Comments: This paper raises interesting and important questions with policy and measurement implications.  It needs more work but is worth the additional effort.  The overall design of comparing three time periods is sound.  Other commenters have pointed out that this is not quasi-experimental.  It’s a time-series.  The use of one quartile vs three others is questionable, needs more discussion/refinement.  Statistical analysis needs work.

**We have further refined the analysis so that we now compare individual quartiles rather than grouping them into three, and have added additional analysis by decile.**   
  
Introduction: Good overview.  First sentence unaccountably does not cite CMS policy; obviously, it should.  Implication of socioeconomic status on readmissions: please cite evidence (this is quite important later in the paper).  Citation 2 is a poor choice (WSJ article); there must be more comprehensive information about insurance plan design.

**We now cite CMS policy and have removed the WSJ citation.**   
  
A significant lack of clarity in the paper could be remedied by addressing the fact that there are two key dynamics here: (1) do patients experience higher readmissions due to better access to insurance? And (2) are these increased readmissions, if confirmed, concentrated in some hospitals rather than others due to factors out of their control, making readmission an inappropriate quality metric to discriminate among hospitals? These two questions should be addressed clearly and separately.  
  
The hypothesis muddles these two concepts.  It says “expansion of insurance would be associated with higher rates of readmission among hospitals…” but in fact, readmission rates are associated with patents, not hospitals.  The hypothesis SEEMS to be that insurance leads to higher readmissions AND that patients who experience this higher likelihood of readmission will be clustered among selected hospitals, those which had the greatest shift from uninsured to insured status.  And I agree with the reviewer who perceived that this appears to be a post-hoc hypothesis.

**Although we agree that the framing of the paper needs clarification (and we have attempted to make it clearer), we disagree with the framing outlined above. We use a difference-in-difference method to investigate whether readmissions increase in high-uninsured hospitals as a means of determining whether the reform is associated with increased readmissions at the hospital level. We focus on the hospital level, rather than the patient level, for two reasons: First, it allows us to use difference-in-difference analysis as a means of determining whether the insurance expansion results in increased readmissions, while addressing potential omitted variable bias. Second, it allows us to determine whether the readmissions program unfairly penalizes certain hospitals. In other words, the two questions above are inextricably linked.**

**We recognize that our paper cannot, and does not claim to, determine the impact of health insurance expansions on the likelihood of individual patients being readmitted. Rather, we examine the empirical effect of Massachusetts’ health expansion law on hospital readmission rates. The reviewer suggested a way to break down our question into two narrower ones. The suggestion has merit, but we believe our approach has merit as well. While our approach will not be able to answer the academic questions the reviewer proposes, our approach provides an understanding of the empirical result of Massachusetts’ health reform law on hospital readmission rates that will help forecast the ACA’s coming impact on hospital readmissions.**

Stratification – as the other reviewers commented, the quartile approach is not clearly appropriate.  A better analysis of baseline data would help, e.g. how was the “most uninsured” quartile different from the other three, and how were they like each other?  What is the data rationale for the one-versus-three design?  Agree with the reviewer who cautioned about the ecological fallacy.

**We now stratify hospitals by deciles and repeat our analysis to make sure that the results are not dependent on the quartile specification. In addition, we no longer take a one versus three approach with quartiles but analyze the quartiles individually. In addition, we provide an analysis of baseline data for each of the quartile.**  
  
From other reviewers’ comments it appears that there was a Table (not present in the draft I reviewed) which discussed demographics of the patients in each quartile.  This would seem to be useful information. Further, the analysis might have controlled for variables such as poverty (by zip code or SMSA), and perhaps age group even within the 18-to-65 cohort.  Another useful variable (based on literature on readmissions) could have been mental health or substance abuse comorbidities if available.

**Thanks for the suggestions. We have now included a table describing the demographics of the patient in each quartile. We control for age, gender, and race as well as a validated risk-adjusted scheme that includes patient co-morbidities, including mental health and substance abuse.**  
  
How did the quartiles vary during the baseline period in readmissions (ideally, controlling for poverty, mental health, perhaps reason for admission or comorbidity)?   How did the magnitude of that difference change over the three study periods?

**We include this analysis now.**  
  
RESULTS  Figure 1a – very good, but needs to be supported with data on the three other-than-highest quartiles to support the concept of blending them into one control group.  
  
Figure 1b – If the three-quartile combination is supported, then what is the significance of the magnitude of the difference between the highest & all-other groups at baseline, during intervention, and at conclusion?  The data are incomplete to answer this question. The confidence intervals may be wide and it would be useful to see them graphed or described.

**We have now stratified quartiles individually and described the patient characteristics as quartile.**   
  
The authors characterize the change in insurance status in the “control” quartiles as “minor.”  However, it was reduced by 58% (from 5.9% to 2.5%) which is certainly nontrivial.

**We have removed this language.**  
  
What I want to know is this –  
How were the four quartiles the same/different from each other at each of the three time periods?  
How was each quartile’s readmission rate different from baseline to conclusion? Were these statistically significant changes?  
How were the magnitudes of the changes in each quartile different from the magnitude in other quartiles, and how significantly different?

**Our analysis now encompasses these factors.**

The results statement doesn’t make sense. …”the study hospitals had an increase in their readmission rate of 0.6%...during the reform and post-reform periods as compared to the comparison hospitals.”  What is the 0.6% referring to?  
Table 1 is unclear.  To what do the coefficients refer?

**We have clarified the description of the data.**

This sentence is unclear: (p 6 line 51) …each of the control quartiles individually had no statistically significant change in the readmission trend at the beginning of the reform [ how can there be a change at the beginning? ] while the increase in the readmissions trend among study hospitals was stable [how can an increase be stable?].”

**We have clarified this sentence.**  
  
Another question not addressed at all in this paper is – were the specific insurance plans the same among the patients who received care at each of the quartile hospitals?  Was there a feature of the insurance plan designs which may have contributed to different readmission rates?  Can this be controlled-for in the analysis?

**While certainly an important and interesting question, we think this is outside the scope of our dataset and analysis.**  
  
DISCUSSION:  The discussion is generally good though not comprehensive enough.  It will be stronger when the authors can address the statistics more robustly as outlined above, and also if they can address the socioeconomic questions more effectively.  Furthermore - ALL hospitals saw readmissions increase, just possibly not at the same rate.  What does this imply?

**We have expanded the discussion to address these issues.**  
  
Additional Comments  
  
Phrases like “readmissions at affected hospitals” (p 3 line 37) are misleading, because the readmissions may occur at other hospitals yet are typically still included in the quality metric for the original hospital.

**We have changed this language.**  
  
The introduction does not speak to the extensive literature on impact on utilization of reduced or zero copays (eg the Rand health insurance experiment and many subsequent studies).  Surely there should be some relevance here?

**Yes- absolutely. Thank you**  
  
Page 4 line 11 – the summary of the Oregon Health Study is inadequate.  What do the authors mean that a “quantitatively large effect could not be ruled out”?  (isn’t that redundant, and what effect, and why not?)

**The Oregon study itself actually uses this language. Their confidence intervals were too large to rule out large effects. However, we have clarified the language. The point we were trying to express is that Oregon found statistically insignificant changes in readmissions, but given the large confidence interval the authors believe this is due to an inability to measure readmissions well rather than evidence that insurance expansion has no effect on readmissions.**  
  
METHODS: specify which government entity. Was the project exempted BY the IRB (should have been) or did they decide not to seek review?

**We have clarified this.**  
  
It is interesting that the researchers don’t comment regarding the site of readmission during the baseline period.  Were the insured and uninsured patients more likely to be readmitted to the same hospital or a different hospital?  What might this say about readmission as a quality metric?  
  
**Unfortunately, due to the way readmissions were tracked, we are unable to distinguish readmissions to the same hospital versus readmissions to another acute-care hospital. Medicare and other private payer programs track readmissions to any acute-care hospital, not specifically just to the same hospital as the index admission.**

*Reviewing: 2*  
  
Comments to the Author:  
This is an important paper that I think will be of interest to your readership and should have implications for national readmissions policy as well.  More information in a number of areas would be helpful.  
  
Major Issues  
1.      My biggest issue with this paper is the lack of important information on the methods and results (outlined in more detail below).  If this was submitted for a brief report, a few key pieces of information should be provided, such as what risk-adjustment was used and why log-transformation was done.  The intro and discussion also need clarifying.  If this was submitted for a full manuscript, quite a bit of additional information needs to be provided and the discussion needs a few more paragraphs of commentary on the potential mechanisms underlying the findings.

**We have expanded our Methods, Results, and Discussion sections. We now include risk-adjusted analyses and we preform analysis results using deciles as well as quartiles.**  
  
Minor Issues  
1.      It would be helpful in the abstract if you gave a hint as to why you think this phenomenon might exist, either in the intro or the conclusion, to key the reader in as to why this is such an important policy question.

**We have added this.**  
2.      Need a reference at the end of the first paragraph of the introduction.  
3.      The second paragraph of the introduction would be strengthened by being a bit more straightforward about the potential effects of insurance expansion on readmissions.  I think many people’s first knee-jerk reaction would be that expansion would reduce readmissions (though on further thought many might change their minds), and I think I’d set it up in that same order.  For example, it would be helpful to just say “on one hand, insurance expansion may reduce readmissions by improving access to outpatient and preventive care.  On the other hand, insurance expansion may increase readmissions by…”  
4.      The Oregon Health Study stuff currently in intro paragraph 3 would go more naturally in paragraph 2 (which could be broken into two paragraphs, each about one of the potential effects of insurance expansion).  The last paragraph of the introduction should just set up your study without bringing in more evidence.  
5.      The methods section requires additional detail if this is to be a full-length paper.  In particular, more information about risk-adjustment should be included, since the penalties are assessed not on raw readmission rates but on adjusted ones.  Additionally, why were log transformations undertaken?  
6.      The results section is too thin for a full-length paper.  If this is to be a full-length paper, it should have at least two additional tables, one of patient characteristics at high-uninsured versus other hospitals and of hospital characteristics in each group.  
7.      The second paragraph of the results is confusing because it gives the overall results and then returns to “prior to reform” in the second sentence.  Would move the first sentence later in the paragraph.  
8.      Table 1 is confusing and would be easier to read with better labeling and the addition of percentages, as are listed in the text.   What are the units?  Is it necessary to do natural log spline regressions for something your figures suggest is relatively linear after accounting for seasonal variation?  Can you put the actual percent readmission rates somewhere in this table (or a different one) to help the reader find the numbers you talk about in the results section?  
9.      The discussion lacks adequate commentary on why the authors think they saw the phenomenon they did.   Why, in and of itself, would insurance lead to an increase in readmissions?  (I can think of a few reasons – fewer delays in care, more new diagnoses with ongoing changes in med regimen etc.; some mechanisms should be discussed here).  If a brief report, this could be discussed in little detail, but if a full manuscript, needs  a great deal more content.

**We have expanded our discussion section. We cannot definitively show what caused the increase in the unadjusted readmission rate, but we provide an expanded discussion that we hope readers will find thought-provoking and informative.**  
  
  
*Reviewing: 3*  
  
Comments to the Author:  
This manuscript discusses the potentially problematic use of 30-day readmission rates as a metric of hospital performance, particularly as an influx of previously uninsured people will receive care.  There are major concerns regarding the use of non-risk-adjusted readmission rates and the absence of a discussion of risk-adjustment. While the manuscript could use some more detail in the methods and results section describing the dataset, it is overall well-written and the "natural experiment" design is compelling.  
  
A list of some major concerns, along with more minor comments that should be considered is outlines below:  
  
1. The manuscript does not fully describe the conceptual framework of how low SES and previous uninsured status is related to increased hospital admissions. While it may appear obvious, I believe it is still important to describe how (presumably) a previously-uninsured population that has received substandard preventive care, or none at all, for an extended period of time and will directly lead to more co-morbidities and higher severity than the insured population. Therefore, when this population has access to insurance, they will present with more severe illnesses leading to worse outcomes and more frequent admissions.  Using an unadjusted metric for 30-day readmission rates may unfairly penalizes hospitals who treat these sicker patients. However, this is really only true for readmission rates that are unadjusted for the disease risk/severity of the population each hospital treats.

**We have added a parenthetical note explaining this mechanism. Your point about the importance of risk-adjustment is well-taken.**   
  
2. The idea of risk adjustment is unmentioned in the manuscript and really should be included. Lisa Iezzoni and others have written extensively about this for years, and risk adjusted readmission rates appear to be part of the CMS guidelines ([http://www.cms.gov/Medicare/Medicare-Fee-for-Service-Payment/AcuteInpatientPPS/Readmissions-Reduction-Program.html](http://www.cms.gov/Medicare/Medicare-Fee-for-Service-Payment/AcuteInpatientPPS/Readmissions-Reduction-Program.html" \t "_blank)) as well as in the cited Reference 9.  This deserves more discussion, particularly since the conclusions essentially argue for risk adjustment to level the playing field between hospitals.  
  
The present analysis apparently does not use any risk adjustment, which as the authors conclude, will unfairly penalize hospitals who treat sicker patients at high risk of readmission. The authors describe this as a deficiency and major limitation in the metric, and this is problematic as a primary method for hospital evaluation. The authors state: "Nevertheless, the general hospital thirty-day readmission rate is widely used as a quality metric and has been endorsed as such by the National Quality Forum (9)."  
  
My major concern is that the two endorsed readmission rate metrics by the NQF do use some form of risk adjustment and the risk adjustment component is clearly an essential part of calculating these rates. Indeed, these rates are not simple proportions of readmissions for individuals (which is what the manuscript calculates), but rather use other data as a marker for risk (such as previous admissions, reasons for admission and weighting by probabilities, etc.). The authors do not acknowledge this crucial part of assessing hospital performance. The following excerpts are from Reference 9 ([http://www.qualityforum.org/WorkArea/linkit.aspx?LinkIdentifier=id&ItemID=70957](http://www.qualityforum.org/WorkArea/linkit.aspx?LinkIdentifier=id&ItemID=70957" \t "_blank))  
1768: Plan all-cause readmissions (NCQA) This measure – developed by the National Committee for Quality Assurance (NCQA) – counts the number of acute inpatient hospital  
stays for patients aged 18 and older during the measurement year that were followed by an acute readmission for any diagnosis within 30 days, as well as the predicted probability of an acute readmission. The measure reports data in the following categories: count of index hospital stays; count of 30-day readmissions; \*average adjusted probability of readmission\*; observed readmission; and total variance.  
  
  
"1789: Hospital-wide all-cause readmission measure (CMS/Yale)  
This measure estimates the hospital-level, \*risk-standardized\* rate of unplanned, all-cause readmissions for any eligible condition within 30 days of hospital discharge for patients aged 18 and older. The measure will result in a \*single summary risk-adjusted readmission rate for conditions or procedures that fall under five specialties: surgery/gynecology, general medicine, cardiorespiratory, cardiovascular, and neurology.\*"  
  
  
Had the authors analyzed the data incorporating this risk adjustment from either of these two metrics, it is quite possible the readmission rates between the study and comparison groups would be comparable. While the current analysis is valid as an unadjusted proportion, it really only underscores the importance of risk adjustment, rather than highlighting a deficiency in a metric that may not exist if the risk adjustment is good. The NCQA or CMS/Yale algorithms may be sufficient to overcome the differences in disease severity presented in the analysis, and therefore may be a useful metric for hospital evaluation. However, since the authors did not use these tools to calculate risk-adjusted readmission rates for the different time points, the claims that readmission rates are unfair are not supported.

**Thank you very much for pointing this out. This was a major oversight. We have now re-run our analyses using risk-adjusted readmission rates, and as you predicted, we found that the risk adjusted readmission rate does not change. We have re-written our paper to incorporate these results. As you suggest, the divergence between the unadjusted and the adjusted results underscores the importance of risk adjustment, rather than representing a reason to critique readmission penalties. That being said, we believe that the finding that Massachusetts’s insurance expansion increased its unadjusted readmission rate is worth considering—regardless of whether risk-adjustment takes this into account—because addressing increased readmissions is important to reduce unnecessary care and costs.**

Minor concerns:  
3. It would be essential to see a description of the data. How many subjects/subject-admissions were included, and from how many hospitals? What was the median number of readmissions and was is the IQR for each hospital group (study/comparison) from the three periods? It would be very helpful to see the denominators of the proportions presented in the figures to determine the size and reach of the change in health law in the state (particularly to show how it relates in size and demographic proportions to the rest of the US).

**We have added more detail to more demographic descriptions.**   
  
4. This is an excellent use of differences-in-differences design, which is unfortunately rare in epidemiologic studies. It would be very helpful to have a statistical methods section in which the linear model is presented (or described in words) so that readers could see how this setting employs the widely used and familiar linear regression structure. This would also make it more easy to understand the null hypothesis, which should also be described and explicitly stated: that if the disease severity in the study hospitals were the same as the comparison hospitals, despite increased insurance coverage, there would be no difference in readmission rates between the two groups.  
  
It would also be helpful to present the results of these coefficients in a table, or impose the parameters as lines in Figure 1b to show how well the model agrees with the data.  
**Thank you for the suggestions. We have expanded our description of our models to clarify for readers.**

Grammatical/editorial comment:  
5. The second sentence of the results states: "uninsured patients were disproportionately concentrated into the highest uninsured hospital quartile." While this is true, it is circular, since the authors categorized hospitals by quartiles of proportions of uninsured subjects.  
  
The major concern of this reviewer is that the authors do not use risk-adjusted readmission rates, particularly in the context of evaluating its use as a metric for hospital performance. The NQF endorses risk-adjusted readmission rates which are calculated from complex algorithms that are developed to take into account the very problem the authors describe: hospitals treating high-risk populations should not be judged the same as hospitals who treat lower-risk populations. Several algorithms are available that attempt to account for differences in disease severity (such as 3M's All patient-refined diagnosis related groups [APR-DRG]), however many are not publicly available. Therefore, in the absence of risk adjustment, it is inappropriate to conclude that the readmission rates as a performance metric suggested by the NQF are problematic. As a suggestion, it would be suitable to present this data and analysis as a case study in the importance of risk adjustment; or as an even more compelling extension, whether the differences in readmission rates persist when endorsed (i.e., validated) risk adjustments are used. An alternative is to present the data as a predictor of increased burden (in terms of more frequent readmissions) in hospitals serving high risk population who were previously uninsured with ACA insurance expansion. Insurance companies and Medicare have used risk adjustment extensively to get at this problem, and this needs to be addressed in the manuscript, since this phenomenon is already well described in the literature (for example, PMID: 10789599) and tools have been developed to address differences in risk. I commend the quasi-experimental study design, but have major issues with the conclusions drawn regarding unadjusted hospital readmission rates, which are rarely used currently for hospital performance standards. Better tools which incorporate risk adjustment are the new standard and this should be considered in this dataset.

**As discussed above, we have now incorporated risk-adjustment and have revised our paper accordingly. Thank you for the thoughtful suggestions.**  
  
*Reviewing: 4*  
  
Comments to the Author:  
Thank you for your analysis on the timely topic.  
I believe the argument about HRRP's disproportionate impact on safety net hospitals could be articulated more crisply. Additionally, which data source was used? Were Pt Unique Health Indentification Numbers recycled? How were multiple readmissions or readmissions to different hospitals addressed? Regarding the methodolgy, a difference-in-differences was used but to a degree both cohorts were impacted by the intervention. Also, it would be good to clarify why the during and post-reform time periods were chosen as opposed to pre and post. Finally, several limitations should be fleshed out including the unique contexts of MA that may make it less generalizable in the context of the ACA, pt acuity, types of admissions, amount some hospitals were spending at this time on readmissions reduction initiatives, etc.

**Thank you for the suggestions; we have clarified many of these issues.**  
  
*Reviewing: 5*  
  
Comments to the Author:  
Overview  
This is a fascinating subject with focus in this manuscript on an area of particular interest to hospitalists and with large implications for the national conversation given the relevance of the Mass insurance expansion to the ACA design.  My concerns rest in the alternative explanations for the authors’ findings which are not adequately investigated or elaborated in analysis and discussion.  
  
Major Issues  
There are several countervailing and reinforcing trends that may have occurred following the implementation of the Massachusetts coverage expansion which need to be addressed and if possible incorporated into the analysis.  
  
First, did patient crossover from the low insurance quartile to others occur following insurance expansion (ie did patients receiving care at the low insurance hospitals relocate their care to others as a result of improved coverage, and did the newly insured continue to seek care at the bottom quartile hospitals or did they receive care at higher quartiles once insured)? It would be very worthwhile to examine these trends and could be intended for separate publication, however in the absence of this consideration in this paper the observed increase in readmission rates in the bottom quartile hospitals is of unclear significance.

**This is an important and interesting question.** **It certainly is worth having academic clarity on these mechanisms. However, the purpose of our paper is to examine the empirical effect of the MA reform law on readmissions, because it provides some insight into what we may expect to come under the ACA. Thus, our focus is understanding the empirical landscape rather than teasing out the mechanism.**  
  
Second, it has been observed in other work around readmissions (Brock, Mitchell et al. 2013) that readmission rates may not accurately reflect overall utilization of hospital care if the underlying index admission rate is dynamic.  I would expect that to be the case in the setting studied in this manuscript, but there is no information provided regarding overall volumes of hospital care. One possible scenario is that readmission rates rose as avoidable index hospitalizations decreased, leaving a more morbid population behind.

**That is certainly an interesting question. In the paper, we discuss the Oregon Health Study’s findings that patients who are newly insured are more likely to be admitted. So we think it is unlikely that hospital admissions decreased. Regardless, it is an interesting possibility.**  
  
Related to both of these, would it be possible to use the health identifier number to distinguish the newly insured from the previously insured? Separate consideration of the newly insured might be illuminating as well.

**This is certainly an interesting question, but since our focus is on hospital-level rates, we chose not to pursue it.**

Minor Issues  
I would appreciate a brief note explaining what about the data supported the use of a nonparametric spline regression.

**This has been added to the methods section.**

The use of the term “study hospitals” in the second paragraph of the results is confusing to me.  It seems to me all Massachusetts hospitals are study hospitals for the work. “Highest uninsured hospital quartile” as is used elsewhere in the manuscript might be a more appropriate alternative.

**We have clarified this language.**