Operating on commission: How physician financial incentives affect surgery rates

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Abstract

This paper employs a nationally representative, household-based dataset in order to test how the compensation method of both specialists and primary care providers affects surgery rates. After controlling for adverse selection, I find that when specialists are paid through a fee-for-system scheme rather than on a capitation basis, surgery rates increase 78%. The impact of primary care physician compensation on surgery rates depends on whether or not referral restrictions are present.

1 Introduction

The question of how financial incentives affect physician decision-making has been a frequent subject of investigation in both the economics and medical fields. Are doctors perfect agents for their patients, solely basing their medical care decisions on what is in the patient's best interest, or do physicians behave as *homo economicus*, strictly acting in a profit maximizing fashion? While many studies have focused on the relationship between financial incentives and primary care services, the treatment of specialist services has been inadequate. The topic of specialist care is particularly important as more and more graduates of U.S. medical schools are choosing to practice in specialty fields (Newton and Grayson, 2002). This paper explains how the joint financial incentives of the specialist and primary care physicians affect surgery rates.

In this study, I test two hypotheses. The first, or direct, hypothesis predicts that changing specialist compensation from capitation to fee-for-service will increase surgery rates. I find that financial incentives do significantly influence patient surgery frequencies. If a specialist is compensated via fee-for-service (FFS) as opposed to capitation, surgery rates increase by approximately 78%. In the outpatient setting, changing physician compensation increases surgery rates by 84%.

The second, or indirect, hypothesis examines the relationship between primary care compensation and surgery rates. If the quantity of primary care services influences the quantity of specialist services, one must take into account how primary care physician compensation affects the level of primary care services and, indirectly, the number of surgeries. Primary care can act as either a substitute or complement for specialist care. For example, it is possible that a drug administered to the patient within the primary care setting may be a substitute for surgery provided by a specialist. In some cases, having the primary care physician (PCP) prescribe the drug may negate the need for the specialist to perform a surgery. In other cases, primary care may be a complement to specialist care. For instance more frequent primary care visits may increase the likelihood of catching breast cancer at an early stage where surgery would be beneficial. If patients of fee-for-service PCPs have more office visits, I would observe higher rates of surgeries among patients with FFS PCPs compared to patients with PCPs paid via capitation. Economic theory predicts that increasing primary care services should decrease (increase) specialist services if the two are net substitutes (complements).

My analysis indicates that the impact of PCP compensation on surgery depends on whether or not a patient's health insurance policy includes referral restrictions. In the absence of referral restrictions, primary care and specialist care are net substitutes. Changing the PCP's compensation from FFS to capitation increases surgery rates by 35% when the health plan does not apply referral restrictions. On the other hand, in the presence of referral restrictions, no statistically significant relationship appears between PCP compensation and surgeries. The presence of referral restriction likely tempers the substitutability of primary care and specialist medical services.

While previous studies have examined the relationship between physician compensation and specialist care, this study is unique because it uses nationally representative household data. Shrank et al. (2005) and Brinker et al. (2006) analyzed the connection between financial incentives and specialist care, but use patient visit data that is subject to bias from non-random sampling. Unlike most studies of specialist medical service provision, I use nationally representative data from the Community Tracking Study (CTS). The CTS includes not only survey response variables, but also physician compensation data collected directly from the patient's health plan. Relying on direct measures of physician compensation rather than proxies such as an HMO insurance dummy variable allows more accurate analysis of the influence of physician financial incentives. Taking into account both primary care and specialist physician compensation facilitates a more comprehensive analysis of the data.

The remainder of the paper proceeds as follows. Section 2 reviews the literature's findings about how financial incentives affect medical service provision. Also discussed are methods used in the literature to control for adverse selection and how I employ these techniques in this paper. Section 3 develops a theoretical framework which generates two testable hypotheses. Section 4 describes the Community Tracking Study (CTS) 1996/1997 Restricted Use data file. Section 5 describes the how the hypotheses derived from the theoretical framework will be tested econometrically and section 6 shows the results of these tests. A concluding discussion can be found in section 7.

2 Literature Review

The manner in which financial incentives affect the provision of medical services has been studied extensively in both the economics and medical fields. The most recognized of these studies is the RAND Health Insurance Experiment (HIE). In one portion of the RAND HIE, households were randomly assigned between fee-for-service or prepaid group plans (Manning et al. 1987). Manning and co-authors concluded that patients in prepaid group practice plans had only 72% of the expenditures of those in fee-for-service plans. Other randomized trials such as Hickson, Altemeier, and Perrin (1987) and a randomized survey by Shen et al. (2004) found similar results. A review paper by Gosden et al. (2000) examined this literature in more detail.

While these randomized trials provide strong evidence that financial incentives matter to physicians, they are not without problems. The Manning and Hickson studies are over 20 years old and neither is nationally representative. The Shen paper is more recent, but the study used hypothetical survey responses rather than real-world data. Further, none of these papers examine on the connection between specialist compensation and the quantity of specialist care provided. Most importantly, these studies ignore interaction effects between PCP and specialist compensation.

Myriad studies have been conducted outside the RCT framework. Seminal work by Akerlof (1970) and Rothschild and Stiglitz (1976) suggests that when operating outside of the RCT setting, researchers must be mindful of adverse selection issues. In the medical setting, adverse selection can occur when individuals use their private information about their health risks to select physicians with varying compensation methods in ways that are correlated with this underlying risk. In the presence of adverse selection, it may appear that patients of capitation-reimbursed physicians utilize fewer medical services because of their compensation structure, when in reality, the reason for the lower patient utilization may be a healthier patient base. Evidence demonstrating the existence of adverse selection has been documented within Medicaid (Leibowitz, Buchanan, and Mann, 1992), Medicare (Dhanani et al., 2004), and employer-provided insurance plans (Cutler and Reber, 1998; Buchmueller, 2000). Since this paper relies on observed insurance type rather than random assignment, it is imperative that I address the issue of adverse selection.

There are three prominent methods used to deal with adverse selection. The first method is to control for the selection problems using observables. The use of primary diagnosis (Brinker et al. 2006), Diagnostic Cost Group (Yu, Ellis and Ash, 2001) or self reported health measures (Ettner 1997) is common. A second group of papers uses variation in plan compensation occurring due to a 'natural experiment.' Studies employing the 'natural experiment' framework have utilized changes in primary care compensation in England (Dusheiko et al., 2006), in Ireland's 'medical card' population (Madden et al., 2005), and within certain U.S. medical groups (Gosden et al., 2000). This 'quasi-experimental' approach is very appealing though it generally restricts the scope of the study to individuals affected by a government or employer policy change.

A final effective means to control for adverse selection is to examine a subpopulation of the data that has a restricted choice of insurance plans. Using the Community Tracking Study—the same dataset utilized in this paper—Polsky and Nicholson (2004) and Nicholson et al. (2004) analyzed medical expenditure data for privately insured individuals who only received one choice of health insurance from their employer. According to Pauly and Percy (2000), nongroup insurance plans tend to have significantly higher load factors, provide less generous benefits, and are at a significant tax disadvantage compared to group policies. Consequently, for most individuals who have the option of only one type of health insurance at their place of work, this is their only *de facto* health insurance option. Adverse selection should not be a problem for individuals offered only one choice of health insurance at work.

This study significantly reduces the adverse selection problem using two of the these three methods. First, I control for observable health level using a detailed, twelve component measure of each individual's health. Second, I limit the data to a subsample of individual whose employers offer them only one choice of health insurance. Since the dataset comes from a single year, longitudinal analysis employing a 'natural experiment' policy shock is infeasible using the CTS data.

Among the few papers using household data that examine specialist service provision and adequately control for adverse selection are papers by Polsky and Nicholson (2004) and Saver et al. (2004). The Polsky and Nicholson paper analyzed how HMO-membership affects total expenditures, as well as hospital admissions. Limiting their sample to employed individuals offered only one choice of insurance from their employer, the authors found that the $188 difference between HMO and non-HMO medical expenditures per enrollee was explained by differences in physician compensation. HMOs, however, are only a proxy for physician compensation since many HMOs pay some physicians on a capitation basis and others on a FFS basis. Saver et al. (2004) compared the procedure frequencies within three different HMOs that paid some doctors on a FFS basis and others on a salaried or capitation basis. The authors found that switching from capitation to FFS compensation led to a 28% increase in the medical care provision rates and switching from salaried to FFS compensation led to a 44% increase. Unfortunately, the Saver study did not employ city or region fixed effects despite the authors' claim that variation in payment methodology within each HMO was largely due to local market considerations. Furthermore, the paper only looked at three HMOs in the western United States, and thus the paper is not nationally representative.

3 Theoretical Framework

Economic theory predicts that specialist physician compensation should influence the quantity of specialist care a patient receives. Ellis and McGuire (1986) derive a model where physicians choose quantities of medical care in order to optimize a utility function that depends on patient health and the physician's own profit level. The model shows that physicians will under-supply medical care when they are paid via a prospective payment system such as capitation, but will over-supply medical care when they are paid based on a fee-for-service or cost-based paradigm. Intuitively, physicians are more likely to supply medical care when their marginal revenue is positive (i.e., when they are paid via FFS) compared to when the marginal revenue is zero or negative (i.e., when they are paid via capitation). The theoretical prediction from their model leads to my first hypothesis.

Hypothesis 1 (Direct Hypothesis) *Holding all else constant, specialists paid via a fee-for-service (FFS) compensation method will perform more surgeries than those paid via capitation.*

This paper diverges from the standard literature in that it takes a comprehensive view of how medical care is provided. Not only should specialist physician compensation affect surgery rates, but primary care physician compensation should influence surgery rates as well. Under a comprehensive view of the supply of medical care, the quantity of primary care services should influence specialist care. Using similar logic as in the specialist case, PCPs will provide more medical services to their FFS patients than their capitation patients. The conclusion that primary care physicians provide more medical care to FFS patients is widely accepted in the literature (Hickson, Altemeier, and Perrin, 1987; Krasnik et al., 1990; Polsky and Nicholson, 2004; Shen et al. 2004;) but not universally so (Davidson et al., 1992, Frank and Zeckhauser, 2007).

If one accepts that PCP compensation affects primary care levels, then one must also analyze how the quantity of primary care services affects the number of surgeries. If primary care and surgeries are substitutes, an increase in primary care services will decrease the marginal health benefit a patient receives for a given quantity of specialist services. For example, a salubrious drug may decrease the marginal benefit from surgery. When the specialist trades off profits against patient health, he will see that the marginal benefit of any specialist procedure will be lower when the patient is already receiving medication, and thus he will decrease the quantity of specialist care. On the other hand if primary care and specialist care are complements, one would find the opposite relationship. As the marginal benefit of surgery increases with additional quantities of primary care medicine, the specialist will increase the quantity of specialist care. This would be the case if visiting a PCP more frequently leads to a higher probability of an early diagnosis. The early diagnosis could increase the marginal benefit of surgery and also increase the quantity of surgeries per person. Since the quantity of primary care services influences the quantity of specialist care, primary care compensation should indirectly affect the quantity of specialist care provided.

The literature is mixed as to whether or not primary and specialist care are substitutes or complements. Using data from Italy, Atella and Deb (2008) demonstrated that primary and specialist care are in fact substitutes. In Fortney et al. (2005), primary care was found to be a complement for mental health services, a substitute for speciality care, and have a negative but statistically insignificant relationship with inpatient care. It is likely that primary care is a substitute for some forms of specialist care and a complement for other forms of specialist care. With the data available, this paper will only be able to test whether the primary care substitution or complement effect dominates with respect to surgery rates, but can not isolate whether primary and specialist care are substitutes or complements on a procedure by procedure basis.

The above arguments are summarized in the following hypothesis:

Hypothesis 2 (Indirect Hypothesis) *When primary care physicians are compensated via capitation rather than a fee-for-service mechanism, then the amount of surgeries performed will* increase (decrease) *if primary care and specialist care are net* substitutes (complements).

Further, non-financial incentives may influence how PCP care compensation affects the number of surgeries a patient receives. In the absence of referral requirements, the indirect hypothesis can be tested without complication. On the other hand, the presence of referral restrictions may reduce the substitutability between primary and specialist care. Later in this paper, I will examine in detail how referral restrictions affect the results from testing the Indirect Hypothesis.

4 Data

In order to test the two hypotheses outlined above, this paper employs the 1996/1997 Community Tracking Study (CTS) Household Survey Restricted Use dataset. The CTS is a nationally representative household survey of over sixty thousand individuals. The survey methodology uses stratified sampling and all subsequent coefficient estimates and standard errors derived in this paper will account for the idiosyncracies of this survey design. For more detailed information on the survey design of the CTS see: Center for Studying Health System Change (2000).

In general, household surveys have the advantage of providing a representative view of the health of a society. These surveys, however, often have inaccurate data regarding patient insurance information and diagnostic variables and health status is often imprecisely reproted. Survey respondents generally do not know how their physician is compensated. On the other hand studies using the patient-physician encounter as the unit of observation give more detailed diagnostic information and more accurate information regarding physician compensation measures. Yet the physician-visit surveys also have their flaws. These studies often lack important demographic information such as patient income and education levels, and come from a non-representative sample. For instance, surveying individuals who visit a doctor will necessarily produce a sample that is more heavily weighted towards patients who are sick and who prefer more aggressive treatment methods. Furthermore, the number of visits may themselves be affected directly by physician compensation.

The CTS, however, draws from the best aspects of both household and physician visit data. The CTS is a nationally representative household survey. Thus, the data are not overly weighted towards sick individuals or those who treat their illnesses agressively. Furthermore by using the Restricted Use Followback Survey, I am able to observe detailed information regarding patient insurance information. In the Followback section, survey workers collected information regarding an individual's insurance coverage by directly contacting the patient's insurance company. The data collected from the insurance company include variables such as patient deductibles, copayment and coinsurance rates, referral requirements, and—most importantly for this paper—how the insurance company compensates primary care and specialist physicians.

The benefits of having observational data from the individual's insurance company include not only more information, but information less susceptible to measurement error. Using the CTS, Cunningham, Denk and Sinclair (2001) showed that only 30% of individuals were able to correctly answer four basic questions about their own insurance coverage. A study by Reschovsky et al. (2002) found that patient satisfaction with their health plan depended on whether they believed they were in an HMO, not whether they were actually in an HMO. These two studies demonstrate the importance of using observational data when using insurance or physician compensation variables.

In the empirical analysis section of this paper, the data set is limited to only those covered by private insurance in which the Followback survey was able to match the respondent's information with the insurance companies' information. Among the 38,310 observations with private insurance, the match rate was 75%, limiting our survey to 28,578 people. On average, individuals for whom a match was not obtained were poorer, less educated, less likely to be married and more likely to be a minority than those for whom a match was possible. In the subsequent analysis, I use Followback survey weights which correct for this attrition bias.

The sample is then further pared down to include only individuals between ages 18 and 64. The new sample size is 22,958 observations. The elderly are excluded in order to avoid insurance coverage confusion after one becomes eligible for Medicare. Children are excluded because of missing values in important covariates (i.e.: health level, years of education, number of health plans offered by an employer).

5 Estimation Strategy

5.1 Creating the variables

The goal of this paper is to find how different physician compensation schemes affect surgery rates. In order to do this, I create four dummy variables: CC, FF, CF, and FC. The first letter of the two letter abbreviation determines whether the primary care physician is paid via fee-for-service (F) or capitation (C). The second letter represents the manner in which the specialist is compensated.[1] Thus, the variable CF is equal to unity when the individual's insurance plan pays the primary care provider with a capitation payment and the specialist on a fee-for-service basis. In the data, I combine capitation and salaried physicians into one "capitation" category. This is done because both salaried and capitated physicians receive a non-positive pecuniary benefit from providing marginal services and thus both have have an incentive to provide less treatment than in the fee-for-service setting. All subsequent regressions were also run separating out salaried and capitation physicians. The coefficient estimates—which are not reported here—are similar to when the capitation and salaried physicians are grouped together, but the standard errors are less precise.

In the data 27.5% of patient's insurance plans pay their primary care physician via capitation compensation. These same insurance companies pay specialists via capitation

[1] The questions used in the survey are: "What is the typical method of payment that your organization uses for primary care providers (specialists)?"

compensation 16.2% of the time. Remler et al. (1997) completed a national survey of physicians in 1995 and found that the mean primary care physician received capitation payment for 18% of patients and the mean specialist received capitation payment for 10% of patients. Remler's capitation reimbursement figures are likely lower than the ones found here for three reasons. First, the Remler data is survey based, not observational, and thus there could be some response bias. Secondly if physicians base their responses on their typical patient interaction, in the presence of adverse selection the physician will interact with capitation patients less frequently if this sub-population is comprised of healthier individuals. Finally, Remler included Medicare-eligible individuals in the data. Medicare generally pays physicians on a FFS basis and this will increse the proportion of observations with fee-for-service physicians. For these reasons, the proportion of fee-for-service PCPs in the CTS data appear to be reasonable.

One complication arising in the data is that 13% of patients have physicians who are compensated through a global capitation scheme. Under this arrangement, the costs for primary, specialist and hospital care are born entirely by the primary care provider. Examples of group practices paid via global capitation include single-plan group practices such as Kaiser Permanente, individual practice associations (IPAs), and large medical groups. [2] Under global capitation, I observe how the medical group, but not the individual physician is paid. Previous studies by Rosenthal et al. (2002) and Conrad et al. (1998) document significant variation in how medical groups compensate individual physicians under global capitation. [3] Further, neither of these studies distinguishes the manner in which specialists are paid.

In order to isolate the ambiguities regarding physician compensation under global capitation, I create a fifth physician compensation variable, G, which is equal to unity if the individual is part of a health plan with global capitation and zero otherwise.[4] A

[2]Ogrod (1997) notes that the physician structure under global capitation "...is usually a large, multi-specialty structure spread over a large geographic area."

[3]Rosenthal et al. (2002) find that Californian IPAs are more likely to compensate primary care physicians through capitation, but medical groups are more likely to utilize either FFS or salaried payments. Further, many of these practices include significant bonuses based on cost of care, quality of care, productivity or profit sharing. Using data from Washington state, Conrad et al. (1998) find that 35% of medical groups pay PCPs through a salaried or modified salaried system, while 45% of medical groups pay PCPs strictly on a production basis.

[4]The variable G also includes 'full professional capitation' in which the practice is liable for paying for primary and specialist care, but do not have to pay for hospital care. Under global capitation, the

description of all five physician compensation dummy variables is given in Table 1.

Table 2 breaks down the financial compensation dummy variables by type of health plan. The series of physician compensation variables do not fall neatly into insurance product categories. For instance, assuming that HMOs pay primary care physicians under a capitation scheme would be incorrect. Approximately 32% of PCPs and 54% of specialists who work for HMOs are paid via FFS. On the other hand, one fifth of PCPs paid via capitation do not work for HMOs.

In order to analyze the effect of physician compensation on specialist medical services in the data, I assume that individual i's number of surgeries is a function of the five physician compensation dummy variables as well as a vector of covariates $\mathbf{z}_i$. The vector of all independent variables is defined to be $\mathbf{x}_i$.

$$Surgeries_i = f(FF_i, CC_i, CF_i, FC_i, G_i, \mathbf{z}_i) = f(\mathbf{x}_i) \tag{1}$$

The dependent variable that will be used in the subsequent analysis is the number of surgeries each individual received during the previous year. Using the 'number of surgeries' is chosen as the dependent variable has potential advantages and disadvantages. Patient recall of the number of surgeries undergone in the past year is likely more accurate compared to other medical utilization metrics. Further, the number of surgeries is the only variable in the data that pertains exclusively to specialists. On the other hand, because less that 5% of the sample has more than one surgery, subsequent regression estimates will be less precise.

The vector of explanatory variables, $\mathbf{z}_i$, includes individual information such as age, age-squared, gender, income, marital status and a constant term. Other covariates in $\mathbf{z}_i$ are education and race dummies, and each individual's deductible and coinsurance rate. Papers such as Saver et al. (2004) claim that much of the variation in physician compensation method is due to local market conditions and in order to control for this, dummy variables for each metropolitan statistical area (MSA) are included in all regressions. Finally, a health variable, which is proportional to the Physical Component Summary (PCS), is included. The PCS is continuous variable constructed from twelve detailed questions regarding the respondent's health status.[5] Summary information for

practice is also responsible for hospital expenses as well as primary and specialist care.

[5]For example, two of the twelve questions included in the PCS calculation ask whether a person's health limits them from "Moderate activities, such as moving a table, pushing a vacuum cleaner, bowling,

each of these measures is shown in Table 3.

While some physicians may have all patients pay them in a homogenous manner, the majority of physicians treat some patients whose insurance company will pay them via capitation and other patients whose insurance will pay them via FFS. Newhouse and Marquis (1978) provide some evidence that physicians can differentiate levels of care for each patient in a mixed payment environment. The authors observed that individual patient compensation arrangements do affect physician behavior in the case of hospital admissions, the length of a hospital stay and the number of office visits. On the other hand, physicians are not able to discriminate care levels by insurance type in the case of the length of an office visit (Glied and Zivin, 2002; Frank and Zeckhauser, 2007) and prescribing behavior (Frank and Zeckhauser, 2007; Hellerstein, 1998). If discriminating medical care quantities on a patient-by-patient basis is impracticable, then subsequent results should be interpreted as the effect of increasing the percentage of the physician's patient base who is paid in certain manner rather. Thus, regression coefficients will underestimate the marginal impact of changing physician's patient base from one that pays him entire via capitation to a patient base who only pays him FFS.

5.2 Regression specification

I will be using the number of surgeries an individual has had during the prior year as the dependent variable. Since the number of surgeries is restricted to be a non-negative integer, a negative binomial regression is employed. A logical starting point when working with count data is to use the Poisson model, but a Poisson regression will systematically underestimate standard errors if the dependent variable's conditional mean and variance are not equal. To test whether or not the Poisson model is appropriate, one can estimate a type I negative binomial regression (Negbin I) where $Var(S|\mathbf{x}) = (1+\alpha)E(S|\mathbf{x})$, and use a Wald test to verify whether or not $\alpha = 0$ (Cameron and Trivedi, 1986). When applied to the CTS data, the Wald test rejects the null hypothesis that $\alpha = 0$ ($p \leq 0.01$). Therefore, I employ the Negbin I model through the majority of the paper.[6]

or playing golf" and "Climbing several flights of stairs?" Other questions inquire as to the emotional state of the person and whether or not they suffer significant pain.

[6]Although the results from the Negbin I model are presented in this paper, the analysis was also conducted using the Negbin II framework in which it is assumed that $Var(S|\mathbf{x}) = E(S|\mathbf{x})[1+\alpha E(S|\mathbf{x})]$. Neither the qualitative or quantitative results change significantly when the Negbin II specification is

Coefficients are reported as the marginal effect of a change in the explanatory variable on the absolute surgery rate. For the physician compensation variables, estimates are calculated as the absolute change in the surgery rate for an average individual when the binary variable changes from zero to one.

The parameter estimates on the physician compensation variables are estimated in a partial equilibrium setting. These parameter estimates should be interpreted as the effect of moving a single patient from a physician compensated in one manner to a different physician compensated in a different manner. Hellinger (1996) notes that it is possible that physicians who treat patients in a less aggressive manner may migrate towards jobs with capitation payment while those with more aggressive treatment styles may choose FFS employment. With the data used in this paper, however, it is impossible to separate whether the physicians migrate to financial plans whose incentives favor their practice style or whether the financial incentives actually change the physician's practice style from an unobserved counterfactual. It may also be the case that financial incentives affect the number of specialist hours worked, and thus differences in surgery rates by physician compensation may be due to differences in patient wait times rather than an explicit physician decision. From the data used, I am unable to determine through which of these pathways does the causal mechanism operate.

5.3 Controlling for Adverse Selection

All non-randomized health economics studies must address the issue of adverse selection. If physicians who are compensated via capitation provide less generous medical services, but also contract with health plans with lower premiums, it is likely that healthy individuals will choose these health plans. If this was the case, individuals with capitated PCPs would have fewer surgeries not due to financial incentives, but because they are healthier patients.

To control for these selection effects, I examine a subsample of the data where individuals have no choice of health insurance. Individuals whose employers offer them only one health plan choice do not have this sorting option. Non-group insurance is generally a poor substitute for group insurance purchased through one's employer. Thus, individuals offered one choice of health insurance at work are *de facto* constrained to choose

used.

this plan. In fact, the main determinants of whether or not a worker is offered more than one health plan at work is the firm's size and location—not the individual's characteristics.[7] By using the subsample of individuals offered a single health insurance plan, adverse selection will not contaminate coefficient estimates from subsequent regression analysis.

Even if different types of individual are not sorting to firms offering only one health plan, there are still three confounding factors that could bias the coefficients estimated within the"no choice" subsample; these are spousal insurance, job choice, and employer agency. Individuals who have only one choice of insurance at work may not, in fact, be limited to this single option if they are married. In the data, I count individuals as having no choice of health insurance even if they take up their spouse's health insurance. The take-up of spousal insurance could lead to significant endogeneity problems since 11.1% of individuals are in households where both spouses maintain employer-provided health insurance. In order to verify whether or not having a spouse with employer-provided health insurance alters the results, I run the preferred negative binomial regression while excluding working individuals who can take-up health insurance both at their own and their spouse's place of employment. The results are quantitatively similar to those forthcoming.[8]

I also assume that an individual's place of employment is chosen without regard to physician compensation. Since Cunningham, Denk and Sinclair (2001) demonstrate that individuals have poor knowledge of their own insurance, it is unlikely that health plans are chosen based on physician compensation method. Individuals may, however, select their employer based on whether they offer an HMO, preferred provider organization (PPO), point-of-service plan (POS) or FFS health plan. Health plan type is correlated with the manner in which physicians are compensated so any remaining bias could be driven by endogenous job choice.

[7]To verify whether or not this is the case, I run a probit regression (not shown) of whether or not a firm offers more than one insurance plan on a variety of individual and firm characteristics. The psedo-R^2 including all variables is 0.103. When I include only individual demographic and socioeconomic variables, the psuedo-R^2 falls to 0.018. If I include only firm size, industry, and city fixed effects variables, the pseudo-R^2 decreases only slightly to to 0.098.

[8]Excluding spouses who both have employer-provided health insurance, I observe a 61.6% (75.7%) increase in total (outpatient) surgeries compared to a 77.5% (84.3%) increase in surgeries when these couples are included in my later regressions.

Furthermore, employers could act as agents for their workers. An employer with sicker employees may elect to offer a single plan which pays physicians on a FFS basis. Another employer composed of healthier workers may choose to offer one plan which pays physicians via capitation. Thus, average employee health levels may be correlated with plan choice even if the employers only offer workers one health plan.

In order to test empirically whether or not job selection will contaminate the identification strategy, Table 4 shows mean covariate values by physician compensation type. The top portion of the table displays the results for individuals who are offered one choice of insurance through their employer (i.e., "no choice") and the bottom portion of the table displays the covariate means for individuals who are offered multiple health plans at work (i.e., "choice"). I test whether or not the mean covariate values for each physician compensation type are the same.

For individuals with a "choice" of health plans, there are statistically significant differences by compensation type for nearly all the covariates. Even though I cannot statistically reject the equality of means for the $Health$ variable, the p-value is 0.108. On the other hand, for individuals in the "no choice" subsample, the observations are much more homogenous across physician compensation types. I observe that the p-value for equality of means for $Health$ is 0.577 and for Age is 0.193. Since health and age are the observable variables most likely to uncover selection effects, this is strong evidence that limiting the sample to individuals offered only one choice of health insurance at work has significantly attenuated any adverse selection problems.

I do note, however, that there appears to be statistically significant differences in income, gender and minority status in the "no choice" subsample. As a robustness check, I use two regression variations to analyze the impact of income on surgery rates. First, I run the preferred regression separately for high and low income individuals; secondly, I add (*income*)(*physician compensation*) interaction terms in the regression. In both cases, the results from testing the direct and indirect hypotheses are similar to those of subsequent regressions, but the magnitude of this effect is larger for those in higher income brackets. To further test the sensitivity of my results, I run the preferred negative binomial regression separately by gender and by minority status. The results of these four regressions (i.e., Caucasian-only, minority-only, male-only, female-only) are quantitatively very similar to the results found in the rest of the paper. These results provide support that adverse selection issues are attenuated when the sample is limited to individual offered one choice of health insurance at work.

6 Results

Now I will test the direct and indirect hypotheses proposed in earlier sections with the CTS data.

6.1 'No Choice' Specification

In the first set of specifications I restrict the sample to individuals whose employers offer them only one health plan. As discussed previously, this methodology—along with the use of the PCS variable to control for health level—should largely eliminate biases resulting from adverse selection. The first three columns of Table 5 give the results from the negative binomial regression for individuals in the "no choice" subsample.

Looking across all three specifications, the parameter estimates from variables in the **z** vector are mostly as expected. Healthier individuals have fewer surgeries. Age does not have any impact on surgery rates, but this is likely due to the fact that age is correlated with health. Because they are at-risk for pregnancy, women are more likely to have surgery. Individuals who are married, have more education, come from a Caucasian background and earn more income are more likely to have surgery. These patterns may be explained by differences in insurance quality, however due to data restrictions I can not empirically test this possibility. Finally, it seems that coinsurance and deductible rates have little impact on the quantity of surgeries observed.

Let us examine physician compensation dummy variables using total surgeries as the dependent variable. Since the omitted dummy variable in the regression is FF, the marginal effects should be interpreted as the absolute change in the number of surgeries per person relative to this baseline where both the PCP and specialist are compensated via FFS. The average number of total surgeries per thousand persons with FF health plans in the 'no choice' specification is 143. The CF compensation scheme leads to 12 more surgeries per thousand people than the FF group; being in the CC group decreases surgeries by 56 for every thousand people compared to the FF plans. The estimates for FC should be viewed with caution since there are only 28 individuals in the 'no choice' subsample where $FC = 1$. Global capitation payment, G, has no significant effect on surgery rates, likely due to the ambiguous nature in which the physicians are compensated.

Let us now test the direct and indirect hypotheses. The direct hypothesis predicts that FFS specialists will preform more surgeries than specialists paid via capitation. If this hypothesis is true, this would mean that holding the primary care physician's compensation constant, a change in the specialist compensation from capitation to fee-for-service will increase surgery rates. In the regression this implies that $\beta_{CF} > \beta_{CC}$. Column 1 reveals that switching specialist compensation from capitation to fee-for-service increases surgery rates by 77.5%. The prediction that surgeons paid via FFS perform more operations than surgeons paid via capitation holds true in the data at the 5% significance level ($p \leq 0.011$).

This result is in line with some of the results found in related literature. Saver et al. (2004) found that switching from capitation to FFS increased average procedure rates by 28% and switching from salaried to FFS compensation increased average procedure rates by 44%. On the other hand, Shrank et al. (2005) found that switching from capitation to FFS increased cataract surgery rates by 94% for Medicare beneficiaries and by 123% for individuals in commercial plans. The magnitude of effect of physician compensation on specialist care found in this study falls in between the Saver and Shrank results.

The indirect hypothesis predicts that holding specialist compensation constant, changing PCP from FFS to capitation compensation will increase (decrease) surgery rates if primary care and surgeries are net substitutes (complements). With respect to the regression, the indirect hypothesis implies that $\beta_{CF} > \beta_{FF}$ if primary care and surgery are net substitutes but $\beta_{CF} < \beta_{FF}$ if they are net complements. Since FF is the omitted variable, the prediction becomes $\beta_{CF} > 0$ and $\beta_{CF} < 0$ respectively. The coefficient estimates show that switching the primary care physician from capitation to FFS increases surgery rates 9% but this estimate is not statistically different from zero. The interpretation of this coefficient is ambiguous. It could mean that quantities of primary care and surgeries are unrelated. On the other hand, the estimate could imply that primary care and specialist care are sometimes substitutes and sometimes complements, but the net effect is no relationship between the two sectors.

The results of the negative binomial regression for outpatient and inpatient surgeries are shown in columns 2 and 3 respectively. When I test the direct hypothesis, I find that outpatient surgery rates are 84.3% higher when the specialist switches from capitation to fee-for-service compensation. This result is significant at the 1% level ($p \leq 0.003$). With respect to the indirect hypothesis, changing the primary care provider from a fee-for-service to a capitation compensation basis increases surgery rates by 15.2%, but this result is not statistically significant at even the 10% level.

The third column of Table 5 shows the results from the negative binomial regression using inpatient surgeries as the dependent variable. The test of the direct hypothesis is signed as predicted but statistically insignificant and of a very small magnitude. The indirect effect of the PCP compensation on surgery rates is also not statistically different from zero.

The finding that the effect of physician compensation is stronger in the outpatient than the inpatient case should not come as a surprise. Shen et al. (2004) found survey responses of treatment intensity varied greatly between fee-for-service and capitation patients in three cases where treatment was 'more elective,' but in the relatively non-elective case of the management of end-stage heart failure, the authors found no difference in the physician's intended treatment intensity for FFS and capitation patients. Shrank et al. (2005) found that cataract surgery rates were responsive to financial incentives, but ophthalmological surgery rates for non-elective procedures did not respond when physician financial incentives changed. It seems reasonable to believe that outpatient surgeries are more likely to be considered 'elective' than inpatient surgeries; thus observing that outpatient surgeries are more responsive to financial incentives than inpatient surgeries is consistent with the findings in the literature.

6.2 'Choice' Specification

Although Table 4 gives evidence that patients are not randomly distributed across physician compensation types in the "choice" specification, one may still worry whether or not subdividing the sample was a necessary step. Subdividing the sample decreases precision and thus should only be utilized if adverse selection occurs in the "choice" subsample. If I observed that $\tilde{\beta}_{CF}$ in the "choice" specification were of a smaller magnitude than the same coefficient in the "no choice" setting, this would give some suggestive evidence that adverse selection is present. Two separate effects are operating in the "choice" specification; PCP capitation should increases surgery rates if primary care and specialist care are substitutes, but PCPs paid via capitation should attract healthier patients and thus reduce surgeries. If adverse selection is present in the "choice" specification, then the patient sorting effect should be stronger than in the "no choice" subsample. If selection is based on how the primary care physicians are paid, healthier individuals will sort into the CF physician compensation group.

In table 5, columns 4, 5, and 6 give the results of a negative binomial regression when individuals have a choice of insurance plans at their work. The analysis is conducted using total surgeries, outpatient surgeries and inpatient surgeries as the dependent variable. In all three specifications, the parameter estimate for $\tilde{\beta}_{CF}$ changes signs compared to the "no choice" results and becomes negative—implying $\tilde{\beta}_{CF} < \tilde{\beta}_{FF} = 0$. Assuming primary and specialist care are substitutes, these results suggest that omitted factors affecting surgery rates are correlated with a person's choice of health plan.

6.3 Robustness Check

To further test for the presence of adverse selection, one ideally would like to examine health behaviors unrelated to physician compensation. As a robustness check, I evaluate if physician compensation affects the probability an individual receives a flu shot. While it is possible that differences in primary care compensation could influence vaccination rates, specialist compensation should have no effect on the probability a patient receives a flu shot. Any differences in vaccination rates across the different types of specialist compensation are attributable to patient sorting.

To test this hypothesis I run a probit regression of flu shot receipt on physician compensation and all other covariates included in the preferred specification. Table 6 displays the results from this regression. For the "no choice" group, I fail to reject the null hypothesis that physician compensation has no effect on flu shot probabilities ($p \leq 0.199$). I also fail to reject the null hypothesis that specialist compensation has no effect on flu shot probabilities ($p \leq 0.249$). On the other hand, when individuals have a choice of health insurance, PCPs paid via capitation have patients who are more likely to get flu shots. I reject the hypothesis that physician compensation does not influence the probability of getting a flu shot ($p \leq 0.001$). Further, I find that having a specialist paid via capitation increases the probability that an individual will receive a flu shot ($p \leq 0.061$). Because specialist compensation is only strongly correlated with flu shot frequency when individuals are offered multiple health plan choices, restricting the sample to individuals with only one health plan choice has attenuated problems of adverse selection.

Another method to test for adverse selection is to investigate whether or not individual covariates affect health plan choice. Even if employees are not able to choose health plans individually, one could still observe sorting behavior if firms choose health plans based on underlying worker characteristics. In order test whether or not employee covariates predict health plan choice, I use a two stage methodology. In the first stage, I exclude the physician compensation variables and run the preferred negative binomial regression for all insured individuals (i.e., the 'choice' and 'no choice' groups combined). In the second stage, I run a probit regression of health plan choice on the predicted values from the initial regression. Table 7 displays these results.

For the cases of selecting an HMO or a PPO/FFS health plan, individual characteristics related to surgery are more highly correlated with health plan choice when the individual is able to choose from a menu of health plans. Individual characteristics correlated with surgery decrease the probability of choosing an HMO when the individual has a choice of plan ($p \leq 0.015$), but these same predicted values have no statistically significant relationship to HMO choice when I limit the sample to those offered only one health plan ($p \leq 0.420$). Similarly for the case of choosing a PPO or FFS plan, demographic variables predicting surgery also are correlated with an increased probability of choosing a PPO or FFS when individuals are offered multiple plans ($p \leq 0.011$), but the same information has no predictive value of a choice of a PPO or FFS plan when individual are only offered one health plan ($p \leq .870$). This provides further evidence that even if employers are selecting health plans based on average worker characteristics, this problem is fairly small in magnitude and unrelated to surgery rates.

6.4 Alternate Regression Specifications

Even though the negative binomial is my preferred econometric specification, it has some limitations. For instance, let us assume that person A has one surgery and person B has two. While it would be reasonable to assume that person B utilizes more medical services person than person A, she may not use twice as many services. To take this into account, columns 1 and 2 of Table 8 use an ordered probit regression, so that the total number of surgeries becomes a categorical variable rather than a count variable. The results shown in Table 8 confirm that the ordered probit regression does not materially change the results in either the "choice" or "no choice" subpopulations.

Also, since less than 5% of individuals in the Community Tracking Study have more than one surgery during the year, the surgery variable can be redefined to be a dummy variable equal to unity when an individual had one or more surgeries during the year and zero otherwise without much loss in information. A logit regression is run using this binary variable as the dependent variable. This ensures that a few outliers are not driving the results. The results from the logit regression are reported in Columns 3 and 4 of Table 8. In the logit, "no choice" case, individuals with CF health plans are 60.3% ($p \leq 0.045$) more likely to have surgery during the year than than those with CC plans and 6.8% ($p \leq 0.758$) more likely to have surgery than individuals with FF plans. The similarities across parameter estimates of the negative binomial, ordered probit, and logit regressions indicate that the findings of this study are robust.

To this point in the paper, I have treated global capitation of group practices as distinct from direct physician compensation via capitation. However, Rosenthal et al. (2002) show that in California, about 50% of medical groups who receive global capitation have profit sharing. Profit sharing would create incentives for physicians with global capitation to act more like physicians paid directly via capitation. To control for this, I conduct all previous regressions combining the CC and G groups. This implicitly assumes that all specialists in global capitation groups are paid via capitation. The results combining the capitation and global capitation groups are qualitatively similar to previous results, but parameter estimates of the direct hypothesis are generally of smaller magnitude.

6.5 Referral Requirements

One confounding factor ignored to this point is the possibility that non-financial incentives play a role in surgical rates. Referral requirements compel patients to receive 'permission' from their primary care doctor before they visit a specialist. These referral restrictions act as a constraint on how primary care financial incentives can indirectly affect the quantity of surgeries.

A new specification to isolate the effects of referrals would be:

$$Surgeries = g(nFF, nCC, nCF, nFC, nG, rFF, rCC, rCF, rFC, rG, \mathbf{z})$$

where $rFF = ref * FF$ and $nFF = (1 - ref) * FF$, where ref is a dummy variable equal to one when a referral is required by the health plan and zero otherwise. I now test whether or not referral requirements have an impact on my tests of the direct or indirect hypotheses. The direct hypothesis would predicts that $\beta_{nCF} > \beta_{nCC}$ and $\beta_{rCF} > \beta_{rCC}$; the indirect hypothesis predicts that $\beta_{nCF} > \beta_{nFF}$ and $\beta_{rCF} > \beta_{rFF}$ if primary and specialist care are net substitutes.

Table 9 displays the results of the negative binomial regression by referral requirement status for the direct and indirect hypotheses. The theoretical prediction that the direct effect of moving specialist compensation from capitation to FFS will increase surgery rates still holds. When referrals are not required, there is an 87% ($p \leq 0.112$) increase in the total surgery rate when the specialist is paid via FFS rather than capitation. When referral requirements are present, there is a 66% ($p \leq 0.074$) increase in the total surgery rate when specialist compensation changes from capitation to FFS.

For the indirect hypothesis, the net effect of changing primary care compensation rates from FFS to capitation depends on whether or not there are referral requirements. With no referral requirement in place, when the PCP is paid via capitation, surgery rates increase by 35% ($p \leq 0.082$). On the other hand, when referral restrictions are in place, having a capitation-compensated PCP does not seem to have an impact on surgery rates. The regression shows a statistically insignificant 13% decrease ($p \leq 0.477$) in surgery rates when there are referral restrictions and the PCP is paid on a capitation basis. These results suggest that primary care and surgery are net substitutes in the absence of referral restrictions, but that referral restrictions attenuate this effect. It may be the case that capitation compensated PCPs who would have referred patients to a specialist for surgery are now compelled to treat them themselves because of the referral restrictions.

6.6 The problem with using HMO dummy variables as a proxy for capitation payment schemes

A naive researcher may use an HMO dummy variable as a proxy for how capitation payment affects surgery rates. Yet from Table 2, one can clearly see that assuming that all HMO's pay their physician on a capitation basis is unwise. Over 30% of HMO primary care physicians and over 50% of HMO specialists are paid on a FFS basis. Table 10 shows us the results of the standard negative binomial regression using an HMO dummy variable to proxy for capitation payment.

In the first column, I examine the entire sample of individuals with employer provided insurance aged 18-64. Patients who have an HMO health plan have about 10% fewer surgeries than those with a POS, PPO or FFS health plan. These results are statistically significant at the 10% level ($p < .080$) but do not control for adverse selection. When one limits the sample to individuals with only one choice of employer-provided insurance, I see that HMO patients only have -0.4% fewer surgeries and this coefficient has a large p-value ($p \leq 0.96$). The naive researcher would conclude that capitation payment schemes do not materially affect surgery rates.

The findings using an HMO dummy variable starkly contrast with the results using direct observation of physician compensation. Using the physician compensation data, I find that the surgery rates for patients who pay their specialist on a FFS basis are 78% higher than the surgery rates for patients whose health plan uses a capitation scheme. By using more accurate data with using robust controls for adverse selection, I directly identify the impact of different physician compensation schemes without resorting to noisy measures such as an HMO health plan dummy variable.

7 Discussion

This study has found that switching specialist compensation from capitation to FFS increased total (outpatient) surgery rates by 78% (84%). As predicted by the literature, the results using inpatient surgeries were not statistically significant. Even when taking into account referral requirements or altering the econometric specification, it was consistently shown that FFS specialists preform more surgeries per person on average than specialists paid on a capitated basis.

The relationship between PCP compensation and surgery rates depended on the presence of referral restrictions. In the absence of referral requirements, changing PCP compensation from FFS to capitation increased total surgeries rates by 35%. This provides some evidence that primary care and surgeries are net substitutes. When referral requirements are in place, however, this relationship disappears, likely because referral requirements restrict capitation-compensated PCPs from sending patients to specialists.

This study is one of the first to use nationally representative, household data to estimate how physician financial incentives affect the provision of specialist services. Further, using the Community Tracking Study Restricted Use data set eliminates serious measurement error biases that have been shown to occur in household data. Any selection effects that may remain after restricting the sample to individuals offered one health insurance plan through their employer would bias the estimates against concluding in favor of the indirect hypothesis. The effect of any remaining adverse selection bias on direct hypothesis is unknown.

Future research should investigate how specialist financial incentives affect a broader range of specialist medical services. Also, subsequent empirical studies should model physician compensation in an even more sophisticated manner, taking into account the possibility that physicians are paid simultaneous via capitation and fee-for-service by the same patient. For instance, physicians may receive a capitation payment, but their contract with a health plan may allow carve-outs where the physician receives FFS compensation for performing certain procedures. Finally, collecting data on patient outcomes or surgical quality would be useful in addressing the welfare implications of different physician compensation schemes.

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8 List of Figures

Table 1: Physician Compensation Variables Explained

Variable	**Explanation**
FF	PCP paid via fee-for-service; Specialist paid via fee-for-service
CC	PCP paid via capitation; Specialist paid via capitation
CF	PCP paid via capitation; Specialist paid via fee-for-service
FC	PCP paid via fee-for-service; Specialist paid via capitation
G	Practice paid via *global* capitation, physician compensation uncertain

Table 2: Physician Compensation by Insurance Type

	FF	CC	CF	FC	G	Total
HMO	2726	1101	1956	78	2767	8628
	(0.173)	(0.853)	(0.682)	(0.907)	(0.924)	(0.376)
POS	2129	173	897	6	207	3412
	(0.135)	(0.134)	(0.313)	(0.070)	(0.069)	(0.149)
PPO	7698	16	15	2	20	7751
	(0.490)	(0.012)	(0.005)	(0.023)	(0.007)	(0.338)
FFS	3167	0	0	0	0	3167
	(0.201)	(0.000)	(0.000)	(0.000)	(0.000)	(0.138)
Total	15,720	1290	2868	86	2994	22,958
	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)	(1.000)

Number of observations; proportion of column in parentheses

Table 3: Table of Means

Variable	Mean	Std. Dev.	Min	Max
Total Surgeries†	0.187	0.524	0	5
Outpatient Surgeries†	0.137	0.448	0	5
Inpatient Surgeries†	0.050	0.257	0	5
FF	0.721	0.448	0	1
CC	0.054	0.227	0	1
CF	0.106	0.308	0	1
FC	0.004	0.064	0	1
G	0.114	0.318	0	1
$Health$	5.126	0.844	1.1	6.8
$Male$	0.506	0.500	0	1
Age	40.0	11.98	18	64
Highest Grade Completed	13.5	2.40	6	19
$Married$	0.713	0.453	0	1
$Caucasian$	0.778	0.415	0	1
$African - American$	0.101	0.302	0	1
$Asian - American$	0.039	0.193	0	1
$Latino$	0.082	0.274	0	1
$Coinsurance$ (%)	5.2	8.6	0	50
$Deductible$ ($)	125.7	174.9	0	3000
$Income$ ($\$10,000s$)†	5.38	3.37	0	15
n	22958			

† Surgeries top-coded at 5; Income top-coded at 15 (i.e.:$150,000)

Table 4: Variable Means by Physician Compensation

No Choice

	FF	CC	CF	FC	G	Mean	P(equal means)
Health	5.19	5.27	5.19	5.22	5.17	5.19	0.577
Male	0.583	0.570	0.508	0.747	0.550	0.573	**0.003**
Age	39.0	37.6	38.4	39.9	38.4	38.9	0.193
Highest Grade Completed	13.4	13.9	13.6	13.5	13.4	13.4	0.050
Married	0.699	0.686	0.652	0.717	0.692	0.693	0.272
Minority	0.166	0.425	0.317	0.464	0.293	0.203	**0.000**
$Income(\$10,000s)$	5.07	5.42	5.36	6.71	5.18	5.12	**0.006**

Choice

	FF	CC	CF	FC	G	Mean	P(equal means)
Health	5.08	5.12	5.13	5.23	5.10	5.09	0.108
Male	0.469	0.470	0.468	0.453	0.487	0.472	0.641
Age	41.0	40.1	39.6	40.7	39.5	40.6	**0.001**
Highest Grade Completed	13.5	13.7	13.8	13.3	13.7	13.6	**0.001**
Married	0.738	0.658	0.688	0.848	0.698	0.720	**0.001**
Minority	0.196	0.387	0.295	0.516	0.285	0.236	**0.001**
Income ($10,000s)	5.49	5.77	5.58	6.16	5.38	5.51	**0.014**

Table 5: Results from the Negative Binomial Regression

	No Choice			Choice		
Variable	Total	Outpatient	Inpatient	Total	Outpatient	Inpatient
CC	-0.0557*	-0.0354*	-0.0042	-0.0248	-0.0243	0.0001
	(0.0308)	(0.0195)	(0.0037)	(0.0234)	(0.0258)	(0.0094)
CF	0.0123	0.0143	-0.0019	-0.0293**	-0.0299**	0.0003
	(0.0309)	(0.0274)	(0.0039)	(0.0124)	(0.0121)	(0.0039)
FC	0.0563	0.0423	0.0026	-0.0711***	-0.0718*	-0.0001
	(0.0713)	(0.0468)	(0.0045)	(0.0172)	(0.0378)	(0.0286)
G	0.0201	0.0278	-0.0036*	-0.0242*	-0.0204**	-0.0048
	(0.0233)	(0.0216)	(0.0021)	(0.0130)	(0.0104)	(0.0053)
$Health$	-0.0477***	-0.0238***	-0.0051***	-0.0825***	-0.0541***	-0.0231***
	(0.0077)	(0.0048)	(0.0017)	(0.0078)	(0.0058)	(0.0018)
$Male$	-0.0489***	-0.0168**	-0.0097***	-0.0329***	-0.0082	-0.0223***
	(0.0091)	(0.0079)	(0.0028)	(0.0066)	(0.0070)	(0.0038)
Age	-0.0020	0.0010	-0.0005	0.0005	0.0016	-0.0006
	(0.0033)	(0.0033)	(0.0004)	(0.0023)	(0.0024)	(0.0010)
$Married$	0.0180	0.0180*	-0.0014	-0.0328***	-0.0497***	0.0076**
	(0.0149)	(0.0101)	(0.0026)	(0.0116)	(0.0106)	(0.0032)
$Coinsurance$	0.0001	-0.0006	0.0001**	0.0001	-0.0001***	0.0001
	(0.0008)	(0.0007)	(0.0001)	(0.0006)	(0.0005)	(0.0002)
$Deductible$	0.0000	0.0000	0.0000	-0.0001*	0.0000	0.0000**
	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$Income$ (\$10k)	0.0011	-0.0002	0.0005	0.0061**	0.0062**	0.0002
	(0.0024)	(0.0021)	(0.0004)	(0.0012)	(0.0013)	(0.0005)
CF vs. CC	77.5%**	84.3%***	2.5%	-2.8%	-4.7%	0.6%
CF vs. FF	8.6%	15.2%	-2.0%	-16.0%**	-20.8%**	0.7%
$P(CF = CC)$	0.011	0.003	0.677	0.835	0.818	0.982
$P(CF = 0)$”	0.692	0.601	0.621	0.018	0.014	0.944

Age^2; Education, Race and City Dummies included in regression but not shown.

***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.10$

Table 6: Probability of Receiving a Flu Shot

	No Choice	Choice
CC	0.005	0.049***
	(0.048)	(0.011)
CF	-0.039	0.029**
	(0.027)	(0.015)
FC	-0.007	-0.004
	(0.143)	(0.018)
G	0.049*	0.0173
	(0.028)	(0.016)
P(All coeff. equal 0)	0.199	0.001
$P(CF = CC)$	0.249	0.061

All covariates from Table 5 included in regression but not shown

Coefficients reported as percentage point marginal effects

Table 7: Covariate Index and health plan choice

	HMO		PPO/FFS	
Variable	No Choice	Choice	No Choice	Choice
Index	-0.221	-0.469**	0.046	0.471**
	(0.275)	(0.193)	(0.283)	(0.185)
Constant	-0.643***	-0.238*	0.319	-0.086
	(0.195)	(0.143)	(0.208)	(0.160)

***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.10$

Table 8: Alternative Econometric Specifications

Dependent Variable: Total Number of Surgeries

	Ordered Probit		Logit	
Variable	No Choice	Choice	No Choice	Choice
CC	-0.2240	-0.0712	-0.0384	-0.0140
	(0.1739)	(0.0855)	(0.0283)	(0.0200)
CF	0.0579	-0.0944***	0.0078	-0.0192***
	(0.1179)	(0.0343)	(0.0254)	(0.0065)
FC	0.0258	-0.2833***	-0.0282	-0.0642***
	(0.2111)	(0.0780)	(0.0295)	(0.0168)
G	0.0757	-0.0638	0.0130	-0.0113
	(0.0771)	(0.0446)	(0.0160)	(0.0105)
$Health$	-0.2007***	-0.2767***	-0.0363***	-0.0605***
	(0.0273)	(0.0158)	(0.0053)	(0.0034)
$Male$	-0.1915***	-0.0988***	-0.0453***	-0.0224***
	(0.0303)	(0.0201)	(0.0074)	(0.0054)
% Change CF vs. CC			60.3%**	-3.8%
% Change CF vs. FF			6.8%	-12.8%***
$P(CF = CC)$	0.023	0.778	0.045	0.786
$P(CF = 0)$	0.623	0.006	0.758	0.003

All variables from Table 5 included in the regression but not shown.

***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.10$

Table 9: Referral Requirements (No Choice)

Dependent Variable: Total Number of Surgeries

	No Referral Requirement		Referral Requirement	
Hypothesis Test	% Change	p-value	% Change	p-value
Direct (CF vs. CC)	87.0%	0.112	66.0%*	0.074
Indirect (CF vs. FF)	35.3%*	0.082	-12.5%	0.477

All covariates from Table 5 included in regression but not shown

***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.10$

Table 10: HMO Naive Regression

	Full Sample	No Choice
HMO	-0.018*	-0.001
	(0.010)	(0.015)
Health	-0.074***	-0.048***
	(0.007)	(0.007)
Male	-0.039***	-0.050***
	(0.005)	(0.009)
Predicted Surgeries/person	0.173	0.145
HMO (% Change)	-10.2%*	-0.4%

All covariates from Table 5 included in regression but not shown

***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.10$