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Detecting Medicare abuse

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Abstract

This paper identifies which types of patients and hospitals have abusive Medicare billings that are responsive to law enforcement. For a 20% random sample of elderly Medicare beneficiaries hospitalized from 1994 to 1998 with one or more of six illnesses that are prone to abuse, we obtain longitudinal claims data linked with social security death records, hospital characteristics, and state/year-level anti-fraud enforcement efforts. We show that increased enforcement leads certain types of types of patients and hospitals to have lower billings, without adverse consequences for patients' health outcomes.

JEL classification: I1; K0

Keywords: Medicare abuse; Anti-fraud enforcement; Medicare beneficiaries

1. Introduction

Anecdotal evidence suggests that there is significant fraud and abuse in the Medicare and Medicaid programs. Asymmetries of information about the health conditions of and the medical treatment received by beneficiaries, combined with the programs' interest in offering wide access to patients and prompt compensation to providers, make health insurance claims reimbursement a breeding ground for illicit activity. Sparrow (2000) catalogues an

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extensive list of such behavior, including billing for services never provided; billing individually for services that should have been provided as part of a bundle warranting a single, lower payment; "upcoding" marginal patients to having had a related but more complex illness or treatment in order to obtain higher reimbursement; and providing services that were not medically necessary. Although there are no agreed-upon estimates of the magnitude of the problem,[1] the US Department of Health and Human Services (US HHS) has estimated "improper" Medicare fee-for-service payments at approximately $12–$23 billion, or approximately 7–14% of all reimbursements.

There is also anecdotal evidence that health care fraud and abuse are responsive to enforcement efforts. For example, hospitals reported fewer cases of "complex" versus "simple" pneumonia cases among Medicare beneficiaries in 1998 than they did in 1997 and 1996, where complex cases receive a higher reimbursement (Birch, 2000). This occurred contemporaneously with the 1997 investigation by the New York Times and the US Government into Columbia/HCA's coding practices for this illness, and the release of Government guidelines for hospital coding compliance programs.

Yet, despite these facts, little research has used observational data to investigate how the cost and quality of medical care responds to enforcement. Most existing work uses audit data to identify the aggregate amount of abuse, or the characteristics of hospitals or patients associated with abuse. This omission is important for several reasons. First, the existing literature's focus on audit data, which are costly to obtain (both in financial and political terms),[2] has severely limited its scope; methods that can guide enforcement based on observational data only offer an important practical advantage. Second, from a policy perspective, optimal targeting of enforcement requires estimates of the effects on cost and quality of the *interaction* between enforcement and patient or hospital characteristics, not estimates of the correlation between characteristics and the *level* of abusive billings. Third, from the perspective of economic theory, estimates of how enforcement affects the care for different types of patients, and the care supplied at different types of organizations, are at least as important as estimates of the effect of patient and provider characteristics on the level of fraud and abuse. For example, if managers of nonprofit hospitals value their reputation for supplying "socially responsible" medical care more than do managers of for-profit hospitals, then the response to enforcement of fraud and abuse at nonprofits could be greater than the response at for-profits, regardless of the overall level of fraud and abuse at nonprofits versus for-profits.

This paper seeks to fill this gap. We match longitudinal data on the health expenditures, days in the hospital, and outcomes of a 20% random sample of Medicare beneficiaries from 1994 to 1998 with illnesses that are particularly prone to fraud and abuse; data on the characteristics of all U.S. general acute care hospitals; and data on states' Medicaid anti-fraud enforcement expenditures. We estimate the effects of enforcement on levels of and differences between types of patients and hospitals in treatment intensity and health outcomes. We conclude that increased enforcement leads to declines in abusive treatment either if expenditures decline in response to enforcement, with no accompanying increase in

[1] See, e.g., GAO (2000a) which makes this point.

[2] See, for example, the discussion in GAO (1996) of the effect of political pressure from Congress and interest groups on IRS's decision to postpone its intensive random Taxpayer Compliance Measurement Program audits.

the rate of adverse health outcomes, or if the rate of adverse outcomes declines in response to enforcement, with no accompanying increase in expenditures.[3]

We seek to distinguish the effects of enforcement on purely financial behavior from its effects on medical treatment decisions, because the latter may have far greater social costs. Financial misconduct involves only a transfer from the government to private parties (and therefore a deadweight loss equal to the cost of public funds), whereas enforcement-induced changes in treatment involve the reallocation of real resources. Furthermore, enforcement-induced declines in the intensity of medical care that have minimal outcome benefits save patients the substantial nonfinancial costs of invasive treatment, and enforcement-induced improvements in the quality of care provide patients with longer lives and better health. To distinguish between these two effects, we first estimate the effects of enforcement on health outcomes such as mortality and readmission with complications. Then, we estimate the effect of enforcement on the total days in the acute care hospital, to provide at least a coarse nonfinancial measure of the real resources used to treat a given episode of illness.

Our analysis has four important limitations that we discuss in detail below. First, because we do not observe whether services were actually provided, we cannot distinguish between activity meeting the legal definition of fraud (which may or may not include the actual delivery of health care) and that meeting the legal definition of abuse (which generally involves the delivery of unnecessary care). For convenience, we describe our work as testing for abusive behavior. Second, we assume that Medicaid enforcement affects the extent of Medicare abuse, based on the administrative overlap between the agencies responsible for policing the Medicaid and Medicare programs. Third, we classify treatment behavior as abusive if it has no significant measured consequences for patient health. If we neglect to measure important health benefits of a given form of treatment—for example, the rapidity and/or completeness of recovery attributable to rehabilitation in a skilled nursing facility—then we may classify hospitals that supply such treatment as abusive even though they are providing valuable services to their patients.

Fourth, we make two assumptions in order to ensure that our estimates of the responsiveness of billings to enforcement are lower bounds for the true effects. First, we assume that states with high levels of abuse invest at least as much in enforcement as do states with low levels of abuse, and that states target enforcement towards those types of patients or hospitals with more severe abuse problems. This enables us to sign any bias due to the endogeneity of enforcement. Second, we assume that unobservably sicker patients are at least as likely to choose high-billing types of hospitals in high enforcement states versus low enforcement states. This enables us to sign any bias due to unobserved patient heterogeneity across hospitals.

This paper proceeds as follows. Section 2 provides some background on corruption, fraud, and abuse and a review of the empirical literature. Section 3 presents our models.

[3] In models of the effects of enforcement on differences between types of patients and hospitals, we conclude that increased enforcement leads to greater declines in abusive treatment for a given type of patient or hospital either if expenditures for that type of patient or hospital decline by more in response to enforcement than do the expenditures of the complementary type, with no accompanying differential increase in the rate of adverse health outcomes, or if the rate of adverse outcomes for that type of patient or hospital declines by more in response to enforcement than does the rate of the complementary type, with no accompanying differential increase in expenditures.

This section describes how we identify the effects of fraud control expenditures and hospital and patient characteristics on upcoding, treatment intensity and health outcomes. Section 4 discusses our data sources. Section 5 presents our results and Section 6 concludes by discussing the implications of our findings.

2. Corruption, fraud, and abuse: background

Commentators have long acknowledged theoretically that corruption can have substantial effects on social welfare (e.g., Rose-Ackerman, 1975; Shleifer and Vishny, 1993). Existing empirical work on the effects of corruption (reviewed in Duggan and Levitt, 2002) spans a wide range of topics, including studies of tax compliance and tax evasion,[4] of the extent and determinants of bid rigging in public contracting (e.g., Porter and Zona, 1993), of the effects of government influence-peddling on firms' stock prices and macroeconomic outcomes (e.g., Fisman, 2001, Mauro, 1995), and of teacher cheating on standardized testing of secondary school students (Jacob and Levitt, 2001).

But despite the importance of corruption in health care, due to the prevalence of third-party payment systems and the sheer size of the sector, little empirical research has investigated how enforcement affects the cost and quality of medical care. Most of the investigation into the extent and determinants of health care fraud and abuse has been based on audit studies done by the Office of the Inspector General of US HHS (HHS OIG). One arm of this work, discussed in Section 1, estimates the magnitude of improper Medicare fee-for-service payments (HHS OIG 2001). In these studies, medical review personnel from the Health Care Financing Administration's (HCFA)[5] Medicare contractors compare all of the claims from a stratified random cross-section of 600 beneficiaries to the beneficiaries' medical records. The studies conclude that improper payments—which include payments for noncovered services, services billed without documentation, coding errors, and medically unnecessary services—ranged from \$23.2 billion in 1996 to \$11.9 billion in 2000, or 14%–6.8% of Medicare fee-for-service payments. As HHS and many analysts have observed, these estimates may be either an overstatement or an understatement of the total amount of fraud and abuse. The total volume of improper payments likely includes waste and errors that are neither fraudulent nor abusive; and, because the estimates are obtained by comparing bills submitted to HCFA with retrospective review of patient records, they exclude any fraudulent or abusive billings substantiated by medical records, even if the medical records were themselves inaccurate or deceptive.[6] Psaty et al. (1999) extend the HHS OIG approach by

[4] Dubin et al. (1990) and Engel et al. (2001) estimate the correlation between enforcement efforts and tax revenues to identify the magnitude of tax evasion; Andreoni et al. (1998) provide a comprehensive review of this literature.

[5] Now called the Centers for Medicare and Medicaid Services (CMS).

[6] However, the HHS OIG estimates likely understate the total amount of fraud and abuse. The proportionately low levels of expenditures to investigate fraud and abuse are consistent with high equilibrium levels of such behavior. Medicare "benefit integrity" funding, which finances fraud control units and the handling of beneficiary complaints of fraud, was just \$78 million in FY2000, or 0.03% of total program spending. Similarly, spending on Medicaid fraud Control Units amounted to approximately 0.07% of total Medicaid spending in FY2000 (Sparrow, 2000, p. 73).

comparing Medicare bills to information collected from both medical records and patient interviews. Based on this information, they conclude that 37.5% of all heart failure cases reflect incorrect diagnoses, so that fraudulent and abusive coding of heart failure alone cost Medicare as much as $933 million in 1993.

A second set of audit studies seeks to identify the determinants of fraud and abuse, in particular to identify observable characteristics of providers or claims associated with fraudulent or abusive behavior. HHS OIG (1998c) compares audits of a sample of claims from 50 hospitals considered to be likely abusers with audits of claims from a random sample of 20 hospitals. It reports both the 10 diagnosis-related groups (DRGs) with the highest rates of upcoding—classification of marginal patients as having had a related but more complex condition or treatment than was actually the case to obtain greater reimbursement—and the characteristics of hospitals and patient populations associated with upcoding. Swedlow et al. (1992) apply a similar methodology to calculate rates of inappropriate use of MRI among physicians who self-refer for imaging versus physicians who independently refer.

A handful of studies seek to use observational data to identify types of hospitals or physicians that behave abusively. Hillman et al. (1990) and GAO (1994) show that physicians who have a financial interest in imaging facilities are more likely to order imaging services than physicians who do not. HHS OIG (1998b; 1999a,b,c) explain how to identify hospitals with atypically high rates of billing for certain high-reimbursement DRGs that are thought to be prone to upcoding. Silverman and Skinner (2001) extend this approach, investigating whether for-profit hospitals and the area density of for-profit hospitals are correlated with upcoding of patients with certain illnesses. They find that for-profit hospitals are more likely to engage in upcoding behavior than most nonprofit hospitals, but that the upcoding behavior of nonprofits more closely resembles the behavior of for-profits in markets in which for-profits account for a high share of patient discharges.

Previous empirical research shows the tradeoffs between existing uses of audit-based and observational data. By construction, the audit studies provide consistent estimates of the magnitude of and types of claims and providers associated with improper billing. But because of the high cost of medical record reviews, the audit studies' small samples limit the power with which they can identify the types of patients, illnesses, and providers prone to abuse. Studies using observational data, on the other hand, use very large samples to identify precisely differences in billing behavior by individual hospital or hospital type. But because observational data contains information only on total billings (or some variant thereof, such as the patient's probability of being coded into a high-reimbursement DRG), reported correlations between patient or provider characteristics and billings could be due to differences in fraud or abuse, or to differences in valid billings across provider types. Distinguishing fraud or abuse from valid differences in billings across providers requires the additional assumption that there are no unobserved differences across providers in their characteristics or the characteristics of their patients that affect valid billings—an assumption that is likely to be incorrect (e.g., Kessler and McClellan, 2002). And, existing studies using observational data do not investigate the consequences of allegedly abusive treatment for patient health outcomes; without information on outcomes, classification of a pattern of treatment as abusive is necessarily speculative. Finally, these studies do not provide any evidence on a key policy question of interest—how different types of doctors or hospitals would respond to increased enforcement.

3. Empirical models

We examine the impact of Medicaid fraud control expenditures that are determined at the state level, individual patient characteristics, and the characteristics of hospital of initial admission on upcoding, hospital expenditures, length of stay, and health outcomes using longitudinal data on cohorts of elderly Medicare recipients who were hospitalized with one or more of six types of illness between 1994 and 1998. We use variation in state-level Medicaid enforcement to identify the responsiveness of Medicare abuse to enforcement because of extensive administrative overlap between the agencies responsible for policing the Medicaid and Medicare programs (see HHS OIG, 2000). State Medicaid agencies are required to report all suspected incidences of provider fraud to their state's Medicaid Fraud Control Units (MFCUs) through a unified sub-system (Surveillance and Utilization Review Sub-system) of their Medicaid Management Information Systems.[7] The MFCUs have developed uniform procedures to coordinate their efforts with HHS OIG through the National Association of Medicaid Fraud Control Units (Jost and Davies, 2000, p. 369).

More importantly, HHS OIG formally oversees the state MFCUs as well as Medicare fraud enforcement efforts. HHS OIG's authority to conduct coordinated investigations of fraud and abuse across programs has been strengthened by laws passed both during and after our study period. The Health Insurance Portability and Accountability Act of 1996 established a National Health Care Fraud and Abuse Control Program under the joint direction of the Attorney General and the Secretary of HHS, acting through HHS OIG. And, The Ticket to Work and Work Incentives Improvement Act of 1999 allows state MFCUs in certain circumstances to investigate fraud in the Medicare program if the case is "primarily related to Medicaid".

In 3-digit zip-code k during year $t = 1, \ldots, T$, the observational units in our analysis of the effects of fraud control expenditures on upcoding, expenditures, length of stay, and outcomes consist of individuals $i = 1, \ldots, N_{kt}$ who are initially admitted to hospital j with a new occurrence of a given type of illness. Each patient has a vector of characteristics X_{it}: four age indicator variables (70–74 years, 75–79 years, 80–89 years, and 90–99 years; omitted group is 65–69 years), gender, and black/non-black race; a set of interaction effects between age, gender, and race; and an indicator variable denoting whether the patient had been admitted to the hospital in the 365 days prior to the onset of the study illness, to capture the overall status of the patient's health upon admission to the hospital. Each patient also has a vector of indicator variables Z_{jt} that describes the characteristics of their hospital of initial admission (e.g., for-profit ownership and size). In all model specifications we include year-fixed-effects that are allowed to vary by size of metropolitan area. We also include patient-diagnosis-group fixed effects that we allow to vary by patient-3-digit zip-code, to control for patients' initial health conditions and heterogeneity across geographic areas in treatment patterns. We group patients into 72 diagnosis groups based on their ICD-9 code on admission (in practice, the vast majority of patients with a given type of illness will be classified into a small subset of these groups). Finally, depending on their state of residence and year of admission, each patient is subject to a given level of enforcement, as

[7] MFCUs were created as part of the 1978 Medicare/Medicaid Anti-Fraud and Abuse Amendments.

measured by ln(Medicaid fraud control unit expenditures per general medical, nonfederal hospital) or ln(Medicaid fraud control unit expenditures per Medicare beneficiary), P_{kt}.

We measure four types of outcomes. The binary variable C_{it} is set equal to 1 if the individual is coded into the highest-paid DRG of the set of DRGs into which a patient with a particular illness could be coded, 0 otherwise. The individual incurs total Medicare bills of Y_{it} (where Y is total expenditures in the year after and including the admission to the hospital for the study illness) in connection with total days in the acute care hospital L_{it} (where L includes the all days in the hospital in the year after and including the day of initial hospitalization). We identify separately inpatient acute, inpatient nonacute (largely skilled nursing), outpatient, and home health/hospice utilization. The patient has health outcome O_{it}, where $O = 1$ denotes an adverse health outcome in the 365 days following the initial hospital admission, 0 otherwise.

We estimate linear versions of the model suggested by our theory, which are of the form:

$$\begin{array}{l} C_{it} \\ \ln(Y_{it}) \\ \ln(L_{it}) = \alpha_k D_{it} + \sigma_t M_k + X_{it}\phi + Z_{jt}\gamma + P_{kt}\beta + \varepsilon_{ikt} \\ O_{it} \end{array} \tag{1}$$

and

$$\begin{array}{l} C_{it} \\ \ln(Y_{it}) \\ \ln(L_{it}) = \alpha_k D_{it} + \sigma_t M_k + X_{it}\phi + Z_{jt}\gamma + P_{kt}\beta + X_{it}P_{kt}\delta + Z_{jt}P_{kt}\theta + \varepsilon_{ikt} \\ O_{it} \end{array} \tag{2}$$

where α_k is a 3-digit zip-code fixed-effect; D_{it} is a 71-dimensional vector of indicator variables denoting individual i's diagnosis; σ_t is a time fixed effect; M_k is a six-dimensional vector of indicator variables denoting the size of individual i's MSA (including one group for individuals who do not live in an MSA); and ε_{ikt} is an error term, where $E(\varepsilon_{ikt}|\ldots) = 0$.

The coefficients of interest are β, δ, and θ which reflect the responsiveness and the difference in responsiveness by patient and hospital type to enforcement of billings or health outcomes. We define hospitals with characteristic Z^1 as providing abusive treatment that is more responsive to enforcement if $\theta^1 < 0$ in models of Y and/or C, and $\theta^1 \leq 0$ in models of O—that is, billings of hospitals of type $Z^1 = 1$ compared to hospitals of type $Z^1 = 0$ are significantly more negatively responsive to state Medicaid fraud control expenditures, and the enforcement-induced reduction in treatment associated with that decrease in billings (if any) has no significant adverse consequences for patient health outcomes. Similar tests can be constructed to investigate whether certain types of patients receive abusive treatment that is more responsive to enforcement, and whether the quality of care improves by more at certain types of hospitals, with no accompanying increase in expenditures.

We make two assumptions in order to ensure that our estimates of the responsiveness of billings to enforcement are lower bounds (in absolute value) for the true effects. First, we assume that states with high levels of abuse invest at least as much in enforcement as do states with low levels of abuse, and that states target enforcement towards those types of patients or hospitals with more severe abuse problems. Suppose state s at time t had a high level of abuse, either among all hospitals or among a particular type of hospital (e.g., hospitals of type $Z^1 = 1$ submit more abusive bills than do hospitals of type $Z^1 = 0$). If this led state s to invest more in enforcement in aggregate—the most likely type of simultaneity bias—then estimates of β from Eq. (1) would be biased upward, i.e. downward in absolute value. Similarly, if hospitals' behavior led state s to target hospitals of type $Z^1 = 1$ because they were abusive, then estimates of θ^1 would be biased upward, i.e. downward in absolute value, for the same reason.

Second, we assume that unobservably sicker patients are at least as likely to choose high-billing types of hospitals in high enforcement states versus low enforcement states. If high levels of enforcement mitigate both adverse cost and adverse quality consequences of abuse, then unobservably sicker patients would be more willing to choose high-billing hospitals in high enforcement states, and estimates of θ^1 would be biased upward, i.e. downward in absolute value. But if high levels of enforcement lead to reductions in intensive treatment that patients value, and sicker patients value enforcement-induced reductions in intensive treatment more than do healthy patients, then unobservably sicker patients would be less willing to choose high-billing hospitals in high-enforcement states, and estimates of θ^1 would be biased downward, i.e. upward in absolute value.

4. Data

We examine the response of abusive behavior to enforcement in patients with a new occurrence of one or more of six illnesses identified by HCFA contractors (HHS OIG 1998c) as being particularly vulnerable to fraud and abuse: respiratory infections and pneumonia (DRG 79, 80, 89, 90); chronic obstructive pulmonary disease (COPD) and generalized respiratory disorders (DRG 87, 88, 96, 97, 99, 100, 101, 102); circulatory system disorders (DRG 132, 133, 138, 139, 140, 141, 142, 144, 145); kidney disorders/renal failure (DRG 326, 331, 332); diabetes and nutritional/metabolic disorders (DRG 182, 294, 296, 297); and cerebrovascular disorders/stroke (DRG 12, 14, 15, 16, 17). We define a patient as having had a new occurrence of illness in a given year if that patient was admitted with one of the group of DRGs associated with that illness without having been admitted under any of the DRGs in the group in the previous 12 months. Additionally, we excluded patients who were in HMOs (because claims data are not available for Medicare HMO patients from our study period), patients suffering from end-stage renal disease, and nonelderly patients.

We use data for the years 1994–1998 from three principal sources. First, we use the 20% Medicare Provider Analysis and Review (MEDPAR) file to construct total Medicare payments (including patients' deductibles and copayments) in the year after initial admission for a new occurrence of illness. We calculate acute hospital length of stay and acute inpatient, nonacute (primarily skilled nursing) inpatient, outpatient, and home health/hospice expenditures.[8] Expenditures include all reimbursements (including copay-

[8] For patients with no nonacute care, outpatient, or home health/hospice expenditures in the year after admission, we set the logarithm of the respective variable to 0. Future work could estimate the models of ln(expenditures) with Tobit or could estimate separately the determinants of zero/nonzero expenditures and ln(expenditures) conditional on expenditures >0.

ments and deductibles not paid by Medicare) from insurance claims for all hospitalizations in the year following each patient's initial admission.[9] We also tabulate whether each patient received acute hospital care in the 365 days prior to admission for his study illness, as a measure of the patient's health status on entry to the study cohort. We measure the occurrence of complications with a variable indicating whether the patient was readmitted within 1 year with a DRG in the same illness group.[10] Data on patient demographic characteristics were obtained from the Health Care Financing Administration's HISKEW enrollment files, with death dates based on death reports validated by the Social Security Administration. We used these death dates to create a one-year mortality indicator variable.

Our second principal data source is the comprehensive information on U.S. hospital characteristics provided by the annual American Hospital Association (AHA) Survey. We restrict our sample to nonfederal hospitals that ever reported providing general medical or surgical services. From the survey we obtain information on hospital ownership type, size, teaching status, system membership, and other characteristics that might affect the incentives of the hospital and its managers. We classify hospitals into three ownership categories (nonprofit, for-profit, and public (the omitted group)) and two size categories (small (<100 beds) and large (the omitted group)). We classify hospitals as teaching hospitals if they report at least twenty full-time residents. We also represent whether the hospital is a member of a multihospital system with an indicator reflecting system membership (see Madison (2001) for details on the construction of our system variable). Finally, we measure whether the hospital participates in a physician-hospital organization (PHO),[11] and whether the hospital provides skilled nursing care,[12] home health and hospice services,[13] or any outpatient services. Integration by hospitals into related markets such as skilled nursing care may create opportunities both for enhanced cost-effectiveness (e.g., Robinson, 1996) and for abuse of the Medicare reimbursement system (e.g., Banks et al., 2001), so the effect of the interaction between ownership structure and enforcement is theoretically indeterminate. Data on hospital characteristics are matched to individual patients based on the hospital to which the patient was initially admitted.

Finally, we match our patient level data with state MFCU expenditures, as reported in HHS OIG (1998a, 2000, 1995, 1997). These expenditures serve as a proxy for overall anti-

[9] Because Medicare's diagnosis-related group (DRG) payment system for hospitals appears to compensate hospitals on a fixed-price basis per admission for treatment, and Medicare does not bargain with individual hospitals, enforcement might appear to be irrelevant to Medicare patients' hospital expenditures. However, the intensity of treatment of most health problems varies enormously, and the DRG system contains important elements of cost sharing (e.g., McClellan, 1997). Thus, for most health problems, hospitals and physicians that provide more intensive treatment can receive considerable additional payments.

[10] We exclude readmissions within 30 days, which may represent a continuation of the initial course of treatment.

[11] A physician-hospital organization is a joint venture between the hospital and members of the medical staff that may act as a unified agent in managed care contracting, own a managed care plan, own and operate ambulatory care centers or ancillary services projects, or provide administrative services to physician members.

[12] Skilled nursing care is defined as nonacute services provided under the supervision of a licensed registered nurse on a 24 h basis.

[13] Home health services include any health-related services provided at the patient's residence; hospice is defined as any program providing palliative care for terminally ill patients in either an inpatient setting or at the patient's residence.

fraud enforcement efforts at the state level. The MFCU program matches state funding of the units with federal funding at a ratio of 3-to-1. As of 1997, 47 states participated in the program. There was substantial variation in spending per beneficiary from state to state, and most states invested well below the cap on matching funds (0.25% of program costs). We construct the measures of enforcement used in analysis by dividing total fraud control expenditures paid by the Federal government by the number of general medical, nonfederal hospitals and by the number of Medicare beneficiaries in the state. Table A.1 reports each state's average MFCU expenditures per hospital and per beneficiary for the 1994–1998 period in 1995 constant dollars. For purposes of calculating the logarithm of enforcement expenditures in the regression models below, we recode states with no expenditures as having $1 in enforcement on a per hospital basis and $0.10 in enforcement on a per patient basis.

Table 1 provides descriptive statistics for patients in each of the six illness groups for the study years 1994–1998. The table underscores the seriousness of illness suffered by our study patients. Study patients had Medicare hospital expenditures in the year after the onset of illness of $13,807 (circulatory disorders) to $17,301 (cerebrovascular disorders and stroke) (1995 constant dollars). One-year mortality rates ranged from 15.8% (circulatory disorders) to 37.9% (pneumonia and respiratory infections). Slightly less than half of patients had an acute care hospital admission in the year prior to their study illness. The table also shows the tremendous variation in intensity of treatment received by a patient with a given illness. Even patients with relatively well-defined illnesses such as cerebrovascular disorders and stroke experienced tremendous variation in their hospital utilization. The distribution of patients across hospital types, such as for-profit versus nonprofit hospitals, roughly resembles the distribution of hospital beds in the US (e.g., Hansmann et al., 2003).

5. Results

Table 2 presents estimates and standard errors of β from Eq. (1), the effects on billings, length of stay, and outcomes of state anti-fraud enforcement efforts, holding constant the characteristics of individuals and their hospitals of initial admission. The standard errors in Table 2 and all subsequent tables are based on an estimator of the variance–covariance matrix that is consistent in the presence of heteroscedasticity and of any correlation or grouping of regression errors within states over time. According to the table, enforcement generally has an economically and statistically insignificant estimated total effect on hospital utilization and health outcomes. For example, a one percent increase in Medicare fraud control unit expenditures per hospital leads to an statistically insignificant 0.04% increase in acute care hospital expenditures (coefficients in Table 2 and all subsequent tables are multiplied by 100 for ease of interpretation). However, if state/years with high levels of abuse invest more in enforcement, then estimates of β will be biased upward, i.e. toward zero. Because this is the likely direction of any bias from endogeneity, our results provide neither a useful estimate of the marginal payoff of an enforcement dollar nor a powerful rejection of a null effect.

Tables 3 and 4 present estimates and standard errors of β, δ, and θ from Eq. (2), the effects on billings, length of stay, and outcomes of state anti-fraud enforcement efforts by type of

Table 1
Descriptive statistics for patients with illnesses prone to abusive Medicare billing

	Pneumonia/resp infections	COPD/general resp infections	Circulatory disorders	Kidney disorders	Diabetes/metabolic disorder	Cerebrovasc disorder/stroke
Characteristics of patients						
1-year acute inpatient expenditures	$11,982 (13,031)	$11,801 (13,883)	$11,461 (14,260)	$12,831 (14,720)	$10,711 (12,977)	$11,751 (12,735)
1-year nonacute inpatient expenditures	$2,731 (7,186)	$1,967 (6,275)	$1,520 (5,429)	$2,929 (7,811)	$2,750 (7,182)	$4,703 (9,701)
1-year outpatient expenditures	$794 (1,846)	$745 (1,606)	$826 (1,770)	$1,129 (2,489)	$879 (1,946)	$847 (1,999)
1-year home health and hospice expenditures	$1,573 (4,680)	$1,731 (4,508)	$1,445 (4,147)	$2,590 (6,424)	$2,023 (5,396)	$2,110 (5,274)
1-year acute length of stay	14.45 (15.24)	13.71 (15.36)	11.21 (14.27)	15.19 (17.41)	13.50 (15.87)	14.58 (16.83)
Coded with highest reimbursement DRG	0.271	0.112	0.071	0.696	0.401	0.648
1-year mortality rate	0.379	0.253	0.158	0.317	0.286	0.291
1-year readmission rate	0.137	0.187	0.126	0.077	0.100	0.130
No hospital admission in year prior to illness	0.534	0.581	0.597	0.394	0.491	0.667
Age < 80	0.480	0.651	0.604	0.571	0.543	0.526
Male	0.446	0.402	0.406	0.595	0.323	0.401
Black	0.074	0.069	0.074	0.119	0.109	0.097
Characteristics of hospital of admission						
For-profit	0.107	0.105	0.092	0.102	0.102	0.099
Nonprofit	0.721	0.730	0.759	0.744	0.733	0.755
Hospital participates in physician/hospital organization	0.611	0.608	0.625	0.625	0.613	0.626
Hospital owns skilled nursing facility	0.453	0.441	0.428	0.434	0.438	0.447
Hospital owns outpatient facility	0.851	0.877	0.869	0.874	0.859	0.877
Hospital owns home health/hospice	0.650	0.644	0.640	0.634	0.641	0.648
Small size (<100 beds)	0.388	0.360	0.332	0.306	0.349	0.304
Teaching hospital	0.166	0.163	0.195	0.223	0.185	0.200
System hospital	0.567	0.567	0.562	0.583	0.569	0.586
N	383,983	241,245	387,028	25,403	310,019	330,473

Note: expenditures in 1995 dollars. Standard deviations in parenthesis.

Table 2
Effect of enforcement on Medicare hospital expenditures, upcoding, and patient health outcomes

ln(acute hospital expenditures)	ln(nonacute hospital expenditures)	ln(outpatient hospital expenditures)	ln(home health/hospice expenditures)	ln(acute length of stay)	Initially admitted to high-payment DRG	Readmit w/same illness	Mortality
Using ln(medicaid fraud control expenditures per hospital) to measure enforcement							
0.043	−0.392	−0.644**	−0.307	0.090	0.017	−0.039	−0.041
(0.088)	(0.362)	(0.232)	(0.459)	(0.129)	(0.013)	(0.018)	(0.031)
Using ln(medicaid fraud control expenditures per medicare beneficiary) to measure enforcement							
0.199	−1.213	−1.354	−1.393	0.924	−0.031	−0.078	−0.107
(0.250)	(1.100)	(0.741)	(1.220)	(0.392)	(0.040)	(0.050)	(0.366)

Note: expenditures in 1995 dollars. All coefficients multiplied by 100 for ease of interpretation. Coefficients from expenditure models are elasticities; coefficients from all other models are in percentage points. Standard errors (in parentheses) are based on an estimator of the variance–covariance matrix that is consistent in the presence of heteroscedasticity and of any correlation or grouping of regression errors within states over time. *Significantly different from zero at the 10% level. **Significantly different from zero at the 5% level.

Table 3

Differential effects of state Medicaid fraud control unit expenditures per hospital on Medicare hospital expenditures, upcoding, and patient health outcomes, by patient and hospital characteristics

	ln(acute hospital expenditures)	ln(nonacute hospital expenditures)	ln(outpatient hospital expenditures)	ln(home health/hospice expenditures)	ln(acute length of stay)	Initially admitted to high-payment DRG	Readmit w/same illness	Mortality
ln(Medicaid fraud control unit expenditures per hospital)	1.325** (0.332)	4.795** (1.131)	−2.467** (0.891)	1.498 (1.797)	0.982** (0.319)	0.148** (0.049)	0.082** (0.040)	0.014 (0.066)
Interactions between patient characteristics and ln(Medicaid fraud control expenditures per hospital)								
No hospital admission in prior 365 days × enforcement	−0.465** (0.155)	−0.833 (0.716)	−1.323 (0.955)	−0.915 (0.734)	−0.485** (0.131)	0.017 (0.19)	−0.067** (0.028)	0.043 (0.042)
Age <80 × enforcement	−0.285** (0.097)	−3.002** (0.578)	1.924** (0.429)	−0.096 (0.560)	−0.375* (0.181)	0.030 (0.020)	−0.051* (0.028)	−0.084 (0.044)
Male × enforcement	−0.165** (0.097)	−1.712** (0.365)	0.597 (0.348)	−0.375 (0.399)	−0.296** (0.112)	−0.029* (0.021)	−0.037 (0.036)	−0.061 (0.054)
Black × enforcement	−0.160 (0.215)	0.487 (0.568)	0.882 (0.836)	−3.735** (1.298)	−0.247 (0.181)	−0.052 (0.033)	0.047 (0.044)	−0.073 (0.064)
Interactions between hospital characteristics and ln(Medicaid fraud control expenditures per hospital)								
For profit × enforcement	−1.458** (0.502)	−1.899 (1.382)	−1.094 (1.028)	1.450 (1.078)	0.672** (0.290)	−0.106* (0.059)	−0.096 (0.071)	−0.133 (0.097)
Nonprofit × enforcement	−0.697** (0.182)	−2.153** (0.924)	−0.198 (0.597)	0.308 (0.560)	0.217 (0.154)	−0.011 (0.031)	−0.060 (0.037)	−0.004 (0.048)
PHO × enforcement	−0.267** (0.080)	−1.014** (0.428)	−0.881** (0.377)	0.228 (0.599)	−0.195* (0.101)	−0.030 (0.020)	−0.063** (0.032)	−0.021 (0.039)
Owns skilled nursing × enforcement	−0.096 (0.103)	−0.690 (0.902)	−0.039 (0.614)	−0.745 (0.523)	−0.229 (0.169)	−0.012 (0.021)	0.009 (0.034)	0.062 (0.050)
Owns outpatient × enforcement	−0.114 (0.137)	0.127 (0.491)	1.413** (0.463)	−1.364 (0.847)	0.000 (0.105)	−0.108** (0.042)	0.032 (0.035)	0.036 (0.060)
Owns home health/hospice × enforcement	0.086 (0.130)	−0.215 (0.413)	1.036** (0.504)	0.069 (0.332)	0.055 (0.157)	0.073** (0.023)	0.043 (0.048)	−0.069* (0.038)
Small × enforcement	0.265** (0.051)	−1.269** (0.188)	0.283 (0.183)	0.654** (0.170)	−0.143** (0.048)	−0.015* (0.009)	−0.022** (0.010)	−0.028** (0.014)
Teaching × enforcement	0.056 (0.331)	1.936* (1.032)	−1.039 (0.944)	0.034 (0.874)	−0.029 (0.229)	0.077** (0.031)	−0.027 (0.061)	−0.028 (0.077)
System × enforcement	−0.010 (0.121)	−0.366 (0.456)	0.423 (0.442)	0.572 (0.401)	−0.400** (0.124)	−0123 (0.025)	−0.024 (0.034)	0.033 (0.046)

Notes: see Table 2.

Table 4

Differential effects of state Medicaid fraud control unit expenditure per Medicare beneficiary on Medicare hospital expenditures, upcoding, and patient health outcomes, by patient and hospital characteristics

	ln(acute hospital expenditure)	ln(nonacute hospital expenditure)	ln(outpatient hospital expenditures)	ln(home health/hospice expenditures)	ln(acute length of stay)	Initially admitted to high-payment DRG	Readmit w/same illness	Mortality
ln(medicaid fraud control unit Expenditures per beneficiary	2.624** (0.656)	6.332** (3.096)	−8.925** (2.992)	1.540 (3.801)	3.841** (0.905)	0.154 (0.115)	0.270** (0.114)	−0.155 (0.111)
Interactions between patient characteristics and ln(Medicaid fraud control expenditures per beneficiary)								
No hospital admission in prior 365 days × enforcement	−0.787** (0.313)	2.061 (2.215)	−0.255 (3.355)	0.029 (1.492)	−0.888** (0.307)	0.058 (0.047)	−0.111* (0.062)	0.225** (0.101)
Age <80 × enforcement	−0.825** (0.254)	−5.922** (1.671)	4.508** (1.183)	−0.085 (1.324)	−1.826** (0.500)	0.056 (0.045)	−0.147** (0.070)	−0.048 (0.124)
Male × enforcement	−0.371* (0.216)	−4.687** (0.874)	1.978** (0.846)	−1.160 (1.076)	−0.916** (0.269)	−0.045 (0.050)	−0.081 (0.078)	−0.269** (0.125)
Black × enforcement	−0.227 (0.526)	0.355 (1.739)	4.784* (2.566)	−12.579** (3.282)	−0.359 (0.541)	−0.159* (0.095)	0.117 (0.109)	−0.264 (0.174)
Interactions between hospital characteristics and ln(Medicaid fraud control expenditures per beneficiary)								
For-profit × enforcement	−3.982** (1.006)	−6.665* (3.836)	−2.475 (3.268)	3.732 (2.720)	0.779 (0.739)	−0.516** (0.149)	−0.335* (0.184)	−0.108 (0.264)
Nonprofit × enforcement	−1.731** (0.471)	−4.322* (2.204)	0.893 (2.221)	2.688 (1.767)	0.230 (0.398)	−0.025 (0.084)	−0.203* (0.115)	−0.015 (0.129)
PHO × enforcement	−0.598** (0.273)	−1.670** (1.033)	−0.157 (0.964)	1.285 (1.537)	−0.522* (0.307)	−0.166** (0.057)	−0.095 (0.076)	−0.071 (0.103)
Owns skilled nursing × enforcement	−0.041 (0.285)	−4.590** (2.012)	0.971 (1.445)	−2.558* (1.460)	−0.761* (0.401)	0.022 (0.057)	0.023 (0.078)	0.166 (0.123)
Owns outpatient × enforcement	−0.026 (0.341)	2.040 (1.527)	3.475** (1.415)	−4.357** (1.902)	0.172 (0.335)	−0.109 (0.090)	0.000 (0.108)	0.128 (0.156)
Owns home health/hospice × enforcement	0.420 (0.317)	1.140 (1.282)	0.730 (1.127)	−0.164 (0.983)	−0.221 (0.389)	0.049 (0.057)	0.143 (0.100)	−0.097 (0.100)
Small × enforcement	2.392** (0.557)	−6.136** (1.960)	−0.504 (1.840)	6.189** (1.656)	−0.910* (0.519)	−0.096 (0.092)	−0.135 (0.098)	−0.168 (0.174)
Teaching × enforcement	0.112 (0.886)	6.919** (2.321)	−2.719 (2.302)	−3.022 (2.184)	0.490 (0.491)	0.058 (0.085)	−0.060 (0.128)	0.061 (0.171)
System × enforcement	−0.017 (0.339)	0.330 (1.343)	0.620 (1.384)	1.240 (1.159)	−1.059** (0.342)	0.004 (0.061)	−0.095 (0.091)	0.055 (0.121)

Notes: see Table 2.

patient and hospital, holding all else constant. Table 3 presents estimates of the effect of fraud control unit expenditures per hospital; Table 4 presents estimates of the effect of fraud control unit expenditures per beneficiary. The top panels of the tables highlight the differential responsiveness to enforcement of the Medicare utilization of certain types of patients. Increased enforcement has greater negative effects on the acute and especially the nonacute expenditures of young (age < 80) male patients. In models that measure enforcement on a per hospital basis (Table 3), a 1% increase in enforcement leads to an approximately 0.92% (=0.465 + 0.285 + 0.165) decrease in the acute inpatient expenditures of a young male patient without a prior year's hospital admission, relative to the expenditures of an older, more infirm female patient. A 1% increase in enforcement leads to a significantly greater (5.55% = 0.833 + 3.002 + 1.712) relative decline in nonacute inpatient expenditures for the same population. Increased enforcement also has greater negative effects on the home health/hospice expenditures of black versus non-black patients.

There is little systematic evidence of relative increases in rates of adverse health outcomes from these enforcement-induced reductions in care. Although the greater enforcement-induced decline in acute expenditures for healthier patients is accompanied by a greater increase in relative mortality rates in one specification (Table 4), younger male patients show a statistically significantly greater decline in readmission rates in response to enforcement in both specifications (Tables 3 and 4).

Taken together, these findings are consistent with the hypothesis that greater enforcement-induced reductions in expenditures occur in patient populations for whom additional treatment would be of more marginal benefit. Younger male elderly patients are more likely to have a living spouse (US Department of Commerce, 1999), and healthier patients are easier to rehabilitate at home. Thus, healthier young male patients may be more able to substitute informal for formal inpatient care.

The bottom panels of the tables highlight the differential responsiveness to enforcement of different types of hospitals. In both specifications, increased enforcement leads to statistically significantly greater declines in acute expenditures for patients who are initially admitted to a for-profit hospital, compared to patients initially admitted to a public hospital. Increased enforcement also leads to statistically significantly greater declines in acute expenditures for patients who are initially admitted to a nonprofit hospital, compared to patients initially admitted to a public hospital. The for-profit/public gap in the responsiveness of acute expenditures is statistically significantly greater than the nonprofit/public gap, i.e., increased enforcement leads to statistically significantly greater declines in acute expenditures for patients initially admitted to for-profit versus nonprofit hospitals (*T*-statistics on the for-profit/nonprofit difference in responsiveness, not reported in the tables, are 2.01 and 2.96 for Tables 3 and 4, respectively). For patients initially admitted to a for-profit hospital, this takes the form of upcoding, i.e., initially admitting patients into the highest-reimbursement DRG in their illness group. Increases in enforcement lead patients initially admitted to a for-profit versus a public hospital to be statistically significantly less likely to be coded in the highest-reimbursement DRG.

Increased enforcement leads to statistically significantly greater declines in nonacute expenditures for patients initially admitted to both for-profit (in one specification) and nonprofit (in both specifications) hospitals, compared to patients initially admitted to public hospitals. The for-profit/public gap in nonacute care responsiveness is not statistically distinguishable from the nonprofit/public gap at conventional levels of significance (*T*-statistics on the for-profit/nonprofit difference in responsiveness, not reported in the tables, are 0.28 and 0.75 for Tables 3 and 4, respectively).

Enforcement affects the financial behavior of for-profit and nonprofit hospitals more than it affects our (coarse) nonfinancial measure of treatment intensity. In neither for-profits nor nonprofits are enforcement-induced differential decreases in acute expenditures accompanied by enforcement-induced decreases in length-of-stay; in one specification, increases in enforcement lead to increases in the length-of-stay for patients initially admitted to a for-profit versus a public hospital. There is also no evidence of harm to patient health of these enforcement-induced reductions in expenditures. In one specification (Table 4), increases in enforcement lead to decreases in relative readmission rates for patients initially admitted to both for-profit and nonprofit hospitals.

Other hospital ownership characteristics are also associated with significant, systematic differences in responsiveness to enforcement. First, the care of patients initially admitted to a hospital that participates in a PHO is more responsive to enforcement than the care of patients initially admitted to a hospital that does not. Increases in enforcement lead to greater decreases in acute expenditures, acute length-of-stay, nonacute expenditures, and (in one specification) outpatient expenditures for patients initially admitted to a PHO hospital. In one specification, increases in enforcement lead to greater decreases in upcoding in PHO hospitals; in the other specification, increases in enforcement lead to greater decreases in readmission rates in PHO hospitals. These empirical findings are consistent with the concern that PHOs may provide a vehicle for hospitals to disguise illegal compensation to physicians for referrals (see, e.g., Blumstein, 1996a and the work cited there).

Second, in one specification, increases in enforcement lead to statistically significantly greater decreases in nonacute expenditures for patients who are initially admitted to a hospital that owns a skilled nursing facility (SNF), compared to patients initially admitted to a hospital that does not. This is consistent with the fact that the vast majority of nonacute expenditures are for SNF admissions, and with widespread concern over wasteful use of SNF and other post-acute care in the 1990s (MEDPAC, 2001).[14]

Third, increases in enforcement lead to significantly greater increases in outpatient expenditures and significantly greater decreases in home health expenditures (in one specification) for patients initially admitted to hospitals that have integrated into markets for related services, compared to stand-alone hospitals. In particular, increases in enforcement lead to greater increases in outpatient expenditures for patients initially admitted to a hospital that owns an outpatient or (in one specification) a home health/hospice facility, and to greater decreases in home health expenditures (in one specification) for patients initially admitted to a hospital that owns an outpatient or skilled nursing facility. At least for outpatient and home health/hospice, different forms of integration have similar effects on hospitals' responsiveness to enforcement. In addition, these results are consistent with the hypothesis that enforcement alters the setting in which patients receive care, such as rehabilitation, that can be provided on an in-hospital or at-home basis.

[14] Indeed, in response to these concerns, Congress completely changed the reimbursement system for post-acute care as part of the Balanced Budget Act of 1997, adopting a (more high-powered) prospective payment system for SNF and other post-acute care.

Finally, hospital size, teaching, and system status affect responsiveness to enforcement. Small, non-teaching hospitals' patients' nonacute care expenditures decline by more in response to enforcement, but large hospitals' patients' acute care and home health/hospice expenditures decline by more in response to enforcement. System and independent hospitals' patients' expenditures are similarly responsive to enforcement, but system hospitals' patients' length of stay declines by more in response to enforcement, consistent with the hypothesis that subjecting one member of a hospital system to scrutiny affects the behavior of other members of the system.

The elasticities reported in Tables 3 and 4 imply very large differences in responsiveness to enforcement by patient and hospital type. For example, based on the estimates in Table 3, a 1% or $173 increase in Medicaid enforcement expenditures per hospital (based on a sample average of $17,313) translates into a 0.267% or $31 decrease in the relative acute care expenditures for each patient initially admitted to a hospital that participates in a PHO (based on a sample average of $11,580). The differential effects of enforcement on patients by age, gender, and illness, and for patients initially admitted to for-profit or nonprofit versus public hospitals, are even larger.

There is some limited evidence of adverse outcome consequences of enforcement-induced reductions in treatment. We estimated models analogous to (2) separately for patients with each of the six study illnesses. Results not presented in the tables show, for example, that for patients with circulatory disorders, enforcement-induced declines in the relative intensity of acute care provided after initial admission to a hospital that participates in a PHO are accompanied by statistically significant increases in the relative mortality rate. However, for other illnesses, enforcement-induced reductions in treatment are accompanied by improvements in outcomes. For patients with COPD and generalized respiratory disorders, enforcement-induced declines in the relative intensity of acute care provided after initial admission to a hospital that participates in a PHO are accompanied by statistically significant *decreases* in the relative mortality rate.

6. Conclusion

Anecdotal evidence suggests that fraud and abuse in health care reimbursement systems are both widespread and responsive to law enforcement efforts. Understanding how the care provided to different types of patients, and the care supplied by different types of hospitals, responds to enforcement is important both to develop efficient fraud control policies and to test between competing economic theories of organizations. Despite this, little research has used observational data for this purpose.

This paper seeks to fill this gap by identifying the types of patients (hospitals) who receive (supply) abusive medical treatment that is most responsive to enforcement. For a 20% random sample of elderly Medicare beneficiaries hospitalized between 1994 and 1998 with one or more of six illnesses—respiratory infections and pneumonia, COPD and generalized respiratory disorders, circulatory system disorders, kidney disorders and renal failure, diabetes and nutritional/metabolic disorders, and cerebrovascular disorders and stroke—we obtain longitudinal claims data linked with Social Security death records, hospital characteristics, and state/year-level anti-fraud enforcement efforts. We conclude that increased enforcement leads to greater declines in the abusive treatment of a given type of patient or hospital either if the expenditures of that type of patient or hospital decline by more in response to enforcement than do the expenditures of the complementary type, with no accompanying differential increase in the rate of adverse health outcomes, or if the rate of adverse outcomes of that type of patient or hospital declines by more in response to enforcement than does the rate of the complementary type, with no accompanying differential increase in expenditures.

We find significant differences in the effects of enforcement across types of patients and hospitals. Healthy young male patients' acute care expenditures decline by more in response to states' anti-fraud enforcement efforts, as compared to the expenditures of their more infirm, older, female counterparts. Patients initially admitted to certain types of hospitals also systematically receive enforcement-sensitive treatment: the expenditures of patients initially admitted to both for-profit and nonprofit hospitals are more responsive to enforcement than those of patients admitted to public hospitals; the expenditures of patients initially admitted to hospitals that participate in a PHO are more responsive to enforcement than those of patients admitted to hospitals that do not; and the nonacute expenditures of patients initially admitted to hospitals that own a SNF are more responsive to enforcement than those of patients admitted to hospitals that do not. In the case of for-profit and nonprofit hospitals, enforcement affects financial measures of treatment intensity more than it affects nonfinancial measures; but in the case of PHO hospitals, enforcement has significant effects on both financial and nonfinancial measures. None of the effects of enforcement on treatment are accompanied by systematic or substantial effects on patient health outcomes.

These results are unlikely to be due to endogeneity of enforcement or unobserved patient heterogeneity across states or hospitals. As long as hospital types more prone to abuse receive more attention from law enforcement, the endogeneity of enforcement would tend to bias the magnitudes of the estimated differential responsiveness to enforcement toward zero. Along these lines, there is no obvious process that would bias the results by patient type, i.e., that would lead to unobserved changes in the composition of male versus female patients in high-enforcement versus low-enforcement states. Unobserved patient heterogeneity is also unlikely to explain our estimated enforcement/hospital-characteristic interaction effects. Estimated enforcement/patient-characteristic interaction effects suggest that increased enforcement leads to greater declines in treatment intensity for observably healthier patients. If anything, this would suggest that the unobservably healthy—rather than the unobservably sick—would be less willing to choose high-billing hospitals in high-enforcement states. But patient selection of this form would tend to bias estimates of the effect of interest toward zero as well.

Our results by hospital type support the theoretical concerns expressed by many commentators. Although the acute expenditures of patients initially admitted to a for-profit hospital decline by more in response to enforcement than the acute treatment of patients initially admitted to a nonprofit hospital, the nonacute expenditures of for-profits' patients do not. However, both the acute and nonacute expenditures of the patients from for-profits and nonprofits are significantly more responsive to enforcement than are the expenditures of patients from public hospitals. Taken with other work by two of us and by others on the effects of hospital organizational form on medical productivity (Kessler and McClellan, 2002; Hansmann et al., 2003), this supports a mixed view of for-profits (and to a lesser extent nonprofits), as being more responsive to incentives in both socially constructive and socially harmful ways. Expenditures that decline in response to enforcement are only one among many margins of behavior on which hospitals might operate.

The observed negative interaction effect on expenditures of enforcement and hospital participation in a PHO is consistent with the enhanced moral hazard inherent in such arrangements. Indeed, the objective of the vast body of law and regulation prohibiting kickbacks and self-referrals in the Medicare and Medicaid programs is to control such behavior (Jost and Davies, 2001, chapters 3 and 4). Our results confirm empirically several commentators' hypothesis that widespread tolerance of questionable but technically legal behavior has led to cynical disregard for the spirit of these laws in practice (e.g., Blumstein, 1996b). Because many forms of organization that are both efficient and common practice would be prohibited by a strict reading of the original fraud and abuse statutes, courts, administrative agencies, and Congress have adopted numerous exceptions and safe harbors to the statutes, thereby providing potential opportunities for evasion. For example, rental of hospital space and equipment to physician-owned radiology groups in exchange for a percentage of gross receipts is a commonly accepted practice, even though this amounts to exactly what the law supposedly prohibits—payment for referrals (Hall, 1988).

The negative differential effect of enforcement on the level of nonacute expenditures for patients initially admitted to a hospital that owns a SNF also may be due to moral hazard on the part of hospitals. However, this finding should be interpreted with caution because of the limited scope of the measures of health outcomes that we observe. The consequences of intensive skilled nursing care—most notably, faster and more complete rehabilitation—may not be captured fully by our very coarse measures of outcomes, which would lead us to classify such expenditures as abusive even if they were socially constructive.

More generally, the incompleteness of our measures of health outcomes substantially limits the welfare implications of our results; further study using more clinically detailed, audit-based data could at least begin to address these limitations. Audit data could also be used to validate our findings by investigating whether the characteristics of hospitals and patients identified here are actually more prone to abuse. If these findings are valid, then they could be used to target intensive audits on well-defined types of hospitals and patients, which might improve both the efficiency and the equity of both public and private-sector efforts to detect illicit behavior by health care providers. Additional analysis of observational data could explore in greater detail the mechanisms through which the potentially abusive behavior identified here might occur.

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Appendix A

See Table A.1.

Table A.1
Average MFCU expenditures per hospital and per beneficiary, 1994–1998

State	MFCU expenditures per hospital	MFCU expenditures per beneficiary	State	MFCU expenditures per hospital	MFCU expenditures per beneficiary
Alabama	$11,000	$2.16	Montana	$3,331	$1.85
Alaska	$23,415	$6.44	Nebraska	$0	$0
Arizona	$17,717	$2.04	Nevada	$31,962	$6.13
Arkansas	$13,597	$2.79	New Hampshire	$16,625	$4.51
California	$20,726	$1.53	New Jersey	$20,145	$2.06
Colorado	$8,834	$1.91	New Mexico	$16,364	$2.10
Connecticut	$16,393	$1.47	New York	$97,364	$6.73
Delaware	$80,609	$5.44	North Carolina	$11,181	$1.18
District of Columbia	$0	$0	North Dakota	$0	$0
Florida	$19,387	$2.14	Ohio	$12,417	$1.45
Georgia	$13,837	$1.80	Oklahoma	$5,478	$1.62
Hawaii	$43,103	$10.72	Oregon	$5,388	$0.69
Idaho	$0	$0	Pennsylvania	$15,463	$2.27
Illinois	$7,817	$1.06	Rhode Island	$58,740	$4.59
Indiana	$10,524	$2.01	South Carolina	$8,404	$0.95
Iowa	$3,484	$1.31	South Dakota	$4,088	$2.68
Kansas	$3,958	$2.00	Tennessee	$7,300	$0.64
Kentucky	$6,942	$1.10	Texas	$5,580	$0.85
Louisiana	$7,997	$1.33	Utah	$25,008	$5.59
Maine	$8,664	$1.94	Vermont	$19,749	$2.60
Maryland	$24,791	$2.67	Virginia	$6,974	$0.95
Massachusetts	$21,680	$2.19	Washington	$10,351	$1.17
Michigan	$14,902	$1.89	West Virginia	$7,396	$1.10
Minnesota	$4,748	$1.32	Wisconsin	$4,010	$1.02
Mississippi	$7,971	$1.54	Wyoming	$8,191	$4.07
Missouri	$12,646	$2.30			

Note: Federal Government share only. MFCU expenditures in constant 1995 dollars.

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