Author's response to reviews

Title: Health Status After Cancer: Does It Matter Which Hospital You Belong To?

Authors:

Jon H. Fiva JHF (j.h.fiva@econ.uio.no)
Torbjørn Hægeland THD (thd@ssb.no)
Marte Rønning MRO (mro@ssb.no)

Version: 2 Date: 6 May 2010

Author's response to reviews: see over
RESPONSE TO REVIEWER 1, BRETT SHEPPARD

Before I go through the revisions you have suggested for our paper, I wish to thank you for your valuable comments. In the following I will rewrite the specific comments from your report in italics and then explain the changes we have made to incorporate them in the paper.

Major compulsory revisions

1) One element of the results involves differences across catchment areas, however, the paper states that this does not necessarily correlate with treatment hospitals. Therefore, conclusions about patients outcomes and employment status can’t be made based on catchment area because they were not treated there.

Our response:
We have devoted two new paragraphs in the empirical approach section where we discuss in detail why we believe it is inappropriate to conduct the analysis at the level where the hospital where patients actually were treated. The key point is that non-random selection or sorting of patients into hospitals based on characteristics that we do not observe and simultaneously affect outcomes, may bias the results. Our approach avoids this bias. At the same time, the interpretation of our quality indicator is slightly different (see the discussion at p. 4 in the paper).

Since the hospitals are anonymized in our data, the data do not reveal to what extent the catchment area hospital corresponds to the treating hospital. In a future project for which we are awaiting more detailed data, we intend to study this in more depth.

2) The exclusion criteria of age limits the interpretation of the results. The conclusion would be strengthened if the exclusion criteria became those without pre-diagnosis employment, instead of age.

Our response: Whether we rely on this alternative exclusion criterion do not impact our main findings. We have elaborated our results from specifications where the exclusion criteria are those without pre-diagnosis employment in paragraph 7 of subsection of ‘variation in quality of care’.

3) Table 2 and 3: Statistical values are difficult to interpret. If these results could be simplified, and expressed in a more widely known term such as an Odds Ratio or a p value, it would help with understanding the results.

Our response: Since we employ a linear probability model, the values in Tables 2 and 3 have straightforward interpretation as the all-else-equal differences in the survival/employment probability between the actual category and the reference category. E.g., in Table 2, column 4, the coefficient 0.0480 for Female implies that women, all else equal, have a survival probability that is 4.80 percent higher than men. The standard error of this estimate of this coefficient is 0.0047, and the three asterisks denote a p-value of less than 0.01. We have updated the text with a similar explanation, and also improved readability of the tables by making some of the variable names more understandable.
4) Please separate out the discussion from the results section. For example paragraph 5 under Variation and Quality of Care, compares the findings with the literature, which should be reserved for the discussion section.

**Our response:** The manuscript is updated accordingly.

5) If you could stratify the population by disease severity, the results would be more meaningful. For example, comparing a patient with widely metastatic disease versus someone with very localized disease, should dramatically affect their future employment.

**Our response:** In our baseline specifications we include control variables for the severity of disease, and we find that these are strongly associated with positive outcomes. More severe diseases do indeed dramatically affect future employment. An alternative way to hold the impact of severity of diseases constant is to stratify the sample, as you suggest. In the revised manuscript, results from this exercise is reported in the last paragraph of the results section. Our main results concerning the importance of different outcome measure is confirmed, but also some new insights are obtained, which we discuss in the last paragraph of the discussion section.

6) Overall statistical analysis is difficult to understand. This is a clinical paper, so reference to the article that defined the equation is appropriate (so that if readers are really interested in the statistics behind it they can read the original article), so some of the definition of the actual equation could be left out. More importantly, you could then include more of a summary of WHY you used this equation, what its strengths are, and why it was appropriate for your analysis. Also, by doing this it may make the results tables more understandable.

**Our response:** Our strategy is to view the outcome of a cancer patient as depending on characteristics of the disease (e.g. the degree of metastasis at the time of diagnosis), characteristics of the patient (e.g. the age of the patient) and the hospital to which the patient belongs. In addition, general time trends and random variation may affect the outcomes. To isolate the effect of quality of care we base our analysis on the model presented in the paper and apply multivariable regression techniques. An important strength of this approach is that we can hold constant factors that are likely to affect survival and future employment, when studying variation in quality of care. We rely on a very rich and comprehensive dataset, containing not only detailed information on diagnoses, but also detailed characteristics of patients with respect to socioeconomic characteristics and labor market status before illness. Failing to control for such factors will attribute (un)favourable patient or disease characteristics to quality of care. We have updated our “empirical approach” section and hope that the strength of our empirical approach is better communicated.
Minor compulsory revisions

1) Figure 4 - seems that the legend is mislabeled

Our response: The manuscript is updated accordingly

2) Please define all abbreviations with their first use in the manuscript.

Our response: The manuscript is updated accordingly.

3) please define phrases such as somatic hospital and dummy variable

Our response: The manuscript is updated accordingly.

4) please address syntax

Our response: We have polished the paper and corrected some language issues
RESPONSE TO REVIEWER 2, RYAN SPENCER

Before I go through the revisions you have suggested for our paper, I wish to thank you for your valuable comments. In the following I will rewrite the specific comments from your report in italics and then explain the changes we have made to incorporate them in the paper.

Major Compulsory Revisions

1) On page 5 of the manuscript the authors disclose that, “more than 70 percent of the total number of cancer patients is excluded from the analysis” due to restrictions placed on age. While the study population is still > 46, 000, my concern is the ability to appropriately generalize the study to the clinical setting and to compare the merits of the proposed quality indicator – post diagnosis employment – with traditional quality indicators (i.e. survival as suggested by the authors) if such a large majority of cancer patients is excluded from analysis. The reason for the exclusion is clearly laid out by the authors but no mention is made as to the potential biases/consequences to the results or conclusions of using “return to employment” as an appropriate quality indicator for patients diagnosed with cancer.

Our response: You raise an important point. Our analysis compares the two indicators for the set of patients where both are relevant – the working-age population. Our main finding is that only using indicators based on survival may be insufficient, and that indicators based on different outcomes may capture quality differences in different parts of the health status distribution. To do the similar analysis for other age groups would require another measure of health status for the whole population. Unfortunately, we do not have such a measure available. Whether our findings may be generalized to the whole population of patients depends critically on the extent to which indicators are invariant with respect to patient and disease characteristics (which will typically vary with the age restrictions of the sample). The data we have available does not permit a comparison between indicators based on survival for the working-age population and the whole population, since (hospital catchment) area of residence is only available for the working-age population.

2) In the introduction, the authors state that “in our empirical analysis, [we] assign patients to the hospital they belong (i.e. to their hospital catchment area) rather than to the hospital(s) they were actually treated at.” The authors then write in their conclusions that “it may actually matter which hospital you belong to” due to “large differences in outcomes.” It seems difficult to draw conclusions about a hospital or health-care system’s quality measures if you are using outcomes for patients who should have been treated at a particular hospital rather than patients who were actually treated at that hospital. Would it not have been possible or appropriate to have conducted the empirical analyses based on the hospital patients were actually treated at?

Our response: We have devoted two new paragraphs in the empirical approach section where we discuss in detail why we believe it is inappropriate to conduct the analysis at the level where the hospital where patients actually were treated. Since the hospitals are anonymized in our data, the data do not reveal to what extent the catchment area hospital corresponds to the treating hospital. However, evidence from other sources suggests that the majority of
patients are treated locally. In a future project for which we are awaiting more detailed data, we intend to study this in more depth.

Additionally, how can we be sure that there are not variables intrinsic to the regions of poorer performing hospitals that account for these differences (i.e. socioeconomic status, urban vs. rural, etc)?

**Our response:** In our empirical analysis we investigate how robust our estimates are to inclusion and exclusion of our large set of covariates capturing important patient characteristics. Since our estimates are insensitive to a large battery of observable (patient and disease) characteristics, they are unlikely to change much if we could control for potentially relevant unobservable characteristics (see Altonji et al.). However, in the absence of experimental data, or some natural experiment, the worry of a potential selection bias cannot be totally resolved.

3) Since there is now “free hospital choice,” what effect do the authors believe that this has on the quality indicators measured in this study and how are these quality indicators still applicable to the hospital catchment areas currently?

**Our response:** Free hospital choice (conditional on availability) may alter the sorting and selection of patients into hospitals, although we argue in the paper that there may have been some degree of sorting into hospitals even before the reform. Over time, free hospital choice may have two effects. First, patients are less tied to their local hospital, and it may matter less which hospital you belong to, i.e. the quality differences as we estimate them may be reduced. Second, free hospital choice and publishing of quality information may induce competition between hospitals, leading to quality improvements and/or low quality hospitals being driven out of the market. We discuss these issues in the last paragraph of the Discussion section. Since Vrangbæk et al find that the increase in sorting of patients following the reform is modest, we believe that the results we present are likely to be relevant also today.

- **Minor Essential Revisions**

4) Figures 1 and 2: It may be helpful to the reader to have a note stating that the catchment area rank as noted along the ordinate axis is listed from top (#55 appears to be assigned to the best performer) to bottom (#1 assigned to the worst performer) based on post hoc analysis of performance in that variable (survival rate or employment rate).

**Our response:** We have added the table note that you suggest. We have also reversed the ranking across the vertical axis, such that rank #1 relate to the best performer, and the rank #55 refers to the worst performer, which is more intuitive than vice versa.

- **Discretionary Revisions**

5) The use of e.g. in paragraph #2 of discussion and in paragraph #1 of conclusion. In the majority of instances “e.g.” is used in parentheses, however in these two instances they are not and the phrasing may be awkward for some readers.

**Our response:** We have rephrased these sentences.