Dear Dr. McCormick

Manuscript ID BMJ.2014.022293 entitled "The Effect of Massachusetts Health Care Reform on Racial and Ethnic Disparities in Hospitalizations for Ambulatory Care-Sensitive Conditions: A Retrospective Analysis of Hospital Episode Statistics"

Thank you for sending us this paper, which we were pleased to have the chance to consider, and enjoyed reading. We recognize its potential importance and relevance to general medical readers, but I am afraid that we have not yet been able to reach a final decision on it. This is because several important aspects of the work still need clarifying.

We hope very much that you will be willing and able to revise your paper as explained below in the report from the manuscript meeting, so that we will be in a better position to understand your study and decide whether the BMJ is the right journal for it. Looking forward to hearing from you again and, we hope, to reaching a decision.

Deadline: Your revised manuscript should be submitted within 6 to 8 weeks

How to submit your revised article: Log into http://mc.manuscriptcentral.com/bmj and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision.

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You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript using a word processing program and save it on your computer.

Once the revised manuscript is prepared, you can upload it and submit it through your Author Center. When submitting your revised manuscript, you will be able to respond to the comments made by the reviewer(s) and Committee in the space provided. You can use this space to document any changes you make to the original manuscript and to explain your responses. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s).

IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission.

Many thanks again. We look forward to seeing your revised article within 6 to 8 weeks.

Yours sincerely

Jose Merino
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**Report from the BMJ’s manuscript committee meeting**

These comments are an attempt to summarize the discussions at the manuscript meeting. They are not an exact transcript. Members of the committee were: Elizabeth Loder (chair), Jon Deeks, (statistical consultant), Tiago Villanueva, Georg Roeggla, Rebecca Burch, Alison Tonks, Kristina Fister, José Merino

Decision: Request revision before decision

Detailed comments from the meeting:

Please revise your paper to respond to all of the comments by the reviewers. Their reports are available below. In your response to the reviewers and committee please provide, point by point, your replies to the comments made by the reviewers and the editors, and please explain how you have dealt with them in the paper. It may not be possible to respond in detail to all these points in the paper itself, so please do so in the box provided.

Please also respond to these additional comments by the committee:

1. The editors found the paper interesting but some of the international editors found it difficult to follow. Considering that The BMJ is an international Journal, the authors should strive to clarify the topic for an international audience, and provide greater background information. Why is the experience in Massachusetts so important? Why did you select New York, New Jersey and Pennsylvania as the comparators? Placing the research question in context will help readers appraise the results.

2. The most recent data in the study is 5 years old, and only extends 21 months after reform was initiated. Do you have more recent data with longer follow-up? There was a lot of chaos for several years after reform went through in Massachusetts, as patients, hospitals, doctors, and insurers were figuring out a new system. We would like to know the impact of health reform after a longer period of time (up to the present time, perhaps), when the system is mature.

3. As mentioned by one of the reviewers, you need to take into account the fact that there are a lot of variables other than insurance status that affect hospitalization, including access to primary care, culture, availability of specialists, etc. Massachusetts may have a very high bias toward specialty care rather than primary care, which is not the case in many other parts of the country. We are not sure how you could quantify this, but the wide availability of specialty and subspecialty care does strongly affect care plans and is likely driving a lot of hospitalization. The authors need to address the differences in health care cultures between states.

4. As a result of the reform, a new set of previously uninsured (or underinsured) patients may have gained access to specialty care and an increase in hospitalizations. But those patients may now be healthier. Hospitalization rates are not a health outcome, and may be problematic as a metric. One of the reviewers mentioned that mortality went down after reform, which suggests better care. Do you have mortality or other data to address this concern?

5. There were significant sociodemographic differences between Massachusetts and the other states at baseline. Based on the data presented, it seems there was less inequality in access to care before the reform was implemented. This raises the question whether the findings in Massachusetts can be generalized to other states. Can you please comment on these differences in the manuscript? The disparity problem seems much smaller and the numbers affected by the intervention in the intervention region compared to the control regions, so only a small affect would be anticipated.

6. Do you information about hospitalizations by type of hospital (safety net vs. not safety net; for profit vs. non-for profit; tertiary vs. community; academic vs.
not academic; etc.)?

7. The study did not find a difference over time. But there are some possible explanations that you should explore. You are looking at an indirect outcome, inpatient hospitalizations, as a proxy of the quality of outpatient care. If the outcomes are not identical (hospitalizations may actually reflect better care for some patients, for example, as discussed above) then the differences may hide any effect. Can you comment on the issue of using hospitalizations as a proxy and the possibility that the choice of proxy may hide the effect?

8. With all studies like this you also need to ask whether there are other temporal or geographical factors which would confound the association. Can you please comment?

IMPORTANT

When you revise and return your manuscript, please take note of all the following points. Even if an item, such as a competing interests statement, was present and correct in the original draft of your paper, please check that it has not slipped out during revision.

REFEREES COMMENTS

Reviewer: 1

Recommendation:

Comments:

TO THE AUTHORS:

This paper analyzes changes in ambulatory-care sensitivity admissions before and after Massachusetts' health reform, compared to several other states. This is an important policy question and adds to a large body of evidence on the MA reform experience.

The general study design is appropriate, though the authors have not done enough to demonstrate that:

a) the choice of control states was appropriate, especially given the markedly different racial/ethnic make-up and income in these states compared to Massachusetts. Did they test whether the pre-expansion trends were similar? That would be an important step. They should also seek to broaden their control group or find states with more similar demographic features, especially since disparities are an important focus of their study design.

b) how sensitive are their findings to the choice of functional form and how they model their standard errors;

c) The "DDD" model based on county uninsured rates isn't really an ideal approach, since even low-uninsured counties aren't a control group, as the DDD framework suggests. See my comments below with an alternative test that would be more useful.

Assuming that the results are similar even after accounting for these concerns, the authors need to be much more circumspect about their findings. The paper states numerous times that the MA reform has produced "modest changes" – this seems to be out of whack with many of the changes documented in the literature. See below for some of my thoughts on why this might be. For instance, cutting the uninsured rate in half in the state can hardly be described as "modest." More broadly, the authors make a very strong conclusion that access to care in the state didn’t improve, when they are looking at one fairly narrow outcome. Much broader outcomes affecting far larger shares of the population – such as whether one has a primary care doctor, can afford medical care, and see doctors for preventive health visits – show major changes in the state. The authors are generalizing far too broadly from this one measure, especially when their point
estimates are potentially consistent with reasonably sized population impacts.

Smaller comments below:

INTRO
The authors describe the gains in access to care under MA reform from prior studies as “modest,” which I think is not really accurate and understates the policy’s impact. Long & Masi (Health Affairs), for instance found that the number of people in the state without a usual source of care fell by more than one-third, and Van der Wies et al (Milbank) found that cost-related barriers fell by one-third. The fact that MA already had high coverage rates at baseline may obscure these relative changes, but the authors should not minimize this impact on those who were previously uninsured.

Your literature review also doesn’t mention the recent Annals paper (Sommers, Long, & Baicker) looking at mortality (all-cause and health-care related) before and after the reform, which would certainly qualify as an “objective” health measure.

Methods:
--How did you select a 21 month pre & post timeframe? What happens if you add extra data? Some changes from coverage expansion may take longer, and this would also increase your power. The sensitivity analysis with a shorter time frame is less helpful.

--If you have the county-level unemployment and income measures, just use those measures as the covariates – you lose a lot of information in collapsing them into two binary variables (high/low).

--do the data fit a Poisson distribution well? If there is evidence of overdispersion, a negative binomial model would be more appropriate. Also, it’s generally become standard in the econ literature to prefer linear models to non-linear models for diffs-in-diffs analysis (see Norton and Karaca-Mandic) to allow for straightforward interpretation of the coefficients, so this would be a helpful sensitivity analysis.

--the comparison between the high and low counties within Massachusetts doesn’t really make sense as a diffs-in-diffs model, since neither is a control group (they both were impacted by the policy, though potentially to a different degree). What would be more meaningful would be to stratify your control group in the other states similarly – compare high uninsured counties in MA to high uninsured counties elsewhere, and low uninsured counties in MA to low uninsured counties elsewhere.

--the authors provide no details on how they modeled the standard errors, which is an important issue in any diffs-in-diffs analysis, especially with a small number of states – see Bertrand et al QJE 2004.

-- Massachusetts is a referral center for New England more generally. If patients are coming to MA hospitals from outside the state, they will show in your dataset, yes – even though they aren’t eligible for the state’s health reform. This will bias your study towards not finding any impact.

Results:
--the descriptive statistics suggest that MA looked quite different from your control states, particularly for race and ethnicity – which is pretty troubling for an analysis focusing on racial/ethnic disparities. Income also looks very different, which is of course critical for such a study of access to care. How were these control states chosen? Why not broaden to other states with closer demographic match, and also to increase the precision of your estimates?
Did the authors formally test whether the pre-expansion trends were similar in your outcomes for treatment and control states which is the underlying assumption for the D-in-D model?

--Table 4 shows pretty large point estimates for ACSC changes among blacks compared to whites (-4 to -6%), though p-values were non-significant. Given that the policy extended coverage to 5-7% of the adult population, this could represent a really large impact on disparities, but the precision of the estimates are quite broad. Also, technically, I wouldn't call this a DDD – whites in MA aren't really a control group, are they?

--Linear models would be very useful here, since the poisson models provide relative change - but in linear models measuring absolute changes in ACSC, these might be statistically significant and look quite different from the Poisson results.

DISCUSSION

“Hence, our results using this measure do not suggest that overall access to outpatient care, or racial and ethnic disparities in access to such care, improved to a significant degree following implementation of Massachusetts health care reform.”

This is way too strong a statement. The abundance of evidence already shows that outpatient access to care under the state reform improved significantly – more people with a primary care provider, more able to afford their medical care, more with a preventive visit. I think a more accurate way to frame your findings would be to say, "For one specific hospital-related measure of outpatient access we don’t find any evidence of change after MA reform, unlike numerous previous studies of other access measures such as having a PCP, being able to afford care, and having a preventive health visit."

Limitations: If your study is underpowered (wide confidence intervals on the D-in-D that can’t rule out relevant population-level changes), then dropping individual control states only makes the problem worse, not better. You come back to this point eventually on p. 20, but given this concern – that there may actually have been large worthwhile changes included in your confidence intervals (and perhaps even your point estimate). Accordingly, much more tempered conclusions would appear to be in order. Also, the survey-based studies being cited are done with gold-standard national health interview data and similar sources; I’m hard pressed to imagine a source of recall bias large enough to explain all the evidence on MA reform’s improvements in access to care.

p. 20: The absolute changes being cited seem small only when you ignore the denominator. These correspond to cutting rates of people without a PCP by 30-40% - that’s a big deal. As the authors mention, the ACA itself is "only" going to increase coverage by 5-10%. Do the authors contend that coverage changes of that size are not particularly important?

p. 22: cutting the uninsured rate in half is hardly modest.

p. 24: the authors cite a study showing mortality reductions and access to care changes after Medicaid expansion. They do not mention the similar study in Annals of Int Med showing similar findings in Massachusetts, which would be worth comparing to the current results.

Additional Questions:
Please enter your name: Benjamin Sommers

Job Title: Assistant Professor
Reviewer: 2

Recommendation:

Comments:
This is a well-executed and well-written study showing that Massachusetts health reform did not improve rates of potentially preventable hospitalizations or disparities in these rates. The impact of Massachusetts health reform and of the Affordable Care Act on health outcomes has received too little attention and this study provides important evidence with a rigorous study design. Although the study design is strong, and the difference-in-differences analysis is robust, this analytic approach could be missing a post-reform *upward* trend in avoidable hospitalizations in Massachusetts, a potentially important finding. An aggregate interrupted time series analysis on a "differenced" plot (intervention group rate minus control group rate) would be stronger and would have the ability to detect such a trend. Otherwise, the authors should consider better tailoring the implications of this study to an international audience.

Major Comments:
1. The authors could improve on the difference-in-differences approach. On a related note, it would be beneficial for the authors to state their hypothesis in the Introduction. A plausible hypothesis could be that the influx of newly insured could cause poor/delayed outpatient access (survey data notwithstanding) and therefore cause increasing preventable hospitalizations. Figure 1b shows a (slight but clear) increase in the Massachusetts’ rate relative to the secular trend in the follow-up period, and it seems important to determine if this is a statistically significant trend increase. The study design is well-suited for using an aggregate interrupted time series approach (see Wagner AK et al. Segmented regression analysis of interrupted time series studies in medication use research. J Clin Pharm Ther. 2002 Aug;27(4):299-309).
2. Presumably the authors used a 3-way interaction term in their regression models to generate their estimates of impacts on disparities in "difference-in-differences-in-differences" analyses. This is acceptable and correct but it is very difficult to obtain "statistically significant" results with 3-way interaction terms, and this approach could be missing important effects among key subgroups. Stratifying results by e.g. race/ethnicity might be a better option. This would not directly test whether disparities changed, but would detect the impact of Massachusetts health reform on these subgroups.
3. While this study is of substantial importance to a US audience and US policymakers, the interest to an international audience or the generalizability to international settings is not as clear. The authors discuss this briefly but should enhance their discussion of implications for non-US settings. One angle might be to discuss that potentially suboptimal outpatient access in Massachusetts after reform is similar to some countries with universal health insurance coverage that have long wait times.

4. The authors had "insurance type" as a patient-level variable, presumably implying that they could detect rates of uninsurance at each hospitalization in addition to major types of insurance such as commercial/Medicare/Medicaid. This is important information that should be shown at baseline in Table 2 and potentially displayed as a before-after trend.

Middle Comments:

5. The authors should describe their creation of a rolling average in more detail. Presumably this means that, in a given quarter, the number reported represents that quarter averaged with 3 other surrounding quarters. Were those quarters before, after, or surrounding the quarter reported? If I understand their approach correctly, Figures 1a and 1b should *not* have corresponding x-axis intervals because the "raw" Figure 1a results will either include quarters that preceded or followed the 21 month baseline period. It would also be important to know whether the moving averages include quarters that preceded the 21-month baseline or fell within the phase-in period.

6. A prominent paper found that Massachusetts health reform was associated with lower mortality rates. Such a finding could be mediated by a reduction in preventable hospitalizations, yet the present study did not detect this pattern. This might be worth a brief mention.

7. Did the authors control for repeated events within individuals? Although somewhat unlikely, this could bias results if a given state is better than another at preventing readmissions. Perhaps the authors do have a unique patient identifiers across hospitals? If so, please state.

8. The authors presumably had the zip code of their subjects, but they do not list zip code in their list of variables that they obtained directly from hospital records.

9. Presumably patient zip codes were used to link patients to their county. Please explicitly state.

10. The authors should note higher up in the text (e.g. in Data Sources rather than in Analytic Data Structure) that many of their variables are assigned at the county level because this is the only level that has an annual denominator and because some key variables such as SAHIE and Area Resource File are at the county level. Otherwise readers would wonder why they did not use zip code to assess community characteristics. Please also briefly note the limitations (and strengths) of geographically assigned characteristics, including county level versus more granular levels.

11. Standardizing rates by age and sex is not the strongest approach and leaves something to be desired because it is adjusting for so few characteristics. I understand that bin sizes limit the ability to further standardize. While I do not think it is crucial, the authors could potentially use marginal predictions to generate rates that are fully adjusted for all covariates, not simply age and sex. Also, it would be helpful to have one sentence that clearly states where the standardize rates were used (plots and semi-adjusted results). Presumably the adjusted regression analyses did not use standardized rates as the dependent variable (because these could be adjusted for age and sex)? Please clarify.

Minor Comments

12. Abstract, Conclusions: While understanding that the finding of no change in preventable hospitalizations after Massachusetts health reform is not completely novel, I believe this important, top-level finding should be reported in the abstract conclusion.

13. Abstract, Conclusions: "Our findings do not suggest..." is a bit awkward. How about, "Our findings suggest that..... did not significantly lower..."

14. Abstract, Conclusions, last sentence: The end of this sentence is not clear. Putting the word "reduce" in front of "preventable hospitalizations" would clarify.
15. Page 6, line 6: "potentially" preventable.  
16. Use of the term "trends" in several places in the manuscript is confusing and might give the impression that the analysis is actually controlling for baseline trends (differing slopes between the groups) or analyzing changes in trends.  
17. Page 10, line 34: Should read "...(short-term diabetes complications, long-term diabetes complications, chronic obstructive ... etc)"  
18. Did the authors have hypotheses about how health reform would affect acute versus chronic preventable hospitalizations?  
19. "Difference-in-differences" with the "s" on the end should be used consistently throughout.  
20. How did the authors deal with out-of-state residents? Did they consider the possibility that Massachusetts health reform could influence in-state hospitalizations among out-of-state residents or vice-versa?  
21. Stryjewski has a recent HSR paper showing no impact of Massachusetts health reform on chronic disease outcomes.

Additional Questions:  
Please enter your name: Frank Wharam  
Job Title: Assistant Professor  
Institution: Harvard Medical School  
Reimbursement for attending a symposium?: No  
A fee for speaking?: No  
A fee for organising education?: No  
Funds for research?: No  
Funds for a member of staff?: No  
Fees for consulting?: No  
Have you in the past five years been employed by an organisation that may in any way gain or lose financially from the publication of this paper?: No  
Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this paper?: No  
If you have any competing interests (please see BMJ policy) please declare them here: None  

END  

Date Sent: 21-Oct-2014