6-Jun-2016

Dear Prof. Jena

Manuscript ID BMJ.2016.032144.R1 entitled "Adverse outcomes during the transition to a new electronic health record"

Thank you for sending us the revised version of your paper. We recognize the 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 because several important aspects of the work still need clarifying. Please see the comments from the reviewers below. In addition, please also consider these issues:

* Some of the editors have been experienced "live go dates" at several institutions and appreciate the problem. We also think that it may be relevant to a broad international audience. Some context is needed, however. You may consider adding a box explaining the reason (mandate) for the switch to EHRs in the US, different types of EHRs, etc. Also it is important to clarify who is in the Medicare population (see below)

* The study looks at macro outcomes: all cause mortality and the like. EHR implementation may lead to prescribing errors and other "minor" events that may not affect macro outcomes. Can you please address this limitation of the paper? Are any data available that can help address this limitation of the study?

* The omission of information on non-medicare patients and EDs seems very important. EDs aren't able to decrease their patient volume or postpone high acuity patient visits the way that can be done for elective surgeries, admissions, or outpatient clinics. Some of these limitations could be dealt with by changing the title to make clear this deals with a subset of medicare patients and by acknowledging these limitations more carefully in the abstract and paper.

* Another limitation is the time frame studied. Some hospitals may limit elective admissions and visits and also have "champions" on site in the first few weeks of implementation of an EHR. After some time volumes return to normal and champions leave. Does the study capture problems that may arise when this happens?

* This is an observational study (not a natural experiment) and hence use of "impact" should be cautious.

* The authors have to be more explicit and provide more details to convince the authors that there is no self-selection problem and that control and implementation hospitals are indeed similar. Was each implementation hospital within a separate HRR or were some hospitals controls for more than one case? If the latter, how was this addressed in the analyses?

We hope very much that you will be willing and able to revise your paper.

Yours sincerely,

Jose Merino
jmerino@bmj.com

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** Comments from the external peer reviewers**

Reviewer: 1

Recommendation:

Comments:
I would like to thank the authors for thoughtfully and thoroughly addressing my comments. I believe that this revised manuscript is stronger and will be of great interest to BMJ readers. The only loose end that I found was my comment #7 re: diffusion of innovation and its effect on EHR implementation. Namely, that hospitals adopting EHRs in the study period may have had a unique experience due to the insights gained by innovators and early adopters such that the results of the present study may not be generalizable to a hospital implementing in 2016--at a much later stage in the diffusion of innovation.

Additional Questions:
Please enter your name: Edward Melnick

Job Title: Assistant Professor

Institution: Yale School of Medicine

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: Yes

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:

Reviewer: 2

Comments:
There are several points/queries to make in addition to those of the reviewers and still of concern in the revised manuscript:

1. Although the authors call this a natural experiment it is still observational and hence use of impact should be tempered. There was no association found and this was not different to control hospitals over the same period. However, there is a self-selection problem and no guarantee that the implementation hospitals are not different.

- Suggested changes to wordings are for example : objectives (study cannot assess impact), conclusions (no overall negative 'association' were found) and what study adds (cannot evaluate how 'affected', nor the 'impact').

2. Was each implementation hospital within a separate HRR or were some hospitals controls for more than one case? If the latter, how was this addressed in the analyses?

3. Model as defined within statistical analysis section: Which factors are considered as random effects and which fixed? The 'Covariates' term may be misleading as this will lead to more than one beta value (whereas here it is quite clearly only one- beta4).

Please verify that clustering within the same HRR is accounted for as well as clustering of patients within hospitals and admissions from the same patient over time. The description sounds as though fixed effects were used, but then this is given as a sensitivity analysis. Beta3 also requires a further subscript I think? Removing the equation and giving a written description may be clearer.

Hierarchical logistic regression, with clustering of admissions within patients, patients within hospitals and hospitals within HRR, adjusting for covariates (as given in table 1) and MDC (how many terms does this entail? what are the categories?) should be used to model the probability of the outcomes (mortality, readmission, adverse event) to determine changes over time (pre to post implementation date) according to whether the hospital was an EHR implementer or control. An interaction term between the time and EHR indicators quantifies the difference-in-differences.

4. Not enough information is given to replicate the power calculation. What is the anticipated starting percentage and is any account taken of ICC (different admissions for the same patient or within the same hospital/HRR)?

5. Table 1: How useful is it to present significance tests pre-post within the study and control groups, especially given the large numbers of patients and hence significance of unimportant clinical differences. For example, it is not surprising that the race breakdown does not differ from pre to post and a relatively small difference in the % females is highly statistically significant. Of more interest perhaps is the fact that the control hospitals tended to have older and/or more white patients who didn't stay as long on average and who had a different diagnostic distribution. How might these differences in patient mix affect interpretation of the results? At the very least, there should be discussion of the generalisability of results given this selection bias.

Additional Questions:
Please enter your name: Angela Wade

Job Title: Professor of Medical Statistics

Institution: UCL Institute of Child Health

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

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c. Introduction: This should cover no more than three paragraphs, focusing on the research question and your reasons for asking it now.

d. Methods: For an intervention study the manuscript should include enough information about the intervention(s) and comparator(s) (even if this was usual care) for reviewers and readers to understand fully what happened in the study.

To enable readers to replicate your work or implement the interventions in their own practice please also provide (uploaded as one or more supplemental files, including video and audio files where appropriate) any relevant detailed descriptions and materials. Alternatively, please provide in the manuscript urls to openly accessible websites where these materials can be found.

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i. For a clinical trial: Absolute event rates among experimental and control groups; RRR (relative risk reduction); NNT or NNH (number needed to treat or harm) and its 95% confidence interval (or, if the trial is of a public health intervention, number helped per 1000 or 100,000.)
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iii. For a case control study:OR (odds ratio) for strength of association between exposure and outcome.
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v. For a systematic review and/or meta-analysis: Point estimates and confidence intervals for the main results; one or more references for the statistical package(s) used to analyse the data, eg RevMan for a systematic review. There is no need to provide a formal reference for a very widely used package that will be very familiar to general readers eg STATA, but please say in the text which version you used. For articles that include explicit statements of the quality of evidence and strength of recommendations, we prefer reporting using the GRADE system.

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g. Footnotes and statements

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Date Sent: 06-Jun-2016