Financial Incentives and Physician Treatment Decisions: Evidence from Lower Back Pain

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July 6, 2020

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Abstract

In response to the high cost of health care, the capitated payment model has become more popular in recent years. Under capitation, physicians are compensated a fixed amount per patient regardless of the services generated. This study quantifies the effects of capitated payment models on physicians' treatment decisions about patients with lower back pain in the United States. We use data from 2003 to 2006 from a large employer-sponsored health insurance claim database, and we leverage capitation variation within the plan and physician to mitigate selection concerns. We find that the treatment intensity—mainly from therapy, diagnostic testing, and drugs—of patients under a capitation system is 10% lower than otherwise similar patients in a noncapitated plan. We also find no evidence of increased readmission rates for patients in a capitated plan.

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1 Introduction

Health care spending accounts for a large and increasing share of gross domestic product in the United States. In response, some payers deviate from the traditional fee-for-service payment model and have adopted the capitated payment model. Under capitation, physicians are compensated based on the number of patients they treat rather than the volume of services they prescribe. Payers who have adopted capitation payments claim that it can reduce the use of medical services and the provision of low-value care—two factors contributing to the high cost of health care. For instance, Shrank, Rogstad and Parekh (2019) estimated that the annual cost of overtreatment rose from about \$75.7 billion in 2012 to \$101.2 billion in 2019. The literature also documents a range of potentially low-value care.[1] Essentially, the capitated payment contract transfers all or part of the financial risk to the physicians, encouraging them to be accountable for the quantity and quality of services they provide. The recently established accountable care organizations in Medicare and private insurers are an example of a capitation payment model.

It can be challenging to assess whether capitated contracts lead to cost savings because there is selection into which providers and payers use capitation. For example, capitation contracts are more common in managed care plans such as health maintenance organizations (HMOs), and these plans may attract patients with lower medical needs rather than truly reduce unnecessary care. Understanding the source of any potential cost differences driven by capitation contracts is important in evaluating whether such incentives should be implemented more widely.

In this paper, we empirically examine the effects of capitated payment models on physicians' prescribing decisions. The movement toward managed care in the 1990s and early 2000s led to the growing popularity of capitation contracts in many places.[2] This historical movement toward capitation contracts provides an opportunity to study the issue. We focus on the treatment of lower back pain (LBP). The disease is economically significant: about 80% of the US population is affected by lower back pain at some point, and people with this condition spend more than \$50 billion annually on treatment. More importantly, the treatment varies greatly across patients and providers (Smith, 2011). For example, using surgeries to treat lower back pain is costly, and the effects are unclear for most people (Mirza and Deyo, 2007; Goodney et al., 2015). However, more than 1.2 million spinal surgeries are performed each year, and the number of elective lumbar fusion surgeries increased by 62.3% from 2004 to 2015 (Martin et al., 2019). This raises

[1] Examples include treatment of marginally-ill patients (Currie and Slusky 2020; Alalouf, Miller and Wherry 2019), long-term care (Einav, Finkelstein and Mahoney, 2018), heart disease treatment (Chandra and Staiger, 2017), and C-section (Jacobson et al., 2013).

[2] For example, Ho and Pakes (2014) document that in 2003 74% of primary care physicians in California were paid under capitation.

the question of the efficacy of the treatment, and whether the capitated payment model helps reduce the overuse of surgery for treating lower back pain.

To collect data for the period 2003–2006, we use Truven MarketScan, a large commercial insurance claim data set based on the working-age US population. We construct and identify around 80,000 episodes treating lower back pain. For each episode, we identify the primary care physicians who refer patients to subsequent care. We directly observe from the data whether these physicians are paid under capitation, and we use this information as the key independent variable.[3] For each episode, we also construct a treatment intensity measure based on the weighted sum of the procedures performed. We construct the weights using a hedonic regression, where we regress price on procedure code dummy variables, patient age, and gender using noncapitated contracts, and we predict the average price for each procedure code. By taking this step, price variation is removed from the data and we are able to focus only on variation in utilization.

We use a fixed-effects model to control for patient and physician selection into a capitation payment arrangement. First, patients who are treated by a primary care physician under a capitated plan may differ from other patients. To address this selection problem, we control for patient demographic information and chronic conditions generated from past claims. We also identify patients who stay in the same set of plans over the sample period and control for the plan-group fixed effects. Variation in capitation within a plan-group can be generated either from a plan contracting with multiple providers with different capitation contracts or from employers switching plans—for example, from a traditional plan to a managed care plan—over time. This procedure allows us to control for unobserved patient selection into capitated plans. We also control for physician fixed effects to account for physician selection into capitated contracts. This leverages the variation of within-physician capitation arrangements, because the same physician may contract with multiple plans and the capitation contracts are bargained separately.

We find that patients referred by primary care physicians in a capitated plan experience moderate reduction in their overall treatment intensity. The overall treatment intensity is 25% lower for patients in a capitated model than for other patients under no control, and the difference dropped to 4% to 10% when controlling for patient individual characteristics and different fixed effects. The treatment difference is mainly driven by the utilization of diagnostic testing (21%), therapy (14%), and drugs (13%). There is almost no difference in the use of

[3]We use the capitation status of the primary care physician as the measure on capitation. Given that patients are often treated by multiple physicians and capitation status of these physicians is correlated, we interpret our results as identifying the effects of capitated episodes rather than the capitated primary care physician alone.

surgery.

We also find that the differences in treatment lead to very little difference in the subsequent likelihood of LBP-related claims. For a fraction of patients in our benchmark sample who we were able to track over the next four years after the end of their episodes, we find that those in a capitated system have a very similar likelihood of having another LBP-related episode one to four years after the end of their last episode. This finding suggests that capitation is effective in reducing the use of treatment in LBP episodes without causing negative treatment outcomes.

This paper contributes to the growing literature studying whether the capitated payment model reduces unnecessary care. Researchers have found mixed evidence on the impact of capitated contracts on the cost and quality of health care. Some studies provide evidence that capitation leads to lower costs (Gaynor, Rebitzer and Taylor, 2004; Ho and Pakes, 2014; Andoh-Adjei et al., 2018), while others show a limited effect of capitation in controlling total health care expenditure or improving health care quality (Altman, Cutler and Zeckhauser, 2003; Duggan, 2004; Kontopantelis et al., 2015; Zhang and Sweetman, 2018). Many studies examine the effects of capitated arrangements using cross-plan or cross-insurer variation (e.g., Altman, Cutler and Zeckhauser 2003 Ho and Pakes 2014). The problem with such an approach is that capitation often exists along with other cost-control methods, such as a narrow network, utilization authorization, and selected covered benefits (Glied and Zivin, 2002). We offer new insights by leveraging episode-level variation in capitation and use plan-year fixed effects to separate the effects of capitated contracts from other cost-control methods. This study also contributes to the literature by considering employer-sponsored plans of a large-scale national sample, as opposed to plans only in a specific state (Ho and Pakes, 2014) or only Medicare/Medicaid plans (Duggan, 2004).

More broadly, our work advances the literature exploring physician behaviors and the organization of care. Recent research finds that physicians respond strongly to financial incentives, including payments from drug firms (Carey, Lieber and Miller, 2020), reimbursement from Medicaid (Alexander and Schnell, 2019) and Medicare (Einav, Finkelstein and Mahoney 2018; Maclean et al. 2018), physician ownership of practices (Howard, David and Hockenberry, 2017), and episode-based payment (Carroll et al., 2018), etc. Our results indicate that physicians respond to the capitated compensation model in the treatment of lower back pain.

The rest of the chapter is organized as follows. Section 2 provides background information on lower back pain and capitation contracts. Section 3 presents the empirical strategy. Results are presented in Section 4. Section 5 concludes.

2 Background

2.1 Lower Back Pain

Lower back pain (LBP) is defined as "pain in the area on the posterior aspect of the body from the lower margin of the twelfth ribs to the lower gluteal folds with or without pain referred into one or both lower limbs that lasts for at least one day" (Deyo, Von Korff and Duhrkoop, 2015). LBP affects most adults, causes disability for some, and is a common reason for seeking healthcare (Deyo, Von Korff and Duhrkoop, 2015). According to the estimation of Luckhaupt et al. (2019), 26.4% of US workers have LBP, 8.1% have frequent and severe LBP, and 5.6% have work-related LBP.

Despite the prevalence of LBP, generally accepted guidelines for diagnosing LBP are absent (Koes et al., 2010). The diagnostic methods include medical history and physical exam and imaging tests. When combined with clinical evaluations, imaging tests may help diagnose spinal problems. However, imaging tests are not always associated with clinically meaningful benefits, and they can even be harmful. In addition, many imaging tests poorly predict which patients will benefit from surgery (Chou et al., 2011; Goodney et al., 2015). Nevertheless, the utilization of imaging tests is high in the United States. For instance, Schwartz et al. (2014) estimated that annual Medicare spending on imaging for uncomplicated LBP ranges from $82 million to $226 million, which does not include costs associated with follow-up testing and care due to the results.

There is no consensus on the best way to treat LBP either. Treatments for lower back pain include medications, noninterventional treatments such as physical therapy and exercise programs, and interventional spine surgeries and procedures. Surgical procedures range from well-established approaches for discectomies and spinal canal decompression to multiple means of addressing segmental fusion using several different approaches, materials, instruments, and indications. However, medical researchers find limited evidence to support the use of many interventional surgical procedures (See Friedly, Standaert and Chan (2010) for a review of the literature.) Meanwhile, the utilization of LBP surgeries continues to increase. For instance, the rate of spinal fusion operations for stenosis increased 67%, from 31.6 per 100,000 Medicare beneficiaries in 2001 to 52.7 per 100,000 Medicare beneficiaries in 2011 (Goodney et al., 2015). Disagreement also exists regarding the benefits of physical therapy, and "international guidelines contain conflicting recommendations for manipulation and exercise therapy" (Koes et al., 2001; Chou et al., 2007). Fritz et al. (2012) and Fritz, Brennan and Hunter (2015) find a large variation among physicians about whether to use and when to use physical therapies.

In summary, due to LBP's proliferation and wide variation in the treatment choices, we concentrate on LBP to examine how the capitation arrangement influences the treatment decisions of physicians.

2.2 Capitation

To control health care expenditures, payers may replace a fee-for-service payment model with a capitated payment model by paying physicians based on the number of patients they treat instead of the volume of services they prescribe. Capitation contracts are most common with HMO plans and are less common with preferred provider organization plans. But even among HMO plans, there is large variation in whether capitation contracts are used. For instance, according to Zuvekas and Cohen (2010), only 15%–33% of physician office visits for private HMO plan enrollees are under a capitation arrangement.

The forms of capitation payments can vary. One extreme is the global capitation payment system, which bundles all providers and covers the cost of all services received by patients, including inpatient hospital stays. At the other extreme is payment that covers only the services provided by the primary care physician or physician group. The latter type is almost always accompanied by "shared risk arrangements," under which a target is set for total spending. Cost savings or overruns relative to the target are shared between the primary care physicians and the insurers. Overall, the capitation payment system deviates from the traditional pay-for-volume model and generates incentives for physicians to share the financial risk of a patient's entire treatment episode.[4]

The capitation payment model appeared in the 1980s and thrived with the proliferation of HMOs. The rate of capitation payment among physicians has decreased since the early 2000s (Zuvekas and Cohen, 2010). Recent years have seen new reforms toward a bundled payment model and Medicare accountable care organization initiatives; these variations of the capitation idea are intended to create financial incentives for physicians to curb medical expenditures (Friedberg et al., 2015).

[4]See Ho and Pakes (2014) for details about capitation arrangements.

3 Empirical Strategy

3.1 Data and Sample

The data for this study are from the Truven MarketScan Commercial Claims and Encounter Data from 2003 to 2006.[5] It is a large commercial insurance claim data set based on the working-age US population. For each claim record, the data set provides diagnosis and procedure codes and detailed payment information. We can directly observe whether a claim is paid under capitation, which allows us to compare the effect of capitation payment on treatment intensity. Because the Truven MarketScan data also track enrollees over time, we can observe an individual's full history of medical service use. We also observe other demographic and socioeconomic characteristics, including age, gender, and employment status.

We construct a sample of patients with nonemergency LBP-related episodes. To build this sample, we identify a patient's claim encounters with LBP-related diagnoses and assign these encounters into episodes by time.[6] An LBP episode starts from a patient's earliest LBP encounter, followed by subsequent encounters with a time gap shorter than 180 days. An episode ends if there is no additional LBP encounter within 180 days of the last record. Two consecutive LBP encounters that occur more than 180 days apart are designated as two separate episodes.[7] We then keep only episodes that initiated from a primary care office visit.[8] Finally, we remove from the data set pregnant women, people under age 18 or over age 65, and people with severe chronic diseases.[9] We also exclude episodes involving emergency care or out-of-network encounters, because we want to focus on the non-urgent development of treatment.[10] In total, the sample includes 82,156 episodes from 76,407 patients.

The key independent variable of capitation is defined based on the payment arrangement of a patient's primary care physician. We choose primary care physicians because they play a critical role in deciding different treatment options. They are also most frequently targeted by capitation arrangements. When a capitation arrangement is set up, insurers often remunerate primary care

[5] 2003 is the first year the capitation measure is reported. Starting in 2007, few observations are under capitation.

[6] We follow Cherkin et al. (1992*a*) to define the LBP diagnosis. The specific International Classification of Diseases (ICD-9) diagnosis codes for lower back pain are presented in Table 8 in the Appendix.

[7] Most individuals have one episode during the sample period. Table 7 in the Appendix displays the robustness of our main results using episodes defined based on a 90-day window rather than a 180-day window.

[8] Episodes that begin with surgical treatment or urgent care may be different from episodes that begin with primary care physicians. Such episodes might be more acute, or part of the episode might not be included in the data set.

[9] The chronic conditions we rule out include colorectal cancer, lung cancer, female/male breast cancer, endometrial cancer, prostate cancer, Alzheimer's disease and related disorders or senile dementia, heart failure, acute myocardial infarction, stroke/transient ischemic attack, and hip/pelvic fracture.

[10] Occasionally, in-network physicians may refer patients to out-of-network facilities or providers. But patients might also use out-of-network facilities without referral, which we want to rule out. Since we cannot distinguish the actual referral pattern in the data, we drop from the sample all out-of-network episodes.

physicians through fixed monthly payments per patient to cover the cost of patient services. Primary care physicians can also be rewarded for savings from the entire episode. Therefore, the capitation arrangement generates a financial incentive for primary care physicians to save on patient treatment.[11] Even though the process by which a patient receives medical treatment involves multiple players, primary care physicians can influence the treatment intensity of the entire episode. A primary care physician under a capitation arrangement can decrease the treatment intensity for patients (for example, prescribing fewer physical therapy sessions), or the physician can refer patients to specialists who are less likely to prescribe expensive treatments, or who charge lower price for the same service.[12]

Patients in capitated plans seem to be healthier. The sample defined above includes 10,274 capitated LBP episodes and 71,882 noncapitated episodes. In Table 1 we compare the patient individual characteristics of capitated and noncapitated episodes. The patients in capitated plans are slightly younger than their counterparts receiving treatment in a noncapitated system, and they are less likely to have chronic conditions. We also find that patients who receive care in a capitation arrangement are more likely than others to be paid hourly and to work part-time.

3.2 Treatment Intensity Measures

We construct the overall treatment intensity measure for medical services based on the procedures used in each episode. In our sample, we observe a primary procedure code associated with each medical claim. An episode often contains tens to hundreds of medical claims and procedure codes. To aggregate all procedures at the episode level, we calculate the weighted sum of all the procedures performed in that episode, where the weights are the expected average price of each procedure $\bar{p}_z$:

$$t = \sum_z \bar{p}_z f_z, \tag{1}$$

where f_z is the quantity of each procedure code z, and $\bar{p}_z$ is the weight.

For each medical claim, we observe the price p as the sum of the insurer payment to the provider, including consumer cost-sharing. This price represents the overall resources used for each procedure and captures price variation among insurers and providers. Since our focus is

[11] In our data, we do not observe the specific financial terms of the capitation arrangements; rather, we observe whether a claim is paid under capitation. The capitation status may represent different types of capitation contracts.

[12] The capitation status of primary care physicians and downstream providers (such as radiologists, surgeons, and therapists) is positively correlated in our data. One should think of the treatment effects as not only from the capitated primary care physician but as representing the capitation status of the entire episode.

on understanding utilization patterns, we want the measure of treatment intensity to reflect only differences in service utilization but not differences in prices for services across different contracts. To eliminate the role of prices, we calculate the average price of each procedure by regressing price on the patient's age, gender, and chronic conditions. We control for these patient characteristics because they might affect the resources used. In this estimation step, we only use the claims from noncapitated claims, because the price is often not accurately reported for capitation contracts. We then predict the price for all claims with that procedure code to get $\bar{p}_z$.

The treatment intensity measure has a bimodal distribution and is highly skewed. Most people receive minimum or no treatment, while some patients receive very intensive treatment. To account for the skewness of the data, we transform the raw treatment intensity measure into log scale using the inverse hyperbolic sine transformation:

$$IHS(t) = log(t + \sqrt{t^2 + 1}).$$

The inverse hyperbolic sine transformation behaves similar to log and preserves zero as zero. As shown in Table 6 in the Appendix, the main results are robust using the raw value.

For each episode, we can also construct the treatment intensity measure for different medical services. We classify LBP-related medical claims into five categories: office visit, diagnostic testing, therapy session, surgeries directly related to LBP treatment, and other surgeries.[13] We also construct a dummy variable indicating whether each type of service is used at all in an episode. Every observation will have an office visit, but some may not have other services.

Table 2 shows the summary statistics of the outcome variables. The average treatment intensity for all services within an LBP episode for patients in a capitation system is around \$438, while that of patients in other types of plans is \$590. The average treatment intensity is significantly higher for patients in noncapitated plans for nearly all categories of service except for back surgery.

For 75% of the episodes in our baseline sample, we observe whether there is any drug use, and the related drug claims. For these episodes, we identify LBP-related drug prescriptions and all subsequent refills for these prescriptions. We then construct a similar treatment measure for overall drug use, and also for the two most common types of drugs: opioids and muscle relaxants. To do so, we group drug claims by a national drug code. We then calculate the average per-day price for each drug in our sample by year. Finally, we multiply the average

[13]We define LBP surgery based on Cherkin et al. (1992*b*).

price by the number of days of supply to determine the per-drug spending. The episode-level total drug usage is the sum of the spending on all drugs. This usage measure takes the same price for a specific drug across different plans and insurers and reflects only usage difference, not price differences.

3.3 Regression Model

As noted in Section 3.1, selection is a potentially large problem in our data. As a first way to address the problem, we control for patient characteristics X, including age, gender, employment status, and the existence of chronic conditions:

$$y_{it} = \alpha + \beta_1 CAP_{it} + X_{it}\beta_X + t + \epsilon_{it}, \tag{2}$$

where i is the index for each episode (level of observation), and y_{it} is either the log treatment intensity measure or a dummy variable indicating whether a certain service is used. CAP_{it} is a dummy variable indicating whether the patient in that episode is referred by a primary care physician under a capitation system. A time trend t controls for aggregate movement in treatment style over time.[14]

One important channel where selection might happen is when different patients choose different plans. For example, capitation contracts are more common in HMOs than in preferred provider organizations. The former also impose other cost-control tools, such as a narrower network. So variation in capitation status may reflect patients' selection into these different insurance plans. To address this concern, we want to control for plan fixed effects. To do so, we first classify patients as in the same group if they enroll in the same set of plans over the sample period. For example, if patient 1 chooses plan A in year 1 and plan B in year 2, patient 2 chooses plan A in year 1 and plan B in year 2, and patient 3 chooses plan C in year 1 and plan B in year 2, then only patients 1 and 2 will be classified in the same group, and patient 3 will be in another group. The plan group classification is hereafter referred to as plan fixed effects. We estimate the following equation:

$$y_{it} = \alpha + \beta_2 CAP_{it} + X_{it}\beta_X + \gamma_g + t + \epsilon_{it}, \tag{3}$$

where γ_g are dummy variables for different plan groups. The estimated β_2 captures the treatment difference for patients in both capitated and noncapitated plans who have similar demo-

[14]Time is included as a continuous variable because some episodes span multiple years.

graphic characteristics and who are enrolled in the same set of plans over time.

The coefficient of capitation within a plan group, β_2, is identified based on two types of variations. First, a plan often covers multiple physician groups with different capitation status. However, patients are less likely to actively select physicians in a capitation system based on their health status, because patients often cannot observe the specific capitation arrangement of a physician within a plan. Second, employers may switch plan types over time. For example, an employer might offer an HMO in year 1 and switch to a preferred provider organization in year 2, while the same set of employees remain in the risk pool. Controlling for plan fixed effects in this case is similar to controlling for individual fixed effects over time, where individuals remain in the same plans over time. Under the assumption that both sources of variation are not affected by active patient selection, β_2 will identify the true treatment effects of a capitation arrangement on physicians' treatment decisions.

We further decompose the treatment effects estimated in equation (3) into the cross-section variation (based on the first source of variation) and cross-time variation (based on the second source of variation) in the following two models:

$$y_{it} = \alpha + \beta_{21} CAP_{it} + X_{it}\beta_X + \gamma_{gt} + \epsilon_{it}. \tag{4}$$

$$y_{it} = \alpha + \beta_{22} CAP_{gt} + X_{it}\beta_X + \gamma_g + t + \epsilon_{it}. \tag{5}$$

Model (4) controls for plan-year fixed effects, so the variation in capitation is due only to the contract differences within a plan-year, and there is no cross-time variation. One benefit of this model is that it also separates the effects of capitation from other cost-control methods that vary at the plan level. Often capitation happens along with other supply-side cost-control methods, such as utilization authorization and referral restriction. These measures, however, usually vary across plans and are the same within a plan-year. By controlling for plan-year fixed effects, we can hold fixed the variation of other supply-side cost-control measures and identify the net effects of capitation.

Model (5) calculates the average capitation rates within a plan-year, CAP_{gt}, and we use this as the new independent variable. Since we control for plan fixed effects, the coefficient of CAP_{gt} reflects the change in capitation of a specific plan over time.

The second source of selection comes from the physician side. Physicians may have different preferences toward capitated arrangements and treatment philosophies, and they may actively select into capitation contracts based on their treatment style. For example, physicians who prescribe less intense treatment on average may be more willing to join a capitation contract.

To address this type of selection, we control for physician fixed effects:

$$y_{it} = \alpha + \beta_3 CAP_{it} + X_{it}\beta_X + \delta_s + t + \epsilon_{it}, \tag{6}$$

where $y_i t$ is either the log treatment intensity measure $IHS(t)$ or a dummy variable indicating whether a certain service is used, and δ_s are dummy variables for different providers. β_3 will then capture the difference in treatment decisions for patients in both capitated and noncapitated arrangements who are treated by the same provider.

Similar to the plan fixed effects model, the coefficient of capitation for a physician, β_3, is identified based on two types of variations. First, the same physician may enter different contracts with different plans in the same year. Controlling for physician fixed effects in this case will remove the concern that a physician's treatment style is correlated with her or his decision to enter a capitation arrangement, because we compare the treatment within a year. Second, physicians may switch capitation arrangements over time. Controlling for physician fixed effects in this case will remove time-invariant physician characteristics correlated with treatment and capitation choice. Under the assumption that there is no change in treatment philosophy that is correlated with the decision to switch between capitation contracts, the model will identify the true treatment effects.

We further decompose the treatment effects we estimated in equation (6) into the cross-section variation (based on the first source of variation) and cross-time variation (based on the second source of variation) in the following two models:

$$y_{it} = \alpha + \beta_{31} CAP_{it} + X_{it}\beta_X + \delta_{st} + \epsilon_{it}. \tag{7}$$

$$y_{it} = \alpha + \beta_{32} CAP_{st} + X_{it}\beta_X + \delta_s + t + \epsilon_{it}. \tag{8}$$

In model (7), β_{31} identifies differences in the same physician's treatment decisions in the same year; it does not identify the effects if a physician switches from no capitation to treating only patients in a capitated plan. Anecdotal evidence indicates that physicians may not be able to vary their treatment decision among patients with different underlying reimbursement contracts in the same year. To the extent that this is true, a null effect in this model might not indicate that the true treatment effect is zero. Model (8) identifies how the treatment decision will change over time when physicians move from fewer patients in capitated contracts to more patients in capitated contracts.

Finally, we include both plan fixed effects and physician fixed effects in the same regression:

$$y_{it} = \alpha + \beta_4 CAP_{it} + X_{it}\beta_X + \delta_s + \gamma_g + t + \epsilon_{it}. \tag{9}$$

$$y_{it} = \alpha + \beta_5 CAP_{it} + X_{it}\beta_X + \delta_s \gamma_g + t + \epsilon_{it}. \tag{10}$$

Controlling for physician fixed effects removes the impact of time-invariant physician characteristics on capitation, and plan fixed effects removes the impact of time-invariant plan characteristics on capitation. Equation (9) controls for both of these effects separately in the same equation. Under the assumption that physician fixed effects are similar across different plans conditional on all other variables, this model controls both physician and patient selection. β_4 represents the treatment effects for similar patients treated by the same physician. In equation (10), we control the interactive term of plan fixed effects and provider fixed effects. Since there is no variation of capitation status for a plan-provider pair in the same year, β_5 identifies the effects of capitation using cross-time variation within a plan-provider pair.

Even though we cannot directly examine whether these fixed effects models remove all selection concerns, we can at least show that they reduce selection on observable patient characteristics. Figure 1 offers a comparison of the likelihood of having chronic conditions among patients in capitated and noncapitated plans. The first panel on the left contains the raw mean differences. Patients in a capitation system are less likely to have most of the chronic conditions without controls. Controlling for plan and provider fixed effects reduces the differences to almost zero for almost all chronic conditions. For example, patients under capitation are 7.5% less likely to have high blood fat (hyperlipidemia) than patients in other types of plans under no controls. The estimated difference for hyperlipidemia decreases to 4% once we add plan or provider fixed effects separately, and the difference is 2.6% when we include both plan and provider fixed effects. If unobservables are of a similar nature to observables, then our fixed effects model will account for the selection problem.

Anther way to assess whether plan and provider fixed effects removed the selection concerns is to estimate a model without controlling for individual characteristics:

$$y_{it} = \alpha + \beta_6 CAP_{it} + \delta_s + \gamma_g + t + \epsilon_{it}. \tag{11}$$

If β_6 is similar to β_4, then the fixed effects model is effective in removing selection concerns.

4 Results

In this section, we present the results of treatment decisions for medical services and drugs.

4.1 Using Any Medical Services

Figure 2 presents the impact of capitation status on the likelihood of patients using a certain category of service. The dependent variables are the dummy variables of using therapy (panel 1), back surgery(panel 2), other surgeries (panel 3), and diagnostic (panel 4) during a lower back pain (LBP) episode.[15] Each panel lists, from bottom to top, six specifications: raw difference without any controls ("no control"); controlling for patient characteristics and time trend ("individual"); controlling for time trend, patient characteristics, and plan fixed effects ("plan"); controlling for time trend, patient characteristics, and provider fixed effects ("provider"); controlling for year, patient characteristics, plan, and provider fixed effects ("provider, plan"); controlling for year, patient characteristics, and provider times plan fixed effects ("provider × plan").

Patients who are experiencing LBP episodes and treated by capitated physicians are less likely to have therapy and diagnosis testing. We observe that with no control variables, the patients in a capitated plan are 4.1% less likely to use any therapy treatment. Controlling for patient characteristics, this difference is 3.3%. When we further add plan fixed effects, the difference in likelihood decreases to 2%. When we include provider fixed effects instead of plan fixed effects, the difference changes to 1.6%. Further, there exists a 2.1% difference in the likelihood of using therapy between capitated and noncapitated patients if we control for provider and plan fixed effects at the same time. Finally, when we control for provider × plan fixed effects, there exists a 1.3% difference.

For diagnostic testing, controlling for the provider and plan fixed effects leads to a difference of around 4% between capitated and noncapitated arrangements. Medical literature suggests that diagnostic testing has low value for most patients. Our findings indicates that capitation contracts are effective in reducing the usage of such services.

On the other hand, we do not observe that capitation status significantly influences the utilization of back surgery or other surgeries. This might be due to the fact that the surgeries are used mainly for very severe cases of LBP and are not excessive treatments that could be removed.

[15] We do not include the office visit category here because every episode by definition contains an initial office visit to a primary care physician.

4.2 Treatment Intensity of Medical Services

Table 3 shows the results with the treatment intensity of all services. The outcome variable is the inverse hyperbolic sine transformation of treatment intensity of all services. Column 1 shows that, with no controls, the patients in a capitation system utilize 25.2% fewer medical resources than patients in noncapitated plans. Column 2 addresses the concern that healthy patients may endogenously select into capitated plans by controlling for patient individual characteristics. Under this specification, we find that treatment intensity is 16.4% lower with capitated episodes. Further controlling for the plan fixed effects in column 3 shows that capitation results in a difference of 10.2% in overall treatment intensity. In column 4, we control for provider fixed effects and patient characteristics. The difference in treatment intensity between capitated and noncapitated plans is 4.4%. Column 5 simultaneously addresses the concern of patient selection and provider selection by controlling plan fixed effects and provider fixed effects. We find that capitation reduces treatment intensity by 9.4%.

One way to evaluate whether the fixed effects model is effective in controlling for selection is to remove patient characteristics from the regression and see whether the results are similar. As shown in column 6, removing patient characteristics yields very similar estimates, which means provider and plan-group fixed effects are adequate to absorb the impact of individual-level observable differences. Finally, column 7 shows the effects of capitation based on cross-time variation within a provider-plan pair. For this specification, we are not able to use all the observations in the sample and the standard errors are larger, yet we still find a modest (though not significant) reduction in treatment intensity.

Figure 3 shows the results with treatment intensity by medical service categories. The dependent variables are the inverse hyperbolic sine transformation of treatment intensity for certain service categories. When we compare the raw differences with no control variables, capitation is related to significant decreases in treatment intensity, ranging from 4.5% to 30.0%. However, when patient individual characteristics, provider, and plan fixed effects are jointly controlled, the results indicate that primary care physicians' capitation status does not significantly influence the intensity of office visits, back surgery, and other surgeries. Capitation mainly impacts the utilization of therapy and diagnostic testing, decreasing the treatment intensity by 13.5% and 20.6%, respectively.

Table 4 and 5 display the effect of capitation on the treatment intensity of all services by cross-sectional and cross-time variation. Table 4 show separate results of capitation variation within a plan with cross-section variation and with cross-time variation. Column 1 uses the same specification as column 3 in Table 3, controlling for time trend, patient characteristics, and plan fixed effects. In column 2, we absorb the cross-time variation of the insurance plan's capitation arrangement by controlling the plan $\times$ year fixed effects. This specification shows that the treatment intensity of patients treated by capitated physicians is 9.6% lower than that of patients treated by noncapitated physicians. This difference is solely driven by capitation status and is not driven by any other supply-side cost-control methods varying between but not within a plan year.

In column 3, we replace the capitation status of an episode by the average capitation rate of a plan within a year. This specification leads to all patients in the same plan-year getting the same capitation status and takes the plans' variation across years to identify the effect of capitation. The result indicates that a plan change from 0% of capitation to 100% capitation would lead to a 32.1% reduction in treatment intensity. Column 4 changes the plan-year's average capitation rate to a dummy variable indicating whether a plan-year has any capitation arrangement. This specification shows that having capitation with some physicians in a plan leads to a 10.4% reduction in the treatment intensity of all services relative to a plan with no capitated patients.

Similar to Table 4, Table 5 analyzes the effect of capitation using providers' cross-sectional and cross-time variation. Column 1 examines each episode's capitation status and controls for the provider fixed effects, time trend, and patient characteristics (similar to column 4 in Table 3). The cross-time variation in providers' capitation status is shown in column 2 by adding the provider $\times$ year fixed effect. This specification does not find a significant effect of capitation on treatment intensity. This may suggest that physicians are not able to vary treatment based on patients' insurance status within a year. Column 3 uses the cross-time variation of a provider's capitation status by using the provider-year's average capitation rate as the regressor. Under this specification, capitation on average results in a 10.7% reduction in treatment intensity. Finally, when we replace the average capitation rate in column 3 with whether a provider-year has any capitated arrangement as in column 4, the adoption of capitation status on average reduces the treatment intensity of all services by 9.2%.

4.3 Drug Utilization

Figure 4 shows the results with drug utilization. Panel A analyzes the effect of capitation on whether an episode includes any LBP-related drug claims. The three specifications, from bottom to top, are the differences with no controls, controlling for individual characteristics, and simultaneously controlling for individual characteristics and plan fixed effects. We find that when controlling for patient individual characteristics, time trend, and plan fixed effects, the patients receiving care under a capitation arrangement are 2% less likely to use any drugs, 2.3% less likely to use opioids, and 3% less likely to use muscle relaxants.[16]

Panel B analyzes the impact of capitation on treatment intensity related to the use of drugs. After controlling for patient individual characteristics, time trend, and plan fixed effects, capitation results in an 11.9% reduction in the use of all drugs. Capitation also reduces the use of opioids and muscle relaxants by 2.6% and 10%, respectively.

4.4 Placebo Test: Emergency Room Visits

The previous results provide evidence that the capitation status of providers leads to reductions in the intensity of treatment for lower back pain. In this section, we use emergency room (ER) visits as a placebo test. We analyze ER visits that are unrelated to patients' LBP conditions.[17] Unlike LBP episodes, ER visits are typically initiated by patients with urgent conditions needing immediate care. The treatment patients receive during a visit to the ER should be less affected by primary care physicians.

For the same patient episodes identified in our baseline analysis, we construct several measures on the ER services utilization. First, we construct a dummy variable indicating whether a patient has at least one ER visit during the episode. The procedure codes related to ER visits also document the severity of the illness and the urgency for care (in five levels). We thus construct a measure for any ER visits, or any ER visits with the most severe conditions (Level 5). To reflect the overall utilization intensity, we also calculate the number of days with ER visits for all ER visits and ER visits with the most severe conditions, respectively. Finally, we construct a measure of the overall ER visit treatment intensity based on a weighted average of the number of procedures performed, where weights are calculated by average payment.

As shown in Figure 5, the capitation status of a patient's primary care physician has almost no impact on the patient's utilization of ER services. Patients with a primary care physician in a capitated plan are slightly less like to use any ER services (1%) under no controls, indicating selection into these services is much smaller, consistent with the nature of this type of utilization. After controlling for patient individual characteristics, the coefficients are almost zero, though the standard errors are larger when we control for provider fixed effects. The effects are even closer to zero using ER visits with severe conditions as dependent variables. The treatment intensity measures are more noisy with larger confidence intervals. But again, none of the coefficients are significant. These results show that our benchmark results are not driven by

[16]We do not control for provider fixed effects for the smaller drug sample because there are not enough observations to accurately identify capitation effects within a provider.

[17]Patients with LBP ER visits are excluded from our sample, as mentioned in Section 3.1.

potential factors other than capitation that might affect the treatment intensity for all services.

4.5 Readmission Rates

The previous sections describe modest treatment differences between patients who are being treated by a physician in a capitated plan and those receiving treatment by a physician under a noncapitated arrangement, especially for therapy and diagnostic testing. A natural question is whether this treatment difference represents a reduction in overtreatment or indicates under-treatment by capitated physicians. Understanding the question is important for understanding the welfare implication of the capitated payment model.

One way to measure the quality of care is to compare the readmission rates for lower back pain. If a reduction in treatment for patients in a capitation system leads to readmission for lower back pain, then the differences may reflect underuse of valuable care. On the other hand, if we find that patients in a capitated plan have similar likelihood of having a lower back pain diagnosis in the future, then this indicates that capitation might reduce overtreatment.

We construct readmission measures by tracking patients in our sample over time. We are able to track about 65% of the baseline sample over the next four years. We examine whether these patients have any LBP-related diagnosis in the four years after the end of their episodes. We then run a regression of this readmission measure on their original capitation status with other controls and fixed effects specified in Section 3.3.

Figure 6 shows the results of our analysis. In general, after controlling for individual characteristics and plan fixed effects, patients receiving treatment under a capitation arrangement are either slightly less likely to be readmitted for LBP, or the differences are not significant.

5 Conclusion

This paper explores the effect of capitated payment models on the treatment of lower back pain in employer-sponsored health insurance plans. We find that patients who are referred by primary care physicians under capitated contracts receive significantly less treatment. The overall treatment intensity is 10% lower. Capitation contracts reduce the utilization of therapy, diagnostic imaging, and drugs such as muscle relaxants, but capitation contracts have almost no effect on surgeries or the prescription of opioids. Our identification relies on the panel feature of the claim data. Although patients rarely have multiple LBP episodes, we identify a group of people who are enrolled in the same plan over time and control for the plan group fixed effects. We also control for physician fixed effects to further isolate selection from true treatment effects.

We choose to study patients with lower back pain because evidence from the medical literature indicates that many of the services used to treat this condition have low value to patients. We find that capitation leads to differential treatment for otherwise similar patients, suggesting inefficiency in care from either undertreatment of patients under a capitated arrangement or overtreatment of patients in a noncapitated plan. More detailed data are needed to evaluate whether capitation moves treatment closer to the optimal level.

In our data, we do not observe the specific capitation arrangement details. Specifically, we do not observe how the incentives are shared among different physicians in a group, and whether they face dynamic incentives over time. Our sample period is one characterized by the declining popularity of capitation payment contracts. Physicians might not respond to the incentive if they did not stay in this type of contract for the following period, especially if some of the contracts offer dynamic incentives. The current movement toward value-based care may suffer from the same concern. More research is needed to understand the incentives of different capitation contracts, and how they affect patients' long-term health status.

Using only within-provider-year variation to identify the effects of capitation on treatment yields almost zero treatment effects, suggesting that physicians do not differentiate care for patients with different insurance plans in the same period. The treatment effects solely come from variations in the average capitation rates a physician faces over time. These findings are consistent with other empirical works providing evidence that treatment is homogeneous within a physician practice in the same year, even though the patients are from both fee-for-service plans and managed care plans (Glied and Zivin, 2002). Most physician groups in the United States contract with multiple insurers and face variation in their compensation incentives within the practice. However, the pattern is changing due to the recent trend toward fully integrated systems. For example, providers in certain vertically integrated health care systems, such as Kaiser, almost exclusively treat managed care patients from their own system. A natural next step is to study whether and to what extent fully integrated systems change physician behaviors relative to a simple capitation model.

Table 1: Summary Statistics of Capitated/Non-capitated Patients, Patient Characteristics

	Capitated		Non-capitated		Difference	
	Mean	SD	Mean	SD	Mean	SE
Demographics						
Female (%)	56.61	49.56	55.92	49.65	0.68	0.52
Age	43.79	10.94	44.85	10.93	-1.07	0.12
Health Status (%)						
Acquired Hypothyroidism	5.54	22.87	7.74	26.72	-2.20	0.25
Anemia	4.20	20.05	5.91	23.59	-1.72	0.22
Cataract	2.62	15.97	3.40	18.13	-0.79	0.17
Obstructive Pulmonary/Bronchiectasis	5.67	23.14	7.25	25.93	-1.58	0.25
Chronic Kidney Disease	1.90	13.65	1.89	13.63	0.00	0.14
Diabetes	7.97	27.09	9.35	29.12	-1.38	0.29
Hyperlipidemia	20.75	40.55	28.40	45.10	-7.65	0.43
Depression	8.32	27.62	9.47	29.28	-1.15	0.29
Hypertension	19.28	39.45	26.60	44.19	-7.32	0.42
Glaucoma	2.64	16.03	3.53	18.45	-0.89	0.17
Ischemic Heart Disease	3.70	18.87	5.36	22.51	-1.66	0.20
Atrial Fibrillation	0.59	7.68	0.80	8.88	-0.20	0.08
Asthma	5.99	23.72	5.97	23.68	0.02	0.25
Benign Prostatic Hyperplasia	1.51	12.19	2.61	15.96	-1.11	0.13
Rheumatoid Arthritis/ Osteoarthritis	14.12	34.83	18.57	38.89	-4.45	0.37
Osteoporosis	2.84	16.62	2.94	16.90	-0.10	0.18
Compensation Classification (%)						
Salary Non-union	2.99	17.03	11.67	32.11	-8.69	0.21
Salary Union	0.18	4.18	0.88	9.31	-0.70	0.05
Salary Other	0.05	2.21	1.02	10.03	-0.97	0.04
Hourly Non-union	1.35	11.55	8.49	27.87	-7.14	0.15
Hourly Union	4.33	20.36	7.20	25.85	-2.87	0.22
Hourly Other	0.02	1.40	1.06	10.22	-1.04	0.04
Non-union	3.39	18.09	7.73	26.71	-4.35	0.20
Union	0.15	3.82	1.56	12.40	-1.41	0.06
Unkown	87.55	33.02	60.39	48.91	27.16	0.37
Employment Status (%)						
Active Full Time	17.88	38.32	45.66	49.81	-27.78	0.42
Active Part Time or Seasonal	0.13	3.56	1.33	11.44	-1.20	0.06
Early Retiree	1.42	11.84	5.43	22.67	-4.01	0.14
Medicare Eligible Retiree	0.10	3.12	0.49	6.99	-0.39	0.04
Retiree (status unknown)	0.19	4.41	0.19	4.38	0.00	0.05
COBRA Continuee	0.07	2.61	0.53	7.26	-0.46	0.04
Long-Term Disability	0.05	2.21	0.32	5.61	-0.27	0.03
Surviving Spouse/Depend	0.00	0.00	0.20	4.44	-0.20	0.02
Other/Unknown	80.16	39.88	45.85	49.83	34.31	0.44
Number of Observations	10274		71882			

Note: The table shows the summary statistics of patient characteristics for capitated/non-capitated patients separately. SD = standard deviation. SE = standard error.

Table 2: Summary Statistics of Capitated/Non-capitated Patients, Outcome

	Capitated		Non-capitated		Difference	
	Mean	SD	Mean	SD	Mean	SE
Treatment Intensity, $						
All Medical Services	438.08	1507.88	590.00	1586.65	-151.92	16.01
Office Visit	151.62	220.17	171.46	234.91	-19.84	2.34
Therapy	49.21	262.10	97.95	404.82	-48.74	2.99
Back Surgery	81.91	981.26	97.37	968.28	-15.46	10.33
Other Surgery	67.44	485.61	101.54	583.88	-34.10	5.26
Diagnostics	81.14	294.93	113.23	317.87	-32.09	3.14
Number of Observations	10,274		71,882			
Drug Usage Intensity, $						
All Drug Usage	202.02	11.49	375.18	8.11	-173.16	19.89
Muscle Relaxants	5.32	0.29	8.97	0.22	-3.64	0.53
Opioids	23.41	2.79	34.07	1.99	-10.66	4.87
Number of Observations	9,230		52,269			

Note: The table shows the summary statistics of outcome variables for capitated/non-capitated patients separately. SD = standard deviation. SE = standard error.

Table 3: Regression Results Comparing the Treatment Intensity of All Services for Patients under Capitated and Noncapitated Arrangements

	1	2	3	4	5	6	7
Capitated	-0.252***	-0.164***	-0.102***	-0.044	-0.094**	-0.088**	-0.053
	(0.029)	(0.036)	(0.036)	(0.041)	(0.037)	(0.037)	(0.058)
Number of Observations	82,156	82,156	81,058	61,370	60,206	60,206	36,477
R-squared	0.004	0.042	0.073	0.326	0.352	0.335	0.403
Prov Fixed Effects				×	×	×	
Plan Fixed Effects			×		×	×	
Prov × Plan Fixed Effects							×
Individual Characteristics		×	×	×	×		×

Note: The table shows the regression results comparing the treatment intensity of capitated/non-capitated patients. The dependent variable is the inverse hyperbolic sine transformation of treatment intensity of all services. Standard errors are clustered at the data contributor level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Cross-sectional and Cross-time Plan Capitation Variation on Overall Treatment Intensity

	1	2	3	4
Capitated	-0.102***	-0.096***		
	(0.036)	(0.035)		
Average Capitation Rate of Plan-Year			-0.321**	
			(0.158)	
Any Capitated within Plan-Year				-0.104**
				(0.044)
Number of Observations	81,058	79,849	81,058	81,058
R-squared	0.073	0.087	0.073	0.073
Plan Fixed Effects	×		×	×
Plan × Year Fixed Effects		×		

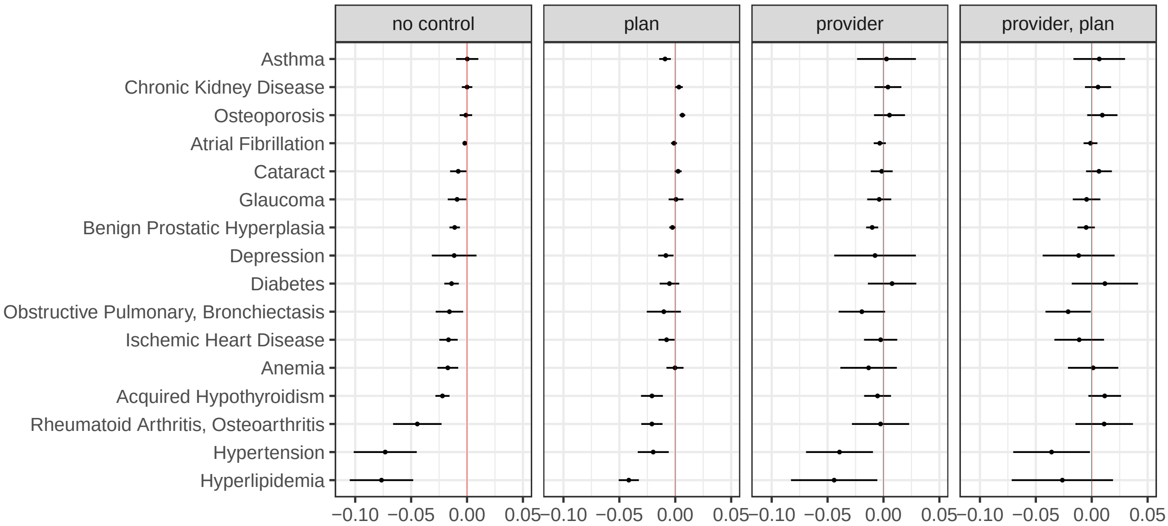
Note: The table examines the cross-sectional and cross-time variation of plan's capitation status change on the overall treatment intensity. The dependent variable is the inverse hyperbolic sine transformation of treatment intensity of all services. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Cross-sectional and Cross-time Provider Variation on Overall Treatment Intensity

	1	2	3	4
Capitated	-0.044	0.007		
	(0.041)	(0.052)		
Average Capitation Rate of Prov-Year			-0.107	
			(0.067)	
Any Capitated within Prov-Year				-0.092**
				(0.044)
Number of Observations	61,370	47,094	61,370	61,370
R-squared	0.326	0.368	0.326	0.326
Prov Fixed Effects	×		×	×
Prov × Year Fixed Effects		×		

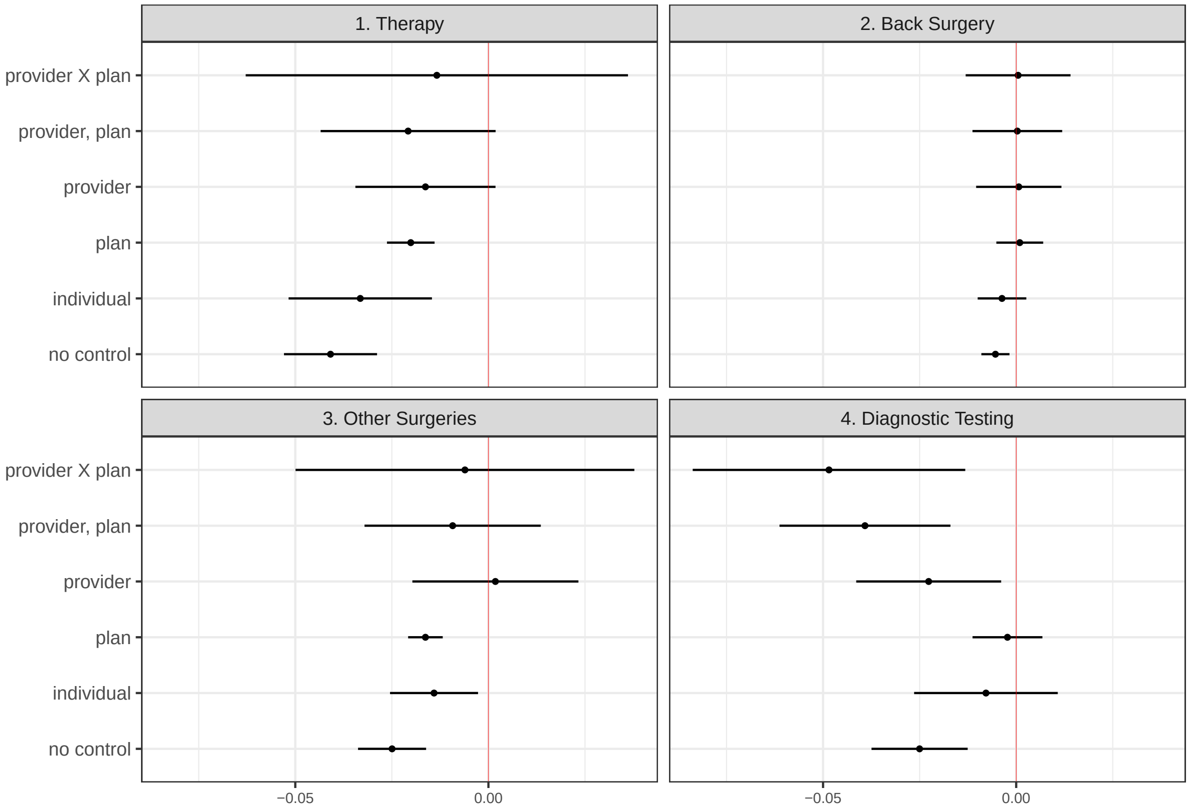
Note: The table examines the cross-sectional and cross-time variation of provider's capitation status change on the overall treatment intensity. The dependent variable is the inverse hyperbolic sine transformation of treatment intensity of all services. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1: Chronic Condition Rate Differences between Capitated/Noncapitated Patients



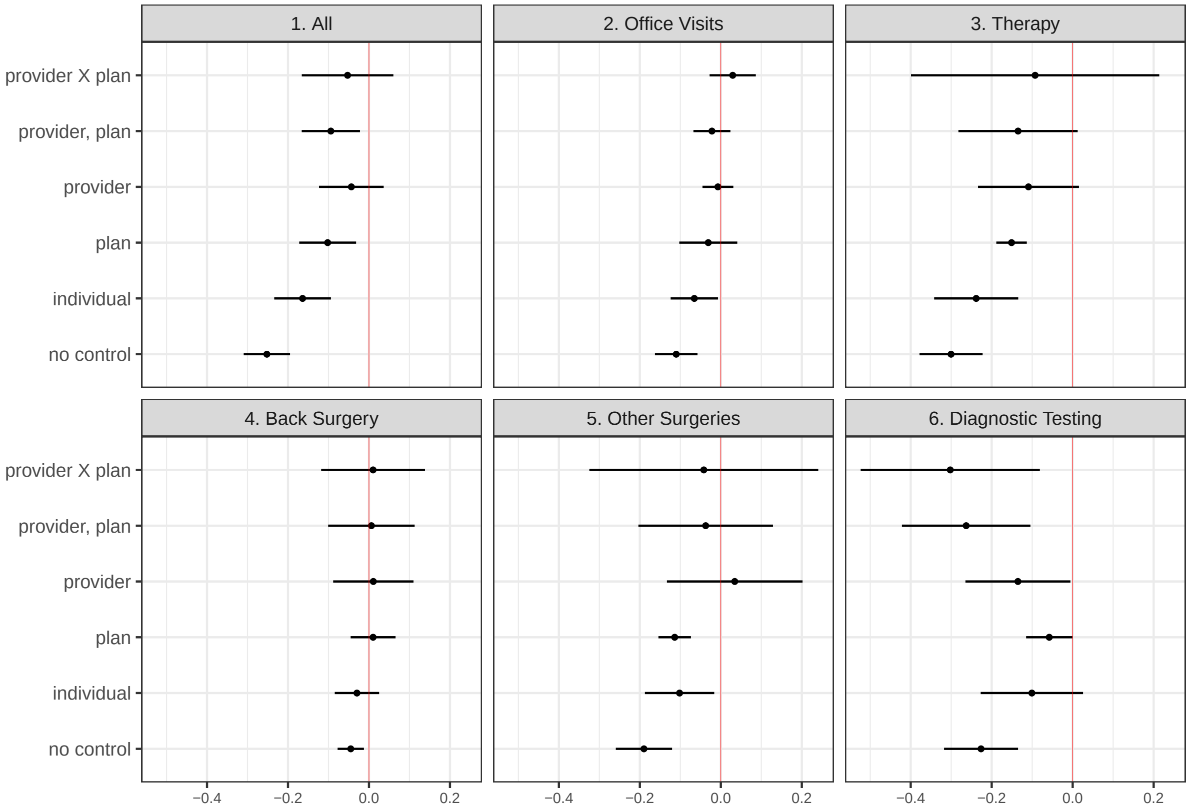
Note: The four panels from left to right indicate no controls; controlling for year, age, and plan fixed effects; controlling for year, age, and provider fixed effects; and controlling for year, age, and plan and provider fixed effects.

Figure 2: Extensive Margin: Using Any Medical Services



Note: The figure shows the estimated results that whether capitated patients use certain type of services compared to noncapitated patients. The dependent variables are the indicators of using any services of certain types. Panels 1 to 4 show the analyses with the usage of therapy services, back surgery, other surgeries, and diagnostic testing. Each line represents a different specification: "no control" represents no control variable; "individual" controls for patient characteristics; "plan" controls for plan fixed effects; "provider" controls for provider fixed effects; "provider, plan" controls for both plan and provider fixed effects separately; "provider X plan" controls for plan and provider fixed effects interactively. Standard errors are clustered at data contributor level.

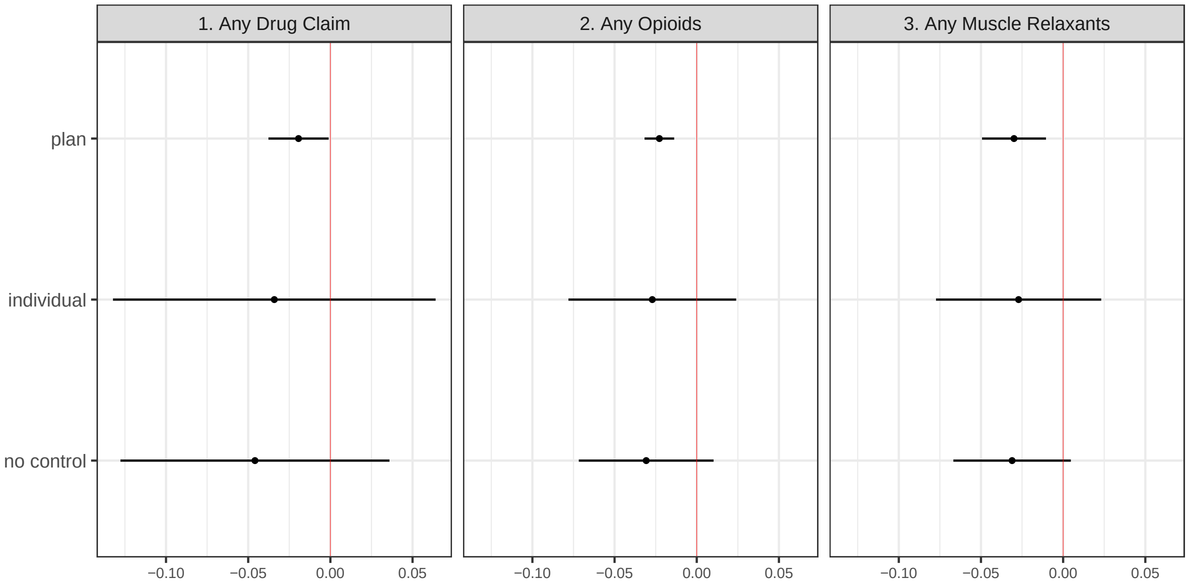
Figure 3: Treatment Intensity of Medical Services



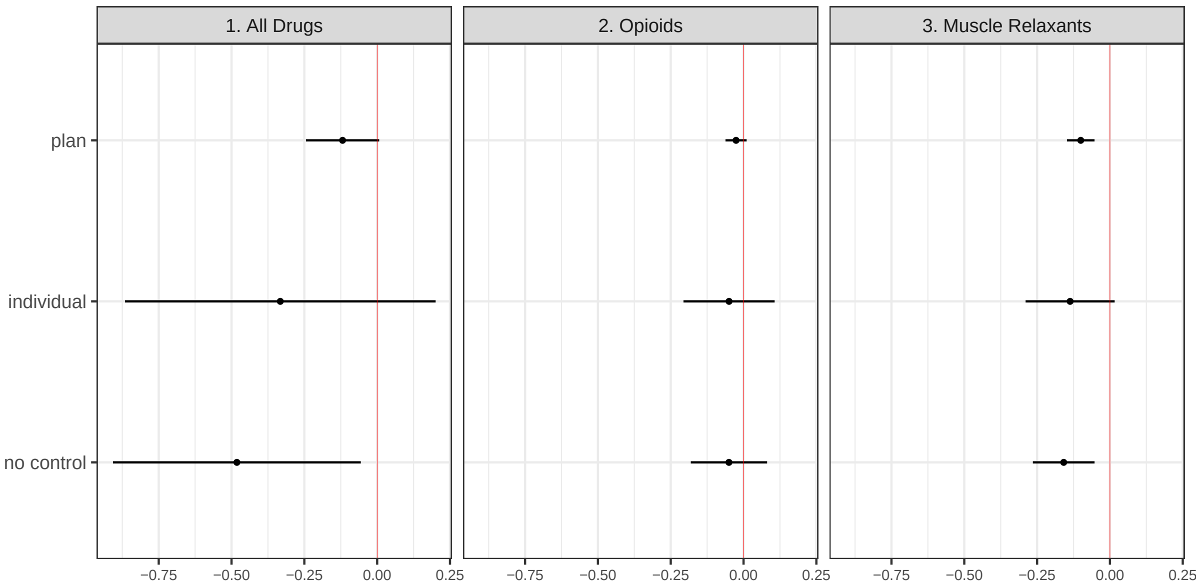
Note: The figure shows the estimated results studying the treatment intensity differences of capitated and noncapitated patients. The dependent variables are the inverse hyperbolic sine transformation of treatment intensity. Panels 1 to 6 present the effects with all medical services, office visits, therapy, back surgery, other surgeries, and diagnostic testing separately. Each line represents a different specification: "no control" represents no control variable; "individual" controls for patient characteristics; "plan" controls for plan fixed effects; "provider" controls for provider fixed effects; "provider, plan" controls for both plan and provider fixed effects separately; "provider X plan" controls for plan and provider fixed effects interactively. Standard errors are clustered at data contributor level.

Figure 4: Drug Usage Intensity

Panel A: Any Drug Claim

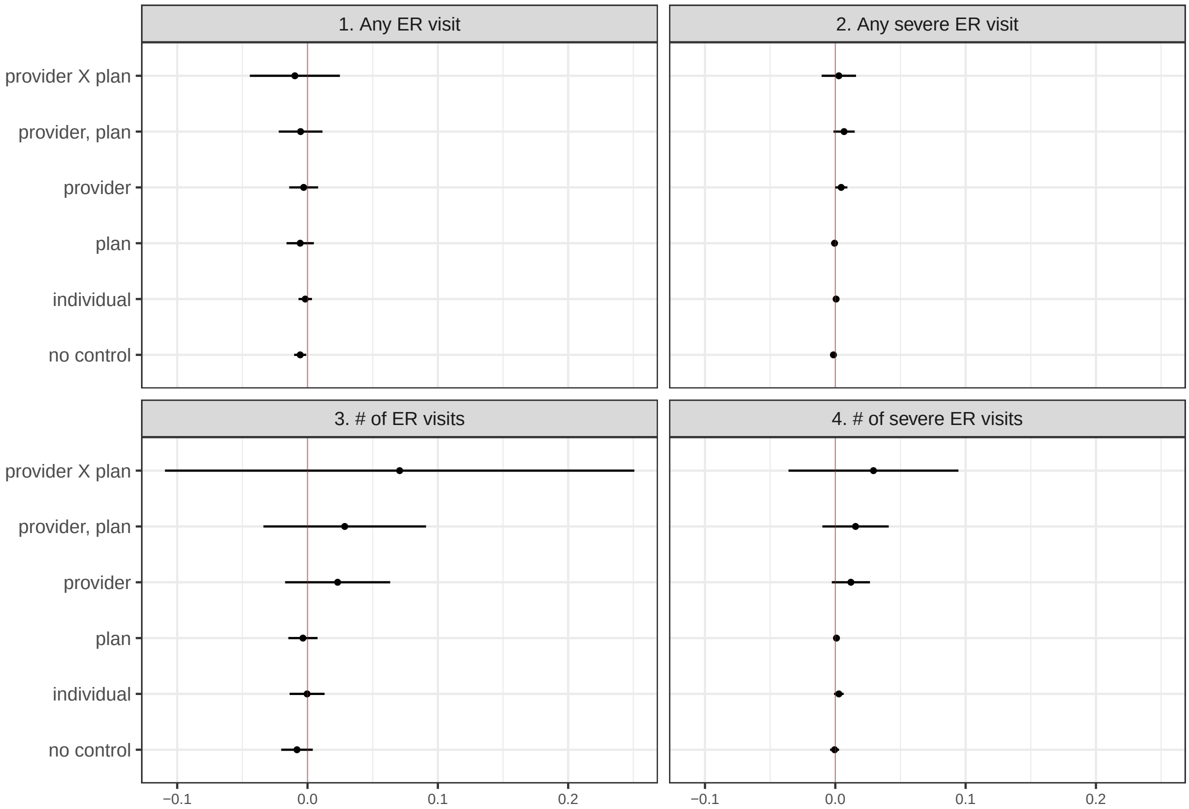


Panel B: Drug Usage Intensity



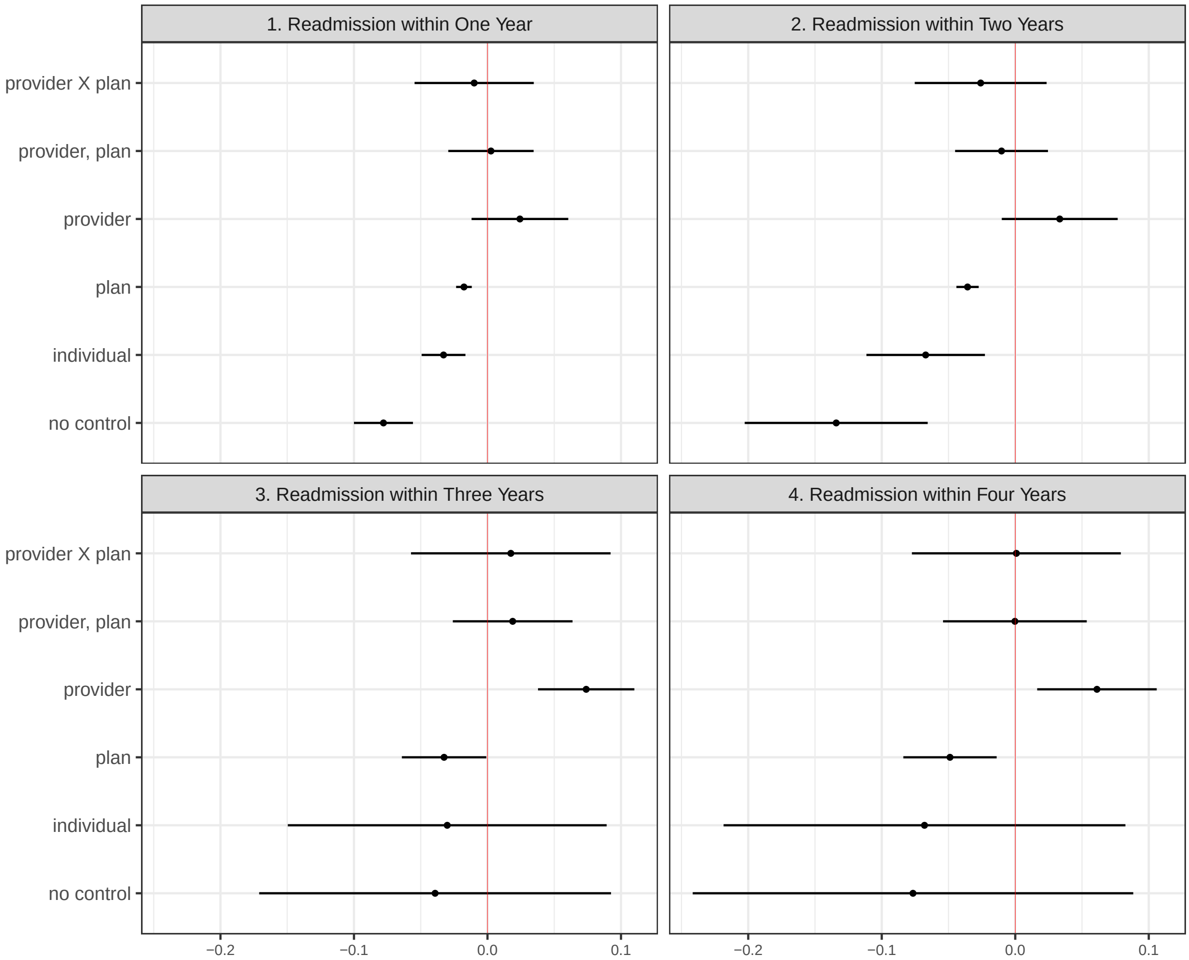
Note: The figure shows the coefficient estimates of capitation and the 95% confidence interval. In Panel A, the dependent variable is whether there is any claim. In Panel B, the dependent variables are the inverse hyperbolic sine transformation of treatment intensity. Subpanels 1 to 3 show the effects with all drugs, opioids, and muscle relaxants. Each line represents a different specification: "no control" represents no control variable; "individual" controls for patient characteristics; "plan" controls for plan fixed effects. Standard errors are clustered at data contributor level.

Figure 5: Placebo Test: Emergency Room Visits



Note: The figure shows the treatment intensity differences for ER visits of capitated and non-capitated patients. The dependent variable in panel 1 is a dummy variable indicating whether there is any ER visit. The dependent variable in panel 2 is a dummy variable indicating whether there is any ER visit associated with severe conditions. The dependent variable in panel 3 is the number of ER visits. The dependent variable in panel 4 is the number of ER visits with a severe condition. Each line represents a different specification: "no control" represents no control variable; "individual" controls for patient characteristics; "plan" controls for plan fixed effects; "provider" controls for provider fixed effects; "provider, plan" controls for both plan and provider fixed effects separately; "provider X plan" controls for plan and provider fixed effects interactively. Standard errors are clustered at data contributor level.

Figure 6: Readmission Rates



Notes: The figure shows the differences in readmission rates of lower back pain for capitated and noncapitated patients. The dependent variables are a dummy variable indicating whether there is any lower back pain related claims within a certain period of the end of the episode. Each line represents a different specification: "no control" represents no control variable; "individual" controls for patient characteristics; "plan" controls for plan fixed effects; "provider" controls for provider fixed effects; "provider, plan" controls for both plan and provider fixed effects separately; "provider X plan" controls for plan and provider fixed effects interactively. Standard errors are clustered at data contributor level.

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Appendix

Table 6: Treatment Intensity of All Services, Raw Values

	1	2	3	4	5
Capitated	-151.922***	-81.797**	-23.777	38.640	13.230
	(30.112)	(35.733)	(27.304)	(80.469)	(77.402)
Number of Observations	82,156	82,156	81,058	61,370	60,206
R-squared	0.001	0.028	0.054	0.262	0.289
Prov FE				×	×
Plan FE			×		×
Individual Characteristics		×	×	×	×

Note: The table shows the regression results comparing the treatment intensity of capitated/noncapitated patients. The dependent variable is the treatment intensity of all services. Standard errors are clustered at the data contributor level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Robustness: Treatment Intensity of All Services with 90-Day Episode Definition

	1	2	3	4	5
Capitated	-0.223***	-0.146***	-0.085***	-0.024	-0.055*
	(0.027)	(0.030)	(0.023)	(0.033)	(0.030)
Number of Observations	87,838	87,838	86,790	67,833	66,784
R-squared	0.003	0.030	0.062	0.308	0.335
Prov Fixed Effects				×	×
Plan Fixed Effects			×		×
Individual Characteristics		×	×	×	×

Note: The table shows the regression results comparing the treatment intensity of capitated/noncapitated patients. Each observation is an episode. For a patient, an LBP episode starts from his/her earliest LBP encounter, followed by subsequent encounters with a time gap shorter than 90 days. An episode ends if there is no additional LBP encounters within 90 days of the last record. Two consecutive LBP encounters with larger than 90-day gaps are designated to two separate episodes. The dependent variable is the inverse hyperbolic sine transformation of treatment intensity of all services. Standard errors are clustered at the data contributor level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Diagnoses for Lower Back Pain (Cherkin et al., 1992*a*)

ICD-9 Code(s)	Diagnosis
721.3	Lumbosacral spondylosis without myelopathy
721.42	Spondylogenic compression of lumbar spinal cord
721.9	Spondylosis of unspecified site without myelopathy
721.91	Spondylogenic compression of spinal cord, not specified
722.1	Displacement of thoracic or lumbar disc without myelopathy
722.1	Displacement of lumbar disc without myelopathy
722.2	Displacement of unspecified disc without myelopathy
722.52	Degeneration of lumbar or lumbosacral disc
722.6	Degeneration of disc, site unspecified
722.7	Disc disorder with myelopathy, site unspecified
722.73	Lumbar disc disorder with myelopathy
722.8	Postlaminectomy syndrome, unspecified region
722.83	Postlaminectomy syndrome, lumbar
722.9	Other and unspecified disc disorder, site unspecified
722.93	Other and unspecified lumbar disc disorder
724	Spinal stenosis, unspecified site (not cervical)
724.02	Lumbar stenosis
724.09	Spinal stenosis, other
724.2	Lumbago
724.3	Sciatica
724.4	Thoracic or lumbosacral neuritis or radiculitis, unspecified
724.5	Backache, unspecified
724.6	Disorders of sacrum (including lumbosacral joint instability)
724.8	Other symptoms referable to back
724.9	Other unspecified back disorders
738.4	Acquired spondylolisthesis
739.3	Nonallopathic lesions, lumbar region
739.4	Nonallopathic lesions, sacral region
756.11	Spondylolysis, lumbosacral region
756.12	Spondylolisthesis
847.2	Sprains and strains, lumbar
847.3	Sprains and strains, sacral
847.9	Sprains and strains, unspecified region
307.89*	Psychogenic backache
721.5-8*	Unique or unusual forms of spondylosis
722.30*	Schmorl's nodes, unspecified region
722.32*	Lumbar Schmorl's nodes
737.10-737.30*	Idiopathic scoliosis
738.5*	Other acquired deformity of back or spine
756.10*	Anomaly of spine, unspecified
756.13-756.19*	Various congenital anomalies
805.4*	Lumbar fracture
805.6*	Sacral or coccygeal fracture
805.8*	Vertebral fracture of unspecified site
846.0-9	Sprains and strains, sacroiliac
996.4	Mechanical complication of internal orthopedic device, implant and graft

Note: This table exhibits the ICD-9 diagnosis codes related to LBP (Cherkin et al., 1992*a*). * refers to diagnoses applicable only to nonsurgical cases.