The Long-term Impact of Medicare Payment Reductions on Patient Outcomes

February 2012

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Abstract

We examine the long-term effect of Medicare payment reductions on patient outcomes as a result of Balance Budget Act (BBA) of 1997. We classify hospitals into small, moderate, and large cuts categories using instrumental variable approach, and examine outcomes throughout1995-1997 (pre-BBA), 1998-2000 (initial-BBA), and 2001-2005 (post-BBA). We find that mortality trends stay similar across hospitals during the first two periods. Mortality trend diverged in 2001-2005: hospitals facing large payment cuts experienced increased mortality rates relative to that of hospitals facing small cuts. The results are partly explained by reduced staffing and operating cost following BBA in large cut hospitals.

AEA-JEL Classification: I11, I18

1. Introduction

Health policy researchers and decision makers have long been concerned about the relationship between provider payment generosity and quality of care for many reasons. On one hand, there is a common perception that providers 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. On the other hand, there are concerns that provider payment cuts may affect the access and quality of care. Conceptually, a reduction in provider reimbursement does not necessarily lead to worse quality, because such effect will depend on multiple factors, including size of the payment cuts, the incentives imbedded in the payment methods, the production efficiency of the provider, and the payment generosity of other payers. For example, a hospital facing large Medicare payment reductions can potentially recuperate some of the Medicare revenue loss by charging private health plans higher prices (a practice that is commonly referred to as “cost shift”), thus limiting the potential adverse effect on quality. Alternatively, hospitals facing increased financial pressure may try to become more cost efficient without hurting patient care. However, it is still possible that some hospitals facing large reductions from payers will have to implement more drastic cost saving actions that can lead to worse patient outcomes.

This important topic has sparked a long stream of research examining Medicare payment reductions due to the introduction of Prospective Payment System (PPS) in 1980s and private payment reductions due to the rise and growth of managed care in the late 1980s till mid 1990s. The collection of these literature points to a consensus that these past payment reductions have lead to cost cutting responses in the management and provision of care. At the same time, there is very limited or no long-term adverse impact on patient outcomes due to these payment reductions. This may partly explain why we find relatively few studies in recent years addressing the issue of Medicare payment generosity or adequacy.

In this paper, we argue that it is important to reconsider the effect of payment generosity or adequacy on quality of care in the context of BBA for several reasons. First, BBA contained the most significant Medicare payment reductions in decades. With the exception of PPS, BBA is the only legislation that *reduced* Medicare inpatient payments in nominal terms, rather than just slowing down the *rate of growth*. Second, the BBA payment cut can have a long lasting effect on hospitals, because the legislation not only reduced DRG payment levels between 1998-2002 (although legislations reversed some of the cuts in 2001 and 2002), but also permanently altered the formula for special add-on payments.[1] As illustrated in Figure 1, even though hospital payment grew at the full market basket update after the 1998-2002 period, the gap in payment due to formula changes across hospitals is permanent (more detailed explanation of Figure 1 is presented in the results section). Third, Medicare BBA reductions occurred after a sustained period of declining inpatient admissions and length of stay, as well as aggressive payment negotiations from managed care plans. As a result, hospital actions to produce further savings in this environment are likely to have direct consequences on patient outcomes than in the previous decade. Forth, the relationship between Medicare payment and patient outcomes depends on how much providers can recuperate the Medicare payment reduction from private payers (i.e., the degree of cost shifting). Cost shifting might be more likely to occur in early 1980s when the hospital environment was less competitive, thus moderating the degree of

[1] For example, a teaching hospital's indirect medical education (IME) subsidy is capped at its 1996's resident-to-bed (RTB) ratio, and the marginal subsidy for the RTB is permanently reduced.

financial pressure. In contrast, a hospital's ability to cost shift was more limited in late 1990s as the environment became more price competitive. A recent study shows that hospitals on average shifted just 21% of BBA payment reductions to private payers between 1996 and 2000 (Wu 2010). Therefore, hospitals may have to absorb most of the BBA cuts. Lastly, the effect of payment cuts on quality might not be immediate as quality changes are the result of rearrangements of resource allocation over time. Therefore, it is important to examine the impact over a longer period of time.

Examining the effect of BBA on patient quality is especially timely in light of the recent Affordable Care Act of 2010 (ACA), which include legislated Medicare payment cuts to providers. Understanding the long-term effect of BBA payment reduction can provide insight on the likely long-term effect of the ACA.

This study uses a plausible exogenous source of Medicare payment reduction – the BBA of 97- to study the long-term effect of provider payment cuts. The BBA serves as a valid instrument because it is an exogenous shock to the hospitals, and its broad provisions means the payment cuts affect a heterogeneous group of hospitals, ranging from major teaching hospitals, hospitals that serve disproportionately low-income patients, and hospitals that treat high cost patients. We also fill in a very important gap in the empirical literature, which has very limited evidence on the impact of BBA on patient outcomes. Our study examines both short-term (less than one-year post hospital discharge) and long-term patient outcomes, as well as tracking patient outcomes many years after BBA was first implemented. Using an instrumental variable approach with difference-in-difference-in-differences (DDD) design, we find evidence that Medicare AMI 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 and the total inpatient volume is declining.

2. Background and Literature Review

The pressure to reduce the growth rate in Medicare spending coupled with the concern that Medicare may be overpaying providers in the mid-1990s led to the enactment of the Balanced Budget Act (BBA) of 1997 by the Clinton Administration (Newhouse, 2002). Fundamentally, the BBA slowed Medicare spending growth by reducing inpatient PPS payments and eliminating most of the cost-reimbursement portion that remained in the program. BBA reduced inpatient payment by reducing (i) the yearly overall payment update factor that is supposed to reflect inflation, (ii) the reimbursing increment for teaching hospitals, (iii) the amount available to hospitals serving disproportionate share of low-income patients (DSH); and by (iv) changing the payment formula of outlier payments for costly patients. These payment formulas were permanently altered, such that the add-on payments were reduced every period after BBA. Therefore, BBA generated a long-term expected financial loss that extended beyond the BBA period.

The BBA has instituted the largest public payment reduction since the introduction of PPS in 1983. Researchers estimated that changes in inpatient payment alone would reduce Medicare expenditures by 40 billion between 1998-2002 (Guterman 2000), and about 35% of US hospitals' profit margins fell to negatives by 2000 as a result of BBA (Bazzoli, Lindrooth et al. 2005). Since then, several legislations were passed to relief some of the BBA cuts. For example, the Balanced Budget Refined Act (BBRA) of 1999 and Benefit Improvement and Protection Act (BIPA) of 2000 restored the inpatient PPS update factors to the full market basket increases in 2001 and close to full in 2002, and delayed the reductions in bad debts, teaching and DSH subsidies from 2001/2002 till 2003 onwards. The Medicare Modernization Act of 2003 included provisions to increase inpatient PPS updates and teaching subsidies between 2004-2009, and created new payments for small urban and rural hospitals. In summary, hospitals experienced the largest payment reduction between 1998 and 2000 as a result of BBA, but received some financial relief after 2000 due to the subsequent legislations.

Historically, the hospital sector has experienced three major payment reductions: Medicare payment reduction due to the switch of cost-based payment to PPS in early 1980s, the reduction from private payers due to the rise of managed care in the 1990s, and again another major Medicare payment cuts in the late 1990s due to BBA of 1997. There is a large volume of empirical literature examining the effects of these payment reductions on different dimensions of quality from the first two events. Evidence suggests that the introduction of PPS led to decreased length of stay (Newhouse and Byrne 1988; Hodgkin and McGuire 1994; Cutler 1995), reduced service intensity (Feder, Hadley et al. 1987), and more patients being discharged in 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 general consensus is that the growth and expansion of managed care has resulted in lower hospital payments, fewer inpatient admissions and expensive services/procedures, and no noticeable changes in patient outcomes (Miller and Luft 1994; Miller and Luft 1997; Miller and Luft 2002).

In contrast to the prior 2 major events, there are fewer studies examining the impact of BBA 97 and results from these studies are mixed. 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) showed 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 affected by BBA of 97, but there is no short-term effect of Medicare reimbursement cuts on patient mortality up to one year after effective BBA provisions (i.e., 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, which has not been studied much in the literature. 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 hospitals facing large payment cuts before and after BBA, relative to the pre-post change among hospitals facing small cuts. 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-BBA 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 Medicare Provider Analysis and Review (MedPAR) File 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 have enough information to instrument the impact of Medicare payment cuts due to BBA. We focus on urban hospitals because the HMO data are available 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. Our final sample consists of about 1,400 urban hospitals between 1995 and 2005 for a total of 14,021 observations.

4. Empirical Methodology

4.1 First Stage Estimation: Instruments for Medicare revenue change

An empirical concern in estimating the effect of Medicare revenue reduction on patient outcomes 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 and such behavior is likely correlated with patient outcomes. In addition, there may be unobserved hospital characteristic or market conditions that are correlated with both Medicare revenue and outcomes. 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"). We focus on the cumulative payment cuts between 1996 and 2000 for the "pure" BBA effect, because most of the BBA reductions in 2001 and 2002 were reversed by BBRA of 1999 and BIPA of 2000.

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 inpatient revenue[2] is calculated as the difference between actual Medicare revenue in 2000 per total discharge and the hypothetical Medicare revenue in 2000 that is computed by updating the 1996 actual Medicare revenue to 2000 using market basket divided by total discharge in 1996. 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:

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

It is worth noting that changes in DRG prices due to BBA vary across hospitals. The BBA reduced inpatient payments by holding the DRG annual updates well below the market basket growth, and by altering payment formulas that effectively reduce payments to indirect medical education (IME), disproportionate share hospitals (DSH), and outlier adjustments (OUT). 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

[2] Medicare inpatient revenue is the total of primary payer amount and payments from beneficiaries.

total BBA impact is moderated by a hospital's Medicare share to render it comparable to other hospitals that serve predominately Medicare patients. As such, BBA exposes a heterogeneous group of hospitals to experience a similar degree of Medicare payment cuts.

Based on the predicted Medicare revenue change from this first stage estimation, we classify hospitals into 3 payment cut categories: small, moderate, and large cut (details below). The categorical variable then becomes an input in the second stage estimation.

4.2 Second Stage 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.[3] "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

[3] We use the 3 categories rather than instrumented Medicare revenue change in the second stage for 2 reasons. First, we are interested finding the evidence of BBA payment reductions rather than estimating the elasticity of payment reductions. Second, while we tried our best to simulate total BBA inpatient cuts by component, we are unable to simulate all components with precision. For example, the precise simulation of change in outlier payment cost threshold and the elimination of day-outlier payment requires detailed patient-level data on cost/charges. We proxy the effect of outlier cuts by using aggregated hospital-level data in 1996 to apply to the change in payment formula, assuming the size of the cut is proportional to the 1996 level.

the dummy for hospitals in the interquartile range (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 period that allows us to control for possible differential trend in the baseline across different payment cut categories. The period 1998-2000 is the “initial BBA” period when the majority of 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 (the reference hospital 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. These fixed effects also control for the differences in initial hospital status such as payer mix and financial conditions prior to BBA. 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). We estimated bootstrap standard errors in the second stage regressions to account for the fact that our payment cut categories are based on predicted values from the first stage regression.

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 from admission date. 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, as well as the total inpatient volume measured by total (all-payer) inpatient discharges (log transformed).

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, captured by 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 staffed beds (log transformed) and occupancy rates to control for such characteristics.

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 (government, for-profit, and not-for-profit). 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 trends in DRG payment by the degree of instrumented BBA cuts. The graph illustrates the key argument of our study that BBA payment reductions have a lasting effect that extends beyond the initial BBA period. In figure 1, Medicare payment per Medicare discharge (DRG payment) differed little between large- and small-cut hospitals in the pre-BBA period. During the 1998-2000 initial BBA period, BBA reduced DRG payment for both groups, but more so for large-cut hospitals and thus creating a payment gap between the 2 groups. This payment gap *persisted* in the post-BBA period, despite Medicare payment grew at similar rates between these hospitals in the post-period. The gap was generated due to the permanent change in DRG payment formula.

Figure 2 shows the following mortality trend between 1995 and 2006: in-hospital, 7-day, 30-day, and 90-days. We show the trend separately for hospitals that experience the largest cut and the smallest cut. The two vertical lines separate the figure into 3 periods: pre-BBA (1995-1997); initial BBA (1998-2000), and post-BBA (2001-2005) periods. We normalized the mortality rates, so the average value for the entire period is 0 in all plots. In general, the mortality rates exhibit a 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 diverging trend in figure 2 begs the question whether there is systematic difference between the two groups of hospitals. To shed light on this issue, table 1 presents the summary statistics of key variables for the entire study sample, and separately for the large cut (top 25th percentile) and small cut (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). Large-cut hospitals on average experienced a loss of $616 Medicare dollars (predicted using instruments) per total discharge, while the small-cut hospitals saw $260 increase. Comparing the characteristics in the pre-BBA period, large-cut hospitals are more likely to be major teaching or regular not-for-profit (NFP) hospitals, while small-cut hospitals are more likely to be for-profit (FP) or public hospitals. This is consistent with the BBA payment change that affects large Medicare-share and major teaching hospitals the most. Both groups of hospitals have similar bed-size, occupancy rates, and mortality outcomes. Differences in other characteristics are small except for market competition variables. 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.

Table 1 also shows some important change in trends between large- and small- cut hospitals during the study period. Large-cut hospitals grew much slower in size (as measured by number of beds) and cost, dropped their occupancy rates, switched from having sicker to healthier Medicare patients, and changed the payer mix to have less Medicare and more private discharges. Large-cut hospitals also changed from having more to less Medicare AMI cases.

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 26, indicating that the instruments are indeed significant predictors of the actual payment change. We performed the Sargan over-identification test 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 3 BBA periods (initial BBA years 1998-2000, and post-BBA years 2001-2005; where pre-BBA years 1995-1997 serve as the reference period). 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 0.7 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 (about 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 16 FTE RNs ($p<0.05$) during the initial BBA period, and the downsizing trend continues in the 2001-2005, with the RN level further decreased by another 4 bodies ($p<0.05$). 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 large cut hospitals have $15,305 ($p<.001$) less operating cost per bed in the 1998-2000 period and $67,701 less ($p<.001$) in the 2001-2005 period compared to the change in the small cut group.

In addition, we explore the changes in length of stay to understand the different results we obtained in Table 2 between in-hospital mortality rate and the other mortality rates. We found that patients are being discharged earlier in hospitals with large payment cut (shorter length-of-stay by 0.23 and 0.28 days in 1998-2000 and 2001-2005, respectively, $p<0.01$). The early discharge might be part of the reason that we found no effect of BBA cuts on in-hospital mortality. 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, in table 4, that the largest cut hospitals replace some Medicare patients with private patients, as the percent of Medicare discharges go down by 4 and 5 percentage points in the 1998-2000 and 2001-2005 periods, respectively, and the percent of private discharges move up by about the same amount. In addition, large cut hospitals saw a general decline in inpatient volume over time: total discharge went down by 2% and 3% in the initial- and post-BBA periods, respectively. 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.

6. Sensitivity Tests

In our main models, we categorize hospitals into 3 payment cut categories for ease of interpretation. In one sensitivity analysis, we examine whether our finding is dependent on this particular functional form specification. We estimated the 2nd stage regressions using the actual predicted change in Medicare revenue. We reached the same conclusion using this linear specification and report the regression coefficient and corresponding elasticity in Table 5.

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). First, in our main analysis we simulate the cumulative effect of BBA cut only up until 2000, whereas the BBA cuts were implemented through 2002. As discussed in previous section, we took this approach because we want to capture the effect of BBA reduction alone without the effect being muddled by later legislations that restored most of the BBA cuts after 2000. We would expect the hospital's Medicare revenue and financial position to improve somewhat in 2001 and 2002 as a result of BBRA and BIPA (relative to the scenario without these new legislations), making the simulated payment reduction a weaker instrument. We test this hypothesis by repeating our analysis, but re-estimate our first stage using the simulated cumulative BBA cut between 1996 and 2002 (net of BBRA and BIPA) to instrument for the actual change in Medicare revenue between 1996 and 2002. We found that by 2002, most hospitals indeed have returned to their 1996 payment level, thus making simulated BBA cuts less predictive of actual change (increase) in Medicare revenue. As a result, coefficients are smaller in the second stage using the 1996-2002 instrument (by about half of a percentage point), but remain statistically significant for large-cut hospitals.

We also would like to rule out the possibility that the worsened mortality estimated in the 2001-2005 period is caused by co-temporal changes in the same period. Specifically, we test the co-temporal short-term effect of Medicare and private payment change in the regression formally, by adding two measures in the main model. We calculate (1) "Medicare payment" as actual Medicare inpatient revenue divided by Medicare discharges, and (2) "private payment" as the total non-Medicare inpatient revenues divided by non-Medicare discharge. The results of the BBA effects are virtually unchanged after inclusion of these 2 measures.

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 for each hospital, a good proxy for the overall health status of the admitted patient population (Shen 2003), using the same specification. The results show that change in AMI patient age are about 0.6 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 experience a deterioration of quality due to declining AMI volume), we regress Medicare AMI cases using the same model. We observe that large-cut hospitals have reduced Medicare AMI admissions by 18 and 23 cases in 1998-2000 and 2001-2005 periods, respectively. 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.

7. Discussion

Our results indicate that Medicare BBA of 97 lead to a widening gap in patient outcomes for AMI between hospitals experiencing small and large payment cuts. We find that Medicare AMI mortality rates between large-cut and small-cut hospitals had similar trends prior to and immediately after BBA was implemented, but diverged between 2001 and 2005. The finding that adverse patient outcomes were not detectable in the initial years of 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, particularly among registered nurses. However, there is a delay between payment reductions and their ultimate impact on patient outcomes. It is possible that 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 affect resources needed for direct 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 between hospitals 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 suggest one worthy of attention here. A 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 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. In table 5, we take the coefficient estimates using linear specification of the instrumented BBA loss variable, and covert the coefficient effects into elasticity. Every 1000 dollar instrumented Medicare revenue loss due to BBA, which is about 21% of Medicare revenue per total discharge in 1997, is associated with 6% to 8% increase in mortality rates.[4] Taken together, the elasticity is about -0.3, implying a 1% reduction in payment would translate to a 0.3% increase in mortality rates. 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

[4] Take 90-day mortality rate, for example. The baseline 90-day mortality rate is 24 percent, so the 1.67 percentage point increase we observe in the post-BBA period would translate into 7% increase (1.67/24=0.07).

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. It is also important to keep in mind that the adverse effect is "relative" in a sense that the absolute mortality rates did not go up during this period. Rather, while AMI mortality rates have steadily declined in hospitals facing small BBA cuts during the entire study period, the mortality rates remained at the same level in hospitals facing large BBA cuts experience.

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 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.

8. **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 only became measurable several years after the policy took place. Our results point to an important issue that payment level, especially under PPS, is as important as payment method. If Medicare considerably over- or under-pays providers, the expected higher efficiency under PPS may not realize. Medicare is currently operating in a difficult environment where treatment costs are increasing rapidly after a decade of relatively slow growth and yet Medicare reimbursement is unable to keep up with the pace of cost growth. Given the growing fiscal pressure in the federal government, Medicare will most likely reduce hospital/provider's reimbursements further, as is mandated in the Affordable Care Act (if not reversed later by other legislation). While it is possible that payment cuts might just force hospitals to be more efficient without compromising patient care, our study suggests that future substantial Medicare reimbursement cuts will likely have adverse consequences on Medicare patient outcomes. Furthermore, across-the-board payment reductions may have an unintended 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.

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Figure 1 Trends in Medicare Payment per Medicare Discharge by BBA cuts, 1995-2005

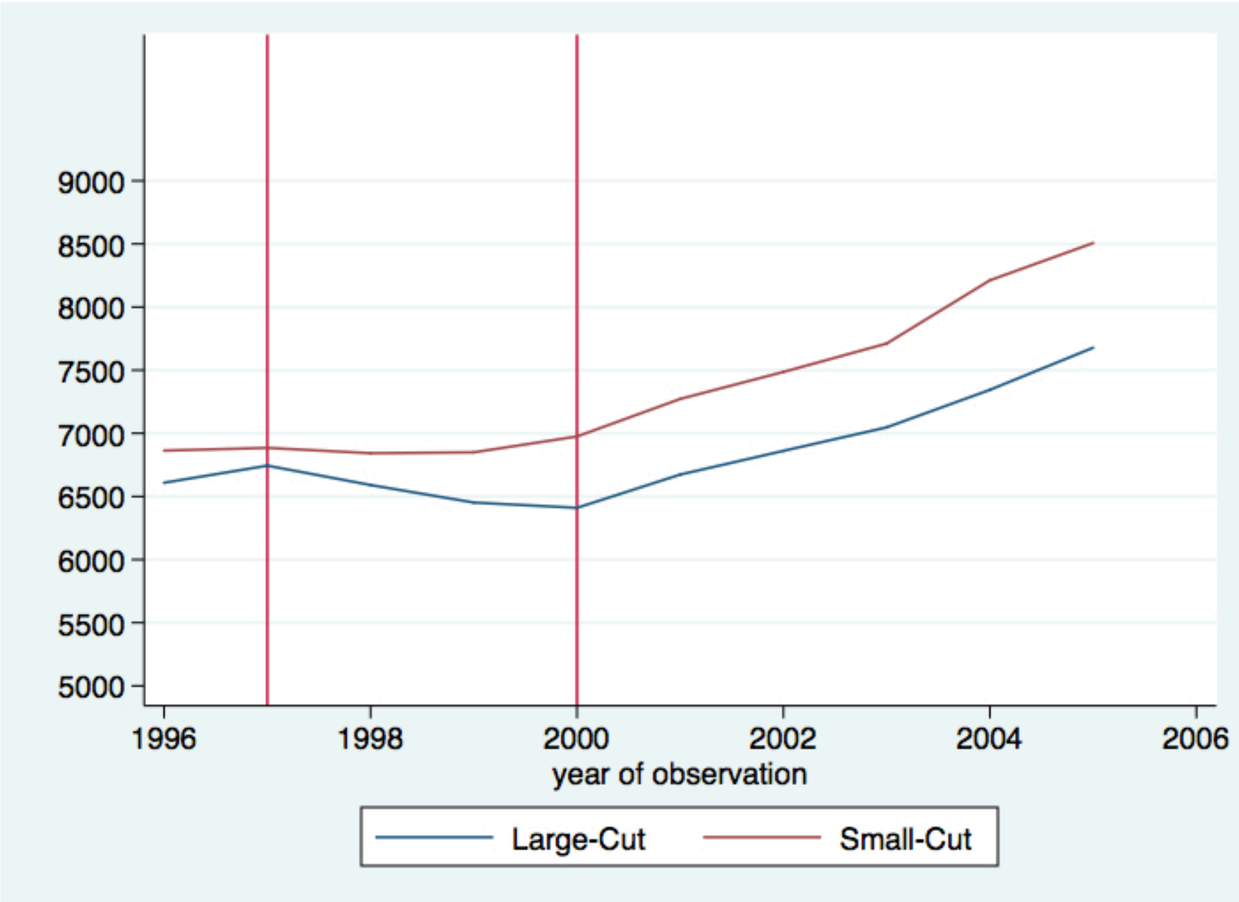
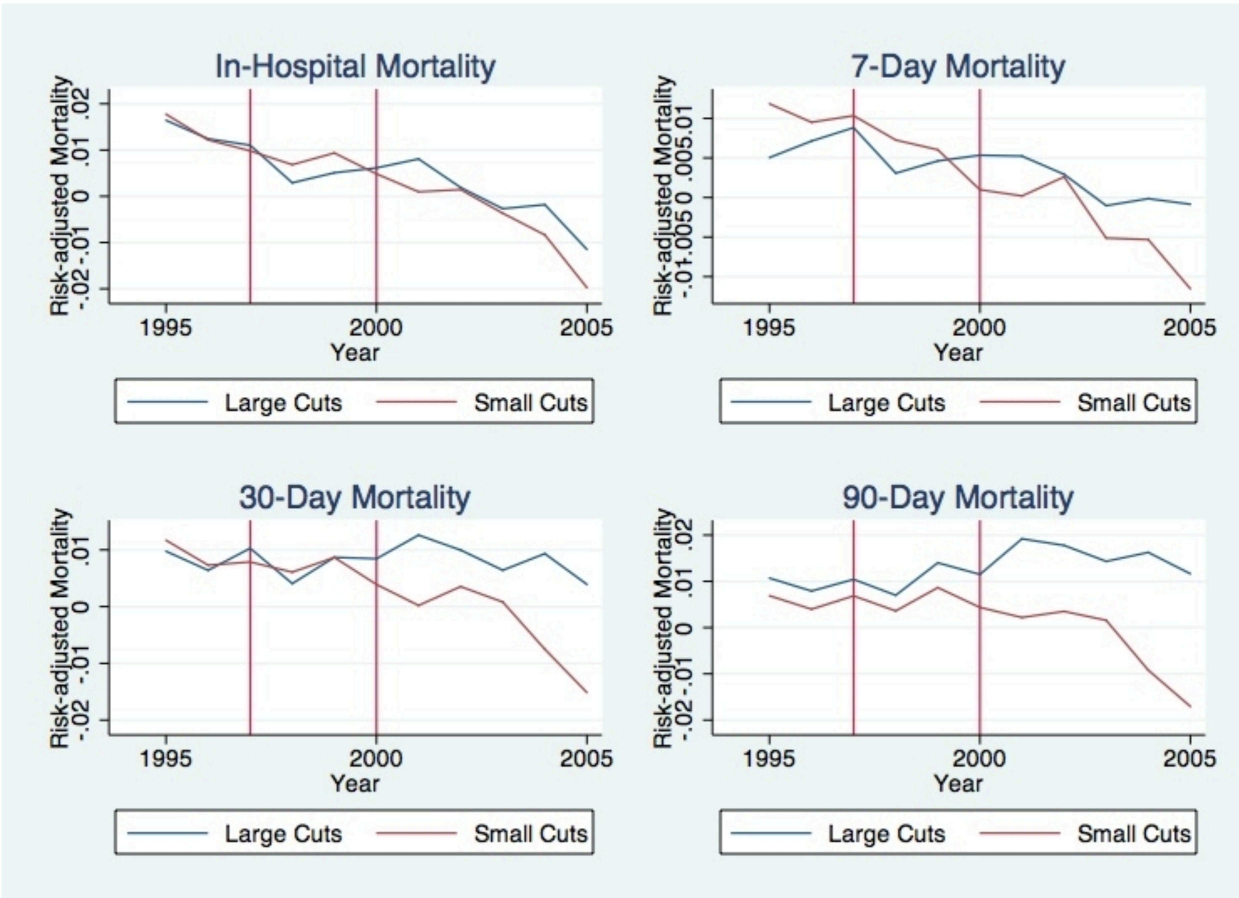


Figure 2. Trends in Medicare AMI patients' mortality rates: Difference between hospitals facing large and small Medicare BBA cuts



Notes:
1. The risk-adjusted 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 BBA years), and 2001-2005 (post-BBA).

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
Instrumented Loss in Medicare Revenue due to BBA	-166	-616	260	-616	260
Raw AMI 90-day Mortality	0.23	0.24	0.23	0.24	0.21
Operating Cost per 1996 bed	505442	358976	388999	568993	700263
% FP Hospital	15%	8%	15%	10%	18%
% Public Hospital	12%	8%	14%	8%	12%
% Teaching Hospital	9%	13%	4%	15%	4%
Medicare CMI	1.44	1.39	1.42	1.40	1.46
Hospital Beds	285	275	278	281	297
Occupancy Rate	0.59	0.56	0.53	0.64	0.67
% Hospital System	65%	51%	67%	63%	72%
HMO Penetration	0.23	0.24	0.18	0.25	0.18
Hospital HHI	0.35	0.33	0.38	0.34	0.39
Total Discharge	13679	10084	12079	12091	14671
% Discharge Medicare	38%	44%	36%	41%	37%
% Discharge Medicaid	13%	13%	15%	11%	16%
% Discharge Private	49%	43%	50%	48%	47%
Medicare AMI Cases	94	99	91	88	104
Observations (# of hospitals years)	14,021	982	1,028	1,216	1,316

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 * 1998-2000	-0.30	-0.22	-0.03	-0.11	0.02
	(0.25)	(0.21)	(0.26)	(0.29)	(0.31)
Large Loss * 1998-2000	-0.26	0.24	0.09	-0.06	0.12
	(0.30)	(0.26)	(0.30)	(0.37)	(0.40)
Moderate Loss * 2001-2005	0.16	0.26	0.78**	0.84**	0.77*
	(0.25)	(0.23)	(0.28)	(0.31)	(0.32)
Large Loss * 2001-2005	0.32	0.73**	1.08**	1.36**	1.57**
	(0.30)	(0.28)	(0.33)	(0.36)	(0.42)
Control variables					
For Profit	-0.07	-0.13	-0.12	-0.09	-0.49
	(0.35)	(0.29)	(0.40)	(0.43)	(0.49)
Government	-0.09	0.16	-0.25	0.13	-0.12
	(0.55)	(0.44)	(0.54)	(0.64)	(0.72)
Teaching	0.08	0.13	0.08	-0.02	0.06
	(0.30)	(0.24)	(0.35)	(0.34)	(0.38)
Medicare Casemix	-1.75*	-2.03**	-3.20**	-4.57**	-3.79**
	(0.73)	(0.65)	(0.83)	(0.86)	(1.03)
Log(Hospital Beds)	-0.08	-0.14	0.03	-0.10	-0.46
	(0.28)	(0.24)	(0.30)	(0.35)	(0.40)
Occupancy Rate	1.02+	0.07	0.20	0.11	0.12
	(0.54)	(0.67)	(0.74)	(0.75)	(0.67)
Member of Hospital System	0.21	0.14	0.32	0.29	0.77*
	(0.21)	(0.20)	(0.27)	(0.26)	(0.32)
HMO Penetration	3.21*	1.82	4.02**	3.78*	3.32+
	(1.27)	(1.15)	(1.51)	(1.78)	(1.76)
Hospital HHI	3.48*	3.12*	1.98	1.11	3.10
	(1.61)	(1.57)	(2.01)	(2.26)	(2.15)
Constant	2.18	3.15+	3.70+	6.59**	6.03*
	(1.82)	(1.70)	(2.12)	(2.45)	(2.76)
Observations	14021	14021	14021	14021	14021
R-squared	0.36	0.29	0.32	0.34	0.38
Sargan Test for first stage					
NR^2	2*e-12	2*e-11	1*e-11	2*e-11	9*e-12
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
	RN FTE	LPN FTE	Total FTE		
Moderate Loss * 1998-2000	-11.23*	-4.60**	-33.23*	-9,742*	0.03
	(5.23)	(1.70)	(16.66)	(3,966)	(0.05)
Large Loss * 1998-2000	-15.42*	-3.67*	-37.95+	-15,305**	-0.23**
	(6.05)	(1.69)	(21.28)	(5,781)	(0.06)
Moderate Loss * 2001-2005	-16.45*	0.12	-38.43	-34,818**	0.01
	(8.18)	(1.46)	(29.18)	(9,126)	(0.06)
Large Loss * 2001-2005	-19.93*	0.40	-46.34	-67,701**	-0.28**
	(9.88)	(1.53)	(37.40)	(12,623)	(0.08)
Observations	14021	14021	14021	13656	14021
R-squared	0.93	0.70	0.96	0.89	0.78
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%					

Table 4. The Effect of BBA Cuts of 1997 on Payer Mix and Total Discharge			
	Change in Payer mix		% Change in Total Discharge
	% Discharge Medicare	% Discharge Private	
Moderate Loss * 1998-2000	-0.01**	0.01**	-0.01
	(0.00)	(0.00)	-0.01
Large Loss * 1998-2000	-0.04**	0.04**	-0.02*
	(0.00)	(0.01)	-0.01
Moderate Loss * 2001-2005	-0.01**	0.02**	-0.02
	(0.00)	(0.01)	-0.01
Large Loss * 2001-2005	-0.05**	0.07**	-0.03*
	(0.00)	(0.01)	-0.01
Observations	12626	12522	12626
R-squared	0.89	0.80	0.12
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%			

Table 5 Elasticity of Medicare Payment Cuts					
	Mortality				
	in-hospital	7-day	30-day	90-day	1-year
Regression Coefficient[T] (in 2001-2005)	-0.49+	-0.90**	-1.33**	-1.67**	-2.07**
Elasticity	-0.16	-0.35	-0.33	-0.32	-0.30

[T]Coefficients taken from regressions using linear instrumented BBA loss specification.