HOW DO HOSPITALS RESPOND TO PRICE CHANGES?

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November 2002

Abstract

This paper investigates whether hospitals respond in profit-maximizing ways to changes in diagnosis-specific prices, as determined by Medicare's Prospective Payment System and other cost-conscious insurers. Previous studies have been unable to isolate this response because changes in reimbursement amounts (prices) are typically endogenous: they are adjusted to reflect changes in hospital costs. I exploit an exogenous 1988 policy change that generated a relative price increase of 7 percent (around $300) for 43 percent of all Medicare admissions. Using the unaffected admissions as a control group, I find that hospitals did not increase the intensity of care provided to affected admissions, where intensity is measured by total costs, length of stay, number of surgical procedures, number of intensive-care-unit days, and in-hospital death rate. Neither did hospitals increase the volume of patients admitted to more remunerative diagnoses, notwithstanding the strong a priori expectation that such a response should prevail in fixed-price settings. However, hospitals did exhibit a strong *nominal* response to the policy change, "upcoding" patients to diagnosis codes associated with large reimbursement increases, and earning $300-$410 million in extra reimbursement annually. This response was particularly strong among for-profit hospitals. Taken together, these findings suggest that hospitals do not alter their treatment or admissions policies based on diagnosis-specific prices; however, they employ sophisticated coding strategies in order to maximize total reimbursement. The results also suggest that models of quality competition among hospitals may be inappropriate at the level of specific diagnoses ("products").

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e-mail: l-dafny@kellogg.northwestern.edu. I am grateful to Jonathan Gruber, James Poterba, David Cutler, and Glenn Ellison for excellent guidance. I thank Josh Angrist, Mark Duggan, Julian Jamison, David Levine, Joseph Newhouse, Scott Stern, Nancy Rose, and seminar participants at several universities for helpful comments. Support from the National Science Foundation, the National Bureau of Economic Research, and the National Institute on Aging is gratefully acknowledged. The MEDPAR data are confidential and cannot be released.

1 Introduction

The vast majority of U.S. healthcare is privately provided. Yet until the 1980s, the sector was largely immune from standard market forces promoting efficiency in production. The canonical healthcare market imperfections – informational asymmetries between providers and consumers, and an insurance-induced wedge between marginal out-of-pocket costs and patient benefits – were exacerbated by a cost-plus reimbursement system and primarily not-for-profit providers. So long as providers could always earn non-negative profits, there was little supply-side incentive to cut costs, and consumers' incentives via co-payments and deductibles were weak. In 1984, the federal government injected market discipline into the system by establishing fixed prices for Medicare hospitalizations. Other public and private insurers soon followed suit, wresting price-setting control from providers and imposing yardstick competition.

A large literature documents hospitals' responses to the introduction of fixed prices, but few studies have explored reactions to *changes* in these prices. Yet once the transition to a fixed-price regime is completed, price levels constitute the sole lever in the system, and there remain several unanswered empirical questions regarding their effect. In the face of a price increase for a particular diagnosis, will hospitals find ways to admit more such patients? Will they compete more vigorously for these patients by improving the quality of their care, thereby dissipating part of the rents from the price increase? The answers to these questions are not only critical to ongoing policy decisions, but can also provide valuable insights into hospital industry conduct and the effectiveness of fixed-price regulation at large.

This study focuses on inpatient care for Medicare beneficiaries, who account for 37% of hospital discharges and 31% of total revenues.[1] Since 1984, hospital reimbursement for Medicare patients has been governed by the Prospective Payment System, which provides a fixed payment for

[1] 1999 figures from the Centers for Medicare & Medicaid Services, *Health Care Financing Review,* summer 2001, and author's tabulations from the 1999 Survey of Hospitals (administered by the American Hospital Association).

each Medicare patient in a given hospital and diagnosis-related group (DRG). Standard models of hospital behavior, reviewed in section 2, predict that hospitals will respond to a diagnosis-specific price increase by raising the intensity of care provided to patients in that diagnosis. According to these models, hospitals behave much like multiproduct firms, where the products are DRGs, and the choice variables are not prices but intensity of care within each DRG. Both patient volume and hospital costs are assumed to increase in the intensity of care provided. A price increase for a given DRG raises the profitability of that DRG, creating an incentive to attract more patients by increasing the intensity of care that is provided. The few studies that investigate the effect of DRG-level price changes on intensity levels all find a positive relationship, where intensity of care is measured by length of stay, number of surgical procedures, and/or death rates. Thus, all evidence to date suggests that a "flypaper effect" operates in the hospital industry: wherever new monies are assigned, there the monies are stuck (or rather, spent).

All of the aforementioned studies utilize data from a transition to a prospective payment system, either PPS or one of the many systems implemented by state Medicaid programs. These studies therefore face the formidable challenge of separating two simultaneous changes in incentives: the elimination of marginal reimbursement, and changes in the average *level* of payments for each DRG. By investigating responses to average payment levels (i.e. prices) in the post-implementation period, I circumvent both this challenge and the concern that transitory responses are driving previous results. Estimating responses to price changes in post-implementation eras is difficult, however, because price changes are typically endogenous: they are adjusted to reflect changes in hospital costs. Thus, positive associations between changes in price and changes in spending or intensity likely reflect bilateral causality, and do not constitute a priori evidence that hospitals alter treatment patterns in response to price changes.

To obtain unbiased estimates of hospital responses to price changes, this study exploits an exogenous 1988 policy change that generated a relative price increase of 7 percent (around $300) for

43 percent of Medicare admissions.[2] The policy change was simply the elimination of "age over 69" and "age under 70" in the descriptions for the diagnosis-related groups (DRGs) to which patients may be assigned. Qualifiers that formerly read "with complications or age over 69" and "without complications and age under 69" now read "with complications" or "without complications." This seemingly innocuous change, which is described in greater detail in Section 3, actually led to large increases in reimbursement for patients assigned to DRG codes with these qualifiers ("affected DRGs"), as compared to patients in other codes ("unaffected DRGs").

Using the unaffected admissions as a control group, I find that hospitals did not increase the intensity of care provided to affected admissions, where intensity is measured by total costs, length of stay, number of surgical procedures, number of intensive-care-unit (ICU) days, and in-hospital death rate. Neither did hospitals increase the volume of patients admitted to more remunerative diagnoses, a finding that is theoretically consistent with the intensity non-response, but perhaps surprising given theoretical predictions of firm behavior in fixed-price settings. I do find evidence that hospitals spent the extra funds they earned on patient care, but these funds were spread across *all* admissions. Correspondingly, overall hospital volume growth was also stronger for hospitals with larger price gains (and therefore intensity increases) arising from the policy change.

Real responses to price changes constitute one important class of possible reactions; nominal responses are another. Because hospitals are responsible for coding patients to the appropriate DRGs, raising prices for certain DRGs may simply entice hospitals to "upcode," or switch patients from lower-paying DRGs into higher-paying DRGs. Upcoding does not affect real elements of patient care, but it inflates hospital reimbursements. This was the primary response of hospitals to the 1988 policy change. Furthermore, although the policy shock created a blanket incentive to increase upcoding in dozens of diagnoses, hospitals were savvy in their upcoding practices, upcoding more in those

[2] Dollar figures are for 2001, unless otherwise indicated.

diagnoses where the incentive to do so was larger. In addition, the upcoding response was strongest among for-profit hospitals, a finding that is consistent with prior research.

Taken together, these findings indicate that hospitals do not alter their treatment or admissions policies based on diagnosis-specific prices; however, they employ sophisticated coding strategies in order to maximize total reimbursement. The results also suggest that healthcare insurers cannot effect an increase in the quality of care provided to patients with a particular diagnosis simply by increasing their reimbursement rates for that diagnosis. Another important implication is that models of quality competition among hospitals may be inappropriate at the level of specific diagnoses. Finally, this research illustrates the difficulties inherent in regulating prices in an industry where the products are hard to define.

The remainder of the paper is organized into 5 sections. Section 2 describes PPS and prior related research, and introduces a hospital objective function that provides a theoretical framework for the empirical sections that follow. Section 3 gives a detailed explanation of the 1988 policy change. The data are described in Section 4, followed by an evaluation of the impact of the policy change on price levels in Section 5. Section 6 explores the intensity and volume responses to the price changes identified in Section 5, and Section 7 investigates the role of upcoding in generating these price changes. Section 8 concludes.

2 Background

2.1 A PPS Primer

The Prospective Payment System (PPS) for hospitalizations of Medicare beneficiaries was implemented in October 1984 by the Health Care Financing Administration (HCFA), now known as the Centers for Medicare and Medicaid Services (CMS). The defining element of the system is a reimbursement amount that is fixed regardless of a hospital's actual expenditures on a patient. This payment does vary, however, by the patient's medical diagnosis. Diagnoses are grouped into approximately 500 Diagnosis-Related Groups (DRGs). Each DRG is assigned a weight (called a "DRG weight") that reflects the relative resource intensity of admissions within that group. Reimbursement to hospital h for an admission in DRG d is given by

$$P_{hd} = P_h \cdot (1 + IME_h) \cdot (1 + DSH_h) \cdot \text{DRG weight}_d$$

where P_h is a hospital-specific amount (inflated annually by a Congressionally-approved "update factor"), IME represents an adjustment for indirect medical education (teaching), and DSH adjusts payment levels to compensate hospitals with a disproportionate share of indigent patients.[3] Most of the variation in P_{dh} is due to the DRG weights, which range between .09 (DRG 448 for allergic reactions) to 22.8 (DRG 480 for liver transplants).[4] HCFA uses hospital charge data to recalibrate the weights annually, raising weights for DRGs that experience relative increases in average charges, and reducing weights for DRGs with relative decreases in average charges. The average DRG weight per hospital admission has risen substantially over time, from 1.13 in 1984 to 1.36 in 1996.[5] This phenomenon is known as "DRG creep," the tendency over time to code more patients into DRGs with higher weights. A 1 percent increase in the average case weight is associated with an additional $930 million in *annual* Medicare payments to hospitals.[6]

Although the implementation of PPS eliminated *marginal* reimbursement for services rendered (within a given DRG, hospitals are not compensated more when they spend more on a patient), economists have noted that *average* payment incentives remain. If P_{hd} is low relative to actual costs in DRG d, hospitals have an incentive to reduce the intensity of care and the number of admissions in that DRG. Section 2.2 illustrates this incentive more formally.

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[3] This simplified formula appears in Cutler (1995).
[4] The range for DRG weights is given for 1985-1996.
[5] Steinwald and Dummit (1989), author's calculations. The original 1984 weights were constructed so that the average DRG weight for hospitals, called the *case-mix index*, would equal 1.
[6] Centers for Medicare and Medicaid Services, *Program Information*, June 2002.

Few studies have investigated hospital responses to average payment incentives. These papers, all of which employ data from transitions to prospective payment systems, are reviewed in section 2.3. Examining reactions to average payment incentives during post-implementation periods is difficult because most weight changes are due to the regular recalibrations described above. When costs increase, DRG weights increase. Thus, the coefficient on DRG weight in a regression of costs (or some other measure of intensity of care) on DRG weight would suffer from a strong upward bias. To obtain an unbiased estimate of this coefficient, exogenous variation in payment levels is required. This variation is provided by the natural experiment described in section 3.

2.2 Hospital Objective Functions

To illustrate how average payment incentives might affect hospital behavior, it is helpful to introduce a simple model for the hospital objective function. I begin with the traditional assumptions that hospitals attach non-negative weights to both patient care (often called "intensity") and profits, and that the objective function is separable in these arguments:

$$\max\ G_h = \alpha_h f(I_h) + (1-\alpha_h)\pi_h$$

where $0 < \alpha < 1$, h is a hospital index, I denotes intensity, and π denotes profits.

The PPS system effectively defines D "product lines" for every hospital, where D is the number of DRGs. Each hospital selects an intensity level I_{hd} for each DRG d, attracting $N_{hd}(I_{hd}, I_{\sim hd})$ patients, where ~h denotes hospital h's competitors. Patient demand is increasing in a hospital's own intensity level (at a decreasing rate), and decreasing in that of its competitors. The average severity of patients served, $S_{hd}(I_{hd})$, is also increasing in a hospital's intensity level. For each admission, the hospital earns $P_{hd} - C_{hd}(I_{hd}, S_{hd}(I_{hd}))$, where P_{hd} is as defined above, C_{hd} is the average cost per patient assigned to DRG d, and $\frac{\partial C_{hd}}{\partial I_{hd}}$ and $\frac{\partial C_{hd}}{\partial S_{hd}}$ are greater than zero.[7] Thus, the hospital's problem becomes

$$\max\ G_h = \alpha_h f(I_{h1}, I_{h2} \ldots I_{hD}) + (1-\alpha_h) \sum_{d=1}^{D} \left([P_{hd} - C_{hd}(I_{hd}, S_{hd})] N_{hd}(I_{hd}, I_{\sim hd}) \right),$$

and the first-order condition for I_{hd}, taking competitors' behavior as given, is

$$\frac{\partial G_h}{\partial I_{hd}} = \alpha_h \frac{\partial f}{\partial I_{hd}} + (1-\alpha_h) \left[(P_{hd} - C_{hd}) \frac{\partial N_{hd}}{\partial I_{hd}} - N_{hd} \left(\frac{\partial C_{hd}}{\partial I_{hd}} + \frac{\partial C_{hd}}{\partial S_{hd}} \bullet \frac{\partial S_{hd}}{\partial I_{hd}} \right) \right] = 0$$

For every DRG, the hospital equates the marginal benefit of intensity with its marginal cost. This expression implicitly defines the optimal intensity choice, I^*_{hd}. To illustrate that an increase in DRG price P_{hd} raises optimal intensity, write $\frac{\partial G_h}{\partial I_{hd}}(I^*_{hd}, P_{hd}) = 0$, differentiate with respect to P_{hd}, and solve for $\frac{dI^*_{hd}}{dP_{hd}}$. Under the assumptions that G_h is twice differentiable and concave in I_{hd} $\forall$ d, and that I_{hd} and $I_{\sim hd}$ are strategic complements $\forall$ d,

$$\frac{dI^*_{hd}}{dP_{hd}} = \frac{-(1-\alpha_h)(\partial N_{hd}/\partial I_{hd}) - \left(\partial^2 G_h / \partial I_{hd} \partial I_{\sim hd} \bullet dI_{\sim hd}/dP_{hd}\right)}{\partial^2 G_h / \partial I_{hd}{}^2} > 0.$$ [8]

This result suggests that price increases should be associated with a "flypaper effect" of the sort widely-documented in the public sector: wherever funds are allocated, that is the area in which they are spent. My primary empirical objective is to test this prediction explicitly by investigating whether hospital costs and other measures of intensity increased more for DRGs that were more highly reimbursed after the policy change. This analysis is presented in Section 6.

Section 6 tests another prediction that follows from the flypaper effect: the *volume* of admissions in DRGs subject to price increases should grow.[9] If intensity levels rise as a result of price

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[7] This model is based on Dranove (1987), Hodgkin and McGuire (1994), Ellis and McGuire (1996), and Gilman (2000).

[8] I adopt the definition of Bulow, Geanakoplos, and Klemperer (1985) by using $\partial^2 G_h / \partial I_{hd} \partial I_{\sim hd} > 0$ to denote strategic complements

increases, by assumption volume should increase as well. This is the classical response expected in fixed-price industries: when price increases, so long as it exceeds marginal cost, firms will want to produce more.

There are several reasons these results may not obtain. First, the link between N_{hd} and I_{hd} may be very weak, reducing the effect of a price increase on intensity levels. Patients may respond to a hospital's overall choice of intensity ("I_h"), but not to I_{hd}, which is more difficult to ascertain.[10] Second, hospitals may be unable to select different intensity levels for each DRG (i.e. intensity is "lumpy" across DRGs). New technologies or practice patterns, once put in place, may be difficult to apply to only a select group of patients. Third, if intensity choices are not initially in equilibrium, a hospital may allocate new funds earned in affected DRGs to overdue investments in unaffected DRGs. Finally, hospitals may maximize objectives that are not captured in the functional form above, such as the total volume of patients.

The objective function G_h is quite general. In particular, hospitals may react differently to the same payment incentives. Any characteristic that affects the parameter α_h will affect the intensity response to a price increase. For example, for-profit hospitals should place a higher weight on profits (lower α_h), as should hospitals under financial duress. The "mission" of a hospital, reflected in such characteristics as teaching status and certification as a trauma center, may also affect the tradeoff between intensity and profits. Alternatively, different hospitals with the same α_h may be differentially-equipped to respond to reimbursement incentives. Small hospitals in particular lack the resources needed to reoptimize quickly in the face of price changes. Finally, there are important regional differences in hospital behavior, although there are no strong theoretical explanations for this phenomenon other than "cultural norms."

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[9] Strictly speaking, this is true so long as price-induced changes in a hospital's own intensity have a greater impact on its volume than the price-induced changes in the intensity of its competitor(s).
[10] Note that patients themselves need not have detailed knowledge of intensity levels; their primary care physicians and specialists may refer them to hospitals based on their assessments of intensity.

Differences across hospitals are one possible source of variation in intensity responses; differences across DRGs are another. For example, patient demand for planned or elective admissions may be more sensitive to changes in intensity than demand for urgent care. When a hospitalization is anticipated, a patient can "shop around," soliciting advice and information directly from the hospital (e.g. the level of patient amenities), as well as from physicians and friends. The elasticity of demand with respect to quality is therefore larger for such admissions, raising hospitals' incentives to increase quality in the face of price increases. Thus, the same price increase may elicit different intensity responses across DRGs. I explore differences in intensity and volume responses across hospitals and admission types in sections 6.2.1 and 6.2.2, respectively.

2.2.1 Incorporating Upcoding

The general model outlined above can be easily expanded to include upcoding effects. Using U_{hd} to denote an "upcoding index," the number of patients N_{hd} can be redefined as an increasing function of U_{hd} and a decreasing function of $U_{h\sim d}$, the degree of upcoding in other DRGs. Holding the number of patients constant, if more patients are upcoded into DRG d, fewer patients are assigned to other DRGs. Upcoding a patient to DRG d also reduces average severity in DRG d (else it would not be upcoding); the effect on average severity in the original DRG is ambiguous. To summarize,

$$N_{hd} = N_{hd}(I_{hd}, I_{\sim hd}, U_{hd}, U_{h\sim d}),\ \frac{\partial N_{hd}}{\partial I_{hd}} > 0, \frac{\partial N_{hd}}{\partial I_{\sim hd}} < 0, \frac{\partial N_{hd}}{\partial U_{hd}} > 0, \frac{\partial N_{hd}}{\partial U_{h\sim d}} < 0$$

$$S_{hd} = S_{hd}(I_{hd}, U_{hd}, U_{h\sim d}),\ \frac{\partial S_{hd}}{\partial I_{hd}} > 0, \frac{\partial S_{hd}}{\partial U_{hd}} < 0, \frac{\partial S_{hd}}{\partial U_{h\sim d}} <> 0\ .$$

Adding a probability of detection μ_h that is increasing in the level of upcoding, a penalty T_h if the hospital is caught upcoding, and a total cost of upcoding R, the objective function becomes

$$G_h = \alpha_h f(I_{h1}, I_{h2}..I_{hD}) + (1-\alpha_h)\begin{bmatrix} \sum_{d=1}^{D}\left([P_{hd} - C_{hd}(I_{hd}, S_{hd}(I_{hd}, U_{h1}, U_{h2}..U_{hD}))]N_{hd}(I_{hd}, I_{\sim hd}, U_{h1}, U_{h2}..U_{hD})\right) \\ -\mu(U_{h1}, U_{h2}...U_{hD})T_h - R(U_{h1}, U_{h2}...U_{hD}) \\ \end{bmatrix}$$

with the following first-order condition for U_{hd}:

$$\frac{\partial G_h}{\partial U_{hd}} = (1-\alpha_h)\left[\sum_{j=1}^{D}\left((P_{hj} - C_{hj})\frac{\partial N_{hj}}{\partial U_{hd}} - N_{hj}\left(\frac{\partial C_{hj}}{\partial S_{hj}} \bullet \frac{\partial S_{hj}}{\partial U_{hd}}\right)\right) - \frac{\partial \mu}{\partial U_{hd}} T_h - \frac{\partial R}{\partial U_{hd}}\right] = 0 .$$

Hospitals trade off the added revenue (less any change in treatment costs) from shifting patients into higher-weighted DRGs against the increased risk of detection plus the cost of upcoding. In its purest form, upcoding implies no effect whatsoever on the amount of care received by patients, so treatment costs are unchanged. Holding the penalties and costs associated with upcoding constant, a price increase for a given DRG increases the incentive to upcode patients into that DRG.[11]

Upcoding costs depend on the availability of multiple DRG codes for similar diagnoses. It is theoretically possible to assign a patient with bronchitis to the heart transplant DRG, but such overt miscoding requires altering medical records substantially and increases the risk of detection later on (whistle-blowers are rewarded by the government). The policy change I study affected DRGs that are particularly susceptible to upcoding because these are DRGs for which the same diagnosis has two different codes, one with a significantly higher weight. A former manager from the largest for-profit hospital chain, Columbia/HCA (now HCA), reported that hospital managers were rewarded for upcoding patients with these diagnoses into the higher-weighted "with complications" codes (Lagnado 1997). Section 7 presents results on upcoding following the 1988 price shock.

As with intensity levels, there are many reasons that upcoding behavior may differ across hospitals and DRGs. Hospitals with a lower α_h should upcode more, while hospitals with a greater

[11] The conditions for this prediction to hold are analogous to those in section 2.2: G_h must be twice differentiable and concave in U_{hd}, and the cross-partial $\partial^2 G_h / \partial U_{hd} \partial I_{hd} \geq 0$. This cross-partial can reasonably be expected to equal zero, as the marginal benefit of intensity should not vary with upcoding.

penalty T_h (real or perceived, monetary or otherwise) or a higher probability of detection μ_h should upcode less. There are a number of theories of the effect of hospital ownership on upcoding, but few consensus predictions (see Silverman and Skinner 2000 for a comprehensive discussion). Hospitals experiencing financial distress should be more willing to risk detection, all things equal, while larger hospitals may be "savvier" in training their coding personnel. Practices of competitors may also affect upcoding indirectly through pressure on hospital profits, or directly via the dissemination of upcoding practices.[12]

Finally, upcoding may also vary across DRGs. Diagnoses based on subjective interpretations of patient conditions are more prone to upcoding, as are diagnoses for which minor variations (e.g. presence of a complication) are associated with large reimbursement differences. The upcoding analysis in section 7 focuses on diagnoses in this latter group. Within this subset of conditions, I also investigate the relationship between the extent of upcoding in a particular diagnosis and the financial incentive to upcode.

2.3 Previous Research

Within the voluminous PPS literature, two distinct lines of research lay the groundwork for the analysis undertaken here. The first consists of a small number of papers that address the impact of average reimbursement amounts on hospital behavior and patient outcomes. These papers all find a positive association between price changes and intensity levels. However, as detailed below, these findings may be biased because the price changes identifying the intensity responses are associated with transitions to prospective payment systems (or, in one case, a major simultaneous change in marginal and average reimbursement incentives). While my empirical results differ from this body of literature, they are consistent with pre-existing research on DRG creep. Studies of DRG creep attempt to

[12] See Duggan (2002) for evidence that not-for-profit hospitals respond more strongly to financial incentives to treat indigent patients in markets with greater for-profit penetration.

ascertain how much of the increase in average case weight is due to real increases in the morbidity of the patient population, and how much to upcoding by providers. My analysis confirms earlier findings that hospitals engage in upcoding, but the majority of the case-mix increase is real.

2.3.1 Average Reimbursement Effects

Virtually all of the papers that evaluate the impacts of PPS do not distinguish between the effects due to changes in marginal reimbursement (during the phase-in of the system) and those due to changes in average reimbursement levels (P_{dh}).[13] The first papers to distinguish these effects at the diagnosis level are Cutler (1990) and Cutler (1995).[14] Cutler (1990) studies the transition to PPS in Massachusetts, finding that length of stay and number of procedures per patient declined the most in DRGs subject to the largest price reductions. Despite finding an elasticity of intensity with respect to price of .2, Cutler does not find a corresponding volume response.[15] Cutler (1995) studies the impact of PPS on adverse medical outcomes, again finding an intensity response: reductions in average price levels are associated with a compression of mortality rates into the immediate post-discharge period, although there is no change in mortality at one year post-discharge. Both papers assume that eliminating the marginal reimbursement incentive affects all DRGs equally. However, intensity reductions may be easier to make in certain DRGs and/or hospitals, and to the extent that price reductions were more prevalent in such DRGs and/or hospitals, the intensity responses to price changes will be overstated. More generally, the elasticity estimate will be biased by any omitted factor influencing both price and intensity changes during the transition to PPS.

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[13] Hodgkin and McGuire (1994) provide an excellent overview of empirical research on this subject.

[14] Studies of *hospital*-level responses to changes in average reimbursement amounts include Hadley, Zuckerman, and Feder (1989) and Staiger and Gaumer (1992). These works find positive intensity responses as measured by length of stay and patient survival, respectively. Cutler (1998) studies responses to average payment reductions implemented through the annual update factor. He finds cost-shifting to private payors in the early PPS era (1985-1990), and cost-cutting through capacity and nursing staff reductions in the later PPS era (1990-1995).

[15] Such a result could be consistent with a model in which volume is not a function of intensity, and hospitals maximize intensity within each DRG subject to a breakeven constraint. Note that this model requires money to be spent where it is earned (i.e. the flypaper effect must hold). Absent this restriction, hospitals would optimally allocate additional funds across all DRGs.

Cutler calculates the change in average price as the difference between the 1988 PPS price and the price that Medicare would have paid in 1988 were cost-plus reimbursement still in effect. To estimate this latter figure, he inflates 1984 costs for each DRG by the overall cost-growth rate for 55-64 year-olds. However, DRGs with disproportionately stronger cost growth between 1984 and 1988 received weight increases, yielding higher 1988 PPS prices and generating the concern that the positive relationship between price changes and intensity levels may be spurious. The possibility that these estimated price changes are not exogenous is reinforced by the use of hospital-specific prices in the specifications. The average price changes are therefore related to hospitals' pre-PPS DRG-specific costs; hospitals with high costs faced price reductions when transitioning to national payment standards. Such hospitals may have had "more fat to trim" in terms of intensity provision.

The two additional studies addressing DRG-specific intensity responses to price changes employ very different identification strategies, but reach the same conclusion. Gilman (2000) investigates the impact of a 1994 reform to Medicaid DRGs for HIV diagnoses in New York. The reform added several procedure-based DRGs with large weights, introducing a marginal incentive to perform these procedures. The large weights for these new DRGs were offset by large reductions in the weights for non-procedural DRGs; Gilman therefore identifies the average price effect by comparing changes in length of stay for procedural and non-procedural admissions. He finds increases in length of stay for procedural admissions, and decreases for non-procedural admissions, although only the latter result is statistically significant. Assuming the controls for patient severity adequately capture the severity changes in the patient population for both admission types, these results also suggest that hospitals adjust DRG-specific intensity in response to price changes. Newhouse (1989) finds some evidence that private hospitals successfully shifted patients in unprofitable DRGs to public hospitals following the implementation of PPS; the mechanism for this shift is not specified, but the finding implies real responses to incentives at the DRG level.[16] As with the Cutler studies, these works investigate simultaneous changes in marginal and average reimbursement incentives. The policy change I assess affects only average reimbursement levels, eliminating the need to disentangle the responses to changes in marginal incentives. In addition, because the policy change affected a large proportion of DRG codes (40 percent), the analysis produces representative estimates of DRG-specific intensity responses.

2.3.2 Upcoding

Because the single largest source of increased hospital spending by Medicare is the rapid rise in the average case weight, the subject of upcoding has received substantial research attention. Most of this research is dated, focusing on the first few years of PPS. Coulam and Gaumer (1991) review this literature through 1990, concluding that there is evidence of upcoding during the first few years of PPS, but the amount of the case-mix increase attributable to this practice is unknown. There are two general empirical approaches to estimating the magnitude of upcoding: detailed chart review, and comparisons of case-mix trends over time and across hospitals.

Carter, Newhouse, and Relles (1990) use the 'gold standard' in chart review to estimate the role of upcoding in the case-mix increase between 1986 and 1987: they send a nationally representative sample of discharge records from 1986 and 1987 to an expert coding group (called the "SuperPRO") that regularly reviews samples of discharges to enforce coding accuracy. They find that one-third of the case-mix increase was due to upcoding, although the standard error of this estimate is large. More recently, Psaty et al (1999) use detailed chart review to estimate that upcoding is responsible for over one-third of admissions assigned to the heart failure DRG (DRG 127).

[16] Newhouse specifically considers the possibility that private hospitals transferred unprofitable patients to public hospitals after admission, but does not find any evidence to support this mechanism for case redistribution.

Most of the non-medical analyses of case-mix increases (e.g. Steinwald and Dummit 1989) are descriptive, focusing on which types of hospitals exhibit faster case-mix growth (large, urban, and teaching hospitals), and when these increases occur (there is a big jump in the first year a hospital is paid under PPS). As with intensity responses, upcoding responses are difficult to estimate using data from the transition period, when patient severity changed dramatically due to changes in patient composition. However, Silverman and Skinner (2000) present strong evidence of post transition-era upcoding for pneumonia and respiratory infections between 1989 and 1996. Focusing on the share of patients with these diagnoses that are assigned to the most expensive DRG possible, Silverman and Skinner document large increases in upcoding, despite a downward trend in mortality rates. Interestingly, the authors find that for-profit hospitals upcode the most, and that not-for-profit hospitals are more likely to engage in upcoding when area market share of for-profit hospitals is higher, independently of financial distress and other control variables. This finding is consistent with a contagion model described in Cutler and Horwitz (1999), or an environment dominated by "cultural norms." In addition, hospitals under financial distress upcode *less* than financially sound institutions.

My upcoding analysis takes a similar approach, but the policy change I analyze offers two important advantages. First, I study an abrupt change in upcoding incentives that should be met with a similarly abrupt change in upcoding if hospitals are responsive to these incentives. The continuous time-series trends identified by Silverman and Skinner could be due to real trends in increasing patient severity, notwithstanding the decline in mortality rates during the study period. Mortality rates are imperfect measures of severity, and they are likely to be endogenous to intensity of care. Second, because the policy change created upcoding incentives that vary by diagnosis, I am able to investigate not only whether hospitals respond to upcoding incentives in general, but also whether they respond to upcoding incentives *on the margin*, upcoding more when the payoff is greater.

3 A Price Shock: HCFA's Elimination of the Age > 69/Age < 70 Criterion

Although there were 473 individual DRG codes in 1987, 40 percent of these codes belonged to a "pair" of codes that shared the same main diagnosis. Within each pair, the codes were distinguished by age restrictions and presence of complications (CC). For example, the description for DRG 96 was "bronchitis and asthma age>69 and/or CC," while that for DRG 97 was "bronchitis and asthma age 18-69 without CC." Accordingly, the DRG weight for the top code in each pair exceeded that for the bottom code. There were 95 such pairs of codes, and 283 "single" codes.

In 1987, separate analyses by HCFA and the Prospective Payment Assessment Commission (ProPAC) revealed that "in all but a few cases, grouping patients who are over 69 with the CC patients is inappropriate" (52 Federal Register 18877).[17] The ProPAC analysis found that hospital charges for uncomplicated patients over 69 were only 4 percent higher than for uncomplicated patients under 70, while average charges for patients with a CC were 30 percent higher than for patients without a CC. In order to minimize the variation in resource intensity within DRGs and to reimburse hospitals more accurately for the affected diagnoses, HCFA eliminated the age over 69/under 70 criterion beginning in 1988. The agency recalibrated the weights for all DRGs to reflect the new classification system. This resulted in a large increase in the weight for the top code within the DRG pairs, and moderate declines for the bottom code.

Table 1 gives the three most commonly-coded pairs and their DRG weights before and after the policy change.[18] These examples are fairly representative of the change overall. Using 1987 admissions from a 20 percent sample of Medicare discharge data as weights, the weighted average increase in the top code for all affected DRG pairs was 11.3 percent, while the weighted average

[17] ProPAC, now incorporated into MedPAC (Medicare Payment Advisory Commission), was an independent federal agency that reported to Congress on all PPS matters.
[18] The large volume increase for the bottom code in each pair is due to the new requirement that uncomplicated patients over 69 be switched from the top to the bottom code.

decrease in the bottom code was 6.2 percent. In the final notice of the policy change, HCFA clearly states that all weights were recalibrated to ensure no overall change in reimbursement to hospitals; that is, the average national DRG weight was constant whether the 1987 or the 1988 classification system (called the GROUPER program) was employed on a given set of discharge records.[19] It is worth emphasizing here, however, that while annual recalibrations are intended to be revenue-neutral *overall*, there is no requirement that they be revenue-neutral for any subset of DRGs.

Indeed, as the analysis in Section 5 reveals, this policy change resulted in a moderate absolute price increase and a large relative price increase for discharges coded in DRG pairs, compared to discharges in DRG singles. There are two sources for this price increase: a *coding* component, due to hospital upcoding or real increases in patient severity, and a *mechanical* component, due to the recalibration. I investigate the coding component in section 7; the mechanical component is inferred from the residual because the data and programs needed to calculate it directly are not available to me. Following the large 3.9 percent increase in the average case weight between 1987 and 1988, HCFA published its own (unfortunately flawed) analysis of the contribution of annual recalibrations to the case weight increase between 1986 and 1988. HCFA concluded that .93 percentage points could be attributed to faulty recalibration of DRG weights for 1988 (their estimate of the mechanical component), and an additional .29 percentage points to similar errors in 1987. In response to these discoveries, HCFA instituted an across-the-board reduction of 1.22 percent in all DRG weights beginning in 1990. Because this reduction applied uniformly to all DRGs, the large effects on the DRG pairs were unabated.

This policy change provides an excellent opportunity to study hospital responses to changes in DRG-specific prices. After describing my data sources, I analyze the effects of this price shock in three parts. First, I assess the magnitude of the shock to reimbursement levels for affected DRGs. Second, I investigate whether hospitals increased the intensity of care provided to patients in DRG

[19] There were only a few minor changes to the GROUPER program between 1987 and 1988 that were not associated with the elimination of the age criterion.

pairs relative to patients in DRG singles, and whether the volume of admissions in these DRGs increased accordingly. Third, I explore the role of upcoding in generating the reimbursement increases I identify.

4 Data

My primary data sources are the 20 percent Medicare Provider Analysis and Review (MEDPAR) files (FY85-FY91), the annual tables of DRG weights published in the Federal Register (FY85-FY91), the Medicare Cost Reports (FY85-FY91), and the Annual Survey of Hospitals by the American Hospital Association (1987). The MEDPAR files contain data on all hospitalizations of Medicare enrollees, including select patient demographics, diagnoses, measures of intensity of care (e.g. length of stay and number of surgeries), and hospital identification number. The data span the three years before and after the policy change. After matching DRG weights from the Federal Register to these individual records, I construct group means for DRG weight (henceforth DRG price), volume, intensity, and upcoding measures.[20] The level of aggregation varies by analysis, and is described in greater detail below. Hospital characteristics from the year preceding the policy change are then merged onto these cells, where appropriate.

From the Cost Reports, which contain annual financial data on all Medicare providers, I obtain the hospital's debt:asset ratio.[21] From the Annual Survey of Hospitals, I obtain two additional financial distress measures, Medicare "bite" (the fraction of a hospital's discharges reimbursed by Medicare) and Medicaid "bite" (similarly defined), as well as several other hospital characteristics that may be associated with responses to the shock: ownership status (not-for-profit, for-profit, and government), region (South, Northeast, Midwest, West), teaching status, number of general beds and intensive care

[20] Per the discussion in Section 2, the terms *price* and *DRG weight* can be used interchangeably, so long as price is logged.
[21] The Cost Reports also contain an indicator for whether a hospital is paid under the PPS system (certain hospitals are exempted). I omit exempt hospitals from my sample.

beds, and service offerings (trauma center, open heart surgery). Descriptive statistics for the hospital variables are provided in Appendix Table 1. Descriptive statistics for drg-year, hospital-year, and hospital-drg-year cells are in Table 2.

5 Assessing the Magnitude of the Price Shock

The elimination of the age criterion resulted in large price changes for DRGs belonging to pairs, as described in section 3. However, it would not be informative to investigate whether intensity levels rose (fell) for patients admitted to the top (bottom) code of DRG pairs, because the composition of patients admitted to each code changed as a result of the policy reform. Top codes, which were formerly assigned to all older patients as well as to young, sick, patients, are now used exclusively for sick patients, young or old. A finding that average intensity of care increased in top codes would not yield information on whether hospitals increased intensity of care for sick patients, the only patients who were subject to a price increase. In order to keep the reference population constant before and after the policy reform, I combine data from the top and bottom codes, effectively creating a single DRG for each pair. It is therefore critical to illustrate that the average price paid for patients in these newly-created paired DRGs did indeed increase following the 1988 elimination of the age criterion.

To assess the magnitude of the price increase, I employ a differences-in-differences technique, comparing the time-series changes in price for the paired DRGs (henceforth the "affected DRGs") with the changes in price for the single DRGs (the "unaffected" DRGs). While the DRG price for unaffected DRGs is given annually by HCFA, the price for affected DRGs is a weighted average of the prices for the top and bottom codes in each pair. For example,

$$\text{price}_{\text{DRG138/139,1988}} = \frac{\text{price}_{\text{DRG 138,1988}} * \text{N}_{\text{DRG 138,1988}} + \text{price}_{\text{DRG 139,1988}} * \text{N}_{\text{DRG 139,1988}}}{\text{N}_{\text{DRG138,1988}} + \text{N}_{\text{DRG 139,1988}}}$$

$$= \frac{.8535 * 35{,}233 + .5912 * 16{,}829}{35{,}233 + 16{,}289} = .7687$$

where N denotes the number of total admissions in the MEDPAR sample. I use this formula to calculate prices for the affected DRGs in every year. To evaluate the aggregate impact of the policy change, I assemble a dataset of annual prices for the affected and unaffected DRGs between 1985 and 1991, and estimate the following basic specification:

(1) $$\ln(\text{price})_{dt} = \alpha + \varsigma \text{DRG}_d + \delta \text{year}_t + \gamma_1 \text{affected DRG}_d \bullet \text{post}_t + \varepsilon_{dt}$$

where d indexes DRGs and t indexes years, affected DRG is a dummy variable that equals one for the treatment group (DRGs affected by the policy change), post is an indicator for the years following the policy change (1988-91), and the dimensions of the coefficient vectors are ς (1 x 387), δ (1 x 6), and γ_1 (1 x 1).[22] Note that the affected DRG main effect is absorbed by the inclusion of the DRG fixed effects. The coefficient of interest, γ_1, captures the average price change for paired DRGs *relative to* single DRGs during the post period. Each observation is weighted by the number of discharges for that DRG-year cell.

The results of this analysis are displayed in column 1 of Table 3. To address the concern that price trends may differ for affected and unaffected DRGs prior to the policy change, column 2 replaces affected·post with individual affected·year dummies. Column 3 returns to the basic specification above but includes a time trend for the affected DRG group. As a final robustness check, column 4 presents estimates including individual DRG time trends. The $\hat{\gamma}_1$ reveal a statistically significant and robust impact of the policy change on reimbursement for patients assigned to the affected DRGs. Beginning in 1988, hospitals were paid 7 percent more for admissions to paired DRGs. Importantly, prices for the affected DRGs did not display a different trend from prices for the unaffected DRGs in the years prior to the shock, as demonstrated by the graph of affected·year coefficients in Figure 1. This finding supports the contention that the price change was in fact exogenous, and not attributable to different

[22]Of the 95 DRG pairs and 300 single DRGs in place by 1991, 2 pairs are dropped because the age criterion was eliminated one year early for these pairs, and 5 single DRGs are dropped because there were no admissions coded in these DRGs in the MEDPAR sample.

pre-existing trends for DRG prices in the two groups. The 7 percent price increase is substantial because it represents pure profits in an industry where profit margins are on the order of 1-2 percent.

The coefficients on the year dummies in columns 1 and 2 are also informative, as they summarize the effects of HCFA's annual recalibrations. The decline between 1989 and 1990/91 reflects HCFA's across-the-board price reduction, while the decline between 1987 and 1988 indicates that the recalibration following the policy change decreased the prices for unaffected DRGs.[23] Summing the year and affected·post DRG coefficients, it is evident that the elimination of the age criterion caused a large relative increase in prices for the affected versus the unaffected DRGs (~7 percent), and a smaller absolute increase (~2 percent in 1988).

6 Intensity and Volume Responses

Given that the 1988 policy reform resulted in substantial relative price increases for affected DRGs, and that these increases were not associated with any pre-existing cost trends, the intensity and volume responses to price changes can be identified using the differences-in-differences strategy outlined above. If the flypaper effect operates in this setting, intensity levels should rise in affected DRGs relative to unaffected DRGs after 1988. I use five different measures of intensity and quality of patient care to investigate this response: total costs (=total charges from MEDPAR deflated by annual cost:charge ratios from the Cost Reports and converted to $1990 using the hospital services CPI), length of stay, number of surgeries, number of ICU days, and in-hospital deaths. All variables are normalized by the number of admissions in the relevant cell (i.e. average cost per patient in "DRG" 138/139 in 1987). The first four measures are strong indicators of hospital expenditures on behalf of

[23] Using the regression results from column 1, the null hypothesis $\hat{\delta}_{89} = \hat{\delta}_{91}$ against $\hat{\delta}_{89} > \hat{\delta}_{91}$ can be rejected at the p=.05 level, and $\hat{\delta}_{87} = \hat{\delta}_{88}$ against $\hat{\delta}_{87} > \hat{\delta}_{88}$ can be rejected at p=.001.

patients.[24] Death rate is clearly an important, albeit limited, indicator of quality of care. Although these measures are commonly used in the health economics literature, they are imperfect. One of the most common measures, length of stay, could be correlated positively or negatively with quality of care: better care may enable a patient to leave sooner; on the other hand, hospitals may discharge patients too early in order to cut costs. (The latter was of greater concern in the 1980s, as lengths of stay fell dramatically in response to PPS.) However, the consistency of the results across all of the variables suggests that the findings are robust.

Given the model outlined in section 2, another way to identify an intensity response is to look at the volume of patients admitted. If hospitals do increase intensity of care within the affected DRGs, they should also admit more patients in these DRGs. Stated another way, hospitals seeking to increase volume in affected DRGs following the price shock must increase their investment in intensity.[25]

6.1 Aggregate Intensity and Volume Responses

To identify the aggregate effect of the policy change on intensity and volume, I estimate the same specification used for the price analysis, replacing ln(price) with ln(intensity) or ln(admissions):

$$\ln(\text{intensity})_{dt} \text{ or } \ln(\text{admissions})_{dt} = \alpha + \varsigma \text{DRG}_d + \delta \text{year}_t + \gamma_2 \text{affected DRG}_d \cdot \text{post}_t + \varepsilon_{dt} \qquad (2)$$

As with the price analysis, this reduced-form specification produces transparent estimates of the policy effect. To ensure that $\hat{\gamma}_2$ is not capturing pre-existing trends in intensity or volume, I again estimate this specification with separate affected·year dummies, an affected DRG time trend, and individual DRG time trends. For each dependent variable, the $\hat{\gamma}_2$ from this last specification (akin to column 4 in

[24] Total charges should be positively correlated with the services provided to patients; indeed, this is the measure HCFA uses to calculate DRG weights, so that diagnosis groups with higher average charges are reimbursed more relative to diagnosis groups with lower average charges. Because costs are a better measure of intensity, however, I adjust charges by annual hospital cost:charge ratios.

[25] Note that advertising can certainly be one component of intensity, although I do not have data on such expenditures.

Table 3) is reported in Table 4A.[26] I find no evidence that hospitals altered their treatment policies or increased their admissions differentially for patients in affected DRGs as a result of the 1988 classification change.

The ratio of $\hat{\gamma}_2$ from Table 4A and $\hat{\gamma}_1$ from column 4 of Table 3 produces the IV estimates of the elasticity of DRG quality/volume with respect to price.[27] These estimates and their standard errors are presented in Table 4B. Although the point estimates for the intensity elasticities are small, the standard errors are large due to the imprecision with which the γ_2 coefficients are estimated. To obtain more precise estimates of the upper bounds for these elasticities, I run OLS regressions of intensity on price. These estimated elasticities, reported in Table 4C, are upward-biased due to the price recalibration method.[28]

Notwithstanding this bias, the point estimates are extremely small. For example, the OLS estimate indicates that only 13 cents of every additional dollar of reimbursement within a DRG is spent on care for patients in that DRG. The elasticity of length of stay with respect to price (.18) is similar to the estimate reported in Cutler (1990) (.23), but the elasticity of surgeries is much smaller (-.03 as compared to .23), and there is no evidence that in-hospital mortality rates decline in price, as reported in Cutler (1995). Together with the results in Table 4A, these estimates suggest that the flypaper effect does not operate, on average, in this industry.

The estimates for the elasticity of DRG volume with respect to DRG price are less consistent across the specifications. While the data cannot reject H_0: $\eta_{volume:price} = 1$ against H_1: $\eta_{volume:price} < 1$ using the IV estimate, the data easily reject H_0: $\eta_{volume:price} = .15$ in favor of H_1: $\eta_{volume:price} < .15$ using the

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[26] Observations with a value of zero for the unlogged dependent variable are dropped. Regressions of 1(intensity>0) reveal no relationship with the year and year*affected dummies; hence, Tobit estimates using the unlogged dependent variables did not differ from OLS results for the same specifications. The interpretation is that there are some DRGs for which an intensity measure is typically zero, such as death rate in the DRG for tonsillectomy, and excluding such DRGs from the intensity analyses does not affect the estimation.

[27] Because N varies slightly for each dependent variable, the IV coefficients in Table 3 may not be exactly equal to $\hat{\gamma}_2/\hat{\gamma}_1$.

[28] One manifestation of this bias is the positive estimated elasticity of death rate with respect to price; the explanation for this puzzling result is simply that those DRGs that experience increases in death rates receive higher reimbursements because in-hospital care for the dying is very expensive.

OLS estimate. Furthermore, there is no a priori expectation of bias in the OLS regression, and a Hausman specification test accepts the hypothesis that OLS is consistent with p=1.0. I therefore conclude that there is no evidence of a positive volume response at the DRG level.

6.2 Intensity and Volume Responses Across Hospitals and DRGs

6.2.1 Responses Across Hospitals

The aggregate analyses capture the average intensity and volume responses across all admissions, but mask potentially different responses across hospitals. According to the model defined in section 2.2, hospitals with stronger profit objectives and/or more quality-elastic demand should increase intensity (and therefore volume) more in response to the price increase. To determine whether individual hospitals responded differently, I disaggregate the data into hospital-DRG-year cells. I then match these cells to hospital characteristics from the 1987 AHA file and the 1987 HCFA Cost Reports, as described in Section 4, and estimate the following specification *separately* by hospital sub-group:

(3) $$\ln(\text{intensity})_{hdt} \text{ or } \ln(\text{admissions})_{hdt} = \alpha + \varsigma DRG_d + \omega X_h + \delta year_t + \gamma_3 affectedDRG_d \cdot post_t + \varepsilon_{hdt}$$

where X_h is a vector of hospital characteristics (listed in Appendix Table 1).[29] I then compare $\hat{\gamma}_3$ for the different groups. For example, I estimate (3) separately by hospital ownership type (for-profit, not-for-profit, government). I also consider stratifications by financial status, region, size, teaching affiliation, service offerings, and market-level Herfindahl indices.[30] Each observation is weighted by the number of admissions in the MEDPAR sample, and the standard errors are corrected for heteroskedasticity and correlation within DRG-year clusters.

The results of estimating this specification using 6 dependent variables and 21 subsamples are virtually unanimous: uniformly, hospitals did not exhibit DRG-specific intensity or volume responses

[29] The dimensions of ω vary by specification. For example, when the model is estimated separately for hospitals in each region, the region dummies are not included. Exact specifications are available by request. The size of the dataset precludes estimation with hospital fixed effects, drg·hospital fixed effects, and/or drg·year trends.
[30] The Herfindahl index is calculated as the sum of squared market shares for all hospitals within a health service area, as defined by the AHA in 1987.

to the 1988 price shock. $\hat{\gamma}_3$ is only significant in four of the 105 specifications, and in these cases it is negative.[31] Due to the large volume of coefficients generated by these models, they are not reported here.

In sum, there is no evidence of real responses by any subset of hospitals to the very real price increases documented in Table 3. Section 2 gives several possible explanations for these findings, focusing on the potential inability of hospitals to alter intensity at the DRG level, and of patients in turn to respond. The quality elasticity of demand is paramount in this response, and there is reason to believe that this elasticity is extremely low for certain diagnoses. For example, hospitals may invest substantial funds in improving care for amputees, but these investments are unlikely to yield additional volunteers for the surgery. This reasoning suggests that it may be more fruitful to examine intensity responses separately by DRG type.

6.2.2 Responses Across DRGs

All admissions in the MEDPAR files are assigned to one of 5 categories: emergency (admitted through the ER, 44 percent of admissions in 1987); urgent (first available bed, 29 percent); elective (23 percent); newborn (0.1 percent); unknown (4 percent). To see how intensity and volume responses differed across these admission types, I assign each DRG to one of the three main groups and re-estimate the aggregate analysis (equation 3 above) separately by group. To designate groups, I use the distribution of admission type shares in 1985; if a DRG had an emergency (elective) share that exceeded the 75th percentile of such shares, it was designated an emergency (elective) DRG.[32]

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[31] Tables available by request. The significant estimates are: $\hat{\gamma}_3$ for ln(surgeries) in government hospitals = -.042 (.016), $\hat{\gamma}_3$ for ln(surgeries) in hospitals with 100-200 beds = -.035 (.018), $\hat{\gamma}_3$ for ln(ICU days) in hospitals with a high Medicaid share = -.104 (.045) , $\hat{\gamma}_3$ for ln(ICU days) in Western hospitals = -.099 (.050).

[32] Because type designations may be involved in upcoding efforts, I use data from the earliest year possible (1985) to classify DRGs by main type. Designating type using the plurality of admissions within a given DRG is not feasible, since fewer than 3 percent of DRGs would be classified as "elective" by this method. There are 15 DRGs that belong to both the elective and the emergency groups.

Again, the intensity and volume non-responses prove remarkably robust. Notwithstanding the strong financial incentive to attract more patients in affected DRGs, hospitals did not increase intensity levels or volume differentially for affected DRGs in *any* admission category following the 1988 relative price increase.

6.3 Why Didn't Hospitals Respond?

Given the simultaneous price increase for top codes and decrease for bottom codes within DRG pairs, one possibility is that hospitals may not have realized they were receiving a relative price increase for the pairs as a whole.[33] Even if hospitals were cognizant of the price increase in affected DRGs, their response may have been muted because of the simultaneous price decrease in unaffected DRGs. The net result was that average prices overall did not increase, as evidenced by the coefficients reported in Table 3. A positive intensity response would therefore involve a decrease in intensity for unaffected DRGs, and to the extent that decreases are more difficult to implement than increases, the coefficients I obtain may underestimate the true intensity-price relationship. This explanation, though certainly a possibility, is by no means a certainty: immediately following the implementation of PPS, hospitals showed themselves quite capable of reducing overall intensity in all of the dimensions I explore.

Another possibility is that hospitals optimize overall intensity, rather than intensity by DRG. To investigate this hypothesis, I aggregate the hospital-drg-year data into hospital-year cells. The relationship of interest is the elasticity of intensity with respect to price, which can be estimated from

$$\ln(\text{intensity})_{ht} = \alpha + \omega\kappa_h + \delta \text{year}_t + \beta \ln(\text{price})_{ht} + \varepsilon_{ht}, \tag{4}$$

where κ are hospital fixed effects. However, there are two sources of bias in the OLS estimate of $\hat{\beta}$ (1) the DRG recalibration method; (2) the omission of an annual hospital-level measure of patient severity. As with the hospital-DRG-year analyses, the policy change can be used to identify β, but

[33] I thank David Cutler for this insight.

hospital-level variation in the impact of the policy change is required – the differences-in-differences strategy with affected and unaffected DRGs cannot be used with hospital-year data. Because hospitals with a large fraction of admissions in the "with CC" DRGs benefited the most from the policy reform, the interaction between this measure and a "post" dummy for the post-reform years can serve as an instrument for average price in equation 4.

In constructing this instrument, I use the 1987 share of young patients with CC (hereafter called *share CC*). I select the pre-shock year because contemporaneous *share CC* would be affected by post-shock upcoding responses, and I use young patients only because the data do not indicate whether old patients had CC before the policy change. This instrument captures the exogenous component of the hospital-level price increase: hospitals with a large *share CC* in 1987 enjoyed larger increases in their average DRG price *independently* of their upcoding response to the policy change, which is explored in section 7. Eliminating the upcoding response from the instrument is essential because upcoding proclivity may be associated with intensity decisions.

Table 5 gives the results from the first-stage regression of ln(price) on *share CC*·post,

(5) $$\ln(\text{price})_{ht} = \alpha + \omega\kappa_h + \delta \text{year}_t + \tau_1 \text{share}\,\text{CC}_h \bullet \text{post}_t + \varepsilon_{ht},$$

where ω is 1 x 5,335. The mean (standard deviation) of *share CC* is .084 (.043). A two-standard-deviation increase in *share CC* is associated with a two percent increase in the average price paid to a hospital following the policy change. To illustrate that *share CC* is uncorrelated with average hospital prices in the pre-reform years (after hospital fixed effects are included), column 2 presents the results from a regression of ln(price) on *share CC* ·year dummies.[34]

Coefficient estimates from the reduced-form equation,

(6) $$\ln(\text{intensity})_{ht} = \alpha + \omega\kappa_h + \delta \text{year}_t + \tau_2 \text{share}\,\text{CC}_h \bullet \text{post}_t + \varepsilon_{ht},$$

[34] An alternative to *share CC* is the share of patients in affected DRGs. However, the affected share·year coefficients reveal that this variable is correlated with pre-reform price declines, so it fails the orthogonality condition for a valid instrument.

are presented in Table 6A, followed by IV estimates in Table 6B and OLS estimates (of equation 4) in Table 6C. The IV estimates for the elasticity of hospital intensity with respect to average admission price are positive for 4 of the 5 intensity measures, and statistically significant for 3. The exception is the in-hospital death rate, for which estimated elasticity is negative, but insignificant (a positive coefficient on death rate implies a negative intensity response). The elasticity results reveal that *an additional dollar of reimbursement goes wholly toward patient care*. Extra reimbursement is associated with longer stays, more surgeries, more ICU days, and possibly worse outcomes.

Hospitals subjected to price increases also appear to have increased their patient volumes: a one percent price increase is associated with a 1.7 percent increase in volume. Contrary to the hospital-drg-year analysis, this estimate is statistically significant, and a Hausman test rejects consistency of the OLS estimator.

The results in Table 6 suggest that hospitals may compete in overall intensity, or that they simply spend the money they earn on patient care. Competition in DRG-specific intensity cannot, however, be completely ruled out. If intensity is "lumpy" across DRGs, hospitals will be unable to fine-tune intensity at the DRG level, even if patients would be responsive to such changes.

Although hospitals did not fine-tune their intensity response to the 1988 reform, the following section documents a high degree of fine-tuning in upcoding decisions.

7 DRG Creep or Jump?

In fixed-price regimes, real responses are only one source of concern. Nominal changes in behavior, reflected in this setting by hospital coding practices, can profoundly impact the success of such systems. By 1987, upcoding was widely regarded as a major problem for PPS, and HCFA's policy change only strengthened the incentives for hospitals to engage in this practice. The increase in prices for the top codes in affected DRGs, together with the decrease in prices for the bottom codes, provided a strong incentive *to continue using the top code for all older patients (not just those with CC), and to use it more frequently for younger patients*. Because all older patients were assigned to the top codes during the pre-shock years, upcoding older patients is the easier of the two options; a hospital assigning a large proportion of older patients to the top codes following the policy change could argue that its older patients were always relatively complicated. After all, it was not necessary to code complications for older patients during the pre-shock period, so a comparison of pre/post behavior cannot be conclusive. Upcoding among the young requires *shifting* patients into the top codes, and is therefore easier to detect. For this reason, my identification strategy provides upper and lower bounds for upcoding among the young, but only lower bounds for upcoding among the old.

7.1 Aggregate Upcoding Analysis

The dependent variable for this analysis is fraction_{dt}, the share of admissions to pair d in year t that is assigned to the top code in that pair. Because this variable can only be defined for DRG pairs, single DRGs cannot serve as a control group. For young patients, time-series identification is a possibility; a discrete jump in the fraction of patients coded with complications after 1988 suggests an upcoding response to the classification change. However, confounding factors such as an increasing trend in real severity of patients may also contribute to this response. For old patients, it is impossible to use the time-series decline in fraction_{dt} to estimate the upcoding response because the magnitude of the decline in the absence of upcoding cannot be determined. I therefore introduce a new independent variable,

$$\text{spread}_{dt} = \text{DRG weight in top code}_{dt} - \text{DRG weight in bottom code}_{dt}\,,$$

e.g. $$\text{spread}_{\text{DRG } 138/139,\ 1988} = \text{weight}_{\text{DRG } 138,\ 1988} - \text{weight}_{\text{DRG } 139,\ 1988}$$

$$= .8535 - .5912 = .2623.$$

$spread_{dt}$ is simply a measure of the upcoding incentive in pair d at time t. Between 1987 and 1988, the mean spread increased by .24, or nearly $1000. The standard deviation of this increase was .19, however, illustrating the wide variability in spread changes across DRGs. In the regression

$$\text{(7)} \qquad fraction_{dt} = \alpha + \varsigma DRG_d + \delta year_t + \psi spread_{dt} + \varepsilon_{dt}$$

δ captures the *average* impact of the policy reform on all DRGs, while ψ captures the *marginal* effect of differential upcoding incentives. $\hat{\psi} > 0$ signifies that hospitals upcoded more in DRGs where the incentive to do so increased more.

The OLS estimate of ψ is identified off all contemporaneous changes in spread. By instrumenting for spread with $\Delta spread_{1987\text{-}88} \cdot post$, ψ will be identified only off changes in spread associated with the 1987-1988 price changes.[35] I estimate equation (7) separately for old and young patients, and present OLS and IV results in Table 7.

Table 7 shows that upcoding is sensitive to changes in spread, even after controlling for the large annual shifts in this measure. For young patients, there is a large average increase in $fraction_{dt}$ between 1987 and 1989. However, this response is modulated by the precise incentives within each DRG. The IV estimates indicate that a spread increase of one standard deviation is associated with an increase of .011 in the fraction of young patients coded with complications, or 42 percent of the average time-series increase between 1987 and 1989. Using only the spread coefficient to estimate the upcoding response for the young, the 1988 policy change is associated with an increase of .5 percent in the average price for these admissions. Adding the increase between 1987 and 1989 brings this figure to 1.5 percent, although this is an upper-bound estimate due to the potential role of confounding factors.

[35] The recalibration of weights for FY88 also reflects charge growth between 1985 and 1986 (recalibrations are based on lagged charge data). However, regressions of $\Delta spread_{1987\text{-}88}$ on $\Delta charges_{1987\text{-}88}$ or $\Delta \ln(charges)_{1987\text{-}88}$ reveal no relationship between these measures, ruling out the possibility that $\hat{\psi}$ is capturing a relationship between fraction and lagged charges (as could arise if cost growth spurs upcoding, and errors are serially correlated.)

The coefficient on spread among the older subset of patients suggests that the upcoding response was indeed greater for this group; an increase of one standard deviation in spread is associated with an increase of .022 in the fraction of old patients coded with complications. The year coefficients indicate that the fraction of patients assigned to the top code declined in 1988 as expected, but this decline was least where the incentive to retain patients in the top code was greatest. Because the average weight for old patients in 1987 was automatically higher than that for young patients, the spread-related increase in upcoding translates into a .9 percent increase over 1987 reimbursement for these patients.

Combining the estimates for the young and old subsamples, upcoding following the 1988 policy change is associated with increased annual payments of $300 to $410 million, with the first figure including only the upcoding estimates from the spread coefficients, and the second incorporating the average increase in upcoding of the young between 1987 and 1989. HCFA's 1990 reduction in case weights decreased payments by $1.05 billion, more than wiping out these estimated windfalls. However, this analysis underestimates upcoding among the old, which is likely to be important both because the old account for 70 percent of Medicare admissions, and because upcoding is more prevalent in this group. Nevertheless, the results do suggest that the vast majority of the 7 percent relative price increase in affected DRGs was due to mechanical rather than coding changes.

7.2 Upcoding Across Hospitals

To investigate upcoding differences across hospitals, I estimate

$$\text{fraction}_{dht} = \alpha + \varsigma \text{DRG}_d + \omega X_h + \delta \text{year}_t + \psi \text{spread}_{dt} + \varepsilon_{dht} \tag{8}$$

by the various hospital subsamples, using $\Delta\text{spread}_{1987\text{-}88} \cdot \text{post}$ as an instrument for spread. Standard errors are corrected for heteroskedasticity and correlation within DRG-year clusters. For both young and old patients, there are no statistically significant differences in spread across the hospital groups.

The discussion here is therefore limited to the results for young patients, for whom the year coefficients are relevant.

Table 8 presents estimates of δ and ψ by hospital ownership type, financial status, and region. Figure 2 plots the $\hat{\delta}$ from these specifications. The main conclusion is that for-profit hospitals upcoded more than government or not-for-profit facilities following the 1988 reform. Consistent with the incentive provided to for-profit managers to *globally* code more patients with complications, the heightened for-profit response is manifested in the time-series increase in fraction_{dt}, *not* in the spread coefficient. Figure 2 illustrates that upcoding trends were the same for all three ownership types until 1987, but thereafter the trend for for-profits diverges substantially. By 1991, the fraction of young patients with complications had risen by .17 in for-profit hospitals, compared with ~.12 for the other two groups. Given a universal mean of .65 in 1987, these figures are extremely large.

Hospitals with high debt:asset ratios and hospitals in the South also exhibited very large increases in fraction, although these trends pre-date the policy change. Moreover, the strong presence of for-profits in the South and the tendency of for-profits to be highly-leveraged suggests that for-profit ownership is driving the large fraction gains in these subsamples as well.

HCFA's decision to increase the difference between the weights for complicated and uncomplicated patients with the same diagnosis unleashed a substantial upcoding response. I estimate that the 1988 reform alone increased the average price for affected patients (that is, patients in DRG pairs) by approximately one percent. These estimates come from an especially robust and comprehensive empirical investigation; I study not only the time-series response to an unanticipated policy reform, but also differential responses across 93 DRG pairs.

8 Conclusion

As public and private healthcare insurers continue to strengthen financial incentives for efficiency in the production of healthcare, it is critical to understand what the implications of such incentives are for health care quality and expenditures. The fixed-price system used by many insurers makes hospitals the residual claimants of profits earned on inpatient stays. These profits differ by DRG, creating incentives for hospitals to increase the volume of admissions in profitable DRGs relative to unprofitable DRGs. If hospitals respond to these incentives, we may see them encouraging certain types of admissions and discouraging others, a practice that could be innocuous in other industries (e.g. utilities) but is disconcerting in this setting.

Resolving the question of how hospitals respond to changes in DRG prices, which are simply shocks to the profitability of DRGs, is therefore critical from a policy standpoint. In addition, these responses provide a window into industry conduct. In theory, quality erosion is kept in check by interhospital competition.[36] Responses to individual price changes can reveal whether this competition occurs at the level of the DRG, or product line.

This paper illustrates how a simple change in the DRG classification system in 1988 generated large and exogenous relative price increases for 40 percent of DRG codes, accounting for 43 percent of admissions. Using a differences-in-differences identification strategy, I find that hospitals did not increase the volume or intensity of care in these DRGs relative to others. This non-response is extremely robust, persisting across various hospital subsamples as well as admission types (e.g. elective admissions, for which volume should theoretically be more responsive to hospital efforts). I find evidence that hospitals spent the extra funds they received on patient care in *all* DRGs, suggesting that

[36] Of course, physicians also play an important role in ensuring appropriate care for their patients, as highlighted by Arrow (1963).

they do not optimize intensity choices by product line, and may compete instead in overall quality levels.[37]

Although admissions and treatment policies proved resistant to financial incentives, coding behavior did not. The 1988 price shock strengthened the incentive for hospitals to code complications on patient records, and they responded by doing so. In addition, hospitals proved quite sophisticated in their upcoding strategies, coding more complications in those diagnoses where the reward for doing so increased more. Finally, while all subsamples of hospitals upcoded more following the policy change, for-profit facilities availed themselves of this opportunity to the greatest extent.

To summarize, I find that hospitals respond nominally to price changes by upcoding patients to more remunerative codes, but they do not alter intensity of care or boost admissions in these codes. The failure of hospitals to respond in real terms to product-line incentives suggests that quality competition does not occur at the DRG-level. This finding may help to explain the relative lack of specialization in the hospital industry. One anticipated benefit of PPS was that hospitals would specialize in admissions in which they were relatively cost-efficient. If, however, hospitals do not balance costs and benefits within individual product lines, such specialization is unlikely to occur. More generally, this research suggests that better models of hospital behavior are necessary for anticipating the impacts of public and private-sector actions in this important industry.

[37] Previous studies have also found a positive relationship between *overall* hospital intensity and financial pressure; see footnote 7.

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Table 1. Examples of Policy Change

DRG code	Description in 1987 (Description in 1988)	1987 weight	1988 weight	% change in weight	1987 volume (20% sample)	1988 volume (20% sample)	% change in volume
96	bronchitis and asthma age>69 and/or CC (bronchitis and asthma age>17 with CC)	0.8446	0.9804	16%	44,989	42,314	-6%
97	bronchitis and asthma age 18-69 without CC (bronchitis and asthma age>17 without CC)	0.7091	0.7151	1%	4,611	10,512	128%
138	cardiac arrhythmia and conduction disorders age>69 and/or CC (cardiac arrhythmia and conduction disorders with CC)	0.8136	0.8535	5%	45,080	35,233	-22%
139	cardiac arrhythmia and conduction disorders age<70 without CC (cardiac arrhythmia and conduction disorders without CC)	0.6514	0.5912	-9%	4,182	16,829	302%
296	nutritional and misc. metabolic disorders age>69 and/or CC (nutritional and misc. metabolic disorders age>17 with CC)	0.8271	0.9259	12%	45,903	38,805	-15%
297	nutritional and misc. metabolic disorders age 18-69 without CC (nutritional and misc. metabolic disorders age>17 without CC)	0.6984	0.5791	-17%	2,033	12,363	508%

Notes: Of the 95 affected pairs, these three occur most frequently in the 1987 20% MEDPAR sample.
Sources: Federal Register 51 FR 31454, 52 FR 33034; MEDPAR 20% sample for 1987

Table 2. Descriptive Statistics

Unit of Observation	DRG-year			Hospital-year			DRG-hospital-year		
Used in Tables		3,4,7			5,6			8	
Used to estimate equations		1,2,6			4,5			7	
	N	mean	std devn	N	mean	std devn	N	mean	std devn
weight	2482	1.26	(.91)	36651	1.27	(.19)	814262	1.13	(.64)
year	2482	.14	(.35)	36651	.14	(.35)	814262	.14	(.35)
post	2482	.59	(.49)	36651	.58	(.49)	814262	.57	(.50)
observations per cell	2482	6128	(12817)	36651	373	(389)	814262	2.22	(2.24)
spread	2482	.37	(.33)				814262	.37	(.34)
Instruments									
affected	2482	.45	(.50)				814262	1.00	(.00)
affected*post	2482	.26	(.44)						
share CC				36651	.09	(.03)			
share CC*post				36651	.05	(.05)			
1988 spread-1987 spread	2482	.20	(.16)				814262	.20	(.16)
(1988 spread-1987 spread)*post	2482	.12	(.16)				814262	.12	(.16)
Real Responses									
mean cost	2474	6000	(4889)	36169	6450	(3005)			
mean LOS	2482	10.64	(5.64)	36651	8.81	(2.21)			
mean surgeries	2450	1.15	(.73)	35897	1.21	(.55)			
mean ICU days	2290	.72	(1.18)	28226	.76	(.61)			
death rate	2123	.07	(.10)	34992	.06	(.02)			
mean volume	2482	32921	(30981)	36651	778	(538)			
Nominal responses									
fraction(young) w/CC	650	.68	(.14)				814262	.68	(.35)
fraction(old) w/CC	650	.74	(.11)						

Notes: DRG-hospital-year sample is for DRG pairs only; fraction variables are calculated for DRG pairs only. Means are weighted by the number of observations in the 20 percent MEDPAR sample, with the exception of observations per cell

Table 3. Effects of Policy Change on DRG Prices

	Dependent variable is ln(price) (N = 2,482, mean(price) = 1.26)			
Affected*post	.071 ***		.066 ***	.065 ***
	(.012)		(.016)	(.013)
*Affected*year dummies*				
1986		.014		
		(.028)		
1987		.015		
		(.028)		
1988		.079 **		
		(.027)		
1989		.086 **		
		(.028)		
1990		.084 **		
		(.028)		
1991		.076 **		
		(.029)		
Year dummies				
1986	-.017	-.024	-.018	-.009
	(.014)	(.020)	(.015)	(.012)
1987	-.017	-.024	-.018	.000
	(.014)	(.020)	(.015)	(.019)
1988	-.049 ***	-.053 **	-.049 ***	-.019
	(.015)	(.020)	(.015)	(.027)
1989	-.045 **	-.051 *	-.045 **	-.005
	(.016)	(.021)	(.016)	(.035)
1990	-.055 ***	-.061 **	-.057 ***	-.006
	(.016)	(.021)	(.017)	(.044)
1991	-.061 ***	-.063 **	-.062 ***	-.001
	(.016)	(.021)	(.018)	(.053)
DRG fixed effects	Y	Y	Y	Y
Affected trend	N	N	Y	N
DRG trends	N	N	N	Y
Adj. R-squared	.977	.977	.990	.990

Notes: The unit of observation for this analysis is DRG-year (where "DRG" refers to single DRGs as well as to DRG pairs). The dependent variable is the log of the DRG weight, labeled "price." All observations are weighted by the number of admissions recorded in the 20% MEDPAR sample. The sum of the weights is 15.2 million. Standard errors are robust.

* signifies p<.05, ** signifies p<.01, *** signifies p<.001

Table 4A. Effects of Policy Change on DRG Intensity of Care and Volume

	Dependent variable is					
	ln(cost)	ln(LOS)	ln(surg)	ln(ICU)	ln(death	ln(volume)
Affected*post	.007	.013	-.005	-.022	-.018	.040
	(.011)	(.017)	(.016)	(.034)	(.038)	(.024)
Year fixed effects	Y	Y	Y	Y	Y	Y
DRG fixed effects	Y	Y	Y	Y	Y	Y
Affected trend	N	N	N	N	N	N
DRG trends	Y	Y	Y	Y	Y	Y
Adj. R-squared	.993	.986	.994	.985	.989	.994
N	2474	2482	2450	2290	2123	2482

Table 4B. IV Estimates of Price Elasticities of DRG Intensity and Volume

	Dependent variable is					
	ln(cost)	ln(LOS)	ln(surg)	ln(ICU)	ln(death	ln(volume)
ln(price)	.111	.197	-.079	-.336	-.270	.620
	(.173)	(.261)	(.251)	(.532)	(.589)	(.409)
Parametric Tests of H_0: ln(price)=x; H_1: ln(price)<x (p-values are reported)						
x = .5	.01	.12	.01	.06	.35	.62
x = 1	.00	.00	.00	.01	.11	.18
Year fixed effects	Y	Y	Y	Y	Y	Y
DRG fixed effects	Y	Y	Y	Y	Y	Y
Affected trend	N	N	N	N	N	N
DRG trends	Y	Y	Y	Y	Y	Y

Table 4B. OLS Estimates of Price Elasticities of DRG Intensity and Volume

	Dependent variable is					
	ln(cost)	ln(LOS)	ln(surg)	ln(ICU)	ln(death	ln(volume)
ln(price)	.127 ***	.182 ***	-.029	.253 **	.258 *	-.048
	(.037)	(.043)	(.047)	(.089)	(.115)	(.092)
Year fixed effects	Y	Y	Y	Y	Y	Y
DRG fixed effects	Y	Y	Y	Y	Y	Y
Affected trend	N	N	N	N	N	N
DRG trends	Y	Y	Y	Y	Y	Y
Adj. R-squared	.995	.990	.996	.989	.992	.996

Notes: For ln(death rate), the p-values presented are for x=-.5 and x=-1. The unit of observation for this analysis is DRG-year (where "DRG" refers to single DRGs as well as to DRG pairs). All observations are weighted by the number of admissions recorded in the 20% MEDPAR sample. The sum of the weights is 15.2 million. Standard errors are robust.

* signifies p<.05, ** signifies p<.01, *** signifies p<.001

Table 5. Effects of Policy Change on Average Hospital Prices

	Dependent variable is ln(price) (N = 36,651, mean(price) = 1.27)	
Share CC•post	.233 ***	
	(.021)	
*Share CC*year dummies*		
1986		-.022
		(.040)
1987		-.015
		(.038)
1988		.229 ***
		(.038)
1989		.212 ***
		(.039)
1990		.174 ***
		(.040)
1991		.270 ***
		(.047)
Year dummies		
1986	.039 ***	.041 ***
	(.001)	(.004)
1987	.057 ***	.058 ***
	(.001)	(.004)
1988	.063 ***	.064 ***
	(.002)	(.004)
1989	.088 ***	.090 ***
	(.002)	(.004)
1990	.094 ***	.099 ***
	(.002)	(.004)
1991	.119 ***	.116 ***
	(.002)	(.004)
Hospital fixed effects	Y	Y
Adj. R-squared	.890	.890

Notes: The unit of observation for this analysis is hospital-year. The dependent variable is the log of a hospital's average DRG weight, labeled "price." Share CC•post = (1987 share of a hospital's Medicare patients that are under 70 and assigned to the top code of a DRG pair)•indicator variable for year>1987. All observations are weighted by the number of admissions in the 20 percent MEDPAR sample. The sum of the weights is 13.7 million. Standard errors are robust.

* signifies p<.05, ** signifies p<.01, *** signifies p<.001

Table 6A. Effects of Policy Change on Hospital Intensity and Volume

	Dependent variable is					
	ln(cost)	ln(LOS)	ln(surg)	ln(ICU)	ln(death rate)	ln(volume)
Share CC•post	.234 ***	.069 *	.067	.684 ***	.122	.403 ***
	(.064)	(.030)	(.092)	(.171)	(.088)	(.046)
Year fixed effects	Y	Y	Y	Y	Y	Y
Hospital fixed effects	Y	Y	Y	Y	Y	Y
Adj. R-squared	.796	.883	.809	.718	.483	.976
N	36169	36651	35897	28226	34992	36651

Table 6B. IV Estimates of Price Elasticities of Hospital Intensity and Volume

	Dependent variable is					
	ln(cost)	ln(LOS)	ln(surg)	ln(ICU)	ln(death rate)	ln(volume)
ln(price)	.998 ***	.296 *	.291	3.457 ***	.536	1.728 ***
	(.288)	(.130)	(.444)	(.971)	(.424)	(.276)
Year fixed effects	Y	Y	Y	Y	Y	Y
Hospital fixed effects	Y	Y	Y	Y	Y	Y

Table 6C. OLS Estimates of Price Elasticities of Hospital Intensity and Volume

	Dependent variable is					
	ln(cost)	ln(LOS)	ln(surg)	ln(ICU)	ln(death rate)	ln(volume)
ln(price)	.769 ***	.350 ***	.867 ***	1.483 ***	.601 ***	-.022
	(.027)	(.011)	(.036)	(.065)	(.031)	(.018)
Year fixed effects	Y	Y	Y	Y	Y	Y
Hospital fixed effects	Y	Y	Y	Y	Y	Y
Adj. R-squared	.805	.888	.815	.727	.568	.976

Notes: For ln(death rate), the p-values presented are for H0: ln(price) <-.5 and H0: ln(price) <-1. The unit of observation for this analysis is hospital-year. Share CC•post = (1987 share of a hospital's Medicare patients that are under 70 and assigned to the top code of a DRG pair)•indicator variable for year>1987. All observations are weighted by the number of admissions in the 20 percent MEDPAR sample. The sum of the weights is 13.7 million.

* signifies p<.05, ** signifies p<.01, *** signifies p<.001

Table 7. Effects of Policy Change on Upcoding

Young (patients < 70)

Dependent variable is fraction(young) (N = 650, mean(fraction)=.68)		
	OLS	IV
spread	.038 **	.056 ***
	(.014)	(.013)
Year dummies		
1986	.039 ***	.036 ***
	(.009)	(.009)
1987	.072 ***	.070 ***
	(.008)	(.008)
1988	.061 ***	.054 ***
	(.011)	(.011)
1989	.098 ***	.091 ***
	(.010)	(.010)
1990	.117 ***	.109 ***
	(.010)	(.010)
1991	.129 ***	.122 ***
	(.011)	(.011)
DRG dummies	Y	Y
Adj. R-squared	.955	

Old (patients 70+)

Dependent variable is fraction of old in top code (N = 650, mean(fraction) = .85)		
	OLS	IV
spread	.115 ***	.114 ***
	(.022)	(.022)
Year dummies		
1986	-.017	-.017
	(.015)	(.015)
1987	-.028	-.028
	(.015)	(.015)
1988	-.340 ***	-.340 ***
	(.015)	(.016)
1989	-.309 ***	-.309 ***
	(.016)	(.017)
1990	-.294 ***	-.294 ***
	(.017)	(.018)
1991	-.279 ***	-.279 ***
	(.017)	(.018)
DRG dummies	Y	Y
Adj. R-squared	.861	

Notes: The unit of observation for this analysis is DRG-year (where only DRG pairs are included). The instrument for spread is (spread88-spread87)d·postt. All observations are weighted by the number of admissions recorded in the 20% MEDPAR sample. The sum of the weights is 1.96 million (young) and 4.96 million (old). Standard errors are robust.

* signifies p<.05, ** signifies p<.01, *** signifies p<.001

Table 8. Effects of Policy Change on Upcoding of Young, by Hospital Characteristics

By Ownership

	For-profit	Not-for-profit	Government	$\hat{\beta}_{FP}-\hat{\beta}_{NFP}$	t	$\hat{\beta}_{GOV}-\hat{\beta}_{NFP}$	t
spread	.052 **	.058 ***	.043 ***	-.006	-.279	-.015	-.809
	(.017)	(.013)	(.013)	(.022)		(.019)	
Year fixed effects							
1986	.031 ***	.038 ***	.034 ***	-.007	-.576	-.005	-.411
	(.009)	(.008)	(.008)	(.012)		(.011)	
1987	.075 ***	.068 ***	.070 ***	.006	.522	.002	.172
	(.009)	(.008)	(.007)	(.012)		(.011)	
1988	.077 ***	.051 ***	.060 ***	.026	1.646	.009	.572
	(.012)	(.010)	(.012)	(.016)		(.016)	
1989	.136 ***	.088 ***	.098 ***	.048	3.322	.010	.792
	(.011)	(.009)	(.009)	(.014)		(.013)	
1990	.142 ***	.107 ***	.114 ***	.035	2.397	.007	.538
	(.011)	(.009)	(.010)	(.015)		(.013)	
1991	.174 ***	.116 ***	.128 ***	.057	3.633	.012	.832
	(.012)	(.011)	(.010)	(.016)		(.014)	
DRG fixed effects	Y	Y	Y				
Hospital characteristic	Y	Y	Y				
N	100,773	561,211	152,278				
mean (fraction)	.69	.68	.66				
	(.36)	(.36)	(.35)				
Adj. R-squared	.155	.174	.141				

By Financial State, Distressed = Debt: Asset Ratio > 75th percentile

	Distressed	Not distressed	$\hat{\beta}_{DIS}-\hat{\beta}_{NOT}$	t
spread	.060 ***	.053 ***	.007	.389
	(.014)	(.012)	(.019)	
Year fixed effects				
1986	.051 ***	.033 ***	.018	1.559
	(.008)	(.008)	(.011)	
1987	.091 ***	.064 ***	.027	2.424
	(.008)	(.008)	(.011)	
1988	.079 ***	.050 ***	.029	1.971
	(.010)	(.011)	(.015)	
1989	.121 ***	.088 ***	.032	2.494
	(.009)	(.009)	(.013)	
1990	.137 ***	.105 ***	.032	2.407
	(.009)	(.010)	(.013)	
1991	.154 ***	.117 ***	.036	2.458
	(.010)	(.010)	(.015)	
DRG fixed effects	Y	Y		
Hospital characteristic	Y	Y		
N	174,243	640,119		
mean (fraction)	.67	.66		
	(.36)	(.35)		
Adj. R-squared	.149	.169		

Table 8, continued. Hospital Analysis: Effects of Policy Change on Upcoding

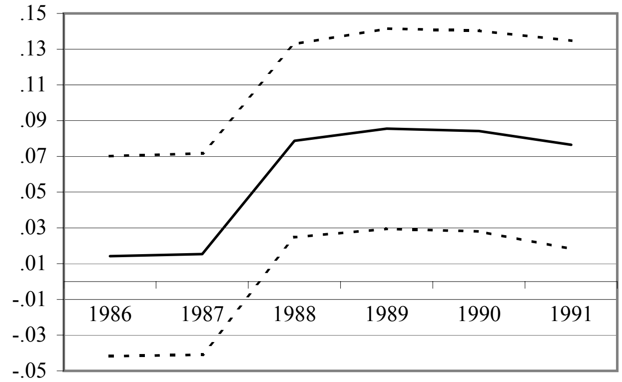
By Region

	Northeast	Midwest	South	West	$\hat{\beta}_S - \hat{\beta}_W$	t
spread	.061 ***	.045 ***	.060 ***	.058 ***	.002	.103
	(.011)	(.013)	(.013)	(.017)	(.021)	
Year fixed effects						
1986	.086 ***	.020 ***	.025 ***	.027 ***	-.002	-.142
	(.008)	(.008)	(.008)	(.011)	(.014)	
1987	.115 ***	.047 ***	.069 ***	.044 ***	.026	1.936
	(.007)	(.008)	(.008)	(.011)	(.013)	
1988	.100 ***	.032 ***	.059 ***	.020 ***	.038	2.161
	(.009)	(.010)	(.011)	(.014)	(.018)	
1989	.125 ***	.070 ***	.105 ***	.061 ***	.044	2.878
	(.009)	(.009)	(.009)	(.012)	(.015)	
1990	.141 ***	.086 ***	.126 ***	.073 ***	.052	3.254
	(.008)	(.009)	(.010)	(.013)	(.016)	
1991	.152 ***	.097 ***	.141 ***	.085 ***	.056	3.294
	(.010)	(.011)	(.010)	(.014)	(.017)	
DRG fixed effects	Y	Y	Y	Y		
Hospital characteristic	Y	Y	Y	Y		
N	162,185	208,803	314,272	129,002		
mean (fraction)	.65	.66	0.67	0.68		
	(.35)	(.35)	(.34)	(.36)		
Adj. R-squared	.162	.164	.167	.180		

Notes: The unit of observation for this analysis is hospital-DRG-year, who only DRG pairs are included. Instrument for spread is $(\text{spread88-spread87})_d \cdot \text{post}_t$. Hospitals with missing values for any of the covariates are dropped. All observations are weighted by the number of admissions recorded in the 20% MEDPAR sample. The sum of the weights is 1.45 million. Standard errors are corrected for heteroskedasticity and correlation within drg-year clusters.

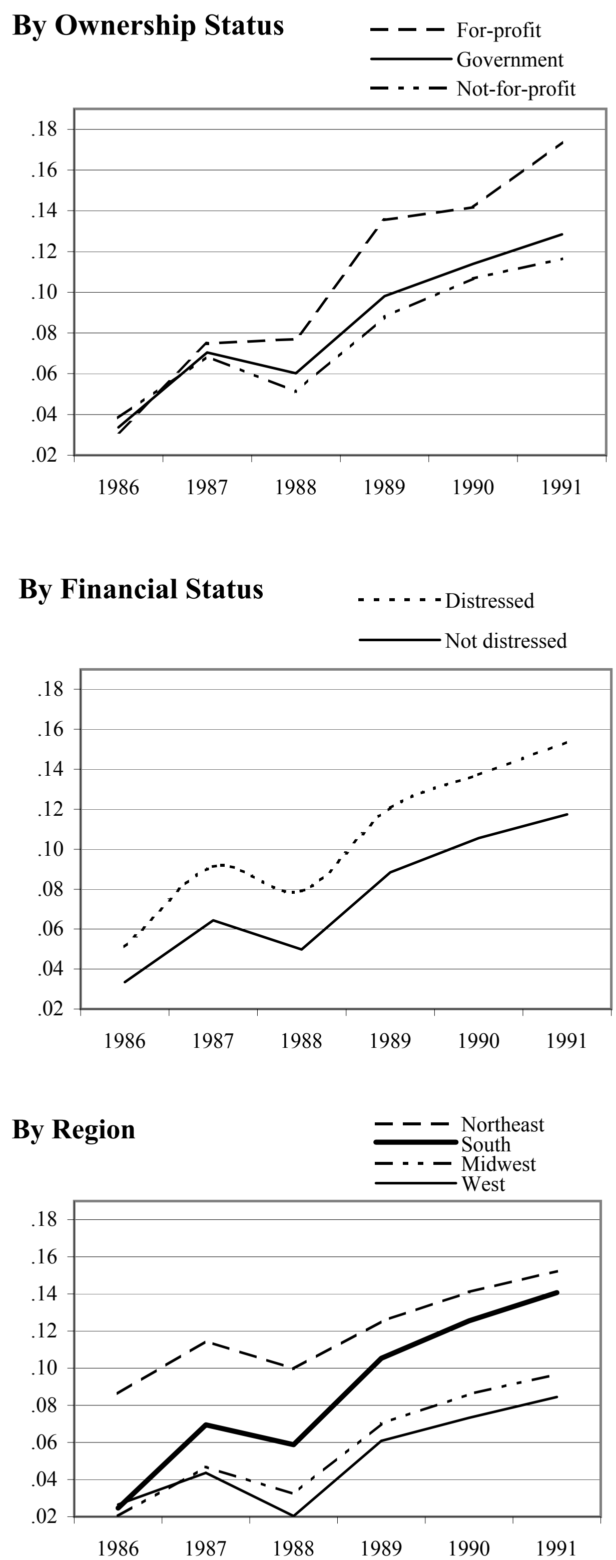
* signifies p<.05, ** signifies p<.01, *** signifies p<.001

Figure 1. Effects of Policy Change on DRG Price, By Year



Source: Affected·year coefficients and 95% confidence intervals from Table 3, column 2.

Figure 2. Effects of Policy Change on Upcoding of Young, by Hospital Characteristics



Source: Year coefficients from Table 8.

Appendix Table 1. Descriptive Statistics for Hospital Characteristics

Variable	Mean	Std. Deviation	Min	Max
Ownership				
For-profit	0.14	0.35	0	1
Non-profit	0.58	0.49	0	1
Government	0.28	0.45	0	1
Financial Distress Measures				
Debt:asset ratio	.52	.32	0	2.17
Medicare bite	.37	.11	0	1.00
Medicaid bite	.11	.08	0	.84
Region				
Northeast	.14	.35	0	1
Midwest	.29	.46	0	1
South	.38	.49	0	1
West	.18	.38	0	1
Size				
1-99 beds	.46	.50	0	1
100-299 beds	.37	.48	0	1
300+ beds	.17	.37	0	1
Service Offerings				
Teaching program	.06	.23	0	1
Open heart surgery	.13	.34	0	1
Trauma facility	.19	.39	0	1
ICU beds (except neonatal)	10.27	12.29	0	194
HSA Herfindahl	.07	.05	0.016	1

Notes: N=5336. Data pertains to hospitals included in the hospital-year and hospital-year-DRG samples. Excludes hospitals with missing values for any of the variables, or with debt:asset ratios in the 1% tails of the distribution.

Sources: HCFA Cost Reports (1987), American Hospital Association Annual Survey of Hospitals (1987)