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MARKET POWER, CONTRACTS AND OUTCOMES: THE CASE OF PATIENTS WITH LONG-TERM DISEASES IN THE COLOMBIAN HEALTH CARE SYSTEM*

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January 30, 2017

Abstract

The Colombian health system has two main types of agents: the insurers and the service providers, which interact with each other through bilateral contracts. The types of contracts that these agents can write is restricted to a limited menu established by the regulator. The two most prevalent types of contract in the data are, by far, capitation contracts and fee-for-service contracts, which distribute risk and incentives differentially across both parties. We use a detailed data set of services and payments of all insurers and service providers at the individual user level to study the determinants of contract choice and their effect on health outcomes of a large sample of patients with chronic diseases. We focus on patients who are identical at the type of diagnosis, except for the contract type under which they are served, and show that capitation contracts are strongly correlated with lower rates of return to emergency care and lower rates of reincidence, compared with fee-for-service contracts. Both types of contracts lead to statistically different treatment paths. These results are consistent with contract theory and the economics of asymmetric information. Moreover, we show that the contract type depends on the market power of insurers and providers as predicted by a bargaining model. More generally, the results highlight the relevance of vertical contracts for the performance of health systems.

Keywords: Vertical contracts, health insurance, asymmetric information. JEL codes: D86, I11, L14

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I Introduction

The mainstay of the Colombian health care system is a government agency that collects income-based health contributions from individuals and then pays private insurers for individual users based only on their age, gender and location. The users choose freely among insurers, who in turn build a network of providers (e.g. hospitals) across their area of coverage and compete to attract users. Since contributing is compulsory for all formal employees and everybody who wants to have access to the more expensive premium health plans, the enrollment rates are high. Also since eligible individuals cannot choose not to enroll, there are no selection biases among the population of patients.

By law, there are strong limitations to ownership of providers by insurers. Therefore, the insurers build their networks writing bilateral contracts with mostly independent providers choosing from a fixed and very limited menu of contract types established by the government. By far, the most common contract types during the time-span of our study are capitation contracts and fee-for-service contracts, which distribute risk and incentives in totally different ways across agents. Capitation contracts contain a fixed payment per user (for a given health condition) and therefore shift the risk to the health provider, who therefore has strong incentives to make sure that the user gets healthy. On the other hand, fee-for-service contracts eliminate the risk for providers, who also have incentives to maximize the number of services that they charge the insurer. These contracts are **not** observed by the users, who therefore choose their insurers and providers fully unaware of them.

The focus of this paper is the understanding of the effects of contract types on health outcomes using an extensive individual-level data set that contains the type of contracts between insurers and service providers, and the services provided over the years 2009-2011. The monthly data contains the health condition and the services received by every individual in the system who claims a service. The data also identifies her insurer and the type of contract between the insurer and the health provider. We focus on patients who are diagnosed with chronic conditions and who are healthy at the time of diagnosis. Since we can track individual patients over time, we can compare the health condition of patients who have similar characteristics at the time of diagnosis, except for the contract type of their insurer and provider.

Our study is motivated by the contract theory and the literature of mechanism design under asymmetric information, which predicts that contract design has a strong effect on the behavior of agents when effort is not observed (Salanié (2005)). In the case of the Colombian health markets, insurers and health have stringent regulatory limits to their vertical integration and should in general interact via bilateral contracts[1][2]. There

[1]Law 1122 of 2007.

[2]The restrictions to the vertical integration of insurers and providers are meant mostly to protect

is a growing literature that studies the effects of contract design on health outcomes, but to our knowledge this is the first study to document the effects of contract type on health outcomes using micro-level data.

Our paper contributes to a growing empirical literature that focuses on the interactions of upstream and downstream firms in health care markets.[3] There is a body of descriptive empirical literature that studies the implications of the interactions between insurers and health providers, focusing mostly on prices as in Moriya et al. (2010), Dafny et al. (2012) and Trisch and Herring (2015). Recent work by Gowrisankaran et al. (2014), Ho and Lee (2013), Lewis and Pflum (2015) model and estimate the details of the interactions between health providers and insurers without addressing their implications on health outcomes, due mostly to lack of data. Our work is descriptive and focuses on the implications of the contracts between insurers and health providers on health outcomes.

We proceed as follows. In the next section, we describe the Colombian health care system, emphasizing the role of insurers and service providers. In the third section we describe our analytical framework. In the fourth section we describe our data, perform the empirical analysis and show its results. The fifth section concludes.

II The Colombian health care system: insurers, providers and contracts

There are two relatively separate systems within the Colombian health system: the "contributory" system which is fully funded by the required contributions of users, and the "subsidized" system which is funded by the government. This study refers just to the "contributory" system which covers 44% of the population and to which all formal employees are required to pay a fixed portion of their income[4]. The "subsidized" system covers 56% of the population and serves people who have no formal employment and who are poor enough to qualify.

As indicated above, all formal employees and individual contractors are required to contribute a fixed portion of their income to the contributory system and choose among a set of private national and regional insurers (called EPS). A government entity, called FOSYGA, collects these payments and distributes them to insurers based on health-relevant and income-neutral characteristics of enrollees (e.g. age, gender, etc.). Each

independent independent providers, especially public hospitals who serve the poorest uninsured population.

[3]For a comprehensive survey of recent research, see for example, Gaynor et al. (2015).

[4]Independent workers, entrepreneurs and other non-employees who want to purchase premium health insurance policies are also required to contribute to this system based on their income. For a more detailed description of the Colombian health system, see Law 100 of 1993.

insurer has to offer a standard coverage policy established by the regulator (called "compulsory health plan" or POS for its acronym in Spanish) and users can change their insurer once a year (though not many people do). Additional coverage and premium policies can be purchased but require, by law, the previous purchase of the standard "compulsory" plan.

The Colombian law imposes strong limits to the vertical integration of insurers and service providers. Therefore, each insurer serves its users through a network of largely independent service providers, called generically IPS, which include all service providers from hospitals to stand-alone doctor clinics or small therapy centers. Insurers build their service networks via bilateral contracts with service providers to cover all conditions included in the basic complulsory health plan (POS). Even though the exact terms of these contracts are unobserved to us, the regulation allows the choice of a very limited menu of contract *types*. As we describe in more detail below, we focus on the two most popular contract types in our data set, which are *capitation* contracts and $fee-for-service$ contracts.

These two types of contracts imply a different distribution of risk and incentives between insurer and service provider. On the one hand, *capitation* contracts specify a fixed payment for every potential user who visits a provider during a year. For example, an insurer may contract all emergency services with a hospital in a given city using a capitation contract under which the hospital gets a fixed payment per year for every enrolled individual who lives in that city, even if they don't claim any service. Under this contract type, the service provider "purchases" the potential user from the insurer.

On the other hand, under $fee-for-service$ contracts the service provider charges the insurer for every service that it provides to the user, independently of the health outcome. Following our example above, under this type of contract a hospital is paid for the emergency services it provides to patients. It is important to point out that no user is ever aware of the type of contract between her insurer and her service provider, which means that patients cannot their insurer or provider based on the type of contracts they have. Also, given any choice of contract type, insurers and providers bargain further over the prices of the contract. Finally, all services are reported to FOSYGA which also processes all payments. Our empirical analysis below is based precisely on this data base.

As we explain in the empirical sectionbelow, we focus on the type of contract between insurer and provider under which a patient is diagnosed with a chronic condition after being healthy for at least six months. After diagnosis, and for any given insurer-provider pair, these contract types may change. We will show that the type of services received by patients $after$ diagnosis changes depending precisely on the type of contract under which they received their initial diagnosis.

III The analytical framework

The interaction between the insurer and the service provider can be characterized as a moral hazard problem, in which the insurer who is neutral to risk wants the risk-averse service provider (e.g. hospital) to take partly unobserved actions based on information that is not fully observed by the insurer at the time the contract is written. Specifically, the insurer needs the provider to choose observed and unobserved inputs based on the condition of patients, which is not fully observed by the insurer. Even if the patients' health condition is observed, its characterization is in general so complex or costly that no contract can be written that accounts for all the contingencies that arise during treatment.

In this environment, insurer and provider write contracts with payments based on simple verifiable variables. From the literature on contract theory we know that the first best choice of inputs and payments is not attainable (Myerson and Satterthwaite (1983); Laffont and Martimort (2002)). Depending on the contract, the provider chooses inputs close to the optimal level but bears too much risk, or the provider bears less risk but has incentives to choose inputs that are not in line with the objective of the insurer.

In this paper we are not interested in studying the distortions of incentives away from the first best, but just in showing how different contracts generate different choices by providers which, in turn, have an effect on the health outcomes of patients. As indicated above, there are two types of polar contracts that are most prevalent in the data: *capitation* contracts and $fee-for-service$ contracts. Under a *capitation* contract, the provider gets a fixed payment per patient. Under this type of contract, the provider bears all risk but fully internalizes the costs and benefits of its choices. Under a $fee-for-service$ contract, the provider gets paid for every service or procedure it chooses to give the patient and, therefore, does not internalize its costs. Therefore, we expect both types of contracts to lead to different types of services. Moreover, if the choice of services yields different health outcomes, we should expect both types of contract to be associated with different outcomes, conditional on all the characteristics of the patient, the provider and the diagnosed health condition.

To better illustrate the kind of distortion that we have in mind, consider a simple model of provider's behavior under the two polar contract types described above. Assume that everytime a health service provider faces a patient i with a random health shock, it faces a concave payoff function $c_i(x, Y)$ which depends on the level x of service provision and the level Y of payments. By assumption and with some abuse of notation, $lim_{x\to 0}c_x = \infty$, $lim_{x\to\infty}c_x = -\infty$, $c_Y > 0$ and $c_{YY} < 0$; moreover, we assume for simplicity that $c_{xY} = c_{Yx} = 0$. This function is a measure of the expected health of their patients net of the costs of the provided services x, which are not only pecuniary costs but might also include psychological costs associated with the unobserved effort of attending to patients in a medical environment. The concavity of the function is a reflection of the risk aversion of the provider.

Under a capitation contract, the provider gets a fixed payment $Y = T$ for treating the patient. This payment is decided before patient's i health shock is realized. Once the payment is fixed and the health shock is known, the provider chooses the level of inputs that maximizes its payoffs depending on the realization of health shocks:

$$x^{cap} = argmax_x\{c_i(x, T)\} \tag{1}$$

The solution to this simple optimization problem is x^{cap} such that $c_x(x^{cap}, T) = 0$. Notice that even if the payoff of the provider were the same as the payoff of the insurer, this contract would not implement the first best solution because the provider is bearing all the risk of the health shocks, and by assumption the provider is risk-averse whereas the insurer is risk-neutral. In other words, depending on the stochastic evolution of the patient, the provider has to incur more or less costs than expected.

Under a fee-for-service contract, the provider is paid a price p for every unit of services x, so that payments are $Y = p * x$:

$$x^{fee} = argmax_x\{c_i(x, p * x)\}. \tag{2}$$

Therefore, in order to maximize utility, the provider chooses the level of inputs x^{fee} such that $c_x(x^{fee}, p * x^{fee}) + p * c_Y(x^{fee}, p * x^{fee}) = 0$. It is easy to see that $x^{fee} > x^{cap}$, which means that fee-for-service contractors *overprovide* services relative to capitation contractors, which is not surprising given increased marginal benefit of service provision under fee-for-service contracts. This is the prediction of the model that is the main focus of our empirical analysis below.

The effect of the contract type on health outcomes is less clear, though. If health outcomes were strictly increasing in health services, then more services should always lead to better health and, therefore, fee-for-services contracts would never be detrimental to the health of patients, even if they result in wasteful service provision. On the other hand, if contractable services are imperfect substitutes for some other more productive but unobserved/non-contractable input (like effort or experienced personnel) then the marginal benefit of service provision under fee-for-service contracts distorts the incentives of providers to combine inputs efficiently which might lead to the underprovision of unobserved inputs and to *worse* health outcomes.

We will just assume that different service levels might lead to different outcomes and will analyze the data to see what kind of distortion there is in reality. To formalize our predictions, let $h_i(x)$ be the health outcome of patient i which depends on health services x. If $h' \neq 0$, then:

$$x^{fee} > x^{cap} \rightarrow h^{fee} \neq h^{cap}. \tag{3}$$

In other words, differences in contract types between EPS and IPS have an effect on the type of service that IPS provide their patients, which in turn, may have an effect on health outcomes. We can test this prediction comparing identical patients who are served under different contract types, which we do using data of the Colombian health system that contains patient-level information about every health service received by a large sample of patients enrolled in the contributory system and who claimed *any* health services over a span of three years. Notice that our model does not predict that health outcomes are different in any particular direction depending on the contract type, but only that health outcomes are different. Our empirical analysis will just test whether this distortion is present in the data, comparing the outcomes of patients with either type of contract.

The model above assumes that the choice of contract type is exogenous, whereas it really is the result of a negotiation between insurer and provider. Even if the contract type was exogenous, the terms of the contract (i.e. the values of T and p in the model) are the result of a bargaining process which depends on variables that may vary systematically across insurers and providers. The understanding of the mechanisms of contract choice and bargaining between insurer and provider in the Colombian health care system goes beyond the scope of this paper. Nevertheless, our empirical analysis below will account for the fact that contract choice is not purely random.

IV Empirical analysis

I Empirical framework

We hypothesize that contracts between insurers and providers have an impact on the decisions of service providers when serving a patient. These choices, in turn, may have an effect on health outcomes, as discussed above in our theoretical framework. In our data set, it is difficult to separate services and outcomes. For example, since we focus on patients with long-term diseases, we see patients that go to the emergency room for no other reason in the data than their underlying long-term condition. On the other hand, there are some conditions (like strokes) that are well defined outcomes that are also clearly recorded in the data. We will call all these outcome variables "outcomes" in the sense that they are stochastic consequences of the health services received by patients.

In order to test our hypothesis, we will compare the outcomes of patients during the months after being diagnosed with a long term condition. The patients are similar according to their observable characteristics, except for the type of contract between their IPS and their EPS. Specifically, we compare several outcomes of patients who have been healthy for six months and are diagnosed with a chronic condition under capitation and fee-for-service contracts. We follow each diagnosed patient during eight months after the diagnosis and control for all other observed characteristics, so that we can attribute any difference in outcomes to the contract type.

Define formally H_i^j as the stochastic outcome j for patient i. Let Z_i be the observed characteristics of patient i, including the characteristics of her IPS provider, her EPS insurer and all her observed demographic characteristics. Let also $\mathbf{1}_i^{cap}$ be an indicator that takes value one if patient i is being served under a capitation contract, or value zero if patient i is being served under a fee-for-service contract. We do not observe the exact level of services received by each patient, but our model suggests that they depend only on the type of contract and the characteristics of the patient.

The health outcome of patient i is a function of the observed characteristics of the patient and provider, the type of contract and some other unobserved shock:

$$H_i^j = H^j(Z_i, \mathbf{1}_i^{cap}, \epsilon_i), \tag{4}$$

where ϵ is the unobserved shock which may be an unobserved characteristic of the patient, the provider, or both, and which is assumed to be uncorrelated with the contract type. We discuss the merits of this assumption below. Given (4), we will test whether outcomes are different depending on the contract type, conditional on the observed characteristics of patients and providers:

$$E_\epsilon[H_i^j|Z_i, \mathbf{1}_i^{cap} = 1] - E_\epsilon[H_i^j|Z_i, \mathbf{1}_i^{cap} = 0] \neq 0. \tag{5}$$

In simple terms, we will just compare the outcomes of patients who are similar to each other, according to their observed characteristics, who are diagnosed for the first time with a long term condition and whose only difference is the type of contract between the IPS and the EPS. We will compare outcomes using standard regression techniques and semiparametric matching estimation techniques. We will consider a number of different outcomes and will also focus on specific long term conditions.

II Description of the data

Our analysis is based on the administrative data of the contributory system known as the “Base de Suficiencia”, which contains information of all the system’s registered users. The system contains the basic information of every user, even those who never receive a service. Moreover, the system registers every time a user receives a service by any health service provider in the system. Every type of service generates a claim that is codified, as well as the corresponding health diagnosis. Since the system coordinates all the payments from insurers to providers, it contains information about each of the payments associated with each claim.

To study the relation between market power, contracts and health outcomes in the Colombian health system, we use an unbalanced panel of 8.7 millions of enrollees in the contributory system and their claims during the years of 2009, 2010, and 2011. The data base was build by the Ministry of Health and Social Protection by randomly selecting a sample of patients who claimed at least one service and who did not change insurer during the time-span. The full data base comprises nearly 460 million claims made by around 40% of the users' population who receive health services during the time span of the sample. We focus only on the set of patients who are diagnosed with a chronic disease during the time span of our data set under either a capitation or fee-for-service contract. Since many patients had been diagnosed with a chronic disease before 2009, we focus only on those who are healthy during the first six months of our data and then are diagnosed for the first time with a long term disease. We follow then each diagnosed patient during a window of eight months and register all their interactions with the system over this time window.

The data set contains the age, gender and the municipality of residence of each individual user. Moreover, it contains the income on which the payroll tax is based (called IBC for its Spanish acronym). This income is a very precise measure of actual income for employees, which correspond to the vast majority of cases in the system. The municipality of each user's residence is matched to one of the three geographic area types defined by the National Administrative Department of Statistics (DANE): metropolitan, normal, or special. The first type corresponds to metropolitan areas and its adjacent municipalities, the second type corresponds to small municipalities around metropolitan areas, and the third type corresponds to more isolated municipalities. Age is also categorized using 12 age groups: 0, 1-4, 5-14, 15-18, 19-44, 45-49, 50-54, 55-59, 60-64, 65-69, 70-74, and 75 or older.

For each user, the data set records the EPS (insurer) to which she is enrolled, the services she demands identified with a procedures code (CUPS by its Spanish acronym)[5], the IPS (provider) that provides the services, the cost per service, the date of provision and the International Classification of Diseases (ICD) 10 codes associated to each service. These ICD 10 codes are categorized following 29 long-term diseases proposed by Alfonso et al. (2013).[6]

Importantly for our purposes, the data set contains the type of contract under which each payment to the IPS is made. In other words, for every claim we observe the payment made from the EPS to the IPS and the type of contract between the EPS and the IPS under which the service is provided. The problem is that type of contract under which any given patient is served can change over time or can be different depending on the IPS that they go to, even after being diagnosed with a chronic disease. In fact, the contract type can change *endogenously* after a patient has been diagnosed with a long-term condition. Since we are interested in uncovering the causal effect of contract type on outcomes, we have to use only the variation in contract types that is not correlated with the condition of each patient.

[5]CUPS stands for Código Único de Procedimientos and is a dictionary of all services, procedures, and medications included in the colombian benefits package.

[6]For more details on the construction of these long-term disease groups see www.alvaroriascos.com\research\healthEconomics

Therefore, we classify each patient's contract type according to the type of contract under which they were first diagnosed. In other words, we assign each diagnosed patient the type of contract between her EPS and the IPS where she was diagnosed with a long term condition as recorded in our data set. We also choose patients that are first healthy according to our data, in the sense that they have no diagnosis of a long term condition during the first six months of our sample. We also perform robustness checks using much smaller samples of patients who don't receive *any* service during the first six months in our data. Then, as indicated above, we keep track of their outcomes during the following eight months, which means that we also have to disregard patients who are first diagnosed during the last eight months of our data set. We also include in our sample only those patients who are diagnosed under *capitation* or $fee-for-service$ contracts, who constitute the great majority of the patients in our sample.

Since our data set is very large, we still end up with a very sizable sample. Table I contains some relevant descriptive statistics of our data set. As shown in panel I, the full data set contains information on more than 7.5 million patients, of which more than 3.5 million have long term diseases, according to our definition. The full sample contains more than 460 million claims. As indicated above, we cannot use the full sample of patients with long-term conditions, because a big portion of them were diagnosed before the start of our sample and, therefore, we don't observe the contract type under which they were first diagnosed. Moreover, we have to also eliminate patients who are first diagnosed during the last eight months of our sample because we cannot observe them over a full 8-month window, as indicated above.

As shown in panel II, the sample of patients who are healthy during the first six months of our sample contains almost 4.9 million individuals and more than 150 million claims. Of these individuals, a total of more than 1.2 million were first diagnosed with a long term condition $after$ the first six months and before the last eight months of our data. The table shows the distribution of long-term conditions among these diagnosed patients. Notice that more than half of these long-term patients have been diagnosed with either hypertension or other cardiovascular disease. Other types of chronic diseases with large shares are different types of cancer and arthritis.

Of these patients who were first diagnosed with a chronic disease, we focus only on those who are diagnosed under *capitation* or $fee-for-service$, whose outcomes are described in panel III of the table. Notice first that *capitation* and $fee-for-service$ patients are 57% and 31% of all the chronic patients described in panel II. This means that around 12% of these patients are diagnosed under a different type of contract, which we are going to ignore.[7]

[7] Most of these remaining patients are served under a different type of contract newly allowed by the regulation called "bundle" ("paquete" in Spanish) which allows charging for bundles of services depending on the diagnosis. The incentives generated by this type of contract are less clear and its exclusion doesn't affect our analysis because patients do not observe the contract type when they choose an IPS to be diagnosed with a chronic condition for the first time.

A first inspection of the unconditional mean outcomes in panel III reveals that they vary systematically across contract types. Columns (a) and (b) show the mean outcomes for patients who are first diagnosed under a capitation or a fee-for-service contract, respectively; the outcomes are registered over an eight month window after the diagnosis. The outcomes that we focus on are the number of visits to the Emergency Room (ER visits), the number of hospital days (Length of stay or LOS), the likelihood of being admitted to an intensive care unit (ICU admission), the likelihood of a septic infection (sepsis), the likelihood of a pneumonia contracted directly at a hospital (nosocomial pneumonia), the number of heart attacks (AMI) defined broadly or severe enough to require an angioplasty and the likelihood of an ischemic attack.

We also measure the costs associated with each patient during the same time window. The table shows that the costs of capitation patients is lower than the costs of fee-for-service patients, which is consistent with the type of distortions predicted by our model. This difference is nevertheless misleading, because it does not take into account the capitation costs of patients who did not make any claim. Recall that under a capitation contract the provider gets a fixed payment for a set of potential patients whether they receive any service or not. Since we only observe payments related to the treatment of the patients who get sick, the recorded cost under capitation underestimates the full cost of the contract, and for this reason we won't show any result related to costs in the remainder of the paper.

It is quite apparent from panel III that, across the board, outcomes under fee-for-service are "worse" than under capitation. For all outcomes the difference between both mean outcomes shown in column (c) is statistically significant. The difference in the unconditional mean outcomes across contract types is consistent with the theoretical notion that contracts distort the incentives of providers, and therefore generate different outcomes. Nevertheless it is no proof that the contract type *causes* the difference, because the contract choice may be systematically correlated with characteristics of the patients that both EPS and IPS observe when negotiating their contracts. In fact, the basic demographic characteristics of these long term patients differs systematically, as shown in panel IV of table I. In particular, patients who are diagnosed with chronic diseases under capitation contracts tend to be older and to be more concentrated in urban areas than long term patients who are diagnosed under fee-for-service contracts. Since both insurers and providers observe these characteristics when writing their contract it might well be that the difference in outcomes is just a reflection of these characteristics.

In order to show that the difference in the contract type actually causes the difference in outcomes, we have to examine the differences in outcomes, conditional on the characteristics of patients that are observed by the IPS and EPS before the patient is diagnosed. We need to do so, because IPS and EPS can condition their bilateral contracts on whatever variable that they observe. Therefore, we perform a detailed econometric analysis to compare similar patients who are diagnosed under capitation and fee-for-service contracts, as we describe in the following section.

III Econometric analysis of the relationship between contract types and outcomes

We have shown so far that capitation and fee-for-service contracts are correlated with different outcomes and costs for patients who are healthy and then are diagnosed with a chronic condition for the first time. Nevertheless, we have also shown that patients who are diagnosed under the different types of contracts have different observed characteristics. In order to show that the contracts *cause* the difference in outcomes the way the theory predicts, we need to show that these differences in outcomes persist when we control for the characteristics of patients that the insurer and the provider observe when they write the contract.

To do so, we will compare similar patients who are healthy in the sense that they have not received *any* long-term disease diagnosis for at least six months and then they are first diagnosed with a chronic condition. The only difference between patients is the type of contract. Since we look at patients who are similar in terms of their age, gender, etc, the variation in contract type at the time of diagnosis is orthogonal to their characteristics and is, therefore, exogenous[8]. We compare the outcomes that we listed above for patients with chronic conditions described in panel III of Table I.

In order to compare similar patients with different contract types, we estimate (5) using standard econometric techniques. Consider first the following linear regression:

$$H_i^j = \beta_0^j + \beta_Z^j Z_i + \beta_C^j \mathbf{1}_i^{cap} + \epsilon_i^j, \tag{6}$$

where H_i^j is the outcome j of interest for patient i; Z_i is the vector of patient, IPS and EPS characteristics; and $\mathbf{1}_i^{cap}$ is a capitation indicator that takes value one when the patient i was diagnosed under a capitation contract, and takes value zero if the patient was diagnosed under a fee-for-service contract. The error term ϵ_i^j in (6) is assumed to be uncorrelated with the contract choice $\mathbf{1}_i^{cap}$, conditional on Z_i. For some binomial outcomes, we will also consider analogous logit regressions of the form:

$$H_i^j = logit(\beta_0^j + \beta_Z^j Z_i + \beta_C^j \mathbf{1}_i^{cap}), \tag{6'}$$

The inference of the effect of capitation on outcomes relies on the orthogonality assumption of the error term. Given that we use only patients who are healthy during the first six months of our sample, this is a reasonable assumption. The reason is that EPS and IPS only observe the same variables Z_i that we observe before the patient is first

[8] After diagnosis the contract type under which each patient is treated may change endogenously in response to the condition of each patient *and* the type of contract under which she was diagnosed. We show, in fact, that for each condition treatment changes systematically across contract types. Notice that these endogeneity, if anything, should atenuate the estimated effect of contract types in the data. The fact that we find statistical significant effects of contracts on outcomes is therefore evidence of a substantial economic effect.

diagnosed, and therefore the contracts cannot be conditioned on anything that we don't observe[9]. Moreover, the type of contract is never observed by patients and, therefore, they cannot systematically select themselves into specific IPS or EPS depending on unobserved characteristics of their health, specially before they are first diagnosed.

We estimate (6) and (6') via MLS and ML, respectively, for the same outcomes listed in panel III of Table I. We run specifications using as regressors Z_i different subsets of the characteristics listed in in panel IV of Table I. We also run specifications with fixed effects for the specific diseases listed in panel II of Table I. Our parameter of interest is β^j_{cap}, which measures the effect of the capitation on contract on the outcome H^j_i relative to patients under fee-for-service contracts.

We show the results of these regressions on table II. We run one separate regression for each outcome j and show the estimated β^j_{cap} coefficient, which measures the effect of the capitation contract on each outcome according to (6) or (6'). Each column corresponds to the same regression with controls as indicated in the lower panel. The first column corresponds to the regression with no controls, the results in the second column control for patient demographic characteristics including the number of services received by patients during the six months $before$ the diagnosis, which allows us to compare patients with similar contact with the system before the diagnosis. The results in the third column include additional long-term disease fixed-effects; the results in the fourth column include additional insurer fixed effects; and the results in the fifth column include provider controls. The provider controls are a set of characteristics of the IPS related to the number and type of patients they serve. Models for ER visits, LOS and AMI (i.e. heart attacks, under its two definitions) are linear regressions as (6), while for the remaining outcomes logistic regressions as (6') were used. Each outcome is measured during the eight months following the moment the patient receives her first long-term disease diagnosis conditional on not having a diagnosis during the first six months of data. Variations in the number of observations are due to missing values in the demographic characteristics.

Notice that the results of the regressions with no controls, shown in column (1), reflect the differences in means shown in table (I), except for the functional form of the regression. In any case, the difference in the unconditional means shown in column (1) is statistically different for all outcomes. As we add controls in columns (2)-(5), these negative differences are still significant. In fact, there is no evidence that the differences decrease at all once we condition on the characteristics of patients, providers and insurers. In other words, the differences in the underlying characteristics of patients, providers and insurers cannot account for the differences in outcomes across contract types.

[9]The only exception is when contracts are based on health conditions detected before the six-month window. Since our results are robust to the increase in the size of the window, we do not believe that this effect is significant.

The results shown in Table II are based on the data of all patients who were diagnosed with *any* long-term condition after the first six months of the sample. Even though we condition on specific diseases, the estimated effect of capitation assumes that all the observed patient, insurer and provider characteristics have similar effects on the outcomes across diseases. In Table III we show the results of separate regressions for the most notorious groups of long-term patients in our data base, which are patients diagnosed with cardiovascular disease, renal disease, cancer, diabetes and hypertension. We show the results of the regressions with all controls.

The results are qualitatively similar as in table II. The difference in the conditional means is negative and statistically significant across almost all outcomes. In particular, the effect is negative and significant for outcomes that are common to all conditions, like ER visits, LOS and ICU admissions. It is worth noting that the effect of capitation on the outcomes is nevertheless heterogenous among the different conditions.

In some cases, like renal disease in column (2) the lack of significance of capitation on outcomes, such as heart and cerebral attacks should be a reflection of the specificity of the condition or a reflection of the much smaller sample. The one big exception is the effect of capitation $vis-a-vis$ fee-for-service on ischemic attacks among cancer patients, which is positive and significant at the 90% confidence level. We can not provide an explanation for this result because the medical connection between ischemic attacks and cancer is not clear. Notice, though, that capitation has a negative effect on the ER visits, LOS and ICU admissions of cancer patients within the eight months window after diagnosis.

The results shown so far come from linear regressions, which might be problematic if the support of the distribution of patient characteristics is different across contract types. In order to solve this problem we estimate directly the effect of capitation on outcomes for similar patients using matching regressions. Specifically we use a matching estimator to infer the average treatment effect (ATT) of capitation vis-a-vis fee-for-service on all the outcomes above.

In Table IV we present the average treatment effect on the treated (ATT) on outcomes. In this case, the treatment is defined as the event of being diagnosed under a capitation contract, while the control is the event of being diagnosed under a fee-for-service contract. Treatment and control patients are compared directly based on their demographic characteristics (gender, age group, and income group) and the insurer to which they are enrolled. Specifically, for each treated (capitation) patient we find a similar control (fee-for-service) patient, based on the vector of characteristics. We then compute the difference in outcomes and average across patients to obtain the ATT. We restrict the number of controls per treated to one, but a single control can be matched to several treated individuals. The results that we show are based on the subset of more than 500.000 patients that we can match.

Notice in Table IV that the results resemble the LS results shown in Table II. The effect is negative and significant for all outcomes. Notably, for these matched patients, the negative effect of capitation on the outcomes that are common to all conditions (ER visits, LOS and ICU admissions) is larger. In other words, for comparable patients, the data suggest that the negative effect of capitation on outcomes is larger. The difference in magnitude between the matching and LS regression results suggests also that the effect of contract types on health outcomes is heterogeneous across patient types.

In Table V we show the results of the same matching regression performed for patients diagnosed with each chronic condition. Qualitatively, the results of this table are very similar to the results of the linear regressions shown in table III. It is still the case that, relative to fee-for-service, capitation has a significant negative effect on the outcomes that are common to all conditions (ER visits, LOS and ICU admissions). The magnitude of these effects is nevertheless larger for these matched patients, which again implies that the effects are heterogenous across patients and larger than what the linear regression suggests.

The effects of capitation on the more specific medical outcomes (sepsis, pneumonia, heart attacks and brain strokes) vary across conditions in a manner that is consistent with the type of condition. For example, capitation has a strong negative effect on heart attacks and strokes among patients with heart disease, diabetes and hypertension, but has no effect among patients with renal disease. These differences in effects across conditions are not surprising, since the mechanics of the distortion caused by contracts on outcomes is mediated by the medical peculiarities of each condition. It is clear, though, that among these matched patients, capitation has no positive effect for any of the included chronic conditions.

The conclusion of the econometric analysis so far is that the contract type is strongly correlated with outcomes, conditional on large set of patient characteristics. This result is consistent with the hypothesis that contract types *cause* the differences in outcomes via the distortion of the incentives predicated by the theory. The concern with this type of results is that there always might be additional patient characteristics that are observed by the insurers and providers, but are unobserved to the econometrician, which determine both the contract type *and* the outcome.

In order to alleviate this concern, we performed a number of robustness checks. First, we change the size of the time window before diagnosis that we use to construct our data set. We focus on this specific window because we are concerned about the ability of insurers and providers to use any previous interaction of patients with the system to screen them and assign them a specific type of contract. In our specifications above, we only look at patients that are "healthy" during the six months before the diagnosis of a long term condition. In table VII we show the results corresponding to the same regressions of tables II and III except that we use bigger windows of 9 and 12 months. The results are less precise, because the samples are smaller, but the results qualitatively identical. We also experimented with different number of months over which we follow patients *after* diagnosis and the results do not change at all.[10]

Second, we focus on patients who have had no interaction with the system before diagnosis, since it is very unlikely that these patients have been screened before diagnosis. Without any interaction between patients and providers, the set of patient characteristics that we observe is the same set of patient characteristics observed by insurers and providers and therefore the potential endogeneity of the contract type is much less likely. To do so, we construct a smaller sample with patients that have at most one claim during the six months before diagnosis. We use chronic patients with at most one service before diagnosis because there are very few observations with zero services during these previous six months since a diagnosis of a long-term condition usually requires at least one initial consultation with a doctor.

The results of this exercise using only patients who have had at most one service before being diagnosed with a long term condition are shown in table VI. We show results based on all patients and results for patients diagnosed with each set of long term conditions. The results correspond to the capitation coefficient obtained from the regressions with the full set of controls. Notice that the samples are much smaller, but still the results are generally consistent with our previous results. For all patients, capitation has a negative and statistically significant effect on length of stay (LOS) and admissions to ICU. For all other outcomes, except pneumonia, the estimated effect is negative albeit non-significant.

For the remaining diseases, the results are similar: the estimated coefficients are largely negative and some of them are statistically significant, despite the size of the sample. In the case of the disease with the largest number of patients, which is cardiovascular disease, capitation has a negative and statistically significant effect on ER visits, LOS, ICU admissions, heart attacks (AMI, broadly defined) and ischemic attacks. All of these outcomes are directly associated with heart disease. The effect of capitation on sepsis and pneumonia is negative but insignificant, which might be a reflection of the fact that these outcomes are only loosely related with heart disease. These results reinforce our conclusion that contract types have a casual effect on outcomes, like the theory predicts.

IV Additional results: the mechanics of the distortions and the determination of contract types

We have shown that outcomes vary significantly across contract types. In particular, we have shown that patients who are first diagnosed with a chronic condition have outcomes over the following eight months that are significantly different when first treated under a capitation contract than when they are first treated under a feee-for-service contract. This result is consistent with the distortion in incentives predicted by the theory of

[10] We do not show these results but they are available upon request.

contracts. In this case, the distortion leads to outcomes that are apparently better when the patient is diagnosed under a capitation contract than a fee-for-service contract. We finish this empirical analysis exploring the mechanics of the distortion and the determination of contract types between insurers and providers.

In order to scrutinize how contract types and incentives translate to different outcomes, we estimate the likelihood of being referred to a specialist after being diagnosed with a long term condition depending on contract type using a logit regression as (6'). In this case, the outcome of interest is a discrete variable that takes value 1 if the patient is referred to a specialist during the eight months after diagnosis. We also estimate the same equation defining the dependent variable based on visits to a specialist before *and* after diagnosis to make sure that we are not picking up a trivial effect of being referred to a specialist just after the diagnosis. The dependent variables are the same as before, and include a capitation indicator variable and the same rich set of controls.

We also estimate the correlation between the what are called 2nd and 3rd degree services and contract type using a linear regression identical to (6). These services are relatively complex lab tests and procedures different than a visit to a specialist. We use as a dependent variable the total number of these services during the eigth months after diagnosis, and the same set of regressors as before. We also run the same regression using the total number of these services before *and* after diagnosis inorder to make sure, again, that we are not picking effects related to the timing of the services instead of the real effect of contract type on these services.

We show the results of these regressions based on the sample that includes all long-term patients who were diagnosed first after the first six months of our sample in table VIII. The main result of these exercises is the striking difference between the effect of capitation on specialist referals vis-a-vis other complex services. Capitation is in general correlated with a significantly *higher* likelihood of referal to a specialist, except when we condition on the insurer but not on the characteristics of the provider. On the other hand, capitation is correlated with significantly *less* complex services across all specifications. These results suggest that capitation has an effect on the incentives on the diagnosing provider to follow up the diagnosis with referals to a specialist, wheras fee-for-services generates incentives to order more lab tests and procedures. In turn, as we have shown already, these differential treatment patterns lead to different outcomes. This result is consistent with anecdotal evidence among industry insiders suggesting that capitation leads to a referal to a specialist because it is the easiest (and cheapest) action[11].

We finalize this empirical analysis examining the determinants of contract types. In a market with no frictions, the difference in outcomes across contract types would be wiped out by the arbitrage among patients who would choose the insurer depending

[11]There is even a dictum in the industry saying "capito ergo remito" which translates literally as "capitate then refer" that illustrates the preponderance of this incentive.

on the expected outcome, until expected outcomes are equalized across insurers and, at least to some extent, contract types. In our case, the contract type is completely unobserved by patients and the costs of switching between insurers are high and limited by law. This inobservability of contract types by patients is what allows us to infer the causal effect of contract types. For practical purposes, the assignment of contract types is exogenous for patients who are diagnosed with a long term condition and who have been seemingly healthy for the previous six months, and who are similar according to any other observable characteristic. It can therefore be inferred that any difference in outcomes thereafter is the result of the contract type, just like the theory predicts.

However, contract types are the result of the systematic interaction of providers and insurers. We show now that the contract type is correlated with the bargaining power of either the provider or the insurer within the relevant market, just like a standard bargaining theory would predict. The understanding of the bargaining mechanism is beyond the scope of this paper, but the data shows that there is a coherent economic principle behind the assignment of contract types.

Consider the following regression to infer the correlation of capitation and the market shares of insurers and providers:

$$\mathbf{1}^{cap}_{i\in m} = \gamma_0 + \gamma_1 s_{IPS_i,m} + \gamma_2 s_{EPS_i,m} + \gamma_z Z_i + u^{\gamma}, \tag{7}$$

where $\mathbf{1}^{cap}_{i\in m}$ is the capitation indicator variable described above for patient i who is located in market m, defined as the regional jurisdiction (departamento) in which we compute the market shares. The variable EPS_i is the insurer of user i, while IPS_i is the IPS that provides most services to patient i during the span of our sample. The variable $s_{IPS_i,m}$ is the market share of users served by the IPS_i in market m, and $s_{EPS_i,m}$ is the share of all users that are enrolled in insurer EPS_i. The variable Z_i is the same vector of controls used in regressions (6) and (6').

The results of this regression are displayed in table IX. We show the estimated coefficients γ_1 and γ_2 which measure the correlation of market shares and the likelihood of capitation, conditional on the controls. The results imply that capitation at the patient-level is positively and significantly correlated with the market share of the insurers, while it is negatively and significantly correlated with the market share of the providers. In other words, the higher the market power of the insurer, the higher the probability of capitation; on the other hand, the higher the market power of each provider within its local market, the lower the likelihood of capitation. The result is robust to the addition of the controls.

These results highlight the fact that capitation contracts which shift all risk to providers are preferred by insurers, while fee-for-service contracts that shift all risk to the insurer are preferred by providers. Understanding the detailed mechanics of this correlation is not the focus of this paper, but still the correlation clarifies the process that generates the assignment of contract type to each patient, and justifies the exogeneity assumption made above, since the market shares of insurers and providers are not observed by patients when choosing a provider before being diagnosed with a chronic condition.

V Conclusion

We have shown that there is evidence of substantial distortions in the health outcomes of long-term patients, associated with the contract type of each patient at the time of diagnosis. These distortions are not explained by differences in the observable characteristics of patients that could be used by insurers or providers to select patients into contract types. Therefore, the distortions are consistent with a theory that predicts that contract types cause them.

The mechanism through which contract types affect outcomes seems to be related to systematic differences in the patterns of treatment following diagnosis. Moreover, the determination of contract types is correlated with the market shares of insurers and providers in a manner that is consistent with the prediction of the bargaining theory. Further research should address in more detail the mechanisms through which contracts affect patient outcomes and evaluate counterfactual policies, probably through an empirical structural model.

The data show that capitation contracts lead to better outcomes across the set of most important long-term conditions in the Colombian health care system. Since contract types are regulated by the government, the theory and the empirical evidence suggest that the regulation can be adjusted to improve patient outcomes. It is also important to highlight that the relevance of the results goes beyond the Colombian case, since market design in health care systems is not only a important and open research topic, but also an urgent policy problem around the world.

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Table I: Descriptive statistics

I. Full data base				
Patients				8,683,839
Claims				460,368,082
Patients with long-term diseases				3,561,033
II. Sample conditional on not being diagnosed with a long-term disease during the first six months				
Patients				4,922,052
Claims				168,140,177
Patients with long-term diseases after the sixth month				1,429,509
	Disease (%)†			
	Genetic Anomalies			6.7
	Arthritis			5.7
	Pyogenic Arthritis			0.4
	Osteoarthritis			11.1
	Asthma			6.5
	Autoimmune Disease			2.0
	Cervical Cancer			19.7
	Invasive Cervical Cancer			0.2
	Male Genitalia Cancer			1.8
	Breast Cancer			8.3
	Melanoma			1.2
	Digestive Organ Cancer			0.6
	Respiratory Organ Cancer			0.2
	Other Cancer			5.0
	Female Genitalia Cancer			0.9
	Lymphatic Tissue Cancer			0.6
	Cancer Therapy			0.1
	Diabetes			5.5
	Hypertension			26.4
	Other Cardiovascular Disease			26.3
	Long-term Lung Disease			6.5
	Chronic Renal Failure			1.3
	Other Renal Failure			0.4
	Other Renal Disease			1.5
	Long-term Renal Disease			0.1
	AIDS-HIV			0.7
	Epilepsy			2.0
	Organ Transplant			0.1
	Tuberculosis			1.3
III. Contract types (%) and outcomes ‡		***(a) Capitation: 57.7%***	***(b) Fee-for-service: 31.1%***	***(c) diff (a)-(b)***
	Outcomes			
	ER visits	0.2726	0.3851	-0.1125***
	Length of stay (LOS)	1.1110	2.0260	-0.9154***
	ICU admission	0.0034	0.0098	-0.0064***
	Sepsis	0.0002	0.0006	-0.0004***
	Nosocomial pneumonia	0.0016	0.0044	-0.0028***
	AMI (broad)	0.0382	0.1357	-0.0975***
	AMI (angioplasties)	0.0009	0.0028	-0.0019***
	Ischemic attack	0.0006	0.0012	-0.0006***
	Costs	515,100	844,800	-329,717.4***
IV. Mean demographics				
	Male	0.3585	0.3534	0.0050***
	Urban	0.8109	0.6775	0.1334***
	Normal	0.1735	0.2791	-0.1056***
	Special	0.0156	0.0434	-0.0278***
	Age	44.1339	41.3408	2.7931***
	Log(Income)	13.4981	13.5046	-0.0065***

Note: This table presents the number of patients, claims, and patients with long-term diseases in the full sample and in the sample conditioned on not receiving a long-term disease diagnose during the first six months. For patients with long-term diseases in the conditioned sample, the distribution by illness and the percentage under capitation and fee-for-service is displayed. Average health outcomes for patients under capitation and fee-for-service are also reported. (†) Percentages do not add up to 100 because a patient can have more than one long-term disease. Column *diff* shows the confidence level at which differences between the average outcome in capitation and the average outcome in the fee-for-service are significant: (*) for 90%, (**) for 95%, and (***) for 99%. (‡) Percentages do not add up to 100 because some patients are treated under contracts other than capitation or fee-for-service. Authors' calculations based on data of the "Base de Suficiencia" of the Ministry of Health and Social Protection in Colombia.

Table II: Effect of capitation (as defined by first diagnosis) on several health outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
ER visits	-0.113^{***}	-0.113^{***}	-0.105^{***}	-0.091^{***}	-0.143^{***}	-0.129^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
LOS	-0.915^{***}	-0.921^{***}	-0.941^{***}	-0.764^{***}	-1.392^{***}	-1.078^{***}
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.018)
ICU admission	-1.018^{***}	-1.021^{***}	-1.191^{***}	-0.976^{***}	-1.653^{***}	-1.405^{***}
	(0.026)	(0.026)	(0.026)	(0.027)	(0.031)	(0.032)
Sepsis	-0.975^{***}	-0.972^{***}	-1.055^{***}	-0.871^{***}	-1.038^{***}	-0.848^{***}
	(0.098)	(0.098)	(0.100)	(0.102)	(0.121)	(0.123)
Nosocomial pneumonia	-0.995^{***}	-0.996^{***}	-0.957^{***}	-0.768^{***}	-0.686^{***}	-0.553^{***}
	(0.035)	(0.036)	(0.036)	(0.037)	(0.042)	(0.043)
AMI (broad definition)	-0.098^{***}	-0.098^{***}	-0.114^{***}	-0.104^{***}	-0.179^{***}	-0.168^{***}
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.018)
AMI (angioplasties)	-0.002	-0.002	-0.002	-0.002	-0.003	-0.003
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.018)
Transient cerebral ischemic attack	-0.650^{***}	-0.650^{***}	-0.816^{***}	-0.602^{***}	-0.524^{***}	-0.438^{***}
	0.062	(0.062)	(0.063)	(0.064)	(0.074)	(0.076)
N	1,269,002	1,269,002	1,268,483	1,268,483	1,268,483	1,268,483
Controls						
Number of previous claims		yes	yes	yes	yes	yes
Demographics			yes	yes	yes	yes
Long-term disease				yes	yes	yes
Insurer					yes	yes
Provider						yes

Note: This table reports the estimated coefficient of the capitation indicator in regressions of the health outcome (in each row) on different sets of controls. Estimations are over the sample of patients diagnosed with any long-term disease after the first six months of data. Column 1 has no controls, column 2 controls for number of claims during the six months before entering the sample, column 3 includes patient demographic characteristics, column 4 adds long-term disease fixed effects, column 5 includes insurer fixed effects, and column 6 additionally controls for provider characteristics. For ER visits, LOS, AMI (broad definition) and AMI (angioplasties) models correspond to OLS regressions, while for ICU admission, Sepsis, Nosocomial pneumonia, and Transient cerebral ischemic attack models correspond to logistic regressions. Authors' calculations based on the "Base de Suficiencia" of the Ministry of Health and Social Protection. Robust standard errors in parenthesis. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table III: Effect of capitation (as defined by first diagnosis) on several health outcomes conditional on subsets of diagnosis

	(1)	(2)	(3)	(4)	(5)
	Cardiovascular disease	Renal disease	Cancer	Diabetes	Hypertension
ER visits	-0.214^{***}	-0.110^{***}	-0.008^{***}	-0.124^{***}	-0.134^{***}
	(0.002)	(0.008)	(0.002)	(0.006)	(0.003)
LOS	-1.610^{***}	-2.847^{***}	-0.474^{***}	-2.082^{***}	-0.959^{***}
	(0.034)	(0.213)	(0.035)	(0.135)	(0.050)
ICU admission	-1.619^{***}	-1.024^{***}	-0.865^{***}	-0.749^{***}	-1.025^{***}
	(0.041)	(0.241)	(0.098)	(0.171)	(0.086)
Sepsis	-0.889^{***}	-1.975^{***}	-0.318	-0.468	-0.933^{***}
	(0.218)	(0.617)	(0.333)	(0.501)	(0.330)
Nosocomial pneumonia	-0.686^{***}	-0.322	-0.280	-0.813^{***}	-0.681^{***}
	(0.085)	(0.445)	(0.173)	(0.299)	(0.130)
AMI (broad definition)	-0.440^{***}	-0.006	-0.002	0.004	-0.044
	(0.034)	(0.213)	(0.035)	(0.135)	(0.050)
AMI (angioplasties)	-0.006	0.0003	-0.0001	0.001	-0.001
	(0.000)	(0.213)	(0.035)	(0.135)	(0.050)
Transient cerebral ischemic attack	-0.586^{***}	0.373	0.627^{**}	-0.131	-0.213
	(0.098)	(0.844)	(0.297)	(0.510)	(0.167)
N	457,348	16,622	292,492	70,289	334,893
Controls					
Number of previous claims	yes	yes	yes	yes	yes
Demographics	yes	yes	yes	yes	yes
Insurer	yes	yes	yes	yes	yes
Provider	yes	yes	yes	yes	yes

Note: This table reports the estimated coefficient of the capitation indicator in regressions of the health outcome (in each row) on different sets of controls. Each column reports results conditional on a subset of patients with particular long-term diseases. Column 1 focuses on patients with any cardiovascular disease, column 2 on patients with renal disease, column 3 on patients with cancer, column 4 on patients with diabetes, and column 5 on patients with hypertension. All the models include patient demographics, insurer fixed effects, provider charac teristics, and number of claims during the six months before entering the sample. For ER visits, LOS, AMI (broad definition) and AMI (angioplasties) models correspond to OLS regressions, while for ICU admission, Sepsis, Nosocomial pneumonia, and Transient cerebral ischemic attack models correspond to logistic regressions. Authors' calculations based on the "Base de Suficiencia" of the Ministry of Health and Social Protection. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table IV: Average capitation effect on several health outcomes in matched individuals

	Capitation at First Diagnosis
ER visits	-0.246^{***}
	(0.0009)
LOS	-2.496^{***}
	(0.017)
ICU admission	-0.028^{***}
	(0.0002)
Sepsis	-0.0004^{***}
	(0.00004)
Nosocomial pneumonia	-0.002^{***}
	(0.00009)
AMI (broad definition)	-0.534^{***}
	(0.009)
AMI (angioplasties)	-0.006^{***}
	(0.0001)
Transient cerebral ischemic attack	-0.001^{***}
	(0.00007)
Matched observations	514,677

Note: This table presents the ATT over different health outcomes -reported in the rows- for the sample of patients diagnosed with any long-term disease after the first six months of data. The treatment is defined as being in a capitation contract and controls are patients in fee-for-service. Individuals are matched directly on gender, age group, number of services before diagnosis, income group and insurer. Authors' calculations based on the "Base de Suficiencia" of the Ministry of Health and Social Protection. Standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table V: Average capitation effect on health outcomes in matched individuals with particular diagnoses

	(1)	(2)	(3)	(4)	(5)
	Cardiovascular disease	Renal disease	Cancer	Diabetes	Hypertension
ER visits	-0.324^{***}	-0.212^{***}	-0.077^{***}	-0.273^{***}	-0.287^{***}
	(0.001)	(0.009)	(0.002)	(0.005)	(0.002)
LOS	-3.036^{***}	-4.583^{***}	-1.916^{***}	-3.842^{***}	-2.670^{***}
	(0.028)	(0.230)	(0.037)	(0.105)	(0.038)
ICU admission	-0.050^{***}	-0.013^{***}	-0.014^{***}	-0.016^{***}	-0.015^{***}
	(0.0005)	(0.002)	(0.0004)	(0.001)	(0.0004)
Sepsis	-0.0004^{***}	-0.003^{***}	-0.0002^{***}	0.0001	-0.0003^{***}
	(0.00006)	(0.0009)	(0.00006)	(0.0002)	(0.00008)
Nosocomial pneumonia	-0.002^{***}	-0.0006	-0.0007^{*}	-0.002^{***}	-0.002^{***}
	(0.0001)	(0.0006)	(0.0001)	(0.0005)	(0.0002)
AMI (broad definition)	-1.203^{***}	-0.007	-0.039^{***}	-0.007	-0.318^{***}
	(0.022)	(0.012)	(0.004)	(0.020)	(0.022)
AMI (angioplasties)	-0.012^{***}	0.0004	-0.00004	0.0007^{*}	-0.006^{***}
	(0.0003)	(0.0005)	(0.00006)	(0.0002)	(0.0003)
Transient cerebral ischemic attack	-0.002^{***}	0.000	0.0003^{***}	-0.0003	-0.0007^{**}
	(0.0001)	(0.0005)	(0.00005)	(0.0002)	(0.0002)
Matched observations	205,428	5,372	115,277	19,019	115,958

Note: This table presents the ATT over several health outcomes reported in the rows. The treatment is defined as being in a capitation contract and controls are patients in fee-for-service. Each column reports results conditional on a subset of patients with particular long-term diseases. Column 1 focuses on patients with any cardiovascular disease column 2 on patients with renal disease, column 3 on patients with cancer, column 4 on patients with diabetes, and column 5 on patients with hypertension. Matching is performed directly over gender, age group, income group, insurer, and number of claims during the six months before entering the sample. Authors' calculations based on the "Base de Suficiencia" of the Ministry of Health and Social Protection. Standard errors in parenthesis.
*p<0.1; **p<0.05; ***p<0.01

Table VI: Effect of capitation on the outcomes of patients with one previous claim being diagnosed with the same disease during consult with specialist

	All patients	Cardiovascular disease	Renal disease	Cancer	Diabetes	Hypertension
ER visits	−0.039	−0.119***	−0.139*	−1.96E-06	−0.125*	0.012
	(0.254)	(0.020)	(0.078)	(0.021)	(0.073)	(0.034)
LOS	−1.925***	−3.074***	−4.305**	−0.667	−6.082***	−0.519
	(0.254)	(0.486)	(1.796)	(0.781)	(1.865)	(0.680)
ICU admission	−1.037***	−1.993***	3.148	1.127	5.486	−14.754
	(0.321)	(0.478)	(180973)	(1.409)	(100246)	(45934)
Sepsis	−6.971	−6.349	−5.00E-08	−7.13E-15	−9.61E-15	2.33E-14
	(10862)	(26702.000)	(65504)	(21715)	(58682)	(29371)
Nosocomial pneumonia	0.080	−2.852	0.313	−13.09	−11.54	−69.57
	(0.525)	(2.405)	(77051)	(20891)	(117953)	(51066)
AMI (broad definition)	−0.318	−0.814*	NA	−0.0002	−0.583	−0.063
	(0.254)	(0.486)	NA	(0.781)	(1.865)	(0.680)
AMI (angioplasties)	−0.005	−0.011	NA	NA	NA	−0.001
	(0.254)	(0.486)	NA	NA	NA	(0.680)
Transient cerebral ischemic attack	−0.721	−1.861*	−1.71E-15	12.32	−3.412	−13.46
	(0.584)	(1.072)	(65504)	(44450)	(194458)	(34336)
N	9,681	2,869	239	2,124	299	1,117
Controls						
Long-term diseases	yes					
Demographics	yes	yes	yes	yes	yes	yes
Insurer	yes	yes	yes	yes	yes	yes
Provider	yes	yes	yes	yes	yes	yes

Note: This table reports the effect of capitation contracts on the health outcomes of patients who claimed one service during the six months before entering the sample, and then were diagnosed with a long-term disease during a consult with specialist. Each column presents estimations on different subsets of patients: column 1 includes all patients, column 2 includes only patients with cardiovascular diseases, column 3 patients with renal diseases, column 4 patients with cancer,column 5 patients with diabetes and column 6 patients with hypertension. NA's are reported in cases where non of the patients in the final subset for estimation presents the outcome being analyzed. Authors' calculations based on the "Base deSuficiencia" of the Ministry of Health and Social Protection. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table VII: Robustness check using different time windows over which patients could not have been diagnosed

	All patients	Cardiovascular disease	Renal disease	Cancer	Diabetes	Hypertension
Panel 1: 9 month window						
ER visits	-0.219^{***}	-0.353^{***}	-0.216^{***}	0.001	-0.225^{***}	-0.230^{***}
	(0.003)	(0.004)	(0.024)	(0.004)	(0.015)	(0.007)
LOS	-1.138^{***}	-1.638^{***}	-3.017^{***}	-0.569^{***}	-2.208^{***}	-0.844^{***}
	(0.021)	(0.039)	(0.238)	(0.042)	(0.154)	(0.059)
ICU admission	-1.435^{***}	-1.638^{***}	-1.383^{***}	-0.889^{***}	-0.754^{***}	-0.995^{***}
	(0.036)	(0.047)	(0.299)	(0.108)	(0.198)	(0.101)
Sepsis	-0.915^{***}	-1.075^{***}	-2.246^{***}	-0.416	-0.967	-1.083^{***}
	(0.137)	(0.244)	(0.773)	(0.374)	(0.638)	(0.354)
Nosocomial pneumonia	-0.544^{***}	-0.739^{***}	-0.419	-0.399^{**}	-1.001^{***}	-0.694^{***}
	(0.051)	(0.100)	(0.572)	(0.195)	(0.345)	(0.154)
AMI (broad definition)	-0.181^{***}	-0.462^{***}	-0.007	-0.003	0.002	-0.046
	(0.021)	(0.039)	(0.238)	(0.042)	(0.154)	(0.059)
AMI (angioplasties)	-0.003	-0.007	0.0001	-0.0001	0.001	-0.001
	(0.021)	(0.039)	(0.238)	(0.042)	(0.154)	(0.059)
Transient cerebral ischemic attack	-0.528^{***}	-0.697^{***}	-0.531	0.748^{*}	-0.247	-0.438^{**}
	(0.087)	(0.113)	(1.758)	(0.385)	(0.702)	(0.193)
N	991,492	343,234	13,138	236,412	31,794	158,688
Panel 2: 12 month window						
ER visits	-0.219^{***}	-0.355^{***}	-0.219^{***}	0.013^{***}	-0.214^{***}	-0.230^{***}
	(0.003)	(0.005)	(0.026)	(0.005)	(0.016)	(0.007)
LOS	-1.186^{***}	-1.661^{***}	-3.148^{***}	-0.664^{***}	-2.219^{***}	-0.831^{***}
	(0.024)	(0.043)	(0.258)	(0.048)	(0.173)	(0.067)
ICU admission	-1.478^{***}	-1.706^{***}	-1.411^{***}	-0.959^{***}	-0.704^{***}	-1.120^{***}
	(0.041)	(0.054)	(0.353)	(0.119)	(0.229)	(0.115)
Sepsis	-0.916^{***}	-0.985^{***}	-3.367^{***}	-0.291	-1.449^{*}	-1.283^{***}
	(0.155)	(0.273)	(1.221)	(0.410)	(0.814)	(0.419)
Nosocomial pneumonia	-0.590^{***}	-0.812^{***}	-0.768	-0.361	-1.217^{***}	-0.847^{***}
	(0.058)	(0.114)	(0.652)	(0.225)	(0.428)	(0.179)
AMI (broad definition)	-0.184^{***}	-0.465^{***}	-0.010	-0.003	0.010	-0.045
	(0.024)	(0.043)	(0.258)	(0.048)	(0.173)	(0.067)
AMI (angioplasties)	-0.003	-0.007	0.0001	-0.0001	0.001	-0.001
	(0.024)	(0.043)	(0.258)	(0.048)	(0.173)	(0.067)
Transient cerebral ischemic attack	-0.498^{***}	-0.671^{***}	-1.972	0.823^{*}	-0.341	-0.376^{*}
	(0.101)	(0.131)	(8,869)	(0.439)	(0.708)	(0.221)
N	790,526	268,447	10,492	193,564	24,536	117,733
Controls						
Number of previous claims	yes	yes	yes	yes	yes	yes
Demographics	yes	yes	yes	yes	yes	yes
Insurer	yes	yes	yes	yes	yes	yes
Provider	yes	yes	yes	yes	yes	yes

Note: This table reports the estimated coefficient of the capitation indicator in regressions of the health outcome (in each row) on the entire set of controls. Panel 1 uses the sample of patients who are not diagnosed with a long-term condition during the first 9 months. Panel 2 uses the sample of patients who are not diagnosed with a long-term condition during the first 12 months. In each case we follow patients 8 months after diagnosis. Each column reports results conditional on a subset of patients with particular long-term diseases. Column 1 includes all patients, column 2 focuses on patients with any cardiovascular disease, column 3 on patients with renal disease, column 4 on patients with cancer, column 5 on patients with diabetes, and column 6 on patients with hypertension. Authors' calculations based on the "Base de Suficiencia" of the Ministry of Health and Social Protection. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table VIII: Insurer and service provider effort (the mechanism)

	(1)	(2)	(3)	(4)	(5)
Panel 1					
Referral to specialist (including claim when diagnosed)	0.200***	0.173***	0.207***	0.007	0.039***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Referral to specialist (including previous claims)	0.036***	0.015***	0.034***	-0.068***	-0.031***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Panel 2					
Number of 2nd and 3rd degree services (claims 8 months after)	-0.046***	-0.052***	-0.038***	-0.059***	-0.049***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Number of 2nd and 3rd degree services (including previous claims)	-0.069***	-0.083***	-0.065***	-0.099***	-0.081***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
N	1,269,002	1,268,483	1,268,483	1,268,483	1,268,483
Controls					
Number of previous claims	yes	yes	yes	yes	yes
Demographics		yes	yes	yes	yes
Long-term disease			yes	yes	yes
Insurer				yes	yes
Provider					yes

Note: The first panel of the table reports the estimated coefficient of the capitation indicator on: (i) the probability of referral to the specialist including both consults during the 8 months after diagnosis and whether the service where the patient was diagnosed was a consult with specialist, and (ii) the latter plus consults with the specialist during the six months before entering the sample. The second panel reports the effect of capitation on (i) the number of second and third level services (high complexity services) measured 8 months after diagnosis and (ii) on the number of such services adding those claimed during the six months before entering the sample. The first column of each estimation controls for number of claims during the first six months before entering the sample, column 2adds patient demographic characteristics, column 3 includes long-term disease controls, column 4 adds insurer fixed effects and column 5 additionally controls for provider characteristics. Authors' calculations based on the "Base de"Suficiencia" of the Ministry of Health and Social Protection. Robust standard errors in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table IX: Correlation between market power and likelihood of capitation

	Capitation due to First Diagnosis		
	(1)	(2)	(3)
Share EPS	0.321***	0.441***	0.436***
	(0.002)	(0.002)	(0.002)
Share IPS	−0.678***	−0.399***	−0.392***
	(0.005)	(0.005)	(0.005)
N	963,882	963,495	963,495
Controls			
Demographics		yes	yes
Long-term disease			yes

Note: This table presents the correlation between insurer and provider market share with the likelihood of being in a capitation contract. Insurer market share is calculated as the share on the number of users in each municipality and provider market share is calculated as the share on total health expenditure in each municipality. Column 1 has no controls, column 2 includes patient demographics and column 3 adds long-term disease fixed effects. Authors' calculations based on the "Base de Suficiencia" of the Ministry of Health and Social Protection.
$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$