# Moral Hazard and Economies of Scope in Physician Ownership of Complementary Medical Services

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**Abstract:** When physicians own complementary medical service facilities such as clinical laboratories and imaging centers, they gain financially by referring patients to their complementary service entities. This situation creates an incentive for the expert to exploit consumers' trust to recommend more services than they would demand under full information. This moral hazard cost, however, maybe offset by gains in economies of scope from the diagnostician also providing the services. We assess the extent moral hazard and economies of scope using data from Taiwan who introduced a policy identical to the Stark Law in the US that restricts physician ownership of pharmacies unless they are fully integrated in the physician's practice. We find the physicians who own pharmacies prescribe 7.6% more drugs than those who do not own pharmacies. While we find no evidence of economies of scope in the treatment of patients with acute respiratory infections and diabetes, we find some limited evidence supporting economies of scope for hypertension.

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## 1. Introduction

Consumers often turn to experts for advice when they have difficulty discerning the extent to which goods and services meet their needs such as in cases of automotive and appliance repair, engineering services, financial services, and medical care. Experts not only give advice but also sell goods and services to consumers based on that advice. This situation creates an incentive for the expert to exploit consumers' trust to recommend more services than they would demand under full information (Taylor 1995 and Wolinsky 1993). This moral hazard cost, however, maybe offset by gains in economies of scope from the diagnostician also providing the services.

This moral hazard inherent in such "credence" goods is present in physician ownership of complementary medical service facilities such as clinical laboratories, imaging centers (i.e., X-rays, CAT scans and MRIs), pharmacies and physical therapy centers. Physicians gain financially by referring patients to a complementary service entity in which the physician has a financial interest. Patients, who rely on physicians for medical advice, have little incentive to reject prescribed procedures especially when insurance covers most of the cost. Because patients have imperfect information on the scope of the problem, diagnosticians have incentive to recommend procedures that are profitable regardless of their cost or appropriateness.

To combat these moral hazard or "self referral" abuses, Congress passed the Stark Law[1] in 1989, which prohibits physician referrals of Medicare and Medicaid patients to complementary care facilities in which the physician or members of the physician's group have a financial interest.[2] The Law has a "safe harbor" exception that permits physicians to escape prohibition if the self-referral is for services provided within physician offices or by a "bona fide employee." In other words, the Law draws a distinction between permissible referrals made within an integrated firm and impermissible referrals made to outside entities. Presumably, the exception for integrated organizations is based on potential economies of scope from the joint production of physician and complementary medical

[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

[2] The services covered under the Stark Law include clinical; physical and occupational therapy; radiology; radiation therapy; medical equipment and supplies; nutrients; prosthetics; home health; prescription drugs; and hospital visits.

services. Indeed, economies of scope may arise from better coordination of care and thereby improve patient outcomes (Afendulis and Kessler, 2007).

However, this exception also allows physicians to circumvent the intent of the Stark law by integrating complementary services directly into their practices. In fact, there has been massive integration of such services into physician groups since the enactment of the Stark Law (Tynan et al., 2008, United States Government Accountability Office, 2008). For instance, over the last two decades 27% of physician groups expanded in-office imaging services.[3] And Medicare spending for imaging services increased on average 13% per year, with over two-thirds of the spending occurring in physician office settings.[4]

Policy makers are keenly aware of the problems with the exemption. The Medicare Patient Advisory Commission recently proposed extensive changes to limit the exemption.[5] And the Center for Medicare/Medicaid Services recommended that physicians who make self-referrals must disclose their financial interests to patients in a June, 2010 Report to Congress under the Patient Protection and Affordable Care Act of 2010.

In this paper, we use data from Taiwan to estimate the moral hazard costs and gains from economies of scope associated with physician ownership of pharmacies. For identification, we exploit a policy prohibiting physicians from owning pharmacies and dispensing drugs that was phased in geographically over time. The policy included an exemption for physician groups that have onsite pharmacist, which is identical to the "in-office ancillary services" safe harbor provision in the Stark Law. We exploit this exemption

[3] Medicare Part B Imaging Services: Rapid Growth and Shift to Physician Offices Indicate Need for CMS to Consider Additional Management Practices, GAO-08-452, June 2008. See also "MedPAC Seeks to Further Stop In-Office Imaging Incentives," HealthImaging.com, June 21, 2010, http://www.healthimaging.com/index.php?option=com_articles&view=article&id=22832:medpac-seeks-to-further-stop-in-office-imaging-incentives, accessed July 12, 2010.

[4] 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.

[5] MedPAC considered four proposals in their draft discussion relating to ancillary services on February 23, 2011: (1) Elmiation of duplicate services and payment that may be furnished when services are provided together, (2) Extension of the multiple procedure payment reduction (MPPR) to the professional component of two or more diagnostic imaging services provided in a single sesion; (3) reduction in the professional component for imaging and other diagnostic tests that are ordered and interpreted by the same physician; and (4) Institution of prior authorization or prior notification of advanced imaging ordered by physicians who are outliers. See American Urological Association's response to these proposals, http://www.auanet.org/content/media/MedPAC%20letter%20AUA%20Comments%20to%20MedPAC%20-%20In-Office%20Ancillary%20Use%20_2_.pdf, accessed October 18, 2011.

to examine the extent to which the policy induced integration of physician groups and pharmacies, and the consequences of integration on costs and health outcomes.

Our analysis is made possible by a novel data set the links patient claims over time to medical care providers for the universe of patients and providers. Taiwan is a country with OECD level income and health status indicators. It also has a sophisticated health care delivery system with a universal single-payer health insurance system similar to Canada (Lu and Hsiao, 2003). The single payer health insurance plan maintains individual claims data that can be linked over time and to providers. Our analysis data set is representative of universe of patients and providers in Taiwan.

We find evidence of a significant moral hazard effect from the self-referral incentives. We estimate that the Taiwanese policy reduced drug expenditures by 7.1% among physicians that did not have an onsite pharmacist. However, the overall effect of the policy on costs was substantially smaller for a number of reasons. First, physicians responded by increasing the overprovision of self-referred diagnostic services not covered by the policy. Second, more than half of the physician groups (clinics) already had an onsite pharmacist prior to the policy and therefore were exempt. Third, close to half of the clinics that did not have an onsite physician group prior to the policy hired onsite pharmacists subsequent to the implementation of the policy.

In addition, we find limited, at best, evidence to suggest that there are economies of scope from the integration of pharmacists into physician offices. First, the integration of pharmacy services into physician offices failed to create efficiency savings – indeed, it generated the opposite result, as moral hazard effects dominate. Second, while integration did not appear to reduce observable patient therapy compliance for diabetes, we find small but significant improvement in therapy compliance for patients with hypertension. Finally, we find no evidence of economies of scope in the treatment of patients with acute respiratory infections, diabetes, and hypertension on preventable adverse medical events.

Our work makes several significant contributions to the literature. First, to our knowledge, this is one of the first to establish a causal role of self-referral incentives in the overprovision of ancillary medical care services. Virtually all of the empirical work to date produced cross-sectional correlations that are consistent with the overtreatment

hypothesis.[6] Iizuka (2007) studies the Japanese prescription drug market where doctors can both prescribe and dispense drugs. He shows that physicians who dispense their own drugs in Japan take a drug's markup into account in prescription decisions. He estimates that the profit incentive for physician to dispense drugs increases prescriptions by 14%.

Our work is also, to our knowledge, the first to examine the effect of policies restricting self-referrals on the organizational structure of the industry. Our results suggest that much of the integration of ancillary services into the physician offices is driven by the desire to use safe harbor exemptions to circumvent the legal prohibition of self-referrals and not from potential savings from the economies of scope of joint production.

Finally, our work contributes to the more general literature on credence goods that investigates the moral hazard that arise when consumers consult an expert who both provides advice and sells a related product in the presence of asymmetric information. Chevalier and Ellison (1997) find that mutual fund managers often try to increase the inflow of funds against the interests of their customers, who would rather maximize their risk-adjusted returns. Hubbard (1998, 2002) investigates the California vehicle emissions inspection market where inspectors also provide repairs to help vehicles pass. Levitt and Syverson (2008) show that homes owned by real estate agents sell for higher prices than comparable homes owned by non-agents. Gruber and Owings (1996) document a negative relationship between birth rate and Cesarean section delivery as evidence of physician-induced demand for costly procedures. Finally, Afendulis and Kessler (2007) examine the tradeoffs between the loss from moral hazard and the gain from economies of scope in the joint production of diagnosis and treatment of cardiac conditions.

Our work also informs policy in a number of specific ways. 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 self-referrals may lead physicians to increase the quantity of a related service, particularly when no limit is placed on the other discretionary service. Finally, the bright-line "bona fide employee" or "in office ancillary services" exception may be misguided when the referring

[6] For example see Childs and Hunter (1972), Strasser et al (1987), Hillman et al (1990, 1995, 1992), Aronovitz (1994), Mitchell and Sass (1995), Gazelle et al (2007), Mitchell (2005) and Mitchell (2008). )

physicians derive profits from the referral of patients to the employee of an integrated practice. In other words, providing safe harbor exceptions may ultimately void the very purpose of the original prohibition as physicians exploit these loopholes to continue to engage in profitable self-referrals. This study empirically validates that an "in-office" exemption not only facilitates overutilization of medical care, but also has a profound impact on the organization of health care services.

## 2. Conceptual Framework

We model the prohibition against self-referrals as a fee reduction in complementary services that the physician proscribes (recommends) to patients. Suppose physicians prescribe two types of complementary services: (1) services that are subject to the prohibition such as drugs and (2) services that are not subject to the prohibition such as imaging. Physician income from complementary services can be represented simply as $Y = N\big(\pi_1 s_1(w_1) + \pi_2 s_2(w_2)\big)$ where $N$ is the number of patients she sees, $\pi_i$ is the profit margins for service *i*, and $s_i$ is the quantity of service *i* provided, which depend on $w_i$ – the amount of overtreatment. If the physician provides the clinically appropriate level of care, then the level of overtreatment is zero.

Physicians gain income from over-treating patients, but pay a cost in terms of disutility. Before the prohibition, the equilibrium amount of overtreatment equates the marginal income from overtreatment of each service to the marginal disutility from total overtreatment. The prohibition reduces the profit margin for drugs, $\pi_1$, to zero. In this case, the physician prescribes only the amount of drugs needed and overprovision of drugs is zero. However, the physician increases over-provision of non-prohibited services from which she still has a positive return.

Physicians with an onsite pharmacist are *de jure* exempt from the prohibition and there is no impact of the policy on their overprovision of drugs. One option for those that do not have onsite pharmacy is for them to legally circumvent the prohibition policy by hiring a pharmacist. However, hiring a pharmacist requires paying the fixed costs that include salary, space, equipment, and training.

The physician integrates pharmacist services into her medical practice if the profits from hiring a pharmacist are larger than not hiring one. The net income from integration is

$Y^{ph} = N\left(\pi_1 s_1(w_1^*) + \pi_2 s_2(w_2^*)\right) - F$, where $F$ is the fixed cost of hiring a pharmacist and $w_i^*$ is the optimal overprovision. The income generated without a pharmacist is $Y^{ph} = N\pi_2 s_2(w_2')$ where $w_i'$ is the optimal overprovision under the prohibition. A physician, then, hires a pharmacist if $Y^{Ph} - Y^{no} > 0$, which translates into

$$N > \frac{\mathrm{F}}{\pi_1 \mathrm{s}_1\left(\mathrm{w}_1^*\right) + \pi_2 \mathrm{s}_2\left(\mathrm{w}_2^*\right) - \pi_2 \mathrm{s}_2\left(\mathrm{w}_2^{'}\right)} \quad . \tag{1}$$

The physician will integrate if the number of patients is larger than the fixed cost divided by the per-patient marginal profit from hiring an onsite pharmacist. The larger the fixed costs and the lower the return to overprovision, the greater the number of patients required to make integration profitable. In other words, there needs to be sufficient scale to be able to cover the fixed costs.

The framework provides us with a number of empirically testable predictions. First, the prohibition policy will reduce drug prescriptions by physicians in clinics that do not have a pharmacist on site. One can interpret this reduction as the moral hazard cost to the financial incentives inherent in physician ownership of complementary medical services. Second, the policy will increase the over-provision of other non-prohibited complementary services meaning that any efficiency gains from reducing unnecessary drug prescriptions will be at least partially offset by increases in other services. Third, the exemption to the prohibition gives clinics an incentive to integrate pharmacists into their practice to circumvent the prohibition if they have sufficient patient volume to cover the fixed costs.

## 3. Institutional Context

Taiwan is a country of 23 million people that has an advanced market economy with a per capita GDP of US$32,200. Taiwan's health indicators are on par with OECD nations: Life expectancy in Taiwan is 72 for men and 78 for women, and infant mortality is 6.14 per 1,000 live births.[7] Taiwan's health care infrastructure is also comparable to that of many OECD countries, with 5.7 hospital beds and 8.7 medical professionals per 1,000 (Lu and

[7] Index China.com, http://www.index-china.com/index-english/taiwan-f.htm, accessed September 21, 2010.

Hsiao, 2003). By 2007, total health expenditures represented 6.3% of Taiwan's GDP, of which 21% consisted of prescription drug expenditures.

Taiwan's health care system, modeled after Canada, is publicly financed with patients free to choose among a mixture of private and public medical care providers. Taiwan's National Health Insurance has achieved universal enrollment for a comprehensive package of medical services. Virtually all health care providers are under contract with the National Health Insurance Bureau (NHIB), which pays physicians fee-for-service for consultations, diagnostic tests performed in the office, and drug prescriptions (Cheng, 2003).

Prior to 1997, physicians were also allowed to dispense drugs directly out of their offices (Wagstaff, 2007). NHIB paid physicians for dispensing drugs based on a set of fixed formulary fees. Physicians, however, were able to procure the drugs directly from manufacturers at a much lower cost than the NHI formulary prices (Eggleston, 2009). As a result, physicians reaped handsome profits from the sale of drugs.

In 1997, facing rapidly rising prescription drug expenditures, Taiwan instituted an anti-self-referrals policy called the "separating" policy, designed to disentangle physicians' diagnosis and treatment decisions from the financial incentive to prescribe drugs (Chou et al., 2003). The "separating" policy prohibited clinics from dispensing drugs directly by physicians from their offices and physicians from owning offsite pharmacies. The NHIB partially compensated physicians for the loss income from drug sales by increasing consultation fees for clinics that did not integrate.

There were a number of exceptions. First, like the Stark Law, physician clinics that have a licensed pharmacist onsite along with a dedicated space with adequate storage including refrigeration and recording keeping were allowed to continue to dispense drugs from an onsite pharmacy. Second, clinics without an onsite pharmacist were allowed to continue to dispense drugs to young children and the elderly for acute medical conditions for which quick dispensing of drugs was deemed important.

The separating policy was rolled out across communities over the course of several years, beginning on March 1, 1997. Before enforcing the policy in a jurisdiction, the NHIB had to ascertain that there were sufficient numbers of independent pharmacies where patients could fill their prescriptions. Jurisdictions became subject to the policy as soon as the NHIB determined there were at least 1 independent pharmacy for every 500 residents.

There was a sharp increase in pharmacists following the implementation of the policy. In 1997 there were 23,892 licensed pharmacists. By 2010, there were over 40,000 licensed pharmacists. The cost of hiring a pharmacist jumped dramatically from an average of US$ 625 per month before the policy to almost US$ 1870.

There was some concern that physicians tried to get around the separating policy through "gateway pharmacies," which are legally independent pharmacies registered in the name of the pharmacist but in reality owned by clinic physicians. Although the true status of independent pharmacies is unobservable, Lee (2007) defined gateway pharmacies as those for whom 70% of their prescriptions came from a single physician group. He estimated that only 3.6% of clinics used gateway pharmacies in 2000.

There is short-run credible evidence that the separating policy did indeed reduce drug expenditures. Nine months after the policy was first introduced, Chou et al (2003) reports that drug prescription rates and average drug expenditures fell by 16% and 12% more than in the 2 cities that implemented first compared to other two cities that had yet to implement the policy.

## 4. Data

The National Health Research Institute maintains all medical claims filed with the NHIB. Our data is a 0.2% random sample of ambulatory care claims from 1997 through 2000.[8] While the separating policy was implemented at the village level, the claims data provide geographic identification only at the more aggregate township level, each of which includes multiple villages. We drop data from townships with mixed implementation dates, which is close to 25% of the sample.

The unit of observation is the outpatient visit. Available information includes use of and expenditures on physician and complementary medical services associated with those office visits, regardless of whether those services were provided on or offsite.

In Table 1 we provide baseline descriptive statistics disaggregated by clinics integration status at baseline – i.e. whether they had a pharmacy inside the clinic. At baseline, prior to the separating policy, 37% of clinics had an onsite pharmacist (Table 1,

[8] We limit our study to the period ending on December 31, 2000 as the separating policy was fully implemented by the end of 2000 and the NHIB changed physician payment to a Global Budget format in 2001.

Panel B). Consistent with our conceptual framework, the integrated clinics were substantially larger than the non-integrated clinics in terms scale (visits and revenues). While integrated clinics are larger than un-integrated clinics, there appear to be no differences in the types of patients that they attract or in they way in which they treat those patients.

## 5. Reduced Form Policy Impact

In this section we estimate the reduced-form impact of the separating policy on clinics that did not have a pharmacist onsite at baseline using the patient visit as the unit of analysis. The effect is reduced form in the sense that it is an average of the effects for clinics subject to the policy at baseline regardless of their decision to integrate in reaction to the policy. We consider the following outcomes: (1) any drug prescription, (2) drug expenditures conditional on any prescription, (3) any diagnostic test, (4) expenditures on diagnostic tests conditional on any test, (5) expenditures on physician services such as consultation, procedures and prescriptions, and (6) total visit cost.

### 5.1. Event History Analysis

Figures 1 and 2 plot the share of visits that receive drug prescriptions and the average drug expenditure per visit conditional on positive expenditures over time for (1) clinics that never had an onsite pharmacist (never) and therefore were subject to the separating policy, (2) those that had an onsite pharmacist prior to the implementation of the policy (always) and therefore were exempt from the policy. The horizontal axis is centered at time zero, which is defined as the month in which the separating policy became effective in the jurisdiction in which the clinic is located. Hence, observations to the right of zero refer to the number of days post implementation, and observations to the left refer to the number of days prior to implementation.

Prior to the implementation of the separating policy, the levels and trends of drug prescriptions are similar for both types of clinics. However, after the implementation of the policy, drug prescriptions fall in clinics that did not have an onsite pharmacist prior to the policy. On the other hand, drug prescriptions in clinics that had onsite pharmacist prior to policy continued on trend after the implementation of the policy.

## 5.2. Identification Strategy

We use a difference-in-differences approach that compares the change in outcomes in the treatment group to the change in outcomes in a comparison group. The change in the comparison group is an estimate of the true counterfactual – that is, what would have happened to the treatment group if there were no intervention. The strategy controls for time-invariant characteristics at the physician level as well as time-varying factors common to both comparison and treatment physicians at the local township level, and for heterogeneity in patient demographic and epidemiological case-mix.

Our comparison group is the set of clinics that had onsite pharmacists at baseline and therefore were not subject to the separating policy. The advantage of this comparison group is that they are located in the same townships as the treatment group. This allows us to control for time-varying factors that may have been introduced contemporaneously at the township level with the implementation of the policy, but allows time-varying shocks to have different impacts across townships. Examples include new local health policies, differential economic growth, and differing changes in the local epidemiological environment.

An additional source of variation that helps identify the impact comes from the fact that the policy was phased in geographically over time. In this case the treatment group consists of clinics located in the townships that were early adopters of the policy and the comparison group consist of those located in late adopter townships.

We use both sources of variation to estimate the following specification:

$$y_{ijkt} = \alpha_j + \gamma_{kt} + \sum_l \delta_l x_{il} + \beta T_{jkt} + \varepsilon_{ijkt} \quad (2)$$

where

- $y_{ijkt}$ is the dependent variable for patient $i$ who is treated by physician $j$ living in township $k$ in period $t$;
- $\alpha_j$ is a fixed effect for physician $j$;
- $\gamma_{kt}$ is a fixed effect for township $k$ in time period $t$;
- $x_{il}$ is a vector of individual characteristics including age, sex and 65 indicators of the patient's primary illness diagnosis;

- $T_{ikt}$ equals 1 for clinic $j$ if that clinic did not have an onsite pharmacist at baseline and is located in community $k$ in period $t$ after the separating policy has been implemented; and
- $\varepsilon_{it}$ is a zero mean random error.

The coefficient $\beta$ is the difference-in-difference estimate of the impact of the policy on drug expenditures and diagnostic expenditures. The physician fixed-effects control for unobserved heterogeneity between physicians. The 65-illness indicators combined with patient age and sex control for heterogeneity in patient case-mix. And the township specific time fixed-effects control for factors that change over time but are common to both treatment and comparison groups within each township. The standard errors are clustered at the township level.

### 5.3. Results

The results are consistent with our theoretical predictions (Panel A of Table 2). The separating policy reduced the likelihood of prescription by 1.4% and the amount of drugs prescribed (expenditures) by 3.6% conditional on a prescription. These translate into a reduction in total drugs expenditures of 5.2%.[9] Physicians did compensate by increasing the share of patients that had diagnostic tests but there was no effect on physician services. Overall, the policy reduced total non-physician health care costs (drug plus diagnostic plus physician services) by 1.3% for clinics not integrated at baseline. However, the policy was binding only for clinics that did had an onsite pharmacist, which at baseline treated 40% of patients. Hence, the overall impact on total drug expenditures was a reduction of 1.7% and on total expenditures was a reduction of 0.5%.

Our results are substantially smaller than those of Chou et al (2003), who reported that nine months into the implementation of the policy drug prescription rates and average drug expenditures fell by 16% and 12%. Their results are come from comparing the average aggregate change in 2 cities that implemented first to the average change in 2

[9] We calculate %Δ in expenditures as follows. Note that $E(Exp) = E(Exp|Exp > 0) * Prob(Exp > 0)$. Totally differentiating and rearranging terms gives us $(\Delta Exp/Exp) * 100 = (\Delta E(Exp|Exp > 0)/E(Exp|Exp > 0) + \Delta Prob(Exp > 0)/Prob(Exp > 0)) * 100$. The terms $\Delta E(Exp|Exp > 0)/E(Exp|Exp > 0)$ and $\Delta Prob(Exp > 0)$ are estimated coefficients and $Prob(Exp > 0)$ is estimated as the baseline mean for the clinics that did not integrate from Table 1.

other cities that had yet implemented the policy. Some of the reasons for these differences could due to: (1) we examine the impact of the policy nationwide and for a substantially longer period; (2) the short recall period in the Chou et al analysis may not have long enough to fully capture the extent to which clinics integrated by employing onsite pharmacists to circumvent the policy, which would tend to lower the estimates; and (3) we employ a visit level analysis that additionally controls for physician fixed effects, patient and illness characteristics, and time-varying factors at the township level.

## 5.4. Control Group Validity

### 5.4.1. Baseline Balance of Treatment and Groups

Panel A in Table 1 compares the means of patients characteristics and treatment patterns of the treatment clinics (Not Integrated) and control clinics (Integrated). Both treatment and control clinics attracted the same type of patients and provided those patterns with the same type of care in terms of drug prescriptions and diagnostic tests. There are no statistically significant differences in the means of 12 out of the 13 patient level variables in Table 1. Specifically, we find no differences in patient sex and or in medical reason for the visit. Slightly over 40% of their office visits to both types of clinics are for a respiratory infection. Physicians in both types of clinics preform diagnostics test on about a quarter of patients and spent about the same amount on those tests. Physicians in both types of clinics also prescribed medications to almost all patients, prescribed identical classes of medications (primarily of antacids, anti-inflammatory agents, antihistamines, and other palliative medications associated with the common cold or upper respiratory infections), and prescribed the same amount in terms of expenditures on drugs. The one difference in that slightly younger patients seem to visit integrated clinics.

### 5.4.2. Pre-intervention Trends

The key assumption underlying the causal interpretation in our difference in differences model is that the change in the comparison group is an unbiased estimate of the counterfactual – i.e. what would have been the change in the treatment group without the separating policy. While we cannot directly test this assumption, we did employ the Heckman-Hotz test that asks whether the pre-intervention trends of the treatment group are different from those of the comparison group. If the pre-intervention trends are not

different, there would be no reason to assume that they would be different in the post period without the intervention.

We implement the test by dropping all post-policy observations from our sample, and running the following model:

$$y_{ijkt} = \alpha_j + \gamma_{kt} + \sum_l \delta_l x_{il} + \sum_k \lambda_k T_{jk} Trend_t + \varepsilon_{it} \quad (3)$$

where $T_{jk}$ is a dummy variable indicating whether physician $j$ in municipality $k$ will be in the treatment group in the post period, $Trend_t$ is a continuous time variable indicating quarter, and all variables are as defined for equation (2). The coefficients of interest are the $\lambda_k$'s on the interaction terms consisting of the municipality, treatment dummy and time trend. Note that we test if the pre-intervention trends are equal for each municipality separately. If these are jointly zero, then we reject the hypothesis that the pre-intervention trends are different for the treatment and comparison groups.

We estimate equation (3) for each of the dependent variables. Almost all of the coefficients (108 out 112) on the interaction terms are not significantly different that zero and the F-statistics for the hypothesis that all of the coefficient interaction terms are jointly for each outcome zero cannot be rejected at conventional significance levels for each of the six dependent variables (Appendix Table B).

### 5.5. Robustness Tests

#### 5.5.1. Using Only Clinics Not Integrated at Baseline

An alternative strategy is to take advantage of the fact that the policy was phased in geographically over time and estimate the model using only the set of clinics that did not have an onsite pharmacist at baseline. In this case the treatment group consists of clinics located in the townships that were early adopters of the policy and the comparison group consist of those located in late adopter townships. This strategy controls for time varying shocks that are common across treatment and comparison areas, but not for time varying shocks that idiosyncratically vary across treatment and comparison townships. The specification is identical to equation (3), except that the time fixed-effects do not varying by township. We find that point estimates, presented in Table 2 Panel B, are very close to the

main regression results in Panel A. However, the estimates are less percise due to substantially smaller sample sizes and more limited variation for identification.

### 5.5.2. Placebo Test

The facts that the integrated and non-integrated clinics were balanced at baseline and that there are no differences in the pre-intervention trends support the identifying assumption that the change in the control group is a consistent estimate of the counterfactual. However, we cannot still not completely rule out that the remote possibility that post trends would have diverged within Townships due to unobservable time-varying factors.

We use a placebo test to try to rule out this possibility. Specifically, we estimate the reduced form specified in equation (2) on the sample of visits to clinics by patients who are exempt from the separating policy i.e. the young and the elderly. Since they are exempt, the introduction of the separating policy should have no effect on drug and diagnostic expenditures for these patients. The results are presented in Panel C of Table 2 and show no effect of the separating policy.

### 5.5.3. Patient Sorting

An alternative explanation of the results could be patient sorting in response to the policy. i.e. patients who had strong preferences for drugs or the convenience of for filling their prescriptions on site switched to control facilities as a result of the policy. To address this possibility, we examined patient clinic switching behavior using a panel data set of 200,000 individuals. Of those, 155,343 sought care at a clinic at least once between January 1, 1997 and December 31, 2000. On average, patients sought care six times per year. Of those that patronized clinics without a pharmacist at baseline, only 347 (0.2%) switched to visit a different clinic with a pharmacist after the policy was implemented in their township.

## 6. Moral Hazard

The moral hazard effect is the extra drug expenditures resulting from the incentive to prescribe more drugs from physician ownership of pharmacies. We estimate this as the difference in drug expenditures with and without pharmacy ownership identified by the

effect of the separating policy on clinics that did not integrate. This requires us to estimate the effect of the policy on physician behavior taking into account the integration decision. Specifically we are interested in separately estimating the effect on physician treatment decisions in clinic that chose not to integrate and those that did choose to integrate. In this case, we are able to identify the moral hazard effect of self-referral incentives as the effect of the policy on visits to clinics that did not integrate.

### 6.1. Integration

A substantial number of clinics that were not integrated before the policy choose to integrate after the policy was implemented in their location. Table 3 reports mean baseline characteristics for sample that did not have an onsite pharmacist at baseline by post-policy integration status in 2000. Thirty-eight percent of these clinics choose to integrate after the policy was implemented (Table 3, Panel B).

Moreover, as predicted by our model, the larger clinics were more likely to integrate than were smaller clinics (Table 3, Panel B). Clinics that integrated had 68% more patients on average than those that did not integrate. Figure 3 displays the probability of integration overtime separately for clinics in different parts of the distribution of patient volume at baseline: (1) large clinics in the top 40% of the patient volume distribution, medium size clinics that are between 10% and 40% of the distribution, and small clinics in the bottom 10%. The probability of integration is effectively zero up until the separating policy becomes effective and increases dramatically shortly thereafter with substantially larger increases for the medium- and large-volume clinics.

These graphical results are confirmed in a difference in difference analysis of the impact of the policy on the integration. In Table 4, reports the results of equation (2) for the integration where the clinic is the unit of observation using the sample of clinics that were not integrated at baseline. Model (1) confirms that 66% of clinics choice to integrate after the policy was implemented in their community. Model (2) adds interactions of treatment with the baseline quartile of the distribution of number of visits to the clinic. The results show that clinics that had larger volumes of patients at baseline were more likely to integrate.

Despite that fact that larger clinics chose to integrate, there was no difference in the types patients nor in the treatment patterns between clinics that choose to integrate and those that did not (Table 3, Panel A). Specifically, we find no differences in patient sex and or in medical reason for the visit. Physicians in both types of clinics preform diagnostics test on about a quarter of patients and spent about the same amount on those tests. Physicians in both types of clinics also prescribed medications to almost all patients, prescribed identical classes of medications, and prescribed the same amount in terms of expenditures on drugs. The one difference is that patients who visited integrated clinics were younger.

### 6.2. Identification Strategy

We employ a triple differences strategy that assumes that the decision to integrate was based on fixed characteristics at the time the implementation of the separating policy. Specifically, we compare the difference-in-difference estimate of the impact of the policy on clinics that did not integrate to the difference-in-difference estimate of the impact on clinics that did integrate. The first double difference compares the change in outcomes of those that did not integrate to those that were integrated at baseline and the second double difference compares the change in those that did integrates to those that were integrated at baseline.

The assumption that the decision to integrate is based on time-invariant characteristics is consistent with the fact the almost all clinics that integrated did so immediately after the policy is implemented in their township (Figure 3). Moreover, while patient volume is correlated with the decision to integrate (Table 3, Panel B) as predicted by theory, there are no baseline differences in the types of patients treated or in the treatment patterns between the types of clinics (Table 3, Panel A).

We adjust the specification in (2) to allow the effect of the separating policy to the clinic's integration status and integration interacted with treatment:

$$y_{ijkt} = \alpha_j + \gamma_{kt} + \sum_l \delta_l x_{il} + \beta T_{jkt} + \phi T_{jkt} I_{jt} + \varphi I_{jt} + \varepsilon_{it} \quad (5)$$

where $I_{jt}$ is an indicator that equals 1 if clinic $j$ is integrated and has an onsite pharmacist in period $t$. The coefficient on pharmacist times treatment, $\phi$, estimates the effect of the policy

on visits to clinics with onsite pharmacists and visits to clinics that did not integrate. The effect of the policy on visits to clinics that integrated is therefore $\beta + \phi$. The direct effect of having a pharmacist onsite is not separately identified from the provider fixed effect. The coefficient $\beta$ is the moral hazard effect. It is the effect of removing the incentive to overprescribe drugs by eliminating physician profits from drug sakes through the ownership of pharmacies.

### 6.3. Results

The results are presented in panel A of Table 5. We find that the separating policy reduced the probability that a physician working in a non-integrated clinic gave a patient drug prescription being by 2.1 percentage points and average drug expenditures conditional on a prescription by 4.9%. Hence, the separating policy reduced drug expenditure fell by 7.1% for this group.[10] We also find that these physicians slightly increased diagnostic tests. Overall, the separating policy reduced total outpatient expenditures by 1.8%. As expected, however, the separating policy had no effect on physicians who worked in clinics that integrated and, as a result, were exempt from the separating policy -- Row (a) + (b).

Our results imply that the moral hazard cost of physicians owning pharmacies is non-trivial. At baseline, mean drugs expenditures at baseline were NT$88.91 for the group of clinics that did not integrate. A 7.1% reduction caused by the separating policy that eliminated the incentives from physician ownership means that costs were reduced by NT$6.31 to NT$82.67, which can be interpreted as the "appropriate" amount of drugs for the treatment of the patient. Moral hazard increased expenditures by NT$6.01 or 7.6%.

[10] We calculate %Δ in expenditures as follows. Note that $E(Exp) = E(Exp|Exp > 0)Prob(Exp > 0)$. Totally differentiating and rearranging terms gives us $(\Delta Exp/Exp) * 100 = (\Delta E(Exp|Exp > 0)/E(Exp|Exp > 0) + \Delta Prob(Exp > 0)/Prob(Exp > 0)) * 100$. The terms $\Delta E(Exp|Exp > 0)/E(Exp|Exp > 0)$ and $\Delta Prob(Exp > 0)$ are estimated coefficients and $Prob(Exp > 0)$ is estimated as the baseline mean for the clinics that did not integrate from Table 3.

## 6.4. Robustness Tests

### 6.4.1. Instrumental variable fixed effects

As a robustness test we also estimate the triple differences model by using instrumental variables to control for the possibility that the integration decision was in part in response to time-varying shocks to specific clinics within villages. Our instrument is **baseline practice volume interacted with the timing of the introduction of the separating policy** in the township as an instrument for integration. The instrument is the empirical analogue of the volume variable in the theoretical model interacted with whether the policy is active.

Recall that the theory predicts the only larger volume physician clinics can cover the fixed costs of having on onsite pharmacist, therefore only the larger volume clinics will choose to integrate. Clinic volume also does not seem to be correlated with the type of patients nor provider treatment patterns. Table 3 shows that baseline patient characteristics and provider treatment choices (drugs and diagnostic tests) are statistically indistinguishable between clinics that did not hire a pharmacist and those that did. We have already shown that the instrument is highly correlated with the probability that the clinic integrates and is also highly correlated with expenditures. Figures 4 and 5 plot the share of visits receiving a prescription and the average drug expenditures per visit over time separately by four baseline practice volume quartiles. Drug prescriptions fall the most post-policy for the smallest group.

Specifically, we re-estimate the triple difference model in equation (8) by instrumental variables. We instrument for treatment times pharmacist with $D_{40}$ (=1 if the baseline practice volume exceeds 40,000 visits per year) and $(1\text{-}D_{40}) \times$ log baseline volume, both interacted with a dummy indicating that the separating policy was active in which the clinic was located.

The results are presented in panel B of Table 5. The estimate of moral hazard in row (a) is almost identical to that which we found using the simple triple difference strategy. Specifically, the separating policy reduced the probability that a physician working in a non-integrated clinic gave a patient drug prescription being by 2.7 percentage points and reduced average drug expenditures per visit by 4.9 percent. This reduced total drug expenditures by 7.7%. We also find almost identical effects on diagnostic test expenditures as well and overall total expenditures.

In contrast to the triple differences results, however, we do find that integration, row (a) + (b), actually increased total expenditures by 2.3% primarily through increased drug expenditures. One reason for this may be that clinics without a pharmacist are not able to dispense certain

types of drugs and must refer to an independent pharmacy. By hiring an onsite pharmacist, they are now able to dispense these types of drugs and hence can exploit this fact for further gain.

### 6.4.2. Placebo Test

We also estimate the triple differences model in equation (3) on the sample of visits to clinics by patients who are exempt from the separating policy i.e. the young and the elderly. Since they are exempt, the introduction of the separating policy should have no effect on drug and diagnostic expenditures for these patients. The results are presented in Panel C of Table 5 and show no effect of the separating policy.

### 6.4.3. Pre-intervention Trends

The key assumptions underlying the causal interpretation in our triple differences model are that (1) the change in the group integrated at baseline is an unbiased estimate of what would have been the change in the treatment group without the separating policy and (2) the change in the group not integrated at baseline and chooses to integrate the policy is implemented is an unbiased estimate of what would have been the change in the that did not integrate without the separating policy. The Heckman-Hotz tests of whether the pre-intervention trends of the treatment group are different from those of the comparison group support the first assumption.

In this section, we report the results of the Heckman-Hotz test for the second assumption. We implement the test by dropping all post-policy observations from our sample and estimate the following model for the group of clinics that are not integrated a baseline:

$$y_{ijkt} = \alpha_j + \gamma_{kt} + \sum_l \delta_l x_{il} + \sum_k \lambda_k T_{jk} Trend_t + \varepsilon_{it} \quad (4)$$

where $T_{jk}$ is a dummy variable indicating whether physician $j$ in municipality $k$ will be in the an integrated group in the post period, $Trend_t$ is a continuous time variable indicating quarter, and all variables are as defined for equation (3). The coefficients of interest are the $\lambda_k$'s on the interaction terms consisting of the municipality, treatment dummy and time trend. Note that we test if the pre-intervention trends are equal for each municipality separately.

We estimate equation (4) for each of the dependent variables. Almost all of the coefficients (108 out 112) on the interaction terms are not significantly different that zero and the F-statistics for the hypothesis that all of the coefficient interaction terms are jointly for each outcome zero cannot be rejected at conventional significance levels for each of the six dependent variables (Appendix Table B).

## 7. Economies of Scope

In this section we examine potential gains from the integration resulting from economies of scope that may improve quality of care. There are two possible routes to improved care. First, integration could improve therapy compliance, i.e. patients following drug prescriptions regimes, as having a pharmacy onsite may lower the costs of filling and refilling prescriptions and thereby increase therapy compliance. Second, integration may improve the coordination of care through better communication between physicians and pharmacists at the prescription phase and through improved patient compliance with pharmacological prescriptions. In fact, there is some evidence to support this hypothesis with respect to the diagnosis and treatment of severe illness (Afendulis and Kessler, 2007).

### 7.1. Therapy Compliance

We examine therapy compliance for two chronic diseases diabetes and hypertension. Both diseases are ambulatory care sensitive conditions for which good primary care and drug adherence can drastically reduce or even eliminate the need for hospitalizations or ER visits. Drug adherence can therefore serve as an important indicator (before more serious complications set in) for patients with these two chronic illnesses.

We measure therapy compliance in term of whether patients actually fill the drugs they are prescribed on time The hypothesis is that patients are more likely to fill drug prescriptions if they are able to do in the clinic at the time of prescription rather than having to fill them later at a pharmacy outside of the clinic. The compliance measure is the share of days per quarter that the patient has medication in her possession for two chronic conditions.

We examine therapy compliance for two major chronic illness Diabetes and Hypertension. Persistent high levels of blood sugar can have immediate short-term effects

that require some form of emergency treatment. Moreover, since most of these patients have sufficiently advanced diabetes or hypertension to warrant treatment, any interruption in treatment is likely to manifest as an adverse event in a relatively short time-horizon such as a one-year period (Ho et al., 2006, Sokol et al., 2005, Wu et al., 2009). Therapy compliance is a major issue for these two diseases as diabetics only have 61% of the drugs they are prescribed and hypertensives only have 45% on average.

We test for economies of scope in therapy compliance using the triple differences model (Table 7). While we find no effects for diabetes, we do find evidence of economies of scope for hypertension. Specifically, we find the policy is associated with a 5.7% reduction in medication compliance for diabetics treated in clinics that did not integrate (row (a)), but that this disappears if the clinic hired an onsite pharmacist (row (a) + (b)).

### 7.2. Health Outcomes

We examine adverse events measured by an avoidable ER visit or hospitalization shortly after the outpatient visits to clinic. . In the medical literature, these adverse events are related to deficient medical treatment (DiMatteo et al., 2002, Lee et al., 2006, Mojtabai and Olfson, 2003, Osterberg and Blaschke, 2005, Simpson et al., 2006, Tabor and Lopez, 2004).

We consider adverse events for three types of prevalent patient diagnoses: (1) acute respiratory infections (ARI), (2) diabetes mellitus (DM) – a chronic condition that is less prevalent but much more serious,[11] and (3) hypertension – the most prevalent form of chronic illness in Taiwan and in the majority of industrialized economies. For ARI, we examine ER utilization and inpatient admissions within 30 days of the office visit. When ARI is not treated it can manifest itself into more serious problems such as pneumonia that could require hospitalization. For diabetic and hypertensive patients, we examine ER utilization and hospitalization rates within 360 days of the visit as medical issues associated with uncontrolled diabetes or hypertension can take time to develop. The results, reported in Table 7, show no significant effects on either adverse event measure for any of the diseases.

[11] Includes all ICD9CM codes of 249 or 250.

### 7.3. Discussion

Since it is virtually costless for physicians to write prescriptions for drugs with low side-effect profiles,[12] the separating policy merely removed the incentive to over-prescribe, but did not create an incentive to under-prescribe medications. Moreover, in contrast to Afendulis and Kessler (2007), who examine a very serious cardiac condition that may benefit from the better coordination of care, we study a far more mundane context, as most outpatient visits to physician offices are for relatively minor illnesses and chronic conditions manageable in ambulatory primary care settings (Appendix Table D). Finally, the drugs most commonly prescribed are palliative rather than curative, such as painkillers, antacids, cough preparations, decongestants, and nonsteroidal anti-inflammatory drugs like Tylenol (Appendix Table E).

This does raise the concern we may be underestimating gains from more palliative care, such as a reduction in the discomfort of ARIs. However, nothing prevents patients who desire palliative care from asking their physicians to prescribe painkillers, cough medication, and decongestants. For these patients, physicians are unlikely to refuse their requests because the writing of the prescription is virtually costless, and because physicians remain incentivized to satisfy patient demand to create repeat business.

The convenience of collecting one's prescriptions before leaving the physician office is one rationale for favoring the integration of pharmacies into clinics. Nevertheless, our study shows adherence to drug therapy among two types of chronically ill patients were not adversely affected by the separating policy. One reason is that independent pharmacies have strong incentives to locate near clinics and often do. Anecdotal evidence also suggests another reason: Gateway pharmacies simply deliver the drugs to the patients at the clinic, dispensing them of a separate trip to pick up their medications.

Thus, at least in the context of physician-owned pharmacies, the two primary reasons for encouraging integration are absent. First, the integration of pharmacy services into physician offices fails to create efficiency savings – indeed, it generates the opposite result, as moral hazard effects dominate. Second, it also does not reduce observable patient health outcomes or overall drug adherence for essential medications.

[12] Indeed, many of the most commonly prescribed drugs at Taiwanese clinics are purchased over-the-counter in the United States, without advice from physicians or even pharmacists.

## 8. Discussion and Policy Implications

A major concern of anyone who consults a doctor, plumber, or auto mechanic to both diagnose and treat a problem in the presence of asymmetric information is the incentive to give advice that is not in the buyer's best interests. Nowhere is this truer than when physicians have ownership interest in ancillary medical services, and is the concern that led the U.S. Congress to pass the Stark Law to prohibit physician referrals of Medicare and Medicaid patients to ancillary care facilities in which the physicians have a financial interest.

In this paper, we investigated an anti-self-referrals policy in Taiwan prohibiting physician from owning pharmacies and dispensing drugs unless they had an onsite pharmacist. We find that the policy reduced drug expenditures by 7.1 percent among physicians that did not have an onsite pharmacist. However, these physicians responded by increasing the overprovision of diagnostic services not covered by the policy, thereby reducing the effectiveness of the policy. In addition, 37% of the physician groups (clinics) already had an onsite pharmacist prior to the policy and therefore were exempt. And 50 percent of the clinics that did not have an onsite physician group prior to the policy, hired onsite pharmacists subsequent to the implementation of the policy. Moreover, clinics that began operation after the separating policy invariably hired a pharmacist. Overall, after integration, the policy only applied to only 8.4 of clinics, and only reduced average discretionary expenditures by less than 1 percent.

Our results show that moral hazard costs of self-referral incentives are significant, that the “safe harbor” exemption severely reduces the effectiveness of the policy, and that the exemption explains much of the recent integration of ancillary services into physician offices. Finally, we also show that the reduction in prescription behavior is not associated with increases in observable adverse health events for patients with three of the most common acute and chronic illnesses. Nor is the reduction in prescription associated with large reductions in drug adherence for patients with diabetes or hypertension.

Our work also informs policy in a number of specific ways. 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 self-

referrals may lead physicians to increase the quantity of a related service, particularly when no limit is placed on the other discretionary service. It appears, however, that the ease of overtreatment of the other discretionary service may have an influence on the rate of substitution between the two services after a fee reduction in one of the services. Finally, the bright-line "in office ancillary services" exception may be misguided when the referring physicians derive profits from the referral of patients to the employee of an integrated practice, especially when there are no efficiency justifications for the joint production of diagnosis and treatment. In other words, providing safe harbor exceptions may ultimately void the very purpose of the original prohibition as physicians exploit these loopholes to continue to engage in profitable self-referrals. This study empirically validates that an "in-office ancillary services" exemption not only facilitates overutilization of medical care, but also has a profound impact on the organization of health care services.

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**Table 1: Baseline (1997) Means for Clinics by Integration Status Prior to Separating Policy Implementation**

| | Not integrated in 1997 | Integrated in 1997 | Difference |
|---|---|---|---|
| Panel A: Office Visit Characteristics | | | |
| Patient is female | 0.57 | 0.55 | 0.02 |
| Patient age | 33.09 | 28.61 | **4.48*** ** |
| Respiratory illness | 0.42 | 0.45 | -0.03 |
| Any drug prescription | 0.98 | 0.98 | 0 |
| Antacids/absorbents prescribed | 0.19 | 0.24 | -0.05 |
| Anti-inflammatory prescribed | 0.15 | 0.19 | -0.04 |
| Analgesics/Antipyretics prescribed | 0.32 | 0.36 | -0.04 |
| Expectorant prescribed | 0.04 | 0.05 | -0.01 |
| Drug expenditures | 88.07 | 92.55 | -4.48 |
| Diagnostic test ordered | 0.03 | 0.03 | 0 |
| Diagnostic test expenditures | 238.76 | 259.24 | -20.48 |
| Physician services expenditures | 244.28 | 239.00 | 5.28 |
| Total expenditures | 377.48 | 352.54 | 24.94 |
| Number of observations | 65,964 | 96,669 | |
| Panel B: Clinic characteristics | | | |
| Monthly practice volume (visits) | 1,591.34 | 2,650.85 | **-1,059.51*** ** |
| Monthly revenues (in 1000 NT$) | 686.80 | 1,085.11 | **-398.31*** ** |
| Number of observations | 3,420 | 1,826 | |

Notes: Integration is defined as having onsite pharmacist prior to the implementation of the separating policy in township in which the clinic is located. Expenditures are reported in 1997 New Taiwanese Dollars. Expenditures for drugs and diagnostic tests are conditional on positive expenditure. Statistical significance levels for tests of the null hypothesis that the difference in means is zero are marked as *** indicating $p < 0.01$ and ** indicating $p < 0.05$. The hypothesis tests are conducted clustering standard errors at the township level.

Table 2: Reduced Form Impact of Separating Policy

| | Any Prescription | Log drug expenditures | Any Diagnostic Test | Log diagnostic expenditures | Log physician services expenditures | Log total expenditures |
|---|---|---|---|---|---|---|
| Panel A: Sample visits to all clinics by non-infant and non-elderly patients | | | | | | |
| Treatment | **-0.014*****<br>(0.002) | **-0.038****<br>(0.018) | **0.003****<br>(0.001) | -0.024<br>(0.022) | 0.000<br>(0.002) | **-0.013*****<br>(0.005) |
| Mean | 0.97 | 96.15 | 0.04 | 255.14 | 254.31 | 377.31 |
| Observations | 871,319 | 641,627 | 871,319 | 35,030 | 871,319 | 673,899 |
| # Physicians | 8,821 | 8,144 | 8,821 | 3,494 | 8,821 | 8,399 |
| Panel B: Sample of visits to clinics not integrated at baseline by non-infant and non-elderly patients | | | | | | |
| Treatment | **-0.012*****<br>(0.002) | -0.024<br>(0.018) | 0.003<br>(0.002) | -0.014<br>(0.024) | -0.004<br>(0.002) | **-0.016*****<br>(0.005) |
| Mean | 0.96 | 95.51 | 0.04 | 247.19 | 266.29 | 388.03 |
| Observations | 428,205 | 308,602 | 428,205 | 18,457 | 428,205 | 329,895 |
| # Physicians | 6,097 | 5,462 | 6,097 | 2,130 | 6,097 | 5,710 |
| Panel C: Falsification test - Sample of visits to all clinics by infants and elderly patients exempt from separating policy | | | | | | |
| Treatment | -0.004<br>(0.003) | -0.002<br>(0.026) | -0.001<br>(0.002) | 0.031<br>(0.086) | -0.001<br>(0.004) | -0.004<br>(0.008) |
| Mean | 0.97 | 91.63 | 0.03 | 225.78 | 275.32 | 394.92 |
| Observations | 135,605 | 97,376 | 135,605 | 4,226 | 135,605 | 101,203 |
| # Physicians | 7,382 | 6,682 | 7,382 | 1,569 | 7,382 | 6,839 |

**Notes:** Treatment is the difference in difference estimate of the effect of the separating policy on the dependent variable listed at the top of the column controlling for physician fixed effects, quarter-year for each county fixed effects, diagnosis fixed effects, and patient age and sex. The standard errors are clustered at the physician level and *** p<0.01, ** p<0.05 for the test of the null hypothesis that the coefficient is different from zero. The sample in Panel A is visits to clinics not integrated at baseline by patients between older than 3 and younger than 60 for diagnoses covered under the separating policy. The sample in Panel B is visits to clinics not integrated at baseline by patients 3 years old and younger and 60 years and older for diagnoses exempted from the separating policy. The sample in Panel B is visits to clinics by patients 3 years old and younger and 60 years and older for diagnoses exempted from the separating policy.

Table 3: Baseline (1997) Means for Clinics Not Integrated at Baseline by Integration Status in 2000 Post Separating Policy Implementation

| | Not integrated in 2000 | Integrated in 2000 | Difference |
|---|---|---|---|
| Panel A: Office Visit Characteristics | | | |
| Patient is female | 0.56 | 0.58 | -0.02 |
| Patient age | 35.06 | 29.65 | **5.40*** ** |
| Respiratory illness | 0.40 | 0.45 | -0.05 |
| Any drug prescription | 0.98 | 0.98 | 0 |
| Antacids/absorbents prescribed | 0.20 | 0.18 | 0.02 |
| Anti-inflammatory prescribed | 0.15 | 0.15 | 0 |
| Analgesics/Antipyretics prescribed | 0.12 | 0.11 | 0.01 |
| Expectorant prescribed | 0.04 | 0.04 | 0 |
| Drug expenditures | 88.91 | 86.46 | 2.45 |
| Diagnostic test ordered | 0.03 | 0.03 | 0.00 |
| Diagnostic test expenditures | 229.06 | 251.09 | -22.03 |
| Physician service expenditures | 248.89 | 236.18 | 12.71 |
| Total expenditures | 384.40 | 364.34 | 20.06 |
| Number of observations | 41,896 | 24,106 | |
| Panel B: Clinic characteristics | | | |
| Monthly practice volume (visits) | 1,265.11 | 2,121.52 | **-856.41*** ** |
| Monthly revenues (in 1000 NT$) | 637.86 | 767.82 | **-129.96** ** |
| Number of observations | 3,420 | 2,128 | 1,298 |

**Notes**: Integration is defined as having onsite pharmacist in 2000 after to the implementation of the separating policy in township in which the clinic is located. Expenditures are reported in 1997 New Taiwanese Dollars. Expenditures for drugs and diagnostic tests are conditional on positive expenditure. Statistical significance levels for tests of the null hypothesis that the difference in means is zero are marked as *** indicating $p < 0.01$ and ** indicating $p < 0.05$. The hypothesis tests are conducted clustering standard errors at the township level.

Table 4: Impact of the Separating Policy on Probability a Clinic Integrates

| | Model (1) | Model (2) |
|---|---|---|
| Treatment | **0.660*** (0.011)** | **0.090** (0.043)** |
| Treat × $D_{25\text{-}50\%tile}$† | | **0.196*** (0.021)** |
| Treatment × $D_{50\text{-}75\%tile}$† | | **0.430*** (0.021)** |
| Treatment × $D_{75\text{-}100\%tile}$† | | **0.887*** (0.020)** |
| Number of observations | 3,916 | 3,916 |
| R-squared | 0.56 | 0.72 |

**Notes:** The coefficient and standard error are the difference in difference estimate of the effect of the separating policy on the probability a clinic integrates controlling for clinic fixed effects and quarter-year effects. The standard errors are clustered at the clinic level. The sample is clinics not integrated at baseline. *** p<0.01, ** p<0.05

Table 5: Impact of Separating Policy on Clinics by Integration Choice

| | Any Prescription | Log drug expenditures | Any diagnostic tests | Log diagnostic expenditures | Log physician expenditures | Log total expenditures |
|---|---|---|---|---|---|---|
| Panel A: Triple Differences | | | | | | |
| (a) Treatment | **-0.021*** ** | **-0.049** ** | **0.005*** ** | -0.020 | -0.000 | **-0.018*** ** |
| | (0.002) | (0.022) | (0.002) | (0.025) | (0.002) | (0.006) |
| (b) Treatment × Pharmacist | **0.019*** ** | 0.030 | -0.004 | -0.010 | 0.000 | **0.014*** ** |
| | (0.003) | (0.018) | (0.002) | (0.024) | (0.002) | (0.005) |
| (a) + (b) | -0.002 | -0.018 | 0.001 | -0.009 | 0.000 | -0.004 |
| | (0.002) | (0.017) | (0.002) | (0.025) | (0.002) | (0.005) |
| Observations | 871,319 | 641,627 | 871,319 | 35,030 | 871,319 | 673,899 |
| Panel B: Instrumental Variable Triple Differences | | | | | | |
| (a) Treatment | **-0.0270*** ** | **-0.049** ** | **0.006*** ** | -0.037 | 0.004 | **-0.0180*** ** |
| | (0.003) | (0.025) | (0.002) | (0.033) | (0.003) | (0.007) |
| (b) Treatment × Pharmacist | **0.033*** ** | **0.106** ** | -0.004 | 0.019 | -0.004 | 0.041*** |
| | (0.005) | (0.046) | (0.004) | (0.058) | (0.006) | (0.013) |
| (a) + (b) | 0.006 | **0.058** ** | 0.002 | -0.179 | -0.001 | **0.023*** ** |
| | (0.003) | (0.032) | (0.003) | (0.040) | (0.004) | (0.009) |
| Observations | 751,004 | 543,114 | 751,004 | 27,836 | 751,004 | 569,665 |
| Panel C: Falsification Tests (Sample of patients exempt from policy) | | | | | | |
| (a) Treatment | **-0.007** ** | -0.002 | -0.003 | 0.094 | -0.002 | -0.009 |
| | (0.003) | (0.029) | (0.003) | (0.099) | (0.004) | (0.000) |
| (b) Treatment × Pharmacist | **0.007** ** | 0.001 | 0.001 | -0.162 | 0.001 | 0.011 |
| | (0.003) | (0.028) | (0.003) | (0.093) | (0.005) | (0.009) |
| (a) + (b) | -0.000 | -0.001 | -0.001 | -0.068 | -0.001 | 0.003 |
| | (0.003) | (0.030) | (0.003) | (0.095) | (0.005) | (0.010) |
| Observations | 135,605 | 97,376 | 135,605 | 4,226 | 135,605 | 101,203 |

**Notes:** Treatment is the triple difference estimate of the effect of the separating policy on the dependent variable listed at the top of the column controlling for physician fixed effects, quarter-year for each county fixed effects, diagnosis fixed effects, and patient age and sex. The standard errors are clustered at the physician level and *** p<0.01, ** p<0.05 for the test of the null hypothesis that the coefficient is different from zero. The sample in Panels A and B is visits to clinics by patients between older than 3 and younger than 60 for diagnoses covered under the separating policy. The sample in Panel B is visits to clinics by patients 3 years old and younger and 60 years and older for diagnoses exempted from the separating policy.

Table 6: Triple Difference Impact Drug Adherence

| | **Diabetes** | **Hypertension** |
|---|---|---|
| (a) Treatment | -0.018 | **-0.026*** ** |
| | (0.018) | (0.009) |
| (b) Treatment × pharmacist | 0.002 | **0.037*** ** |
| | (0.016) | (0.007) |
| (a) + (b) | 0.006 | 0.011 |
| | (0.016) | (0.007) |
| Observations | 86,915 | 155,944 |
| Number of patients | 8,575 | 21,071 |
| Number of Clinics | 2,143 | 6,941 |
| 1997 baseline mean | 0.61 | 0.45 |

Notes: Diabetes mellitus includes all diagnoses with an ICD9CM code of 249 or 250. Hypertension includes all diagnoses with an ICD9CM code of 401-405. CVD (cardiovascular diseases) refers to all diagnoses with an ICD9CM code of 390-459. ** p<0.01, * p<0.05

Table 7: Effects of the Separating Policy on Adverse Health Events

| | *Panel A*: Reduced Form Difference in Differences | | *Panel B*: By Integration Status Triple Differences | |
|---|---|---|---|---|
| | ER visit | Hospitalized | ER visit | Hospitalized |
| Panel A: Patients with Acute Respiratory Infections, 30-day follow-up (N = 261,400) | | | | |
| (a) Treatment | -0.0001<br>(0.0002) | 0.0001<br>(0.0006) | 0.0005<br>(0.0004) | 0.0004<br>(0.0012) |
| (b) Treatment × Pharmacist | | | -0.0008<br>(0.0006) | -0.0001<br>(0.0012) |
| (a) + (b) | | | -0.0003<br>(0.0002) | 0.0003<br>(0.0008) |
| 1997 baseline mean | 0.0001 | 0.003 | 0.00013 | 0.0026 |
| Panel B: Patients with Diabetes Mellitus, 360-day follow-up (N = 14,001) | | | | |
| (a) Treatment | 0.00003<br>(0.00146) | -0.00000<br>(0.022) | 0.0005<br>(0.0016) | 0.0113<br>(0.0301) |
| (a) Treatment | | | -0.0006<br>(0.0016) | -0.0138<br>(0.0311) |
| (a) + (b) | | | -0.0001<br>(0.0016) | -0.0024<br>(0.0242) |
| 1997 baseline mean | 0.00040 | 0.16000 | 0.000408 | 0.16 |
| Panel C: Patients with Hypertension 360-day follow-up (N = 35,895) | | | | |
| (a) Treatment | -0.0017<br>(0.0022) | -0.0063<br>(0.0099) | -0.0014<br>(0.0025) | -0.0217<br>(0.0155) |
| (b) Treatment × Pharmacist | | | -0.0005<br>(0.0017) | 0.0177<br>(0.0153) |
| (a) + (b) | | | -0.0018<br>(0.0023) | -0.0041<br>(0.0108) |
| 1997 baseline mean | 0.0024 | 0.1480 | 0.0024 | 0.1504 |

Notes: Acute Respiratory Infections include all diagnoses with an ICD9CM code of 460-478. Pneumonia refers to both bacterial and viral pneumonia, or diagnoses with an ICD9CM code of 480-486.Diabetes mellitus includes all diagnoses with an ICD9CM code of 249 or 250. Hypertension includes all diagnoses with an ICD9CM code of 401-405. CVD (cardiovascular diseases) refers to all diagnoses with an ICD9CM code of 390-459. ** p<0.01, * p<0.05

**Figure 1**

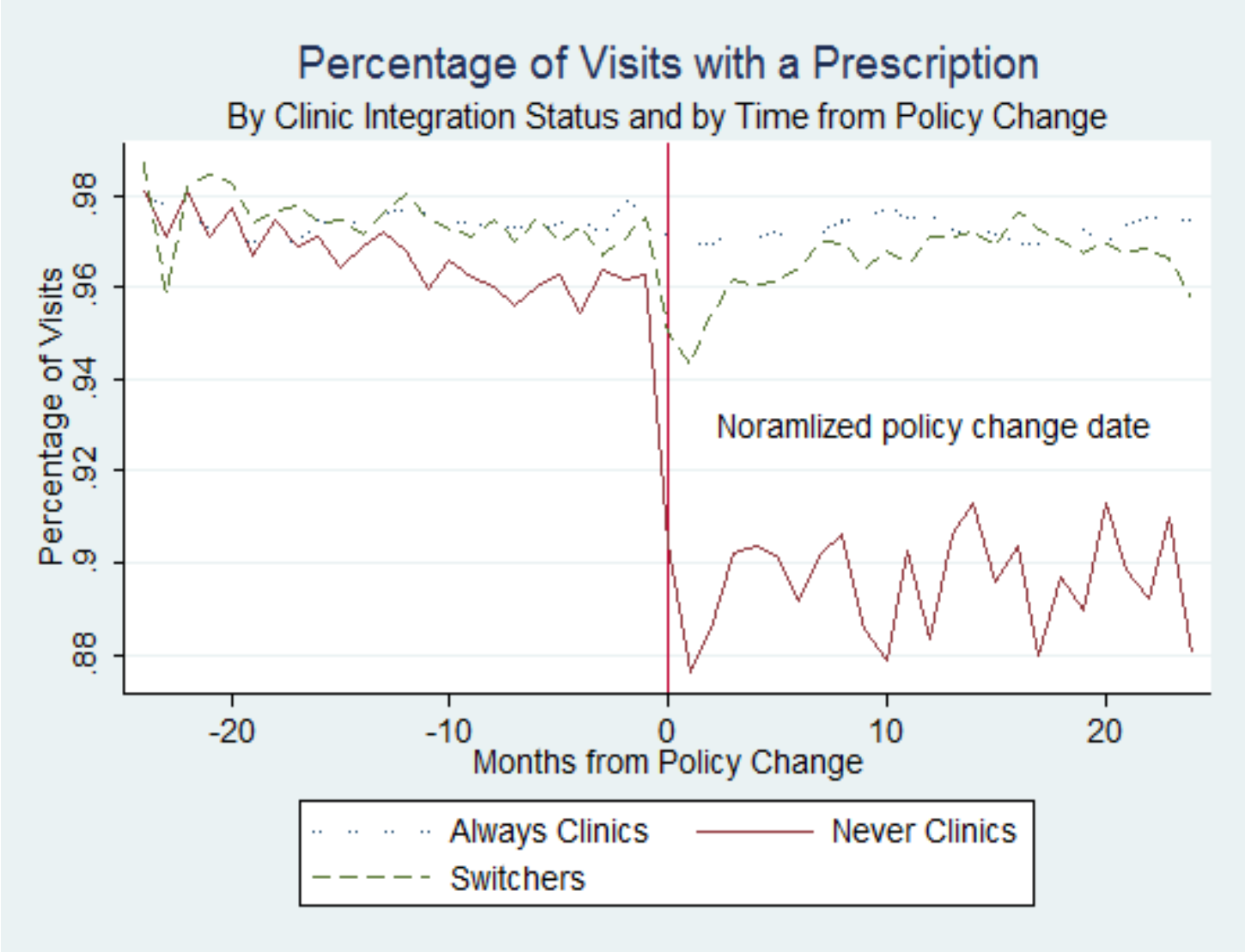

Percentage of Visits with a Prescription
By Clinic Integration Status and by Time from Policy Change
Percentage of Visits
.88
.9
.92
.94
.96
.98
Noramlized policy change date
-20
-10
0
10
20
Months from Policy Change
Always Clinics
Never Clinics
Switchers

**Figure 2**

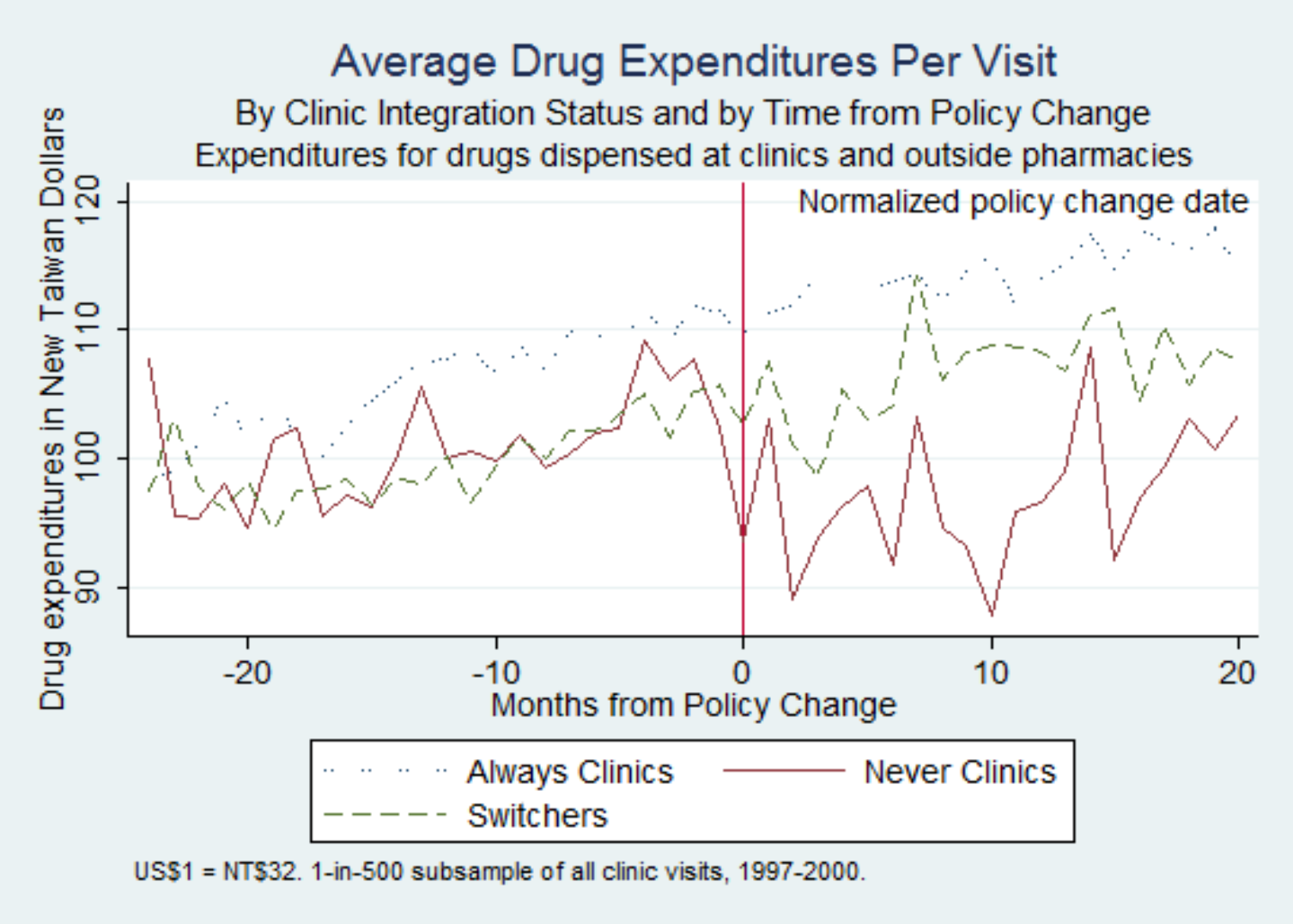

Average Drug Expenditures Per Visit
By Clinic Integration Status and by Time from Policy Change
Expenditures for drugs dispensed at clinics and outside pharmacies
Drug expenditures in New Taiwan Dollars
90
100
110
120
Normalized policy change date
-20
-10
0
10
20
Months from Policy Change
Always Clinics
Never Clinics
Switchers
US$1 = NT$32. 1-in-500 subsample of all clinic visits, 1997-2000.

**Figure 3**

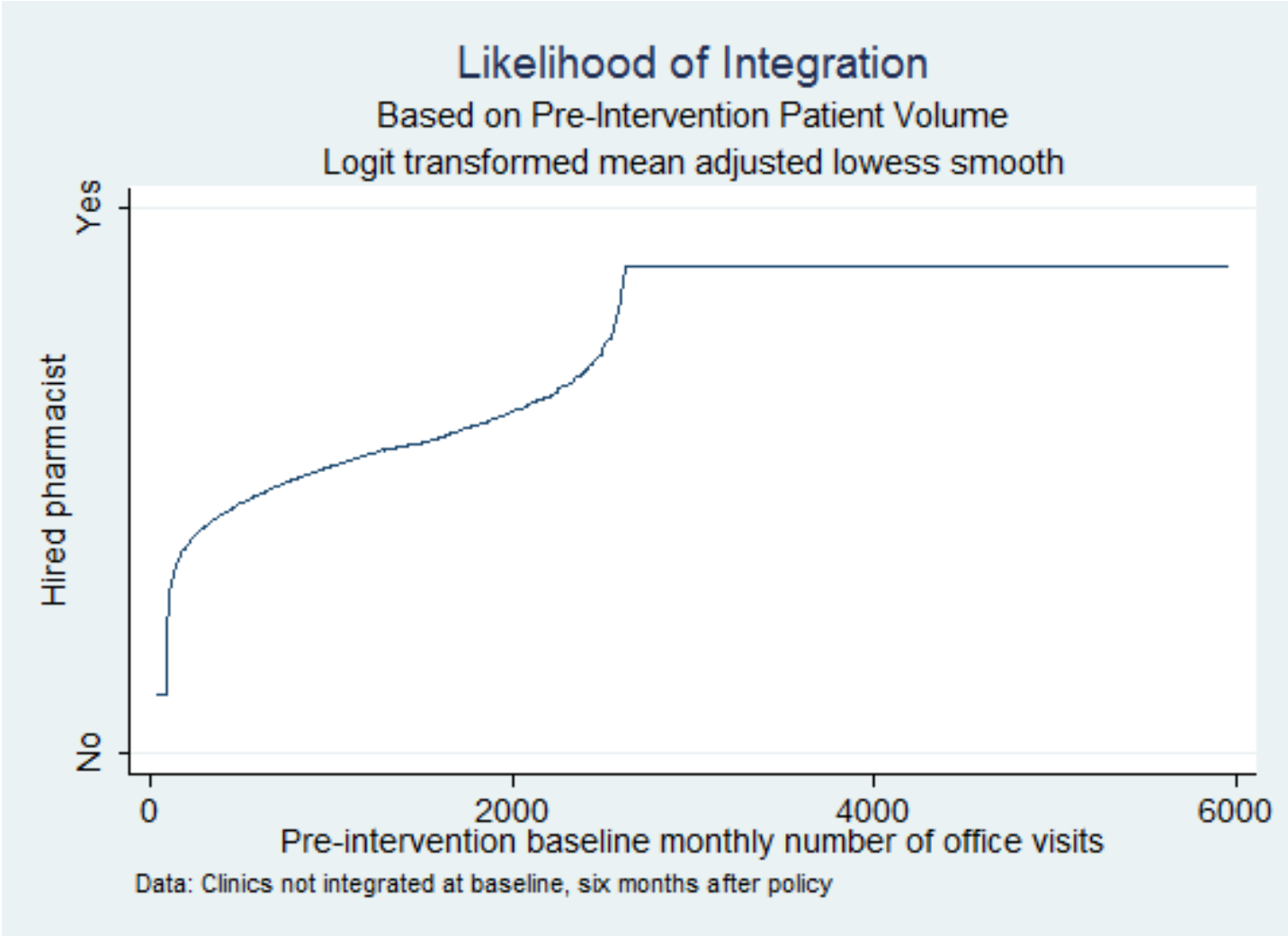

Likelihood of Integration
Based on Pre-Intervention Patient Volume
Logit transformed mean adjusted lowess smooth
Hired pharmacist
Yes
No
0
2000
4000
6000
Pre-intervention baseline monthly number of office visits
Data: Clinics not integrated at baseline, six months after policy

**Figure 4**

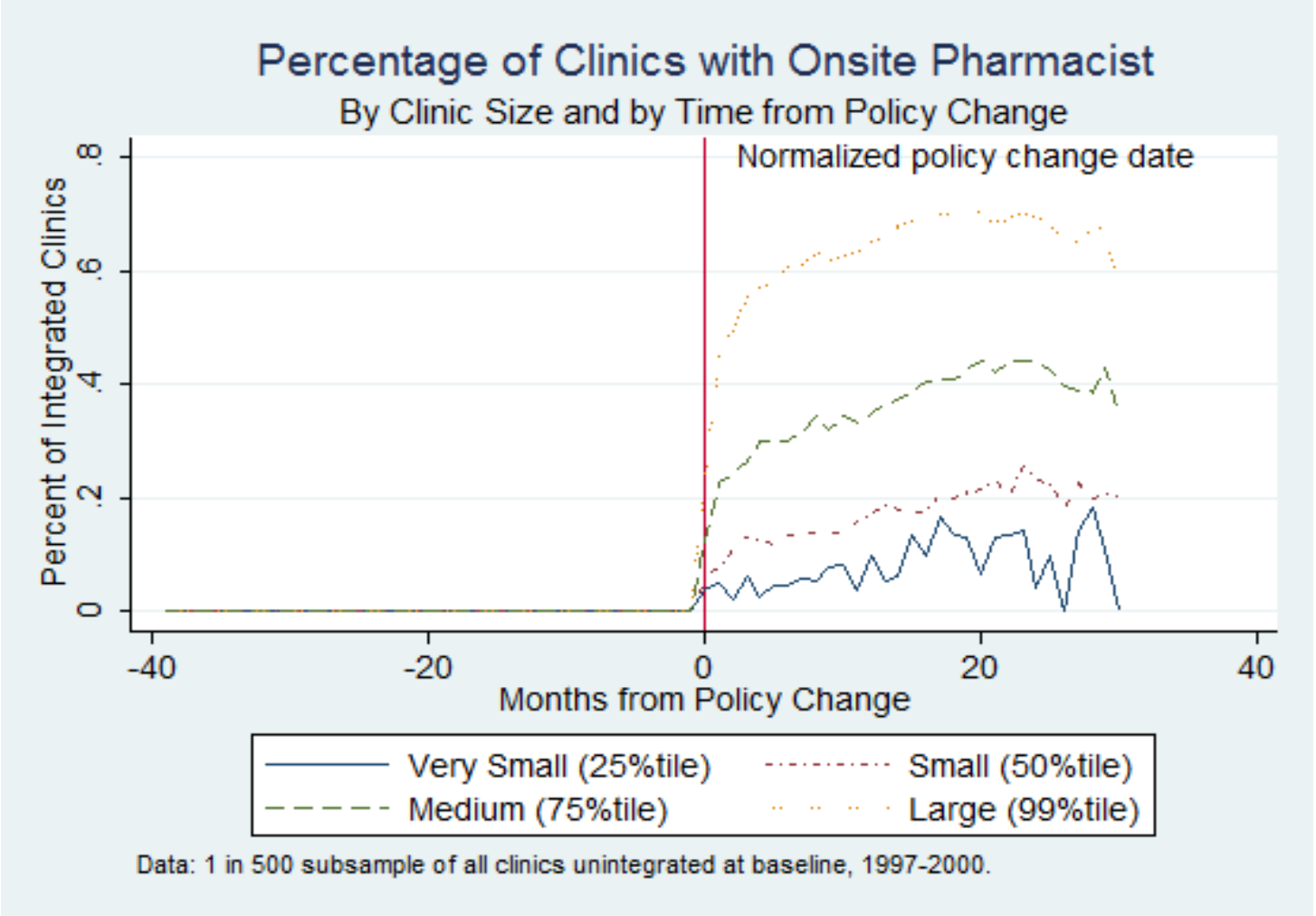

Percentage of Clinics with Onsite Pharmacist
By Clinic Size and by Time from Policy Change
Normalized policy change date
Percent of Integrated Clinics
0
.2
.4
.6
.8
-40
-20
0
20
40
Months from Policy Change
Very Small (25%tile)
Small (50%tile)
Medium (75%tile)
Large (99%tile)
Data: 1 in 500 subsample of all clinics unintegrated at baseline, 1997-2000.

**Figure 5**

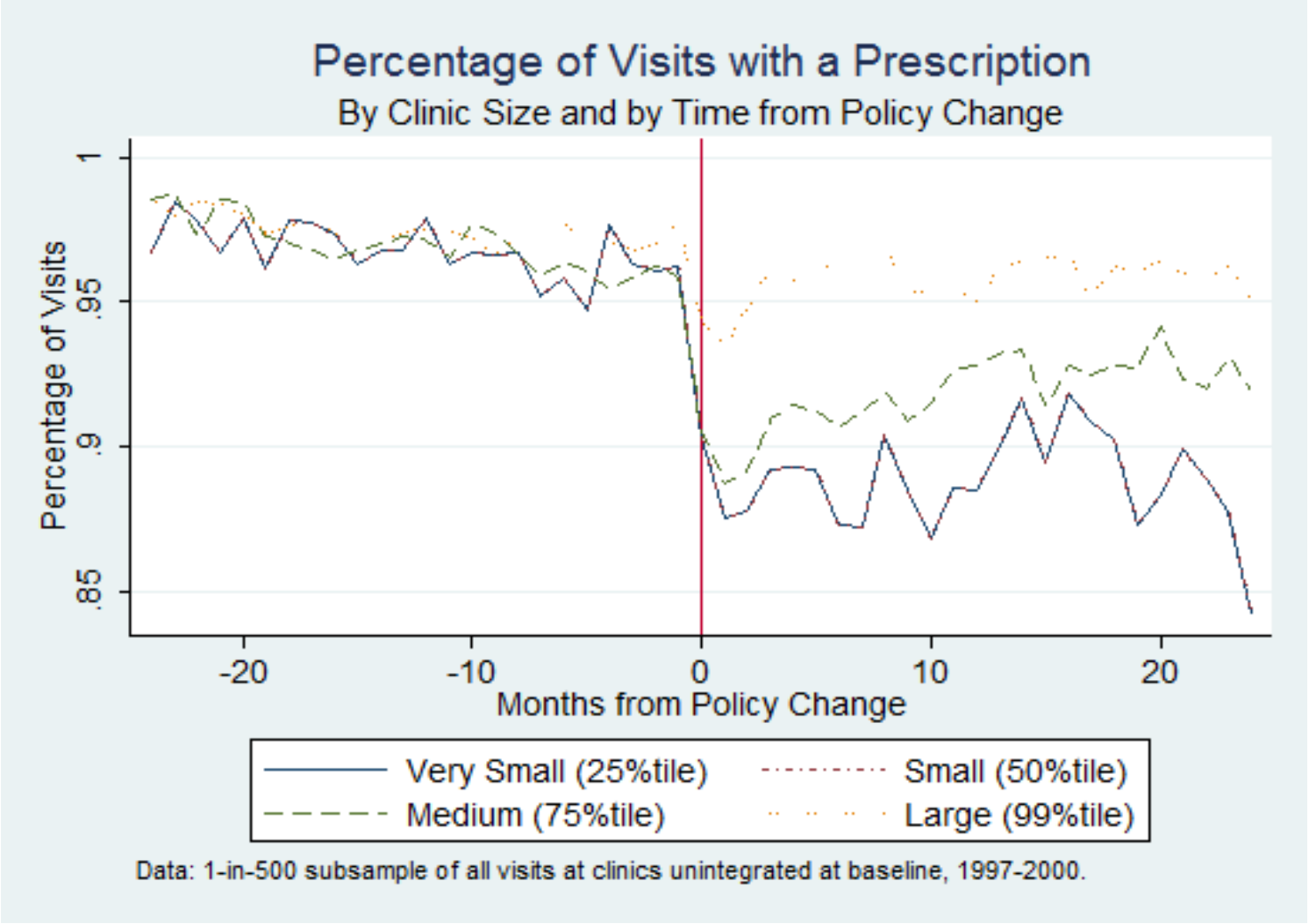
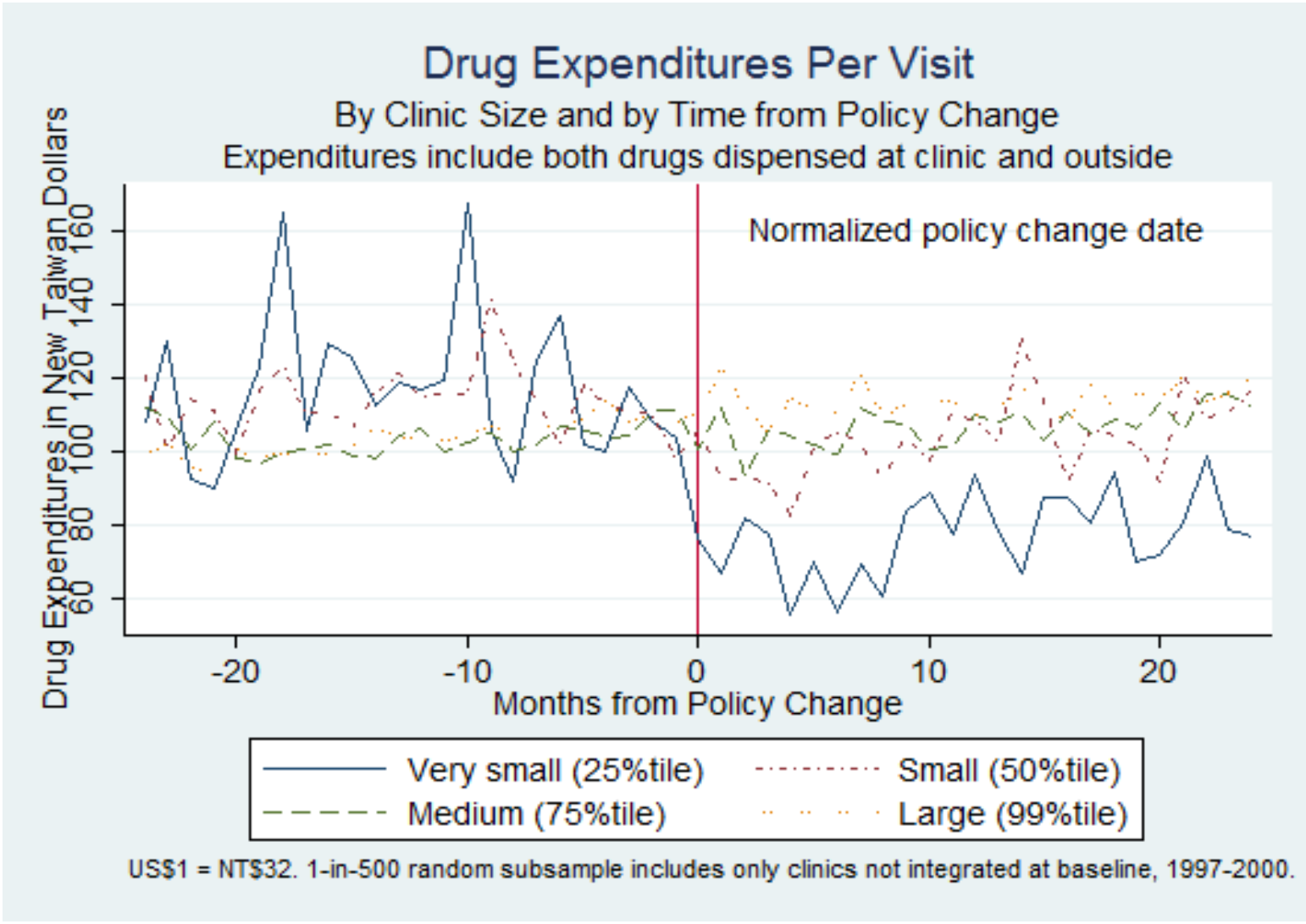

Percentage of Visits with a Prescription
By Clinic Size and by Time from Policy Change
Percentage of Visits
.85
.9
.95
1
-20
-10
0
10
20
Months from Policy Change
Very Small (25%tile)
Small (50%tile)
Medium (75%tile)
Large (99%tile)
Data: 1-in-500 subsample of all visits at clinics unintegrated at baseline, 1997-2000.

**Figure 6**


Drug Expenditures Per Visit
By Clinic Size and by Time from Policy Change
Expenditures include both drugs dispensed at clinic and outside
Drug Expenditures in New Taiwan Dollars
60
80
100
120
140
160
Normalized policy change date
-20
-10
0
10
20
Months from Policy Change
Very small (25%tile)
Small (50%tile)
Medium (75%tile)
Large (99%tile)
US$1 = NT$32. 1-in-500 random subsample includes only clinics not integrated at baseline, 1997-2000.

## APPENDIX TABLES

**Appendix Table A1: P-Values for tests equality of pre-intervention trends for treatment & comparison groups**

| | Prescription | Log drug expenditures | Diagnostic tests | Log diagnostic expenditures | Log total expenditures |
|---|---|---|---|---|---|
| Trend × treatment × municipality 1 | 0.017 | 0.239 | 0.149 | -0.028 | 0.186 |
| | (0.016) | (0.193) | (0.091) | (0.018) | (0.157) |
| Trend × treatment × municipality 2 | -0.079 | -0.016 | -0.153 | 0.493 | -0.477 |
| | (0.067) | (0.164) | (0.108) | (0.500) | (0.297) |
| Trend × treatment × municipality 3 | -0.001 | -0.001 | 0.013 | -0.025 | 0.001 |
| | (0.001) | (0.006) | (0.008) | (0.040) | (0.008) |
| Trend × treatment × municipality 4 | 0.000 | 0.005 | 0.0013 | -0.020 | 0.000 |
| | (0.0017) | (0.004) | (0.0026) | (0.022) | (0.005) |
| Trend × treatment × municipality 5 | -0.003* | 0.005 | -0.003 | -0.014 | -0.000 |
| | (0.001) | (0.003) | (0.003) | (0.021) | (0.004) |
| Trend × treatment× municipality 6 | 0.000 | 0.001 | -0.003 | -0.008 | -0.001 |
| | (0.000) | (0.002) | (0.003) | (0.018) | (0.003) |
| Trend × treatment × municipality 7 | -0.001 | **0.0150*** | 0.007 | 0.002 | 0.015 |
| | (0.002) | **(0.007)** | (0.007) | (0.041) | (0.009) |
| Trend × treatment × municipality 8 | 0.000 | 0.000 | 0.002 | 0.000 | -0.001 |
| | (0.000) | (0.002) | (0.003) | (0.015) | (0.004) |
| Trend × treatment × municipality 9 | -0.000 | -0.002 | 0.000 | -0.017 | -0.005 |
| | (0.001) | (0.002) | (0.002) | (0.015) | (0.003) |
| Trend × treatment × municipality 10 | **-0.003*** | -0.0251 | 0.008 | **0.090*** | -0.010 |
| | **(0.001)** | (0.0155) | (0.007) | **(0.038)** | (0.017) |
| Trend × treatment × municipality 11 | 0.001 | -0.003 | -0.000 | -0.018 | **-0.011*** |
| | (0.001) | (0.004) | (0.004) | (0.023) | **(0.004)** |
| Trend × treatment × municipality 12 | -0.001 | 0.002 | -0.003 | 0.016 | 0.000 |
| | (0.001) | (0.003) | (0.003) | (0.021) | (0.004) |
| Trend × treatment× municipality 13 | 0.000 | -0.000 | -0.003 | -0.020 | -0.002 |
| | (0.001) | (0.005) | (0.005) | (0.033) | (0.008) |
| Trend × treatment × municipality 14 | 0.001 | 0.006 | 0.008 | -0.005 | 0.008 |
| | (0.003) | (0.007) | (0.005) | (0.050) | (0.008) |
| Trend × treatment × municipality 15 | 0.001 | -0.023 | 0.013 | -0.027 | -0.020 |
| | (0.003) | (0.013) | (0.010) | (0.086) | (0.015) |
| Trend × treatment × municipality 16 | -0.002 | -0.002 | -0.009 | -0.026 | -0.015 |
| | (0.002) | (0.008) | (0.009) | (0.048) | (0.010) |
| Trend × treatment × municipality 17 | 0.000 | 0.001 | **-0.007*** | -0.009 | 0.000 |
| | (0.001) | (0.003) | **(0.004)** | (0.015) | (0.003) |
| Trend × treatment × municipality 18 | 0.000 | **-0.010*** | -0.004 | 0.022 | -0.006 |
| | (0.001) | **(0.005)** | (0.005) | (0.031) | (0.006) |
| Observations | 265,070 | 257,341 | 265,070 | 60,492 | 257,376 |
| R-squared | 0.29 | 0.32 | 0.12 | 0.23 | 0.15 |
| F statistic for joint significance | 1.00 | 1.52 | 1.29 | 0.84 | 1.22 |
| Prob > F (p-value) | 0.45 | 0.07 | 0.18 | 0.66 | 0.23 |

**Notes:** The coefficients and standard error are from regressions on the dependent variable listed at the top of the column controlling for physician fixed effects, quarter-year for each county fixed effects, diagnosis fixed effects, and patient age and sex. The sample is visits to clinics prior to the implementation of the separating policy in the county in which the clinic is located. Treatment is a dummy variable indicating the clinic will be treated in the post implementation period. The standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1

**Appendix Table A2: P-Values for test of equality of pre-intervention trends for clinics that hire and do not hire an onsite pharmacist in the post intervention period**

| | Prescription | Log drug expenditures | Diagnostic tests | Log Diagnostic expenditures | Log total expenditures |
|---|---|---|---|---|---|
| Pharmacist × 1997 quarter2 | -0.00421 | 0.000551 | -0.00202 | 0.0484 | 0.00601 |
| | (0.00297) | (0.00694) | (0.00747) | (0.0518) | (0.00827) |
| Pharmacist × 1997 quarter 3 | 0.000539 | 0.0127 | 9.60e-05 | -0.00716 | 0.0138 |
| | (0.00332) | (0.00758) | (0.00866) | (0.0518) | (0.00894) |
| Pharmacist × 1997 quarter 4 | 0.000207 | 0.000342 | -0.00261 | 0.0421 | 0.00270 |
| | (0.00314) | (0.00788) | (0.00839) | (0.0541) | (0.00904) |
| Pharmacist × 1998 quarter 1 | 0.00139 | 0.00798 | -0.00478 | -0.00187 | 0.00673 |
| | (0.00432) | (0.00887) | (0.00939) | (0.0571) | (0.0100) |
| Pharmacist × 1998 quarter 2 | 0.00721 | 0.00444 | -0.0112 | -0.0296 | 0.000477 |
| | (0.00419) | (0.0102) | (0.0112) | (0.0742) | (0.0123) |
| Pharmacist × 1998 quarter 3 | -0.00520 | 0.00641 | -0.0164 | **0.194*** | 0.0150 |
| | (0.00572) | (0.0123) | (0.0128) | **(0.0822)** | (0.0158) |
| Pharmacist × 1998 quarter 4 | -0.00325 | -0.00633 | -0.00460 | 0.0424 | 0.0118 |
| | (0.00569) | (0.0165) | (0.0157) | (0.104) | (0.0190) |
| Pharmacist × 1999 quarter 1 | 0.00115 | 0.0223 | 0.0226 | 0.0457 | 0.0184 |
| | (0.00696) | (0.0237) | (0.0228) | (0.133) | (0.0296) |
| Pharmacist × 1999 quarter 2 | -0.00792 | **-0.0625*** | 0.00145 | 0.148 | -0.0305 |
| | (0.0134) | **(0.0308)** | (0.0480) | (0.263) | (0.0453) |
| Pharmacist × 1999 quarter 3 | -0.0134 | -0.0772 | 0.0112 | 0.504 | -0.0221 |
| | (0.00997) | (0.0590) | (0.0721) | (0.279) | (0.0757) |
| Pharmacist × 1999 quarter 4 | -0.00284 | -0.0157 | -0.0492 | 0.184 | 0.0314 |
| | (0.00872) | (0.0456) | (0.0762) | (0.192) | (0.0664) |
| Pharmacist × 2000 quarter 1 | -0.00312 | -0.0557 | -0.0744 | 0.367 | -0.135 |
| | (0.00911) | (0.0552) | (0.0781) | (0.277) | (0.0713) |
| Observations | 106,876 | 103,325 | 106,876 | 25,180 | 103,357 |
| Number of physicians | 4,297 | 4,256 | 4,297 | 2,418 | 4,257 |
| F statistic for joint significance | 1.03 | 1.47 | 1.11 | 1.58 | 1.62 |
| Prob > F (p-value) | 0.41 | 0.12 | 0.35 | 0.09 | 0.07 |

**Notes:** The coefficients and standard error are from regressions on the dependent variable listed at the top of the column controlling for physician fixed effects, quarter-year fixed effects, diagnosis fixed effects, and patient age and sex. The sample is visits to clinics that did not have an onsite pharmacist prior to the implementation of the separating policy in the county in which the clinic is located in periods prior to the implementation of the policy. Pharmacist is a dummy variable indicating the clinic hired an onsite pharmacist in the post implementation period. The standard errors are clustered at the physician level. *** p<0.01, ** p<0.05, * p<0.1