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

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Abstract

In response to high health care spending, capitated payment model is becoming more popular in recent years. Under capitation, physicians are compensated by a fixed amount per patient regardless of the services generated. In this paper we quantify the effects of capitated payment models on physicians' treatment decisions in the context of lower back pain in the US. We use a large employer-sponsored health insurance claim database from 2003 to 2006, and leverage capitation variation within the plan and physician to mitigate selection concerns. We find that the treatment intensity of capitated patients is 10% lower than otherwise similar non-capitated patients, mainly from therapy, diagnostic testing and drugs. We also find no evidence of increased readmission rates for capitated patients.

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1 Introduction

Health care spending accounts for a large and increasing share of GDP in the U.S.. 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. These payers adopting capitation payments claim that it can reduce the overuse of medical services and low-value care, which are two factors contributing to the high health care expenditure. For instance, Shrank, Rogstad and Parekh (2019) estimated that annual over-treatment costs about $75.7 billion to $101.2 billion from 2012 to 2019. 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 prescribed. The recent establishment of Accountable Care Organizations (ACOs) in Medicare and private insurers is 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 like HMO, 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 and consumers' financial outcomes. We study the issue in the movement toward managed care in the 1990s and early 2000s, where in many places capitation contracts were popular.[2] This historical movement toward capitation contracts provides an opportunity to study the issue. We focus on the treatment of lower back pain. The disease is economically significant: about 80% of the US population is affected by lower back pain at some point in life, and they spend over $50 billion annually on treatment. More importantly, the treatment varies largely 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). On the other hand, 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

[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.

(Martin et al., 2019). This raises the question of the efficiency of the treatment, and whether the capitated payment model helps reduce the overuse of surgeries in treating lower back pain.

We use the 2003 - 2006 Truven Marketscan data, a large commercial insurance claim dataset 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 physician who refers patients to subsequent care. We directly observe whether the physicians are paid under capitation from the data and we use this information as the key independent variable. 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 dummies, patient age, and gender using non-capitated contracts, and predict the average price for each procedure code. By doing so we remove price variation and only keep variation in utilization.

We use a fixed-effects model to control for patient and physician selection into capitation. First, patients who are treated by a capitated primary care physician may differ from the rest. To address the 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 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. By doing so we 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 capitated primary care physicians experience moderate reduction in their overall treatment intensity. The overall treatment intensity is 25% lower for capitated patients than the rest under no control, while the difference dropped to 4% to 10% if we control for patient individual characteristics and different fixed effects. The treatment difference is mainly driven by the utilization of diagnostic testing (21%), therapy (14%), and drug (13%). There is almost no differences in the usage of surgeries.

We also find the differences in treatment leads to very little difference in subsequent likelihood of LBP-related claims. For a fraction of patients in our benchmark sample for whom we could track them over the next four years after the end of their episodes, we find the capitated patients have very similar likelihood of having another LBP related episode 1 to 4 years after the end of their last episode. This suggests that capitation is effective in reducing over use of treatment in LBP episodes.

The 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 cost and quality of care. Gaynor, Rebitzer and Taylor (2004) and Ho and Pakes (2014) provide evidence that capitation causes cost reduction, while Duggan (2004) shows that switching from fee-for-service to managed care in Medicaid leads to higher government expenditure. Many studies examine the effects of the capitated arrangements using cross-plan or cross-insurer variation (e.g., Altman, Cutler and Zeckhauser 2003 and Ho and Pakes 2014). The problem with such an approach is that capitation often exists along with other cost-control methods, like narrow network, utilization authorization, and selected covered benefits, etc. (Glied and Zivin 2002). We offer new insights by leveraging episode-level variation in capitation and use the plan-year fixed effects to separate the effects of capitated contracts from other cost control methods. We also contribute to the literature by considering employer sponsored plans of a large-scale national sample, as opposed to a specific state (Ho and Pakes 2014) or Medicare/Medicaid (Duggan 2004).

More broadly, our work advances the literature studying physician behaviors and organization of care. Recent works show 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), episode-based payment (Carroll et al. 2018), etc. Our results show 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 highlights the background information of 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 glutaeal 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 U.S. 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 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 sometimes not associated with clinically meaningful benefits and can even bring harm. At the same time, many imaging tests poorly predict which patients will benefit from surgery (Chou et al., 2011; Goodney et al., 2015). Meanwhile, the utilization of imaging tests is high in the U.S. For instance, Schwartz et al. (2014) estimated that annual Medicare spending on imaging for uncomplicated LBP ranged 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 practice for the treatment of LBP. The treatments of LBP include medications, non-interventional treatments such as physical therapy and exercise programs, and interventional spine surgeries and procedures. Surgical procedures range from well-established approaches for discectomies and/or 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.[3] At the same time, the utilization of LBP surgeries increases over years. 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 physical therapy's benefits 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 that there exists a large variation of whether to use and when to use physical therapies across physicians.

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 the healthcare expenditures, payers may replace the fee-for-service payment model with the capitated payment model by paying physicians based on the number of patients they

[3] See Friedly, Standaert and Chan (2010) for a review of the literature.

treat instead of the volume of services they prescribe. Capitation contracts are most common with HMO plans and is less common with PPO plans. On the other hand, even among HMO plans there can be large variation in whether capitation contracts are used. For instance, according to (Zuvekas and Cohen, 2010), only 15%-33% of physician office visits for the private HMO plan enrollees are under capitation arrangement.

The forms of capitation payments can vary. One extreme is the global capitation payments, which bundles all providers and covers the cost of all services received by patients, including inpatient hospital stays. Another extreme is that the payment only covers the services provided by the primary care physician or physician group. But this 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 deviates from the traditional pay-for-volume and generates incentives for physicians to share the financial risk of patients' entire treatment episodes.[4]

The capitation payment model appears in the 1980s and thrives with the proliferation of HMOs. The rate of capitation payment among physicians has decreased since the early 2000s (Zuvekas and Cohen, 2010). But in recent years, new reforms towards bundled payment model and Medicare Accountable Care Organization initiatives are all variations of the capitation idea to create financial incentives for physicians to curb the medical expenditures (Friedberg et al., 2015).

3 Empirical Strategy

3.1 Data and Sample

The dataset we use is the Truven MarketScan Commercial Encounters and Encounter Data from the year 2003 to 2006.[5] It is a large commercial insurance claim dataset based on the working-age US population. For each claim record, the dataset provides diagnosis and procedure codes and detailed payment information. We directly observe whether a claim payment is paid under capitation or not. This allows us to compare the effect of capitation payment on treatment intensity. The Truven MarketScan Data also tracks enrollees over time. We thus can observe the full history of medical service use of an individual. We also observe other demographic and socioeconomic characteristics, including age, gender and employment status.

We construct a sample of patients with non-emergency lower back pain (LBP) related

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

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

episodes. To build this sample, we primarily identify a patient's claim encounters with LBP-related diagnoses, and assign these encounters into episodes by time.[6] For a patient, an LBP episode starts from his/her earliest LBP encounter, followed by subsequent encounters with a time gap shorter than 180 days. An episode ends if there is no additional LBP encounters within 180 days of the last record. Two consecutive LBP encounters with larger than 180-day gaps are designated to two separate episodes.[7] We then keep only episodes started from primary care office visits.[8] Finally, we drop individuals who are pregnant women, or with an age under 18 or over 65, or 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 the treatment.[10] In total, our 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 the primary care 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 the 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 whole episode. A primary care physician can decrease his/her treatment intensity for capitated patients (for example, prescribing less physical therapy session for his/her capitated patients), or refer patients to the specialties who are less likely to prescribe expensive treatments or of lower costs. [12]

Patients in capitated plans seem to be healthier. The sample defined above includes 10,274

[6] We follow Cherkin et al. (1992*a*) to define the LBP diagnosis. The specific LBP ICD-9 diagnosis codes are presented in Table 10.

[7] Most individuals have one episode during the sample period. In Appendix Table 9 we show the robustness of our main results using episodes defined based on a 90-day window rather than 180-day window.

[8] Episodes started from surgical treatment or urgent care may be different from the episodes started by primary care physicians. They could either be more acute, or part of the episode is not included in the dataset.

[9] The chronic conditions we rule out include either of the following: 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, hip/pelvic fracture.

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

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

[12] Capitation status of primary care physicians and the downstream providers (like radiologists, surgeons, and therapists are positively correlated in our data. One should think of the treatment effects as not only from the capitated primary care physicians, but represent the capitation status of the whole episode.

capitated LBP episodes and 71,882 non-capitated episodes. Table 1 compares the patient individual characteristics of capitated and non-capitated episodes. The patients in capitated plans are slightly younger than their non-capitated counterparts, and they are less likely to have chronic conditions. We also find capitated patients are more likely to be paid hourly and work as part-time than the rest.

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 insurer payment to the provider and includes consumer cost-sharing. It represents the overall resources used for each procedure, but also incorporates price variation among insurers and providers. Since our focus is on understanding utilization patterns, we want our measure of treatment intensity to reflect only differences in service utilization but not any differences in prices for services across different contract. To eliminate the role of prices, we calculate the average price of each procedure by regressing price on age, gender and chronic conditions of patients. Patient characteristics like age, gender and chronic conditions may affect the resources used so we control for them. In this estimation step, we only use the claims from non-capitated 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 bi-model distribution and is very 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. We also show in the Appendix Table 7 and 8 that 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, other surgeries.[13] We also construct a dummy variable d indicating whether each type of services are used at all in an episode. In our sample, every observation will have an office visit but some may not have other services.

Table 2 shows the summary statistics of the outcome variables. We can see that the average treatment intensity for all services within an LBP episode for capitated patients is around \$438, while that of non-capitated patients are round \$590. The average treatment intensity is significantly higher for non-capitated patients for nearly all categories services except for back surgery. Meanwhile, the out-of-pocket expenditures of non-capitated patients are also significantly higher regardless of the categories of services they received.

For 75% episodes of our baseline sample, we also 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 two most common types of drug: opioids and muscle relaxants. To do so, we group drug claims by national drug code. We then calculate the average per-day price for each drug by year. Finally, we multiply the average price by the number of days of supply to get the per-drug spending. The episode-level total drug usage is the sum of the spending of all drugs. This usage measure takes the same price for a specific drug across different plans and insurers, and will only reflect usage difference but not price difference.

3.3 Regression Model

In Section 3.1, we show that 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,

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

employment status, and existence of chronic conditions:

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

We also include time trend t to control for aggregate movement in treatment style over time.[14]

One important channel where selection might happen is different patients may choose different plans. For example, capitation contracts are more common in HMOs than in PPOs. The former also impose other cost-control tools, like 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, patient 1 chooses plan A in year 1, plan B in year 2, patient 2 chooses plan A in year 1, plan B in year 2, and patient 3 chooses plan C in year 1, plan B in year 2, then only patient 1 and 2 will be classified as in the same group, and patient 3 will be in another group. We call the plan group classification as plan fixed effects henceforth. We estimate the following equation

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

where y is either the log treatment intensity measure or a dummy variable indicating whether a certain service is used. X is the demographic characteristics of patients, and γ_g is dummies for different plan groups. t is the linear time trend. The estimated β_2 captures the treatment difference for capitated and non-capitated patients with similar demographic characteristics and 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 capitated physicians based on their health status, because they often cannot observe the specific capitation arrangement of a physician within a plan. Second, employers may switch plan types over time. For example, employer may offer an HMO in year 1 and switch to PPO in year 2, while the same set of employees stay in the risk pool. Controlling for plan fixed effects, in this case, is similar to controlling for "individual" fixed effects over time, while "individual" is identified as people in the same plans over time. Under the assumption that both sources of variation is not affected by active patient selection,

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

β_2 will identify the true treatment effects of capitation arrangement on treatment decisions.

We could further decompose the treatment effects estimated in 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:

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

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

In model (4), we control for plan-year fixed effects. So the variation in capitation only comes from the contract differences within a plan-year, and there is no cross-time variation. One benefit of this model is that it also separates effects of capitation from other cost control methods varies at plan level. Often capitation happened along with other supply-side cost-control methods, like utilization authorization, referral restriction etc. These measures, however, usually varies 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 only identify the net effects of capitation.

In model (5), we calculate the average capitation rates within a plan-year, CAP_{gt}, and use it 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 towards capitated arrangement and treatment philosophy, and 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:

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

where y is either the log treatment intensity measure $IHS(t)$ or a dummy variable indicating whether a certain service is used. δ_s are dummies for different providers. β_3 will then capture the difference in treatment decision for capitated and non-capitated patients treated by the same provider.

Similar to the plan fixed effects model, the coefficient of capitation within a physician, β_3, is identified based on two types of variation. 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 physicians' treatment style is correlated with their decision into capitation, 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 decision to switch between capitation contracts, the model will identify the true treatment effects.

We could further decompose the treatment effects we estimated in 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:

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

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

Note that in model (7, β_{31} identifies the differential treatment of the same physician in the same year, and is not identifying the effects if a physician switch from no capitation to only treating capitated patients. Anecdotal evidence shows 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 effects in this model may not indicate the true treatment effects is zero. Model (8) will identify how treatment decision will change over time when physicians move from less 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:

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

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

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. In (9) we control them 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 will identify the effects of capitation using cross-time variation within a plan-provider plan.

Even though we cannot directly examine whether these fixed effects model removes all selection concerns, we can at least show that they reduce selection on observable patient characteristics. Figure 1 compares the likelihood of having chronic conditions among capitated and non-capitated patients. The first panel on the left is the raw mean differences. Capitated patients 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, capitated patients are 7.5% less likely to have high blood fat (hyperlipidemia) compared to non-capitated patients under no controls. The estimated difference for hyperlipidemia decreases to 4% once we add plan or provider fixed effects separately, and is 2.6% when we include both plan and provider fixed effects. If non-observables are in similar nature of 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 is to estimate a model without controlling for individual characteristics:

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

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

4 Results

4.1 Treatment

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

Using Any Medical Services Figure 2 presents the results of how the capitation status impacts the likelihood of patients using a certain category of service or not. The dependent variables of Panel 1 to Panel 4 are the dummy variables of using therapy/back surgery/other surgeries/diagnostic testing within a lower back pain (LBP) episode.[15] Within each panel, from bottom to top are five 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 characteris-

[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.

tics, and provider fixed effects ("provider"); controlling for year, patient characteristics, plan and provider fixed effects ("provider, plan").

Patients in capitated episodes are less likely to have therapy and diagnosis testing. We observe that without any control variables, the capitated patients are 4.1% less likely to use any therapy treatment. With patient characteristics controlled, 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%. Finally, there exists a 2.1% difference in the likelihood of using therapy between capitated and non-capitated patients if we control for provider and plan fixed effects at the same time.

For diagnostic testing, controlling for the provider and plan fixed effects leads to a difference around 4% between capitated and non-capitated compared. 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 whether utilizing back surgery/other surgeries. This may due to the reason that the surgeries are mainly for very severe patients and are not excessive treatments that could be removed.

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 without any controls, the capitation patients utilizes 25.2% fewer medical resources compared with non-capitated patients. 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 the capitation results in a difference of 10.2% in the overall treatment intensity. In column (4), we control for provider fixed effects and patient characteristics. The difference in treatment intensity between capitated and non-capitated 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. In column (6) we show that once we remove patient characteristics we get a 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 identified based on cross-time variation within a provider and plan pair. For this specification we are not able to use all of the observations in our sample and the standard errors are larger, yet we still find a modest reduction in treatment intensity (though not significant.)

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 show that the 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.

We show the effect of capitation on the treatment intensity of all services by cross-sectional and cross-time variation in Table 4 and 5. Table 4 separate capitation variation within a plan into the cross-section variation and 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 capitated patients is 9.6% lower than non-capitated patients. 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 get the same capitation status and takes the plans' variation across years to identify the effect of capitation. The result shows a plan change from 0% of capitation to 100% capitation would lead to a 32.1% reduction in the treatment intensity. Column (4) change the plan-year's average capitation rate to a dummy of whether a plan-year has any capitation arrangement or not. And 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 as Column (4) in Table 3. The cross-time variation in providers' capitation status is absorbed in Column (2) by adding the provider $\times$ year fixed effect. This specification does not find a significant effect of capitation on the 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 provider capitation status by using the provider-year's average capitation rate as the regressor. Under this specification, we find that the capitation on average results in a 10.7% reduction in the treatment intensity. Finally, when we replace the average capitation rate in Column (3) to whether a provider-year has any capitated arrangement as in Column (4), we find that the adoption of capitation status on average reduces the treatment intensity of all services by 9.2%.

Drug Utilization Figure 4 shows the results with drug utilization. Panel A analyzes the effect of capitation on whether there is any LBP-related drug claims in an episode. The three specifications from bottom to top are the difference without any controls, controlling for the individual characteristics, and simultaneously controlling for individual characteristics and plan fixed effects. We find that when controlling for the patient individual characteristics, time trend, and plan fixed effects, the capitated patients 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 drug usage intensity. After controlling for the patient individual characteristics, time trend, and plan fixed effects, the capitation results in an 11.9% reduction in the drug usage intensity of all drugs. Capitation also reduces opioids and muscle relaxants usage by 2.6% and 10% respectively.

4.2 Placebo Test: ER Visit

The previous results show evidence that the capitation status of providers leads to reductions in the LBP's treatment intensity. In this section, we use emergency room (ER) visits as a placebo test. We analyze the ER visits unrelated to LBP conditions of the patients in our sample.[17] Different from the LBP, ER visits are typically initiated by patients with urgent conditions needing immediate care. They should be less affected by primary care physicians.

For the same patient-episodes we identified in our baseline analysis, we construct several measures on the ER services utilization. First, we construct a dummy variable indicating whether a patient have 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 5 levels). We thus construct the 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

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

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

ER visits for all ER visits and ER visits with most severe conditions respectively. Finally, we construct a measure of the overall ER visit treatment intensity based on a weighted average of number of procedures performed, where weights are calculated by average payment.

Figure 6 shows that the capitation status of primary care physicians have almost no impacts on ER services utilization. Patients with a capitated primary care physician are slightly less like to use any ER services (1%) under no controls, indicating selection into these services are 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 controlled 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 confounding factors that might affect the treatment intensity for all services.

4.3 Financial Outcome: Out-of-Pocket Expenditures

We examine the effects of capitated contracts on patients' expenditure by comparing the out-of-pocket spending of patients treated by capitated physicians or not. The difference in out-of-pocket spending can come from three sources: difference in treatment decisions, differences in the price insurers paid to providers, and the difference in consumer cost-sharing. Capitated contracts may involve less treatment. As a result, insurers may pay less to the providers overall. Insurers may also pass a higher proportion of the cost savings to consumers. In fact, literature has argued that supply-side cost control methods like capitation are often substitutes for demand-side cost-sharing (Ellis and Zhu 2015). So consumers under capitated plans may face even lower spending as a result of more generous coverage.

Unfortunately, capitated claims often do not record the price insurers paid. This is because in nature capitated physicians are compensated based on multiple services and may share their payments with others, and it is difficult to map the true compensation into itemized services. As such, we are not able to accurately measure the price effects. Instead, we focus on the difference in out-of-pocket spending as these measures are accurately documented. For each episode, we aggregate consumer payment towards deductible, copayments and coinsurance. We also separate them into different categories. Finally, we take the inverse hyperbolic sine transformation of the out-of-pocket spending.

Table 6 presents the results with the out-of-pocket expenditures for all services. Column (1) shows that capitation patients' out-of-pocket payments are 73.1% lower compared with non-capitated patients. Column (2) addresses the observable patient heterogeneity by controlling for the patient individual characteristics and shows that capitation patients pay 64.9% less in their out-of-pocket expenditures. When we further control the plan level unobservables by adding plan fixed effects in Column (3), we observe a 19.4% difference between capitated and non-capitated patients. In column (4), patient characteristics and provider fixed effects are controlled, and the within-provider capitation status difference leads to a 59.3% decrease in the out-of-pocket expenditures. In Column (5), we use patient individual characteristics, plan fixed effects, and provider fixed effects. It indicates that the capitation of primary care physicians leads to a 32.5% decrease in the out-of-pocket expenditures for all services. Compared with Column (5) in Table 3, capitation's savings on out-of-pocket expenditures are larger than the savings on treatment intensity by more than 20%. This suggests that given the same treatment intensity, capitated patients pay less compared to patients in non-capitated plans.

Figure 5 shows the estimated results by service categories. When no patient and provider heterogeneity are controlled, the out-of-pocket expenditures are significantly lower for capitated patients, regardless of service categories. However, when patient individual characteristics, plan fixed effects, and provider fixed effects are included, the capitation status of primary care physicians mainly influences office out-of-pocket expenditures on office visits by decreasing 27.2%. We do not observe significant differences in other categories. The reason could be that the largest fraction of out-of-pocket expenditures is spent on physician office visits.

4.4 Readmission Rates

In the previous sections we show that there is modest treatment differences between patients treated by capitated physicians and the rest, especially for therapy and diagnostic testing. A natural question is whether this treatment difference represents a reduction in over treatment, or indication of under treatment by capitated physicians. Understanding the question is important in thinking of 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 reduction in treatment for capitated patients lead to an increase of readmission of lower back pain for them, then the differences may reflect underuse of valuable care. If capitated patients have similar likelihood of having a lower back pain diagnosis in the future, then it indicates capitation could reduce over treatment.

We construct readmission measures by tracking patients in our sample over time. We are able to track about 65% of our baseline sample over the next four years. Among them, we examine whether they have any LBP-related diagnosis in the next 1 to 4 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 7 shows the results. In general, after we control for individual characteristics and plan fixed effects, capitated patients are either slightly less likely to be readmitted for LBP or the differences are not significantly from zero.

5 Conclusion

In this paper, we study how capitated payment models affect the treatment of lower back pain in employer-sponsored health insurance plans. We show that patients 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 like muscle relaxants, but have almost no effects on surgeries or opioids. Our identification relies on the panel feature of the claim data. Although patients rarely have multiple lower-back pain episodes, we identify a group of people with the same plan enrollment 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 lower back pain because there is evidence from the medical literature that many of the services have low value to patients. We find that capitation leads to differential treatment for otherwise similar patients, suggesting inefficiency in care from either under treatment of capitated patients or over treatment of non-capitated patients. More detailed data is 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 covers a time when capitation payment contracts' popularity declined. Physicians might not respond to the incentive if they did not stay in this type of contract for the following period, if some of the contracts may offer some dynamic incentives. The current movement towards 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 the long-term health status for patients.

When we only use within provider-year variation to identify the effects of capitation on treatment, we find almost zero treatment effects, suggesting physicians do not differentiate care for patients with different insurance in the same period. The treatment effects solely come from variations in the average capitation rates a physician face over time. These findings are consistent with other empirical works showing 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, certain vertically integrated health care systems, like Kaiser, have their providers almost exclusively treat only 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
Observations	10274		71882			

Notes: The table shows the summary statistics of patient characteristics for capitated/non-capitated patients separately.

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

	Capitated		Non-capitated		Difference	
	mean	sd	mean	sd	mean	se
Treatment Intensity						
all	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
Out of Pocket Expenditure						
all	45.95	148.22	129.70	376.75	-83.75	2.03
office visit	25.08	39.41	47.91	81.60	-22.84	0.49
therapy	7.53	50.78	23.36	107.03	-15.84	0.64
back surgery	4.65	92.34	13.08	242.57	-8.44	1.28
other surgery	3.50	32.50	15.53	108.24	-12.02	0.52
diagnostics	4.18	21.20	22.86	130.14	-18.68	0.53
Observations	10274		71882			

Notes: The table shows the summary statistics of outcome variables for capitated/non-capitated patients separately.

Table 3: Treatment Intensity of All Services

	(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)
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 FE				×	×	×	
Plan FE			×		×	×	
Prov × Plan FE							×
Individual Characteristics		×	×	×	×		×

Notes: 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. Column (1) has no control variable. Column (2) controls for individual characteristics (chronic condition, employment status etc.). Column (3) controls for year, patient characteristics, and plan fixed effects. Column (4) controls for year, patient characteristics, and provider fixed effects. Column (5) controls for year, patient characteristics, plan and provider fixed effects separately. Column (6) controls for year, plan and provider fixed effects separately. Column (7) controls for year, patient characteristics, plan and provider fixed effects interactively. Standard errors are clustered at the data contributor level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Cross-sectional & 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)
Observations	81,058	79,849	81,058	81,058
R-squared	0.073	0.087	0.073	0.073
Plan FE	×		×	×
Plan × Year FE		×		

Notes: 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. Column (1) controls for year, patient characteristics, and plan fixed effects. Column (2) controls for patient characteristics and plan × year fixed effects. Column (3) uses the similar specification as Column (1), but replaces the capitation status of each episode with the average capitation rate of the plan-year which the episode belongs to. Column (4) replaces the plan-year's average rate of capitation in Column (3) to the dummy that whether a plan-year has any capitation or not. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Cross-sectional & 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)
Observations	61,370	47,094	61,370	61,370
R-squared	0.326	0.368	0.326	0.326
Prov FE	×		×	×
Prov × Year FE		×		

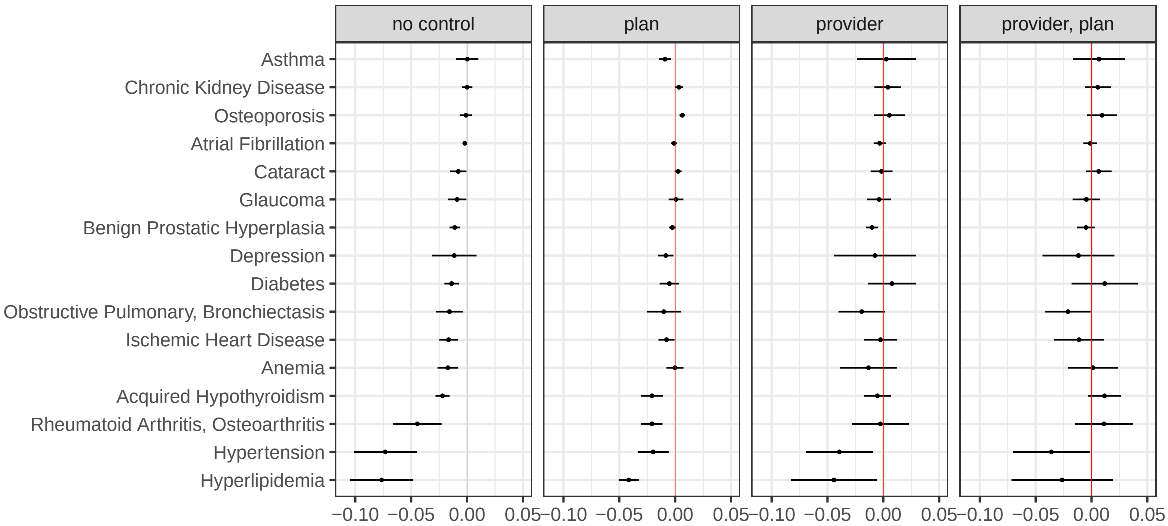
Notes: The table examines the cross-sectional and cross-time variation of provider's capitation status change on the overall treatment intensity. The dependent variable is the inverse hyperbolic sine transformation of treatment intensity of all services. Column (1) controls for year, patient characteristics, and provider fixed effects. Column (2) controls for patient characteristics and provider × year fixed effects. Column (3) uses the similar specification as Column (1), but replaces the capitation status of each episode with the average capitation rate of the provider-year which the episode belongs to. Column (4) replaces the provider-year's average rate of capitation in Column (3) to the dummy that whether a plan-year has any capitation or not. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Out-of-Pocket Expenditures of All Services

	(1)	(2)	(3)	(4)	(5)
capitated	-0.731***	-0.649***	-0.194***	-0.593***	-0.325***
	(0.136)	(0.177)	(0.060)	(0.103)	(0.102)
Observations	82,156	82,156	81,058	61,370	60,206
R-squared	0.023	0.049	0.205	0.365	0.432
Prov FE				×	×
Plan FE			×		×
Individual Characteristics		×	×	×	×

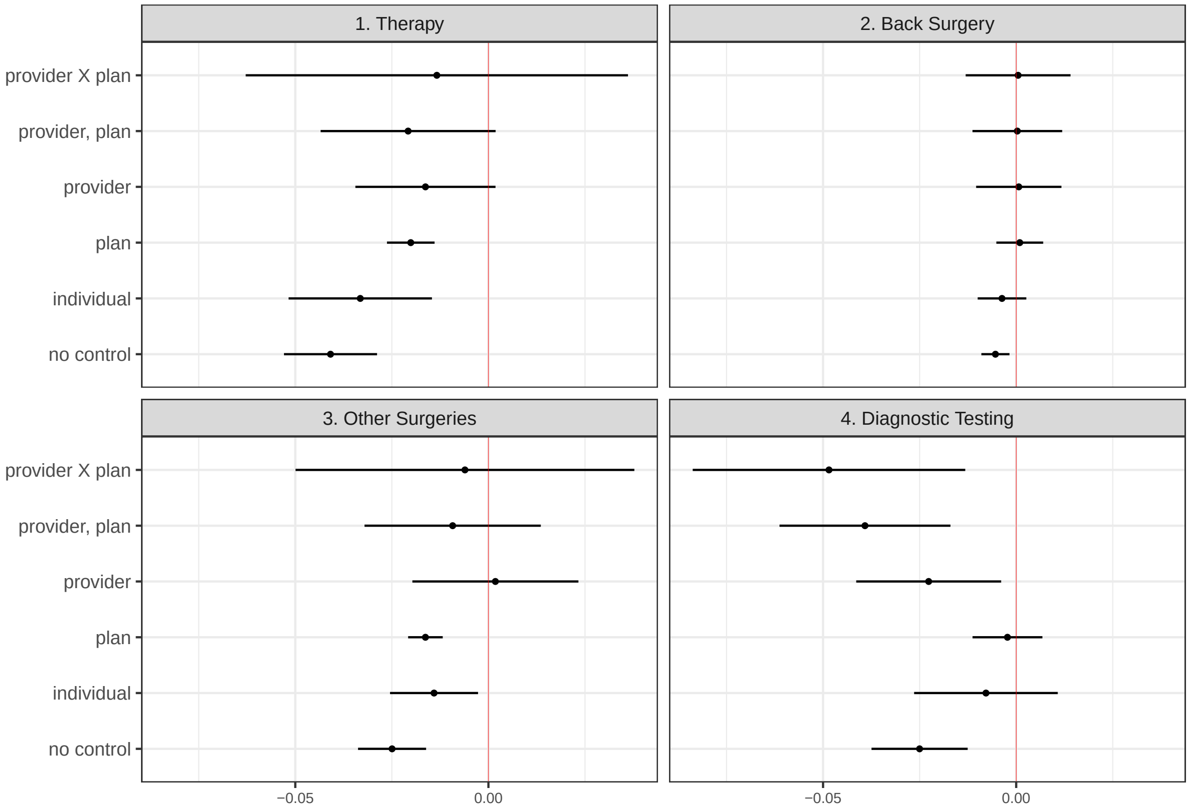
Notes: The table shows the regression results comparing the out-of-pocket expenditures of capitated/non-capitated patients. The dependent variable is the inverse hyperbolic sine transformation of out-of-pocket expenditures of all services. Column (1) has no control variable. Column (2) controls for individual characteristics (chronic condition, employment status etc.). Column (3) controls for year, patient characteristics, and plan fixed effects. Column (4) controls for year, patient characteristics, and provider fixed effects. Column (5) controls for year, patient characteristics, plan and provider fixed effects. Standard errors are clustered at the data contributor level. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Chronic Condition Rate Differences between Capitated/Non-capitated Patients



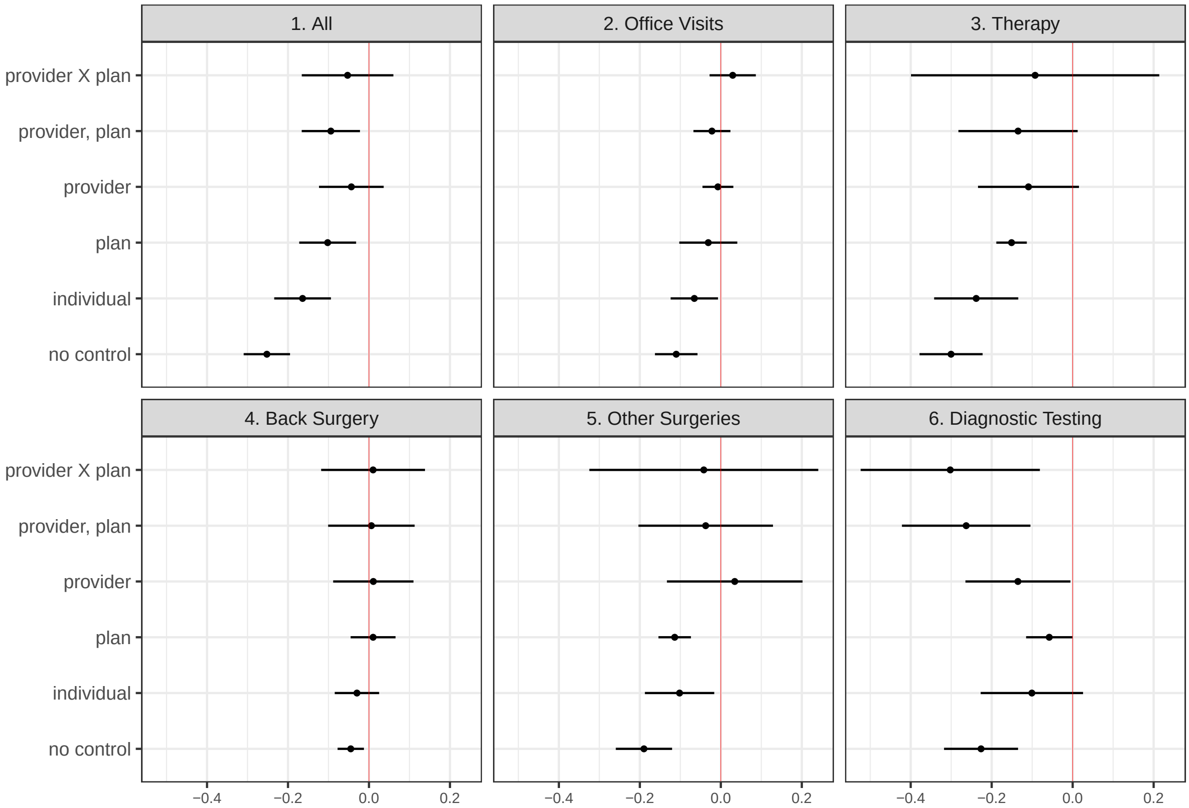
Notes: The figure shows the differences of chronic condition rates between capitated/non-capitated patients. The four panels from left to right indicates no controls, controlling for year, age and plan fixed effects, controlling for year, age and provider fixed effects, and controlling for year, age, plan and provider fixed effects.

Figure 2: Extensive Margin: Using Any Medical Services



Notes: The figure shows the estimated results that whether capitated patients use certain type of services compared to non-capitated patients. The dependent variables are the indicators of using any services of certain types. Panel 1 to 4 examines 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.

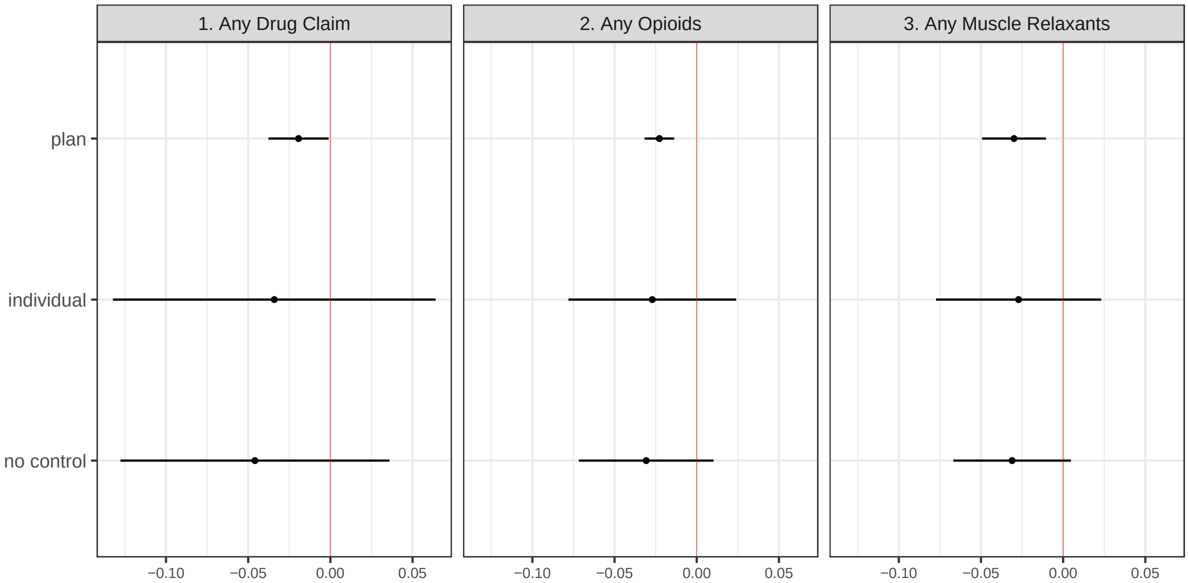
Figure 3: Treatment Intensity of Medical Services



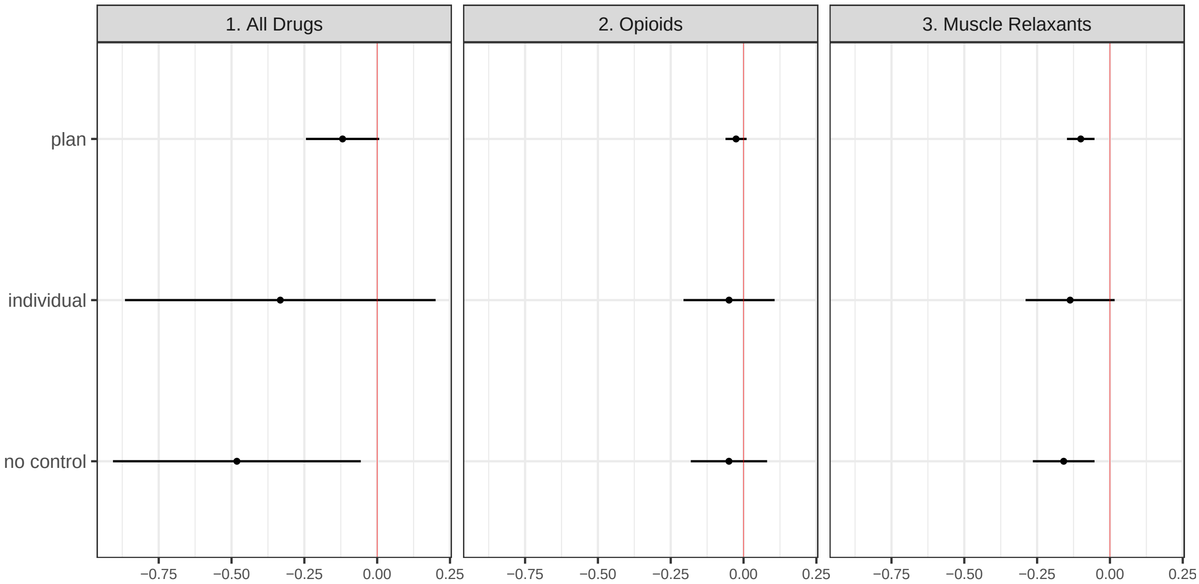
Notes: The figure shows the estimated results studying the treatment intensity differences of capitated and non-capitated patients. The dependent variables are the inverse hyperbolic sine transformation of treatment intensity. Panel 1 to 6 examines the effects with all medical services, office visits, 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

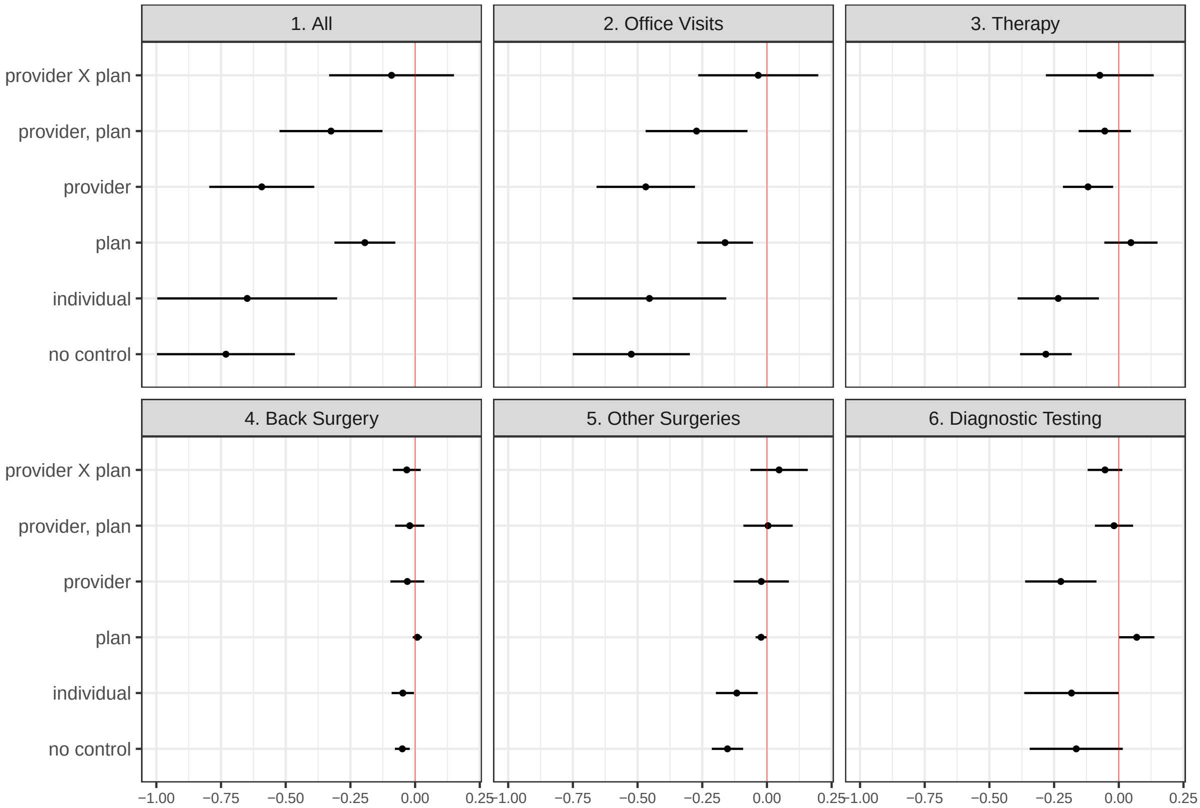


Panel B: Drug Usage Intensity



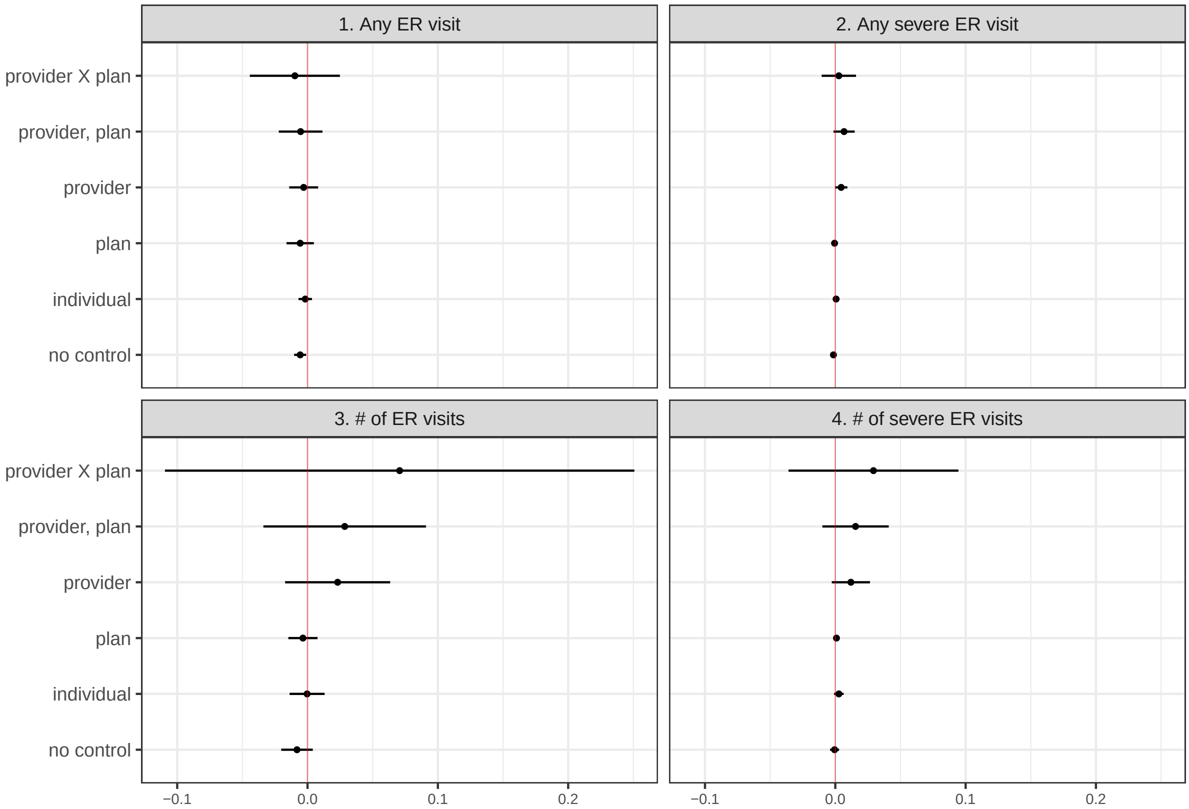
Notes: The figure shows the coefficient estimates of capitation and the 95% confidence interval. In Panel A, the dependent variable is whether there is any claim. In Panel B, the dependent variables are the inverse hyperbolic sine transformation of treatment intensity. Sub-panel 1 to 3 examines the effects 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: Out-of-Pocket Expenditures



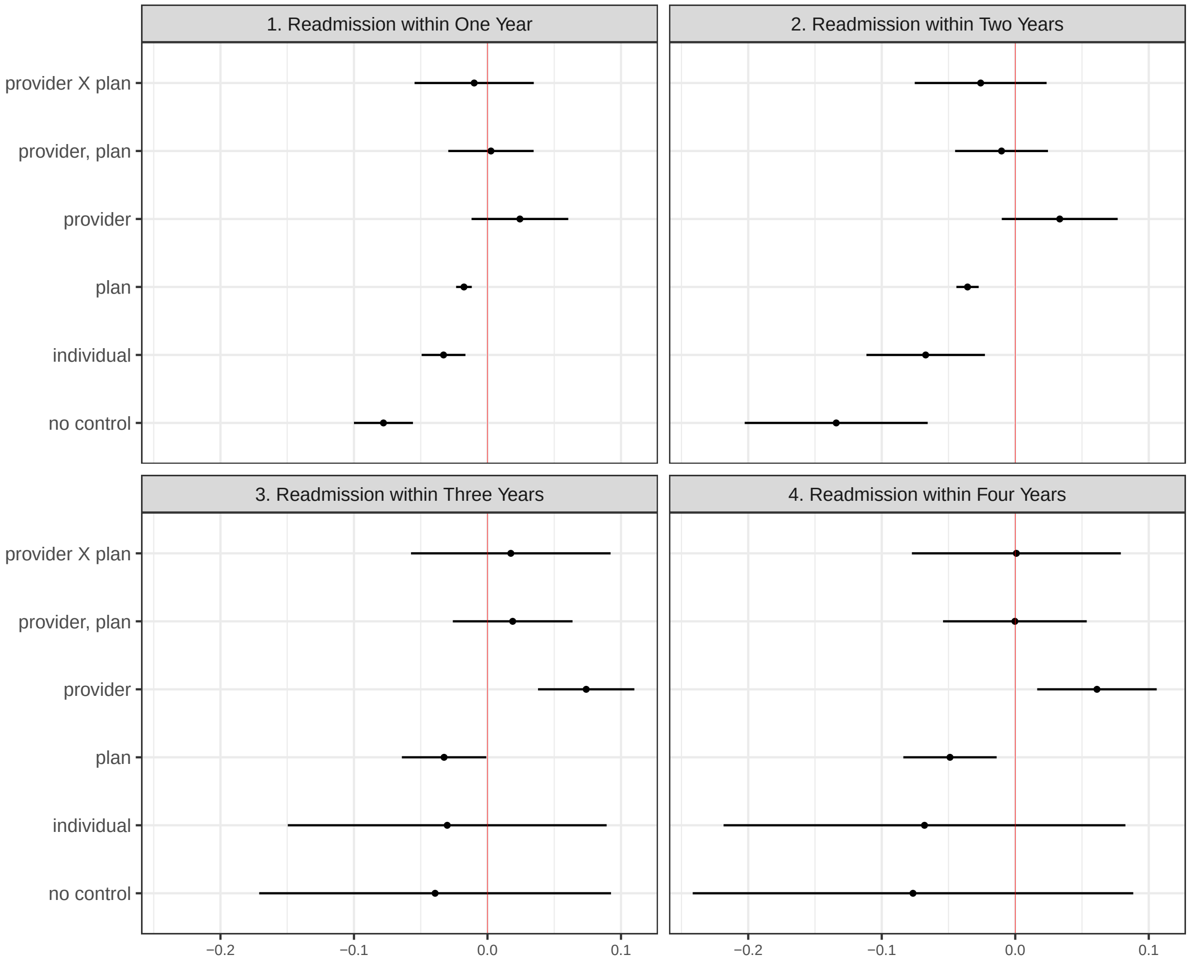
Notes: The figure shows the estimated results studying the out-of-pocket expenditure differences of capitated and non-capitated patients. The dependent variables are the inverse hyperbolic sine transformation of out-of-pocket expenditures. Panel 1 to 6 examines the effects with all medical services, 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 6: Placebo Test: Emergence Room Visits



Notes: 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 7: Readmission Rates



Notes: The figure shows the differences in readmission rates of lower back pain for capitated and non-capitated 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 7: 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)
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		×	×	×	×

Notes: The table shows the regression results comparing the treatment intensity of capitated/non-capitated patients. The dependent variable is the treatment intensity of all services. Column (1) has no control variables other than capitation. Column (2) controls for year and patient individual characteristics. Column (3) controls for year, patient individual characteristics, and plan fixed effects. Column (4) controls for year, patient individual characteristics, and provider fixed effects. Column (5) controls for year, patient individual characteristics, plan and provider fixed effects. Standard errors are clustered at the data contributor level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Out-of-Pocket Expenditures of All Services, Raw Values

	(1)	(2)	(3)	(4)	(5)
capitated	-83.748***	-78.962***	-8.171***	-43.085***	-3.513
	(11.911)	(17.711)	(1.786)	(9.678)	(5.742)
Observations	82,156	82,156	81,058	61,370	60,206
R-squared	0.006	0.018	0.060	0.258	0.291
Prov FE				×	×
Plan FE			×		×
Individual Characteristics		×	×	×	×

Notes: The table shows the regression results comparing the out-of-pocket expenditures of capitated/non-capitated patients. The dependent variable is the out-of-pocket expenditures of all services. Column (1) has no control variables other than capitation. Column (2) controls for year and patient individual characteristics. Column (3) controls for year, patient individual characteristics, and plan fixed effects. Column (4) controls for year, patient individual characteristics, and provider fixed effects. Column (5) controls for year, patient individual characteristics, plan and provider fixed effects. Standard errors are clustered at the data contributor level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: 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)
Observations	87,838	87,838	86,790	67,833	66,784
R-squared	0.003	0.030	0.062	0.308	0.335
Prov FE				×	×
Plan FE			×		×
Individual Characteristics		×	×	×	×

Notes: The table shows the regression results comparing the treatment intensity of capitated/non-capitated 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. Column (1) has no control variable. Column (2) controls for individual characteristics (chronic condition, employment status etc.). Column (3) controls for year, patient characteristics, and plan fixed effects. Column (4) controls for year, patient characteristics, and provider fixed effects. Column (5) controls for year, patient characteristics, plan and provider fixed effects. Standard errors are clustered at the data contributor level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: 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

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