9-Jan-2018

Dear Prof. Jena

Manuscript ID BMJ.2017.041937 entitled "Physicians' Political Preferences and the Delivery of End of Life Care: An Observational Study"

Thank you for sending us your paper, manuscript # XXX entitled "YYY" We sent it for external peer review and discussed it at our manuscript committee meeting. We recognise 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 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. We are looking forward to reading the revised version and, we hope, reaching a decision.

Please remember that the author list and order were finalised upon initial submission, and reviewers and editors judged the paper in light of this information, particularly regarding any competing interests. If authors are later added to a paper this process is subverted. In that case, we reserve the right to rescind any previous decision or return the paper to the review process. Please also remember that we reserve the right to require formation of an authorship group when there are a large number of authors.

Yours sincerely,

Georg Roeggla
groggla@bmj.com

Report from The BMJ's manuscript committee meeting

These comments are an attempt to summarise the discussions at the manuscript meeting. They are not an exact transcript.

Manuscript meeting 21.12.2017

John Fletcher (chair), Richard Riley (stats), Elizabeth Loder, Wim Weber, Jose Merino,: Rubin Minhas, Georg Roggla, Tiago Villanueva.

Decision: Ask for Revision.

The committee was interested in the topic of your research. The following concerns were mentioned:

- This is difficult to understand from an European view. Many doctors treat individual patients together, and there is no reason to believe that they have similar political views.
- Non-US citizen may have difficulty appreciating the RQ.
- Please specify a clear clinical message.
- What a shame this didn't make it to the Christmas edition!
- The number of doctors who donate, though, is small. Compared to non-donors, Democrat or Republic physicians are significantly older, so could it be a sign that they're more established and in a better position to donate? 42% of non-donors are under 40 while 19% and 14% are Democrats and Republicans.
- Interesting differences between Rep and Dem in terms of proportion of doctors who attended top schools and in their geographical distribution and types of hospitals they work in. How convincing is

this ascertainment of this proxy exposure? A physician could make a donation out of sympathy or admiration for a certain party, but that doesn't mean necessarily he / she has a concrete political preference or that he / she will go vote.

- Is end-of-life care the best example for you to make your point? We can see that physician attitudes to practice might track political persuasion for a few select questions like abortion but we also would hope that professionalism would also mean most physicians would put aside their own personal views when treating their patients.
- How are hospitalists assigned to patients? In our experience, this depends on the time of the admission: who is admitting that shift?
- Interesting issue about religious affiliation of the hospitals. This may have a greater impact on the level of end-of-life care. Often, in the EOL situation, groups other than the hospitalists are involved with decisions on care: ethics consultants, palliative care consultants, intensivists, etc. These groups may steer families toward specific decisions, and they may in turn be influenced by the affiliation of the hospital. But in the past we have accepted papers from this group that look at the characteristics of hospitalists and clinical outcomes even when they coordinate but not provide all care. To what extent is EOL care determined by patient and family preferences rather than by physicians?
- There is a clear divide among physicians regarding allocation of resources, social justice, value of different payment modes, role of the state in care, etc.
- Does the academic affiliation of the hospital matter? Are there differences in terms of who staffs which hospitals (for example, older physicians in private practice more likely to staff smaller rural hospitals, etc.) It seems there is a difference ("Patients treated by Republican physicians were more likely to be hospitalized in small, Southern, for-profit, and rural hospitals than patients treated by non-donor or Democrat physicians.")
- We suspect that in the US those that donate are more likely to vote, and those that donate are more partisan. In the current polarized atmosphere, we think it will be rare for individuals to make donations to both parties. This is probably more common in Europe.
- Are financial donations a good way to identify political preferences? You now only assigned a preference to ~ 10% of doctors, which substantially decreases statistical power. We would guess that more than 10% would have a clear political orientation. Should you have asked all doctors anonymously about this?
- Please answer to the concerns in the statistical review.

First, please revise your paper to respond to all of the comments by the reviewers. Their reports are available at the end of this letter, below. Please also respond to the additional comments by the committee.

In your response please provide, point by point, your replies to the comments made by the reviewers and the editors, explaining how you have dealt with them in the paper.

** Comments from the external peer reviewers**

Reviewer: 1

Recommendation:

Comments:
This is a great paper, which I really enjoyed reading. I think it will be a great contribution to BMJ. The richness of the data collected by the authors is striking, and I wish similar data were available in European countries where the political commitment of physicians is also very large. The article is clearly written, and the results and methods convincing. I have listed below some comments/suggestion for sections that may need improvements or clarifications.

1) The authors assume that patients are randomly allocated to physicians when they arrive at the hospital, and check that patients' characteristics do not differ across care providers. They make a convincing case for their quasi-randomization approach. However, the patient population they consider (end-of-life elderly people) is very specific. Usually, these patients present multiple comorbidities and complex care needs, which raises the need for specific skills. Moreover, these patients usually are

"frequent" hospital care users as they experience multiple visits per year. In my opinion, both reasons make it more likely that specific physicians treat them in the hospital. The authors show that in their sample, democrat physicians are more likely to be younger and to have attended top 20 medical schools. Could it be assumed that they have different (better?) skills than other physicians, and therefore end up dealing with these complex cases more often? I think that the authors should provide additional arguments to explain how this may impact their results.

2) I think that a convincing robustness check would be to run propensity score matching analyses, e.g. run the analyses among a subsample of patients where the potential selection bias has been reduced. That analysis would be similar to the approach developed by the authors, but would have the advantage to control for the influence of patients' individual characteristics in the selection process;

3) The authors do not consider the potential influence of care insurance networks membership on their outcomes, which may mitigate their results. I am not a specialist of the US health insurance market, but couldn't it be assumed that some specific networks like Kaiser Permanente promote life care approaches that are closer to democrat preferences? In that case, patients and physicians could choose to be affiliated with specific networks because they reflect their political views and care preferences, which may lead to a selection bias. I would be interested in getting the authors' opinion on how this may impact their results.

4) Would it be possible to get similar data from the VA? It would be a nice control arm to have in the study, assuming that political views are less likely to influence care decisions within that specific population.

5) Authors use data spanning from 2008 to 2012, which is a period of large political changes in the US (especially for health policies). However, the article does not explore to what extend the political context (at both Federal and state levels) is likely to influence physicians' preferences. Indeed, it could be assumed that democrats were confirmed in their ideas after the '08 and '12 elections (especially within swinging states), or when the ACA was adopted. I think that the authors should control for time fixed-effects, and for interactions of state-level and time fixed effect in order to adjust for potential differences.

6) The authors mention that they use multivariable linear regressions to explore the impact of political preferences on spending, use of care, and hospice discharge rates. Recently, the dominant approach to modeling health care expenditures has been the use of generalized linear models (GLM). These models are very flexible as they allow specifying the conditional mean function directly. In other words, no retransformation is required, which makes the estimates more accurate. Why not using that approach?

Additional Questions:
Please enter your name: Thomas Rapp

Job Title: Associate Professor

Institution: Université Paris Descartes - Sorbonne Paris Cité

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

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Reviewer: 2

Recommendation:

Comments:
This article uses data on hospitalizations of elderly U.S. Medicare beneficiaries linked to political contribution data of general internists responsible for those hospitalizations to ask whether the political ideology of a physician is associated with end-of-life treatment patterns for patients who are near the end of life. The basic motivation of the study is that physicians are highly political and that these beliefs may influence the care that they provide patients. For example, in the U.S. physicians have varying views on health policy, e.g., single payer vs market based health care, and recent survey work suggests that physicians may also have different views on how patients should be treated for certain conditions that are often politically charged (e.g., related to contraception, gun ownership, etc.). To date, no empirical analysis has attempted to link data on physician political ideology to actual treatment patterns. The basic finding of the paper is that physicians who have donated to the Republican vs Democratic party do not differ in various measures of end-of-life care: end-of-life spending among hospital decedents, use of aggressive services like gastrostomy tubes and intubation/ventilation among hospital decedents, and referral to hospice among patients who are hospitalized and are discharged alive but at high predicted risk of short-term mortality.

The study is highly innovative, important, and timely. The idea of using physician political contribution data to proxy for ideology is clever and motivated by prior work documenting physician political polarization by several of the study authors. The methods are also strong. The empirical strategy is to focus on hospitalized patients who are treated by Republican vs Democratic physicians within the same hospital, under the assumption that patients do not chose their inpatient general medicine physician (frequently a hospitalist physician) and vice versa. This generates a quasi-random experimental study design that address a potential concern that Republican physicians may treat patients who differ in their own care preferences compared to Democratic physicians. Consistent with the quasi-randomization assumption, the characteristics of patients treated by Republican vs Democrat physicians are remarkably comparable. Given the study design, I am inclined to believe that at least on this measure of end-of-life care provided in the inpatient setting, that political ideology of a physician does not affect their practice pattern. These findings may even be reassuring given the political environment in the U.S. currently.

The authors perform a host of robustness checks. For example, in their hospice referral analysis, they look at patients who die within a pre-specified period of hospital discharge, as well as looking at people who they predict are at high mortality risk following discharge (but who do not necessarily die within a pre-specified period after hospitalization). A prerequisite of their analysis is that patients seem to be similar between Republican and Democrat physicians, which they convincingly show.

My main recommendations are around framing and interpretation of the problem. For international physicians, it might be helpful to provide more evidence (if it is exists, which it may not) of the political polarization in other countries. A quick google search returned some evidence of political polarization in the UK but data on other countries was not readily found. The authors may speculate whether political preferences may influence care provided in other nations as well, e.g., I would think that this issue could be relevant among conservative vs. liberal physicians in other countries as well.

Also, it's worth emphasizing further that the setting that the authors look at - inpatient general medical care – is a specific setting. That said, I think it is a really important setting to consider given the high mortality risk associated with inpatient hospitalization, the fact that hospitalizations are common among the elderly, and the possibility that the relative importance of physician preferences towards care may actually be greatest in settings where a strong longitudinal relationship of a physician and patient does not exist (a point worth making in the paper). Different findings may emerge when looking at primary care relationships or among doctors who frequently care for patients with high-mortality conditions in the outpatient setting (e.g., oncologists). But this is the first study of its kind, and so I think that the limitation of studying the inpatient setting is not a substantive issue. The authors might offer some direction, however, on what future analyses could do were data available. For example, if data on pediatric care were available, future analysis could look at differences in HPV vaccination rates between republican vs democratic pediatricians.

Additional Questions:
Please enter your name: Eric Sun

Job Title: Assistant Professor

Institution: Stanford University

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

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Reviewer: 3

Recommendation:

Comments:
It is refreshing to see this analysis in an era where ideological divides dominate social policy and cultural discussions. Academic researchers exacerbate these tensions by self-selecting sensational studies that corroborate these divisions.

The present study is a dispassionate examination of whether affiliation affects performance in one area where we might expect differences to emerge. The use of hospitalists to deal with self-selection is clever and appropriate.

There are some issues that could be addressed to help strengthen the paper:

First, I think the authors could do a better job motivating their research design. In particular, the need to explain why a "fixed effects" model is so important. They might consider that the audience might appreciate that different end-of-life care patterns may emerge between Catholic-affiliated hospitals and secular facilities, for example, and show that their model adjusts for that fact. Perhaps a cross-sectional figure showing the relationship between EOL spending and %Democratic, by hospital, would show an interesting correlation and could help motivate the design.

Second -- and relatedly -- it would be interesting if these differences DO EMERGE once the authors exclude the hospital fixed effects. That is, suppose the model undergirding Figure 1 didn't have hospital fixed effects and did show a relationship. That might be interesting to note as well.

Third, political preference here may just be a noisy signal for other preferences that matter more -- especially religous preferences -- and we might expect end-of-life practices to differ across groups. This is acknowledged in the Limitations section, but only obliquely, and I think more discussion is warranted.

Finally, we can't dismiss the possibility that such differences would emerge in other types of care. One might expect obstetric and neonatal care, for example, to show differences. Clearly Medicare data are poorly suited to that purpose. All this is by way of saying that we can't rule out ideological beliefs affecting the practice of care as a result of this one study. The authors say this, but perhaps they can do a little more discussion of places we should be investigating rather than just saying "Further research is needed" on end-of-life care.

Ultimately though, the study suggests that the vast majority of doctors in the United States practice medicine in observational equivalent ways, regardless of political affiliation. This is a comforting finding about the professionalism of the practice of medicine in the United States, and the BMJ readership should be interested.

Additional Questions:
Please enter your name: Dana Goldman

Job Title: Distinguished Professor of Public Policy, Pharmacy, and Economics

Institution: Univ of Southern California

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

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Reviewer: 4

Recommendation:

Comments:
Thank you for asking me to review this cohort study on an interesting topic. The authors have clearly worked very hard to produce this work and manuscript. On reviewing from a statistical perspective, I have identified the following areas for improvement and clarification:

1) There is no detail about how missing data were handled, for example in regards to the political affiliation or the adjustment factors. Usually in studies of this type, where routine data are used, missing data are common and this should be handled appropriately in the analysis (e.g. multiple imputation, rather than complete case analysis). Please can the authors clarify this matter. A related point: in table 3, how many patients and physicians were used in each analysis?

2) "Patient demographics, comorbidities, and admission diagnoses were similar across groups (Table 2 and eFigure 1), with statistically significant differences being clinically unimportant" – the clinical importance is not relevant; rather it is whether they are important confounders.

3) Physician groups are very unbalanced at baseline, e.g. see table 1 and the % female, the mean years since residency, and the mean age. Indeed, there are only a few covariates actually summarised here. There is a concern, therefore, of immense confounding. Can we trust the comparisons across physician groups are not due to residual confounding? The authors find no evidence of an association. If there had been a significant difference, the concern of confounding would have led us to play down the findings. I believe we should apply the same principles, even though there is no association being implied by the authors.

A related point is that "Physician covariates included age, sex, and whether the physician attended a top-20 medical school according to U.S. News and World Report" – thus only 3 adjustment covariates were at the physician level (plus hospital). I do not find this satisfactory. It may be a consequence of the limited routine information about the physicians available.

3) "Total unadjusted inpatient spending among 51,621 patients who died in hospital was $18,353 for Democrat physicians, $17,091 for Republican physicians, and $17,271 for non-donor Physicians" – why are totals given and not means? The totals depend on the number in each group. I think they probably do provide means here, but wrongly say totals.

4) "After adjustment for patient and physician covariates and hospital fixed effects, physician political affiliation was not associated with end-of-life spending for patients who died in hospital or within 30-, 60-, or 90-days of discharge. For example, for inpatient deaths, total end-of-life spending was $17,420 (95% CI 16,673 -18,168) among Democrat physicians and $17,675 (95% CI 16,692-18,859) among Republican physicians (p=0.68)." – these adjusted results are not interpreted well. The adjustment makes them conditional on the hospital, covariate values, etc – so to give an overall number without conditioning on a particular set of covariate values is not appropriate. This must be addressed.

5) Percentages are referred to as rates (e.g. see page 15). It is not clear why. Rates are number per person year, for example. Risks are %s by a particular time-point.
However, the patients are followed up for different lengths of time, and some are discharged, and therefore rates (in the truest sense) would be more appropriate than %s. What are the rates of ICU care for example for each group, and what is the adjusted rate ratio between groups?

6) Are the findings consistent across hospitals (i.e. is there heterogeneity in the estimate of association across hosptials)?

7) Abstract says: "Patient demographics and clinical characteristics were similar between groups" – but, crucially, table 1 suggests the physician characteristics were not similar at all. This should also be mentioned.

8) Abstract and elsewhere the focus is on giving the mean spend for each group, and a p-value for their difference. Yet, the estimate of the mean difference and a CI is more important for comparison. If the CI is wide, then it may contain important differences as plausible. This must be addressed. E.g. see the results 'intensive end-of-life treatments' sections, where the focus in mainly on p-values and group-specific results. We need estimated differences and their CIs in the tables and text too.
A related point is that the abstract concludes that there ' Physician political affiliation is not associated with the intensity of end-of-life hospital care that patients receive" – this is a strong statement. Better to say that there was no evidence in this particular study of an important difference between groups (if that is true, based on the width of the CI for the difference).
9) Methods: "We therefore analyzed differences in adjusted 30-day mortality from date of hospital admission, by physician political affiliation" - it is not clear how this analysis was done however.

10) The authors often say there was 'no difference' simply because the p-value is not significant. E.g. "There were no differences in rates of intensive end-of-life treatments between Democrat (38.0%), Republican (40.6%), and non-donor physicians (40.3%) (joint p=0.07, Democrat versus Republican p=0.13)" – clearly here there are observed differences, and therefore the statement of no differences are wrong. Differences are perhaps small, but we need CIs to verify this. This applies in many places.

11) How were the adjusted %s obtained in Table 3 and 4? This must be conditional on adjustment covariates? Similarly, in Figure 1, 'adjusted' total spending is not defined.

In summary, in the context of an interesting piece of research, there are many areas of potential concern and areas for improvement. I think the BMJ would need the authors to respond to the above comments and make additional improvements / analyses as necessary, before a full evaluation can be made. Regardless, I hope my comments help the authors improve their hard work going forward.

Best wishes, Richard Riley.

Additional Questions:
Please enter your name: Richard Riley

Job Title: Professor of Biostatistics

Institution: Keele University

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

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