STARK CONTRASTS:

THE IMPACT OF PROHIBITING PHYSICIAN SELF-REFERRALS ON THE PREVALENCE OF OVERTREATMENT IN HEALTH CARE*

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Abstract

Do physicians invest in medical service facilities to profit from the overtreatment of patients? Current literature only shows that physicians who make referrals to facilities in which they have a financial interest order more services than physicians without such a financial interest. These studies, however, do not establish a causal link between self-referrals and overtreatment. Using medical claims data from Taiwan, we examine the impact of a policy designed to remove physicians' financial incentives to overprescribe drugs by prohibiting clinic-pharmacy integration, and find that (1) self-referrals indeed cause physicians to overtreat patients, and (2) vertical integration also facilitates overtreatment. These findings have important implications for federal Stark Legislation (42 U.S.C.S. §1395nn), which prohibits physicians' referral of Medicare/Medicaid patients to an entity in which they have a "financial relationship" for certain designated health services. In particular, the third finding calls into question Stark Law's implicit assumption that vertically integrated medical providers are unlikely to overtreat patients, as exemplified by the "bona fide employee" and "in house ancillary services" safe harbor exceptions.

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I. Introduction

This paper examines the impact of prohibiting physician ownership of medical service facilities on the prevalence of overtreatment in health care. Theoretically, it is straightforward to understand how self-referrals – defined as referring patients to an entity in which the physician has a financial interest – may lead to overtreatment in a fee-for-service system. On the demand side, patients may have little incentive to reject treatment when insurance covers most of the cost. On the supply side, the physician stands to gain financially when paid on a cost-plus basis by providing more services than necessary (McGuire 2000). However, the empirical literature has not satisfactorily demonstrated the existence of overtreatment in the context of self-referrals. Most existing literature compares the prescribing behavior of physician-owners of medical facilities to that of non-owners, and finds that owners prescribe more services than non-owners. These cross-sectional comparisons, however, cannot distinguish between overutilization by physician-owners and underutilization by non-owners. These studies do not establish a causal link between self-referrals and overtreatment. Furthermore, no study has empirically examined the impact of anti-self-referral legislation on physician's propensity to overtreat patients.

To combat the perceived abuses of self-referrals, especially in physician-owned imaging facilities, Congress passed the Stark Law[1] in 1989. The initial legislation prohibited physician referrals of Medicare and Medicaid patients to clinical laboratories in which the physicians have a financial interest. Phase II (1993) of the Stark Law extended the prohibition to additional health services. Phase III (2007) added an attribution rule to disallow physician employees of group practices from making referrals when the owners of such practices are prohibited from

[1] 42 U.S.C.S. §1395nn in §1877 of the Social Security Act. Additional regulations can be found at 42 C.F.R. §411.350 through §411.389

doing so. However, Phase III keeps both the “bona fide employee” and “in office ancillary services” exceptions, which allow physician self-referrals (1) to an employee of a vertically integrated firm, and (2) for services performed within the physician’s office. Thus, while the new rules attempt to close loopholes in the federal statute, Stark Law still draws a sharp distinction between permissible referrals made within a vertically integrated firm, and impermissible referrals made to outside facilities.

Despite the existence of Stark Law, however, Medicare spending for imaging services doubled from $7 billion to $14 billion annually between 2000 and 2006 – a 13% per annum increase. Moreover, two-thirds of such spending occurred in physician office settings in 2006, up from 60% in 2000[2]. Nevertheless, physician groups continue to oppose legislations such as the Stark Law. In addition to arguments favoring the freedom to contract and the need to provide superior care to their patients, physicians point to deficiencies in the existing empirical literature on self-referrals. They question whether self-referrals *cause* overtreatment, and whether legislations such as the Stark Law are well crafted to address this concern. In this paper, we draw from a physician utility maximization model (McGuire and Pauly 1991) to examine two questions: (1) Do physician-owners who refer patients to their pharmacies overprescribe drugs? (2) If so, does this overtreatment occur even when physicians refer patients in-house to a bona fide employee-pharmacist?

To answer these questions, we exploit a quasi-experiment that occurred in Taiwan between 1997 and 2000. In part because of growing health care, and in particular prescription drug expenditures[3], Taiwan instituted a policy designed to remove power financial incentives for

[2] Medicare: Trends in Fees, Utilization, Expenditures for Imaging Services before and after Implementation of the Deficit Reduction Act of 2005, GAO-08-1102R, September 26, 2008.
[3] In 2007, total health expenditures represented 6.3% of Taiwan’s GDP, of which 21% consisted of prescription drug expenditures.

physicians to overprescribe drugs. This policy, known as the "separating policy," was implemented on a geographical basis over time, and prohibited physicians from dispensing drugs from their pharmacies unless they hired an onsite pharmacist. This quasi-experiment makes it possible to identify the impact of the anti-self-referral policy on several measures of prescription volume.

Using patient-level microdata, a difference-in-differences methodology, and controlling for a wide variety of fixed effects (including physician, quarter, disease, and location fixed effects), we confirm that physicians who refer patients to their own pharmacies overprescribe drugs. Relative to a control group, physicians at clinics that never hired a pharmacist showed statistically significant reductions in various measures of prescription volume after the policy – they reduced drug expenditures by 7.8%, likelihood of prescription by 2.3%, number of drugs by .13 type, and prescription duration by .16 day. At the same time, these physicians increased diagnostic test expenditures by 11.5%. On the other hand, physicians at clinics that always had a pharmacist showed no change in prescribing behavior during the same period when compared to the control group. The results survive several notable threats to identification, including patient self-selection, endogeneity in the choice not to hire a pharmacist, and the appropriateness of the control as the counterfactual.

Using a similar methodology, we find strong evidence that physicians vertically integrate in order to profit from the overtreatment of patients. We first show that physicians were highly rational in their choice to integrate – Generally, only clinics with sufficient patient volume to offset some of the cost of hiring a pharmacist eventually did so. Clinics with an intermediate level of patient volume prior to the policy change were most likely to hire a pharmacist subsequently. Clinics with a high patient volume generally already had an onsite pharmacist.

Clinics with a sufficiently low past practice volume generally did not hire a pharmacist even after the policy effective date. The clinics that did not have a pharmacist before the policy but hired one thereafter, which we call "switchers," showed (1) a marked drop in prescription volume after the policy became effective but before they hired a pharmacist, and (2) a rebound in these same measures after they employed an onsite pharmacist. In fact, if we drop all interim observations – the post-policy, but pre-pharmacist office visits – switchers demonstrated almost no statistically significant change in prescribing behavior. These results strongly support the hypothesis that vertical integration facilitates overtreatment even when physicians refer patients in house to a "bona fide employee."

Our work contributes to the literature in several ways. First, it establishes a causal role of self-referrals in the overtreatment of patients. Virtually all of the empirical work available is suggestive of the overutilization hypothesis, but none shows causality because of their cross-sectional research design. Childs and Hunter (1972), for example, show that patients who receive care at a physician's office with onsite radiological services are more than twice as likely to receive imaging services as, and receive 65% more services than patients at an office without onsite radiological services. Strasser et al (1987) demonstrate that the mere presence of onsite radiological services is correlated with a 2.4 greater likelihood of physicians' prescription of imaging services than centers without onsite radiological facilities. The studies that may have provided the greatest impetus for the passage of Stark Law (Hillman, et al. 1990; 1995; 1992), as well as one by the United States General Accountability Office (Aronovitz 1994), all lend further support to the previous empirical findings. These results have subsequently been confirmed in the context of physician-owned physical therapy offices (Mitchell and Sass 1995), specialty hospitals (Mitchell 2005), and more recent examinations of the imaging services sector (Gazelle, et al. 2007; Mitchell 2008). Our work goes beyond cross-sectional comparisons of utilization, and establishes a causal relationship between self-referrals and overtreatment.

Secondly, no existing studies examine the effect on self-referrals of allowing physicians to vertically integrate to escape regulatory scrutiny. Stark Law safe harbor exceptions (as well as antitrust policy) favor vertical integration, permitting single firms to engage in activities that are prohibited when two or more parties behave similarly in concert. While it is possible that the government must necessarily draw a bright-line rule in crafting legislation, our study shows that self-referrals in house to a "bona fide employee" also result in overtreatment.

Our work informs policy in very specific ways. First, policymakers are correct to be concerned about physicians referring patients to an entity in which they have a financial interest because of the potential for overtreatment. However, prohibiting existing vertical relationship may lead physicians to increase the quantity of a related service. Finally, the bright-line "bona fide employee" or "in office ancillary services" exception may be misguided when the referring physicians derive profits from the referral of patients to the employee.

The remainder of this paper is set up as follows: In Section II, we outline the major features of the health care system in Taiwan and the details of the separating policy; in Section III, we draw upon a physician utility maximization model to provide the theoretical framework to generate testable hypotheses for our empirical investigation; in Section IV, we describe the data and methodology that we use to answer our research questions; in Section V, we discuss the results and findings of our investigation; and in Section VI, we conclude and highlight open questions for future research.

II. Institutional Context

Taiwan's medical providers, like those in many other East Asian nations, dispense the drugs that they prescribe to their patients. Hospitals and clinics in Taiwan are reimbursed for drugs at a standard formulary price, but they procure the drugs from manufacturers at a negotiated – and often much lower – price. As a result, medical providers in Taiwan stand to reap handsome profits from the sale of drugs, creating powerful financial incentives for physicians to overprescribe medication. Anecdotal evidence suggests that this price differential could make up almost 50% of the income of a large hospital.

To remove the financial motivation to overprescribe drugs, Taiwan implemented a policy in 1997 to separate physicians' diagnosing decisions from the incentive to prescribe drugs. As originally conceived, the policy aimed to prohibit the sale of drugs at all physician-owned pharmacies. However, the policy met with strong resistance from physician groups, and a compromise was reached to allow clinics with an onsite pharmacist to continue dispensing drugs from their pharmacies. Clinics without an onsite pharmacist must "release" their prescriptions to be filled at an outside pharmacy, unless one of numerous age- or illness-based waivers applied[4]. The policy was rolled out on a geographical basis over the course of four years, beginning with Taipei and Kaohsiung on March 1, 1997. Jurisdictions became subject to the policy as soon as there was a sufficient number of independent pharmacies in the relevant jurisdictions. See Table I for a list of the implementation dates for counties, cities, and townships from 1997 to 2000.

Under Taiwan's National Health Insurance (NHI), physicians are paid on a fee-for-service basis according to a standardized schedule. Payments made by the NHI represent the bulk of the

[4] The exceptions included: patients under the age of 3 or over 65; and patients diagnosed with severe diarrhea; vomiting, dehydration; headache, backache, joint pain or toothache; vomiting or urinating blood; severe external bleeding; poisoning; severe allergic reaction; highly fluctuating body temperature; breaching difficulty; delirium or fainting; severe eye, ear, breathing, digestive or urinary obstructions; serious psychological problems threatening the safety of the patient and/or others; post-traumatic stress disorder.

medical providers' income, followed by user fees and copayments borne by the patients. In the context of clinics, the sources of revenue are well-defined. In addition to the user fees and copayments, physicians at private clinics earn a per-visit consultation fee, fees for diagnostic tests, a per-prescription fee for the service of dispensing drugs, and proceeds from the sale of drugs.

For clinics without an onsite pharmacist, two sources of revenues were removed: proceeds from the sale of the drugs, as well as the drug dispensing service fee (approximately $.31 USD per prescription)[5]. To partially offset the loss from these two revenue streams, the Bureau of National Health Insurance increased the consultation fee for clinics without an onsite pharmacist by $25 New Taiwan Dollars (NTD) ($.78 USD). To encourage outside pharmacies to dispense drugs, the drug dispensing service fee originally paid to the physicians was increased from $10 NTD ($.31 USD) to $20 NTD ($.63) per prescription, and transferred to the outside pharmacy that fills the prescription.

Clinics without an onsite pharmacist also had to forgo substantial profits from drug sales because they could no longer apply for reimbursements through a system of simplified claims. Under this system, clinics with drug-dispensing privileges may be reimbursed at a fixed $100 NTD ($3.13 USD) rate if total drug costs are $100 NTD ($3.13 USD) or less, and if the prescription is for no longer than three days. Thus, a one-day, $20 NTD ($.63 USD) prescription would automatically be reimbursed at $100 NTD ($3.13 USD), representing a profit of $80 NTD ($2.50 USD) in addition to any profits from the price differential between the $20 NTD ($.63 USD) formulary price and the actual acquisition cost of the drug.

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[5] Taiwan's per capita GDP was $13,130 USD at the official exchange rate in 1997. The exchange rate was approximately $1 USD to $32 NTD.

III. Theory and Hypotheses

In the Taiwanese context, prohibiting physician self-referrals eliminated physicians' financial income from the sale of drugs. Because Taiwanese physicians may also order diagnostic tests for income, the separating policy can be analyzed according to a physician utility maximization model in which physicians choose between (1) prescribing drugs, for which the fee has been removed, and (2) ordering diagnostic tests, for which the fee has not been changed. A rich body of research in physician-induced demand relies on a physician utility maximization model that permits physicians to "trade" units of financial income against units of disutility caused by overtreating patients. (Evans 1974; Fuchs 1978; Gruber and Owings 1996; McGuire and Pauly 1991). McGuire (2000) summarizes the key features of such a model, which we reproduce in the following section.

III.A. The McGuire-Pauly Physician Utility Maximization Model

The model, modified along the lines of Gruber and Owings (1996), can be represented by the following maximization problem:

$$(1) \qquad Max U = U(Y, I)$$
$$where\ Y = N\big(m_1 x_1(i_1) + m_2 x_2(i_2)\big)$$
$$I = i_1 + i_2$$

The McGuire-Pauly model assumes that the physician's utility is increasing in income Y ($U_Y > 0$), and decreasing in overtreatment disutility I ($U_I < 0$). The physician's total income Y is a function of (1) N, the number of patients she sees; (2) m_1 and m_2, the profit margins of the services that she provides for services 1 and 2 respectively; (3) x_1 and $x2$, the quantities of the respective services provided, and (4) i_1 and i_2, the level of overtreatment for these services. Note that the quantities x_1 and x_2 are both functions of, and increasing in i_1 and i_2 – Intuitively, the higher the overtreatment, the more quantities of services provided. Solving the utility maximization problem with respect to i_1 and i_2 yields the follwoing first order conditions:

(2) $$m_1 x_1' = m_2 x_2' = -\frac{U_I}{U_Y}$$

The intuition behind these first order conditions is that the marginal increase in income due to overtreatment in both services must be equal to the marginal increase in "disutility cost" from the overtreatment. According to this model, when the fee for service 1 is reduced, but the fee for service 2 remains unchanged, there are two countervailing forces on the direction of change for the quantity provided for service 1. On the one hand, the falling fee would tend to increase supply because of the income effect. On the other hand, there is a substitution effect that would cause the physician to switch from the less profitable service 1 to the more profitable service 2. In other words, a reduction in m_1 has an ambiguous effect on the sign of change for the quantity of service 1. The same fee reduction in service 1, however, unambiguously predicts that the quantity of service 2 would increase. In this particular context, we expect drug prescriptions (service 1) to fall because no income effect could dominate the substitution effect when profits are completely eliminated. These theoretical predictions underpin our identification strategy for the existence of overtreatment from physician self-referrals. We take an observed increase in service 2 (diagnostic tests) with a simultaneous fall in service 1(prescription drug volume) to be dispositive of overtreatment.

III.B. Hypothesis Generation

III.B.1. Existence and Extent of Overtreatment with Physician Self-Referrals

We exploit the prohibition against self-referrals as a quasi-experiment to identify the existence and extent of overtreatment when physicians refer patients to their own pharmacies. In particular, doctors in Taiwan have only two sources of discretionary income per office visit – the sale of prescription drugs (service 1), and the ordering of diagnostic tests (service 2). We identify the existence of overtreatment by showing that (1) physicians who no longer profit from the sale of prescription drugs increase diagnostic tests and reduce drug prescriptions, and that (2) physicians who continue to sell drugs because they have an onsite pharmacist do not show any change in either drug-prescribing or test-ordering behavior. We set forth these propositions in hypotheses 1 to 4, as follows:

Hypothesis 1: Physicians at clinics that do not have an onsite pharmacist before and after the policy will reduce drug prescribing behavior (relative to a control group) after the implementation of the separating policy.

Hypothesis 2: Physicians at clinics without an onsite pharmacist before and after the policy will increase diagnostic tests (relative to a control group) following the implementation of the separating policy.

Hypothesis 3: Physicians at clinics that always had an onsite pharmacist will not alter their drug prescribing behavior (relative to a control group) after the implementation of the separating policy.

Hypothesis 4: Physicians at clinics that always had an onsite pharmacist will not alter their diagnostic test ordering behavior (relative to a control group) after the implementation of the separating policy.

In the next section, we investigate whether physicians hire a pharmacist in order to continue overtreating patients in outpatient prescription drugs.

III.B.2. Vertical Integration and Overtreatment

The behavior of physicians at clinics that did not have an onsite pharmacist before the separating policy, but eventually hired one after the policy effective ("switchers"), is especially relevant to whether physicians "vertically integrate" in order to profit from overtreating patients.

We identify the relationship between vertical integration and overtreatment by observing the prescribing behavior of switchers (1) before the policy, (2) after the policy but before hiring a pharmacist, and (3) after the policy and after hiring a pharmacist. If vertical integration facilitates overtreatment, we should observe switchers reduce prescriptions after the policy, and again increase prescribing volume after they hire a pharmacist.

Before doing so, we first examine whether physicians are generally rational in their choice to "vertically integrate." Because hiring a pharmacist involves an additional fixed cost to the physician, we expect that only clinics with sufficient patient volume (and therefore income) would hire a pharmacist:

Hypothesis 5: Physicians at clinics without a pharmacist before the separating policy, but with a sufficient level of patient volume, will hire a pharmacist.

The following two hypotheses test whether vertical integration facilitates overtreatment.

Hypothesis 6: Physicians at clinics without a pharmacist before the separating policy, but subsequently hire a pharmacist, will reduce their prescribing behavior after the separating policy but before hiring the pharmacist.

Hypothesis 7: Physicians at clinics without a pharmacist before the separating policy, but subsequently hire a pharmacist, will increase their prescribing behavior after they hire the pharmacist.

In the next section, we describe the data and methodology that we use to test the seven hypotheses set forth above.

IV. Data and Methodology

IV.A. Data

IV.A.1. Description of the Data Sets

The two collections of data that are relevant to our analysis are provided by Taiwan's National Health Research Institute, which maintains an uncensored population of all claims filed with the National Health Insurance Bureau. The first collection consists of random subsamples of two relevant databases from the population data from 1997 to 2004: (1) a random[6] .2% subsample of all outpatient visit claims and (2) the details of all outpatient services ordered during the office visit, including drug names, quantities, and prices. Henceforth we will refer to this collection of data files as the "random subsample" files. The second data set, which we use only to perform a robustness check, consists of the entire medical claims history of 200,000 randomly selected individuals between 1997 and 2004. We refer to this second data set as the "panel data." In constructing the data sets, we retain only data from clinics, because all hospitals have pharmacies staffed with onsite pharmacists, and are therefore exempt from the separating policy.

IV.A.2. Variables and Summary Statistics

Dependent Variables. The dependent variables consist of various measures of prescribing volume. PRESCRIPTION is an indicator variable that is set to 1 if one or more drugs are prescribed. DRUG DAY indicates the prescription duration in days, by law capped at 28 days

[6] The National Health Research Institute has included the following language to describe the method for extracting random subsamples from the population: "We use the systematic sampling method to randomly sample a representative database from the entire database. The size of the subset from each month is determined by the ratio of the amount of data in each month to that of the entire year. Then the systematic sampling is performed for each month to randomly choose a representative subset. This sampling database is obtained by combining the subsets from 12 months. The sampling database of CD/DD [Outpatient/Inpatient] was constructed at first then the relative observations in OO/DO [Details of outpatient/inpatient orders] were drawn out accordingly. The sampling database of DD [Inpatient] and CD [Outpatient] was 5% and 0.2% to the entire database respectively."

(with a few exceptions). If more than one drug is prescribed, the prescription duration of the drug prescribed for the longest period is given. LOGGED DRUG EXPENDITURES is the natural log of the drug expenditures generated at the visit level. Because the details of prescriptions are given in the claims form even when they are filled outside, we can reconstruct drug expenditures by multiplying each drug quantity by its formulary price and summing all the subtotals thus calculated. ADJUSTED DRUG EXPENDITURES must be calculated because of the “simplified claims.” The listed $100 NTD ($3.13 USD) reimbursements for such claims do not represent the true formulary costs of the drugs. We calculate the actual drug expenditures in the same manner as for prescriptions filled outside the clinic, and call this variable “adjusted drug expenditures.” NONDRUG ORDERS is an indicator variable that is set to 1 if one or more diagnostic tests are ordered. LOGGED NONDRUG EXPENDITURES represent the natural log of the total nondrug (diagnostic) expenditures ordered during the visit.

Independent Variables. The independent variables include the following: EFFECTIVE is an indicator variable that is equal to the interaction term TREAT × POST. It is set to 1 if the separating policy is in effect at the time of the office visit. AGE represents the patient’s age in years, and FEMALE is a dummy variable that is 1 if the patient is female. COUNTY/CITY dummy variables – each city and county in Taiwan receives a unique dummy variable, and the dummy for the city of Hsinchu is randomly dropped to avoid perfect collinearity. QUARTER dummy variables – Each quarter beginning in quarter 1, 1997 to quarter 4, 2000 has a dummy variable that equals 1 if the office visit falls in that particular quarter. The dummy variable for quarter 1, 1997 is dropped in all econometric specifications. DISEASE CODE dummy variables – Of 976,420 office visits, only 7,616 observations lack a primary diagnosis code, and these observations are dropped. Only 15.2% of the observations have a secondary diagnosis code, and a mere 3.96% also have a tertiary diagnosis code. We therefore construct dummy variables for each of the top 65 disease codes for the primary diagnosis, and discard the rest. See Table II for the most common primary diagnosis codes in the data.

Summary Statistics. In Table III, we include the summary statistics for the outcome and independent variables, excluding dummy variables for fixed effects, such as disease code, quarter, and jurisdiction dummies. The entire sample includes 976,420 office visits to clinics in Taiwan from 1997 to 2000. The average revenue to the physician (total medical expenditures) per visit during this period was $390 NTD ($12.19 USD), of which $105 NTD ($3.28 USD), or 26.9%, consisted of drug charges, $201NTD ($6.28 USD) (51.5%) represented the average physician fee, $68 NTD ($2.13 USD) were for nondrug expenditures, and the remaining $16 ($.50 USD) were for the service of dispensing drugs. Approximately 97% of all office visits resulted in a prescription for an average duration of 3.29 days, and of these, 94% of prescriptions were dispensed at the clinic's pharmacy. The average prescription comprised of 3.99 different types of drugs. There were 8,289 clinics in the data, and 3,070 of these always had a pharmacist onsite. 2,431 clinics never had a pharmacist onsite during the entire study period. The remaining 674 clinics did not have a pharmacist before the separating policy but hired one thereafter, on average within six months (176 days) after the implementation date of the separating policy. Of all the clinics, 1,204 clinics are located in areas not subject to the policy from 1997-2000 and serve as the control. The remaining 910 clinics are those whose first appearance in the dataset comes after the effective date of the separating policy.

IV.B. Methodology

IV.B.1. Existence and Extent of Overtreatment with Physician Self-Referrals

We use a difference-in-differences model with physician fixed effects to capture the changes in the outcome variables by comparing the physicians' prescribing behavior before and after the separating policy, subtracting changes in the contemporaneous behavior of physicians in the control group. The data set is divided into three subsets: the set of observations including clinics that always had an onsite pharmacist; the set with clinics that hired a pharmacist sometime after the separating policy; and the set of clinics that never had a pharmacist in the study period. All three subsets include observations from clinics located in areas not yet subject to the policy ("control clinics"). We run seven separate fixed effects regressions using each of the subsets for the seven dependent variables pertaining to prescribing behavior. All dependent variables in dollar amounts are logged to estimate percentage changes.

The econometric model for these regressions is described below. Here, y_{it} represents one of the seven outcome variables; θ_i is the physician fixed effects; μ_i, the vector of city/county dummies; π_{it}, the vector of quarter dummies; ρ_{it}, the vector of disease code dummies; and u_{it} is the error term.

(3) $$y_{it} = \alpha_i + \beta \cdot effective_{it} + \gamma \cdot patient\ age_{it} + \delta \cdot patient\ sex_{it} + \theta_i + \mu_i + \pi_{it} + \rho_{it} + u_{it}$$

The primary coefficient of interest is β. The coefficient is meant to reflect changes in the percentage of visits resulting in a (1) prescription or (2) laboratory/diagnostic test, (3) changes in days for the prescription length, (4) changes in the number of different types of drugs prescribed, and percentage changes in the (5) drug, (6) adjusted drug, and (7) nondrug/diagnostic expenditures. The inclusion of numerous fixed effects will control for unobserved heterogeneity among physicians, quarters of the years, county and city characteristics, and differences in treatment plans for different diseases. To account for potentially different treatments based on patient gender and age, we also include these two variables as covariates for all of the regressions in this econometric model.

IV.B.2. Vertical Integration and Overtreatment

We test whether past practice volume predicts the likelihood of hiring a pharmacist graphically by running a lowess-smoothed regression of integration status three months after the policy on total practice volume from the previous year. To verify whether prescription volume falls for switchers after the policy takes effect (but before they hire a pharmacist), we run model (3) as described in Section IV.B.1, except that we drop all observations after the switchers hire a pharmacist. Finally, to confirm whether switchers increase prescribing volume after they hire a pharmacist, we drop all pre-policy observations for the switchers, and employ a fixed effects model that is identical in all respects to model (3) but one – instead of having "effective" as the treatment variable, we use "pharmacist," which is equal to 1 on and after the date an onsite pharmacist is hired at a particular clinic. Specifically, we have:

(4) $$y_{it} = \alpha_i + \beta \cdot pharmacist_{it} + \gamma \cdot patient\ age_{it} + \delta \cdot patient\ sex_{it} + \theta_i + \mu_i + \pi_{it} + \rho_{it} + u_{it}$$

The coefficient β on PHARMACIST reflects the impact of vertical integration on physician prescribing behavior. Of course, the PHARMACIST variable is a highly endogenous choice in this econometric model. Nevertheless, the coefficient estimates on the variable are still relevant for policy analysis: they represent the direction and magnitude of changes in prescribing behavior of physicians who vertically integrate, perhaps in order to capture the rent from overtreating patients.

Finally, as a robustness check, we drop all observations from the interim period (that is, after the policy becomes effective but before the switcher clinics hire a pharmacist) and again run model (3). The coefficients on EFFECTIVE reflect ultimate changes in the behavior of switcher clinics between (a) the period before the policy implementation date, and (b) the period after the clinics hire an onsite pharmacist. Unless there is a fall in prescription behavior after hiring a pharmacist relative to the pre-policy period, we conclude that physicians vertically integrate to overprescribe drugs.

V. Results and Discussion

V.A. Extent and Existence of Overtreatment with Physician Self-Referrals

The results of the regressions are strongly supportive of overtreatment from physician self-referrals. After the separating policy, physicians at clinics that never hired an onsite pharmacist ("never clinics") reduced all measures of prescription volume. Adjusting for the log-level functional forms of all dependent variables denominated in dollar amounts, these physicians reduced drug expenditures by 24.4%[7], likelihood of prescription by 2.3%, number of drugs by .13 type, and prescription duration by .16 day (Table IV). In Table V, we show that even the adjusted drug expenditures fell by 7.8%. This result shows that even accounting for the financial padding in the simplified claim reimbursements in the pre-policy period, there was a

[7] $100 \cdot [exp(-0.28) - 1] = 24.4$

real reduction in drug expenditures after the separating policy. Finally, physicians at "never clinics" increased nondrug (laboratory) expenditures by 11.5%[8].

At the same time, physicians at "always" clinics (clinics which employed a pharmacist before the policy change and continued to have an onsite pharmacist post-policy) made no statistically significant changes in their prescribing behavior: the coefficients on the "effective" dummy variable are imprecisely measured 0s in all of the models (Table VI).

The results for the "never" clinics, together with those of the "always" clinics strongly suggest the existence of overtreatment when physicians make self-referrals: First, the fall in prescription volume when drug sales are no longer profitable is in itself highly suggestive of overtreatment. Second, the contemporaneous increase in nondrug expenditures at "never" clinics following the policy change also suggests overtreatment. Significantly, we find no change in the drug-prescribing and test-ordering behavior of "always clinics," which were *de facto* exempt from the policy. Taken together, these fact patterns strongly suggest a causal role of self-referrals in the overtreatment of patients.

V.A.1. Robustness Checks

While fixed effects regressions greatly reduce the omitted variable bias common in cross-sectional studies, at least three sources of bias remain – patient self-selection, endogeneity in the choice not to hire a pharmacist, and inappropriate controls in the difference-in-differences specifications.

Patient Self-Selection. In our econometric model, we control for physician, quarter, county/city, and disease code fixed effects, in addition to patient age and gender. However, we do not account for changes in patient composition. If patients with a greater demand for

[8] $100 \cdot [exp(0.109) - 1] = 11.5$

prescription drugs switch from clinics with no onsite pharmacists to vertically integrated clinics, there would be a spurious correlation between a fall in prescribing behavior at "never" clinics and the separating policy. We address this concern first by noting that the separating policy does not impose any additional cost on physicians who wish to prescribe drugs to satisfy patient demand. The policy merely removes the financial incentive on the supply side to overtreat. Thus, the only difference to the patients who seek medical care at clinics with no onsite pharmacists is that they must, post-policy, fill their prescription at an outside pharmacy rather than at the clinic itself. Generally, however, these outside pharmacies tend to be in close proximity to the clinics.

Using the panel data set, we also examine actual patient behavior before and after the separating policy becomes effective in their area. Of the 200,000 randomly selected individuals for whom we possess their entire medical history from 1997 to 2004, 155,343 individuals sought care at a clinic at least once between January 1, 1997 and December 31, 2000. Of these, very few patients actually switched from consistently patronizing "never clinics" before the policy to visiting "always clinics" after the change: Only 347 patients visited a never clinic at least 3 times *before the policy change*, representing more than 75% of all pre-policy visits to clinics, *and* visited an always clinic at least 3 times, or more than 75% of all post-policy visits to clinics. The extremely small population of patients who switched from mostly "never clinics" before the policy to mostly "always clinics" after the policy demonstrates that patient self-selection is of limited concern.

Endogenous Choice Not to Hire a Pharmacist. Another potential problem for our specification is that a key independent variable may be endogenous. While the fixed effects model controls for unobservable time-invariant heterogeneity, it remains susceptible to biases created by endogenous variables. Here, we suspect that prescribing behavior (the dependent variable) is simultaneously determined by a factor that also affects the decision not to hire a pharmacist. Perhaps physicians who decide not to hire a pharmacist are exactly those who are least likely to overtreat patients to begin with, so the fall in prescribing volume cannot be generalized to a larger population of physicians. To address this concern, we use past practice volume as an instrument to replace the potentially endogenous choice not to hire a pharmacist. Specifically, we construct a dummy variable based on the inflection point of the lowess-smoothed regression for hypothesis 4. A past practice volume of 15 or lower in the six months prior to the policy change[9] sets the instrumental variable to 1, and replaces the decision not to hire a pharmacist.

We first report in Table VII the results of the instrumental variable regression, then justify our choice of instrument in the subsequent paragraphs. Logged drug expenditures and logged adjusted drug expenditures fell by 22.5% and 12.7% respectively after the separating policy became effective. Furthermore, the likelihood of prescription dropped by 1.6%. These results suggest that the policy would cause a randomly selected physician to reduce prescribing behavior if subject to a prohibition against self-referrals.

We next justify the validity of our chosen instrument. The appropriateness of a given instrument hinges on two requirements: A high correlation between the instrument and the endogenous variable, and an absence of the endogenous component that afflicts the problematic variable in the first place. The instrument must affect the outcome variable only through its effect on the endogenous variable. For the first requirement, we note that the t-statistic obtained from regressing the NEVER dummy on the instrument is 58.64 (with an F-statistic on the

[9] A past practice volume of 15 in a 1-in-500 random subsample implies 750 visits in the six months prior to the policy change.

instrument and constant of 3439.2) (Table VIII). The traditional requirement that the F-statistic be greater than 10 is easily satisfied.

For the second requirement, while we cannot prove conclusively that the instrument is uncorrelated with the endogenous component of the decision not to hire a pharmacist, we offer the following suggestive evidence: If a low lagged practice volume is reflective of an unwillingness to overtreat, physicians with fewer office visits should also have lower measures of prescribing volume before the policy. In Table IX, we see that the pre-policy prescription drug averages for the low-volume clinics are not lower than the rest of the non-always clinics. Physicians at low-volume clinics prescribed an average of $114 NTD ($3.56 USD) of drugs versus $103 NTD ($3.21 USD) for physicians at clinics with a higher lagged practice volume. Furthermore, total medical expenditures for the low-volume clinics averaged $418 NTD ($13.06 USD), again greater than $378 NTD ($11.81 USD) for higher-volume clinics.

To determine the quarter-by-quarter pre-policy differences in prescribing behavior between low-volume physicians and other non-always physicians, we also do the following: We drop all post-policy observations, and regress the dependent variables on all of the covariates described in model (3) in Section IV.B.1 (except the EFFECTIVE variable), as well as the full interaction between the quarter dummies and an indicator for low-volume status. The results in Table X show that there are no consistent differences between these two groups of physicians in their pre-policy prescribing behavior. These results, taken along with the assumption that low-volume physicians could not – rather than chose not to – hire a pharmacist, suggest that the instrument is less likely to be plagued by the potential endogeneity problem associated with a deliberate decision not to hire a pharmacist.

Appropriateness of the Control Group as the Counterfactual. A third potential challenge to our results comes from the suitability of the controls. In our regressions, for both the "always" and "never" clinics, we use observations from clinics located in jurisdictions not subject to the policy as the control group. We address the concern that the control group may not represent a suitable counterfactual by investigating whether clinics in the treatment and control groups have similar pre-period trends. Similar pre-policy trends would give greater support to the claim that the treatment group's divergence from the trend is a result of the policy change. Econometrically, we accomplish this task by first removing all post-policy observations for clinics that are eventually subject to the separating policy. We then regress the dependent variables on the same covariates in model (3) (except the EFFECTIVE variable), and the full interaction between the quarter dummies and a new dummy variable with the value 1 if the clinic eventually becomes subject to the policy. As we see in Table XI, the essentially zero coefficients on the interaction terms demonstrate that no systematic pre-period differences exist between the behavior of physicians in the treatment and control groups.

V.B. Vertical Integration and Overtreatment

Our regression results corroborate that physicians rationally choose to vertically integrate with pharmacists to capture the rent from overtreatment. First, past practice volume predicts whether clinics eventually hire a pharmacist after the separating policy takes effect. Figure I shows that the likelihood of integration rises dramatically when the practice volume in the year three months before and after the policy effective date reaches 75 (or approximately 37,500 visits in a random 1 in 500 subsample). Results are substantially similar if we consider the practice volume for the year beginning six, nine, and twelve months before the separating policy.

The pattern of behavior at "switcher" clinics suggests that physicians hire a pharmacist in order to profit from overtreating patients. Following the policy effective date but before hiring a pharmacist, physicians at switcher clinics reduced all measures of prescribing behavior: they decreased drug expenditures by 26.5%, adjusted drug expenditures by 12.3%, likelihood of prescription by a modest 1.6%, the number of different types of drugs by .14, and prescription duration by .168 day (Table XII).

When physicians then hire a pharmacist, almost all measures of prescribing behavior recovered: Drug expenditures increased by 11%, the likelihood of prescription rose by .4%, and other measures, such as number of drugs and prescription duration, also rebounded, by .11 type of drugs and .93 day respectively (Table XIII). However, adjusted drug expenditures yielded an imprecisely measured reduction.

We next look at the ultimate impact of the separating policy on the switchers' prescribing behavior by dropping all interim observations from the regressions. By interim, we mean the post-policy, but pre-pharmacist observations associated with the switchers. The results of these regressions are presented in Table XIV. When all interim observations are removed, the separating policy had no measurable effect on the switchers' prescribing behavior except two: relative to the pre-policy period, there is a 6.7% reduction in adjusted drug expenditures and a .069 day shortening of prescription duration after the policy became effective. This result contrasts with the fact that always clinics did not change any prescribing behavior post policy (Table V).

A possible explanation for these reductions may be the combined effect of the simplified claim method of reimbursement and the separating policy. As noted previously, switcher clinics are intermediate-volume clinics. It is possible that, although they were financially able to hire a pharmacist after the separating policy was implemented, the added cost of the pharmacist's salary caused physicians to seek additional revenue sources. Under the simplified claims system, a prescription under $100 NTD ($3.13 USD), and for no more than three days, is automatically reimbursed at the standard $100 NTD ($3.13 USD) sum, regardless of the actual formulary cost of the drugs prescribed. Thus, it is possible that switchers shortened the prescription length to pocket a greater share of the $100 NTD ($3.13 USD) fixed reimbursement.

Taken as a whole, these results support the claim that vertical integration facilitates the overtreatment of patients – the absence of integration caused a drop in prescribing behavior, and "reintegration" led to a rebound in almost all measures of prescription volume. Our results show that Stark Law's exclusive concern with physician self-referrals to outside entities, while creating bright-line safe harbor exceptions for "bona fide employees" and "in office ancillary services," may be misguided when self-referrals in house also generate profits to the physicians.

VI. Conclusion and Direction for Future Research

In health care, the combination of numerous factors creates incentives for physicians to overtreat patients. The theoretical literature is clear that patient-physician informational asymmetries, generous insurance coverage, together with a fee-for-service compensation system may lead to an overprovision of services when physicians refer patients to an entity in which they have a financial interest. The empirical literature, however, has thus far failed to establish a causal relationship between self-referrals and overtreatment. Most existing studies rely on cross-sectional comparisons of treatment intensities and volumes between physician-owners of medical facilities, and non-owners. Our study goes further than the current literature by demonstrating a causal link between self-referrals and overtreatment, and empirically examines the impact of prohibiting physician self-referrals.

A policy implemented in Taiwan beginning in 1997, which prohibited physicians from dispensing drugs from their pharmacies (unless they hired an onsite pharmacist), provided a natural experiment to verify whether self-referrals *cause* overtreatment. We identify causality by employing a difference-in-differences model with physician fixed effects that examines (1) the pre-policy/post-policy behavioral change of physicians who were prohibited from making self referrals, and (2) the pre-policy/post-policy change of physicians who continued to refer patients to their own pharmacies. Relative to a control group, physicians who no longer made self-referrals reduced drug prescription along various measures, and simultaneously increased nondrug expenditures after the policy took effect. Relative to the control group, physicians who continued to self-refer patients changed neither their drug-prescribing nor test-ordering behavior. These empirical findings provide the strongest proof to date that physicians who make self-referrals overtreat patients. Furthermore, these results are robust to various threats to identification.

We also show that overtreatment exists when physicians vertically integrate in order to circumvent application of the Taiwanese policy. Clinics initially without an onsite pharmacist reduced prescriptions following the separating policy effective date. Upon hiring an onsite pharmacist, however, the measures of prescription volume generally returned to pre-policy levels. Moreover, the decision to integrate appeared to be highly rational and based on the clinics' practice volume – on average, low-volume clinics did not hire a pharmacist, intermediate-volume clinics hired one sometime after the policy implementation date, and high-volume clinics already had an onsite pharmacist predating the policy. This result suggests that only clinics with sufficient patient volume to profit from overtreating patients expend the cost of hiring a pharmacist.

Our empirical findings contribute to the literature in several important ways. First, our work overcomes the omitted variable bias inherent in cross-sectional studies on physician referral patterns to physician-owned imaging facilities dating from the 1990s, and confirms the underlying motivation for anti-self-referral legislations such as the Stark Law. However, Stark Law's bright-line safe harbor rules permitting physician self-referrals to a "bona fide employee" or for "in office ancillary services" create loopholes for overtreatment to exist. The litmus test should be whether physicians generate additional profits that are based on their referrals, rather than *where* they refer their patients.

Moreover, a comprehensive review of substitute or complementary services should be performed when policymakers adjust or eliminate fees for one among many medical services that can be offered. Reducing or eliminating the income for one type of service may have a direct impact on the volume of another service provided. Finally, it is possible that regulations can interact in unpredictable ways, leading to unintentional consequences. In the specific Taiwanese context, the interaction of the separating policy and the system of simplified claims may have caused switchers to reduce prescription duration without lowering prescription drug expenditures. Indeed, the policy, far from reducing drug expenditures at switcher clinics, may merely have transferred some of the rent from overtreatment to the pharmacists.

We close by proposing several directions for future research. We confirmed that an anti-self-referral policy caused a modest reduction in the overprovision of a relatively noninvasive and highly demanded treatment in Taiwan. However, does patient demand play any part in policy design? For example, can we expect more pronounced results for policies prohibiting self-referrals for treatments that are profitable to physicians but not in particular demand by patients? (Leavitt 2005). Can we rely on patients to reject unnecessary services for higher cost or more invasive segments of medical care, thus reducing the need for regulation in such segments? These questions are significant as physicians expand investments in other sectors of health care, such as physician-owned specialty hospitals.

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List of Tables

Table I: Separating Policy Implementation Dates

Implementation Date	Areas Subject to Policy
March 1, 1997	Taipei City, Kaoshiung City
March 10, 1998	Taichung City, Chiayi City, and 11 townships in Chiayi County
April 20, 1998	Keelung City, 8 townships in Miaoli County, 8 townships in Changhwa County, 5 townships in Hsinchu County, 5 townships in Yunlin County, 11 townships in Kaohsiung
June 6, 1998	County
July 6, 1998	9 townships in Taichung County, Pingtung City and 12 townships in Pintgung County
October 5, 1998	1 township in Chiayi County, 1 township in Yunlin County
November 5, 1998	Tainan City, 2 townships in Nantou County, 1 township in Tainan County
December 12, 1998	6 townships in Taipei County,
January 28, 1999	Hsinchu City, Taoyuan City, 6 townships in Taoyuan County
June 21, 1999	Miaoli City, 1 township in Hsinchu County
September 13, 1999	1 township in Changhwa County
February 20, 2000	1 township in Kaohsiung County
April 10, 2000	4 townships in Kaohsiung County
April 24, 2000	1 township in Pingtung County
Areas Not Subject to Policy as of December 31, 2000	3 townships in Chiayi County, 2 townships in Miaoli County, 4 townships in Changhwa County, 3 townships in Hsinchu County, 2 townships in Yunlin County, 12 townships in Kaohsiung County, 3 townships in Taichung County, 13 townships in Pingtung County, 3 townships in Nantou County, 7 townships in Tainan County, 11 townships in Taipei County, 3 townships in Taoyuan County, 11 townships in Yilan County

Table II: Most Frequent Primary Diagnosis Codes

Rank	Disease Name	Frequency	% of diagnoses	Cumulative %
1	Acute upper respiratory infection	325,069	.3328	.3328
2	Acute bronchitis	85,477	.0875	.4204
3	Conjunctivitis	34,697	.0355	.4559
4	Acute tonsillitis	30,727	.0315	.4873
5	Influenza	28,923	.0296	.5170
6	Carbuncles and furuncles/Cellulitis	22,649	.0232	.5401
7	Chronic pharyngitis/rhinitis/sinusitis	21,288	.0218	.5619
8	Other (epi)dermatological diseases	21,113	.0216	.5836
9	Constipation/Irritable bowel syndrome	19,791	.0203	.6038
10	Acute gastroenteritis/chronic pancreatitis	16,230	.0166	.6204
11	Back pain	15,502	.0159	.6363
12	Chalazion/Eye strain/Refractive error	14,570	.0149	.6512
13	Vulvovaginitis	13,790	.0141	.6653
14	Carpal tunnel syndrome/migraine	11,718	.0120	.6773
15	Abnormal menstruation	11,642	.0119	.6893
16	Hypertension	8,304	.0085	.6978
17	Communicable gastric disease (unclear diagnosis)	8,207	.0084	.7062
18	Acute sinitus	8,146	.0083	.7145
19	Acute pharyngitis	8,099	.0083	.7228
20	Asthma/Chronic bronchitis/Emphysema	7,580	.0078	.7306
21	Osteoporosis	7,211	.0074	.7379
22	Acute corditis/Allergic rhinitis	6,870	.0070	.7450
23	Malaise and fatigue (general symptoms)	6,723	.0069	.7519
24	Joint pain	6,448	.0066	.7585
25	Abdominal pain	6,206	.0064	.7648
26	Acute otitis externa/positional vertigo	6,007	.0062	.7710
27	Sprain	5,255	.0054	.7764
28	Diabetes mellitus(no complication)	5,242	.0054	.7817
29	Cystitis	5,186	.0053	.7870
30	Duodenal/gastric/peptic ulcer	4,981	.0051	.7921
31	Hypertensive heart disease	4,806	.0049	.7971
32	Arthritis/Osteoarthritis	4,568	.0047	.8017
33	Acute laryngitis and tracheitis	4,248	.0043	.8061
34	Tinea pedis/versicolor/Onychomycosis	3,981	.0041	.8102
35	Acute otitis media	3,898	.0040	.8141
36	Pelvic inflammatory disease	3,756	.0038	.8180
37	Cataract	3,743	.0038	.8218
38	Contact dermatitis and other eczema	3,549	.0036	.8255
39	Other acne	3,362	.0034	.8289
40	Supervision of other normal pregnancy	3,181	.0033	.8322
41	Acute laryngitis and tracheitis	3,038	.0031	.8353
42	Other symptoms involving abdomen/pelvis	2,992	.0031	.8383
43	Acute sinitus	2,961	.0030	.8414
44	Hematuria/Irritable bladder	2,795	.0029	.8442
45	Open wound (lower limbs)	2,714	.0028	.8470
46	Menstruation disorder	2,671	.0027	.8497
47	Verruca/Chickenpox/Herpes/Hand foot mouth disease	2,401	.0025	.8522
48	Open wound (upper limbs)	2,311	.0024	.8546
49	Psychosomatic disorder	2,227	.0023	.8568
50	Health checkup/Lab test	1,700	.0017	.8586
51	Oral ulcer	1,626	.0017	.8602
52	Alcoholic liver disease/chronic hepatitis/cirrhosis	1,551	.0016	.8618
53	Other respiratory diseases	1,532	.0016	.8634
54	Gout/Hyperlipoproteinemia/Endocrine diseases	1,440	.0015	.8649
55	Tendinitis	1,406	.0014	.8663
56	Deviated septum	1,389	.0014	.8677
57	Acute gastritis (identified pathogen)	1,378	.0014	.8692
58	Other diseases of skin or subcutaneous tissue	1,366	.0014	.8706
59	Rubella	1,341	.0014	.8719
60	Contact dermatitis and other eczema	1,312	.0013	.8733
61	Acute laryngitis and tracheitis	1,299	.0013	.8746
62	Rheumatoid arthritis	1,197	.0012	.8758
63	Unspecified functional disorder of intestine	1,136	.0012	.8770
64	Open wound (head)	1,111	.0011	.8781
65	Normal child birth	1,091	.0011	.8792

Data: All office visits to clinics in Taiwan except clinics located in jurisdictions with mixed implementation dates, 1997-2000)

Total observations: 976,670

Table III: Summary Statistics

Variable	Obs	Mean	Std. Dev.
Date of visit	976,420	8-Jan-99	417.24
Prescription	976,420	.97	.16
Filled at clinic	943,617	.94	.23
Number of drugs	761,240	3.99	2.15
Prescription duration	976,420	3.29	3.53
Drug expenditures	976,420	104.77	131.48
Adjusted drug expenditures	866,315	37.11	2,034.92
Total medical expenditures	976,420	390.80	1,197.89
Nondrug expenditures	976,420	68.99	1,184.73
Physician fee	976,420	201.21	90.89
Drug dispensing service fee	976,420	15.83	7.43
Pharmacist	976,420	.62	.48
Pharmacist (pre)	836,414	.72	.45
Pharmacist (post)	836,414	.88	.32
Days to hire pharmacist	123,611	176.99	277.68
Practice volume (month)	976,420	6.94	5.11
Owner	976,420	.88	.33
Physician age	968,424	45.46	10.27
Patient age	976,420	31.50	23.60
Patient is female	976,420	.56	.50

Office visits at private clinics in Taiwan, 1997-2000

Table IV: Never Clinics (Clinics that Never Hired a Pharmacist)

Dependent variables (columns):	Drug expenditures (log)	Prescription	Number of drugs	Prescription duration (days)	Nondrug expenditures (log)	Nondrug Order
Treat × Post	-.28 (.024)***	-.023 (.003)***	-.131 (.069)*	-.161 (.036)***	.109 (.041)***	-.01 (.008)
Observations	200,405	209,027	155,905	209,027	47,774	96,830
Number of time series panel id	4,111	4,216	3,732	4,216	2,617	2,962
R-squared	.18	.23	.15	.47	.28	.1

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Data: all "never" and control clinics in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table V: Adjusted Logged Drug Expenditures (Never and Always Clinics)

Dependent variable: logged drug expenditures, adjusted	"Never" clinics	"Always" clinics
Prescription duration (days)	.081	.079
	(.002)***	(.001)***
Treat × Post	-.081	-.012
	(.028)***	(.01)
Observations	206,777	660,463
Number of time series panel id	4,178	6,370
R-squared	.22	.2

All models include prescription duration physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies.

Data: Respectively, "always" and "never" clinics with control in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table VI: Always Clinics (Clinics that Always Had a Pharmacist)

	Drug expenditures (log)	Prescription	Number of drugs	Prescription duration (days)	Nondrug expenditures (log)	Nondrug Order
Treat × Post	-.001	-.001	-.038	-.008	-.006	-.002
	(.003)	(.001)	(.024)	(.014)	(.016)	(.002)
Observations	657,420	669,886	512,980	669,886	132,260	603,685
Number of time series panel id	6,284	6,320	6,086	6,320	4,485	5,072
R-squared	.25	.33	.13	.49	.26	.12

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Data: all "always" and control clinics in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table VII: Instrumental Variable Regression
Clinics with lagged practice volume† of 15 or fewer

	Drug expenditures (log)	Adjusted drug expenditures (log)	Prescription	Number of drugs	Prescription duration (days)	Nondrug expenditures (log)	Nondrug Order
Prescription duration (days)	.067 (.001)***	-.018 (.001)***					
Treat × Post	-.255 (.019)***	-.136 (.024)***	-.016 (.005)***	.263 (.058)***	-.072 (.056)	.039 (.043)	.002 (.009)
Observations	228,129	212,507	239,639	189,227	239,639	58,868	239,639
Number of Time series panel id	4,951	4,908	5,119	4,646	5,119	3,433	5,119
R-squared	.33	.09	.16	.14	.34	.21	.11

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures and logged adjusted drug expenditures include prescription duration as an additional covariate.

Data: Office visits at all specified clinics in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.
†Lagged practice volume refers to the patient volume during the six months before and after the policy date. In a random sample of 1 in 500 outpatient visits, 15 represents approximately 7,500 visits during the year.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table VIII: Test of Instrument Validity

	Never	Never
Lagged practice volume <= 15		.507 (58.64)***
Lagged practice volume <= 75	.366 (32.61)***	
Observations	8,289	8,289
F-statistic	1,063.32	3439.2
R-squared	.11	.29

OLS results obtained by regressing suspected endogenous variable "never" on dummy variables that are set to 1 if lagged practice volume (six months before and after policy implementation date) is less than 15 or 75, respectively.

Absolute value of t statistics in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table IX: Test of Instrument Exogeneity (Summary Statistics)

	Clinics with lagged practice volume ≤ 15		Clinics with lagged practice volume ≤ 75	
Variable	**Obs**	**Mean**	**Obs**	**Mean**
Prescription	175,756	.98	194,358	.98
Filled at clinic	170,646	.97	188,593	.98
Number of drugs	128,592	3.90	111,073	3.96
Prescription duration	175,756	3.42	194,358	3.05
Drug expenditures	175,756	114.15	194,358	103.13
Adjusted drug expenditures	156,090	36.98	163,579	38.17
Total medical expenditures	175,756	418.21	194,358	378.83
Nondrug expenditures	175,756	84.38	194,358	54.10
Physician fee	175,756	206.41	194,358	208.47
Drug dispensing service fee	175,756	12.4	194,358	13.13
Pharmacist	175,756	.24	194,358	.36
Pharmacist (pre)	35,711	.15	194,358	.71
Pharmacist (post)	35,711	.21	194,358	.87
Days to hire pharmacist	2,095	322.3	30,585	177.37
Practice volume (month)	175,756	6.05	194,358	4.59
Owner	175,756	.91	194,358	0.93
Physician age	174,878	47.88	192,012	45.92
Patient age	175,756	35.18	194,358	31.80
Patient is female	175,756	.55	194,358	.56

Table X: Test of Instrument Exogeneity

Pre-Policy Comparison between Instrumented Clinics† and all Non-Always Clinics

	Drug expenditures (log)	**Prescription**	**Number of drugs**	**Prescription Duration**	**Nondrug expenditures (log)**	**Nondrug order**
1997 quarter 2 × instrument	-.002	-.003	-.027	.034	-.024	-.009
	(.007)	(.002)	(.055)	(.040)	(.042)	(.006)
1997 quarter 3 × instrument	-.011	-.001	-.043	.015	-.029	-.012
	(.008)	(.002)	(.060)	(.041)	(.043)	(.008)
1997 quarter 4 × instrument	-.024	-.005	-.201	.023	.042	-.004
	(.008)***	(.002)**	(.062)***	(.043)	(.044)	(.008)
1998 quarter 1 × instrument	-.022	-.004	-.145	.051	.078	-.012
	(.009)**	(.002)	(.071)**	(.045)	(.048)	(.009)
1998 quarter 2 × instrument	-.014	-.001	-.102	.088	.122	-.016
	(.012)	(.003)	(.072)	(.052)*	(.054)**	(.010)
1998 quarter 3 × instrument	-.018	.005	-.122	.030	.100	-.009
	(.015)	(.004)	(.078)	(.066)	(.058)*	(.012)
1998 quarter 4 × instrument	-.015	.009	-.047	.109	.161	-.020
	(.018)	(.005)*	(.081)	(.062)*	(.064)**	(.012)*
1999 quarter 1 × instrument	.042	.017	-.083	.139	.067	-.007
	(.038)	(.009)*	(.097)	(.095)	(.074)	(.016)
1999 quarter 2 × instrument	-.021	.011	-.259	-.096	.059	.001
	(.025)	(.010)	(.157)*	(.196)	(.127)	(.036)
1999 quarter 3 × instrument	.082	.008	-.389	.316	.150	-.041
	(.052)	(.009)	(.187)**	(.120)***	(.173)	(.046)
1999 quarter 4 × instrument	.012	.004	-.155	.443	.197	-.072
	(.041)	(.009)	(.209)	(.130)***	(.093)**	(.057)
2000 quarter 1 × instrument	.050	-.010	-.361	.144	.337	-.088
	(.052)	(.006)*	(.193)*	(.153)	(.244)	(.052)*
2000 quarter 2 × instrument	-.083	-.010	-.925	-1.098	.325	.046
	(.071)	(.006)	(.469)**	(1.326)	(.081)***	(.043)
2000 quarter 3 × instrument	.000	.000	.000	.000	.000	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
2000 quarter 4 × instrument	.000	.000	.000	.000	.000	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Observations	224,559	234,101	162,620	234,101	58,262	234,101
Number of panel_id	4,614	4,707	3,861	4,707	3,047	4,707
R-squared	.36	.21	.15	.35	.23	.11

†Instrumented clinics refer to all clinics (except always clinics) located in jurisdictions eventually subject to the separating policy with a lagged practice volume of 15 or fewer during the six months before and after the policy.

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Data: all pre-policy observations of clinics in Taiwan except always clinics and clinics located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table XI: Test of Appropriateness of Control

Pre-Policy Comparison between Treatment Clinics† and Control Clinics

	Drug expenditures (log)	Prescription	Number of drugs	Prescription duration	Nondrug expenditures (log)	Nondrug order
1997 quarter 2 × treatment	.004	.001	.013	.030	.048	-.001
	(.005)	(.002)	(.043)	(.031)	(.032)	(.005)
1997 quarter 3 × treatment	.007	.001	.045	.016	.005	-.000
	(.006)	(.002)	(.049)	(.035)	(.033)	(.006)
1997 quarter 4 × treatment	.015	.003	.138	.023	-.002	-.001
	(.006)***	(.002)*	(.051)***	(.037)	(.034)	(.007)
1998 quarter 1 × treatment	.003	.001	.146	-.010	-.024	.003
	(.006)	(.002)	(.059)**	(.038)	(.036)	(.008)
1998 quarter 2 × treatment	.002	.003	.094	-.040	-.058	.006
	(.007)	(.002)	(.058)	(.043)	(.041)	(.008)
1998 quarter 3 × treatment	.014	-.002	.093	-.063	-.038	.011
	(.008)*	(.002)	(.063)	(.050)	(.043)	(.009)
1998 quarter 4 × treatment	-.000	-.004	.119	-.097	-.052	.011
	(.008)	(.002)*	(.065)*	(.049)**	(.044)	(.009)
1999 quarter 1 × treatment	-.013	-.003	.160	-.006	.005	-.001
	(.010)	(.003)	(.069)**	(.054)	(.046)	(.010)
1999 quarter 2 × treatment	.006	-.004	.150	-.006	-.020	.016
	(.013)	(.006)	(.089)*	(.077)	(.090)	(.015)
1999 quarter 3 × treatment	.000	.000	.219	-.111	-.189	.025
	(.014)	(.004)	(.098)**	(.090)	(.109)*	(.016)
1999 quarter 4 × treatment	-.003	-.007	.234	-.178	-.091	.045
	(.015)	(.005)	(.100)**	(.073)**	(.093)	(.016)***
2000 quarter 1 × treatment	.000	.005	.218	.059	-.051	.029
	(.018)	(.004)	(.099)**	(.113)	(.098)	(.018)
2000 quarter 2 × treatment	.049	.019	.465	.354	-.012	.041
	(.032)	(.011)*	(.172)***	(.276)	(.282)	(.029)
2000 quarter 3 × treatment	.000	.000	.000	.000	.000	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
2000 quarter 4 × treatment	.000	.000	.000	.000	.000	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Observations	464,918	482,067	307,953	482,067	111,915	482,067
Number of panel_id	8,504	8,638	7,386	8,638	5,901	8,638
R-squared	.34	.23	.14	.31	.22	.11

†Treatment clinics are those situated in jurisdictions that eventually become subject to the separating policy during the study period.

Coefficients are meant to capture pre-policy differences in prescribing behavior between treated clinics and control clinics. All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table XII: Switchers (Comparison between Pre-Policy and Post-Policy/ Pre-Integration Observations)

	Drug expenditures (log)	Adjusted drug expenditures (log)	Prescription	Number of drugs	Prescription duration (days)	Nondrug expenditures (log)	Nondrug Order
Treat × Post	-.308	-.131	-.016	-.144	-.168	.074	-.027
	(.035)***	(.042)***	(.004)***	(.074)*	(.062)***	(.056)	(.014)*
Observations	184,022	167,603	190,086	143,759	190,086	46,763	190,086
Number of time series panel id	2,560	2,533	2,600	2,443	2,600	1,812	2,600
R-squared	.35	.08	.21	.15	.38	.24	.12

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Data: all switchers (all pre-policy observations, and all post-policy observations before clinics hired a pharmacist) and control clinics in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table XIII: Switchers (Comparison between Interim Period and Post-Pharmacist Period)

	Drug expenditures (log)	Adjusted drug expenditures (log)	Prescription	Number of drugs	Prescription duration (days)	Nondrug expenditures (log)	Nondrug Order
After hiring pharmacist	.110	-.016	.004	.107	.093	-.046	.014
	(.015)***	(.024)	(.002)*	(.053)**	(.036)***	(.041)	(.008)*
Observations	229,634	215,870	237,782	203,150	237,782	56,650	237,782
Number of time series panel id	3,442	3,428	3,487	3,390	3,487	2,548	3,487
R-squared	.36	.08	.17	.14	.39	.23	.12

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Data: all switchers (all post-policy observations) and control clinics in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table XIV: Switchers (Interim Observations Dropped)

	Drug expenditures (log)	Adjusted drug expenditures (log)	Prescription	Number of drugs	Prescription duration (days)	Nondrug expenditures (log)	Nondrug Order
Treat × Post	.006	-.069	-.001	.041	-.069	-.032	.004
	(.007)	(.025)***	(.002)	(0.046)	(.037)*	(.041)	(.007)
Observations	255,899	237,433	264,487	214,112	264,487	62,780	264,487
Number of time series panel id	3,440	3,425	3,479	3,365	3,479	2,558	3,479
R-squared	.38	.09	.2	.14	.37	.23	.12

All models include physician fixed effects, patient age, gender, city/county dummies, quarter dummies, and disease code dummies. Logged drug expenditures include prescription duration as an additional covariate.

Data: all switchers (except "interim" observations, meaning office visits after the effective date and before clinic hired a pharmacist) and control clinics in Taiwan except those located in jurisdictions with mixed implementation dates, January 1, 1997 to December 31, 2000.

Robust standard errors clustered by physician ID in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

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Figure I: Likelihood of Integration Based on Past Practice Volume