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THE LONG-TERM IMPACT OF MEDICARE PAYMENT REDUCTIONS ON PATIENT OUTCOMES

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ABSTRACT

This study examines the long term impact of Medicare payment reductions on patient outcomes using a natural experiment - the Balance Budget Act (BBA) of 1997. We use predicted Medicare revenue changes due to BBA, with simulated BBA payment cuts as an instrument, to categorize hospitals by degrees of payment cuts (small, moderate, or large), and follow Medicare patient outcomes in these hospitals over a 11 year panel: 1995-1997 pre-BBA, 1998-2000 initial years of BBA, and 2001-2005 post-BBA years. We find that Medicare AMI mortality trends stay similar across hospitals when comparing between pre-BBA and initial-BBA periods. However, the effect became measurable in 2001-2005: hospitals facing large payment cuts saw increased mortality rates relative to that of hospitals facing small cuts in the post-BBA period (2001-2005) after controlling for their pre-BBA trends. We find support that part of the worsening AMI patient outcomes in the large-cut hospitals is explained by reductions in staffing level and operating cost following the payment cuts, and that in-hospital mortality is not affected partly due to patients being discharged earlier (shorter length-of-stay).

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1. Introduction

The relationship between provider payment generosity and quality has been a concern of health policy researchers and policy decision-makers for many reasons. There has been wide belief that providers/hospitals are operating beyond the "flat of the curve" where additional care/spending may have no health benefits or can actually harm patients' wellbeing. Therefore, reductions in the provider payments may not necessarily hurt quality. However, concerns that reductions in provider payments will reduce quality also have strong support. The public backlash against managed care and the fact that, more recently in Dec 2010, President Obama signed a piece of legislation to delay the implementation of a 23% Medicare payment cut to doctors that was scheduled to take effect in June 2010 by a year, are direct results of the claim and fear that provider payment cuts will lower quality.

Whether or not a cut in provider reimbursement will lead to worse quality does not have a straightforward answer, because it will depend on the size of the cuts, the level of payments, the incentives imbedded in the payment methods, the production efficiency of the provider, the payment generosity of other payers, and other provider and market factors. For example, a hospital facing large Medicare payment reductions can "cost shift" to private payers by charging private health plans higher prices, thus limiting the potential adverse effect on quality. Alternatively, hospitals facing increased financial pressure may try to become more cost efficient, by cutting administrative and/or indirect costs without hurting the true quality of care. Yet it is still possible that some hospitals facing large reductions from payers will have to reduce patient care processes that can lead to worse patient outcomes.

The empirical literature generally has not found a *long-term* adverse effect of hospital payment reductions on patient outcomes, drawing the experiences from Medicare payment reductions due to Prospective Payment System (PPS) and that from private payment reductions due to the rise and growth of managed care. However, we argue that it is important to reconsider the issue in the context of BBA context for several reasons. First, Medicare BBA reductions occurred after a sustained period of low private revenue when there are less "fats to trim," unlike the case of PPS which took place when both private and Medicare payments were both high prior to the cuts, or unlike the case of managed care which cut private payments when Medicare reimbursements were relatively generous. Therefore, hospitals having to operate on very thin margins throughout the 1990s may not be able to absorb all the (substantial) cuts from BBA. Second, the relationship between Medicare payment and patient outcomes depends on the degree of cost shifting. Cost shifting may be more likely to occur around the PPS in early 1980s when the hospital environment was less competitive, thus moderating the degree of financial pressure. In contrast, a hospital's ability to cost shift may be more limited in the more competitive late 1990s environment. A recent study shows that hospitals on average shifted a limited 21% of BBA payment reductions to private payers between 1996 and 2000 (Wu 2010). Therefore, there is a real possibility that patient outcomes may be affected. Third, at the aggregate level, the effect of payment cuts on quality might not be immediate as quality changes are the result of rearrangements of resource allocation. Therefore, it is important to examine the impact over a longer period of time.

This study uses a plausible exogenous source of Medicare payment reduction – the Balanced Budget Act of 1997- to study the long-term effect of provider payment cuts. The BBA serves as a great instrument because it is an exogenous shock to the hospitals, and its wide provisions affect a heterogeneous group of hospitals that experienced large Medicare cuts including major teaching hospitals, hospitals that serve disproportionately low-income, high-cost, or Medicare patients. We also fill in a very important gap in the empirical literature, which has very limited understanding on the impact of BBA and has not examined longer term patient outcomes beyond 30-day mortality or beyond one year after major BBA cuts. Using an instrumental variable approach with difference-in-difference-in-differences (DDD) design, we find evidence that Medicare patient outcomes became worse as a result of BBA, and the adverse effect was not measurable until several years after the policy took place. We also explore the potential mechanisms in hospital operations that lead to the worse patient outcomes. The results appear to indicate a resource-constraint story: the higher Medicare AMI mortalities seen in large cut hospitals are likely the consequence of lower nurses and staff employed per bed, and the substantially less financial resources (operating costs per bed) utilized. Although largest-cut hospitals did try to substitute some Medicare patients with private patients and reduced length-of-stays, the degree of these behavioral responses are small.

2. Background and Literature Review

The fiscal pressure from both the Congress and the Clinton Administration to reduce the rate of growth in Medicare spending and the concern that Medicare might be overpaying providers in the mid-1990s led to the enactment of the Balanced Budget Act (BBA) of 1997 (Newhouse 2002). The BBA was instituted to reduce Medicare inpatient payments by about 40 billion between 1998-2002 (Guterman 2000), generating the largest public payment reduction since the introduction of Prospective Payment System (PPS). Although a later legislation, the Balanced Budget Refined Act (BBRA) in 1999, reversed some of the cuts, it was reported that about 35% of US hospitals' profit margins fell to negatives by 2000 as a result of BBA (Bazzoli, Lindrooth et al. 2005).

Historically, the hospital sector has experienced three major payment reductions: Medicare payment reduction due to the switch of cost-based payment to Prospective Payment System (PPS) in early 1980s, the reduction from private payers due to the rise and growth of managed care in the 1990s, and again another major Medicare payment cuts in the late 1990s due to Balanced Budget Act of 1997 (BBA). Due to the significance of these events, there is a large volume of literature examining the effects of these payment reductions on different dimensions of quality. Evidence from the switch to PPS provides that hospitals did decrease length of stay (Newhouse and Byrne 1988; Hodgkin and McGuire 1994; Cutler 1995) and service intensity (Feder, Hadley et al. 1987), and patients were discharged with more unstable conditions (Kosecoff, Kahn et al. 1990). However, a series of other papers show that while in-hospital and short-term mortality rates might have increased, there is no effect on 1-year mortality in the years immediately following PPS (Kahn, Keeler et al. 1990; Kahn, Rubenstein et al. 1990; Rogers, Draper et al. 1990) or over more extended periods (Staiger and Gaumer 1992; Cutler 1995; Shen 2003). There are also ample studies on the effects of managed care, and the results appear to have reached a consensus: the growth and expansion of managed care has resulted in lower hospital payments, fewer admissions and expensive services/procedures, without compromising patient outcomes (Miller and Luft 1994; Miller and Luft 1997; Miller and Luft 2002).

In contrast to the prior 2 major events, there are significantly fewer studies examining the impact of Medicare BBA cuts, and these results reveal a mixed picture. There is evidence that some aspects of the patient care process were hurt. For example, a collection of studies have shown that BBA has adversely affected hospitals' financial conditions, and hospitals with worse financial conditions cut back on investments in infrastructure, nursing staff, patient support services, patient safety and quality enhancing activities (Bazzoli, Lindrooth et al. 2005; Bazzoli, Clement et al. 2007; Lindrooth, Clement et al. 2007; Bazzoli, Chen et al. 2008; Zhao, Bazzoli et al. 2008). However, studies examining patient outcomes directly find no or minimal impact on surgical or all patient 30-day mortality in 2001 as a result of Medicare BBA cuts (Schwartz, Peterson et al. 2004; Volpp, Konetzka et al. 2005; Seshamani, Schwartz et al. 2006; Seshamani, Zhu et al. 2006). A recent study by (Kaestner and Guardado 2008) shows that up to 10% change in Medicare reimbursement as a result of geographic re-classification has no effect on nurse staffing or patient outcomes. Taken together, prior literature suggests that some aspects of patient care may be cut, but there is no short-term effect of Medicare reimbursement cuts on patient mortality up to one year after effective BBA provisions (year 2001).

The review of literature shows surprisingly little research examining the effect of Medicare reimbursement cuts on quality due to BBA. This study adds to the literature by studying the *long-term* impact of Medicare payment cuts on hospital quality. We use two methodological designs to address the endogenous problem between Medicare revenue and mortality. First, we simulate a "revenue loss" variable due to BBA reimbursement changes, and use it to instrument for changes in actual Medicare revenue. Second, we examine the impact of this instrumented Medicare revenue loss on patient mortality using a pre-post difference-in-differences (DD) estimation. We compare the change in mortality among large-cuts hospitals before and after BBA, relative to the pre-post change among small-cuts hospitals. In essence we implemented a DDD model as we use a long 11-year panel with 3 study periods - 1995-1997 (pre-BBA), 1998-2000 (initial-BBA), and 2001-2005 (post-BBA), so that we can more effectively control for any systematic differences in pre-trends between hospitals that may explain the differences in observed post-trends. We provide details about the data and the methodology in the following sections.

3. Data

Our main dependent variables are Medicare risk-adjusted Acute Myocardial Infarction (AMI) mortality rates, calculated each year from MedPAR Medicare claims data between 1995 and 2005 (details below). Data on Medicare revenues and discharges by payers are obtained from Medicare hospital cost reports, maintained by Healthcare Provider Cost Reporting Information System (HCRIS). We also use PPS Payment Impact Files for variables needed to simulate Medicare DRG price reductions between 1996 and 2000 due to the BBA of 1997. Information regarding proposed and actual changes in DRG payment updates, payment formulas for Indirect Medical Education (IME), DSH and OUT came from series of MedPAC reports and Federal Register (1998-2001). The actual HMO enrollment data between 1995 and 2005 at the Metropolitan Statistical Area (MSA) level are from InterStudy. We use instrumented HMO penetration between 1996 and 2000 in the first stage regression where the instruments are (1) the size distribution of firms obtained from County Business Pattern and (2) the percentage of workers who are white collar obtained from Area Resource Files. Finally, we gather hospital characteristics from American Hospital Association annual surveys between 1995-2005.

Our study sample comprises of urban hospitals that operated continuously between 1996 and 2000 for which we can validly instrument the impact of Medicare payment cuts due to BBA. We include urban hospitals only because the HMO data are for MSAs only. The sample is further restricted to hospitals with at least 20 AMI admissions in a year because CMS regulations stipulate that the hospital-level mortality rates are only publicly released for hospitals with at least 20 AMI admissions per year. We also exclude hospitals whose change in Medicare revenue per discharge between 1996 and 2000 are outliers (fall in the top and bottom 1% of the distribution) in the first stage regression, because reported revenue data are highly skewed. These exclusions yield a final sample of about 1,400 MSA urban hospitals between 1995 and 2005 for the regression analyses, for a total of 13,926 observations.

4. Empirical Methodology

4.1 Instruments for Medicare revenue change

An empirical concern in estimating the effect of Medicare financial pressure is that the observed change in Medicare revenue is endogenous, because hospitals may respond to the BBA cuts by upcoding diagnostic groups or inflating patient charges for higher outlier payments. We use an instrumental variable exploiting the exogenous variation in changes in Medicare reimbursement as a result of policy change following Shen (2003), Dafny (2005) and Wu (2010). We use two instruments: "BBA bite" and share of Medicare discharges in 1996 to capture two exogenous sources of variations in Medicare payments. The "BBA bite" captures policy changes in unit DRG price and is calculated in several steps. We first simulate a Laspeyres DRG price index given the provisions in the BBA while holding inputs at the pre-BBA (1996) level. Because the BBA was passed in 1997 and hospitals may start to respond strategically immediately, we fix DRG input values at the year prior to BBA. We then generate a BBA "bite" to capture the cumulative effect of BBA between 1996 and 2000, which measures the difference between the hypothetical DRG price had there been no BBA and the simulated DRG price index (see Wu, 2010 for the detail construction of the BBA "bite").

Medicare revenue is a function of DRG price and Medicare patients (volume). Clearly, hospital's Medicare volume can change in response to the payment change. Therefore, the second instrument we use is the share of Medicare discharges fixed in 1996 level. The BBA "bite" variable and the 1996 Medicare share of patient load are used together to instrument for actual changes in Medicare revenue between 1996 and 2000. Change in actual Medicare revenue is calculated as the difference between actual Medicare revenue per total discharge and the hypothetical Medicare revenue that is computed by updating the actual Medicare revenue per total discharge in 1996 to 2000 using market basket. This calculation assumes that in the counterfactual world, Medicare revenue will go up by a full market basket update between 1996 and 2000. Change in Medicare revenue is divided by total discharges to obtain the average Medicare income per discharge. The two instruments, "BBA bite" and share of Medicare discharges in 1996, will serve as valid instruments if they affect Medicare patient outcomes only through their impact on Medicare revenue. These first stage regressions are specified as the following:

$$\Delta\ (\textit{Actual Medicare Revenue/total discharges})_{96\text{-}00} = \alpha\ \text{``}\textit{BBA Bite}\text{''} + \beta\ \textit{Share of Medicare Discharges}_{96} + \gamma_i\ \textit{Exogenous Variables}_i$$

It is worth noting that changes in DRG prices due to BBA vary across hospitals. The BBA cut inpatient payments by holding the DRG annual updates well below the market basket growth, and by altering payment formulas to indirect medical education (IME), disproportionate share hospitals (DSH), and outlier adjustments. Therefore, the variation in the total BBA payment reduction comes from both differences in DRG price cut and Medicare share across hospitals. Teaching hospitals, for example, may experience a greater DRG price cut than others due to major reductions in IME. However, because major teaching hospitals don't typically serve large numbers of Medicare patients, the total BBA impact is moderated by a hospital's Medicare share to render it comparable to other hospitals that serve predominately Medicare patients. Therefore, BBA generates a heterogeneous group of hospitals to experience similar degrees of Medicare payment cuts, thus making BBA a great instrument.

4.2 Estimation Model

To estimate the effect of the change in Medicare reimbursement on mortality, we implement a hospital fixed-effects model and use the following specification in the second stage:

$$Mortality_{it} = \alpha + \beta_1\ Large\ cuts*1998\text{-}2000 + \beta_2\ Large\ cuts\ *2001\text{-}2005 + \beta_3\ Moderate\ cuts*1998\text{-}2000 + \beta_4\ Moderate\ cuts*2001\text{-}2005 + \beta_5\ Control\ variables + Hospital_i + \sigma_t\ Year_t + \varepsilon_{it}$$

where *i* indexes hospitals, *t* indexes year. The coefficients of interest are β_1-β_4. Instead of estimating a linear effect using the predicted Medicare revenue changes from the first stage, we categorize the size of BBA cuts into large, moderate, and small cuts groups based on the predicted change from the first stage. “Large Cuts” is the dummy variable that takes on the value of one for hospitals that experienced large cuts in Medicare payment (i.e., they are in the top quartile of instrumented Medicare revenue reductions due to BBA), and “moderate cuts” is the dummy for hospitals in the 25^{th} to 75^{th} percentile of the instrumented Medicare revenue loss. We also define three distinctive periods so we can estimate whether the effect of payment cuts on mortality and other dependent variables changes over time. Specifically, 1995-1997 is defined as the “pre-BBA” period and is the reference group. The period 1998-2000 is the “initial BBA” period when BBA inpatient payment cuts were effective, and 2001-2005 is considered the “post-BBA” period. .

With the inclusion of hospital and year dummies, the interaction between "large cuts" and the "1998-2000" period dummy (i.e., coefficient, β_1) identifies the change in patient mortality rates between pre-BBA and immediately after BBA periods for hospitals that experienced large cuts in payment and compares that difference to the group of hospitals that only experienced small payment cuts based on the instrumented revenue change (the omitted group). One should note that the main effect of period dummies and payment cut categories are not included because they do not vary over time for each hospital, and our identification of the payment cut effects comes through comparing changes across periods and across the payment cut categories. This is essentially a DDD estimation approach. Similarly, β_2, allows us to examine whether the difference in mortality rate trends between pre- and post-BBA periods is the same between hospitals that experience small and large payment cuts. We use moderate cuts as yet another experiment group, and the interaction terms are identified similarly and have parallel interpretations. The hospital's fixed-effects control for idiosyncratic time-invariant hospital characteristics that may affect the change in mortality systematically. Year dummies control for secular time trends in each year. We also add in several time-varying hospital and market variables that are important in determining the change in mortality rates (details in the control variable session below). Bootstrap standard errors are produced for the estimates' hypothesis testing in the second stage regressions.

Dependent Variable

Our main dependent variables are risk-adjusted AMI mortality rates among Medicare patients, measured as death within hospital, within 7 days, 30 days, 90 days, and one year after discharge. These were aggregated hospital-specific outcome measures derived from patient-level regressions that included hospital indicators and fully interacted patient demographic covariates (five age groups, gender, race, and urban or rural residence) as well as 17 comorbidity measures to control for severity of the illness following the methodology in prior work (Skinner and Staiger 2009). In other words, instead of using the actual percentage of patients who die in each hospital as the outcome measure, we use these risk adjusted hospital intercepts, which represent the mean value of outcomes for each hospital holding patient characteristics constant.

To explore the potential mechanisms of how patient mortality may be affected, we examine the level of staffing, operating cost, and length of stay. Nurse staffing levels have been shown to be directly associated with patient outcomes, so we include both full-time equivalent (FTE) registered nurses (RN) and licensed practical nurses (LPN). We also examine the total staffing levels (FTE). To capture overall resources or inputs utilized at a hospital we examine changes in total operating cost per 1996 bed. Hospital beds are fixed at the 1996 level to avoid a potential endogeneity problem between hospital bed (size) and patient outcomes. Because a patient's mortality at discharge is a function of how long the patient stays in a hospital, we couple our analysis on in-hospital mortality with that on length-of-stay. Because BBA cuts DRG price and thus makes private patients more profitable at the margin, hospitals can potentially substitute Medicare patients with more private patients. We examine the effect of BBA on percent total discharges by Medicare and by private payers.

Control Variables

Medicare Case-mix. Patient mortality is a function of patient severity. We explicitly account for AMI patient severity in calculating the risk-adjusted mortality rates. In addition, we control for the general severity of Medicare patients in a hospital - the Medicare case-mix index- in the second stage regression.

Hospital bed sizes and occupancy rates. Hospital size can be correlated with the economy of scales in production and/or efficiency. We use log of staffed beds to control for such characteristics. We use occupancy rates to add additional control for actual use of beds.

Public, teaching, and for-profit (FP). Literature has suggested that public and major teaching hospitals tend to have different levels of quality, thus we include a dummy for each ownership type. While it is controversial whether quality differs by for-profit or not-for-profit, we still include FP to control for potential differences in FP and NFP hospitals that may affect quality. A major teaching hospital is defined as a hospital with resident-to-bed ratio equal to or greater than 0.25.

Hospital system. Systems can provide more resources such as management expertise and information infrastructure that can affect quality. Hospital systems can also affect quality by altering the competitive environment of health plan-hospital negotiation. The variable is an indicator of whether a hospital belongs to a hospital system.

HMO penetration and hospital HHI. Literature suggests that the health plan-hospital negotiation depends in part on both the prevalence of managed care plans in the market and the number and size of competing hospitals in the market where the plan is purchasing services. Therefore, we include HMO penetration as measured by the percent of population enrolled in a HMO plan in a MSA. Hospital market structure is measured by a hospital-specific Herfindahl-Hirschman Index (HHI). This index is derived using actual zip code level patient flow data from Medicare discharge data (MEDPAR file) to calculate the concentration of patient admissions, without imposing a geographic boundary for each hospital market (Melnick, Zwanziger et al.

1992).

5. Results

Figure 1 shows the year trend in the following risk-adjusted mortality rates: in-hospital, 7-day, 30-day, and 90-days. We plot the trend separately for hospitals that experience the largest cut and the smallest cut. The two vertical lines separate the figure into 3 panels: pre-BBA (1995-1997); initial BBA (1998-2000), and post-BBA (2001-2005) periods. Since we normalized the mortality rates, the average value for the entire period is 0 in all plots. In general, the mortality rates exhibit the downward trend during the study period. The in-hospital mortality rates appear to have similar trends between the two categories of hospitals in the first 2 periods. But as we examine mortality rates of longer time horizon, the trend starts to diverge between the two categories. The best illustration is perhaps the trend in 30-day mortality: hospitals with the smallest and largest cut started out with similar level of 30-day mortality rates in 1995 (in fact, hospitals with the largest cut have a slightly lower 30-day mortality rate). While hospitals with the smallest cut exhibit a downward trend especially in 2001-2005 period, hospitals with the largest cut essentially exhibit a flattening trend. The divergence in the trend is also evident when examining the 90-day mortality rates.

The natural question following figure 1 is whether there are systematic differences between the two groups of hospitals. To shed light on this issue, table 1 presents the summary statistics of the study sample and for the largest cuts (top 25th percentile) and smallest cuts (bottom 25th percentile) subsamples by pre-BBA and post-BBA periods (for clarity of presentation, we omit the column for the 1998-2000 period but results are available upon request). Average unadjusted AMI mortality rates for the largest cut hospitals remained at 0.24

percent between pre- and post BBA periods, while AMI mortality rates for hospitals with small payment cuts reduced from 0.23 to 0.21 in the same period. In general across all periods, large-cut hospitals are more likely to be major teaching hospitals and less likely to be FP or public hospitals. Differences in most characteristics are small except for market competition variables. In the pre-BBA period, large-cut hospitals on average have the same Medicare case-mix, higher occupancy rate (by 3 percentage point), and are 5 beds bigger relative to the smallest cut hospitals. Large-cut hospitals are facing more intense market competition, in that these hospitals are less likely to have hospital system affiliation and are in areas with higher HMO penetration and lower hospital concentration.

Most of the changes in hospital characteristics over time are parallel between large- and small- cut hospitals except in the following cases. The large-cut hospitals switched from having relatively sicker to healthier patients (case-mix) and from slightly higher to lower occupancy rates compared to the smallest cut hospitals between pre- and post-BBA periods. Large-cut hospitals have remained the same bed size while the small-cut hospitals have grown in their size. The large-cut hospitals also have substituted 3-4% total discharges from Medicare to private payers. However, most of these changes do not seem to explain why AMI mortality would be widening.

We formally control for the change in characteristics between hospitals in our regression models. Table 2 shows the 2nd stage results where the dependent variables are the AMI mortality rates (first stage regression results are provided in the Appendix). We use two tests to examine the validity of our instruments. The first-stage F-statistics is 22, indicating that the instruments are indeed significant predictors of the actual payment change. We performed the Sargan over-identification tests to evaluate whether the instruments are uncorrelated with the error term, and in all models, we failed to reject the hypothesis of no correlation. As discussed in the methods section, our variables of interest are the three BBA payment cut categories (i.e., whether a hospital is subject to small, moderate, or large cuts due to BBA of 1997, where the reference group is the small cuts category) and their interactions to two time periods (initial BBA years 1998-2000, and post-BBA years 2001-2005). The interaction terms allows us to explicitly test whether the effect of BBA cuts changes over the course of the study period. Table 2 shows that there is no statistically significant difference in the mortality rates for all hospitals immediately following the implementation of BBA, between 1998 and 2000 compared to their 1995-1997 pre-BBA trend: none of the coefficients on the BBA cut variables and their interactions to the initial BBA period is statistically significantly different from zero. In other words, the mortality trends are statistically the same between pre-BBA and initial-BBA periods across the three payment-cuts categories. However, the adverse effect of BBA becomes significant in the post-BBA period (2001-2005). Specifically, hospitals that belongs to the category of the biggest Medicare cuts experienced increased mortality rates in the 2001-2005 period relative to their mortality trend in the pre-BBA period (1995-1997) when comparing the same trend to the reference hospitals (those with small cuts): the coefficient ranges from 0.8 to 1.6 percentage point increase ($p<0.01$) except for in-hospital mortality. Similarly, hospitals with moderate cuts also have increased mortality rates in the post-BBA period, although the magnitude is smaller and only significant for 30-day or longer mortality rates (0.7-0.8 percentage points, $p<0.05$)

In Table 3, we explore the possibility that the worse quality after the initial BBA period for hospitals that experience the largest cut might be due to reductions in staffing levels. We find support for this hypothesis. Specifically, among hospitals that experience large payment cuts due to BBA, the level of registered nurses has the largest reduction compared to hospitals with small or average cuts: the level went down by 17 FTE RNs (p<0.01) during the initial BBA period, and the downsizing trend continues in the 2001-2005, with the RN level further decreased by another 3 bodies (p<0.1). These hospitals' total full-time equivalent personnel also decreased compared to the other hospitals in the initial BBA period. We find a consistent story when examining changes in cost per bed—compared to hospitals in the small cut categories, hospitals in the large cut category have substantial reduction in operating cost relative to their pre-BBA level. The largest cut hospitals have $20,000 (p<.001) less operating cost per bed in the 1998-2000 period and $70,851 less (p<.001) in the 2001-2005 period compared to the change in the smallest cut group.

In addition, we explore the changes in length of stay to understand the different results we obtained in Table 2 between the in-patient mortality rate and the other mortality rates post discharge. The results on length of stay suggest that part of the reason we observe no effect of BBA cuts on in-hospital mortality is that patients being discharged earlier (shorter length-of-stay by 0.20 and 0.26 days in 1998-2000 and 2001-2005, respectively, p<0.01) are in hospitals with the largest cut. We explore further whether hospitals facing large cuts were substituting Medicare patients with more profitable private patients by examining share of patients that are Medicare and private as dependent variables. We find evidence that the largest cut hospitals replace some Medicare patients with private patients, as the percent of Medicare discharges go down by 4% and 6% in the 1998-2000 and 2001-2005 periods, respectively, and the percent of private discharges move up by about the same amount. Overall, our results indicate that although largest-cut hospitals did try to substitute some private patients for Medicare and reduced length-of-stays, the degree of these behavioral responses are limited.

Sensitivity Tests

There are possible unobserved factors that can confound our findings. We ran a battery of sensitivity tests to explore whether the adverse effect we observe can be explained away by factors unrelated to the payment policy (all results are available upon request). To investigate whether the results are due to the fact that patients admitted to the big cut hospitals are getting sicker over time (i.e., the change in patient health distribution is systematically different across cut categories), we regress average Medicare AMI patients' ages, a good proxy for the overall health status of the admitted patient population (Shen 2003), at a hospital on the same specification. The results show that change in AMI patient age are about 0.5 year older in the large cut hospitals in post-BBA period compared to pre-BBA period, relative to the small-cut hospitals. Because the age difference is small and that we have explicitly controlled a full set of interaction terms between 5 categories of age, gender, and race, bias due to differential age trends should be limited.

Similarly, to rule out the possibility that small cut hospitals improved quality over time relative to large cut hospitals due primarily to volume-outcome effect (i.e., large cut hospitals might reduce their Medicare patient volume as the result of payment cut because these patients are no longer profitable compared to pre-BBA period), we regress Medicare AMI cases and total discharges on the same model. We observe that large-cut hospitals have reduced Medicare AMI admissions by 17 and 24 cases in 1998-2000 and 2001-2005 periods, respectively. However, there is no significant difference in total discharges by the degree of BBA cuts. Given that total discharges are similar across hospitals, it is likely that total AMI cases are similar across cut groups. Taken together, although there is evidence that large-cut hospitals have slightly older patients and slightly lower Medicare AMI volume over time, biases from these differences are unlikely to explain away the sizable gap in the outcomes we observe.

In another test to get the pure exogenous variation in Medicare revenue cuts, we use the instruments themselves to categorize hospitals into the payment cuts categories instead of the instrumented revenues. Because the variation in the instruments should be purely exogenous, this will ensure that the identifying variation is indeed driven by financial pressure and not by changes in resources. The results are indeed very consistent.

6. Discussion

Our results indicate that Medicare BBA payment reductions lead to a widening gap in Medicare AMI patient outcomes by the degree of payment cuts. We find that Medicare AMI mortality rates, measured at discharge, 7 days, 30 days, 90 days, and 1 year post-discharge, between large-cut and small-cut hospitals have similar trends prior to and immediately after BBA was implemented, but began to diverge between 2001 and 2005. The findings that adverse patient outcomes were not detectable in the years immediately following BBA (i.e., between 1998 and 2000) were consistent with the existing literature that finds no or limited impact on selected patient mortalities up to year 2001. Our exploratory analysis on staffing and operating cost shows that hospitals respond to the BBA cut fairly quickly by reducing operating cost per bed immediately after the BBA took effect, and such effort involved reduction in staffing. However, there is a delay between payment reductions and their ultimate impact on quality, through changes in resources available for patient care and hospital operation. Hospitals might still have some cushion in their operations in late 1990s, so that the effect of initial cuts in costs/nurses/staff might not be measurable in patient outcomes. However, further cuts in the 2001-2005 period can start to cut resources for needed patient care and thus affect patient outcomes.

Our regression model use hospital fixed effects to control for fixed hospital characteristics that contribute to consistent differences in patient outcomes across hospitals. We also control for time-varying hospital and market environment variables that may cause the change in patient outcomes to diverge by the payment categories. In addition, we tried to rule out confounding factors through a series of sensitivity analysis as discussed at the end of the results section. However, there are still possible unobserved factors that can confound our findings, and we highlight two that are particularly noteworthy here. First, there are still some small-scale legislated cuts in Medicare inpatient reimbursements between 2001 and 2005, and these cuts are correlated with our BBA "bite."[1] Therefore, part of the effect we estimated could be due to the cuts in 2001-2005, if one assumes that these cuts can affect mortality immediately. However, because the magnitude of these reductions is relatively small compared to the BBA cuts, most of the estimated adverse effect should be attributed to BBA cuts rather than the cuts in 2001-2005. This argument, if true, is consistent with our main finding that Medicare payment cuts can adversely affect Medicare quality, but would mean that our estimated adverse effect is biased upwards slightly. A second potential confounder is that if hospitals began to have differential quality improvements since 2001 that is beyond controlled in our model, and the degree of improvement is inversely correlated to the amount of BBA payment cuts, our estimate can be biased upward. However, we are unaware of studies that document this relationship and therefore leave this as an area of future research. Another limitation of the study is that due to CMS regulation, we can only examine hospitals with at least 20 Medicare AMI admissions annually. So our results might not be applicable to very small hospitals (the excluded hospitals

[1] The annual update factor was reduced by 0.55 percentage point in FY2002 and FY2003, total DSH payments for all hospitals were cut by 2-3% in FY 2001 and FY2002, and outlier payments were reduced slightly in FY2004 and FY2005 due to an increase of threshold for reimbursable costs.

represent less than ¼ of the urban hospitals). In addition, rural hospitals might behave different from urban hospitals and our sample excludes rural hospitals.

To understand the magnitude of the adverse effect in the post-BBA period, we can convert the coefficient estimates into elasticity. The average Medicare revenue cut due to exogenous factors is 15% at large-cut hospitals. The coefficient estimates for this group of hospitals indicates they have 0.8 to 1.6 percentage points higher AMI mortality relative to that of small-cut hospitals. These translate to about 5% to 7% percent increase in mortality rates.[2] Taken together, the elasticity is about -0.45, implying a 1% reduction in payment would translate to a 0.4% increase in mortality rates. The elasticity for moderate-cut hospitals in the post-BBA period is about similar around -0.38, given that the average Medicare revenue cut due to exogenous factors for this group is 8% and the effect on mortality is 3% to 4%. These elasticity estimates are very consistent with prior literature that finds short-term adverse effects of Medicare payment reductions (Shen 2003). These calculations are meant to illustrate our main points that with our plausible exogenous identification strategy, pre-post comparison, and the consistent estimates between large- and moderate-cut groups, the significant finding on adverse patient outcomes is unlikely to be completely explained away by potential confounders discussed in the previous paragraph.

Our results also demonstrate that inpatient mortality rate, while highly correlated with 30-day mortality rates, is not a very good measure of hospital quality. In-hospital mortality rate is a function of length of stay, which is more easily subject to a hospital's/provider's discretion. Similar to the findings in many studies, we find that payment reductions can induce providers to shorten length-of-stay. Our findings suggest that using in-hospital mortality rates as the quality

[2] Take 90-day mortality rate, for example. The baseline 90-day mortality rate is 23 percent, so the 1.35 percentage point increase we observe in the post-BBA period among large-cut hospitals compared to small-cut hospitals would translate into 5.6% increase (1.35/23=0.056).

measure when evaluating a payment policy effect on hospital care quality will likely miss the true effect of the policy.

The results in our study point to an important area of future research on whether there are spillover effects across payers within a hospital. Most theoretical and empirical literature assumes that there is a common level of quality across payers (Cutler 1998; Glazer and McGuire 2002). To the extent that this theory is true, the adverse effect of Medicare payment reductions will extend to patients of other payers. It is also important to consider whether there is another direction of spillover from private reimbursements to Medicare patient outcomes, as hospital reimbursements from private payers have increased substantially since the early 2000's while Medicare inpatient reimbursements are still kept at below market rates (White 2008; Stensland, Gaumer et al. 2010). If there is no cross-subsidization from private to Medicare patients, there might potentially be a wedge in quality between private and Medicare patients if the payment discrepancy continues to grow.

7. Conclusion

Overall, our study provides evidence that Medicare payment reductions due to the BBA of 1997 lead to worse Medicare AMI patient outcomes, and more importantly, that the adverse effect of this widening gap only became measurable several years after the policy took place. Medicare is currently operating in a difficult environment where private reimbursements and treatment costs are increasing rapidly after a decade of relatively slow growth. Given the growing fiscal pressure in this current environment, Medicare will most likely reduce hospital/provider's reimbursements further, although it is unclear the magnitude of the cut. While it is possible that payment cuts might just force hospitals to be more efficient without compromising patient care, it appears that average hospitals no longer have the financial cushion that allow them to maintain quality, and our study suggests that future substantial Medicare reimbursement cuts will likely have adverse consequences on Medicare patient outcomes. Furthermore, Medicare payment reductions may have the unintentional consequence of widening the gap in quality across hospitals or even between Medicare and private patients within the same hospital. It would be important for future research to explore payment incentives and mechanisms that could reduce the widening gap.

Appendix

First Stage Regressions for Medicare Revenue IVs

	Δ (Actual Medicare Revenue/discharge)$_{96\text{-}00}$
IV - BBA Bite	0.12*
IV - Medicare Share†	-873**
Medicaid Index, 1998	-809*
Δ Medicaid Index, 98-00	81
Δ Medicare Casemix Index	949**
Occupancy Rate, 1996	77
Hospital HHI, 1996	667**
Teaching Hospital	-88
FP Hosptial	-21
Public Hospital	19
HMO Penetration, 1996	-1256**
Δ HMO Penetration, 96-00	384
Constant	2804
Observations	1679
R-squared	0.38
First-Stage F Statistic	22

Note: 1. The models additionally control for dummies of 7 hospital bed categories and Health Referral Regions (HRR).

2. Standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%

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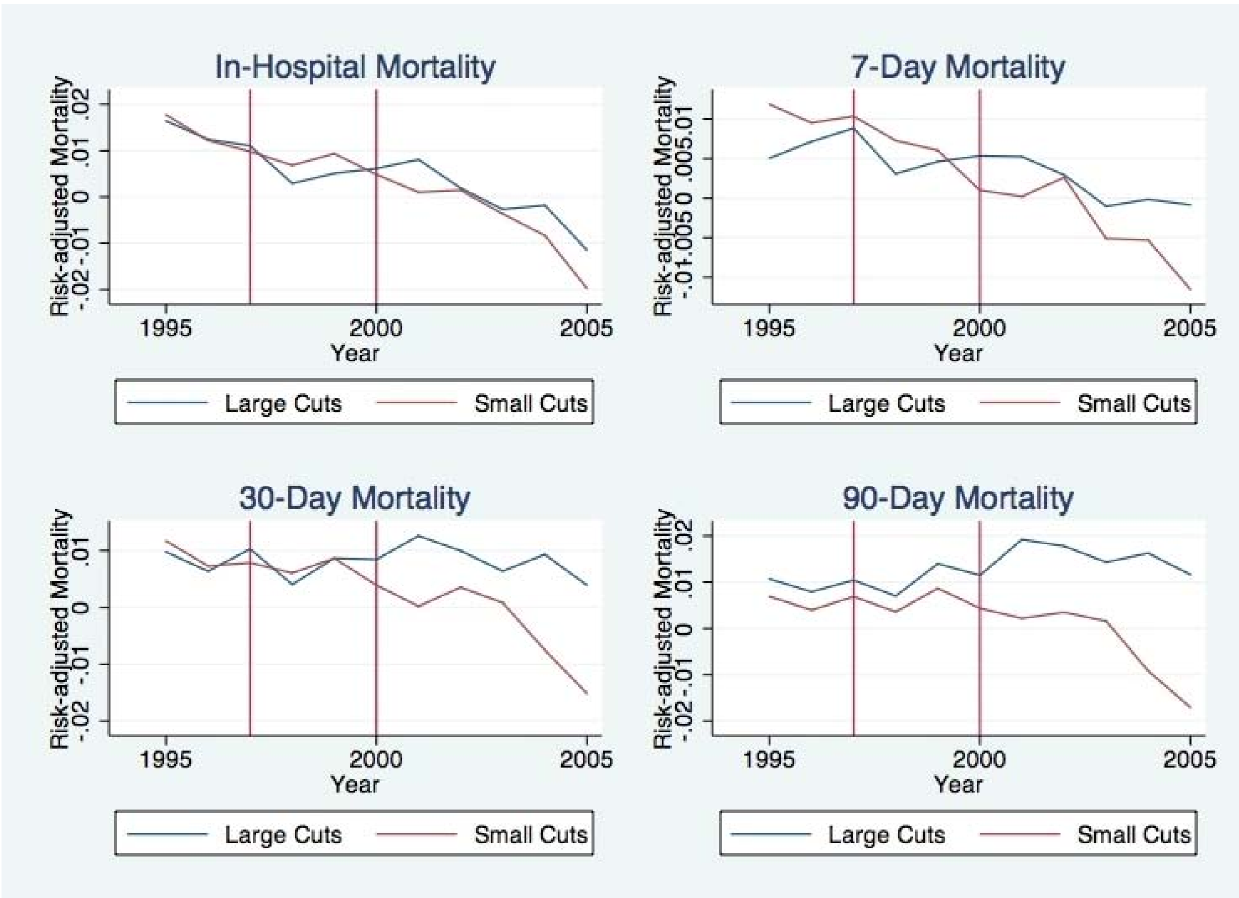
Figure 1

Trends in Medicare AMI patients' mortality rates: Difference between hospitals facing large Medicare BBA cuts and small Medicare BBA cuts



Note: 1. Mortality rate is normalized so the overall average mortality rate is 0 for all graphs.

2. The two lines separate the years into 3 periods: 1995-1997 pre-BBA, 1998-2000 initial years of BBA, and 2001-2005 post-BBA periods.

Table 1 Hospital Characteristics by degree of BBA Medicare revenue cuts over time

	All Hospitals	1995-1997		2001-2005	
	1995-2005	Largest Cuts	Smallest Cuts	Largest Cuts	Smallest Cuts
Raw AMI 90-day Mortality	0.23	0.24	0.23	0.24	0.21
Operating Cost per 1996 bed	500,953	372,073	386,542	582,911	695,185
FP Hospital	0.15	0.09	0.13	0.11	0.17
Public Hospital	0.12	0.09	0.14	0.08	0.12
Teaching Hospital	0.09	0.15	0.04	0.18	0.04
Medicare CMI	1.43	1.40	1.42	1.41	1.48
Hospital Beds	281	277	281	287	303
Occupancy Rate	0.59	0.56	0.53	0.64	0.67
Hospital System	0.65	0.52	0.68	0.64	0.73
HMO Penetration	0.23	0.24	0.18	0.25	0.18
Hospital HHI	0.35	0.33	0.38	0.34	0.39
% Discharge Medicare	0.39	0.44	0.36	0.40	0.38
% Discharge Medicaid	0.13	0.13	0.15	0.12	0.16
% Discharge Private	0.48	0.43	0.49	0.48	0.47
Medicare AMI Cases	93	95	93	86	108
Observations (# of hospitals years)	13,926	1,053	1,041	1,280	1,327

Table 2 Regressions of Risk-Adjusted Mortality Rates on Instrumented BBA Medicare Loss

	Dependent Variable: Risk Adjusted Mortality				
	In Hospital	7 Day	30 Day	90 Day	One Year
Effect of BBA					
Moderate Loss * Initial BBA	-0.0025	-0.0020	-0.0011	-0.0011	-0.0010
	(0.0027)	(0.0024)	(0.0029)	(0.0033)	(0.0036)
Large Loss * Initial BBA	-0.0038	0.0023	-0.0008	-0.0005	0.0013
	(0.0031)	(0.0028)	(0.0034)	(0.0038)	(0.0042)
Moderate Loss * Post BBA	0.0020	0.0024	0.0065*	0.0079*	0.0077*
	(0.0026)	(0.0025)	(0.0030)	(0.0033)	(0.0036)
Large Loss * Post BBA	0.0033	0.0080**	0.0106**	0.0135**	0.0160**
	(0.0032)	(0.0029)	(0.0037)	(0.0040)	(0.0044)
Control variables					
For Profit	0.0015	0.0009	0.0006	0.0011	-0.0013
	(0.0039)	(0.0033)	(0.0043)	(0.0047)	(0.0054)
Government	0.0021	0.0034	0.0015	0.0044	0.0033
	(0.0058)	(0.0048)	(0.0065)	(0.0069)	(0.0076)
Teaching	0.0018	0.0014	0.0027	0.0032	0.0042
	(0.0031)	(0.0024)	(0.0035)	(0.0036)	(0.0040)
Medicare Casemix	-0.0169*	-0.0196**	-0.0310**	-0.0407**	-0.0328**
	(0.0079)	(0.0071)	(0.0090)	(0.0096)	(0.0105)
Log(Hospital Beds)	0.0003	-0.0010	0.0012	0.0002	-0.0040
	(0.0031)	(0.0028)	(0.0034)	(0.0037)	(0.0043)
Occupancy Rate	0.0097+	-0.0004	0.0020	0.0000	0.0009
	(0.0050)	(0.0072)	(0.0074)	(0.0074)	(0.0065)
Member of Hospital System	0.0019	0.0012	0.0024	0.0021	0.0069+
	(0.0024)	(0.0023)	(0.0028)	(0.0030)	(0.0035)
HMO Penetration	0.0343*	0.0215+	0.0428**	0.0423*	0.0401*
	(0.0140)	(0.0127)	(0.0161)	(0.0180)	(0.0189)
Hospital HHI	0.0317+	0.0348*	0.0217	0.0089	0.0292
	(0.0177)	(0.0166)	(0.0206)	(0.0234)	(0.0242)
Constant	0.0154	0.0264	0.0291	0.0528*	0.0492+
	(0.0211)	(0.0190)	(0.0237)	(0.0260)	(0.0292)
Observations	13926	13926	13926	13926	13926
R-squared	0.36	0.30	0.32	0.34	0.37
Sargan Test for first stage					
NR^2	2*e-11	2*e-11	9*e-12	2*e-11	4*e-11
P-value	.82	.82	.82	.82	.82

Bootstarp standard errors in parentheses

Hospital fixed effects and year dummies are included in all models
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3. The Effect of BBA Cuts of 1997 on Hospital Operations

	Change in staffing level			Change in cost per 1996 bed	Change in length of stay	Change in Payer mix	
	RN FTE	LPN FTE	Total FTE			% Discharge Medicare	% Discharge Private
Moderate Loss * Initial BBA	-9.92+	-3.73*	-43.07*	-10,894*	0.05	-0.01**	0.01**
	(5.97)	(1.91)	(18.36)	(4,697)	(0.06)	(0.00)	(0.00)
Large Loss * Initial BBA	-16.81*	-2.87	-40.66+	-20,073**	-0.20**	-0.04**	0.04**
	(6.80)	(1.97)	(23.81)	(6,061)	(0.07)	(0.004)	(0.01)
Moderate Loss * Post BBA	-16.14+	0.29	-43.60	-34,012**	0.08	-0.02**	0.02**
	(9.53)	(1.62)	(30.06)	(10,781)	(0.07)	(0.00)	(0.060)
Large Loss * Post BBA	-20.32+	0.20	-43.34	-70,851**	-0.26**	-0.06**	0.07**
	(11.89)	(1.73)	(39.68)	(13,161)	(0.09)	(0.01)	(0.01)
Observations	13926	13926	13926	13569	13926	12503	12397
R-squared	0.93	0.71	0.96	0.89	0.78	0.88	0.80

Bootstarp standard errors in parentheses

Control variables are identical to the models reported in Table 2

+ significant at 10%; * significant at 5%; ** significant at 1%