Hospital Volume Responses to Medicare's Outpatient Prospective Payment System: Evidence from Florida*

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Abstract: Effective in 2000, Medicare's Outpatient Prospective Payment System (OPPS) sets pre-determined reimbursement rates for hospital outpatient services, replacing the prior cost-based methods of reimbursement. Using Florida outpatient discharge data, we study the effect of OPPS on hospital outpatient volume. In particular, we examine the potential for demand inducement of both Medicare and non-Medicare services when providers face a Medicare payment cut. We find that, on average, hospitals experiencing Medicare rate cuts decreased Medicare volume and increased private fee-for-service (FFS) volume. We also find that responses vary with the hospital's "exposure" to Medicare payment changes, where exposure is measured as the baseline Medicare patient share. Compared to less exposed hospitals, highly exposed hospitals responded with larger increases in private FFS volume and with smaller decreases (in some cases increases) in Medicare volume when payment rates fell. Our results are consistent with provider demand inducement.

Key Words: Outpatient prospective payment system, Medicare payment reform, volume response, substitution, demand inducement

JEL: I11, I12, H32, H51

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1. Introduction

The Balanced Budget Act of 1997 established the Medicare Outpatient Prospective Payment System (OPPS), which went into effect on August 1, 2000. Prior to OPPS, Medicare reimbursed hospitals based on actual costs incurred in outpatient care delivery. Under OPPS, Medicare now classifies hospital outpatient services into approximately 800 ambulatory payment classifications (APCs) based on clinical and cost similarity. Regardless of the actual treatment cost, all services in the same APC are reimbursed at the same predetermined amount, with adjustments for local labor costs, certain hospitals, and outlier cases.

Little is known about the impacts of OPPS on outpatient utilization, even though the use of prospective payment systems (PPS) has been studied widely in inpatient and several other healthcare delivery settings.[1] This may reflect the complexity of the program; some have called the transition to OPPS "the most complex and difficult programmatic change in the history of Medicare" (Mohr and Kintala, 2003). Nonetheless, policies that affect hospital outpatient spending are of significant interest. From 2002 to 2007, Medicare outpatient spending per beneficiary grew 47% while inpatient spending grew only 18% (MedPAC, 2009a), and outpatient services currently constitute more than 20% of Medicare's total payments to hospitals (MedPAC, 2010). Medical technological advancement is likely to further shift many conventional inpatient services to outpatient settings.

[1] To our knowledge, the only study of PPS in the outpatient setting is a working paper by Becker (2007). His study examines changes in reimbursements and costs caused by OPPS using data from 1999 and 2000. For PPS studies in other care settings, see, for example, Cutler (1995) on Medicare PPS in inpatient acute care, Norton et al. (2002) on Medicaid PPS in inpatient psychiatric care, Sood et al. (2008) on Medicare PPS in inpatient rehabilitation care, White (2003) and Grabowski et al. (2010) on Medicare PPS in skilled nursing facility care, and McCall et al. (2003) on PPS in Medicare home health care. In general, many studies find evidence that providers respond to the financial incentives of PPS by reducing lengths of stay and other measures of resource use. Also see Salkever (2000) and Chalkley and Malcomson (2000) for reviews of the effects of Medicare inpatient PPS on hospital costs, length of stay, and other measures of resources.

In this paper, we examine the effect of OPPS-induced payment changes on the volume of hospital outpatient services. In particular, we test for an important yet understudied aspect of the hospital's response to Medicare prospective payment systems, the potential for demand inducement of both Medicare and non-Medicare services, which is usually examined in the context of physician responses to Medicare fee cuts (see, for example, McGuire and Pauly 1991 and McGuire 2000).[2] In the voluminous PPS literature, very few studies systematically test for changes in services reimbursed by both Medicare and non-Medicare payers (Scheffler et al. (1994), on inpatient PPS, is a notable exception). None of the studies of the newer forms of PPS, to the best of our knowledge, examines the effects of Medicare payment changes on services reimbursed by non-Medicare payers.[3]

Using Florida outpatient discharge data from 1997 to 2008, we construct volume measures from counts of outpatient surgical procedures by payer for each hospital and year. To obtain the post-OPPS reimbursement rates, we follow guidelines on APC rate determinations published in quarterly issues of the Federal Register; for the pre-OPPS years, we develop algorithms to impute hospital-specific Medicare payment rates from discharge records and Medicare Cost Reports. Our imputed Medicare payment rates for the top ten most common surgical procedures show an average decrease of 22% in the five-year period before and after implementation of OPPS.

To examine the effects of OPPS-induced rate reductions on outpatient volume, we regress hospital- and procedure-specific utilization counts by payer on Medicare payment rates

[2] Gruber and Owings (1996) examine the potential for demand inducement when OB/GYNs face income shocks caused by declining fertility rates; Gruber, Kim, and Mayzlin (1999) test for physician induced demand in response to Medicaid fee cuts.

[3] Newer forms include PPS in skilled nursing facilities (effective 1998), in home health care (effective 2000), and in inpatient psychiatric hospitals, long term care hospitals, and inpatient rehabilitation hospitals (all effective 2002).

controlling for private payment rates and a wide range of hospital- and county-level explanatory variables. We also test for heterogeneous responses to payment changes; specifically, we examine whether responses vary by a hospital's "exposure" to OPPS, which we measure as the hospital's baseline Medicare patient share. Our results imply that, on average, OPPS-induced rate reductions decreased the number of procedures provided to Medicare patients and increased the number of procedures provided to patients covered by private fee-for-service (FFS) insurance. Further results suggest that both volume responses to OPPS were heterogeneous. Highly exposed hospitals responded with smaller decreases in outpatient service provision to Medicare patients relative to less exposed hospitals; in some instances highly exposed hospitals actually increased Medicare volume. In terms of private FFS volume, Medicare rate reductions led to larger increases at highly exposed hospitals compared to less exposed hospitals. Overall, these results are consistent with provider induced demand: hospitals for which Medicare patients are important induce more demand across payers in order to compensate for lost revenue from Medicare reimbursement rate cuts.

This paper proceeds as follows. Section 2 provides background information on OPPS, and Section 3 describes the conceptual framework for examining its effects on volume paid by Medicare and non-Medicare payers. Sections 4 and 5 describe our data and empirical approach. Regression results are presented and discussed in Sections 6 and 7. Finally, Section 8 concludes.

2. Background on the Outpatient Prospective Payment System (OPPS)

Prior to the implementation of OPPS in August 2000, Medicare used "a confusing mix" of different payment methods for hospital outpatient services (Wynn, 2005, pg. 5).[4] Depending

[4] Note that OPPS and its precursor fee schedules pertain to the reimbursement of *hospital* costs; Medicare uses a separate fee schedule to reimburse *physicians* who work in hospital outpatient settings.

on the type of service, the reimbursement rate could be either: (a) the lesser of costs or charges; (b) the lesser of costs, charges, or a blended rate; or (c) a fee schedule. In particular, payment for medical visits, therapy and rehabilitation services, and certain surgeries were based on hospitals' reasonable costs or customary charges. ASC-approved surgical procedures and certain radiology and diagnostic procedures were reimbursed at the lesser of costs, charges, or a blended rate which combined the lesser of costs or charges with a fee schedule.[5] For clinical laboratory services, prosthetics and orthotics, and durable medical equipment, hospitals were paid according to fee schedules. To make things even more complicated, these methods were applied retrospectively on an aggregate basis only when Medicare determined the final settlement payment after hospitals submitted their annual cost reports (MedPAC, 1999).

Under this mostly cost-based reimbursement system and in a period where technological changes shifted more care from inpatient to ambulatory settings, Medicare payment for hospital outpatient services rose sharply. Between 1983 and 1997, for example, MedPAC (1999) reports an annual rate of increase of 12%. Due in part to concerns that existing payment methods provided little incentive for hospitals to lower costs, the Balanced Budget Act (BBA) of 1997 and the Balanced Budget Reconciliation Act (BBRA) of 1999 established a prospective payment system for hospital outpatient services, the OPPS, effective on August 1, 2000.

OPPS applies to almost all hospitals participating in Medicare and to most hospital outpatient services.[6] It essentially is a new fee schedule that groups outpatient services, sets the

[5] An ASC is an Ambulatory Surgery Center. Medicare reimburses ASCs for performing only those surgeries on an "ASC-approved list." In 1999, about 2,500 surgical procedures were on the ASC-approved list (MedPAC, 1999). Most of the surgical procedures we examine in this paper are ASC-approved.

[6] Some small groups of hospitals are not subject to OPPS; hospitals in Maryland (which are paid under the state's all-payer waiver provisions), Critical Access Hospitals, and Indian Health Services and Tribal hospitals are exempt (CMS, 2010). Examples of excluded services are ambulance services, physical and occupational therapy, and speech-language pathology services (Federal Register, 2000, p. 18442).

same predetermined payment rate per service or procedure for all services in each group, and makes necessary adjustments in certain circumstances.[7] Multiple services are grouped into Ambulatory Payment Classifications (APCs) according to their clinical and cost similarity (CMS, 2010). For example, as of October 2007, APC 141 included 43 CPT codes for different forms of endoscopy of the esophagus and the upper gastrointestinal tract, and APC 143 included 21 CPT codes, most pertaining to colonoscopy.[8]

To determine payment rates for each APC, the Centers for Medicare and Medicaid Services (CMS) first establishes a relative weight for each APC; this weight reflects the resource costs associated with services in the APC and was initially based on the national median cost of the services within each APC as determined by Medicare claims data and cost reports prior to 2000. The relative weight is then multiplied by a conversion factor to arrive at a national unadjusted payment rate for each APC. The labor portion (60%) of this national rate is adjusted for local wage differences using the hospital wage index. CMS annually revises APCs and relative weights in consultation with outside experts; in addition, CMS updates the conversion factor annually using the hospital market basket index (MedPAC, 2008). Hospitals also receive

[7] Despite the similarities in their names, important differences exist between OPPS and prospective payment systems (PPS) used in other healthcare delivery settings such as inpatient acute care, inpatient rehabilitation, mental health, and home healthcare settings. In particular, because payment under OPPS typically is for a particular service or procedure, OPPS essentially just changes the reimbursement rate per service or procedure. As implemented under the BBA of 1997 and the BBRA of 1999, OPPS does not contain any bundled payment provisions. OPPS does include some "packaging," or cases where payment for the primary service also includes reimbursement for ancillary or supportive services that are considered integral to the primary service. Ancillary services that are packaged include routine supplies, anesthesia, operating and recovery room use, inexpensive drugs, and some others (CMS, 2010). In contrast, those other prospective payment systems typically involve substantial service bundling, reimbursing for units of treatment such as hospital discharges or home healthcare episodes. For example, payments under the widely-studied Medicare inpatient PPS are based on the diagnosis-related group associated with the patient's admission, and in most cases, the hospital is reimbursed a flat pre-determined amount for *all* services provided to the patient during the entire hospital stay.

[8] CMS frequently revises the APC system. Since implementation, the number of APCs has increased from 451 in 2000 to more than 800 in 2008.

other adjustments for certain new technologies (called pass-through payments) and for unusually costly services (called outlier payments). [9]

OPPS was also designed to lower the coinsurance paid by Medicare beneficiaries for hospital outpatient services. Prior to 2000, Medicare beneficiary coinsurance for hospital outpatient services was set at 20 percent of the hospital charges. Because hospital charges usually greatly exceeded Medicare payment, coinsurance often amounted to much more than 20 percent of Medicare's payment amount. Under OPPS, coinsurance was to decrease each year until reaching a target of 20 percent of total payment amounts (MedPAC, 2008).

OPPS included a number of features designed to buffer hospitals from the financial shocks anticipated from its implementation. From 2000 to 2003, OPPS allowed all hospitals to receive "transitional-corridor" payments, or supplemental payments for cases in which APC payment rates were less than rates in place under cost-based reimbursement (CMS, 2010). In addition, several types of hospitals are treated differently under OPPS. First, small rural hospitals that achieve the specific designation of a Critical Access Hospital (CAH) are exempt from OPPS. Second, children's hospitals and cancer hospitals have permanent "hold-harmless status," meaning they receive the full difference between the OPPS amount and the pre-OPPS cost-based reimbursement. Third, small rural hospitals (with 100 beds or fewer) receive partial hold-harmless payments ranging from 85% and 95% of the full hold-harmless amount over the post-OPPS years. Fourth, sole community hospitals received full hold-harmless payments

[9] Hospitals receive outlier payments for services that cost much more than the payment rate defined for the APC group. In 2008, CMS defined an outlier as a service costing more than 1.75 times and $1,575 more than the APC rate (MedPAC, 2008). It is not possible for us to identify the services in our dataset that met those criteria.

between 2000 and 2005, and beginning in 2006, these hospitals receive a 7.1% add-on to APC payments (MedPAC, 2008).[10]

3. Conceptual Framework

As the prior section illustrates, OPPS essentially changed (and presumably reduced) the Medicare payment rate to hospital outpatient departments per service or per procedure. According to economic theory, healthcare provider responses to rate or fee reductions can be complex. Standard profit maximization models predict that competitive firms respond to price reductions by reducing quantity supplied. In contrast, models of provider induced demand suggest that rate reductions by one payer do not unambiguously decrease the volume of care provided to patients covered by that payer, and with multiple payers, rate cuts by one payer increase the volume of care provided to patients covered by the other payer.

Below we borrow the exact same model presented in McGuire (2000) to illustrate provider induced demand. Based on McGuire and Pauly (1991) and Gruber and Owings (1996), McGuire (2000) considers a physician who maximizes utility (U) defined as:

$$(1) \quad U = U(Y, I)$$

where Y is net income, I is total inducement, and $U_Y>0$, $U_I<0$, $U_{YY}<0$, and $U_{II}<0$. Net income is a function of the number of patients (N) and the quantity of the two services provided (x_1 and x_2), and is defined by:

$$(2) \quad Y = N(m_1 x_1(i_1) + m_2 x_2(i_2))$$

[10] For more information on OPPS, see the Federal Register for the final OPPS release in 2000 and various updates on the CMS website, available at www.cms.gov/HospitalOutpatientPPS (Accessed May 27, 2010). Another useful website is the APC Reference Library, available at www.irpsys.com/apcref.htm (Accessed May 27, 2010).

Here m is the margin for each service, or the fee received by the physician minus his or her cost of providing the service, and x increases with the level of demand inducement, i. Total inducement (I) is expressed as:

$$(3) \quad I = N(i_1 + i_2)$$

Finally, choosing i_1 and i_2 to maximize utility subject to equations (2) and (3) gives the solution:

$$(4) \quad m_1 x_1' = m_2 x_2' = -U_1/U_Y$$

which equates the marginal return to inducement for each service to the marginal cost of inducement.

McGuire (2000) next considers the effect of a reduction in the fee for service 1 while the fee for service 2 stays the same. Because a reduction in margin m_1 lowers income thus raising the marginal utility of income, $-U_1/U_Y$ or the cost of inducement falls. Inducement for both service 1 and 2 will thus increase under the income effect. At the same time, a reduction in m_1 lowers the return to inducement of service 1. For Eq. (4) to hold, the physician will reduce inducement of service 1 and increase inducement of service 2. This is known as the substitution effect. Assuming that the payer of service 1 is Medicare, then the net effect of a Medicare fee cut on Medicare volume is ambiguous; it depends on the relative sizes of the income and substitution effects. For services paid by the other payer, however, a Medicare fee cut leads to an unambiguous increase in volume.

Several empirical studies test for patterns consistent with physician induced demand. These studies mostly examine physician fee cuts for so-called "overpriced" or "overvalued" Medicare procedures targeted by Congress in the Omnibus Budget Reconciliation Acts of 1987, 1988, and 1989. Nguyen and Derrick (1997) find that volume responses varied by specialty and practice; "losing" practices that experienced the largest fee cuts responded with significant

Medicare volume increases. Yip (1998) finds evidence that thoracic surgeons responded to a large fee reduction by increasing the volumes of both Medicare and private pay procedures. Rice et al. (1999) also find evidence of physician induced demand: fee reductions increased the volume of services paid by private insurers across a large number of procedures.

Our study examines the potential for this type of demand inducement in hospital outpatient departments. A large literature provides convincing empirical evidence that hospitals were able to influence physician decisions regarding admissions, discharges, and setting of care in response to Medicare's inpatient PPS.[11] Further, evidence of substantial integration between hospitals and physicians provides additional support for our investigation. For example, more than 40% of U.S. hospitals engaged in some type of strategic alliance with physicians in the 1990s (Burns et al. 2000). This type of vertical integration in healthcare may increase hospitals' influence over physicians' behavior and increase hospitals' ability to engage in demand inducement.

We examine the effect of OPPS-induced fee changes on several measures of hospital volume. We test whether Medicare volume increased, decreased, or stayed the same in response to Medicare price changes, a result that depends on the relative sizes of the income and substitution effects. We next test whether reductions in Medicare prices caused by OPPS increased the volume of services paid by non-Medicare payers, consistent with income and substitution effects that work in the same direction.

In addition, we examine another feature of the demand inducement hypothesis, namely, whether hospitals' responses vary according to the relative importance of Medicare patients to that hospital. McGuire and Pauly (1991) suggest that income effects from rate cuts by a particular payer would be larger for those physicians who receive a larger share of their income

[11] See, for example, Chalkley and Malcomson (2000) and Salkever (2000).

from that payer. By extension, we posit that hospitals where Medicare patients constitute a larger share of total outpatient patients are likely to experience larger income effects from OPPS-induced rate cuts. In other words, hospitals with larger Medicare patient shares are more "exposed" to OPPS relative to hospitals with lower Medicare shares. If income effects are larger at such hospitals, then we would expect to see heterogeneous effects on hospital volume, such that rate cuts would lead to larger increases in non-Medicare volume in high share hospitals compared to low share hospitals, and that rate cuts may lead to smaller decreases, or even increases, in Medicare volume at high share hospitals compared to low share hospitals.

4. Data

A. Hospital-Level Data on Medicare and non-Medicare Volume

Our main dataset is derived from Florida Ambulatory Discharge Data for years 1997 to 2008, which were obtained from the Florida Agency for Health Care Administration (AHCA). No existing national data suit our purposes, and we chose Florida from various state-level discharge databases because of the high data quality and the availability of multiple pre-OPPS years.[12] As the fourth most populous state in the nation, Florida also has a large number of hospitals, a factor that mitigates some concerns about the generalizability of our study. Further, changes in healthcare spending and utilization in Florida are relevant to Medicare as a whole; Florida has the second highest state Medicare spending and accounts for over 8% of all Medicare spending nationwide (CMS, 2007).

[12] For example, the widely-used Medicare claims data lack any information on services paid by non-Medicare payers. In fact, the Medicare outpatient claims are limited even if we had a narrow focus on Medicare volume alone. In the pre-OPPS years, the claims records have very limited information on the specific procedure codes, thus preventing construction of valid measures of counts and prices for specific outpatient surgeries (ResDAC, 2010). The National Hospital Ambulatory Medical Care Survey does not permit construction of a panel of hospitals because the survey assigns different hospital identifiers each year and the sample size is too small to aggregate to the hospital level.

The unit of observation in the raw data is a patient discharge; each discharge record consists of up to 14 specific procedures performed, the total charge for the discharge, the principal payers, limited patient information (e.g., race, ethnicity, age, and sex), and the facility type and unique identification number. The facility type can be a hospital with an outpatient department, an Ambulatory Surgery Center, or a stand-alone cardiac catheterization or lithotripsy clinic. We focus on hospitals because OPPS applies only to hospital outpatient services. The data aim to include the universe of all short-term acute care hospitals in Florida. In practice, there are about 200 such hospitals in each year, and the exact number varies slightly over time depending on reporting exemptions, entry/exit, and missing data.[13] We use the raw discharge records to construct hospital- and procedure-specific counts of outpatient volume for each year and by payer. For Medicare volume, we focus on fee-for-service Medicare, thus excluding Medicare discharges paid by HMO insurers. For non-Medicare volume, we focus on discharges where the primary payer was reported as private fee-for-service (FFS) insurance.[14] We also examine the combined volume of both payers.

We focus on outpatient surgical procedures given their large share of all hospital outpatient spending.[15] In 2007, surgical procedures accounted for 47% of all Medicare payments for hospital outpatient services (MedPAC, 2009b). Because of the large number of procedures in this group, we study the top ten most common surgical procedures performed in our sample hospitals' outpatient departments in 1999, the year prior to the implementation of OPPS. This

[13] Facilities performing fewer than 200 visits in a quarter are exempt from reporting. Also, a few hospitals are missing from the data for unknown reasons (personal communication with Florida AHCA staff members).

[14] We select private FFS payers because we expect the potential for demand inducement to be greater for this form of payment as compared to various forms of managed care insurance.

[15] These include procedures in the range of 10000-69999 in the Current Procedural Terminology, 4th edition.

list includes certain types of endoscopies, cataract surgeries, and colonoscopies, among other procedures (see Table 1C).

Table 1A reports the summary statistics of the utilization measures; for ease of presentation, data from 1999 are reported. In that year, 182 Florida hospital outpatient departments had an average of 7706 outpatient discharges. Roughly one-third were paid by traditional (fee-for-service) Medicare and another 13% were paid by private fee-for-service payers. The rows in Panel B include counts for the ten most common CPT codes in that year; together these represent about one-third of all surgical procedures performed in Florida hospital outpatient departments. Note that hospital counts vary based on our ability to calculate payment rates from the discharge data (see below).

B. Payment Rate Data

Because we focus on specific procedures and because hospitals were reimbursed different amounts for each procedure, we need data on the Medicare reimbursement rate for each hospital, procedure of interest, and year. For the post-OPPS years, we obtain quarterly CMS publications reporting payments for each ambulatory payment classification (APC) along with quarterly crosswalks from CPT to APC.[16] We adjust the APC payment rate for geographic differences using the hospital wage index to construct a dataset of the APC-based payment rate for each of the top ten CPTs by hospital and for each post-OPPS quarter. We then average the quarterly data to create annual measures.

Obtaining hospital- and procedure-specific payments in the pre-OPPS years is a key challenge when using discharge data, one that we address with an innovative solution.[17] Because

[16] These are available at www.cms.gov/PCPricer/OutPPS and www.irp.com/apc/apc_ref.html.

[17] Due to the complex Medicare reimbursement scheme for hospital outpatient services, "….it was difficult, if not impossible, to know the amount Medicare paid for a given outpatient service; even if this amount could have been

discharge records report *charges*, we convert each charge to a payment using an outpatient surgery payment-to-charge ratio imputed from each hospital's annual Medicare Cost Report. Before this conversion, however, *CPT-specific* charges must be defined because the discharge records report only the *total* charge associated with all procedures on the record.[18] We therefore develop an algorithm to impute those hospital- and procedure-specific charges for each hospital and for each of our top ten CPT codes in the pre-OPPS years. The algorithm, for a given hospital and a given procedure of interest, works as follows:

Step 1: We search among the hospital's single-procedure discharge records (those records with only one CPT procedure code) for the CPT procedure of interest. If such a single-procedure discharge exits, the associated *total charge* on this record identifies the *CPT-specific* charge. If Step 1 fails to produce a match, then we proceed to Step 2.

Step 2: We search for two-procedure discharges which contain the CPT of interest and another procedure that we call the "companion CPT." If we cannot find such a record, we proceed to Step 4. If such records do exist, then we look for single-procedure discharges containing only the companion CPT. If such single-procedure discharges exist, we calculate the charge for the CPT of interest as the difference between the charge for the two-procedure discharge and that for the single-procedure discharge with the companion CPT. If this does not result in a match (i.e., no single-procedure discharge contains the companion CPT), the algorithm goes to Step 3.

Step 3: We search for two-procedure discharges that contain the companion CPT and some other CPT (the "companion of the companion"). If such records exist, we then calculate the companion CPT's charge as in Step 2. Once we obtain the companion CPT's charge, we similarly calculate the charge for the CPT of interest. If this does not result in a match, we proceed to Step 4.

Step 4: We search for three-procedure discharges that contain the CPT of interest and a two procedure pair that we call the "companion pair." If we cannot find this type of record, we proceed to Step 5. If such records do exist, then we look for two-procedure discharges containing the companion pair. If such two-procedure discharges exist, we calculate the charge for the CPT

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determined, it could only have been known once the cost reporting process was complete, long after the service was provided" (MedPAC, 1999, pg.130).

[18] Recall that in our data, each discharge record can contain as many as fourteen procedures.

of interest as the difference between the charge for the three-procedure discharge and the charge for the companion pair discharge. If companion pair discharges do not exist, we search for single-procedure discharges containing each CPT from the companion pair and we calculate the charge for the CPT of interest as the difference between the charge for the three-procedure discharge and the combined charges for both single-procedure discharges in the companion pair. If this does not result in a match, we proceed to Step 5.

Step 5: We search for four-procedure discharges that contain the CPT of interest and a three procedure combination that we call the "companion group." If we cannot find such discharges, then the algorithm ends. If a companion group can be identified, we then search for a combination of discharges that would allow us to calculate the charge for the CPT of interest using the difference between the charge for the four-procedure discharge and the combined charges for the companion group. (For example, the companion group discharges could consist of three single-procedure discharges, or one two-procedure and one single-procedure discharge, or a single three-procedure discharge).

Step 6: Implement Steps 1-5 for all three pre-OPPS years. If Steps 1-5 fail to produce a CPT-specific charge for the hospital for all three years, the algorithm ends: we have no matches for this hospital and CPT procedure.[19] If Steps 1-5 produce the needed charges for all three years, the algorithm ends: we have successfully matched that hospital and CPT procedure to a charge. If any of Steps 1 through 5 results in multiple CPT-specific charges for a particular hospital in a given year, then we take the median charge to avoid the influence of outliers. Otherwise (i.e., the CPT-specific charge is identified for at least one year, but not for all three years), go to Step 7.

Step 7: Apply the medical CPI to the charge for the available year(s) to impute the charge for the missing year(s).

Using this algorithm, we are able to identify CPT-specific charges for 76-93% of the hospitals in our final sample depending on the CPT in question. Once we obtain these procedure- and hospital-specific charges, we apply hospital-specific outpatient surgery payment-to-charge ratios imputed from annual Medicare cost reports from 1997 to 1999. Using a payment-to-charge ratio specific to outpatient surgeries, instead of an *overall* ratio as used in some prior PPS studies, helps reduce the measurement error in our imputed payment rates.

[19] In principle, we could have continued our algorithm to discharge records containing six or more procedures. But each extra step increases measurement error in the imputed charge. We therefore chose to stop at four-procedure discharges.

Figure 1 illustrates our sample average Medicare payment rate (in constant 2008 dollars) over time for each of the top ten procedures. The majority of procedures experienced a decrease in their average Medicare payment rate after the implementation of OPPS. Indeed, average reimbursement rates decreased for all but one procedure between 1999 and 2004, the first year when OPPS was in full effect, with reductions ranging from 7 to 84% and averaging 22% (see Table 1C). For most procedures, there is a drop in the average payment rate between 1997 and 1998, something we return to in our robustness checks.

C. Other Explanatory Variables

As discussed earlier, OPPS also affects Medicare beneficiaries' coinsurance amounts. We obtain coinsurance data from two sources. For the post-OPPS years, we calculate coinsurance amounts from CMS quarterly releases of the OPPS PPS pricer documentation.[20] Because coinsurance was set at 20% of charges during the pre-OPPS years (MedPAC, 1999), we apply this percentage to our imputed hospital- and procedure-specific charges in 1997 through 1999. Coinsurance amounts are measured in constant (2008) dollars.

Given that private payers may have changed their reimbursements during our sample period, we control for private reimbursement rates for each procedure and year at the level of 3-digit zipcode in which the hospital is located.[21] These data were purchased from the MedStat MarketScan databases available from Thomson-Reuters. The 3-digit zipcode is the smallest geographic unit for which these data are available.

We also include various county- and hospital-level control variables in our analysis. We compile county-level data for each year from 1997-2008 from various sources. We construct the

[20] See www.cms.gov/PCPricer/OutPPS, accessed October 20, 2010.

[21] For example, private payers may follow Medicare's suit and modify their reimbursement rates.

number of ambulatory surgery centers by county and year using quarterly ambulatory discharge summaries from Florida. We obtain county-level estimates of the total population, the population age 65 and up, the female population and the Hispanic population from the U.S. Census. Because Census reports population estimates by racial subgroup differently before 2000 and from 2000 on, we obtain estimates of the black population from Florida Charts reports released by the Florida Legislature's Office of Economic and Demographic Research. We obtain estimates of median household income from the March Current Population Survey (CPS) and estimates of county unemployment rates from the BLS Local Area Unemployment reports.[22] We use Medicare managed care penetration rates from CMS for years in which they are available. County-level data on the number of patient-care MDs not employed by the federal government is extracted from the Area Resource File (ARF). We obtain potentially time-varying hospital-level controls such as ownership status, teaching status, bed size, statutory rural hospital status, and critical access hospital status from Florida's January issues of the *Hospital Beds and Services List* for years 1998 to 2009 and we apply January data from each year to the prior year.

Table 1B reports the summary statistics of the main hospital- and county-specific variables, also for 1999.[23] Only 3% of hospitals are teaching hospitals. Forty-five percent of hospitals are not-for-profit, about the same percentage are for-profit, and 11% are public. The

[22] Population estimates from the U.S. Census Bureau for years 1997 to 1999 were obtained from http://www.census.gov/popest/datasets.html#cntyinter. Population data for 2000 to 2008 are from www.census.gov/popest/counties/CO-EST2008-01.html. For all years except 2000, data are estimates as of July 1. Unemployment rates were obtained from annual average labor force data by county, available at www.bls.gov/lau/. Florida population estimates of racial subgroups were obtained from www.floridacharts.com/charts. Median household income data are obtained from the Census Bureau website available at www.census.gov/did/www/saipe/data/statecounty/data/. Median household income is measured in constant (2008) dollars.

[23] Our data use agreement with Thomson-Reuters does not allow us to disclose any information based on their data at the three-digit zipcode level, so summary statistics for private payer prices are not reported in Table 1A.

average bed count is 235. On average, county populations are 12% black, 8% Hispanic, 51% female, and 19% aged 65 and older.

5. Empirical Strategy:

To examine how hospital outpatient volume responds to the OPPS-induced payment changes, we estimate the following model:

$$(5)\quad \log(Surg\_Count_{ht}) = \beta_0 + \beta_1 \log(Payment_{ht}) + \beta_2 \log(Coins_{ht}) + \beta_3 \log(Private\_price) + \lambda_t + \alpha_h + Z_{ct}\prod + X_{ht}\Gamma + \varepsilon_{ht}$$

The dependent variable is the log of payer-specific outpatient surgery counts for a given procedure at hospital *h*'s outpatient department in time period *t*, where *t* ranges from 1997 to 2008.[24] Because OPPS was implemented August 1, 2000, we exclude 2000 data from all of the regression analysis. We estimate three variations of this model, where the dependent variable is defined as either: 1) combined Medicare and private FFS volume; 2) Medicare volume only; or 3) private FFS volume only. We separately estimate Equation (5) for the top ten most common surgical CPTs, allowing for differential responses across procedures.

$Payment_{ht}$ is the Medicare reimbursement rate for a given procedure in hospital *h* at time period *t*. $Coins_{ht}$ is the coinsurance amount associated with each procedure in each hospital and year. $Private\_price_{ht}$ is the procedure's median private payer reimbursement rate in time *t* in the 3-digit zipcode where hospital *h* is located.[25] Z_{ct} is a set of county-year-level controls (such as

[24] In the context of McGuire (2000), our volume measures are comparable to $N*x$, or the product of patients and procedures per patient.

[25] The results are almost identical when we use the mean price. Indeed, the results change little whether we control for the private payer prices or not, which suggests that private payment rates are orthogonal to Medicare payment rates. This is consistent with industry observations that various features of OPPS discouraged private plans from adopting it (see, for example, Leary and Farley, 2005). As examples of obstacles, Leary and Farley (2005) cite the fact that OPPS applies only to certain facilities (ASCs and several types of hospital outpatient departments are excluded) and that certain outpatient services are either excluded or not assigned to APCs.

the number of ASCs, income, and demographics as described in the Data section) and X_{ht} is a set of hospital-year-level controls (namely bed size, ownership status, and teaching status). We also control for year- and hospital- fixed effects (λ_t and α_h). The coefficient of interest in Eq. (5) is β_1, an elasticity measuring the hospitals' volume response to payment reimbursement rate changes.

To test for heterogeneous responses to the rate changes induced by OPPS, we next estimate the model below:

$$(6)\quad \log(Surg\_Count_{ht}) = \alpha_0 + \alpha_1 \log(Payment_{ht}) * Share_h + \alpha_2 \log(Payment_{ht}) + \alpha_3 \log(Coins)_{ht} + \alpha_4 \log(Private\_price_{ht}) + \lambda_t + \alpha_h + Z_{ct} \prod + X_{ht} \Gamma + \varepsilon_{ht}$$

In this model, $Share_h$ represents for each hospital h the share of the total number of outpatient surgeries in which Medicare fee-for-service (FFS) was the primary payer, measured in the last year prior to OPPS (1999). The share measure approximates a hospital's exposure to Medicare program changes; as noted earlier, a high share hospital should experience a stronger income effect because it is more exposed to OPPS. As a result, a high share hospital would respond to a rate cut with a larger increase in non-Medicare volume than a low share hospital. For Medicare volume, a larger income effect implies that compared to low share hospitals, highly exposed hospitals would respond to a rate cut with smaller volume decreases (if the substitution effect dominates) or, possibly, volume increases (if the income effect dominates because of a sufficiently high share value). This pattern of responses would be indicated by a negative α_1 estimate.

6. Results

A. Average Payment Effects

To examine the effects of OPPS on volume, we first estimate Equation (5) for counts of the top ten surgical procedures paid by both Medicare and private FFS combined, Medicare only,

and private FFS only. In the baseline regressions, we exclude all critical access hospitals and cancer and children's hospitals from the sample because these hospitals were either exempt from OPPS or received full hold harmless payments in the post-OPPS period. We also exclude data from 2001 to 2003, the transitional corridor period.[26] All county-level controls shown in Table 1B are included except for the number of patient-care non-federal MDs and the Medicare managed care penetration rate. The former variable is missing for 2008 and the latter is missing for both 2006 and 2007, so including these variables would greatly reduce our post-OPPS observation period.[27] The main regression results control for time-varying hospital traits including ownership status (public, for-profit hospitals, not-for-profit hospitals), teaching status, and acute beds.[28] Finally, we include year and hospital fixed effects in all regressions.

Regression results for Equation (5) are reported in Tables 2A, 2B, and 2C. Each table has ten columns, corresponding to the top ten procedures of interest. The first row in each table reports the estimated coefficient on payment. Given the log-log specification, this coefficient can be interpreted as a price elasticity of volume. For each elasticity estimate we report three standard errors: the unclustered standard errors in parentheses followed by robust standard errors clustered by county (the first row of brackets) and by hospital (the second row of brackets).

Table 2A reports results for models where the dependent variable is the combined count of procedures covered by both Medicare and private FFS insurers. Generally, payment elasticities are positive, although none of the coefficient estimates is statistically significant when robust standard errors are taken into account. When counts are separated by payer, however,

[26] Recall that during the transitional corridor period, hospitals received supplemental payments for cases in which the APC payment rates were less than the rates in place under cost-based reimbursement. Thus, hospitals were not fully subject to OPPS in this period.

[27] We will examine the sensitivity of including these two controls in the robustness check session.

[28] In another robustness check, we exclude the hospital-level controls because those controls are potentially endogenous, i.e., they may reflect hospitals' responses to OPPS.

payment elasticities are generally significant and opposite-signed. In the Medicare volume results reported in Table 2B, the positive elasticity estimates suggest that as Medicare payment rates decrease (increase), Medicare volume decreases (increases). In the private FFS volume results reported in Table 2C, the negative elasticity estimates suggest that private FFS volume is inversely related to Medicare payment changes. These results suggest a pattern consistent with demand inducement, in that Medicare volume declines (which would be the case if the substitution effect outweighs the income effect) and private FFS volume increases as Medicare payment rates are cut.

Turning to the coefficients on the control variables in the models, Tables 2A-2C show that coinsurance elasticities are negative and usually significant in the Medicare models and, for the most part, positive and significant in the private FFS models. The private payment rate elasticities are almost always insignificant. The coefficients on the number of ASCs in the county vary in sign in the private FFS regressions, but are usually negative and significant in the Medicare models. Of the demographic and economic controls, the share of the population aged 65 and up is consistently positive and significant in the Medicare models. There is also some evidence that, relative to for-profit hospitals, not-for-profit and government hospitals have lower volumes.

Despite the fact the payment rate changes were exogenously introduced through OPPS, potential endogeneity of the payment variable remains somewhat of a concern. In particular, the size of the payment rate change induced by OPPS differed across hospitals depending on hospitals' pre-OPPS cost-based reimbursement levels. Hospitals with the highest cost-based pre-OPPS payments would face the largest payment reduction when OPPS is in full effect. Suppose that those hospitals, for whatever exogenous reasons, gain private FFS patients but lose Medicare

patients even in the absence of OPPS. Such a scenario might arise if those hospitals are located in areas experiencing shifts in the distribution of population age (from old to young) or certain changes in the type of insurance coverage (perhaps coincident growth in both Medicare managed care and private FFS insurance). This could cause a spurious correlation between the payment rate and the dependent variables, thus biasing our coefficient estimates.

To test whether such a trend exists in the pre-OPPS period, we conduct the following falsification test. We assign the 2004 OPPS price (recall OPPS went into full effect in 2004) to 1999 and thus create a "fake payment" variable for 1999. Then we use data from only the three pre-OPPS years (1997-1999) to estimate Equation (5), treating 1997 and 1998 as pre-OPPS and 1999 as the fake post-OPPS year. If there had been pre-existing trends in Medicare and private FFS volume in hospitals with significant payment decreases, our "fake payment" coefficient would pick up this trend. If the coefficient estimates do not show a trend, we can be less concerned with the potential endogeneity of the Medicare payment variable.

Table 3 shows the results from this falsification test and the results are generally supportive of the exogeneity of the Medicare payment variable. In the majority of cases the payment coefficients are not statistically significant. Of the twenty payer-specific regressions, the fake payment variable is positive and significant in only two cases and their magnitudes are considerably smaller than those in Table 2B. Note that two estimates in the Medicare count regressions are significant and negative, suggesting that if anything, a pre-existing trend would bias against our findings. In light of these results, we are less concerned about the potential endogeneity of the payment variable.

B. Heterogeneous Payment Effects

We next examine the potential for heterogeneous responses by a hospital's exposure to Medicare. Tables 4A, 4B, and 4C report regressions results from the estimation of Equation (6) for combined Medicare and private FFS volume, Medicare volume, and private FFS volume, respectively. Again, these regressions include the interaction of the payment rate with the Medicare share variable. Negative estimates of the interaction term coefficient would support the demand inducement model by showing that hospitals with a heavier reliance on Medicare had larger income effects in response to a payment rate cut. The first row of these tables reports the coefficient of the interaction term and the second row reports the payment coefficient; again ten columns are included in each table for the top ten most common procedures. For the key explanatory variables, we report unclustered standard errors (in parentheses) and clustered standard errors by county and by hospitals (in brackets).

Table 4A reports results on the combined Medicare and private FFS counts. Generally, the payment coefficients are positive and the payment-share interaction term coefficients are negative. Compared to Table 2A, there are more statistically significant effects (even when robust standard errors are taken into account) now that we allow the payment elasticity to vary with Medicare share. This pattern of the coefficients strengthens when we examine Medicare volume separately. As shown in Table 4B, the coefficient estimates of the payment-share interaction term are negative in all but one case; in the majority of cases the estimates are also statistically significant. The payment coefficient estimates are consistently positive and mostly significant. Interestingly, the opposite signs of the interaction and payment terms in Table 4B are consistent with competing income and substitution effects in models of demand inducement. That is, the substitution effect may dominate a small income effect in a low-share hospital, thus a

low-share hospital experiences a drop in Medicare volume in response to Medicare payment cuts. For a hospital with a sufficiently high Medicare share, however, the larger income effect may dominate the substitution effect, causing Medicare volume to decrease by a lesser extent and possibly even increase, when the hospital faces a Medicare rate reduction. Column 1 of Table 4B illustrates one such case; for a hospital with a share greater than 45%, a reduction in the payment rate will result in a Medicare volume increase.[29]

We see similar evidence of heterogeneous responses from the payment-share interaction terms in Table 4C for the private FFS volume regressions. Of the ten estimates of the interaction term effect, all but one are negative and most are statistically significant. In contrast to the mostly significant and positive payment coefficient estimates in Table 4B for the Medicare volume regressions, the payment coefficient estimates here are mostly negative, although small in magnitude. Consistent with substitution and income effects working in the same direction, the coefficients on payment and the payment-share interaction have the same sign. Our results thus suggest that OPPS-induced price cuts led to increases in private FFS volume, and increases that were larger for hospitals with larger Medicare shares than for hospitals with lower Medicare shares. This is consistent with the higher share hospitals experiencing larger income effects in response to price reductions.

C. Robustness Checks

One concern about the main set of regression results is the quality of the MarketScan private payer reimbursement rate data. Specifically, in the early years of the sample, the Florida MarketScan database was based on a small number of contributing employers and healthcare

[29] For reference, the 75th percentile of the share variable is 43%.

providers.[30] For this reason, we conduct a robustness check which controls for 3-digit zipcode-specific year effects in place of the 3-digit zipcode level private payer price from MarketScan. This set of zipcode-year dummy variables absorbs any factors which vary over time at the 3-digit zipcode level, above and beyond private payer price. For example, ambulatory surgeries are increasingly performed in ASCs and physician offices, and this could affect the volume delivered in hospital outpatient departments. We do control for the number of ASCs, however, we do not observe surgical volumes in physicians' offices at all. Moreover, different payers' reimbursement rates for physicians' professional services may have changed over time; since physicians influence the delivery of care, this potential change may also affect whether and where an ambulatory surgery would be performed. By including a set of 3-digit zipcode-specific year effects, we control for those factors to the extent that they vary at the 3-digit zipcode level, even though we cannot completely eliminate their potential impacts.

Tables 5A and 5B show the results of this robustness check for our average and heterogeneous payment effects respectively. For the Medicare regression results in Panel B of Table 5A, estimated average payment elasticities are all positive, a pattern similar to our main Medicare average payment effect results shown in Table 2B. The magnitudes of the elasticity estimates are generally smaller and fewer are statistically significant, which is expected because the 3-digit zipcode-year fixed effects may have absorbed a considerable portion of the variation in the payment variable. In Panel B of Table 5B, our findings of heterogeneous responses to payment changes for Medicare volume are very robust to this check. In terms of our results from the private FFS volume regressions, the pattern in Panel C of Table 5A is very similar to our main findings for average effects in Table 2C. For the heterogeneous effects, the pattern in Panel

[30] Under the data use agreement with the vendor, we are not permitted to disclose the number of the employers and providers and the number of claims in the zipcode-year cells.

C of Table 5B also shows the same sign pattern as in Table 4C, although again most estimates are smaller in magnitude and fewer are significant.

We next test the sensitivity of our heterogeneous response results with a number of additional robustness tests. In a second robustness check, we add county-level time-varying controls for the number of patient-care, non-federal MDs and the Medicare managed care penetration rate, two variables that were omitted from the main analysis because of missing data in several years. In a third check, we exclude three hospital-level controls (bed size, ownership status, and teaching status) that could be potentially endogenous. A fourth robustness check excludes all of the small rural hospitals and sole community hospitals identified in our dataset; this is because CMS rules continued to shield those hospitals from OPPS even after the expiration of the transitional corridor period, as noted earlier. A fifth check includes data from 2001-2003, the transitional period. Another check excludes all control variables other than the year and hospital fixed effects to test whether our results are sensitive to time varying county-level and hospital-level controls. If they are, we might be more concerned about unobservable time-varying factors driving the observed patterns; if not, then we can be more confident about the exogeneity of our main variable of interest. Finally, we test the sensitivity of our results to excluding one year of the pre-OPPS period, 1997, given the noticeable drop in payment rates between 1997 and 1998 reported in Figure 1.[31]

The results are reported in Tables 6A, 6B, and 6C for the combined Medicare and private FFS volume, the Medicare volume, and the private FFS volume regressions, respectively. The first set of rows reproduces the baseline results. Overall, the tests suggest that our baseline

[31] We suspect that this drop is due to a change in CMS Medicare cost reporting policies. In both 1998 and 1999, the imputed payment-to-charge ratio is 25% while in 1997, that ratio is 30%. The CMS Cost Report Instruction Manual shows that in 1997, CMS changed the way coinsurance and deductibles were handled in the cost reports, so hospitals, depending on when they submitted the cost reports during fiscal year 1997, could have been subject to either the new or the old rules. This adds more noise to the imputed 1997 payment-to-charge ratio.

results are fairly robust to these changes. Importantly, we continue to see the same pattern in the sign and general significance of the coefficients for the payment variable and the share-payment interaction term across these various specifications. In the Medicare models, most payment coefficients remain positive and significant and most interaction term coefficients remain negative and significant. As in the main results presented in Table 4, this suggests that hospitals with more exposure to Medicare responded to rate cuts with smaller decreases (or, in some cases, increases) in outpatient Medicare volume relative to low share hospitals. In the private FFS volume models, some robustness checks produce fewer negative and significant share-payment coefficients (such as the exclusion of the 1997 data), but we continue to see a similar pattern in which hospitals with higher Medicare shares responded to rate cuts with larger increases in private FFS service provision relative to less exposed hospitals. Note that in robustness check 5 of Table 6C, the coefficient estimates are expected to be smaller than those in the main regressions because transitional corridor years are included in the sample.

7. Discussion

There is an explanation other than demand inducement for the *average payment effects* we observe in Table 2. That is, the pattern may also fit the behavior of a monopolist hospital provider facing demand that is segmented by payer. As described in Friedman et al. (2004), a price cut by one payer (e.g., Medicare) would reduce the hospital's intensity of care provided to Medicare patients under the assumption of increasing marginal cost of service intensity. In addition, because demand is segmented by payer, the reduction in Medicare service intensity would lower marginal cost and have the spillover effect of increasing service intensity for patients of the other payers. Because Medicare price and other-payer intensity are negatively

associated, this is deemed a negative spillover effect.[32] Our average effects reported in Tables 2B and 2C are consistent with this pattern, but it is important to note that this alternate theory offers no explanation for the heterogeneous effects we report in Table 4; in contrast, the Table 4 heterogeneous effects are directly supported by demand inducement as described in McGuire and Pauly (1991).

Moreover, we find additional suggestive evidence consistent with provider induced demand by testing whether outpatient volume responses to Medicare fee cuts vary by the level of vertical integration between hospitals and physicians. One might expect that in more integrated hospitals, there is more potential for inducement in response to payment rate reductions compared to hospitals where there is less integration with physicians. Following Cuellar and Gertler (2006) we measure the presence of physician-hospital integration using data from the American Hospital Association Annual Survey of Hospitals. We define an indicator for hospitals that have any of the common forms of integrated arrangements in place in the baseline year of our sample, and we interact the baseline integration indicator with the Medicare payment variable.[33] The results of this exercise are shown in Table 7. In both sets of Medicare and private FFS volume regressions, we see eight out of ten cases where the coefficient estimate on the interaction term is negative. This is what we would expect to see if integrated hospitals (those with a stronger influence over physicians, either through administrative control or

[32] Friedman et al. (2004) find evidence consistent with negative spillovers in that generous payments by other payers reduce hospital cost per discharge. Two other related studies from the hospital literature are also worth noting. Gertler (1989) finds that an increase in Medicaid payments to nursing homes increased the number of Medicaid patients but reduced the number of private pay patients, a pattern consistent with negative spillovers, and Dranove and White (1998) find that a cut in Medicaid reimbursements to hospitals led to declines in daily service levels and services per admission for both Medicaid and private patients, a pattern consistent with positive spillovers.

[33] These arrangements include Independent Physicians Associations (IPAs), Open Physician-Hospital Organizations (OPHOs), Closed Physician-Hospital Organizations (CPHOs), Management Service Organizations (MSOs), medical foundations, and salary or equity models. Also for this exercise, we use 2000 as the baseline year because of a noticeably higher response rate in that year compared to 1999. Specifically, the 2000 AHA survey was completed by 167 Florida general hospitals while the 1999 survey was completed by only 132 hospitals.

financial incentives) are more able to increase volume in response to a fee cut than non-integrated hospitals. Given that few integration-payment interaction terms are statistically significant in the private FFS volume regressions, we caution that these results are only suggestive evidence that is consistent with provider demand inducement. [34]

8. Conclusions

Our results suggest that OPPS-induced payment changes had significant effects on the volume of hospital outpatient surgical procedures provided to both Medicare and private FFS patients. On average, hospitals that experienced rates cuts under OPPS decreased Medicare volume and increased private FFS volume, and hospitals that saw reimbursement rates rise increased Medicare volume and lowered private FFS volume. Moreover, when we account for heterogeneous responses based on a hospital's exposure to OPPS (measured by Medicare share at baseline), we find that highly exposed hospitals responded to payment cuts with larger increases in private FFS volume than less exposed hospitals. In terms of Medicare volume responses to rate cuts, the highly exposed hospitals responded with smaller decreases, and in some cases increases, compared to less exposed hospitals. These results are consistent with highly exposed hospitals experiencing larger income effects in light of payment cuts, and we view these results as consistent with the theory of provider induced demand.

Our results suggest that Medicare payment reforms that retain a fee-for-service approach are unlikely to be effective in containing Medicare costs because such reforms could create the incentive for certain hospitals to induce demand among Medicare patients. Even more worrisome, reductions in fee-for-service payments may create incentives for hospitals to induce

[34] Note that Table 7 has smaller numbers of hospitals and thus observations because of hospital non-response to the AHA survey.

demand among non-Medicare patients. By affecting private payers as well, Medicare fee-for-service-type payment reforms are further limited in their ability to reduce societal costs. Our results provide support for the movement away from fee-for-service style reimbursement schemes and toward alternative forms of provider payment, particularly with regard to cost containment. The Affordable Care Act of 2010 includes provisions for a number of alternatives to fee-for-service, such as bundled payments and medical home payment models. While additional research is needed on the appropriate structures and the effectiveness of these alternative compensation schemes, these schemes essentially depart from fee-for-service payment in a way that OPPS does not, and would be less likely to generate demand inducement.

This study also provides interesting evidence that non-physician providers such as hospital outpatient departments may also engage in demand inducement, a finding that further highlights the serious challenge of containing healthcare costs under fee-for-service payment arrangements. This issue may become more important if recent trends in the vertical integration of physicians and hospitals continue, and perhaps even strengthen with the development of Accountable Care Organizations (ACOs).

Methodologically, this paper studies an important but empirically under-explored area in the voluminous PPS literature. That is, when Medicare changes its payment to healthcare providers, these changes may not only affect Medicare service utilization, but also may affect non-Medicare services. Barring a few exceptions in the 1980s and 1990s, almost all recent PPS studies seem to ignore this important potential response. Data limitations may explain this (e.g., many PPS studies use Medicare claims data, which contain no information on non-Medicare services), and with more and better data available, the non-Medicare service response can be an important future research area. In particular, using discharge data, instead of claims data, is a

promising direction. Finally, our results underscore the continued importance of testing for heterogeneous responses based on providers' exposure to the Medicare program.

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Table 1A. Summary Statistics for the Volume Measures (based on 1999 data)

	Variable Name	Mean	SD	N
Panel A: Overall counts of discharge records				
	Total count	7706	6046	182
	Combined count	3249	2703	182
	Medicare count	2444	2045	182
	Private FFS count	805	1273	182
	Medicare patients share	0.34	0.15	182
Panel B: Counts of the top 10 CPT codes				
1	Combined count	228	276	177
	Medicare count	170	182	177
	Private FFS count	58	143	177
2	Combined count	218	228	179
	Medicare count	167	173	179
	Private FFS count	51	106	179
3	Combined count	198	322	146
	Medicare count	182	305	146
	Private FFS count	16	38	146
4	Combined count	129	447	177
	Medicare count	83	168	177
	Private FFS count	45	290	177
5	Combined count	171	402	167
	Medicare count	152	354	167
	Private FFS count	19	82	167
6	Combined count	129	193	161
	Medicare count	112	169	161
	Private FFS count	18	52	161
7	Combined count	105	175	171
	Medicare count	80	113	171
	Private FFS count	25	84	171
8	Combined count	101	120	177
	Medicare count	80	97	177
	Private FFS count	21	50	177
9	Combined count	35	40	170
	Medicare count	22	30	170
	Private FFS count	13	20	170
10	Combined count	36	39	176
	Medicare count	25	30	176
	Private FFS count	11	16	176

Note: "Total count" in Panel A refers to the total number of discharges that occurred in a given hospital in 1999; "Combined count," in both Panels A and B, refers to the sum of only the Medicare and the private FFS counts.

Table 1B.Summary Statistics for Hospital- and County-Levels Control Variables

	Mean	SD	N
Hospital-Level Controls			
Ownership: Not-for-profit (*nfprofit*)	0.45	0.50	182
For-profit	0.45	0.50	182
Public (*govt*)	0.11	0.31	182
Teaching (*teaching*)	0.03	0.18	182
Acute Care Beds (*acutebeds*)	235	167	182
County-Level Controls			
Total Population (*totpop*)	332844	439114	46
Proportion black (*prop_black*)	12.14	6.99	46
Proportion Hispanic (*prop_hisp*)	8.13	9.26	46
Proportion Female (*prop_female*)	50.55	1.75	46
Proportion Age 65 & up (*prop_age65*)	19.12	7.73	46
Median Household Income (in thousands of $) (*medhhinc*)	33.56	3.	46
Unemployment Rate (*unemprate*)	4.43	2.31	46
Number of Ambulatory Surgery Centers (*num_asc*)	7.51	5.83	46
Patient care MDs	710	1154	46
Medicare Managed Care Penetration Rate	17	16	46

Notes: All data pertain to 1999, as noted in the text. Terms in parentheses indicate the abbreviated variable name used in tables of regression results. The number of Florida counties in our dataset is smaller than the total number of counties (67) in the state because: a) some counties do not have hospitals; b) some small hospitals are not required to report data to the Florida AHCA, and c) some hospitals have missing data on other variables. Missing data on the sole hospital in a county means that county is no longer included in the sample.

Table 1C. Average Medicare Payment Rates for the Top 10 CPT Codes in 1999 and 2004

Code	Index	1999	2004	Diff(%)	Short Description
43239	1	537	458	-15%	Upper GI endoscopy, biopsy
45378	2	480	486	1%	Diagnostic colonoscopy
66984	3	1451	1347	-7%	Extracapsular cataract removal
45380	4	574	486	-15%	Colonoscopy and biopsy
11042	5	931	151	-84%	Debride skin/tissue
36430	6	395	215	-46%	Blood transfusion service
45384	7	590	486	-18%	Lesion remove colonoscopy
45385	8	597	486	-19%	Lesion removal
19120	9	1092	1035	-5%	Removal of breast lesion
49505	10	1653	1580	-4%	Repair inguinal hernia

Table 2A. Combined Volume: Payment-level Regressions (Average Effect)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of Combined Counts (Medicare + Private Fee-for-Service)									
log (payment)	**0.16**	**0.33***	**0.22**	**0.29**	**0.60****	**0.62****	**0.17**	**0.38***	**0.29****	**0.22***
	(0.20)	**(0.19)**	**(0.29)**	**(0.19)**	**(0.29)**	**(0.28)**	**(0.22)**	**(0.20)**	**(0.14)**	**(0.13)**
	[0.24]	**[0.26]**	**[0.55]**	**[0.20]**	**[0.66]**	**[0.48]**	**[0.30]**	**[0.24]**	**[0.24]**	**[0.20]**
	[0.26]	**[0.27]**	**[0.63]**	**[0.25]**	**[0.55]**	**[0.40]**	**[0.33]**	**[0.31]**	**[0.22]**	**[0.20]**
log (coins)	-0.18	-0.53***	-0.10	-0.28	-0.96***	-0.60**	-0.23	-0.35*	-0.20	-0.11
	(0.20)	(0.18)	(0.28)	(0.19)	(0.29)	(0.29)	(0.21)	(0.20)	(0.14)	(0.12)
log (private_pay)	0.12	-0.47**	-0.08	-0.02	0.19**	-0.10	-0.08	0.02	-0.01	-0.03
	(0.13)	(0.20)	(0.20)	(0.15)	(0.08)	(0.06)	(0.13)	(0.16)	(0.08)	(0.06)
num_asc	-0.07***	-0.05***	-0.09***	-0.07***	0.04*	-0.02	-0.08***	-0.08***	-0.01	-0.01
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
totpop	0.00***	0.00***	0.00***	0.00***	-0.00	-0.00*	0.00***	0.00***	0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
prop_black	-0.02	-0.09*	0.06	-0.04	-0.01	0.28***	-0.10*	-0.11**	-0.02	0.01
	(0.05)	(0.05)	(0.07)	(0.05)	(0.08)	(0.07)	(0.06)	(0.05)	(0.04)	(0.03)
prop_hisp	-0.03	-0.04	-0.26***	-0.05	0.00	0.00	0.02	-0.06	-0.02	-0.02
	(0.04)	(0.03)	(0.07)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
prop_female	-0.07	-0.18	-0.25	-0.49***	-0.46*	-0.02	-0.36*	0.06	0.09	0.25**
	(0.18)	(0.17)	(0.25)	(0.18)	(0.27)	(0.27)	(0.21)	(0.19)	(0.13)	(0.11)
prop_age65	0.18***	0.14***	0.19***	0.20***	-0.14***	0.12**	0.22***	0.20***	0.08***	0.07***
	(0.03)	(0.03)	(0.05)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)	(0.02)	(0.02)
unemprate	-0.08	-0.13	0.21	-0.09	-0.79***	0.13	0.08	-0.10	0.09	0.16
	(0.15)	(0.14)	(0.22)	(0.15)	(0.22)	(0.21)	(0.17)	(0.16)	(0.11)	(0.10)
unemprate^2	0.01	0.02	-0.02	0.02	0.05***	-0.00	-0.00	0.02	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
medhhinc	-0.12	-0.20*	0.01	-0.13	-0.35**	0.29*	0.01	-0.29**	-0.02	-0.03
	(0.11)	(0.10)	(0.16)	(0.11)	(0.16)	(0.16)	(0.12)	(0.11)	(0.08)	(0.07)
medhhinc^2	0.00	0.00*	-0.00	0.00	0.00**	-0.00	-0.00	0.00**	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nfprofit	-0.26	-0.33**	-0.30	-0.15	-0.73***	-0.87***	-0.32*	-0.23	-0.21*	-0.34***
	(0.18)	(0.16)	(0.27)	(0.17)	(0.27)	(0.26)	(0.19)	(0.18)	(0.13)	(0.11)
govt	-0.86***	-0.67**	-1.67***	-0.98***	-0.46	-1.41***	-0.64*	-0.76**	-0.39*	-0.64***
	(0.31)	(0.29)	(0.45)	(0.29)	(0.50)	(0.48)	(0.33)	(0.31)	(0.20)	(0.18)
teaching	0.36	-0.19	0.79*	0.25	-1.34***	0.10	0.15	0.20	-0.54***	-0.31*
	(0.28)	(0.26)	(0.43)	(0.26)	(0.42)	(0.40)	(0.31)	(0.28)	(0.20)	(0.18)
acutebeds	0.00	0.00**	-0.00	0.00*	-0.00	0.00**	0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	4.83	18.51**	7.82	26.31***	42.13***	-8.12	17.26	2.27	-3.59	-12.10**
	(9.69)	(9.13)	(13.49)	(9.25)	(13.92)	(13.96)	(11.05)	(9.99)	(6.83)	(6.00)
# of obs	1376	1383	1305	1377	1351	1336	1361	1377	1363	1386
R^2	0.082	0.114	0.289	0.098	0.154	0.058	0.135	0.119	0.369	0.137
# of hospitals	198	199	196	199	198	198	197	199	198	199

Notes: All models also include hospital and year fixed effects. Unclustered standard errors are reported in parentheses. For the key explanatory variable, the first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 2B. Medicare Volume: Payment-level Regressions (Average Effect)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of Medicare Counts									
log (payment)	**0.34***	**0.44****	**0.25**	**0.46****	**0.59****	**0.68****	**0.34**	**0.53*****	**0.56*****	**0.45*****
	(0.20)	**(0.19)**	**(0.30)**	**(0.19)**	**(0.29)**	**(0.28)**	**(0.22)**	**(0.20)**	**(0.14)**	**(0.13)**
	[0.25]	**[0.26]***	**[0.58]**	**[0.21]****	**[0.68]**	**[0.47]**	**[0.28]**	**[0.23]****	**[0.18]*****	**[0.19]****
	[0.26]	**[0.26]***	**[0.65]**	**[0.25]***	**[0.54]**	**[0.41]***	**[0.32]**	**[0.30]***	**[0.19]*****	**[0.21]****
log (coins)	-0.28	-0.58***	-0.05	-0.40**	-0.95***	-0.65**	-0.36*	-0.42**	-0.44***	-0.31**
	(0.21)	(0.19)	(0.29)	(0.19)	(0.30)	(0.29)	(0.21)	(0.20)	(0.13)	(0.12)
log (private_pay)	0.06	-0.58***	-0.19	-0.04	0.21**	-0.10	-0.10	-0.04	0.08	-0.03
	(0.14)	(0.20)	(0.21)	(0.16)	(0.08)	(0.07)	(0.13)	(0.16)	(0.08)	(0.06)
num_asc	-0.08***	-0.06***	-0.09***	-0.08***	0.04	-0.02	-0.08***	-0.08***	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
totpop	0.00***	0.00***	0.00***	0.00***	-0.00	-0.00**	0.00***	0.00***	0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
prop_black	0.01	-0.03	0.06	-0.01	-0.02	0.27***	-0.07	-0.07	-0.00	0.01
	(0.05)	(0.05)	(0.08)	(0.05)	(0.08)	(0.07)	(0.06)	(0.05)	(0.04)	(0.03)
prop_hisp	-0.05	-0.05	-0.25***	-0.07**	0.01	0.00	0.00	-0.07*	-0.03	-0.03
	(0.04)	(0.03)	(0.07)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)
prop_female	-0.22	-0.31*	-0.31	-0.68***	-0.49*	-0.02	-0.47**	-0.08	-0.09	0.15
	(0.19)	(0.18)	(0.26)	(0.18)	(0.27)	(0.27)	(0.21)	(0.19)	(0.13)	(0.11)
prop_age65	0.19***	0.15***	0.18***	0.22***	-0.16***	0.11**	0.24***	0.22***	0.10***	0.07***
	(0.04)	(0.03)	(0.06)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)	(0.02)	(0.02)
unemprate	-0.11	-0.21	0.13	-0.14	-0.90***	0.10	0.00	-0.17	0.10	0.08
	(0.16)	(0.15)	(0.23)	(0.15)	(0.23)	(0.22)	(0.17)	(0.16)	(0.11)	(0.10)
$unemprate^2$	0.01	0.02*	-0.02	0.02	0.06***	-0.00	0.00	0.02	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
medhhinc	-0.14	-0.21**	0.01	-0.15	-0.39**	0.23	0.01	-0.28**	-0.04	-0.04
	(0.12)	(0.11)	(0.16)	(0.11)	(0.16)	(0.16)	(0.12)	(0.12)	(0.08)	(0.07)
$medhhinc^2$	0.00	0.00*	-0.00	0.00	0.00**	-0.00	-0.00	0.00**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nfprofit	-0.26	-0.29*	-0.27	-0.17	-0.76***	-0.85***	-0.35*	-0.18	-0.10	-0.35***
	(0.18)	(0.17)	(0.27)	(0.17)	(0.27)	(0.26)	(0.19)	(0.18)	(0.12)	(0.11)
govt	-0.90***	-0.74**	-1.78***	-1.08***	-0.50	-1.52***	-0.83**	-0.80**	-0.31	-0.65***
	(0.32)	(0.30)	(0.46)	(0.30)	(0.50)	(0.48)	(0.33)	(0.31)	(0.20)	(0.18)
teaching	0.17	0.04	0.87**	0.15	-1.24***	0.22	0.34	0.24	-0.37*	-0.21
	(0.29)	(0.27)	(0.44)	(0.27)	(0.42)	(0.40)	(0.31)	(0.28)	(0.20)	(0.18)
acutebeds	0.00*	0.00***	-0.00	0.00**	-0.00	0.00**	0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	12.11	24.96***	11.77	35.69***	45.08***	-5.98	22.35**	8.48	4.00	-7.24
	(10.13)	(9.38)	(13.82)	(9.48)	(13.99)	(14.04)	(11.07)	(10.05)	(6.63)	(6.00)
# of obs	1376	1383	1305	1377	1351	1336	1361	1377	1363	1386
R^2	0.067	0.102	0.258	0.089	0.177	0.063	0.119	0.111	0.272	0.078
# of hospitals	198	199	196	199	198	198	197	199	198	199

Notes: All models also include hospital and year fixed effects. Unclustered standard errors are reported in parentheses. For the key explanatory variable, the first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 2C. Private Fee-for-Service Volume: Payment-level Regressions (Average Effect)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of Private Fee-for-Service Counts									
log (payment)	**-1.14*** **	**-0.82*** **	**0.02**	**-0.68*** **	**-0.04**	**-0.31**	**-0.62*** **	**-0.68*** **	**-0.42** **	**-0.53*** **
	(0.22)	**(0.23)**	**(0.19)**	**(0.20)**	**(0.23)**	**(0.21)**	**(0.21)**	**(0.20)**	**(0.16)**	**(0.17)**
	[0.36]* **	**[0.38]** **	**[0.26]**	**[0.27]** **	**[0.31]**	**[0.29]**	**[0.39]**	**[0.38]* **	**[0.35]**	**[0.33]**
	[0.36]* **	**[0.39]** **	**[0.32]**	**[0.33]** **	**[0.34]**	**[0.28]**	**[0.35]* **	**[0.38]* **	**[0.30]**	**[0.26]** **
log (coins)	0.91***	0.60***	-0.10	0.47**	-0.30	0.25	0.52**	0.44**	0.36**	0.42***
	(0.22)	(0.22)	(0.18)	(0.21)	(0.23)	(0.22)	(0.20)	(0.20)	(0.16)	(0.16)
log (private_pay)	0.15	-0.25	0.13	0.14	-0.00	-0.09*	-0.12	0.27*	-0.05	0.03
	(0.15)	(0.24)	(0.13)	(0.17)	(0.07)	(0.05)	(0.12)	(0.16)	(0.10)	(0.08)
num_asc	-0.03*	-0.00	-0.05***	-0.02	0.02	0.03**	-0.03**	-0.03	-0.00	-0.00
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
totpop	0.00**	0.00	0.00*	0.00	0.00	0.00	0.00	0.00**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
prop_black	-0.14**	-0.23***	-0.02	-0.14***	-0.12**	0.04	-0.10*	-0.22***	-0.05	-0.03
	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)
prop_hisp	0.06	0.07*	-0.05	0.07*	0.00	-0.02	0.07*	0.05	0.05	-0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
prop_female	0.04	-0.01	-0.07	-0.04	-0.21	0.08	0.00	0.24	0.08	0.24
	(0.21)	(0.21)	(0.17)	(0.19)	(0.21)	(0.20)	(0.20)	(0.19)	(0.15)	(0.15)
prop_age65	0.03	0.01	0.07*	0.07*	-0.10**	0.06*	0.09**	0.03	0.02	0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
unemprate	-0.06	0.05	0.14	0.16	-0.51***	-0.03	0.28*	0.12	-0.08	0.06
	(0.18)	(0.17)	(0.15)	(0.16)	(0.17)	(0.16)	(0.16)	(0.16)	(0.13)	(0.12)
unemprate^2	0.01	0.01	-0.01	-0.00	0.04***	0.01	-0.02	-0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
medhhinc	0.06	0.01	-0.01	0.03	-0.17	0.28**	0.15	-0.14	0.03	0.03
	(0.13)	(0.13)	(0.11)	(0.12)	(0.13)	(0.12)	(0.12)	(0.12)	(0.09)	(0.09)
medhhinc^2	-0.00	-0.00	-0.00	-0.00	0.00	-0.00*	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nfprofit	-0.27	-0.36*	0.05	-0.17	-0.34	-0.33*	-0.19	-0.26	-0.21	-0.29**
	(0.20)	(0.20)	(0.18)	(0.18)	(0.21)	(0.20)	(0.18)	(0.18)	(0.15)	(0.14)
govt	-0.53	-0.19	-0.10	-0.73**	0.29	-0.49	-0.16	-0.33	-0.40*	-0.59**
	(0.35)	(0.35)	(0.29)	(0.32)	(0.39)	(0.37)	(0.32)	(0.32)	(0.24)	(0.23)
teaching	0.34	-0.31	-0.02	0.03	-0.93***	-0.47	-0.25	-0.16	-0.30	-0.48**
	(0.32)	(0.32)	(0.28)	(0.29)	(0.32)	(0.30)	(0.30)	(0.29)	(0.24)	(0.23)
acutebeds	-0.00	-0.00	-0.00***	-0.00*	-0.00**	-0.00	-0.00***	-0.00**	-0.00***	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.67	7.97	4.42	3.76	23.04**	-11.32	-1.81	-6.59	-0.83	-10.43
	(11.02)	(11.12)	(8.84)	(10.10)	(10.77)	(10.69)	(10.57)	(10.20)	(7.94)	(7.80)
# of obs	1376	1383	1305	1377	1351	1336	1361	1377	1363	1386
R^2	0.321	0.260	0.282	0.267	0.109	0.154	0.249	0.226	0.433	0.326
# of hospitals	198	199	196	199	198	198	197	199	198	199

Notes: All models also include hospital and year fixed effects. Unclustered standard errors are reported in parentheses. For the key explanatory variable, the first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 3. Falsification Test of Pre-trending

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Log of Combined Counts (Medicare + Private Fee-for-Service)										
log (fake payment)	**0.23**	**0.02**	**-0.46**	**-0.10**	**-0.53***	**-0.47**	**0.20**	**0.04**	**0.30**	**0.16**
	(0.20)	**(0.17)**	**(0.34)**	**(0.17)**	**(0.28)**	**(0.31)**	**(0.21)**	**(0.20)**	**(0.18)**	**(0.14)**
	(0.20)	**(0.17)**	**(0.34)**	**(0.17)**	**(0.28)**	**(0.31)**	**(0.21)**	**(0.20)**	**(0.18)**	**(0.14)**
	[0.18]	**[0.15]**	**[0.33]**	**[0.13]**	**[0.32]**	**[0.41]**	**[0.26]**	**[0.15]**	**[0.16]***	**[0.17]**
# of obs	484	491	411	485	459	444	469	485	469	493
R^2	0.100	0.157	0.125	0.099	0.200	0.086	0.094	0.153	0.112	0.128
# of hospitals	173	176	146	174	165	160	167	174	166	176
Panel B: log of Medicare Counts										
log (fake payment)	**0.19**	**0.00**	**-0.55**	**-0.14**	**-0.61****	**-0.50**	**0.25**	**-0.01**	**0.29**	**0.04**
	(0.21)	**(0.19)**	**(0.34)**	**(0.18)**	**(0.28)**	**(0.32)**	**(0.22)**	**(0.20)**	**(0.18)**	**(0.16)**
	(0.21)	**(0.19)**	**(0.34)**	**(0.18)**	**(0.28)**	**(0.32)**	**(0.22)**	**(0.20)**	**(0.18)**	**(0.16)**
	[0.18]	**[0.11]**	**[0.32]***	**[0.14]**	**[0.27]****	**[0.42]**	**[0.21]**	**[0.14]**	**[0.17]***	**[0.11]**
# of obs	484	491	411	485	459	444	469	485	469	493
R^2	0.081	0.147	0.120	0.075	0.219	0.065	0.082	0.127	0.064	0.075
# of hospitals	173	176	146	174	165	160	167	174	166	176
Panel C: log of Private Fee-for-Service Counts										
log (fake payment)	**0.04**	**-0.18**	**-0.41**	**-0.31**	**-0.23**	**-0.31**	**-0.04**	**-0.12**	**0.10**	**-0.21**
	(0.23)	**(0.23)**	**(0.29)**	**(0.22)**	**(0.27)**	**(0.30)**	**(0.24)**	**(0.25)**	**(0.23)**	**(0.22)**
	(0.23)	**(0.23)**	**(0.29)**	**(0.22)**	**(0.27)**	**(0.30)**	**(0.24)**	**(0.25)**	**(0.23)**	**(0.22)**
	[0.23]	**[0.23]**	**[0.31]**	**[0.18]***	**[0.36]**	**[0.30]**	**[0.28]**	**[0.24]**	**[0.22]**	**[0.25]**
# of obs	484	491	411	485	459	444	469	485	469	493
R^2	0.080	0.096	0.101	0.079	0.066	0.090	0.081	0.084	0.071	0.064
# of hospitals	173	176	146	174	165	160	167	174	166	176

Notes: Unclustered standard errors are reported in parentheses. The first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. All models control for the additional variables listed in Table 2A plus hospital and year fixed effects. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 4A. Combined Volume: Payment Share Interaction (Heterogeneous Response)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of Combined Counts (Medicare + Private Fee-for-Service)									
log (pay)*share	**-1.46*** **	**-0.70**	**-2.08*** **	**-1.98*** **	**-1.03*** **	**0.39**	**-1.01*** **	**-0.82** **	**-0.80*** **	**-1.33*** **
	(0.41)	**(0.56)**	**(0.54)**	**(0.34)**	**(0.27)**	**(0.46)**	**(0.35)**	**(0.36)**	**(0.28)**	**(0.25)**
	[0.97]	**[1.08]**	**[0.97]** **	**[0.46]*** **	**[0.77]**	**[0.83]**	**[0.57]***	**[0.66]**	**[0.35]** **	**[0.34]*** **
	[0.98]	**[1.10]**	**[1.08]***	**[0.48]*** **	**[0.65]**	**[0.71]**	**[0.53]***	**[0.72]**	**[0.36]** **	**[0.35]*** **
log (payment)	**0.56** **	**0.47***	**0.74** **	**0.90*** **	**0.97*** **	**0.51**	**0.53** **	**0.55** **	**0.48*** **	**0.52*** **
	(0.22)	**(0.24)**	**(0.33)**	**(0.21)**	**(0.32)**	**(0.32)**	**(0.24)**	**(0.23)**	**(0.16)**	**(0.14)**
	[0.39]	**[0.50]**	**[0.54]**	**[0.21]*** **	**[0.89]**	**[0.46]**	**[0.32]**	**[0.27]** **	**[0.27]***	**[0.21]** **
	[0.40]	**[0.46]**	**[0.68]**	**[0.30]*** **	**[0.61]**	**[0.43]**	**[0.36]**	**[0.40]**	**[0.24]** **	**[0.22]** **
log (coins)	-0.24	-0.49***	-0.15	-0.38**	-0.98***	-0.60**	-0.31	-0.34*	-0.21	-0.15
	(0.20)	(0.19)	(0.29)	(0.19)	(0.30)	(0.29)	(0.22)	(0.20)	(0.14)	(0.12)
log (private_pay)	0.08	-0.41**	-0.11	-0.01	0.20**	-0.11	-0.12	0.07	0.01	-0.03
	(0.13)	(0.20)	(0.20)	(0.16)	(0.09)	(0.07)	(0.13)	(0.16)	(0.08)	(0.06)
num_asc	-0.05***	-0.04***	-0.10***	-0.06***	0.05**	-0.02	-0.07***	-0.07***	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
totpop	0.00***	0.00***	0.00***	0.00***	-0.00	-0.00*	0.00***	0.00***	0.00**	0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
prop_black	-0.03	-0.10**	-0.00	-0.06	0.04	0.26***	-0.11*	-0.12**	-0.02	-0.01
	(0.05)	(0.05)	(0.08)	(0.05)	(0.08)	(0.08)	(0.06)	(0.05)	(0.04)	(0.03)
prop_hisp	-0.07*	-0.05	-0.29***	-0.09**	-0.00	0.00	0.00	-0.07*	-0.04	-0.04*
	(0.04)	(0.03)	(0.07)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
prop_female	-0.15	-0.18	-0.31	-0.54***	-0.44*	-0.01	-0.47**	0.02	0.05	0.26**
	(0.18)	(0.17)	(0.26)	(0.17)	(0.27)	(0.27)	(0.21)	(0.19)	(0.13)	(0.11)
prop_age65	0.18***	0.14***	0.19***	0.20***	-0.13**	0.10**	0.24***	0.20***	0.09***	0.06***
	(0.03)	(0.03)	(0.06)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)	(0.02)	(0.02)
unemprate	-0.04	-0.11	0.20	-0.02	-0.69***	0.06	0.10	-0.05	0.09	0.15
	(0.15)	(0.14)	(0.23)	(0.15)	(0.23)	(0.22)	(0.17)	(0.16)	(0.11)	(0.10)
unemprate^2	0.01	0.02	-0.02	0.01	0.05**	0.00	-0.00	0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
medhhinc	-0.09	-0.21*	-0.03	-0.13	-0.34**	0.31*	0.00	-0.29**	-0.05	-0.03
	(0.11)	(0.11)	(0.17)	(0.11)	(0.17)	(0.16)	(0.12)	(0.12)	(0.08)	(0.07)
medhhinc^2	0.00	0.00*	-0.00	0.00	0.00*	-0.00	-0.00	0.00**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nfprofit	-0.31	-0.23	-0.30	-0.15	-0.21	-0.73***	-0.45**	-0.17	-0.11	-0.23*
	(0.19)	(0.18)	(0.30)	(0.18)	(0.30)	(0.28)	(0.21)	(0.20)	(0.14)	(0.12)
govt	-1.34***	-0.74**	-1.69***	-1.25***	0.02	-1.33***	-1.08***	-0.85**	-0.23	-0.49**
	(0.34)	(0.33)	(0.47)	(0.33)	(0.51)	(0.49)	(0.38)	(0.36)	(0.23)	(0.20)
teaching	0.39	0.07	0.81	0.27	-0.67	-0.04	0.38	0.43	-0.57**	-0.25
	(0.29)	(0.28)	(0.50)	(0.28)	(0.46)	(0.42)	(0.34)	(0.31)	(0.22)	(0.20)
acutebeds	0.00	0.00**	-0.00	0.00	-0.00	0.00***	-0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	8.68	18.84**	14.29	29.63***	38.60***	-8.20	22.75**	4.45	-0.38	-10.61*
	(9.49)	(9.13)	(13.87)	(9.12)	(14.08)	(14.16)	(11.12)	(10.02)	(6.83)	(6.02)
# of obs	1275	1281	1209	1275	1254	1244	1262	1275	1262	1284
R^2	0.108	0.126	0.310	0.134	0.161	0.060	0.155	0.139	0.394	0.159
# of hospitals	175	175	174	175	175	175	175	175	175	175

Notes: All models also include hospital and year fixed effects. Unclustered standard errors are reported in parentheses. For the key explanatory variables, the first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 4B. Medicare Volume: Payment Share Interaction (Heterogeneous Response)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of Medicare Counts									
log (pay)*share	**-1.77*** **	**-0.94**	**-2.55*** **	**-2.18*** **	**-0.94*** **	**0.65**	**-1.11*** **	**-1.12*** **	**-0.91*** **	**-1.50*** **
	(0.43)	**(0.57)**	**(0.55)**	**(0.35)**	**(0.27)**	**(0.46)**	**(0.35)**	**(0.36)**	**(0.27)**	**(0.25)**
	[1.17]	**[1.33]**	**[1.02]****	**[0.54]*** **	**[0.82]**	**[0.90]**	**[0.71]**	**[0.77]**	**[0.34]*** **	**[0.33]*** **
	[1.22]	**[1.20]**	**[1.11]****	**[0.56]*** **	**[0.70]**	**[0.73]**	**[0.66]***	**[0.82]**	**[0.34]*** **	**[0.34]*** **
log (payment)	**0.80*** **	**0.68*** **	**0.89*** **	**1.09*** **	**0.93*** **	**0.49**	**0.72*** **	**0.79*** **	**0.80*** **	**0.79*** **
	(0.23)	**(0.25)**	**(0.33)**	**(0.22)**	**(0.32)**	**(0.33)**	**(0.24)**	**(0.23)**	**(0.16)**	**(0.14)**
	[0.41]*	**[0.57]**	**[0.59]**	**[0.23]*** **	**[0.93]**	**[0.47]**	**[0.33]****	**[0.27]*** **	**[0.19]*** **	**[0.19]*** **
	[0.45]*	**[0.49]**	**[0.69]**	**[0.31]*** **	**[0.62]**	**[0.45]**	**[0.38]***	**[0.41]***	**[0.21]*** **	**[0.21]*** **
log (coins)	-0.31	-0.55***	-0.11	-0.45**	-0.97***	-0.64**	-0.41*	-0.41**	-0.47***	-0.35***
	(0.21)	(0.19)	(0.30)	(0.19)	(0.30)	(0.29)	(0.22)	(0.20)	(0.14)	(0.12)
log (private_pay)	0.04	-0.46**	-0.22	-0.01	0.22**	-0.11	-0.14	0.02	0.09	-0.04
	(0.14)	(0.21)	(0.21)	(0.16)	(0.09)	(0.07)	(0.13)	(0.16)	(0.08)	(0.06)
num_asc	-0.06***	-0.05***	-0.10***	-0.07***	0.05**	-0.02	-0.08***	-0.07***	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
totpop	0.00***	0.00***	0.00***	0.00***	-0.00	-0.00**	0.00***	0.00***	0.00**	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
prop_black	0.00	-0.04	-0.02	-0.02	0.03	0.25***	-0.08	-0.08	-0.00	-0.01
	(0.05)	(0.05)	(0.08)	(0.05)	(0.08)	(0.08)	(0.06)	(0.05)	(0.04)	(0.03)
prop_hisp	-0.08**	-0.06*	-0.28***	-0.10***	0.01	0.01	-0.01	-0.08**	-0.05**	-0.04*
	(0.04)	(0.04)	(0.07)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
prop_female	-0.29	-0.31*	-0.38	-0.72***	-0.46*	-0.02	-0.57***	-0.13	-0.13	0.17
	(0.19)	(0.18)	(0.26)	(0.18)	(0.27)	(0.27)	(0.21)	(0.19)	(0.13)	(0.11)
prop_age65	0.20***	0.15***	0.18***	0.23***	-0.14***	0.10**	0.26***	0.23***	0.11***	0.07***
	(0.04)	(0.03)	(0.06)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)
unemprate	-0.03	-0.17	0.11	-0.03	-0.81***	0.02	0.03	-0.09	0.10	0.09
	(0.16)	(0.15)	(0.24)	(0.15)	(0.23)	(0.22)	(0.17)	(0.16)	(0.11)	(0.10)
unemprate^2	0.01	0.02	-0.02	0.01	0.05***	0.01	0.00	0.02	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
medhhinc	-0.10	-0.23**	-0.03	-0.15	-0.37**	0.24	0.00	-0.27**	-0.07	-0.05
	(0.12)	(0.11)	(0.17)	(0.11)	(0.17)	(0.16)	(0.13)	(0.12)	(0.08)	(0.07)
medhhinc^2	0.00	0.00**	-0.00	0.00	0.00**	-0.00	-0.00	0.00**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nfprofit	-0.27	-0.20	-0.27	-0.11	-0.25	-0.71**	-0.48**	-0.12	-0.10	-0.26**
	(0.20)	(0.19)	(0.31)	(0.19)	(0.30)	(0.28)	(0.21)	(0.20)	(0.14)	(0.12)
govt	-1.30***	-0.94***	-1.79***	-1.15***	-0.03	-1.45***	-1.28***	-0.82**	-0.33	-0.50**
	(0.36)	(0.34)	(0.48)	(0.34)	(0.52)	(0.49)	(0.38)	(0.36)	(0.23)	(0.20)
teaching	0.57*	0.40	0.92*	0.55*	-0.57	0.08	0.65*	0.53*	-0.33	-0.00
	(0.31)	(0.29)	(0.51)	(0.29)	(0.47)	(0.42)	(0.34)	(0.31)	(0.22)	(0.20)
acutebeds	0.00*	0.00**	-0.00	0.00*	-0.00	0.00***	-0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	15.04	24.95***	19.58	37.95***	40.93***	-5.66	27.52**	10.84	7.52	-5.73
	(9.97)	(9.39)	(14.18)	(9.39)	(14.19)	(14.26)	(11.15)	(10.11)	(6.67)	(6.02)
# of obs	1275	1281	1209	1275	1254	1244	1262	1275	1262	1284
R^2	0.095	0.117	0.284	0.128	0.182	0.065	0.141	0.135	0.293	0.103
# of hospitals	175	175	174	175	175	175	175	175	175	175

Notes: All models also include hospital and year fixed effects. Unclustered standard errors are reported in parentheses. For the key explanatory variables, the first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 4C. Private Fee-for-Service Volume: Payment Share Interaction (Heterogeneous Response)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log of Private Fee-for-Service Counts									
log (pay)*share	**-0.95****	**-1.43****	**-0.56**	**-1.37*****	**-0.88*****	**-1.08*****	**-0.86****	**0.02**	**-0.85****	**-0.98*****
	(0.48)	**(0.69)**	**(0.36)**	**(0.38)**	**(0.21)**	**(0.35)**	**(0.34)**	**(0.38)**	**(0.33)**	**(0.33)**
	[0.75]	**[1.25]**	**[0.50]**	**[0.35]*****	**[0.47]***	**[0.37]*****	**[0.46]***	**[0.44]**	**[0.46]***	**[0.35]*****
	[0.65]	**[1.43]**	**[0.57]**	**[0.48]*****	**[0.46]***	**[0.54]****	**[0.48]***	**[0.49]**	**[0.48]***	**[0.37]*****
log (payment)	**-0.84*****	**-0.49**	**0.19**	**-0.21**	**0.32**	**0.06**	**-0.31**	**-0.71*****	**-0.22**	**-0.26**
	(0.26)	**(0.30)**	**(0.22)**	**(0.24)**	**(0.25)**	**(0.25)**	**(0.23)**	**(0.23)**	**(0.19)**	**(0.19)**
	[0.49]*	**[0.56]**	**[0.27]**	**[0.28]**	**[0.46]**	**[0.29]**	**[0.40]**	**[0.41]***	**[0.39]**	**[0.33]**
	[0.42]**	**[0.65]**	**[0.37]**	**[0.37]**	**[0.41]**	**[0.34]**	**[0.38]**	**[0.42]***	**[0.32]**	**[0.29]**
log (coins)	0.83***	0.67***	-0.16	0.37*	-0.37	0.19	0.40*	0.47**	0.36**	0.37**
	(0.23)	(0.23)	(0.19)	(0.21)	(0.23)	(0.23)	(0.21)	(0.21)	(0.16)	(0.16)
log (private_pay)	0.12	-0.29	0.13	0.15	0.00	-0.09*	-0.15	0.30*	-0.03	0.05
	(0.15)	(0.25)	(0.14)	(0.17)	(0.07)	(0.05)	(0.13)	(0.17)	(0.10)	(0.08)
num_asc	-0.02	-0.00	-0.05***	-0.02	0.02	0.04**	-0.04**	-0.02	-0.00	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
totpop	0.00***	0.00	0.00*	0.00**	0.00	0.00	0.00	0.00***	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
prop_black	-0.15**	-0.24***	-0.04	-0.15***	-0.09	0.07	-0.11*	-0.22***	-0.06	-0.05
	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)	(0.04)
prop_hisp	0.02	0.05	-0.08*	0.03	-0.01	-0.04	0.05	0.04	0.03	-0.04
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
prop_female	-0.04	-0.03	-0.09	-0.12	-0.24	0.09	-0.11	0.21	0.05	0.23
	(0.21)	(0.21)	(0.17)	(0.19)	(0.21)	(0.21)	(0.21)	(0.19)	(0.15)	(0.15)
prop_age65	0.02	-0.01	0.06	0.06*	-0.08**	0.06	0.11***	0.02	0.02	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
unemprate	-0.10	0.04	0.10	0.15	-0.44**	-0.01	0.26	0.09	-0.06	0.02
	(0.18)	(0.18)	(0.15)	(0.16)	(0.18)	(0.17)	(0.16)	(0.17)	(0.13)	(0.13)
unemprate^2	0.01	0.01	-0.01	-0.00	0.04**	0.01	-0.02	-0.00	0.01	-0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
medhhinc	0.09	0.00	0.00	0.05	-0.18	0.33***	0.14	-0.13	0.01	0.05
	(0.13)	(0.13)	(0.11)	(0.12)	(0.13)	(0.12)	(0.12)	(0.12)	(0.10)	(0.09)
medhhinc^2	-0.00	-0.00	-0.00	-0.00	0.00	-0.00**	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
nfprofit	-0.37*	-0.22	0.02	-0.22	-0.02	-0.28	-0.32	-0.23	-0.09	-0.24
	(0.22)	(0.22)	(0.20)	(0.20)	(0.23)	(0.21)	(0.20)	(0.20)	(0.16)	(0.16)
govt	-0.79**	0.12	-0.15	-0.87**	0.59	-0.38	-0.26	-0.33	-0.11	-0.46*
	(0.40)	(0.41)	(0.31)	(0.37)	(0.40)	(0.38)	(0.37)	(0.37)	(0.27)	(0.27)
teaching	-0.07	-0.39	-0.22	-0.33	-0.59	-0.50	-0.33	-0.25	-0.46*	-0.76***
	(0.34)	(0.35)	(0.33)	(0.31)	(0.36)	(0.32)	(0.33)	(0.32)	(0.26)	(0.26)
acutebeds	-0.00	-0.00	-0.00**	-0.00	-0.00**	-0.00	-0.00***	-0.00**	-0.00***	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	4.74	9.99	6.46	7.91	22.95**	-13.64	4.94	-5.20	1.64	-8.93
	(11.04)	(11.35)	(9.26)	(10.13)	(10.96)	(10.92)	(10.76)	(10.43)	(8.09)	(7.91)
# of obs	1275	1281	1209	1275	1254	1244	1262	1275	1262	1284
R^2	0.349	0.278	0.290	0.295	0.117	0.168	0.266	0.237	0.451	0.349
# of hospitals	175	175	174	175	175	175	175	175	175	175

Notes: All models also include hospital and year fixed effects. Unclustered standard errors are reported in parentheses. For the key explanatory variables, the first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 5A. Robustness Check 1: Controlling for 3-digit zipcode-year dummies, Payment-level Regressions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: log of Combined Counts (Medicare + Private Fee-for-Service)										
log (payment)	**-0.07**	**0.22**	**0.23**	**0.20**	**0.61***	**0.30**	**-0.02**	**0.28**	**0.27***	**0.09**
	(0.21)	**(0.20)**	**(0.33)**	**(0.20)**	**(0.32)**	**(0.28)**	**(0.23)**	**(0.22)**	**(0.15)**	**(0.14)**
	[0.23]	**[0.24]**	**[0.61]**	**[0.22]**	**[0.71]**	**[0.48]**	**[0.35]**	**[0.24]**	**[0.26]**	**[0.22]**
	[0.29]	**[0.27]**	**[0.68]**	**[0.28]**	**[0.59]**	**[0.38]**	**[0.34]**	**[0.32]**	**[0.23]**	**[0.21]**
# of obs	1376	1383	1305	1377	1351	1336	1361	1377	1363	1386
R^2	0.274	0.321	0.394	0.269	0.304	0.324	0.306	0.278	0.502	0.309
# of hospitals	198	199	196	199	198	198	197	199	198	199
Panel B: log of Medicare Counts										
log (payment)	**0.03**	**0.24**	**0.18**	**0.28**	**0.56***	**0.38**	**0.09**	**0.40***	**0.51*****	**0.29****
	(0.22)	**(0.20)**	**(0.34)**	**(0.20)**	**(0.32)**	**(0.28)**	**(0.23)**	**(0.22)**	**(0.15)**	**(0.14)**
	[0.24]	**[0.25]**	**[0.67]**	**[0.22]**	**[0.72]**	**[0.48]**	**[0.35]**	**[0.24]**	**[0.21]****	**[0.21]**
	[0.29]	**[0.27]**	**[0.70]**	**[0.27]**	**[0.59]**	**[0.39]**	**[0.34]**	**[0.32]**	**[0.20]****	**[0.20]**
# of obs	1376	1383	1305	1377	1351	1336	1361	1377	1363	1386
R^2	0.261	0.297	0.374	0.262	0.324	0.321	0.289	0.275	0.419	0.265
# of hospitals	198	199	196	199	198	198	197	199	198	199
Panel C: log of Private Fee-for-Service Counts										
log (payment)	**-0.95*****	**-0.51****	**0.05**	**-0.46****	**-0.01**	**-0.48****	**-0.45****	**-0.45****	**-0.34***	**-0.50*****
	(0.24)	**(0.24)**	**(0.22)**	**(0.22)**	**(0.24)**	**(0.23)**	**(0.23)**	**(0.22)**	**(0.18)**	**(0.18)**
	[0.41]**	**[0.41]**	**[0.31]**	**[0.30]**	**[0.36]**	**[0.30]**	**[0.43]**	**[0.39]**	**[0.37]**	**[0.34]**
	[0.40]**	**[0.42]**	**[0.36]**	**[0.35]**	**[0.37]**	**[0.28]***	**[0.35]**	**[0.38]**	**[0.31]**	**[0.28]***
# of obs	1376	1383	1305	1377	1351	1336	1361	1377	1363	1386
R^2	0.448	0.425	0.390	0.402	0.265	0.306	0.378	0.376	0.542	0.453
# of hospitals	198	199	196	199	198	198	197	199	198	199

Notes: Unclustered standard errors are reported in parentheses. The first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. All models control for the additional variables listed in Table 2A plus hospital and year fixed effects. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 5B. Robustness Check 1: Controlling for 3-digit zipcode-year dummies, Payment Share Interaction Regressions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: log of Combined Counts (Medicare + Private Fee-for-Service)										
log (pay)*share	**-1.27*** **	**-0.40**	**-1.59*** **	**-1.96*** **	**-1.34*** **	**-0.60**	**-0.69***	**-0.67***	**-0.81*** **	**-1.34*** **
	(0.42)	**(0.56)**	**(0.61)**	**(0.35)**	**(0.30)**	**(0.48)**	**(0.37)**	**(0.39)**	**(0.30)**	**(0.25)**
	[1.21]	**[1.16]**	**[1.25]**	**[0.57]*** **	**[0.84]**	**[0.70]**	**[0.71]**	**[0.83]**	**[0.35]** **	**[0.29]*** **
	[1.07]	**[1.13]**	**[1.27]**	**[0.54]*** **	**[0.70]***	**[0.69]**	**[0.63]**	**[0.79]**	**[0.35]** **	**[0.26]*** **
log (payment)	0.31	0.29	0.64*	0.85***	1.19***	0.50	0.26	0.45*	0.44***	0.40***
	(0.23)	(0.25)	(0.38)	(0.23)	(0.35)	(0.32)	(0.27)	(0.25)	(0.17)	(0.15)
# of obs	1275	1281	1209	1275	1254	1244	1262	1275	1262	1284
R^2	0.323	0.360	0.410	0.323	0.320	0.344	0.326	0.317	0.527	0.337
# of hospitals	175	175	174	175	175	175	175	175	175	175
Panel B: log of Medicare Counts										
log (pay)*share	**-1.67*** **	**-0.88**	**-2.04*** **	**-2.26*** **	**-1.24*** **	**-0.38**	**-0.84** **	**-1.09*** **	**-1.00*** **	**-1.49*** **
	(0.44)	**(0.58)**	**(0.62)**	**(0.36)**	**(0.30)**	**(0.48)**	**(0.37)**	**(0.39)**	**(0.30)**	**(0.25)**
	[1.43]	**[1.48]**	**[1.37]**	**[0.66]*** **	**[0.90]**	**[0.82]**	**[0.87]**	**[0.93]**	**[0.39]** **	**[0.28]*** **
	[1.31]	**[1.28]**	**[1.34]**	**[0.61]*** **	**[0.76]**	**[0.70]**	**[0.77]**	**[0.88]**	**[0.39]** **	**[0.25]*** **
log (payment)	0.51**	0.48*	0.72*	0.99***	1.12***	0.51	0.42	0.69***	0.74***	0.63***
	(0.25)	(0.26)	(0.38)	(0.23)	(0.35)	(0.32)	(0.27)	(0.25)	(0.17)	(0.15)
# of obs	1275	1281	1209	1275	1254	1244	1262	1275	1262	1284
R^2	0.310	0.340	0.394	0.324	0.337	0.339	0.312	0.318	0.444	0.293
# of hospitals	175	175	174	175	175	175	175	175	175	175
Panel C: log of Private Fee-for-Service Counts										
log (pay)*share	**-0.73**	**-0.88**	**-0.62**	**-1.18*** **	**-1.01*** **	**-1.29*** **	**-0.54**	**0.28**	**-0.57**	**-1.03*** **
	(0.51)	**(0.71)**	**(0.40)**	**(0.40)**	**(0.23)**	**(0.40)**	**(0.36)**	**(0.41)**	**(0.36)**	**(0.33)**
	[0.91]	**[1.47]**	**[0.55]**	**[0.34]*** **	**[0.48]** **	**[0.43]*** **	**[0.46]**	**[0.48]**	**[0.46]**	**[0.31]*** **
	[0.69]	**[1.53]**	**[0.57]**	**[0.43]*** **	**[0.51]** **	**[0.63]** **	**[0.43]**	**[0.52]**	**[0.44]**	**[0.29]*** **
log (payment)	-0.73***	-0.33	0.24	-0.07	0.44	-0.08	-0.25	-0.57**	-0.25	-0.25
	(0.28)	(0.32)	(0.25)	(0.26)	(0.27)	(0.26)	(0.26)	(0.26)	(0.20)	(0.20)
# of obs	1275	1281	1209	1275	1254	1244	1262	1275	1262	1284
R^2	0.470	0.441	0.399	0.423	0.283	0.324	0.390	0.387	0.558	0.478
# of hospitals	175	175	174	175	175	175	175	175	175	175

Notes: Unclustered standard errors are reported in parentheses. The first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. All models control for the additional variables listed in Table 2A plus hospital and year fixed effects. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 6A. Additional Robustness Checks on the Combined Counts Regressions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Baseline (from Table 4A)										
*log (pay)*share*	**-1.46*** **	**-0.70**	**-2.08*** **	**-1.98*** **	**-1.03*** **	**0.39**	**-1.01*** **	**-0.82** **	**-0.80*** **	**-1.33*** **
log (payment)	0.56**	0.47*	0.74**	0.90***	0.97***	0.51	0.53**	0.55**	0.48***	0.52***
Robustness Check 2: Controlling for Medicare Managed Care Penetration and # of MDs										
*log (pay)*share*	-0.99**	-0.21	-2.20***	-1.45***	-0.72**	-0.05	-0.82**	-0.72*	-0.34	-1.07***
log (payment)	0.40	0.42	0.60	0.71***	0.76**	0.16	0.47	0.60**	0.46**	0.50***
Robustness Check 3: Excluding Hospital-level Controls										
*log (pay)*share*	**-1.46*** **	**-0.82**	**-2.09*** **	**-2.03*** **	**-1.04*** **	**0.32**	**-1.01*** **	**-0.89** **	**-0.80*** **	**-1.33*** **
log (payment)	0.42*	0.42*	0.56*	0.78***	1.00***	0.37	0.41*	0.49**	0.46***	0.45***
Robustness Check 4: Excluding Small Rural Hospitals and Sole Community Hospitals										
*log (pay)*share*	**-1.48*** **	**-0.68**	**-1.91*** **	**-2.03*** **	**-1.04*** **	**0.27**	**-0.99*** **	**-0.90** **	**-0.71** **	**-1.36*** **
log (payment)	0.59***	0.48**	0.71**	0.93***	1.02***	0.50	0.51**	0.60***	0.51***	0.52***
Robustness Check 5: Including the Transitional Corridor Payment Years										
*log (pay)*share*	**-1.14*** **	**-0.67**	**-1.81*** **	**-1.83*** **	**-0.93*** **	**0.33**	**-0.86*** **	**-0.69** **	**-0.71*** **	**-1.19*** **
log (payment)	0.46**	0.47**	0.46	0.79***	0.93***	0.12	0.57***	0.46**	0.39***	0.45***
Robustness Check 6: Excluding all Controls Except for Hospital and Year Indicator Variables										
*log (pay)*share*	**-0.95** **	**-0.97** *	**-2.01*** **	**-1.52*** **	**-0.94*** **	**0.74** *	**-0.59**	**-0.34**	**-0.46** *	**-1.16*** **
log (payment)	-0.13	-0.16	0.22	0.13	0.96***	0.08	-0.10	-0.32	0.06	0.26*
Robustness Check 7: Excluding 1997 data										
*log (pay)*share*	**-1.21*** **	**-0.70**	**-1.62*** **	**-1.77*** **	**-1.02*** **	**0.47**	**-0.68** *	**-0.47**	**-0.53** *	**-1.07*** **
log (payment)	0.43*	0.49*	0.71**	0.83***	1.27***	0.44	0.38	0.44*	0.54***	0.55***

Notes: All models control for the additional variables listed in Table 2A plus hospital and year fixed effects except where indicated by the robustness check description. Statistical significance is based on unclustered standard errors and is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 6B. Additional Robustness Checks on the Medicare Counts Regressions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Baseline (from Table 4B)										
*log (pay)*share*	**-1.77*** **	**-0.94**	**-2.55*** **	**-2.18*** **	**-0.94*** **	**0.65**	**-1.11*** **	**-1.12*** **	**-0.91*** **	**-1.50*** **
log (payment)	0.80***	0.68***	0.89***	1.09***	0.93***	0.49	0.72***	0.79***	0.80***	0.79***
Robustness Check 2: Controlling for Medicare Managed Care Penetration and # of MDs										
*log (pay)*share*	**-1.27** **	**-0.24**	**-2.67*** **	**-1.68*** **	**-0.64** **	**0.20**	**-0.93** **	**-1.04** **	**-0.40**	**-1.16*** **
log (payment)	0.59**	0.51*	0.75*	0.87***	0.68*	0.10	0.64**	0.78***	0.75***	0.69***
Robustness Check 3: Excluding Hospital-level Controls										
*log (pay)*share*	**-1.76*** **	**-1.09** *	**-2.57*** **	**-2.23*** **	**-0.94*** **	**0.57**	**-1.11*** **	**-1.19*** **	**-0.90*** **	**-1.50*** **
log (payment)	0.65***	0.61**	0.70**	0.98***	0.95***	0.33	0.58**	0.74***	0.76***	0.72***
Robustness Check 4: Excluding Small Rural Hospitals and Sole Community Hospitals										
*log (pay)*share*	**-1.81*** **	**-0.91**	**-2.32*** **	**-2.25*** **	**-0.95*** **	**0.53**	**-1.10*** **	**-1.21*** **	**-0.80*** **	**-1.50*** **
log (payment)	0.84***	0.69***	0.83**	1.13***	0.99***	0.47	0.71***	0.85***	0.84***	0.79***
Robustness Check 5: Including the Transitional Corridor Payment Years										
*log (pay)*share*	**-1.27*** **	**-0.84**	**-2.08*** **	**-1.87*** **	**-0.76*** **	**0.65**	**-0.91*** **	**-0.80** **	**-0.63*** **	**-1.17*** **
log (payment)	0.64***	0.66***	0.54*	0.94***	0.88***	0.06	0.73***	0.63***	0.62***	0.64***
Robustness Check 6: Excluding all Controls Except for Hospital and Year Indicator Variables										
*log (pay)*share*	**-1.25*** **	**-1.26** **	**-2.44*** **	**-1.71*** **	**-0.91*** **	**0.93** **	**-0.70** *	**-0.68** *	**-0.60** **	**-1.38*** **
log (payment)	0.10	0.08	0.36	0.35*	0.97***	0.06	0.07	-0.05	0.37***	0.53***
Robustness Check 7: Excluding 1997 data										
*log (pay)*share*	**-1.86*** **	**-1.04** *	**-2.34*** **	**-2.22*** **	**-0.91*** **	**0.66**	**-1.06*** **	**-1.03** **	**-0.86*** **	**-1.42*** **
log (payment)	0.75***	0.66**	0.90**	1.12***	1.21***	0.45	0.63**	0.75***	0.90***	0.86***

Notes: All models control for the additional variables listed in Table 2A plus hospital and year fixed effects except where indicated by the robustness check description. Statistical significance is based on unclustered standard errors and is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Table 6C. Additional Robustness Checks on the Private Fee-for-Service Counts Regressions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Baseline (from Table 4C)										
*log (pay)*share*	**-1.77*** **	**-0.94**	**-2.55*** **	**-2.18*** **	**-0.94*** **	**0.65**	**-1.11*** **	**-1.12*** **	**-0.91*** **	**-1.50*** **
log (payment)	-0.84***	-0.49	0.19	-0.21	0.32	0.06	-0.31	-0.71***	-0.22	-0.26
Robustness Check 2: Controlling for Medicare Managed Care Penetration and # of MDs										
*log (pay)*share*	**-0.41**	**-0.84**	**-0.83***	**-0.90** **	**-0.67*** **	**-1.27*** **	**-0.60**	**0.02**	**-0.40**	**-0.73***
log (payment)	-0.82***	-0.43	0.32	-0.27	0.20	0.09	-0.45	-0.50*	-0.26	-0.24
Robustness Check 3: Excluding Hospital-level Controls										
*log (pay)*share*	**-0.96** **	**-1.39** **	**-0.60***	**-1.40*** **	**-0.88*** **	**-1.02*** **	**-0.80** **	**0.02**	**-0.85** **	**-1.00*** **
log (payment)	-0.91***	-0.48	0.21	-0.28	0.41*	0.01	-0.33	-0.72***	-0.21	-0.30
Robustness Check 4: Excluding Small Rural Hospitals and Sole Community Hospitals										
*log (pay)*share*	**-0.85***	**-1.35***	**-0.59**	**-1.32*** **	**-0.86*** **	**-1.10*** **	**-0.84** **	**-0.04**	**-0.84** **	**-1.01*** **
log (payment)	-0.81***	-0.48	0.26	-0.20	0.33	0.06	-0.32	-0.66***	-0.18	-0.26
Robustness Check 5: Including the Transitional Corridor Payment Years										
*log (pay)*share*	**-0.90** **	**-1.41** **	**-0.69** **	**-1.48*** **	**-0.89*** **	**-0.82*** **	**-0.83*** **	**-0.19**	**-0.84*** **	**-1.04*** **
log (payment)	-0.68***	-0.36	0.12	-0.18	0.32	-0.32	-0.28	-0.70***	-0.27	-0.25
Robustness Check 6: Excluding all Controls Except for Hospital and Year Indicator Variables										
*log (pay)*share*	**-0.74**	**-1.69** **	**-0.44**	**-1.11*** **	**-0.87*** **	**-0.68** **	**-0.57***	**0.37**	**-0.67** **	**-0.75** **
log (payment)	-1.30***	-0.89***	-0.04	-0.72***	0.26	-0.28	-0.65***	-1.25***	-0.53***	-0.49***
Robustness Check 7: Excluding 1997 data										
*log (pay)*share*	**-0.47**	**-1.25***	**0.10**	**-1.02** **	**-0.97*** **	**-0.81** **	**-0.29**	**0.65**	**-0.54**	**-0.54**
log (payment)	-0.94***	-0.38	0.38	-0.26	0.56*	0.03	-0.37	-0.75***	-0.18	-0.12

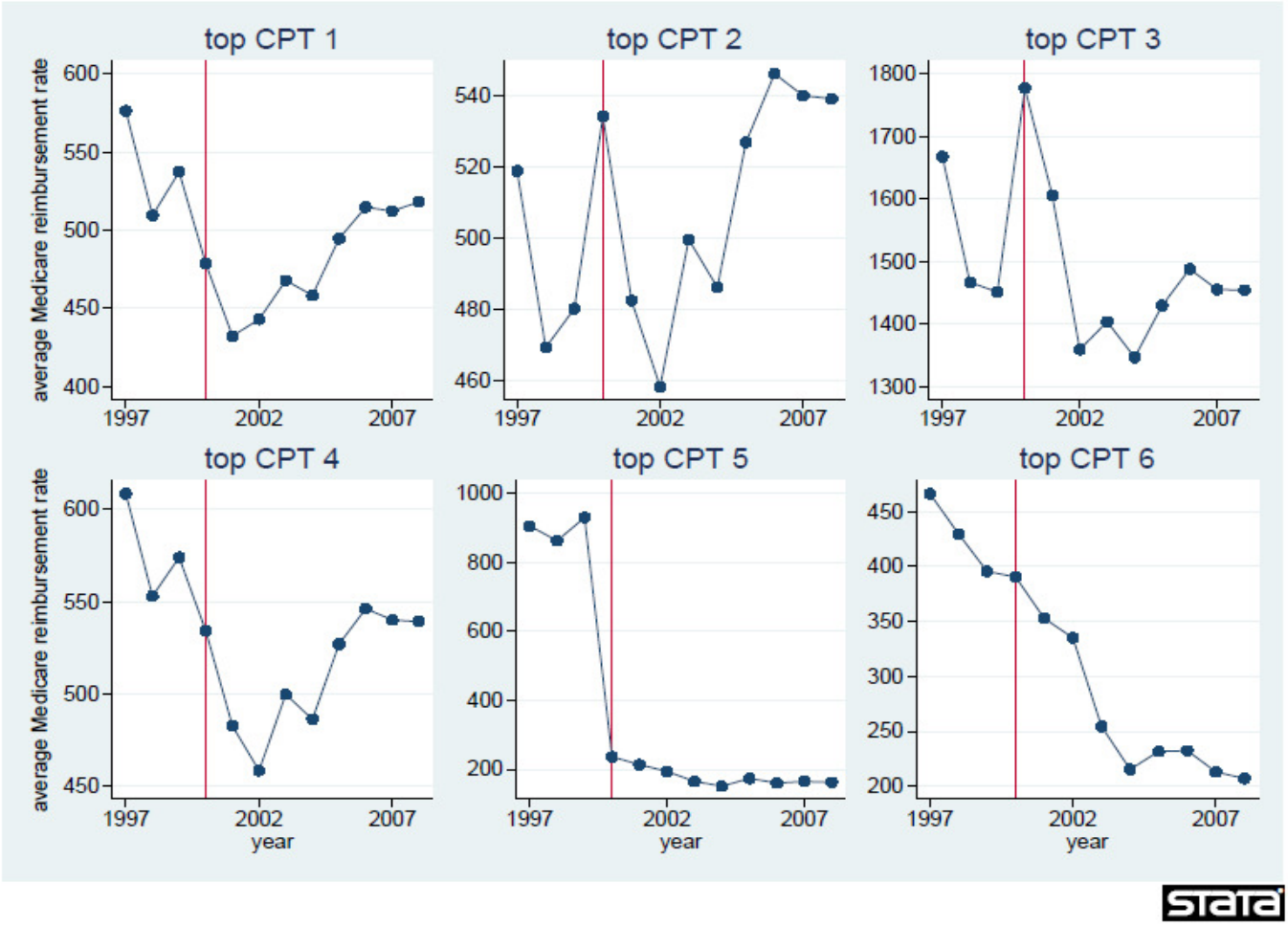
Notes: All models control for the additional variables listed in Table 2A plus hospital and year fixed effects except where indicated by the robustness check description. Statistical significance is based on unclustered standard errors and is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

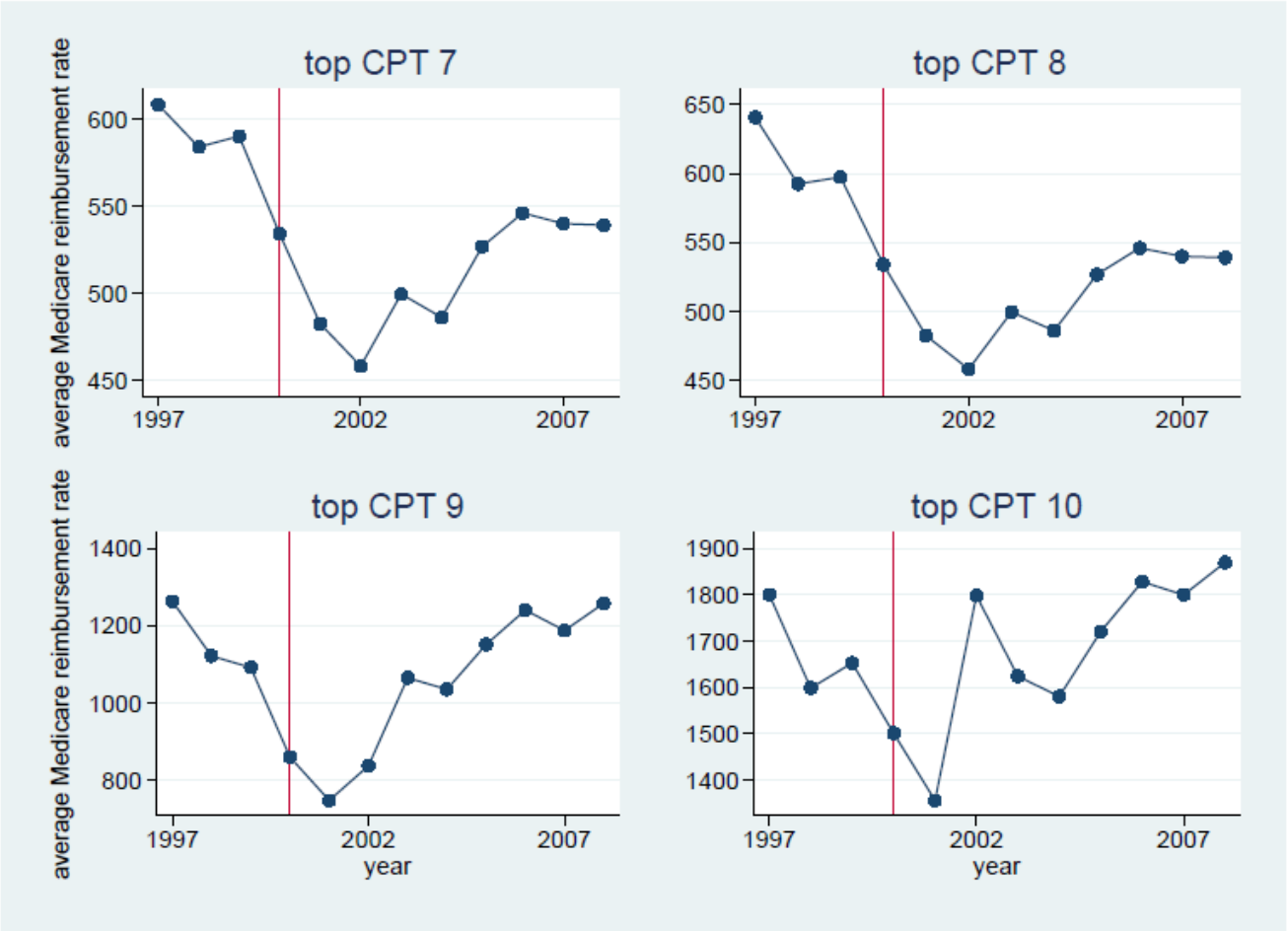
Table 7. Payment Interacted with Integration Indicator

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: log of Combined Counts (Medicare + Private Fee-for-Service)										
log (pay)*integrated	**-0.43*** **	**-0.69*** **	**-0.80*** **	**-0.52*** **	**-0.13**	**0.08**	**-0.39*** **	**-0.71*** **	**0.22***	**0.00**
	(0.15)	**(0.19)**	**(0.30)**	**(0.15)**	**(0.10)**	**(0.17)**	**(0.14)**	**(0.15)**	**(0.12)**	**(0.17)**
	[0.22]*	**[0.35]***	**[0.67]**	**[0.19]*** **	**[0.19]**	**[0.22]**	**[0.24]**	**[0.21]*** **	**[0.19]**	**[0.24]**
	[0.30]	**[0.43]**	**[0.67]**	**[0.33]**	**[0.20]**	**[0.27]**	**[0.26]**	**[0.26]*** **	**[0.21]**	**[0.29]**
log (payment)	0.44**	0.68***	-0.06	0.56***	0.80**	0.82**	0.33	0.88***	0.33**	0.31**
	(0.21)	(0.21)	(0.32)	(0.20)	(0.34)	(0.32)	(0.25)	(0.22)	(0.16)	(0.14)
# of obs	1078	1085	1018	1079	1067	1049	1065	1079	1068	1088
R^2	0.114	0.156	0.354	0.141	0.158	0.070	0.160	0.192	0.391	0.133
# of hospitals	145	146	144	146	146	146	145	146	145	146
Panel B: log of Medicare Counts										
log (pay)*integrated	**-0.56*** **	**-0.78*** **	**-0.81*** **	**-0.63*** **	**-0.12**	**0.05**	**-0.47*** **	**-0.83*** **	**0.10**	**-0.08**
	(0.16)	**(0.20)**	**(0.31)**	**(0.16)**	**(0.10)**	**(0.17)**	**(0.14)**	**(0.15)**	**(0.11)**	**(0.17)**
	[0.25] **	**[0.32]** **	**[0.78]**	**[0.21]*** **	**[0.19]**	**[0.23]**	**[0.27]***	**[0.22]*** **	**[0.16]**	**[0.24]**
	[0.35]	**[0.47]***	**[0.77]**	**[0.37]***	**[0.20]**	**[0.26]**	**[0.28]**	**[0.27]*** **	**[0.18]**	**[0.27]**
log (payment)	0.58***	0.77***	-0.01	0.72***	0.75**	0.90***	0.48*	1.05***	0.51***	0.47***
	(0.22)	(0.21)	(0.33)	(0.21)	(0.34)	(0.33)	(0.25)	(0.22)	(0.16)	(0.14)
# of obs	1078	1085	1018	1079	1067	1049	1065	1079	1068	1088
R^2	0.105	0.154	0.320	0.147	0.179	0.074	0.151	0.195	0.292	0.072
# of hospitals	145	146	144	146	146	146	145	146	145	146
Panel C: log of Private Fee-for-Service Counts										
log (pay)*integrated	**-0.44** **	**-0.79*** **	**-0.28**	**-0.37** **	**-0.07**	**0.07**	**-0.14**	**-0.28***	**0.07**	**-0.31**
	(0.18)	**(0.26)**	**(0.21)**	**(0.18)**	**(0.08)**	**(0.13)**	**(0.14)**	**(0.16)**	**(0.14)**	**(0.22)**
	[0.24]*	**[0.36]** **	**[0.40]**	**[0.18]** **	**[0.13]**	**[0.15]**	**[0.20]**	**[0.23]**	**[0.26]**	**[0.37]**
	[0.22] **	**[0.39]** **	**[0.40]**	**[0.22]***	**[0.13]**	**[0.19]**	**[0.19]**	**[0.18]**	**[0.28]**	**[0.42]**
log (payment)	-0.75***	-0.31	0.03	-0.30	0.13	-0.23	-0.37	-0.26	-0.25	-0.27
	(0.26)	(0.27)	(0.22)	(0.23)	(0.26)	(0.25)	(0.24)	(0.24)	(0.19)	(0.19)
# of obs	1078	1085	1018	1079	1067	1049	1065	1079	1068	1088
R^2	0.340	0.269	0.320	0.284	0.098	0.162	0.245	0.233	0.445	0.327
# of hospitals	145	146	144	146	146	146	145	146	145	146

Notes: Unclustered standard errors are reported in parentheses. The first row of brackets reports robust standard errors clustered by county and the second row of brackets reports robust standard errors clustered by hospital. All models control for the additional variables listed in Table 2A plus hospital and year fixed effects. Statistical significance is indicated by *** for .01 level, ** for .05 level, and * for .10 level.

Figure 1. Time Trend in Average Medicare Payment Rate for the Top 10 Surgical Procedures





Notes: The average Medicare payment rate is reported in 2008 dollars. The red vertical line indicates the year of 2000.