THE UNIVERSITY OF CHICAGO

ESSAYS IN HEALTH ECONOMICS

A DISSERTATION SUBMITTED TO

THE FACULTY OF THE DIVISION OF THE SOCIAL SCIENCES

IN CANDIDACY FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

KENNETH C. GRIFFIN DEPARTMENT OF ECONOMICS

BY

GRANT W. GANNAWAY

CHICAGO, ILLINOIS

JUNE 2020

Dedication Text

To my wife Jenny and my children Jackson, Spencer, Eliza, and Mason. They gave me both a reason to work and a meaningful escape that put all my work in perspective. No success can compensate for failure in the family.

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ACKNOWLEDGMENTS

I would not have been able to complete this research without the extensive help of my committee Neale Mahoney, Jens Ludwig, Michael Greenstone, and David Meltzer. I also acknowledge the many other faculty members and mentors that pushed me and helped all along the way including Joe Price, my mentor since my days as an undergraduate student.

ABSTRACT

With health care spending having increased roughly 35% from 2010 to 2017, now consuming over $3 trillion per year in the US alone, there is growing interest in reducing costs without compromising health outcomes. Since a large share of health care costs come from labor, one approach many states have taken is to expand the set of medical providers, shifting from just medical doctors (MDs) to allow for mid-level providers (MLPs), such as nurse practitioners, as well. Because MLP salaries are so much lower than MDs on average, the hope is to capitalize on their potential comparative advantage in providing routine care to low-risk patients. But there is also the possibility that average care quality declines because of the more limited training of MLPs relative to MDs, and/or the possibility that MLP caseloads wind up including non-routine cases or high-risk patients, which could create health complications and hence increase costs in the longer term. In the first chapter, I study the effects of MLP use on costs and patient outcomes using state law changes as a natural experiment, which provides difference-in-difference type variation. This identification strategy is limited in the aggregate due to weak instrument bias. However, using modern machine learning methods, I am able to narrow in on the subgroup of patients where the first stage is sufficiently strong to produce accurate results in the second stage. These methods are data intensive, but in health care (and increasingly throughout social sciences) large enough data is becoming common, allowing researchers to increasingly capitalize on such methods and more effectively estimate heterogeneous treatment effects. I find that the patients who are most likely to be affected by the policy changes have increased rates of both preventable hospitalizations and total medical spending – that is, increased use of MLPs on net has adverse effects for the most relevant sample of patients. Estimates for heterogeneous treatment effects in both the first and second stage equations for my instrumental variables analysis helps explain why: I show that the patients who are predicted to benefit most from MLP care are not the same patients predicted to shift to MLPs after the policy changes, suggesting that improved sorting of patients between provider types could fully exploit comparative advantages and result in improved patient outcomes

overall.

Poor medication adherence is also responsible for large health care costs. In the second chapter, I examine the extent to which medication adherence is influenced by pharmacy access. I use straightforward intent-to-treat measures of adherence in an event-study approach around two types of events: local pharmacy openings and closings, and network status variation of a major pharmacy chain in and out of the network of a major pharmacy benefits management (PBM) insurance company. I find that pharmacy openings cause roughly a 2 percent increase in local patients' measures of adherence, while removing local pharmacies from the PBM network causes a roughly 5 percent decrease.

CHAPTER 1

COMPARATIVE ADVANTAGES IN HEALTH CARE DELIVERY: A MACHINE LEARNING APPROACH

1.1 Introduction

Each year over $3 trillion is spent on health care in the US, of which over 40% ($1.32 trillion) is devoted to outpatient care. Though appropriate spending on outpatient care is often an efficient use of health care delivery, there is nonetheless widespread consensus that the value of the care provided is substantially less than the costs, in part because of exceptionally high health care labor costs in the US (Papanicolas et al. 2018). There is widespread interest in reducing health care costs without reducing quality, and, given the high labor costs, one popular proposal is to shift health care tasks from medical doctors (MDs) to mid-level providers (MLPs) such as nurse practitioners. Between 2004 and 2015, 11 states relaxed their scope of practice laws, removing restrictions on the medical procedures that MLPs could perform by reducing MD supervision requirements. In this paper, I examine the degree to which this shift in medical care affected costs and quality of care.

How this shift will affect costs and quality is the topic of ongoing policy debate and is not immediately clear as a conceptual matter. On the one hand, proponents of these law changes claim that relaxing restrictions on MLPs will reduce medical spending without reducing quality of care[1] (Adams and Markowitz 2018; Gilman and Koslov 2014; Xue and Intrator 2016). However, opponents (such as the American Medical Association) argue that it is possible that increased MLP use will instead

1. A key supporting point for this claim is that MLPs are significantly cheaper to train and employ than MDs - in 2018 the median nurse practitioner salary was roughly half of the median general practice physician salary (see BLS Occupation Employment Statistics May 2018 for generalist physicians and nurse practitioners).

lead to *increased* costs and/or *decreased* quality[2] (Iglehart 2013).

In this paper, I analyze the effects of MLP use on patient health and spending outcomes in a large private insurance medical claims database. To do so, I leverage changes in state laws regulating the use of MLPs as a natural experiment, which provides difference-in-difference-type variation. This approach is limited due to weak instrument bias - there are tens of millions of types of medical encounters that are simply not "at risk" of being shifted from MDs to NDs. To overcome this issue, a reasonable approach might be to rely on institutional knowledge and theory to develop a model of which types of tasks are most likely to be shifted to MLPs after the law changes. Such an approach has tradeoffs: a model may provide potentially interesting counterfactual predictions regarding the effects of shifting different sets of tasks to NDs, but requires relatively strong assumptions regarding the task allocation decisions. Furthermore, in this setting, institutional knowledge is limited- there are many types of tasks about which a model would not be able to make informed predictions about the propensity for the task to shift to NDs. Another approach is to rely on recently developed machine learning methods in combination with very rich data in order to make empirical predictions about which types of encounters are likely to shift to NDs.

Similar to the more traditional modeling approach, the machine learning approach has tradeoffs as well. While machine learning does not require strong assumptions or institutional knowledge, it is not as transparent as a traditional economic model and

2. This is possible, for example, if MLPs order more tests, refer to specialists more often, and/or mis-diagnose patients more frequently than MDs. Such outcomes may occur as a form of risk aversion by NDs, or if patients or insurers inaccurately predict patient risk or complexity and sort overly-complex patients to NDs.

makes predictions on complex interactions of many different observable features of each encounter. Given the ambiguity of theoretical predictions based on institutional knowledge, and the availability of large data, in this paper I opt for the machine learning approach. Doing so yields predicted first stage effects - the predicted change in the probability of MLP use - for each encounter in the data. I use these predicted effects to remove tens of millions of encounters that have low probability of shifting to NDs, and focus instead on types of encounters with high first stage signal. This is similar in spirit to the approach used in Einav et al. (2018). These methods are data intensive, but the large health insurance claims data I use allows for such an approach. Increasingly, large enough data is becoming more widespread in the social sciences so that researchers will be able to capitalize on the benefits of such data intensive methods.

Interestingly, conclusions based on this high first stage sample are significantly different from those based on the overall sample. In the overall sample, I find an exogenous increase in MLP use, but no significant changes in preventable hospitalizations or spending. In the set of patients most likely to be shifted to MLPs on the other hand, I find statistically significant increases in 1-year preventable hospitalization rates. Instrumental variables (IV) estimates suggest MLPs cause a roughly 0.5 percentage point increase in 1-year preventable hospitalizations in this group, up from a baseline level of 0.2 percent of encounters that were followed by a preventable hospitalization prior to the law changes. The effect on total spending among these patients is imprecise, with a positive point estimate.

To understand the mechanisms behind these results, I estimate the reduced form

effects of the law change on potential drivers of the increased hospitalization. I find that the high first stage sample has a decrease in medication adherence and no increases in prescription fill rates or "diversity" of prescription types[3], relative to the overall sample, which had no decrease in medication adherence and increases in prescription fill rates and diversity. Furthermore, the overall sample saw increases in outpatient and prescription spending, while the high first stage sample instead had increases in inpatient spending.

I seek to understand whether the observed increases in the outcome variables could be mitigated had the sorting of patients between provider types been adjusted. To study this question, I utilize the predictions on the effect of shifting to an MLP (mentioned above) on the *full sample*, instead of just the high first stage group. I combine these predictions with encounter-specific machine learning predictions of the *IV effect* of MLP use on patient outcomes. This is analagous to comparing heterogeneous first stage estimates to IV estimates in a traditional analysis. The benefit of this exercise is that I estimate, *for each type of encounter*, the probability of being shifted to MLPs as well as the predicted changes in outcomes from MLP use. Comparing the two predictions gives an estimate for the efficiency of the sorting of encounters between provider types and provides an answer to the question about whether the types of encounters that *are* shifted to MLPs are the same types of encounters that *should be* shifted to NDs.

The sorting analysis reveals that the sorting of encounters between provider types is less than ideal. Considering only the predicted change in spending from MLP use,

3. Prescription diversity here refers to the number of unique types of prescription therapeutic classes a patient uses over the following year.

actual sorting is almost exactly the opposite of a sorting aimed solely at reducing spending. There are relatively few types of encounters that are both strongly predicted to shift to MLPs *and* predicted to reduce spending as a result of MLP use. Instead, there are many types of encounters that are *not* predicted to shift to MLPs but *are* predicted to *reduce* spending as a result of MLP use. When considering the effect of MLP use on preventable hospitalizations, the results still leave room for improvement: I find a large cluster of types of encounters that are predicted *not* to be shifted to MLPs and are also strongly predicted to *increase* hospitalizations as a result of MLP use. However, the largest cluster of types of encounters are predicted to decrease hospitalizations as a result of MLP use, but are only *moderately* likely to be shifted to NDs. As with spending, there are relatively few types of encounters that are strongly predicted to shift to MLPs as well as decrease hospitalizations as a result of MLP use.

Taking the results together suggests that even though relaxing restrictions on MLPs does not show adverse outcomes in the *overall* set of encounters, this aggregate effect masks substantial heterogeneity. When the heterogeneity is uncovered and exploited, MLPs are shown to statistically significantly increase patient preventable hospitalizations on the group of patients most affected by the law changes (with imprecise estimates of increased spending). These effects appear to be driven by the types of encounters sorted to MLPs after the law changes, as well as the MLP effects on prescription drugs and the substitution of inpatient for outpatient care. By improving sorting of encounters between provider types, comparative advantage could be fully exploited and patient outcomes could be improved.

This paper contributes to the larger health economics literature that tries to understand ways of reducing low-value care without compromising patient health, in this particular case by allocating tasks between provider types to better take advantage of provider specialization. Other papers have studied the effects of state scope of practice law changes and MLP use, but my application of modern statistical methods allows me to answer this question beyond the current literature since I am able to narrow in on the most relevant set of patients as defined by an extensive multi-dimensional set of observable factors. I also contribute to the machine learning for heterogeneous treatment effects literature by providing a blueprint for applying recently developed machine learning methods for estimating heterogeneous treatment effects in randomized experiments to natural experimental settings, and in cases with small observed first stage effects.

The rest of the paper proceeds as follows. In section 1.2 I first explain the background of the outpatient medical setting and the scope of practice laws. Then I explain my contribution to the literature on relaxing scope of practice laws and the effects of NDs, as well as to the literature on machine learning for heterogeneous treatment effects. In section 2.3, I give details on the claims data I use in this paper. Section 1.4 explains the traditional econometric methods and gives their results in the overall sample. In section 1.5, I outline the machine learning methods I use, and in section 1.6 I show the results from these methods in the high first stage sample. Section 2.6 concludes.

1.2 Background Setting

To become an MLP, the most common course is for registered nurses to obtain masters or PhD degrees. These degrees are usually received after completing a two or three year program. The programs do not attempt to cover everything taught in medical school, but instead focus on mastering a subset of practical medical skills. To practice as an MLP, candidates must obtain licenses from the state in which they intend to practice, and must abide by state scope of practice (SOP) laws.

State SOP laws are generally determined by state medical licensing boards in conjunction with state legislatures. These laws primarily regulate the level of physician supervision required for MLP practice. There is substantial heterogeneity in the restrictiveness of state laws, with some states allowing MLPs to both provide medical treatment and prescribe medication without physician supervision, while other states require MLPs to obtain physician approval for both treatment and prescription decisions. A primary mechanism for enforcement is malpractice litigation: in restrictive states, MDs bear the primary responsibility for malpractice. Thus, insurance companies are less likely to cover MLP care in restrictive states.

There has been a recent trend toward relaxing SOP laws, with at least 15 states making some changes to their SOP laws since 2000. The specific timing of changes for each state are shown in table 1.1, which I compiled based on records of state legislatures and state medical boards. In 2002, the Federal Trade Commission held hearings on the level of competition between different medical provider types in the outpatient setting, which likely pressured states into relaxing laws. The major barrier to relaxing SOP laws are powerful state lobbying groups working to protect

physician monopoly power, combined with substantial physician representation on state medical boards.

Table 1.1: State Law Changes

Change DURING Data	Year of Change	Change BEFORE Data	Change AFTER Data	
CO	2010	AK	AL	MO
CT	2014	AZ	AR	MS
HI	2011	DC	CA	NC
ID	2004	IA	DE	NY
MD*	2010	ME	FL	OH
MN	2015	MT	GA	OK
ND	2011	NH	IL	PA
NE	2015	NJ	IN	SC
NV	2013	NM	KS	SD
RI	2008	OR	KY	TN
VT	2011	UT	LA	TX
WY	2005	WA	MA	VA
		WV	MI	WI

Notes: Year of change years represent the years the states changed from supervized or collaboratory agreements between MLPs and MDs to independence of practice for NDs. * I exclude Maryland from the estimation sample due to ambiguity in the law change date.

Facilities vary widely in their approach to sorting of tasks between provider types. Large academic hospitals may never sort any tasks to MLPs alone, while small rural practices or clinics may rely on MLPs for a majority of tasks. In this paper I focus only on outpatient settings. Since MDs can generally bill at a higher rate, if an MLP *can* bill at the MD rate, I assume they do. This biases my results in the sense that any observed MLP encounter is strictly an MLP encounter, while MD encounters may be a mix of MDs and NDs. This makes the MD effects look similar to the MLP effects, biasing any differences I find in the reduced form outcome effects toward zero.

For the first stage effects, I argue that the observed increase in the share of MLP encounters is driven by an actual change in which provider type provides the care,

and not just a change in billing labels. I support this argument with two main points. First, I observe a reduced form outcome effect at the time of the SOP law changes in the high first stage sample. If care practice remained unchanged and billing labels only changed, there should have been no change in any outcome variables at the time of the SOP law change. However, if care practice *did* change, the changes in outcome variables are justified. Second, the incentive for MLPs to bill as "incident to" MDs remains consistent before and after the law change, but MDs are no longer required to supervise MLP care. So even after the law change, MLPs still have an incentive to bill at the MD rate whenever possible.

Changes in SOP laws might affect the NP share of encounters via multiplie potential mechanisms. As discussed in Traczynski and Udalova (2018), a primary burden of SOP restrictions is the administrative burden they create. When SOP laws are relaxed, both MDs and MLPs can reduce their time spent on administrative tasks and increase time spent on care for patients. On the extenisive margin, MLPs may be induced to transfer to markets with less restrictive SOP laws (as discussed in Stange (2014)).

1.2.1 Literature

Several recent economics papers have explored the effects of SOP laws on various aspects of the health care industry. Most similar to this paper are Traczynski and Udalova (2018), Kleiner et al. (2016), and Koch and Petek (2019). Kleiner et al. (2016) show that SOP law relaxations lead to higher wages for nurse practitioners but lower wages for general practice doctors. They also show that there is little

change in the price of a specific procedure that is likely to be performed by both nurse practitioners and general practice doctors (well-child visits). Instead of focusing on provider wages and medical prices, I focus on patient outcomes.

Traczynski and Udalova (2018) uses in-depth survey data to show that nurse practitioner independence increases the frequency of routine checkups, improves perceived care quality, and decreases emergency room visits. They also show that the mechanism behind these effects is based on the reduction of administrative costs and the increase in patient access to care. My paper differs from theirs in that I use claims data instead of survey data, and I focus on the sorting of tasks and specialization of provider types to provide heterogeneous treatment effect estimates at a more granular level. Their survey data is of a wider range of patient types than I use - my data is only privately insured patients who are relatively well employed, while their data includes patients from all insurance types. Furthermore, my paper focuses on different patient outcomes: I focus on hospitalizations and spending, while they focus on access and emergency room visits.

The paper most similar to mine in spirit is Koch and Petek (2019), which uses the same claims data I use in addition to Medicare claims data to examine the effects of the SOP law changes on various patient outcomes. They use a slightly different time frame than I use, and thus have different state policy changes driving their variation. Their aggregate findings are consistent with my results in the priavately insured population, though they find improvements in outcomes for the Medicare population. They use a patient movers design in addition to a traditional difference-in-differences. They do not find any effects on patient access to care or office visit

prices. The main differentiation between our papers is that I emphasize the machine learning for heterogeneous treatment effects, allowing me to narrow in on the set of encounters most likely to be affected by the law changes. Another key difference is that I use an IV approach, focusing on encounter-level effects, while they report aggregate effects from the difference-in-differences.

Stange (2014) shows that expanded NP and PA supply has had minimal impact on the office-based healthcare market overall, but utilization has been modestly more responsive to supply increases in states permitting greater autonomy. Markowitz et al. (2017) studies another important and related set of providers who are impacted by SOP laws: nurse midwives. They find that states with relaxed restrictions on nurse midwives have lower probabilities of C-section deliveries, and improved birth outcomes.

In the retail clinic setting, Spetz et al. (2013) study costs created by SOP laws, concluding that up-front costs are higher in retail clinics because of restrictive SOP laws. Other papers have studied the differences between MLPs and MDs in a variety of settings. Ashwood et al. (2016) study both the intensive and the extensive margins of patient care to show that retail clinics serve to increase medical care utilization and thus total spending. van der Linden et al. (2010) use a small sample survey to test whether nurse practitioners provide less accurate diagnoses than physicians in the emergency care setting. They find that there are small differences in only a few categories.

The main contributions of my paper to the occupational licensing in health care literature are that I use claims data to measure the effects of care from MLPs at the

encounter level, using patient outcomes hospitalizations and spending. The application of the machine learning methods to this granular data allows me to estimate *encounter specific* effects, whereas other papers measure only aggregate impacts. Thus, I provide estimates suggesting which groups of patients benefit from MLP care, and which don't, instead of the aggregrate effect of MLP use in general. Furthermore, the claims data I use also allows me to study the effects of MLP use on the privately insured population, a population responsible for a significantly large share of total medical spending.

I also contribute to the literature on machine learning for heterogeneous treatment effects. I use a generalized random forest (GRF) approach which was developed in Athey and Imbens (2016), Wager and Athey (2017) and Athey et al. (2016). Other papers have used this approach in different setttings. For example, Davis and Heller (2017) show heterogeneous effects of summer work programs on different types of young adults. I contribute to this literature by providing an example of adapting the method to a difference-in-difference setting. The difference-in-difference framework requires some slight modifcations to the traditional approach. Before implementing the GRF, I first residualize the data, removing the variation from state and year fixed effects as well as other variables with variation at levels higher than the encounter level from each variable. This residualization is similar to one of the methods compared in Oprescu et al. (2018), which showed that the residualized GRF did well compared to other HTE methods in non-experimental settings.

1.3 Data

The primary data source for this paper is the IBM®MarketScan®Commercial Database[4], a collection of insurance claims from private insurance agencies and businesses that provide health insurance for their enrollees/employees. Claims are anonymized and aggregated across submitters[5]. I use data from years 2003 to 2017.

I use MarketScan data containing information on patient outpatient visits, hospital admissions and stays, prescription drug fills, and emergency room visits. Variables available include payment information, diagnostic information, procedure information, patient home state, and provider type codes. Patients are uniquely identified by an identification number that is consistent across years and types of claims. Substate patient geographic information is not available for my entire sample period, so I do not include it.

The primary outcome variables I use focus on the year following each outpatient visit. First, I use an indicator for whether the outpatient visit was followed by a "preventable" hospitalization within the next year. To define "preventable" I use the standard definition[6] from the Agency for Healthcare Research and Quality which lists the diagnostic codes for 13 prevention quality indicators for diagnoses that are widely considered preventable in an outpatient setting. This variable is a metric of quality of outpatient care. I also show results adjusting this variable to whether the

4. IBM Watson Health and MarketScan are trademarks of IBM Corporation in the United States, other countries or both.

5. Claims are not collected randomly, but rather as a convenience sample of submitting agencies. The non-randomness of the sample does not present econometric identification issues as long as the decision to submit claims is not correlated with changes in state SOP laws, which seems plausible.

6. `https://www.qualityindicators.ahrq.gov/Modules/PQI_TechSpec_ICD10_v2018.aspx`

outpatient encounter was followed by a preventable hospitalization within 90 days, and for all hospitalizations (including non-preventable).

The other outcome variable I use is the log of total medical spending by or in behalf of each patient over the year following the outpatient visit, excluding cash payments and premiums. This measure includes copayments, deductible payments, and payments by the insurance company for outpatient, inpatient, and prescription drug claims. Ideally, outpatient visits will be effective in reducing future spending. The combination of this spending variable and the preventable hospitalization indicator reflects health outcomes quality and spending.

I provide details of the procedure I use to obtain the estimation sample in appendix 1.8.

I obtained the law change dates from state legislation and statute records, as well as by contacting state boards of nursing. Table 1.1 classifies each state into one of three categories based on SOP law change dates. The first category of states are those that changed their law during the data time frame. The second group of states are those which have MLP independence, but relaxed their law prior to the start of the data. The final group are those that had not relaxed their law prior to the end of the data time frame. For the states that changed during the data time frame, table 1.1 lists the law change year. I limit only to these law-changing states. I focus only on practice authority independence, rather than prescription authority since I am interested in the effect of patients receiving care from an ND, which is directly connected to the NP practice authority.

Since the timing of state law changes varies substantially across the data, and

the effect of the law change likely takes several years to reach its full effect, I use an unbalanced panel of states which includes all states that changed their laws in the data time frame.

Tables 1.2 - 1.3 show summary statistics for the overall sample as well as the high first stage sample. The primary conclusion from these tables is that the high first stage sample is a set of less risky patients who spend less and use the health care system less than the overall sample. The mean age in the overall sample is roughly 35 years old, roughly 40 percent of encounters are male patients, and 13 percent of encounters are from patients from rural (non-MSA) residences. Payments for each encounter and total spending measures have large standard deviations, suggesting a wide range of health care utilitzation and/or intensity of utilization. Roughly 6 percent of encounters were followed by some hospitalization over the year following the encounter, while around 0.6 percent of encounters were followed by a preventable hospitalization over the next year.

Table 1.2: Summary Statistics, Overall Sample

Variable	Mean/(SD)	Variable	Mean/(SD)
Age	35.516	NP Usage	0.034
	(16.404)		(0.182)
Male	0.406	Any Hosp Next Year	0.058
	(0.491)		(0.233)
Rural	0.13	Preventable Hosp Next Year	0.006
	(0.337)		(0.08)
Overall Copay	4.94	Preventable Hosp Next 90 Days	0.003
	(11.836)		(0.05)
Overall Payment	96.099	Total Spending Next Year	6,645.522
	(514.548)		(28967.083)
Total Encounters	24,334,637	Log Total Spending Next Year	6.795
Total Patients	1,388,886		(2.684)

Note: This table shows the means and standard deviations for the variables listed, as well as the total counts of encounters and patients in the overall sample.

Table 1.3: Summary Statistics, High First Stage Sample

Variable	Mean/(SD)	Variable	Mean/(SD)
Age	22.749	NP Usage	0.092
	(15.407)		(0.289)
Male	0.32	Any Hosp Next Year	0.01
	(0.466)		(0.1)
Rural	0.181	Preventable Hosp Next Year	0.002
	(0.385)		(0.039)
Overall Copay	4.41	Preventable Hosp Next 90 Days	0.001
	(9.899)		(0.028)
Overall Payment	65.126	Total Spending Next Year	921.7484
	(249.433)		(6176.96)
Total Encounters	672,408	Log Total Spending Next Year	3.743
Total Patients	138,889		(3.07)

Note: This table shows the means and standard deviations for the variables listed, as well as the total counts of encounters and patients in the large predicted first stage subsample.

1.4 Overall Sample Approach and Results

1.4.1 Overall Sample Approach

The primary research question for this paper is whether receiving care from an MLP has worse patient outcomes than receiving care from an MD. I exploit changes in SOP

laws as a natural experiment that exogenously shifts encounters from MDs to NDs. Thus, the first stage is to measure the effect of the law change on MLP utilization. I estimate the first stage effect using a difference-in-differences specification with state and year fixed effects:

$$ND_{ist} = \beta Post_{st} + \gamma_s + \lambda_t + \Theta X_{ist} + \epsilon_{ist} \tag{1.1}$$

Where γ_s and λ_t represent full sets of state and year fixed effects respectively, and X_{ist} is a vector of control variables. ND_{ist} is an indicator for whether the encounter was handled by an ND, and $Post_{st}$ is an indicator variable equal to 1 if the encounter occurred in state s after the state relaxed its SOP laws, and 0 otherwise. The coefficient of interest is β, which gives the effect of the law change on the probability of MLP use. All standard errors are clustered at the state level. The control variables include encounter-level covariates such as patient age, gender, and rural status (defined as whether the patient lives in an MSA or non-MSA area).

The Affordable Care Act (ACA) contains provisions aimed at increasing the use of NDs. Some states relaxed their SOP laws after the ACA was passed, possibly in response to the federal shift towards increased MLP use. These states are plausibly different in the reasons for changing their SOP laws than states that changed prior to the passing of the ACA, and likely have different trends in MLP use leading up to the SOP law change. To account for these differences, I include a time trend specific to states that changed their SOP laws after the ACA was passed in each of the specifications.

Given that many state legislatures announced the law changes prior to the changes actually taking place, insurance companies and providers were able to pre-emptively adjust their behavior prior to the policy change. For this reason, I omit encounters from the year prior to the law change in all specifications. This allows me to compare encounters that were "safely" in the pre-law change period to those in the post-law change period.

Since I limit the estimating sample to only states that changed their laws, the primary assumption for causal inference in this case is that the timing of the law changes is conditionally independent of the outcome variable. I verify this assumption by replacing the $Post_{st}$ indicator with a set of years-before/since the law change indicators, and testing whether the coefficients in the pre-treatment period are non-zero. In doing so, I cap encounters that occurred more than six years before or after the law change to six years before/after, which allows me to keep the long run observations in the estimatation sample helping to pin down the treatment effect estimates.

The reduced form effect of the law change on patient outcomes is identical to equation 1.1 above, but replaces the outcome variable with a health outcome of interest. The health outcomes I use are an indicator variable for whether the outpatient encounter was followed by a hospitalization for a preventable condition within the next year, and the patient's log total health spending in the next year. I also show results for preventable hospitalizations within the next 90 days and hospitalizations for any reason over the next year. In this reduced form specification, the coefficient on the $Post_{st}$ indicator gives the effect of the law change on the health outcome

variable.

To infer causation from the law change on outcome variables, I again take an event study approach as above and show the pre-treatment trends on the years-since indicators with the health outcomes as the outcome variables. If the pre-treatment trend coefficients are not significantly different from zero, this suggests that the law change was conditionally exogenous of the outcome variables of interest.

To measure the effect of receiving care from an MLP for those encounters that were moved by the law change, I use an IV approach where the MLP indicator is instrumented by the law change. I include the same controls as in the previous specifications. The outcome variables are the health outcomes of interest. The coefficient on the instrumented MLP indicator gives the effect of receiving care from an MLP for those types of encounters that were moved by the law change from MDs to NDs.

The causal interpretation of the IV results relies on the assumption that the law change did not affect the outcome variables through channels other than the use of NDs. This is plausible given the timing of the law change is as good as random across states (as verified by the event studies on MLP use and patient outcomes).

1.4.2 Overall Sample Results

The results from the traditional first stage estimation on the full sample are shown in the first row of table 1.4, labeled "NP Usage". The first column shows the baseline (pre-law change) means and standard deviations. The second column (labeled "OLS") shows the effect of the law change (the coefficient on the post law change

indicator in the difference-in-difference regression). As seen in the OLS column, relaxing the SOP laws increases the probability that an encounter is handled by an MLP by 3.2 percentage points. This estimate is statistically significant and robust to including encounter-level controls. This effect is up from a baseline share of 2.4 percent of encounters handled by MLPs in the pre-law change period.

Table 1.4: Regression Coefficients, Overall Sample

	Baseline Mean	OLS	IV
NP Usage	0.024	0.032	
	(0.153)	(0.012)	
Log 1-Year Spending	6,924.23	0.038	1.169
	(26,331.51)	(0.1004)	(3.143)
Preventable 1-Year Hosp.	0.007	-0.0002	-0.007
	(0.084)	(0.0005)	(0.015)
Preventable 90-Day Hosp.	0.003	-0.00004	-0.001
	(0.052)	(0.0003)	(0.008)
Any 1-Year Hosp.	0.063	-0.002	-0.054
	(0.243)	(0.002)	(0.05)
N		24,334,637	24,334,637
First Stage F			7.341

Note: This table shows the regression output for the reduced form and IV specifications in the overall sample. Each row represents a separate regression with the outcome variable listed. The first column (labeled "Baseline Mean") shows the baseline (pre-law change) means and standard deviations. The second column shows the reduced form effects of the law change on the outcome variable. The final column shows the IV coefficient on the ND use indicator, instrumented by the post law change indicator. All specifications include state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act (2010). Standard errors, clustered at the state level, are shown in parentheses. For the IV specifications, The first stage F-statistic is shown at the bottom of the table.

Table 1.4 also shows the reduced form outcome effects for the full sample in column 2. None of the outcome estimates are statistically significant at conventional

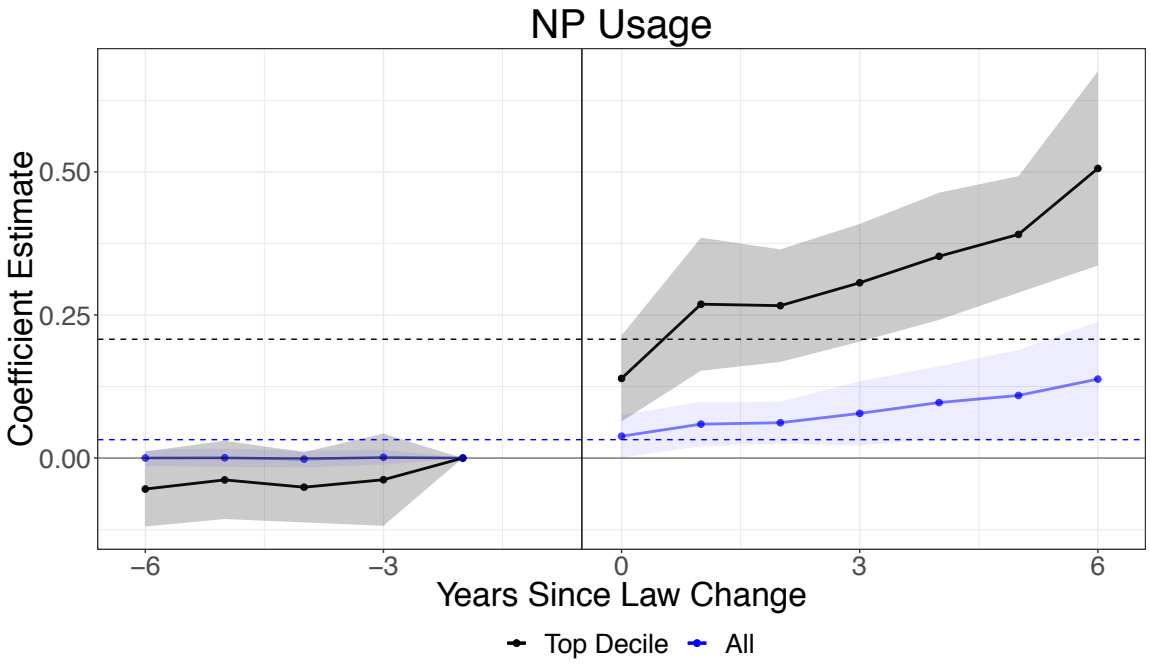
levels. The point estimates suggest a potential decrease in hospitalizations, though relatively large standard errors make any conclusions about these estimates only suggestive at best. In the baseline mean column for the log spending outcome, the entry shows the baseline mean and standard deviation of raw total spending.

Figure 1.1 shows the effects of the event study regression for the overall sample in blue (and the high first stage subsample in black). The vertical axis gives the estimated coefficient on the years-since indicators, and the horizontal axis shows the years since the law change. The shaded area represents 95-percent confidence intervals based on standard errors clustered at the state level. The years since coefficients in the pre-treatment period are not statistically different from zero, and the point estimates show no trend prior to the law change. After the law change, the coefficients increase steadily up to roughly 13 percentage points six years after the law change. As mentioned above, the encounters in the year prior to the law change are omitted to avoid contamination from anticipation effects.

The dynamic reduced form effects on health outcomes in the full sample are shown in figures 1.2-1.5. As with the first stage estimates, the full sample coefficients are shown in blue. None of the outcomes show significant trends in coefficient estimates either before or after the law changes, though the standard errors do not rule out decreases in preventable hospitalizations over the next year.

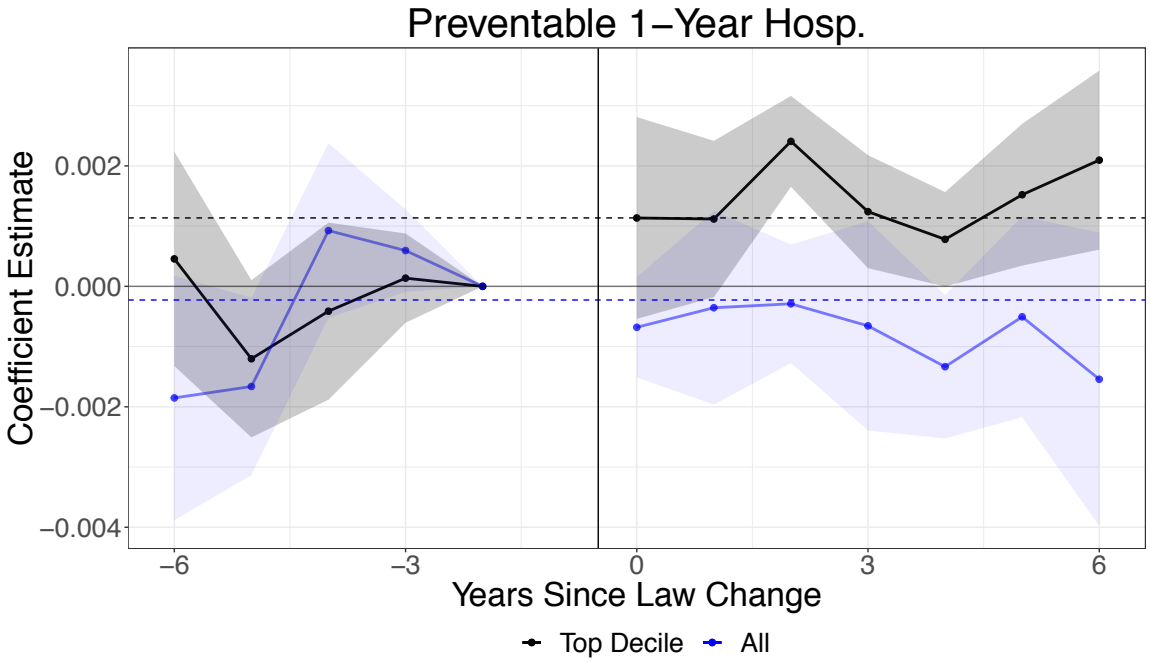
The third column of table 1.4 (labeled "IV") shows the results from the traditional IV specifications in the full sample. The coefficients on each type of hospitalizations are negative (though again with relatively large standard errors). The IV coefficient on spending is large and positive, but again, the standard error is large. Interpretted

Figure 1.1: First Stage Event Study



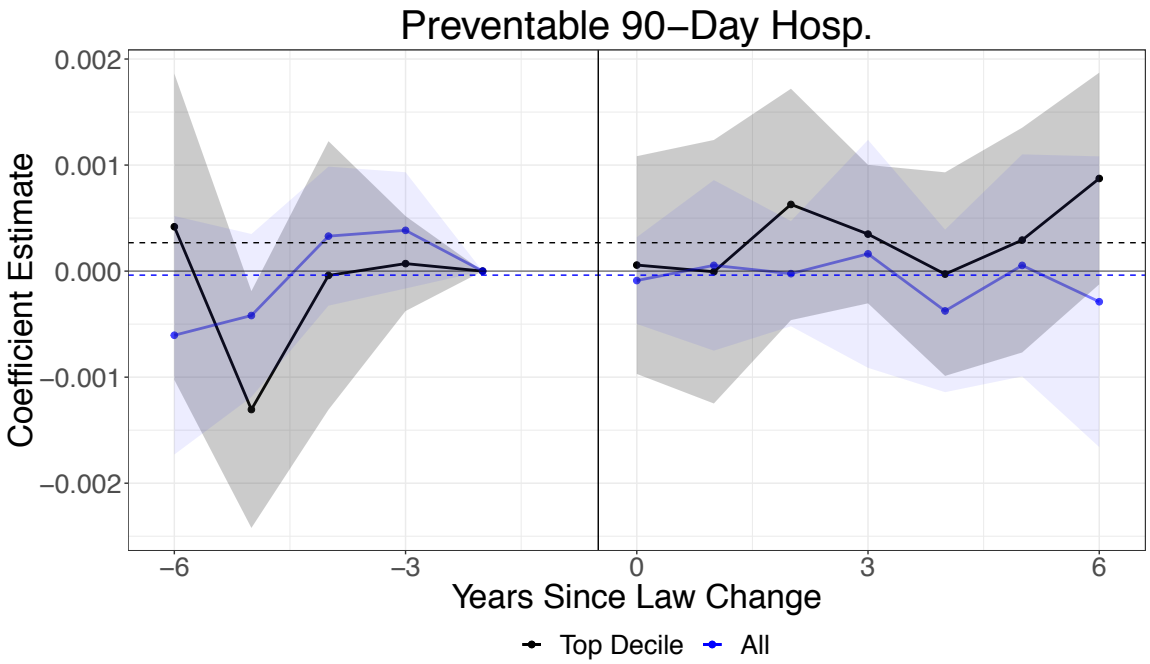
Note: This figure shows the coefficients on years since the SOP law change indicators in the first stage event study, where the outcome is an indicator for whether the encounter was handled by an ND. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

Figure 1.2: Reduced Form Event Study (1-year Preventable Hospitalizations)



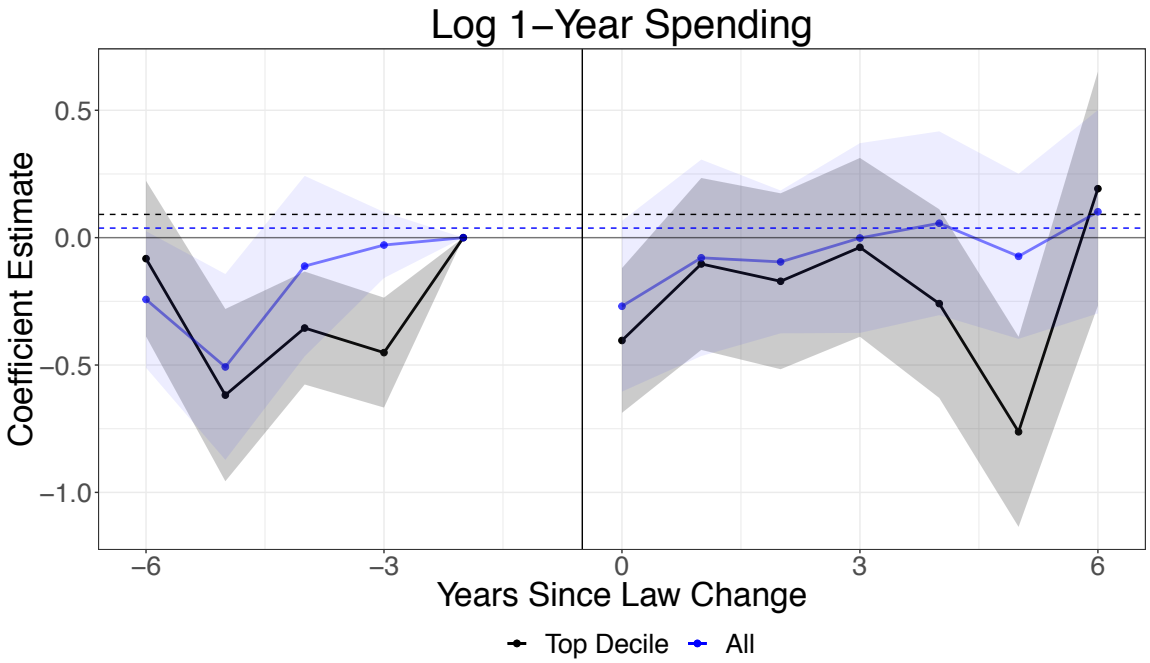
Note: This figure shows the coefficients on years since the SOP law change indicators in the reduced form event study where the outcome is an indicator for whether the encounter was followed by a hospitalization for a preventable reason within 1 year. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

Figure 1.3: Reduced Form Event Study (90-day Preventable Hospitalizations)



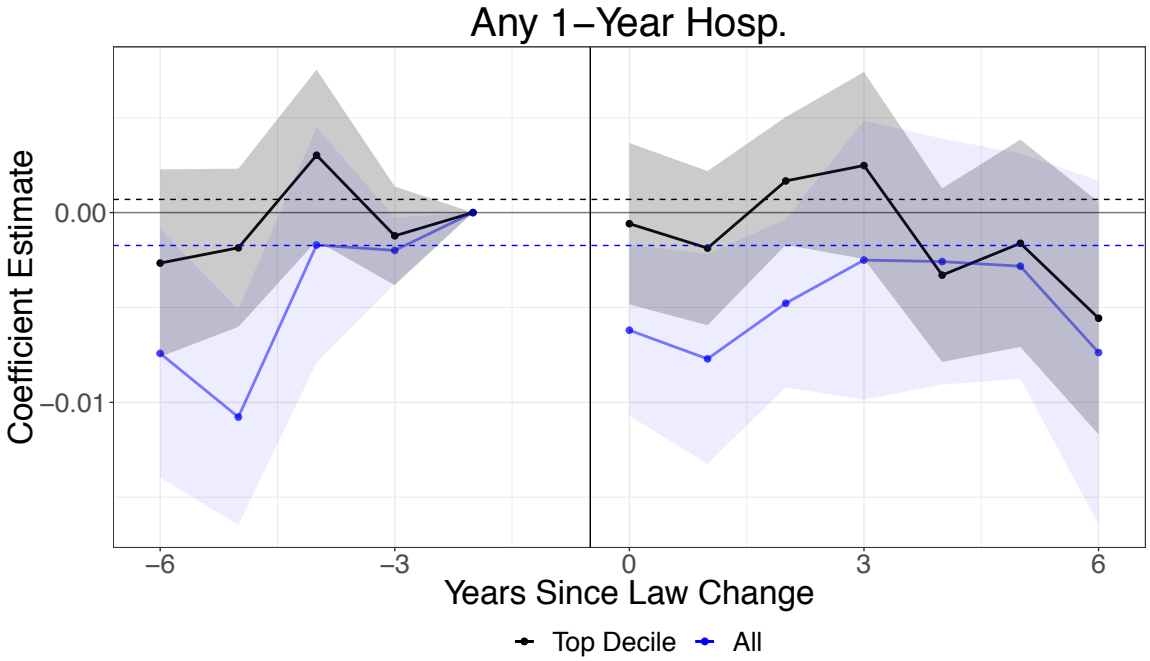
Note: This figure shows the coefficients on years since the SOP law change indicators in the reduced form event study where the outcome is an indicator for whether the encounter was followed by a hospitalization for a preventable reason within 90 days. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

Figure 1.4: Reduced Form Event Study (Log 1-year Spending)



Note: This figure shows the coefficients on years since the SOP law change indicators in the reduced form event study where the outcome is the patient's total medical spending in the year following the encounter. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

Figure 1.5: Reduced Form Event Study (1-year Any Hospitalizations)



Note: This figure shows the coefficients on years since the SOP law change indicators in the reduced form event study where the outcome is an indicator for whether the encounter was followed by a hospitalization for a any reason within 1 year. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

as a local average treatment effect, these estimates do not rule out decreases in preventable hospitalization for those encounters that were shifted to MLPs as a result of the law changes.

Table 1.4 reports the first stage F-statistic for the IV specification at roughly 7.3, which suggests the identification suffers from a weak instrument problem, and motivates alternative approaches.

As expected, relaxing SOP laws exogenously increases the utilization of NDs, though the changes do not appear to influence a large number of encounters (relative to the number of encounters handled by MDs). The small share of affected encounters is likely the reason for the imprecise reduced form estimates. That is, there are many encounters that have no probability of being shifted to an MLP which cloud the identification of the true effect.

Narrowing in on the set of encounters most likely to be impacted by the law change will allow me to increase the power of the first stage estimates. I will then estimate the true effects of receiving care from an MLP using the high-first stage set of encounters. To objectively find the high-first stage set without arbitrarily manipulating the data sampling, I use machine learning to predict the first stage effect on the probability of MLP use for each type of encounter, and then partition the data into groups based on these predictions. I explain the details of this approach in the next section.

1.5 Heterogeneous Treatment Effect Methods

1.5.1 Generalized Random Forest (GRF)

In this section, I briefly explain the details of the machine learning method I use to estimate heterogeneous treatment effects (HTEs). The specific algorithm I use is the generalized random forest (GRF), and a more detailed explanation of the algorithm is found in Athey et al. (2016). I also explain the details of the method in my application in appendix 1.9.

At a high level, the GRF essentially estimates a different weighted least squares regression for each encounter in the data. All encounters are used in each regression, but a different set of weights is used for each encounter. The weights for a specific encounter, encounter i, are defined so as to place higher weight on encounters that have "similar" predicted probabilities of shifting to MLPs as encounter i. "Similarity" is determined using a recursive partitioning approach: encounters are partitioned into subgroups based on cutoffs of predictor variables (ie. age less than 40). Cutoffs are decided by examining the two different probabilities for shifting to MLPs in the two resulting subgroups, and then choosing the cutoff that maximizes heterogeneity in these two estimates. This process continues recursively until the partitions reach a minimum number of encounters. Then, encounters in the same partition as i get higher weight than encounters in separate partitions.

So, for example if the first proposed cutoff is age less than 40, the algorithm estimates the probability of shifting to MLPs in the age less than 40 subgroup as well as the age greater than or equal to 40 subgroup. The variance of these two

estimates is compared to the variance of all other potential cutoffs (such as the male vs female split). The propsed cutoff with the largest resulting variance is executed, and the process repeats in each of the two subgroups. Subgroups are divided until a resulting subgroup reaches a minimum specified number of encounters. Then, all encounters in the same final subgroup as encounter i get higher weights than encounters in different subgroups.

The GRF is designed for experimental settings with exogenous treatment, so I adapt the basic GRF for my quasi-experimental difference-in-difference setting by residualizing out the variation from variables that vary at levels greater than the encounter level from the outcome and treatment variables prior to implementing the algorithm[7]. Thus, I pass the residuals from the outcome and post-law change variables into the algorithm along with the set of predictor variables. The specific variables I residualize out are the state and year fixed effects, and the linear time trend that is specific to the states that changed their SOP law after the passing of the ACA.

The results of the GRF are encounter-specific estimates for the effect of the law change on MLP usage, which I then collapse to the patient level by taking the median of all predictions for each patient. I then divide patients into deciles based on their median predicted first stage probability of shifting to NDs. I then estimate seperate event study regressions, identical to the first stage and reduced form event studies above, in each decile. Thus, I obtain an estimate for the effect of the law change on MLP use, hospitalizations, and total spending in each decile of predicted first stage

7. This is similar to one of the methods for estimating HTEs in Oprescu et al. (2018), which found that the adapted GRF performed well relative to other methods

effect.

After estimating the first stage and reduced form effects for each decile, I then estimate an IV specification for each decile using the same specification as above, where the law change indicator serves as an instrument for the ND-use indicator. Doing so gives an IV estimate for the effect of receiving care from an MLP for those encounters moved by the law change, separately for each decile of predicted first stage effect. I focus on the top decile of patient median predicted first stage effect, which is the set of patients most likely to be shifted to MLPs after the law change. I show that in this group, the law change is no longer a weak instrument, sharpening the analysis.

1.5.2 Instrumental Variables Generalized Random Forest

The final questions I turn to are focused on the efficiency of the sorting that results from the law change. That is, I seek to understand whether the types of encounters that *are* being sorted to MLPs are in fact the types of encounters that *should be* sorted, in view of reducing hospitalizations and spending. There are other reasons for sorting that I do not capture with these two variables, but these two are important aspects of the health system and of the patient's health care experience, and are widely used measures of quality of care.

I run another machine learning algorithm that is very related to the GRF - the instrumental variables generalized random forest or IV GRF. The process for estimating this algorithm is identical to the GRF, but instead of estimating an encounter-specific first stage effect, the IV GRF produces an estimate for the *IV effect* for each

type of encounter. Thus the result of the IV GRF is an encounter-specific prediction for the effect of care from an ND.

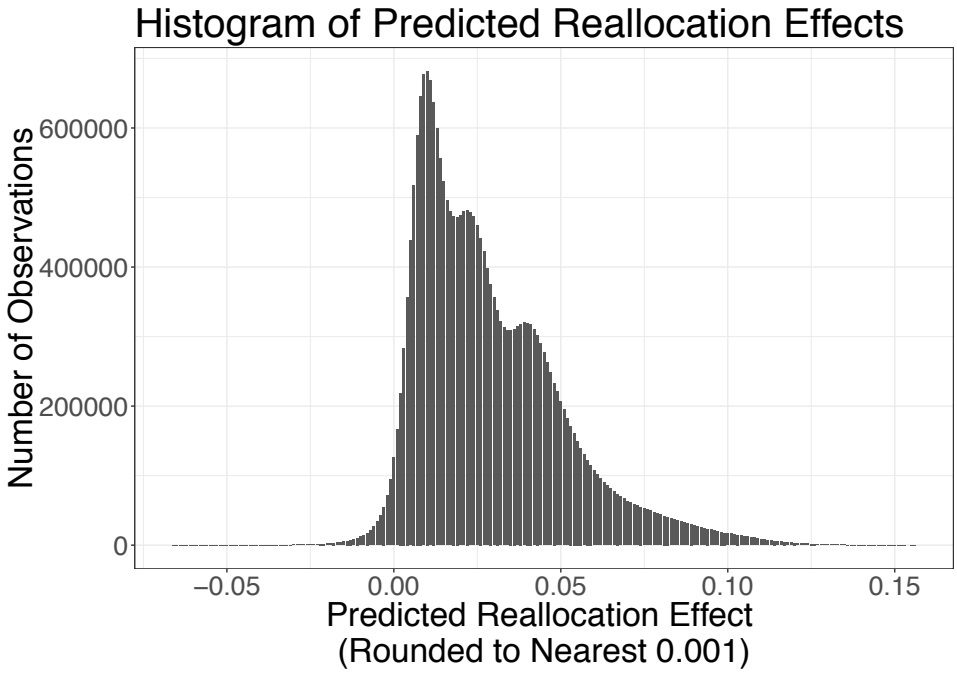
I interpret the IV GRF estimates as the quality differential for each encounter between an MLP and an MD. If the estimate is negative, that suggests shifting the given type of encounter to MLPs would result in *decreases* in spending and hospitalizations, relative to not being shifted to NDs.

To examine the sorting efficiency, I show a heatmap with the decile of predicted first stage GRF shifting probability effects on the x-axis and decile of predicted IV effects on the y-axis. If sorting were perfectly efficient, the types of encounters for which outcomes are predicted to improve after shifting to MLPs should be the most likely to be sorted to MLPs after the law change. This would be represented by a concentration of encounters in the bottom right quadrant of the IV GRF-GRF heatmap: most likely to be shifted to MLPs (furthest to the right) and largest predicted reductions in spending and hospitalizations resulting from MLP care (furthest to the bottom).

1.6 Heterogeneous Treatment Effect Results

Tables 1.3 shows the means and standard deviations for the highest first stage decile. As mentioned above, this table shows that the high first stage sample is a younger, less risky group that spends less on medical care and uses the system less frequently than the overall sample. The high first stage group spends less overall, and per encounter, and are hospitalized less frequently than the overall sample. This suggests the patients most likely to shift to MLPs are the lower complexity patients, a fact

Figure 1.6: Predicted Allocation Effect Distribution



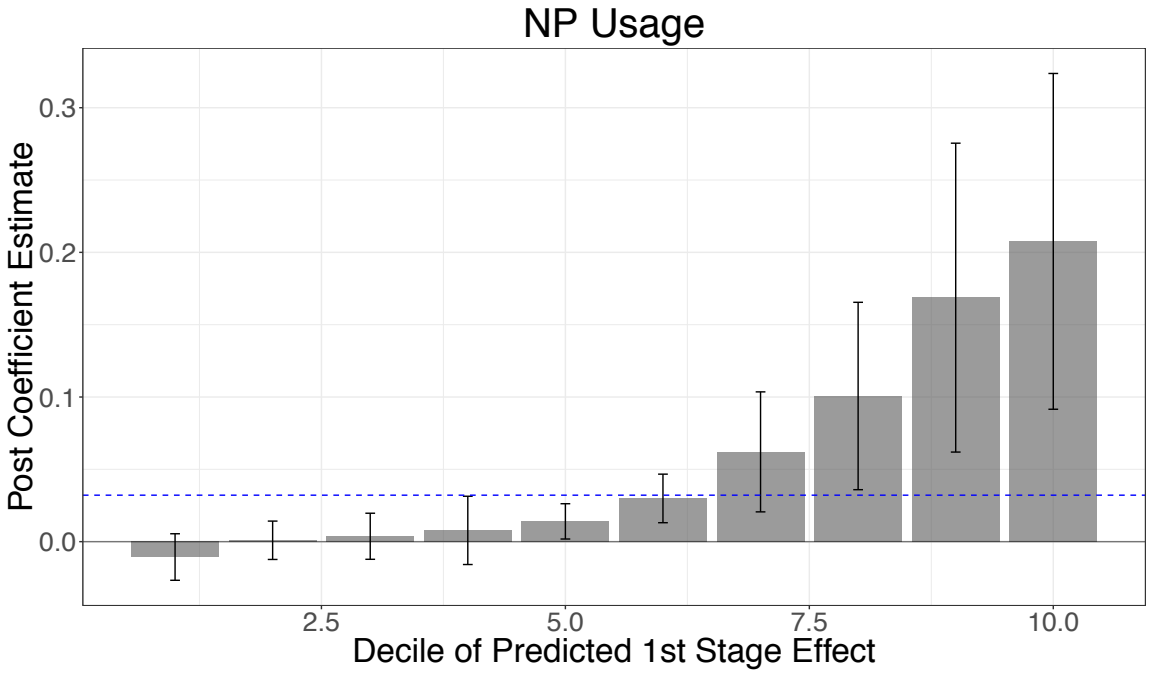
Note: This figure shows the distribution of predicted first stage allocation effects.

consistent with predictions that would be made using institutional knowledge.

The distribution of predicted first stage effects is shown in figure 1.6. To assess the accuracy of the predicted first stage effects, figure 1.7 shows the traditional first stage effects estimated in each decile of predicted first stage effects. The results show a monotonic increase in traditional effect estimates as predicted effects decile increases. In the top decile of predicted effect, the traditional DD estimate is roughly 0.2, implying that in this group, MLP use increased by 20 percentage points after the law changes. This top decile is the high first stage sample I use as the relevant sample of patients affected by the law change.

Figures 1.1-1.5 show the event study results for the high first stage sample in

Figure 1.7: ND First Stage Effect by Predicted First Stage Decile



Note: This figure shows the DD coefficient on the post law change indicator in specification with the MLP use indicator as the outcome. The bars show the effect in each decile of predicted first stage effects. Error bars show 95% confidence intervals based on standard errors clustered at the state level. The blue dashed line shows the effect from the traditional analysis in the overall sample.

black, as well as the overall sample in blue, where the high first stage sample is defined as the top decile of person median predicted first stage effects. The horizontal dashed lines show the DD regression estimates. Figure 1.1 shows the primary purpose of the GRF: the first stage estimates in the high first stage sample are considerably larger than the overall estimates. In the pre-law change period, the high first stage sample has more negative coefficients than the overall sample, but none of the coefficients are statistically significantly different from 0. This illustrates a potential shortcoming of the GRF: it is designed to find heterogeneity in the summarized DD estimates, regardless of pre-trends (though that does not appear to be an issue in this case).

The summarized DD estimates for the high first stage sample are shown in table 1.5. The first stage effect is much larger in the high first stage sample than in the overall sample; roughly a 20 percentage point increase in the share of MLP encounters after the law change, though the baseline mean is also higher than in the overall sample. The number of encounters in the high first stage sample is not equal to ten percent of the overall number of encounters since the high first stage sample is determined by the patient median predicted effect. Ten percent of patients are in the high first stage sample, but these patients do not have as many encounters as patients in other deciles.

Table 1.5: Regression Coefficients, High First Stage Sample

	Baseline Mean	OLS	IV
NP Usage	0.065	0.208	
	(0.246)	(0.059)	
Log 1-Year Spending	859.49	0.091	0.439
	(5,037.18)	(0.2072)	(0.954)
Preventable 1-Year Hosp.	0.002	0.0011	0.005
	(0.041)	(0.0003)	(0.002)
Preventable 90-Day Hosp.	0.001	0.00027	0.00129
	(0.03)	(0.0003)	(0.001)
Any 1-Year Hosp.	0.011	0.001	0.003
	(0.104)	(0.001)	(0.005)
N		672,408	672,408
First Stage F			12.29

Note: This table shows the regression output for the reduced form and IV specifications in the high first stage subsample. Each row represents a separate regression with the outcome variable listed. The first column (labeled "Baseline Mean") shows the baseline (pre-law change) means and standard deviations. The second column shows the reduced form effects of the law change on the outcome variable. The final column shows the IV coefficient on the ND use indicator, instrumented by the post law change indicator. All specifications include state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act (2010). Standard errors, clustered at the state level, are shown in parentheses. For the IV specifications, The first stage F-statistic is shown at the bottom of the table.

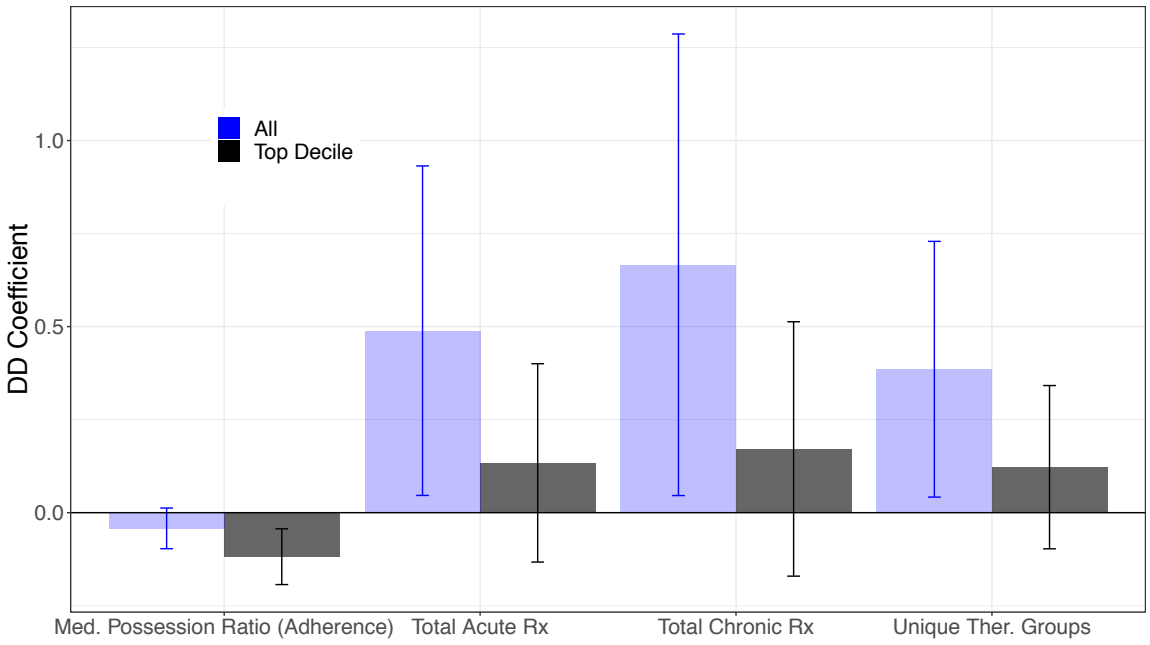
Column 3 of table 1.5 shows the results from the IV specification in the high

first stage sample. First, it is important to note that the first stage F sample is higher in the high first stage sample than in the overall sample, as indicated at the bottom of the table. All IV coefficients in the high first stage sample are positive, and the estimates for the preventable hospitalization outcomes are statistically significant. These results suggest that MLPs increase preventable hospitalization rates by roughly half of a percentage point for those encounters shifted to MLPs after the law change.

Interestingly, the reduced form outcome effects in the high first stage sample are generally positive (as opposed to negative in the overall sample). This is also reflected in the high first stage sample event studies in figures 1.2-1.5. Preventable hospitalization rates over the next year increased by roughly one-tenth of a percentage point, up from a baseline level of 0.2 percent. Preventable hospitalizations in the next 90 days and hospitalizations for any reason over the year following are not significantly different from 0, but are both positive while their full sample counterparts are negative. Log spending does not change in a significantly different amount in the high first stage sample from the overall sample.

To understand the mechanisms behind these results, I estimate the reduced form DD effects of various other outcome variables in the top decile of predicted first stage effects and in the overall sample. Figure 1.8 shows the results of these regressions for both the high first stage sample and the overall sample for outcomes related to prescription drugs. This figure shows an increase in prescription fills for both chronic and acute drugs the overall sample, while the high first stage sample does not. The high first stage sample instead has a decrease in mean medical possession ratio (a

Figure 1.8: RX Effect Breakdown



Note: This figure shows the DD coefficient on the post law change indicator in specification with the listed variable as the outcome. The blue bars represent the effect in the overall sample, while the black bars show the effect in the top decile of first stage effects. Error bars show 95% confidence intervals based on standard errors clustered at the state level.

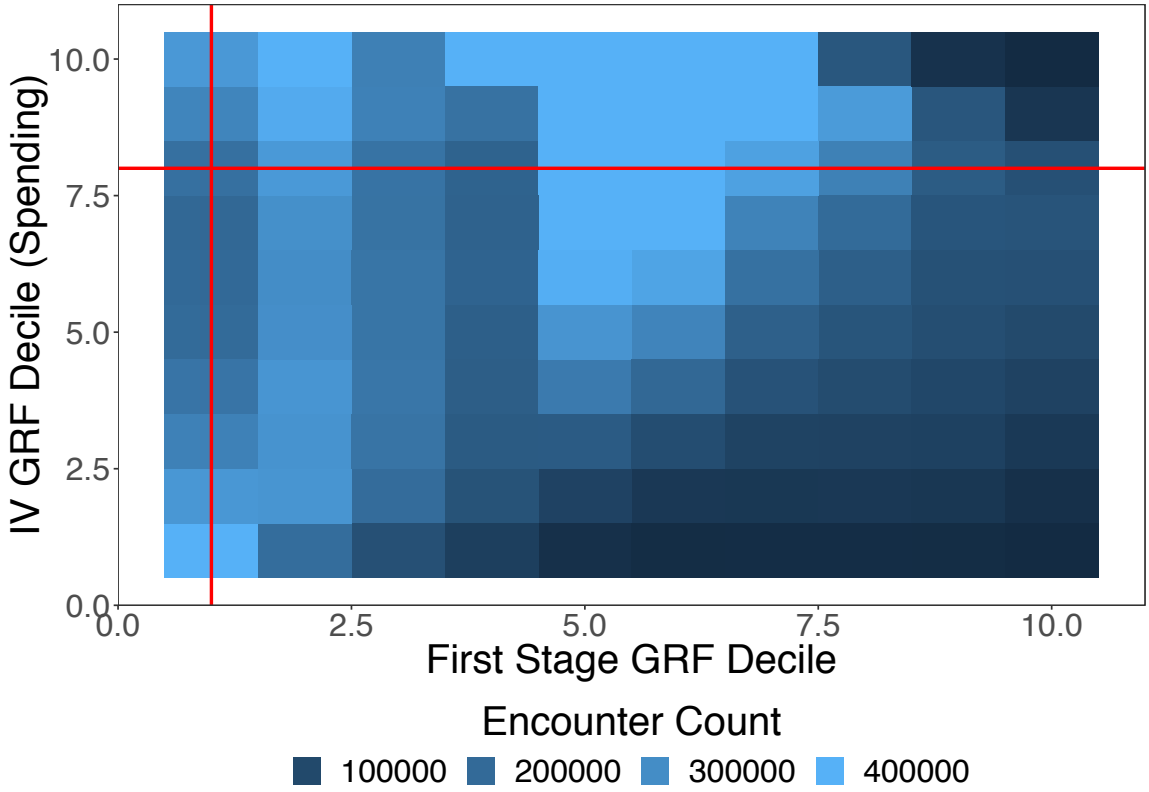
measure of medication adherence), while the overall sample does not. The overall sample sees an increase in the number of unique types of prescription drug therapeutic classes while the high first stage sample does not. This suggests a potential mechanism behind the observed increase in preventbale hospitalizations: changes in prescription drug behavior. This also may explain why the effect is observed at the one-year level and not the 90-day level, since the effects of not taking prescription drugs may take more time to manifest themselves.

The results of the IV GRF are shown in figures 1.9 - 1.10, in comparison to the predicted first stage GRF results. Figure 1.9 shows a heatmap of the count of encounters in each IV GRF decile - GRF decile bin, where the outcome in the IV GRF

is log total spending. Lighter colors represent a higher concentration of encounters. Red lines show deciles above which all predicted effects are positive. Thus, below the horizontal red line represents encounters that are predicted to decrease spending from MLP use, while those to the right of the vertical red line are encounters that are predicted to shift to MLPs after the law changes. The figure shows largely the opposite relationship between predicted change in spending and predicted probability of shifting to MLPs than a sorting aimed solely at reducing spending. The largest cluster of encounters is in the in the middle portion of the figure near the top, which suggests no changes in spending and moderate increases in the probability of MLP use. The next largest cluster of encounters is in the bottom left portion of the figure: low predicted probability of shifting to MLPs but large predicted decreases in spending from MLP use. There are relatively few encounters in the bottom right of the figure - encounters with high predicted probabilities of shifting to MLPs and large predicted decreases in spending as a result of MLP use.

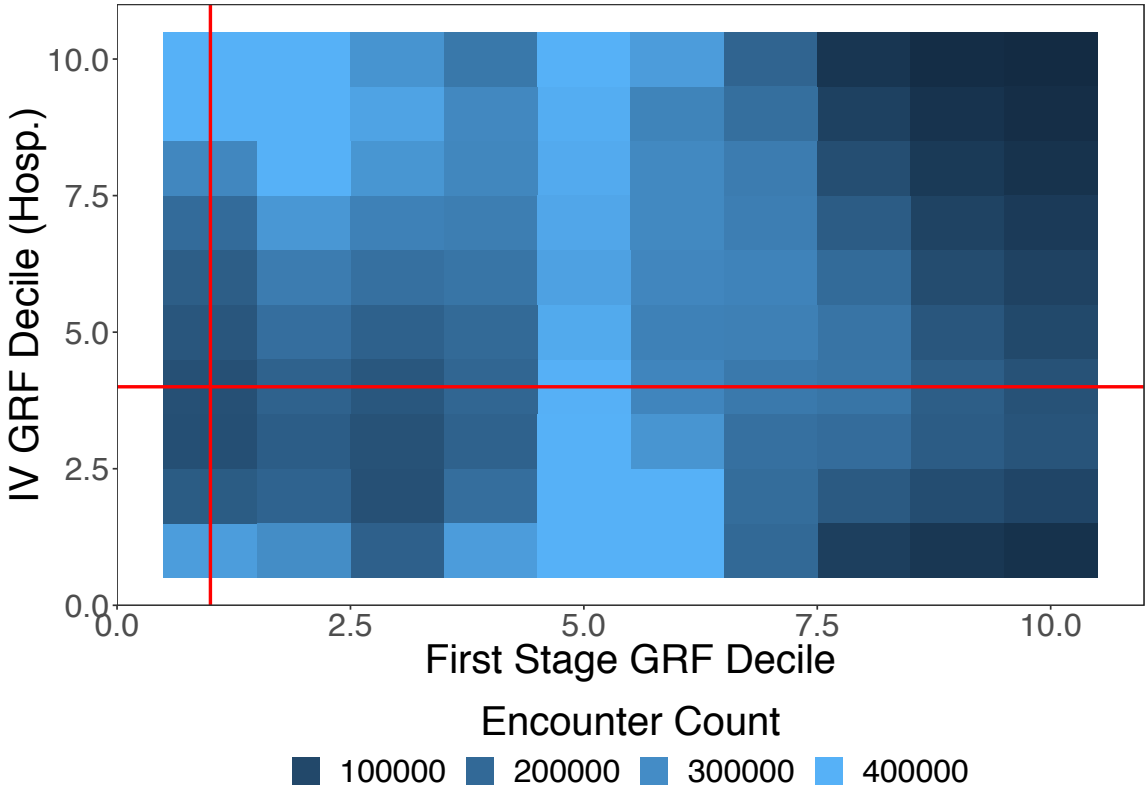
Figure 1.10 shows the sorting of enocunters when the IV GRF outcome is preventable hospitalizations over the next year. This sorting is more encouraging than the sorting with respect to predicted spending, but there is still room for improvement. There is a large concentration of encounters with large predicted decreases in hospitalization rates but only moderate predicted probability of shifting to MLPs (the bottom-middle of the figure). There is also a large concentration of encounters in the top left of the figure: predicted increases in hospitalization rates after MLP use and low predicted probability of shifting to NDs. There are however, also a relatively large set of encounters in the bottom left of the figure, with low predicted

Figure 1.9: Compare First Stage GRF to IV GRF (1-Year Log Spending)



Note: This figure shows a heatmap of the number of encounters in each first stage GRF decile - IV GRF decile bin. Light colors indicate more unique patients. The red lines indicate the decile at or below which predicted effects are negative (so the first stage GRF predicted effects are positive in all but the first decile, and the IV GRF predicted effects are positive in all deciles above the eighth decile).

Figure 1.10: Compare First Stage GRF to IV GRF (1-Year Hospitalizations)



Note: This figure shows a heatmap of the number of encounters in each first stage GRF decile - IV GRF decile bin. Light colors indicate more unique patients. The red lines indicate the decile at or below which predicted effects are negative (so the first stage GRF predicted effects are positive in all but the first decile, and the IV GRF predicted effects are positive in all deciles above the fourth decile).

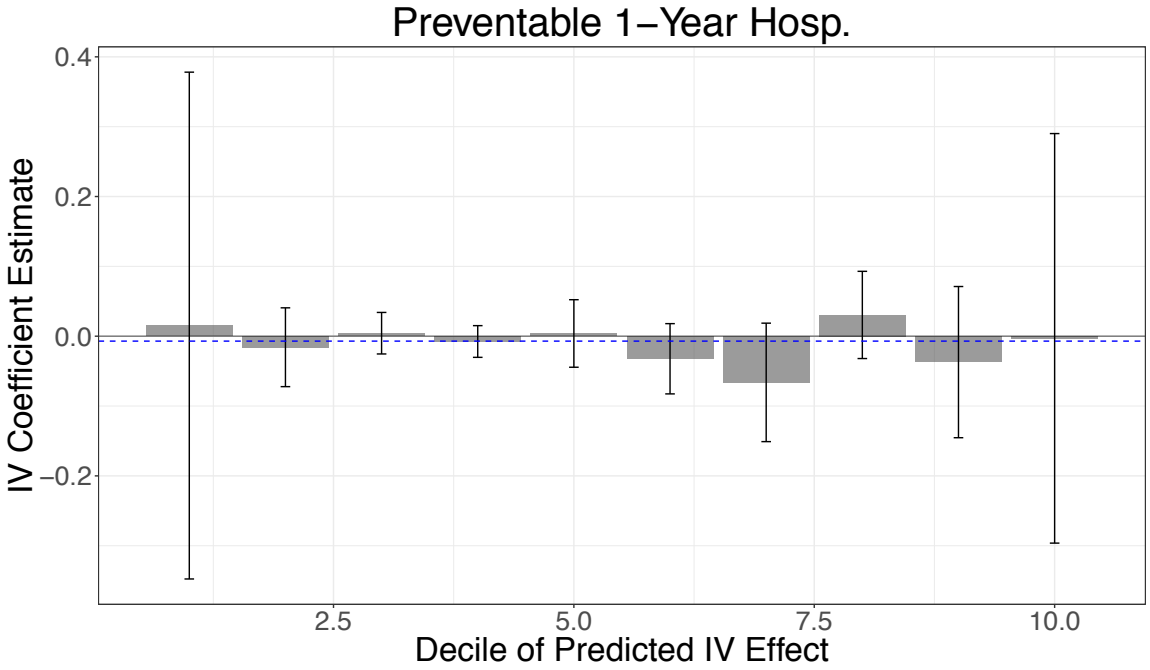
probability of shifting to MLPs but large predicted decreases in preventable hospitalizations. There are relatively few encounters in the bottom right of the figure with large predicted decreases in hospitalizations and large predicted probability of shifting to NDs.

An important caveat of these sorting results is illustrated in figures 1.11 and 1.12, which show the traditional IV analyses results in the deciles of the IV GRF predicted effects for hospitalizations and spending respectively. As opposed to the first stage effect, the IV effects do not show a clear pattern as predicted IV effect increases. This suggests that predicting which encounters will benefit from MLP care is more difficult than predicting which enocunters *will* shift to NDs.

1.7 Conclusion

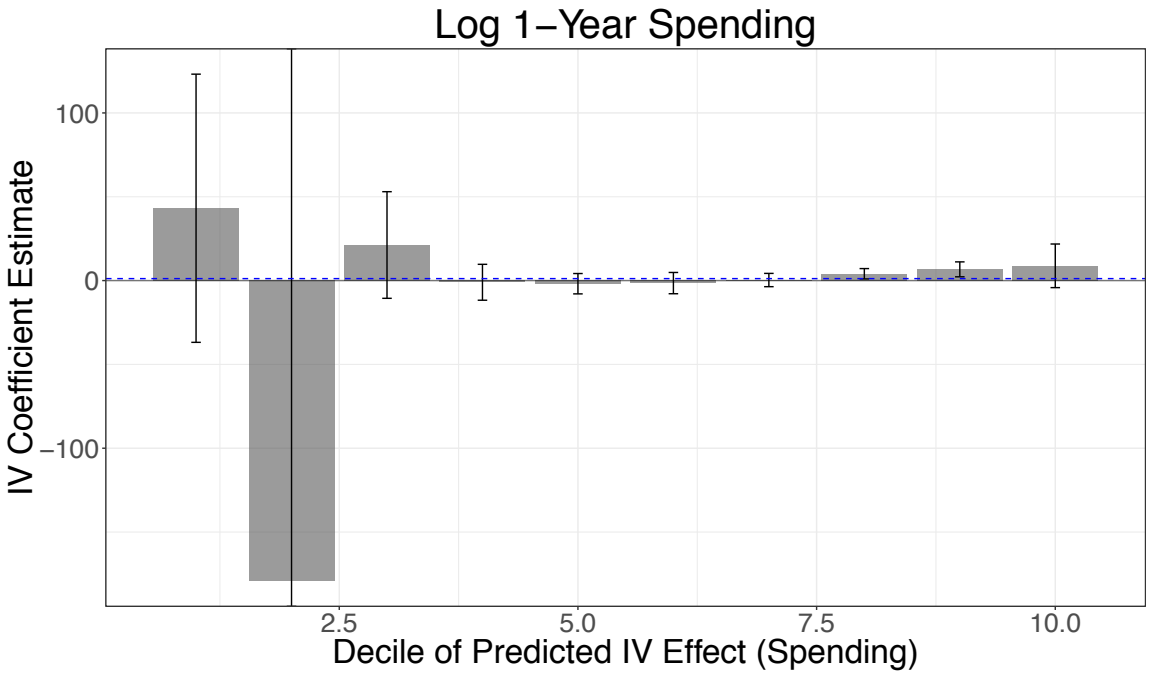
Proponents of increased use of MLPs argue that MLPs do not produce worse quality care than MDs, and that MLPs do not increase (and more likely decrease) patient medical spending. Instead, when I focus on the relevant sample of patients most likely to be affected by relaxing restrictions on NDs, I find an increase in preventable hospitalizations with noisy increases in spending. Specifically, I find that MLP use increases the share of encounters that were followed by a preventable hospitalization by roughly 0.5 percentage points. In other words, roughly 1 in 200 encounters is followed by a preventable hospitalization that would not have been had MDs handled the encounter. With a conservative estimate of the cost of an inpatient admission at roughly $10,000, this suggests that MLP utilization essentially increases the cost of outpatient visits by roughly $50 each.

Figure 1.11: IV Effects (1-year Preventable Hospitalizations)



Note: This figure shows the coefficients on years since the SOP law change indicators in the reduced form event study where the outcome is an indicator for whether the encounter was followed by a hospitalization for a any reason within 1 year. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

Figure 1.12: IV Effects (1-year Log Spending)



Note: This figure shows the coefficients on years since the SOP law change indicators in the reduced form event study where the outcome is an indicator for whether the encounter was followed by a hospitalization for a any reason within 1 year. The regression also includes state and year fixed effects, as well as a linear time trend specific to states that changed their laws after the passing of the Affordable Care Act. The shaded area represents 95 percent confidence intervals based on standard errors clustered at the state level. The results from the full sample are shown in blue, while the results from the top decile of predicted first stage effects are shown in black.

These conclusions are based on a sample of privately insured patients who are relatively well employed. Results are likely to be different on a set of patients with different outside options for care, such as Medicaid enrollees or rural patients who may forgo care entirely if they do not get access to NDs. Nevertheless, the set of patients studied in this paper are generally lower risk patients than the general population, and so increases in preventable hospitalizations in this group may be exacerbated in other more risky groups.

I find that relaxing SOP laws has potential to improve patient outcomes, dependent on the efficiency of sorting tasks between provider types. There is a large collection of encounters for which MLPs provide improved outcomes over MDs, but those are not necessarily the types of encounters most likely to be shifted to MLPs after the law changes. The sorting isn't as bad as it could be either: the types of encounters for which MLPs provide the worst outcomes relative to MDs are also not the most likely to be shifted to NDs.

Another potential mechanism driving the effects is the changes in prescriotion drug behavior in the high first stage sample. Patients in this group decreased medication adherence rates and did not increase their prescription fill rates, while the overall sample did not decrease adherence rates but did increase their fill rates.

There are multiple interacting incentives that must be aligned in order to reach efficient sorting. First, patients or schedulers must be able to accurately assess their "riskiness" or the approximate value added of MLPs so they can be sorted to provider type which will provide the best quality care. Furthermore, insurance companies must provide financial incentives to direct patients to the proper provider

type. Skewed out-of-pocket costs could induce patients to rarely select the most cost effective provider type. Future work investigating the effects of these incentives could provide valuable insight into the policy changes needed to optimize sorting of patients between provider types.

1.8 Data Appendix

This section explains details about the creation of the estimation data sample.

To avoid changes in outcome measures driven by patients "aging out" of the sample into retirement and/or medicare, I limit to patients who are younger than 60 years old throughout the entire duration of the study.

Since some encounters with multiple providers may occur on the same patient "visit" where a patient is billed both for the MLP encounter and the MD encounter, I limit to cases where patients were either seen by an MLP and not seen by an MD on the same day, or seen by an MD and not seen by an MLP on the same day.

A common occurrence in the claims data is that claims will be "adjusted" after initial submission by the insurance agency or the medical providers. For example, suppose an initial claim is filed with payment amount Z. After negotiations between the insurer and the medical provider, the payment amount is negotiated to a different amount X = Z + Y, where Y is the "adjustment" between the two submitted claims and may be negative. To reflect this alteration, another claim is filed with the identical medical information, but with a new payment amount Y. This process may continue for several iterations over the same encounter. I handle these adjustments by collapsing the claims so that each encounter is only represented once, with the payment amount equal to the sum of the payment variables from each of the claims submitted regarding the encounter. Thus, each observation in the data is an encounter with payment equal to the final amount paid by or on behalf of the patient by his or her insurance agency.

The estimation sample is a subset of all patients in the full data, created in the

following way. First, I limited to patients in states which changed their SOP laws between 2004 and 2015, who had at least one medical encounter with a general practice physician or a nurse practitioner. Then I took the full history of all claims for this group of patients. I use the full history for 25 percent of these patients for the estimation, and a different 25 percent for the training of the machine learning algorithm. The remaining 50 percent of the patients are reserved for future robustness analyses. I include all outpatient claims, all inpatient claims, and all prescription drug claims which constitute my three master data sets.

I then collapse each of the data sets to correct for payment adjustments as described above, and create a new data set that contains just the patient identification number and the dates of encounters with general practice physicians or nurse practitioners. I use this as the skeleton for the final estimation sample, merging on outcome variables created using all three master data sets (total medical spending in the year following the MD/ND visit by or on behalf of the patient, and whether or not the MD/ND visit was followed by a preventable hospitalization in the next year). I also merge on control variables created from the three master data sets, such as lagged information about spending, the total unique number of diagnostic categories, the total number of hospitalizations, ER visits, and prescription drug claims; as well as information about the MD/ND visit including patient age and gender, major diagnostic category, facility type, and type of insurance plan. Thus, the result is a data set where each observation is an encounter with an MD or an ND, and each column is a characteristic of that encounter, previous encounters by the patient, or future medical information. I use the same set of patients to estimate the overall traditional

econometric results as well as the final HTE estimates.

1.9 GRF Appendix

The generalized random forest (GRF) is a forest-based weighting algorithm that partitions data by using recursive axis-aligned splits to maximize the heterogeneity of the treatment effect estimates between partitions. This process is repeated across subsamples of the data to create a set of partition rules, which are used to create a vector of weights that establishes the similarity of any two observations. This weighting vector is used to solve a weighted local maximum likelihood optimization problem which provides an estimate for the treatment effect for each observation.

The specific process begins by taking a sample I from the full training data set, where I is s percent of the training data. I cluster at the patient level, so each sample I contains all information for each of the patients in that sample. I is then further split into two equal sized subsets J_1 and J_2, again clustered at the patient level. Subset J_1 is used to grow a regression tree T (see process explanation below), which returns a partitioning rule that fully partitions data J_1 into terminal nodes or "leaves". Next, the algorithm applies the partitioning rule from T to the other subsample J_2, so that J_2 is fully partitioned into leaves. I repeat this process until I reach the total number of trees, B, resulting in B partitioning rules on B (potentially overlapping) subsets of the training data, each of size $s/2$. The partitioning rules are based on the independent variables of each observation.

Then I turn to the testing data set. For each observation x in the testing data set, I "insert" x into each of the B partitioning rules so that x is in B leaves each of

size N_b, where b indexes the specific partitioning rule. For each rule $b = 1, \ldots, B$ I assign a weight equal to $1/N_b$ to all points in the set of points in the corresponding J_2 subsample that are in the same leaf as x. Thus, each observation in the training data has a vector of weights of length B corresponding to the single observation x in the testing data. I then take the mean of all B weights for each observation in the training data, and use the means as the final weight vector, which I then use to solve a local maximum likelihood problem to estimate a treatment effect estimate specific to the observation x. I then repeat this process for each observation in the testing data set, so that each observation in the testing data has a treatment effect estimate.

To grow each tree, the algorithm first proposes a split based on the independent variables (such as $Age > 40$). For each proposed split, I solve a local estimating equation in both of the resulting child nodes. The estimating equation is a local maximum likelihood problem which gives a treatment effect estimate in each child node. The proposed split with the largest difference between in treatment effect estimates between the child nodes is selected as the actual split. The process is then repeated on each of the child nodes, until either of the resulting nodes has no treatment group or control group observations or any proposed split does not increase the heterogeneity in treatment effect estimates between the two child nodes.

To understand which variables drive the prediction estimates, I regressed the first stage predictions on the predictor variables, without any non-linear terms or interaction terms. This approach misses an important feature of the GRF, namely its ability to capture non-linearities and interactions in the data, but the linear ap-

proach can give an approaximation for each variable's explanatory power. I show the 20 variables with the smallest p-values in the regression in the right hand column of table 1.6 (with the lowest p-values at the top and the largest at the bottom). The left column of table 1.6 shows the least important predictor variables (as determined by the p-values). Patient employment industry, age, and gender are important predictors of switching to NDs. Facility type and some (but not all) specific information about a patient's medical history are the least important predictors.

Table 1.6: Variable Importance

Least Significant	Most Significant
mdc 3 last year	outpatient unique mdc 8 count last year
thergrp 31 count last year	outpatient unique mdc 19 last year
any hosp last 3 years	deduct
thergrp 7 last year	mdc8
preventable hosp last 3 years	industry2
outpatient unique spec 430 count last year	tot outpatient spending last year
ER mdc 0 count last year	outpatient unique mdc 9 last year
ER mdc 3 count last year	monthly mean outpatient spending last year
mdc 14 count last year	mdc3
thergrp 30 count last year	industry3
mdc 22 last year	outpatient unique mdc 8 last year
ER mdc 10 count last year	industry5
outpatient unique spec 575 count last year	outpatient unique mdc 3 last year
(Intercept)	tot outpatient spending spike last year
facility type18	male
facility type95	in network
facility type98	outpatient unique mdc 23 last year
facility type42	industry6
facility type49	AGE
facility type35	industryNA

Note: This table shows the 20 least "important" variables (left column) and the 20 most "important" variables (right column). Importance here is determined by the p-values in a linear regression of the first stage GRF prediction on all predictor variables.

CHAPTER 2

PHARMACY DESERTS AND MEDICATION ADHERENCE

2.1 Introduction

It is estimated that each year, the United States health care system spends approximately $290 billion (2.3 percent of GDP) on care *that could have been avoided* had patients increased their adherence to prescription drug regimens (Cutler and Everett 2010). Furthermore, studies have shown that increased medication adherence would reduce deaths: Cutler et al. (2007) found that moderate increases in anti-hypertensive (heart medication) drug adherence alone would reduce annual premature deaths by roughly 89,000. Yet, non-adherence persists, often at rates around 40 percent, even among patients with no medication cost sharing (Doshi et al. 2009). Poor medication adherence rates combined with massive associated costs motivate this paper.

This paper examines the effects of a potential driver of non-adherence: geographic access to pharmacies. I show that distance costs contribute significantly to prescription drug refill decisions. Pharmacy access is not universal; previous studies document "pharmacy deserts" (neighborhoods with low access to pharmacies) concentrated in low income, minority communities (Qato et al. 2014). Using data from the American Community Survey and the National Provider Identification database, I roughly estimate that around 10.3 million Americans live more than 10 miles from a pharmacy. While this estimate does not consider distance from workplace to pharmacy or housing decisions, it still suggests that some individuals pay higher travel costs to reach the pharmacy than others. To examine the effects of travel costs on adherence in this paper, I study the effect of openings and closings of pharmacies and variation in pharmacy-insurance network status.

The main data source I use is the Oregon All-Payer Claims Data (OAPCAD or

APAC data). I use only the pharmacy claims portion of the data from 2011 to 2013. This data provides de-indentified claim-level details about prescription drug patients, prescription fills, and pharmacy locations. It contains over 86 million claims from over 2.9 million Oregon residents. I observe all pharmacy insurance claims made by each individual in the data regardless of where they fill their prescriptions or their insurance provider. I observe patient home zip code and pharmacy exact address, which I use to construct distance measures. For each claim, the data includes the number of pills dispensed, the number of days covered by the prescription, the type of drug, the copay, the amount paid by the insurance company, the day the prescription was filled, and the pharmacy that filled the prescription.

I exploit the openings and closings of pharmacies as changes to the local pharmacy choice set. I take an event-study approach to the 36 openings and 17 closings I observe in the data. To measure adherence, I calculate the total pills dispensed to patients from a given zip code, the total number of unique patients living in the zip code, the total number of claims filed by patients living in the zip code, and the total number of new patients from the zip code. I do not observe whether the patient actually consumed the medications, but the measures I use are straightforward and are prerequisites for consumption. I find that openings cause roughly a 2 percent increase in pills dispensed, in the number of patients, and in the number of claims, and a 3 percent increase in the number of new patients. Closings do not show significant effects on the outcomes.

I argue that given my set of controls, pharmacy entry/exit into a given area is plausibly exogenous. I verify this argument by documenting flat pre-trends in

the outcome variables prior to the openings and closings conditional on covariates. Furthermore, I show these conditionally flat pre-trends in the outcome variables for patients who live in the zip codes where the pharmacies open, regardless of where the patients fill their prescriptions before or after the opening or closing.

Next, I investigate the heterogeneous effects of openings/closings on different types of patients, drugs, insurance plans, and local pharmacy market conditions. To highlight the results, I find that antihypertensives have about a 3 percent increase in the outcome variables after the openings, and that Medicare Advantage enrollees have about a 15 percent increase in the outcome variables.

Mail order prescriptions are a possible solution to pharmacy access problems, but mail order pharmacies are heavily regulated (Arruñada 2004), and are often more expensive than in-store prescriptions. Mail order prescriptions make up 10 percent of all claims in the OAPACD, though this number can be misleading since mail-order prescriptions are frequently dispensed in 90 day increments versus the usual 30-day increments for in store prescriptions. I examine the heterogeneity of opening/closing effects by mail-order status and find that openings have roughly no effect on mail order prescriptions, and larger effects when mail order prescriptions are removed.

Beyond geographic proximity to pharmacies, another important component to pharmacy access is insurance coverage. Even if a patient has many nearby pharmacies, they do her little good in filling prescription drugs if none accept her insurance. I exploit this fact by using variation caused by the Walgreens - Express Scripts network status changes in 2012. Walgreens is the one of the nation's largest pharmacy chain stores, and Express Scripts is one of the nation's largest pharmacy benefit

management companies. Prior to January 2012, Walgreens was in Express Scripts' network, allowing patients covered by Express Scripts to fill their prescriptions at Walgreens. However, in January of 2012, due to failed negotiations between the two firms, Walgreens left Express Scripts' network, essentially closing thousands of pharmacies across the country for millions of patients. Then, in June of 2012, a deal between the two firms was reached, allowing Walgreens to re-enter the Express Scripts network in September 2012. I use this variation to measure the effects of restricting access to pharmacies on patient medication adherence. I find that the separation reduced pills dispensed, total patients, and total claims by roughly 10 percent, with increases nearly back to baseline after re-entry.

By using the variation in Walgreens'-Express Scripts network status, I am able to examine 65 more events that have essentially the same effect as store closings. The exogeneity of this event is plausible, given that negotiations between the two firms continued in the latter half of 2011 and throughout 2012. As late as December 2011, patients were unsure if Walgreens would leave the Express Scripts network. The insurance type variables available in the Oregon all-payer claims data allow me to narrow in on patients who fill their prescriptions using coverage by a pharmacy benefit manager only, though I cannot identify if they are exactly using Express Scripts. Since Express Scripts is the largest pharmacy benefit manager in the nation, handling over 25 percent of prescriptions nationwide, I assume that the share of pharmacy benefit only insurance patients affected by the separation is high.

Einav et al. (2016) showed the overall prescription drug elasticity to be roughly -0.24; a one percent increase in drug price leads to a 0.24 percent decrease in quantity

demanded. If their results were to be interpreted symmetrically, so that a one percent *decrease* in price led to a 0.24 percent *increase* in quantity, and assumed to be linearly scalable, then taken together with my findings suggests that local pharmacy openings have roughly the same effect as an 8-10 percent decrease in prices, and removing Walgreens from the Express Scripts networks has roughly the same effect as increasing prices by roughly 40 percent.

To make my results more meaningful for national policy, I use data from the American Community Survey and Zip Code Business Patterns data from the Census to predict out-of-sample effect sizes of opening additional pharmacies in each zip code nationwide. First, I find zip code specific effects from each of the above estimation procedures, then I use a LASSO approach to find significant predictors of the effect sizes using ACS and ZBP data, and finally I extrapolate the effects for all zip codes in the United States. I find a median effect size of pills dispensed of roughly 3 percent, with a standard deviation of about 40 percentage points.

2.2 Literature

This paper is part of a growing literature that combines an economic framework with geospatial data and methods. A 2013 Science magazine article highlights the turn of health care studies to spatial methods, focusing on the ability of spatial methods to combat the spread of disease (see Richardson et al. (2013)). For example, Shen and Hsia (2016) examine the geographical distribution of emergency room closures and the impact on hospitalizations. Petek (2016) examines the effects of hospital openings and closings on patient health and spending, while Buchmueller et al. (2006) focuses

on the impact of hospital closings. My study contributes to this literature by focusing on the geographical access to pharmacies, a crucial aspect of the health care system. Pharmacy choice is similar to grocery store choice, an area that has been studied recently.

Several recent studies have examined food deserts and the role of access to grocery stores in consumer food purchasing patterns (Handbury et al. (2015), Taylor and Villas-Boas (2016), Chenarides et al. (2016)). Other studies have examined the factors influencing store choice. Briesch et al. (2009) examines how the different purchasing options available at a given store influence consumers to visit that store. Hillier et al. (2015) illustrates a discrete choice approach to store choice. Relatedly, Kremer et al. (2011) studies the relative willingness to pay for upgrades to water springs in Africa, showing that individuals are willing to trade distance costs for improved health. While those studies largely focus on grocery store availability and choice, I focus on pharmacy availability and the consumption of prescription drugs. A key aspect of prescription drug behavior is its impact on total medical spending.

Multiple studies have drawn the connection between medication adherence and total medical spending. Roebuck et al. (2011) shows that total medical spending decreases despite increased prescription drug spending. Cutler et al. (2007) highlights the value of adherence on patient health, as mentioned above. Encinosa et al. (2010) shows that increased adherence to diabetic drugs reduces hospitalizations among certain populations.

Medication adherence is a widely studied aspect of health care, with many studies focusing on factors that contribute to nonadherence. Several papers have studied the

effect of monetary cost-sharing on adherence (for example Eaddy et al. (2012), Huckfeldt et al. (2015), Doshi et al. (2009), Zhang and Meltzer (2016), Dor and Encinosa (2010)). Other papers examine other factors relating to adherence such as Cardon and Showalter (2015) who show that increased advertising efforts increase adherence and Doshi et al. (2016) who show that when patients synchronize their prescription refills, adherence increases. Koulayev et al. (2013) and Osterberg and Blaschke (2005) examine other factors contributing to medication adherence measurement of nonadherence. Carroll (2014) provides a comparison of drug prices between retail and mail order pharmacies for Medicare Part D prescriptions. Finally, Egan and Philipson (2014) presents the theory that patients adhere to medications when the medications perform better. My paper is similar to these studies, but draws on a different cost of filling prescriptions: the distance and time costs. This relates to Becker (1965), in that I am assuming their are positive time costs for the filling of prescriptions, about which patients must make trade-off decisions.

2.3 Data

The main data source for this project is the Oregon All-Payer Claims Database pharmacy claims file from 2011 to 2013. The data includes claim-level information on patients, insurance, and prescription details (such as how many pills are provided with the prescription and the type of pill provided). The data also includes the pharmacy National Provider Identification (NPI) number, which I use to link to the NPI database, a data source that contains pharmacy address and enumeration date for each pharmacy in the United States. Pharmacy enumeration date usually

occurs 3-5 months prior to the first prescriptions being filled at a pharmacy, so I instead use the first date the pharmacy fills a claim in the claims data as its open date. I use population data from the American Community Survey (ACS) to weight the regressions by the total population in each zip code. Summary statistics of the cleaned data are provided in table 2.1.

Table 2.1: Raw Data Summary Statistics

Total Claims	86,433,734
Unique Patients (OR)	2,910,666
Claims per patient	30.12
	(54.47)
Pharmacies per Patient (OR)	1.79
	(1.26)
Patients per Pharmacy (OR)	5,747.58
	(11,322.88)
Age	56.23
	(16.61)
Copay	8.02
	(32.66)
Pills Dispensed	54.32
	(57.08)
Days Covered	34.31
	(24.18)
Oregon Patients	0.99
Oregon Pharmacies	0.84
Private Insurance	0.47
Medicare	0.20
Medicaid	0.20
Other Insurance	0.13
Mail Order	0.10

Note: This table shows the means, standard deviations and shares of the given variables in the claim-level OAPCD.

The data contains about 86 million claims, of which 99 percent were filled by Oregonians (about 2.9 million different patients from Oregon). Patients filled an average of 30 total claims each, and the average patient visited just under 2 different pharmacies (defined by pharmacy physical address). A high standard deviation on the number of claims per patient indicates that some patients filled many claims, while others filled few. Pharmacies had an average of 5,747 patients, but high standard deviations on the number of patients per pharmacy illustrates the difference between urban and rural pharmacies. The average age of patients is 56 years old. Claims had an average copay of $8, but, again, a high standard deviation suggests the wide divide in drug costs between drug types and brands/generics. The average number of pills dispensed per claim was 54, and the average number of days covered on each claim was 34 (with most claims having either 30 or 90 days covered). The data contains claims filled at any pharmacy, by patients from any state, provided the patient has insurance coverage in Oregon. Claims were filled 84 percent of the time at pharmacies located in Oregon. Claims covered by private insurance carriers make up 47 percent of the data, while Medicaid and Medicare each covered 20 percent of the claims in the data, respectively.

This data is ideal to examine the effect of pharmacy access on adherence in at least three ways. First, the data has very narrow geographic identifiers-I observe the patient home zip code and the pharmacy exact address. This allows me to examine effects of pharmacy entry or exit on individuals who are most likely impacted: those that live very near to the opening/closing pharmacy. Second, the data has the universe of insurance prescription claims from Oregon-insured residents, which

constitutes the bulk of prescriptions in the state. This is useful for understanding changes in patient behavior as well as changes in firm revenue. Finally, the data provides de-identified patient codes which allow me to follow individual patients throughout the duration of the data, regardless of who their insurer is, what type of prescription they fill, or where they fill it (provided they fill the prescription through their insurance company).

The data has a few important limitations. First, since it is insurance claims data, I do not observe any transactions that are paid for purely out of pocket. So, for example, if a patient pays for a prescription with cash, without billing her insurance company, the transaction will not show up in the data. This is not an issue for my estimation of the effects of pharmacy openings unless there is a systematic shift in the share of patients paying only with cash around the time of the opening. If patients switch from insurance to cash only payments around the time of the opening, then my results understate the effects of the opening. If, however, patients use the opening as a chance to switch from paying cash to billing their insurance company, then my results will overstate the effects of the opening. This is possible for example, if, prior to a local opening, a patient uses a pharmacy that does not accept her insurance, but then switches to the new pharmacy *because* the new pharmacy accepts her insurance. In general cash-only payments are not a high share of prescription fills, and so should not greatly impact my results.

Prescription drugs are often very expensive to purchase entirely out of pocket. I assume that the frequency of patients switching from cash-only to insurance payments for non-cost related reasons is low. Thus, the marginal patient who pays only

cash but then switches to insurance after an opening is likely benefiting from a very large decrease in distance costs after the opening. That is, I expect the patient who switches from cash-only to insurance after the opening to be paying cash-only prior to the opening *because* the distance costs to reach the closest pharmacy that *does* accept her insurance (plus any insurance copayments) are higher than the out of pocket payment for the prescription at the new pharmacy. Though this presents an issue for measuring the effects of the opening on the intensive margin of existing patients, it highlights the importance of distance costs in pharmacy choice.

To address this issue, I include as an outcome variable the number of "new" patients. This variable will capture patients filling prescriptions for the first time and patients switching from cash only payments to insurance based payments. If openings are able to draw patients from either pool (those that have never filled before, and those that have only filled with cash) this will highlight the salience of the distance costs.

The absence of cash-only payments also has implications for my results of the effects of Walgreen's leaving the Express Scripts network, since Express Scripts patients could adjust to the change simply by paying with cash at Walgreens. I expect this switch to occur more (if at all) in patients for whom paying cash-only is easier than switching pharmacies or filling less prescriptions.

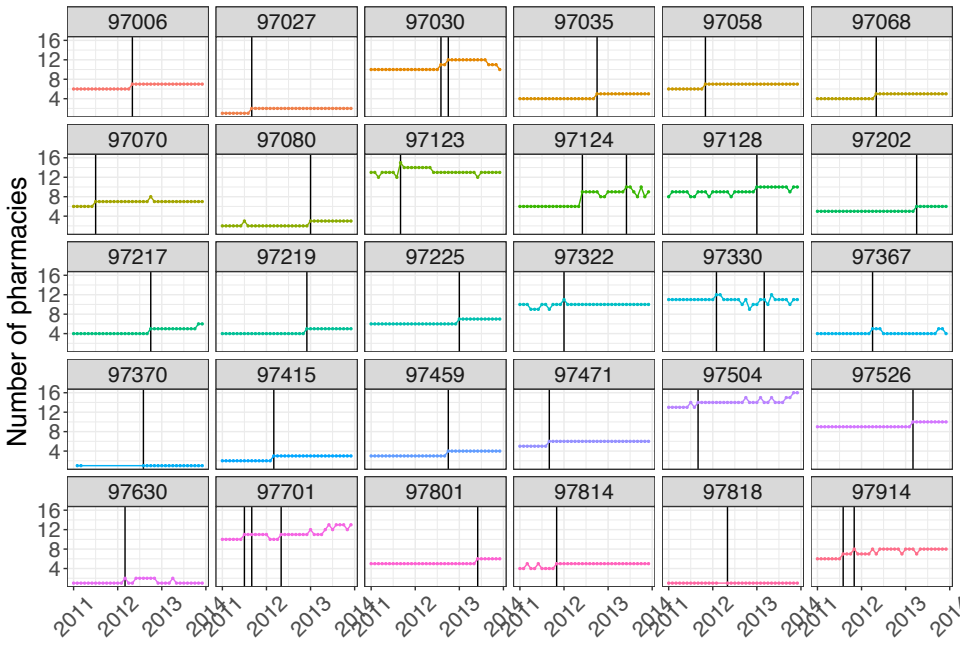
A second limitation in the data is that I do not observe anything about the prescribing physicians. I am not concerned about systematic shifts in the prescribing behavior of physicians, since I measure outcomes in the patient's home zip code, allowing the patient to visit any pharmacy before and after the opening. So, if

physicians somehow systematically started sending patients to the new pharmacy, but patients did not change their prescription filling patterns, I would observe no change in the outcome measures around the time of the opening. It is possible that physicians are writing the same number of prescriptions before and after the opening, but that patients are not filling their prescriptions prior to the opening, and then begin filling them after the opening. This is consistent with patients responding to changes in distance costs, but, since I do not observe physician prescribing rates, I do not know the number of unfilled prescriptions. The new patients outcome measure will help with understanding the extensive margin effects, assuming patients do not systematically change the frequency with which they leave their prescriptions unfilled around the opening of the pharmacy for some confounding reason.

I show the events used in the openings/closings analysis in figures 2.1 and 2.2. These events limit to a 6 month balanced panel of opening/closing pharmacies in Oregon, using the first appearance of the pharmacy address as the open date and the last appearance of the address as the closing date. I further require that all opening pharmacies are actual openings by using only those pharmacies that appear in the data during their opening month and also at least six months after their opening month, and similarly for closing pharmacies. The main results exclude pharmacies that open in zip codes where no patients live. Requiring 6 months before and after the event is an arbitrary choice; other balance requirements led to similar results. Reducing the balance requirement increases the number of usable events, but decreases the time frame that can be analyzed. A 6 month window is long enough for patients to fill most maintenance prescriptions at least four times. There are 36

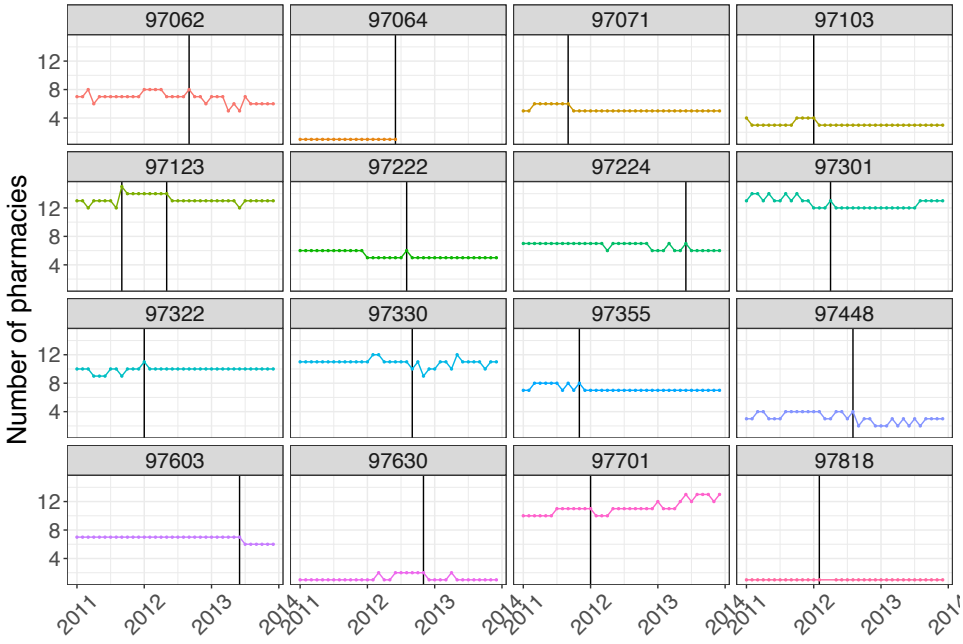
different opening events in 30 different zip codes, and 17 closing events in 16 different zip codes. It is possible that pharmacies are simply moving across the street, or are opening in the same zip code and the same month as another pharmacy closes. Both of these situations imply that I am understating the true effect of the openings and closings.

Figure 2.1: Opening Events



Note: This figure shows the number of pharmacies in zip codes in months relative to opening events.

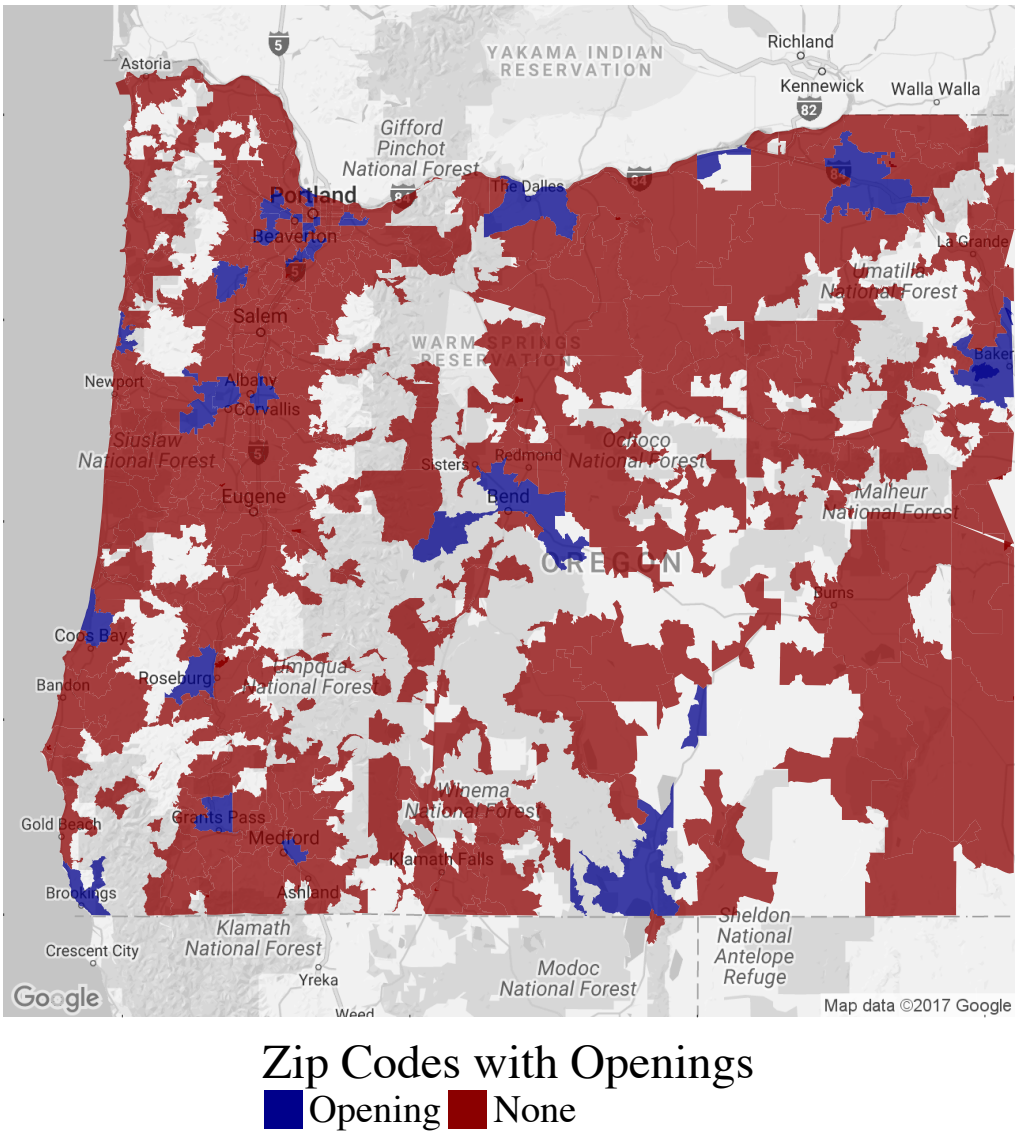
Figure 2.2: Closing Events



Note: This figure shows the number of pharmacies in zip codes in months relative to closing events.

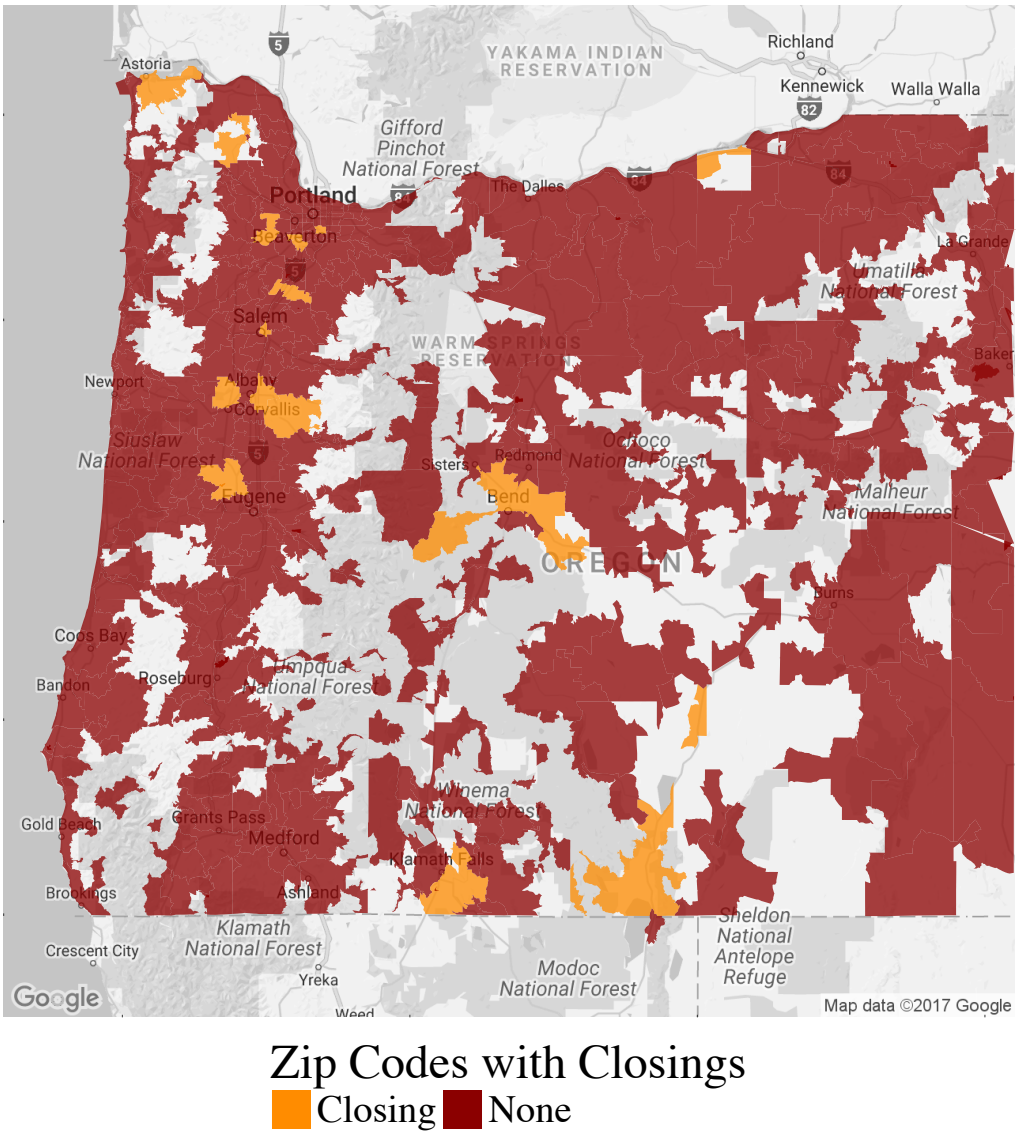
Figures 2.3 and 2.4 map the geographic distribution of the openings and closings used in the event studies. Most of the population of Oregon lives in the I-5 corridor - a corridor running from Portland in the North to Medford in the South along the Western third of the state. Outside this corridor, the major population center is Bend, located in central Oregon. South-Eastern Oregon is largely desert, with low population levels. There are many national and state parks in Oregon. These areas are shown colored gray in the maps - showing that those areas have no associated zip code.

Figure 2.3: Map of Openings



Note: This figure shows the zip codes in which new pharmacies opened.

Figure 2.4: Map of Closings

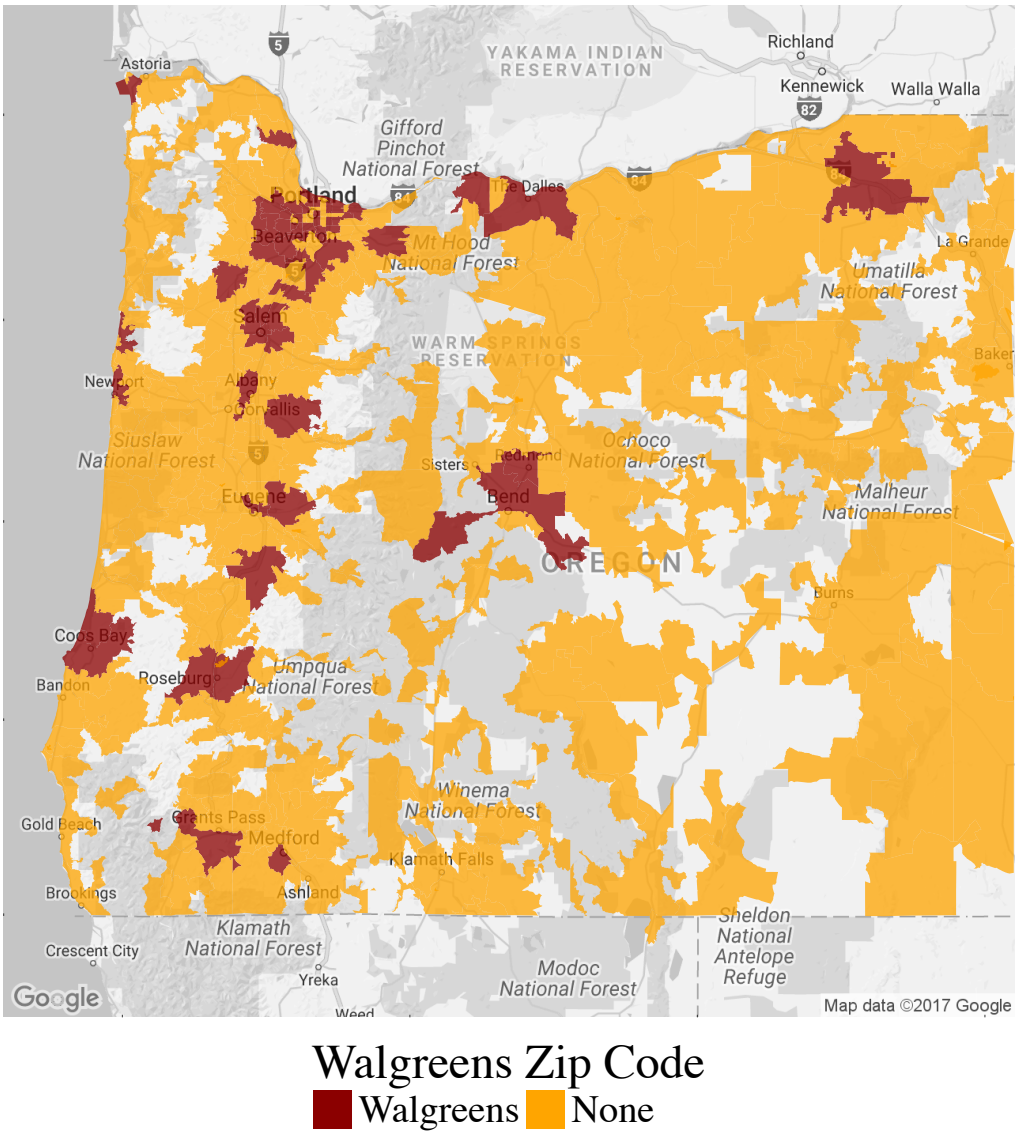


Note: This figure shows the zip codes in which pharmacies closed.

Figure 2.5 shows the geographic distribution of Walgreens stores in Oregon, and

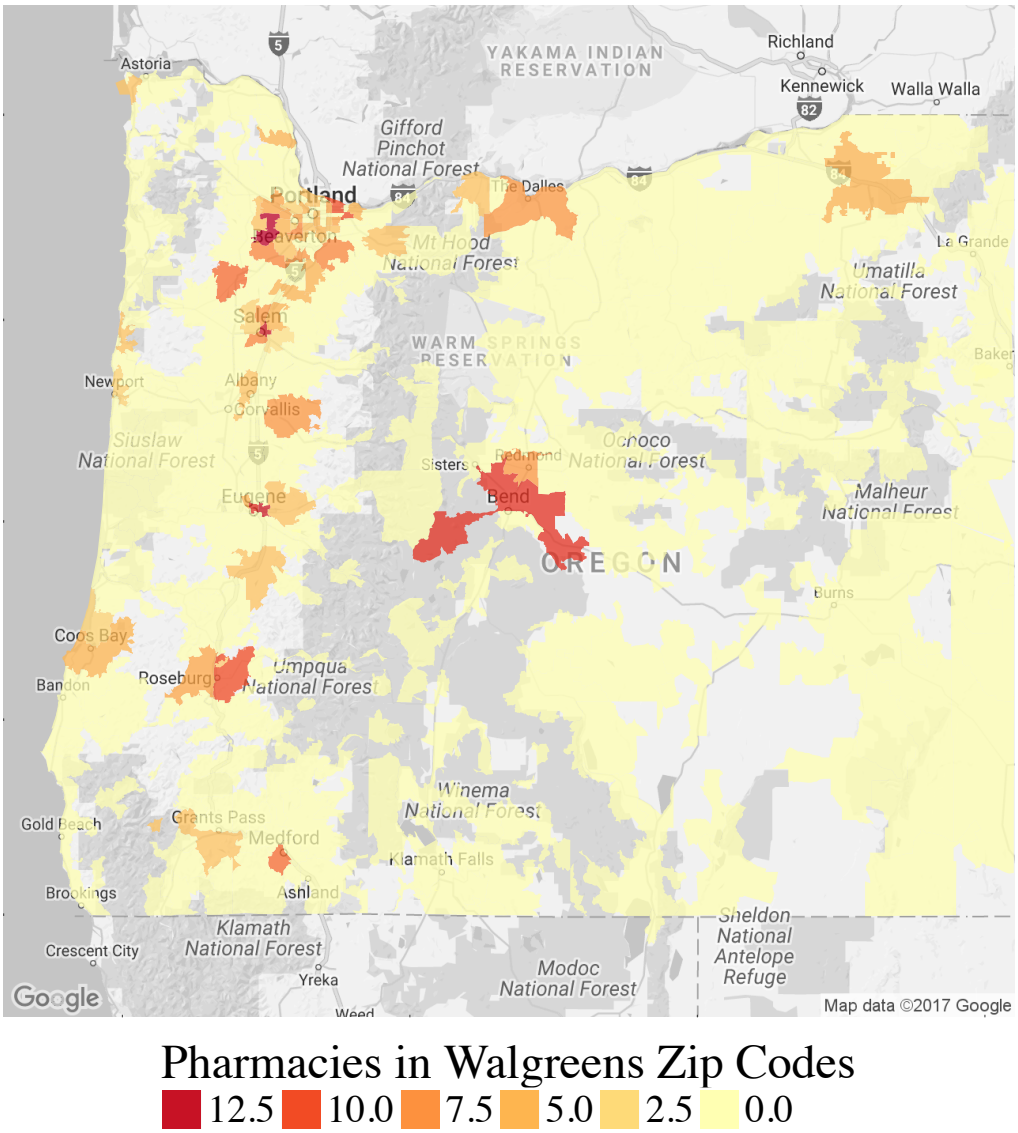
figure 2.6 colors the Walgreens zip codes by the total number of pharmacies in the zip code, with the lightest color indicating a non-Walgreens zip code.

Figure 2.5: Map of Walgreens Zip Codes



Note: This figure shows the zip codes in which Walgreen's pharmacies are located.

Figure 2.6: Number of Pharmacies in Walgreens Zip Codes



Note: This figure shows the number pharmacies in zip codes in which Walgreen's pharmacies are located.

Table 2.2 shows the summary statistics at the zip code-month level of the claims data split into before and after pharmacy opening/closing subsets. It shows that the opening zip codes differ from the zip codes with no openings in several ways. Notably, the number of unique patients is significantly higher in opening zip codes than in non-opening zip codes. This difference is likely due to the higher populations of opening zip codes relative to non-opening zip codes. To compare patient behavior across zip code types, I scale the other outcome variables by the number of patients in the zip code-month. After scaling, the prescription drug behavior in the two types of zip codes is much more similar. Each type of zip code is roughly the same in other observables such as the share of patients on Medicaid, the average copays, and average age.

Table 2.4 summarizes empirical facts about Walgreens in Oregon. There are 77 different Walgreen stores in Oregon, in 65 different zip codes. The total number of pharmacies of any type in Oregon is 790. Over 500,000 patients ever shop at a Walgreens in Oregon, and over 1.5 million patients live in one of the 65 Oregon Walgreens zip codes. Since the OAPAC data does not explicitly indicate which pharmacy benefit management company a patient uses, I limit to patients with insurance that is "only a pharmacy benefit manager". This does not capture all of the Express Scripts customers, but since Express Scripts is the largest pharmacy benefit manager in the nation and handles a large percentage of all prescription drug claims, I assume that patients with coverage that is only through a pharmacy benefit manager are likely to have Express Scripts as their provider. Thus, table 2.4 also reports that there are roughly 156,000 patients living in Oregon Walgreens zip codes

Table 2.2: Pre- and Post-Opening/Closing Summary Statistics

	Opening Zip Codes (Pre)	Opening Zip Codes (Post)	Non-Opening Zip Codes
Age	56.011	56.633	57.685
	(1.746)	(1.931)	(4.582)
Share Medicaid	0.172	0.171	0.196
	(0.081)	(0.075)	(0.105)
Copay	9.243	7.832	7.971
	(5.572)	(2.235)	(15.601)
Total Patients	5,187.111	5,436.699	1,080.349
	(2,321.133)	(2,845.388)	(1,951.177)
Pills (over total patients)	177.695	181.943	186.314
	(22.656)	(23.303)	(45.646)
Claims (over total patients)	3.367	3.402	3.47
	(0.433)	(0.443)	(0.71)
New Patients (over total patients)	0.157	0.087	0.116
	(0.215)	(0.061)	(0.17)
Pharmacies (per 1,000 total patients)	1.252	1.497	0.512
	(0.621)	(0.658)	(2.013)
	Closing Zip Codes (Pre)	Closing Zip Codes (Post)	Non-Closing Zip Codes
Age	56.143	56.238	57.639
	(1.958)	(2.199)	(4.496)
Share Medicaid	0.176	0.173	0.196
	(0.067)	(0.058)	(0.105)
Copay	9.647	8.071	7.988
	(7.343)	(2.698)	(15.292)
Total Patients	4,924.446	5,353.987	1,254.294
	(2,834.036)	(2,922.045)	(2,117.743)
Pills (over total patients)	179.694	185.543	186.062
	(20.656)	(19.975)	(44.934)
Claims (over total patients)	3.413	3.405	3.469
	(0.37)	(0.322)	(0.703)
New Patients (over total patients)	0.155	0.085	0.116
	(0.216)	(0.059)	(0.17)
Pharmacies (per 1,000 total patients)	1.425	1.223	0.53
	(0.489)	(0.501)	(1.96)

Note: This table shows the means and standard deviations of the given variables split by opening/closing status. The zip codes with openings or closings that were too close to the start or end of the data to have balanced panels are excluded from this table.

with "pharmacy benefit manager only" insurance. In table 2.5, I report the share of claims at the 6 largest pharmacy types identified in the data. Walgreens makes up nearly 10 percent of the claims, while around 60 percent are filled at pharmacies other than the chains listed. Furthermore, roughly 2 percent of all claims are filled by patients with pharmacy benefits only insurance at Walgreens stores.

Table 2.3: Average Copays by insurance type and mail-order status

	Copay Means and SD	
Insurance Type	Non-Mail Order	Mail Order
Medicare Advantage	11.06	15.70
	(47.40)	(42.83)
Dual Eligibles	1.17	0.25
	(18.98)	(14.24)
Medicaid FFS	0.13	0.03
	(0.48)	(0.24)
Medicaid MCO	0.07	0.01
	(0.38)	(0.15)
Oregon Education	10.87	9.97
	(26.50)	(15.42)
Other Commercial	9.47	16.11
	(29.78)	(39.92)
Public Employees	6.57	11.25
	(15.25)	(26.80)
Unkown	10.83	8.09
	(109.74)	(46.10)

Note: This table shows the means and standard deviations of copays for all claims in the Oregon All Payer pharmacy Claims data, split by insurance type and mail-order status.

Table 2.4: Walgreens Summary

Walgreens in Oregon	77
Oregon Walgreens Zip Codes	65
Total Pharmacies in Oregon	790
Patients that shopped at Walgeen's at least once	532,345
Patients *only* shopping at Walgreens	228,338
Patients living in Oregon Walgreens zip code	1,525,342
Patients living in Oregon Walgreens zip code and shopping at Walgreens at least once	350,199
Patients living in Oregon Walgreens zip code and *only* shopping at Walgreens	159,929
Patients living in Oregon Walgreens zip code with pharmacy benefit only insurance	156,435
Patients living in Oregon Walgreens zip code with pharmacy benefit only insurance and shopping at Walgreens at least once	41,906

Note: This table shows the counts of the given statistics in the OAPCD.

Table 2.5: Pharmacy Type Share of Claims

Other	0.59
Bimart	0.06
Safeway	0.10
Fred Meyer	0.10
Walgreens	**0.09**
Target	0.01
Walmart	0.04

Note: This table shows the share of all claims filled at each of the given pharmacy types, indicated in the OAPCD.

The main unit of analysis for the event studies is the patient home zip code. Thus, in the event studies where I limit to only those zip codes that had an opening/closing/Walgreens event, I am limiting to the set of patients *from* that zip code. This approach allows me to measure the outcome variables by patients *from* the zip code of interest *at any pharmacy they choose*. So, for example, prior to a pharmacy opening, patients in the opening zip code fill their prescriptions at several different pharmacies, in potentially several different zip codes. Then, after the opening, the

patients may still fill their prescriptions at other pharmacies as well as the new pharmacy. This unit of measurement, combined with flat pre-trends, implies that the effects I observe are not from pharmacies choosing to open where there is increasing demand for prescription drugs, but rather the effects are caused by pharmacy openings changing the behavior of local patients, presumably by decreasing the distance costs to visit the pharmacy.

I am able to identify the effects of pharmacy entry/exit provided there are not confounding factors that both influence prescription drug adherence and that systematically change around the time of the opening/closing. I limit concerns about confounding factors by including controls for observable factors that likely influence zip code aggregate medication adherence. Since older patients generally use more prescription drugs, I control for the average age of the patients in the zip code-month bin. Since monetary costs likely have large effects on adherence decisions, I control for the average copay in the zip code-month bin. I also control for the share of patients on Medicaid in the zip code-month bin as a rough measure of the local income level. Finally, I control for the number of pharmacies in the zip code in the month prior to the opening/closing to remove variation driven by market saturation.

To measure adherence I focus on four main outcome variables: total pills dispensed to patients from a given zip code, total number of patients from a given zip code, total pharmacy claims filed by patients from a given zip code, and total number of new patients from a given zip code. These measures are straightforward and are prerequisites to actual consumption of medications. Other adherence papers have focused on measures such as the medical possession ratio, the percentage of days

covered each month, or the number of days between the end of one prescription and the start of the next. None of these measures is able to capture whether patients are actually taking their medication, but each measure (including my measures) captures the amount of pills available to patients. By measuring the total number of patients, I also provide an economically meaningful measure that is relevant for policy makers and pharmacies seeking to maximize patient prescription fills.

2.4 Methods

2.4.1 Pharmacy Openings and Closings

To estimate the effect of pharmacy entry/exit, I begin with the following baseline specification:

$$log(Y_{zt}) = \gamma_z + \gamma_z t + \lambda_t + \sum_{\substack{\tau=-6 \\ \tau \neq -1}}^{6} \delta_\tau MonthsSince_\tau + X'_{zt}\beta + \epsilon_{zt} \tag{2.1}$$

Observations are at the zip code - month level, indexed by z and t respectively. I include a full set of zip code fixed effects (γ_z), and month fixed effects (λ_t). I include a zip code specific time trend ($\gamma_z t$) to account for trends in adherence that may induce phamracy entry or exit, and $MonthsSince_\tau$ is a set of indicator variables for τ months since the opening. Outcome variables include the total number of pills dispensed, the total number of pharmacy claims filed, the total number of unique patients, and the total number of new patients. Controls (X_{zt}) include the average

age of patients in the zip code - month, the average copay paid in the zip code - month, the share of patients on Medicaid, the number of pharmacies available the month prior to the opening, and the share of patients using mail-order pharmacies the month prior to opening. The coefficients on the $MonthsSince_\tau$ dummy variables give the effect of the opening/closing τ months since the opening/closing occured.

I obtain the final sample used in the baseline regressions by first removing outlier and miscoded observations, limiting to zip codes where an opening occurred, and by balancing the panel to include only openings with at least both 6 months before and 6 months after the opening. For zip codes with multiple opening/closing events in the same time window, I stack "overlapping" observations as in Lafortune et al. (2016).

To summarize the effects succinctly, I include a specification where I replace the $MonthsSince_\tau$ variables with a single indicator $Post_{zt}$ equal to one if the month is after the opening/closing in zip code z.

Drug Type

An important aspect of the effect of pharmacy entry is the type of drugs affected by the entry. To illustrate the heterogeneous effects by drug type, I add an indicator for drug type and an interact the months-since indicator with the drug type indicator. The coefficient on the interaction term gives the effect of the opening τ months since the opening for the given drug type.

Observations for this specification are at the zip code z, month t, drug d level. I define drug type by the drug's 2-digit therapuetic class code. As in the baseline

specification, I summarize the effects by replacing the months-since indicators with an indicator for after the pharmacy opening, and examine the correlations between the drug-specific effect sizes and characteristics about the drug such as average copay amount and average quantity dispensed.

Insurance type

Another important differentiation of the effects is by insurance types. This is important because of the different implications for social costs. Taxpayers pay for public insurance, and so should be more invested in the behavior of public insurance enrollees inasmuch as the behavior relates to health spending. Private insurance enrollees face higher premiums when their overall risk pool becomes less healthy. Thus, the medication adherence of public and private insurance enrollees is a critical concern for policy makers interested in reducing health costs. The Oregon pharmacy claims data includes three variables on insurance type, summarized in table 2.7. Each claim has some combination of the three variables. To measure the effects by insurance type, I run the same regression as in the drug type split specification, but now I split by insurance type instead of drug type.

Table 2.6: Share of Claims to Each Insurance Product Type

Self insured PPO	0.022
Medicare Advantage PPO	0.070
Commercial HMO	0.067
Pharmacy benefits only	**0.160**
Medicare Advantage HMO	0.093
Commercial PPO	0.124
Commercial POS	0.170
Medicare Part D	0.004
Self insured POS	0.011
Commercial indemnity	0.003
Medicaid disabled HMO	0.093
Medicaid restricted benefit HMO	0.057
Medicaid dual eligible HMO	0.018
Medicaid low income HMO	0.048
Medicare Cost	0.034
Special needs plan – dual eligible	0.021
Commercial EPO	0.003
Special Childrens Health Insurance Program (SCHIP)	0.001
Special needs plan – institutionalized	0.001
Unknown	0.001

Note: This table shows the share of all claims filled by each of the given insurance types.

"Deserts"

To measure the effects of opening a new pharmacy in a pharmacy "desert," I interact the $MonthsSince$ indicators with an indicator variable representing whether the zip code had 0 pharmacies in the month prior to the opening. Measuring "deserts" here is very imprecise for several reasons. First, patients may live far from the pharmacy located in their zip code, but close to an adjacent zip code's pharmacy. Second, zip

Table 2.7: Insurance Type Variables

Payer Type	Product Code	APAC Payer
Carrier	Medicaid dual elig. HMO	Medicare Advantage
Medicaid	Medicaid dis. HMO	Dual (Medicare+Medicaid)
Oth. Gov. Agency	Medicaid low inc. HMO	Medicaid fee-for-service
Third-party admin.	Medicaid restr. ben. HMO	Medicaid managed care
Unlicensed Entity	Medicare Adv. HMO	Medicare fee-for-service
	Medicare Adv. PPO	Public Emp. Ben. Board
	Medicare Cost	Oregon Educ. Ben. Board
	Commercial PPO	Other commerical payer
	Commercial POS	Unclassified
	Commercial HMO	
	Medicaid fee-for-service	
	Self insured PPO	
	Self insured POS	
	Self insured HMO	
	Pharmacy benefits only	
	Commercial indemnity	
	Commercial EPO	
	Commercial stop loss	
	Unknown	

Note: This table shows the insurance variables in the OAPCD and their possible values.

codes are not geographically uniform, so a small zip code and a large zip code may both have no pharmacies, but only the larger zip code actually has limited access. Nevertheless, the effects in these types of zip codes are interesting in that the distance costs may be more binding in zip codes with less initial pharmacies.

Adjacent Zip Codes

I further exploit the data's rich geographic identification by measuring the effect of the opening/closing events in zip codes adjacent to the zip code where the event occured. To do so, I group zip codes by the distance of their centroid to the centroid of each opening zip code, then I duplicate and stack the observations for zip codes that are adjacent to opening zip codes. I use the distance groups as indicator variables in the following regression:

$$log(Y_{azt}) = \gamma_z + \gamma_z t + \lambda_t + \theta_a + \sum_a \sum_{\substack{\tau=-6 \\ \tau \neq -1}}^{6} \delta_{a\tau}(MonthsSince_\tau \times \theta_a) + X'_{zt}\beta + \epsilon_{azt} \tag{2.2}$$

Where θ_a is a set of indicator variables specifying the adjacent zip code's distance group. The nearest group is "3 miles or less" and the farthest group is "11 miles or more". Intermediate groups are split at each additional mile. I drop all zip codes with centroids that are further than 12 miles from the centroid of an opening zip code. I repeated the estimations with differing sets of distance groups, and the patterns are robust to different groupings. Since each zip code can be in multiple distance groups depending on the opening zip code, zip code fixed effects and distance group

fixed effects are not perfectly collinear and can both be included in the regression. The distance group fixed effects control for factors consistent within distance groups but different across opening zip codes, such as travel time differences from traffic or terrain restrictions. In the regression, $\delta_{a\tau}$ gives the effect of the opening τ months since in the opening in the distance group that is a miles from the opening zip code. If distance costs are driving the results, I expect the opening effect to decrease for further distance groups. I repeat this estimation replacing the $MonthsSince$ indicator variables with a $Post$ indicator variable for if the observation is after the opening.

2.4.2 Walgreens vs Express Scripts

Pharmacy-insurance networks can severely limit patient access to pharmacies, regardless of patient-pharmacy proximity. I use this fact to exploit the failed negotiations between Walgreens and Express Scripts in 2012 to study the effect of pharmacy closings. Walgreens is one of the nation's largest pharmacy chain store with over 8,000 stores nationwide, and 77 stores in Oregon between 2011 and 2013. Express Scripts is one of the largest pharmacy benefits managers in the nation, covering over 85 million patients nationwide, filling over 1.4 billion 30-day prescriptions annually.

In late 2011, Walgreens and Express Scripts were unable to reach an agreement that would allow Walgreens to remain in the Express Scripts network. Thus, on January 1, 2012, Walgreens left the Express Scripts network. This essentially closed thousands of pharmacies to millions of patients nationwide, as Express Scripts enrollees would no longer be able to fill their prescriptions at Walgreens. In June of

2012, the two companies reached a deal and Walgreens was readmitted to the Express Scripts network in late September 2012.

I exploit this event by examining the effect of the separation on patients who live in the same zip code as a Walgreens location. The OAPCAD does not have specific information on patient insurance to indicate whether the patient was actually covered by Express Scripts, so I limit to patients with insurance covered by a pharmacy benefit manager only as a rough measure of facing the full effects of the separation. Then I estimate the baseline regression specification, initially limiting to patients only in Walgreens zip codes. To avoid concerns about ommitted factors that occur at the same time as the network exit, I then run the specification with all zip codes included, but I interact the $MonthsSince$ indicators with an indicator for Walgreens zip code. I also examine the effects in larger time windows and with month-of-year fixed effects to control for seasonal effects, such as patients filling their prescriptions in abundance at the beginning of the year. Note that in this situation, the time fixed effects are perfectly colinear with the months-since variables, and so the time fixed effects are excluded.

As in the openings and closings framework, I summarize the effects by replacing the months-since indicator with an indicator for after the network exit similar to the opening/closing specifications. In another specification (equation 2.3), I expand the time window to include all months in the data to estimate rebound effects from re-entry:

$$log(Y_{zti}) = \gamma_z + \gamma_z t + \delta_1 Out_t + \delta_2 Returned_t + X'_{zt}\beta + \epsilon_{zti} \tag{2.3}$$

Here, Out_t is an indicator for the time period between January 1, 2012 and October 1, 2012 (representing the time Walgreens was out of the network), and $Returned_t$ is an indicator for if the month is after October 1, 2012 (when Walgreens returned to the network). Thus, δ_1 in equation 2.3 gives the effect of Walgreens leaving the network. The effect of Walgreens returning to the network, δ_2 in equation 2.3, should be 0 if there are no repercussions of Walgreens having spent 9 months out of the Express Scripts network. It's possible that this effect is negative if patients were able to substitute away from Walgreens to non-prescription drug alternative, or it might be positive if patients rush to Walgreens upon re-entry to make up for lost prescription fills.

I re-run these regressions including all zip codes, but in doing so, I interact the $Post_t$, Out_t, and $Returned_t$ variables with an indicator for if the patient's zip code contained a Walgreens.

2.4.3 National Prediction

To expand the scope of my results, I predict out-of-sample effects for the entire United States. I focus on the effects of pharmacy entry, thus the predictions can be interpreted as the effect of opening an additional pharmacy in each zip code in the United States. Though this is an unlikely policy, these results are still useful in understanding where pharmacy entry would have the largest effect.

I begin by finding zip code specific effects of entry in the OAPAC data. To do so, I estimate the following regression:

$$log(Y_{zt}) = \gamma_z + \gamma_z t + \lambda_t + \sum_z \delta_z (Post_{zt} \times \gamma_z) + X'_{zt}\beta + \epsilon_{zt} \tag{2.4}$$

This approach gives an opening effect, $\widehat{\delta_z}$, for each zip code in Oregon in which there was an opening event. Then, I use the estimated effect as the outcome variable in a variable selection regression (LASSO) including zip code measures from the American Community Survey (ACS) and Census Zip Code Business Patterns (ZBP) data from the year 2013. This approach provides a list of significant predictors and their coefficients, which I then apply to the nationwide ACS and ZBP data. This results in a predicted effect for each zip code in the country, based on observable characteristics of the zip code.

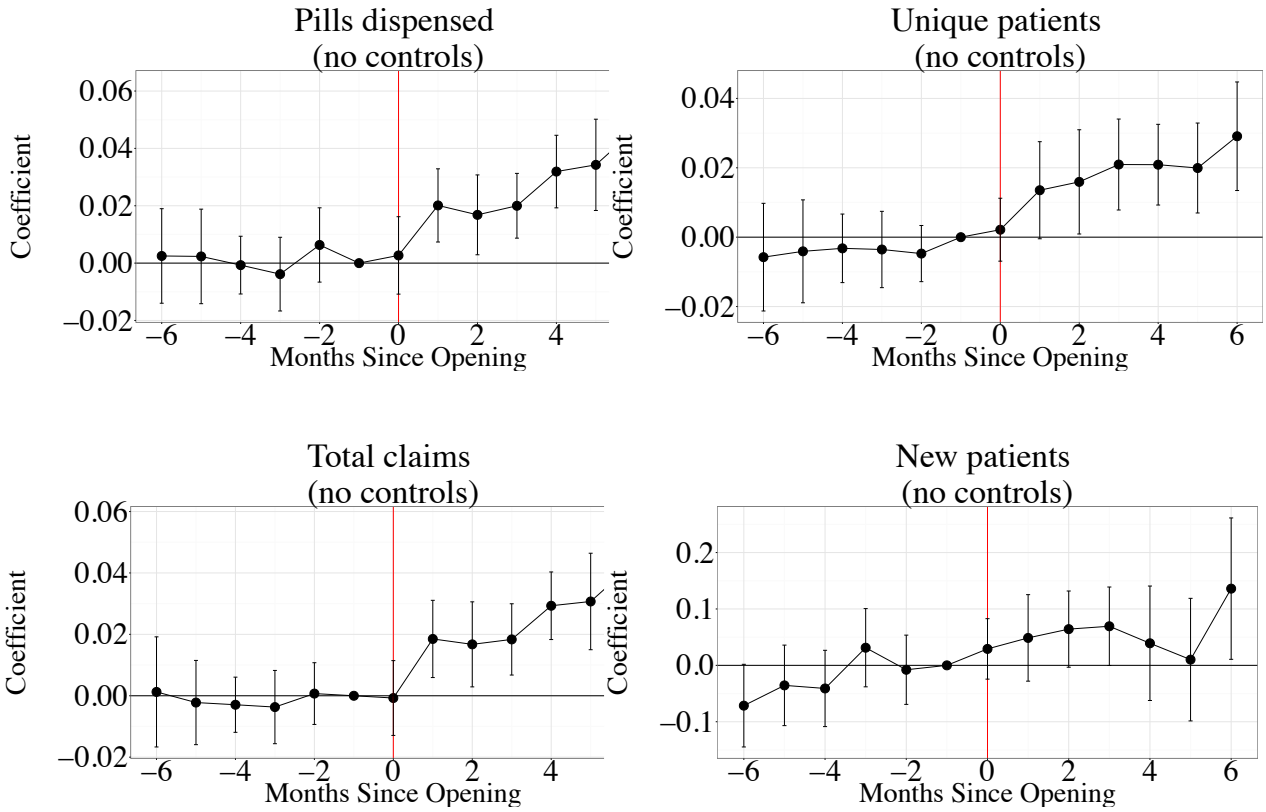
2.5 Results

2.5.1 Baseline Specification

Figure 2.7 plots the δ_τs from the baseline specification, with 95 percent confidence interval bars based on standard errors clustered at the zip code level. The omitted *MonthsSince* is -1, so the points can be interpreted as the effect of the opening τ months since the opening. Each of the outcome variables shows stable pretrends prior to the opening. The outcome variables are logs, so the effects are interpreted as percent changes.

Total claims and total pills dispensed have initial jumps of roughly 2 percent from the baseline level, followed by increases into the 4 percent to 5 percent range. The effect on the total number of unique patients is slightly less, showing an initial

Figure 2.7: Pharmacy Opening Output Regression Coefficients - Baseline Specification



Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

jump of around 1 percent followed by a slight increase to roughly 3 percent above the baseline level. The new patients outcome is shown on a different scale graph, indicating its larger volatility than the other outcome variables. There is an initial jump for the first few months after the opening of around 7-8 percent, then a drop back to the baseline level 4 to 5 months after the opening, followed by a large increase 6 months after the opening. This seems to suggest that new pharmacies pick up many new patients initially after opening, then exhaust the pool of potential patients in several months, then see a large increase in new patients after having been open for 6 months.

Table 2.8 shows the values for δ from the "post" regressions for each outcome variable, for models with and without controls. As a further check, in columns 3-4 of table 2.8, I include all zip codes in the data, but for zip codes without openings, I set the "months since opening" variable equal to -1. Thus, all observations from non-opening zip codes have $Post_{zt} = 0$. Each cell in table 2.8 represents a separate regression. Rows represent different outcome variables, while columns represent the inclusion of controls and/or the inclusion of all zip codes. The opening effects are roughly an increase of 2 percent from the baseline level, and do not change much with the addition of controls, nor with the inclusion of all zip codes.

Closings

The effect of closings is considerably less visible than the opening effects. This might be an artifact of the "messiness" of the closing events - many closings were immediately replaced by pharmacies in different locations within the same zip code.

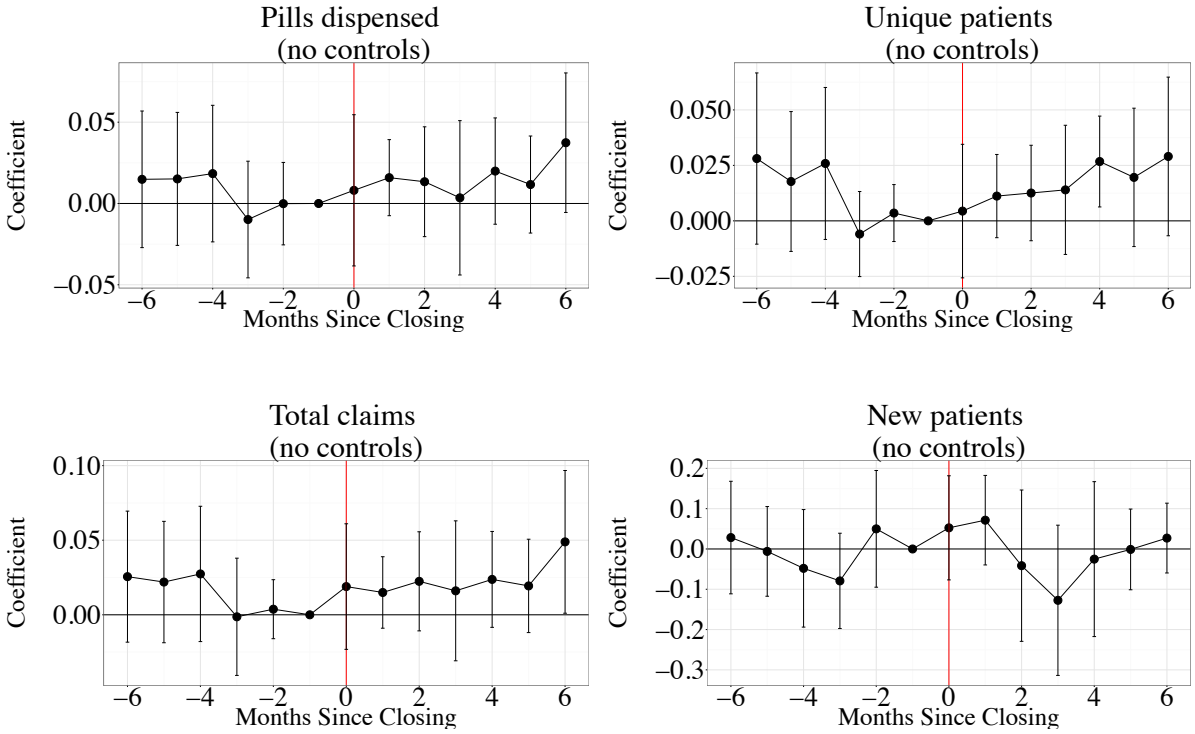
Table 2.8: Post Entry Effects

	Post Opening			
Total Pills	0.018	0.013	0.019	0.018
	(0.007)	(0.007)	(0.008)	(0.008)
Unique Patients	0.016	0.013	0.015	0.014
	(0.007)	(0.007)	(0.007)	(0.007)
Total Claims	0.019	0.015	0.018	0.018
	(0.007)	(0.007)	(0.008)	(0.008)
New Patients	0.032	0.004	0.021	0.023
	(0.028)	(0.035)	(0.029)	(0.030)
Controls	N	Y	N	Y
All Zips	N	N	Y	Y
Observations	468	468	15,804	15,447

Note: This table shows the coefficients on the *Post* variable in the baseline regressions. Each cell represents a separate regression, with the outcome variable specified on each row. The different columns indicate inclusion of control variables and/or inclusion of all Oregon zip codes as controls.

Figure 2.8 shows the analogous results to figure 2.7, but for closings. There does not appear to be any effect from closings on the outcome variables.

Figure 2.8: Pharmacy Closing Output Regression Coefficients - Baseline Specification



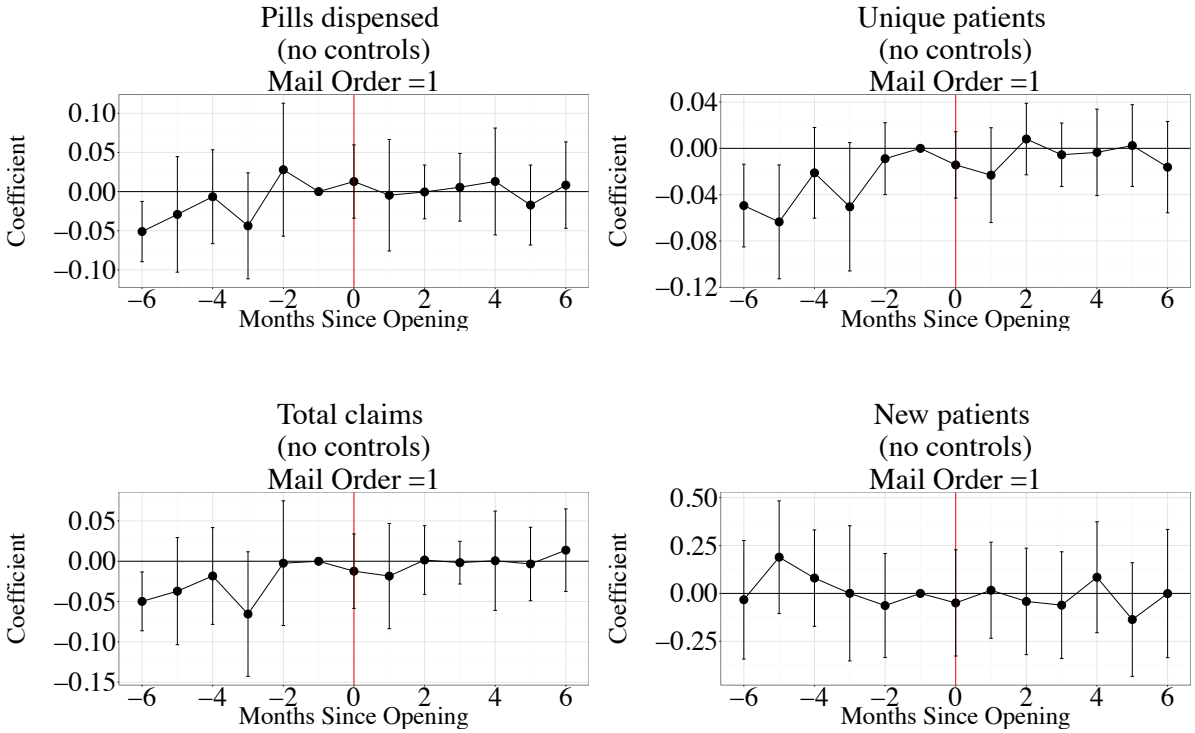
Note: This figure shows the event study plots in the four outcome variables for the pharmacy closing events. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the closing. The horizontal axis shows the number of months before or after the local pharmacy closing. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Mail Order

Since patients who use mail-order pharmacies do not have to travel to their pharmacy to obtain their medication, they are likely less sensitive to changes in distance costs

to refill their prescription drugs. I verify this hypothesis in figure 2.9, which shows that there are no effects of openings on mail-order prescriptions only.

Figure 2.9: Pharmacy Opening Output Regression Coefficients - Mail Order Only

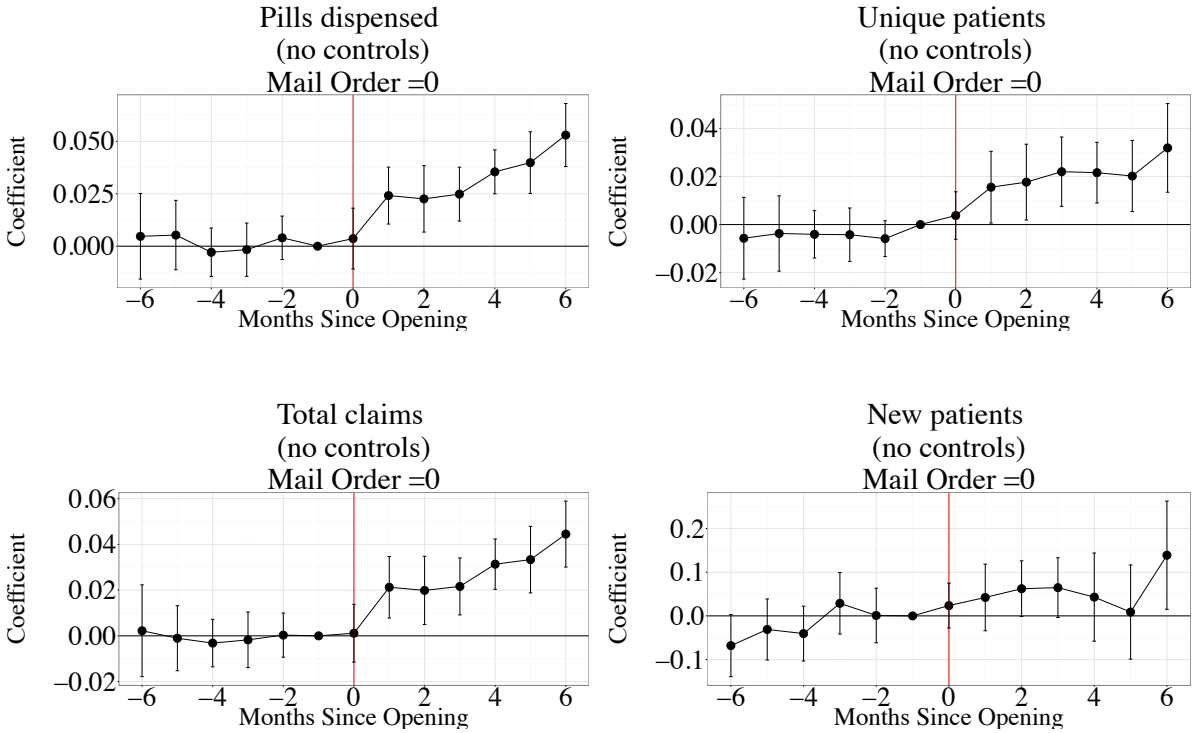


Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events, limited only to prescription drug claims filled through mail-order. The vertical axis shows the coefficient on the *MonthsSince* dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

When I exclude the mail order patients from the baseline specification, I find similar, if not larger, effects from pharmacy entry as the in the baseline specification (see figure 2.10). This is essentially limiting to the patients who have the highest

elasticity of adherence with respect to distance.

Figure 2.10: Pharmacy Opening Output Regression Coefficients - Non-Mail Order Only



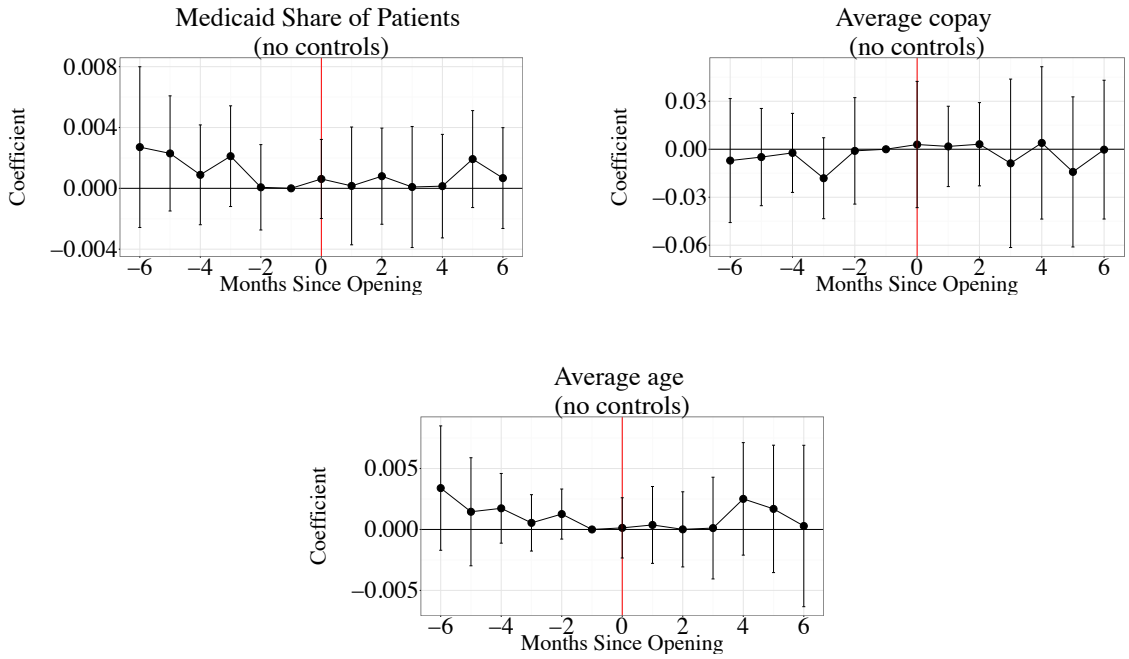
Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events, limited only to prescription drug claims NOT filled through mail-order. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Patient Composition

Another dimension that could be affected by pharmacy entry is the composition of patients who are filling prescriptions. To examine the effects of entry on composition, I use as outcome variables the share of patients on Medicaid and the average age of patients. It is also possible that patients who have different price elasticities are induced to fill claims after openings. To examine this possibility, I use the average copay for prescription drugs in a zip code as the outcome variable.

Figure 2.11 shows little to no effect in patient composition around the entry of a new pharmacy. Of particular interest is the pattern in the share of patients on Medicaid, since this is a proxy for local income levels. Since the share of Medicaid patients is not changing around the time of entry, this suggests that pharmacies are not entering areas with increasing incomes.

Figure 2.11: Pharmacy Opening Output Regression Coefficients - Patient Composition



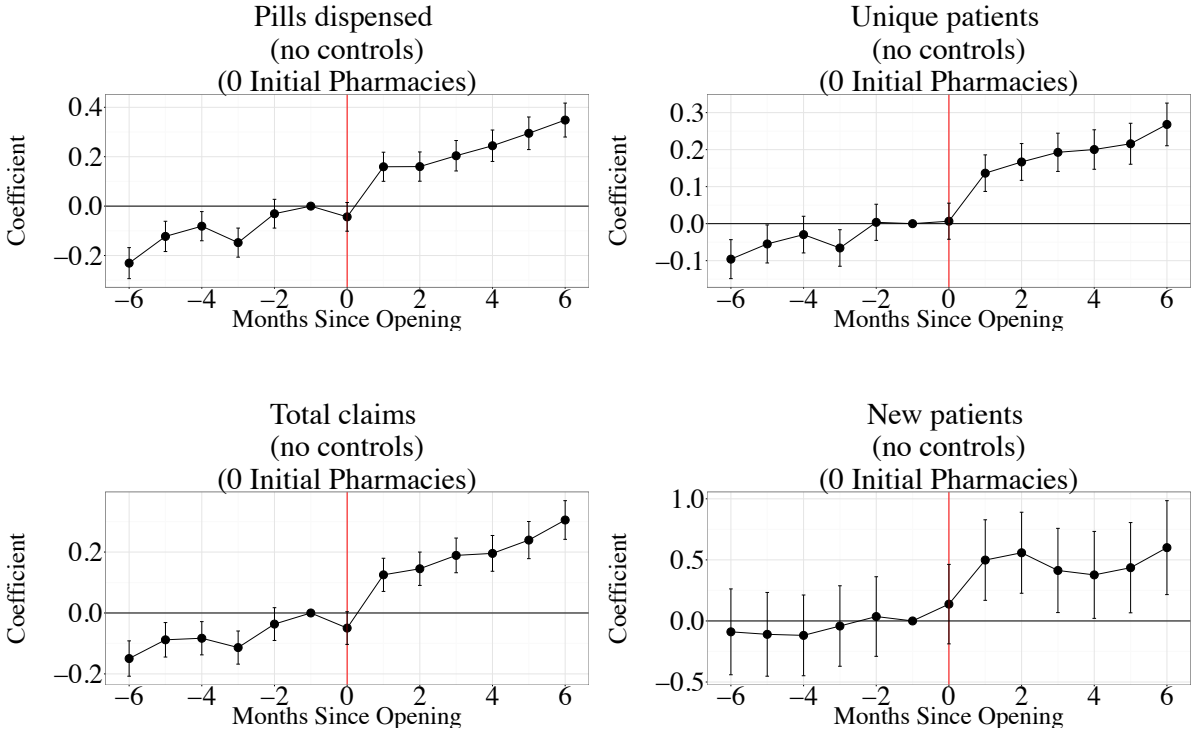
Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events, where the outcome variables give information about the composition of patients in the zip code. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Deserts

In figure 2.12, I show the effects of openings for zip codes with 0 pharmacies present in the month prior to the opening, by interacting the $MonthsSince$ indicators with an indicator for the "desert" zip codes. I am cautious to label these zip codes as "deserts" since these measures do not include anything about adjacent zip codes.

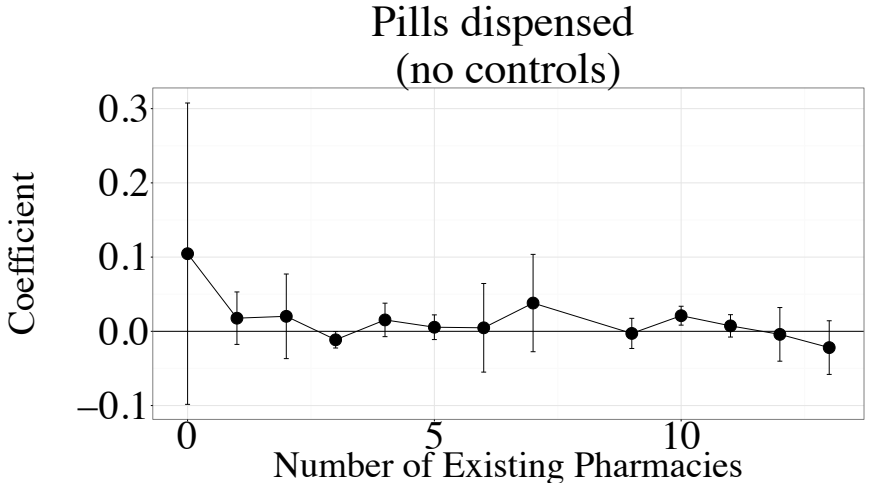
The effects reported in these figures are around 20-30 percent, which are roughly an order of magnitude larger than the effects in all opening zip codes. This at least suggests that these zip codes were more constrained by access than other zip codes, and thus were impacted to a greater degree than zip codes that were less constrained by access. In results not shown in this paper, I use a dose-response approach highlighting that the largest effects are in zip codes with fewer existing pharmacies, and that the zip codes with no existing pharmacies are the primary driver behind the effects.

Figure 2.12: Pharmacy Opening Output Regression Coefficients - "Deserts" Only



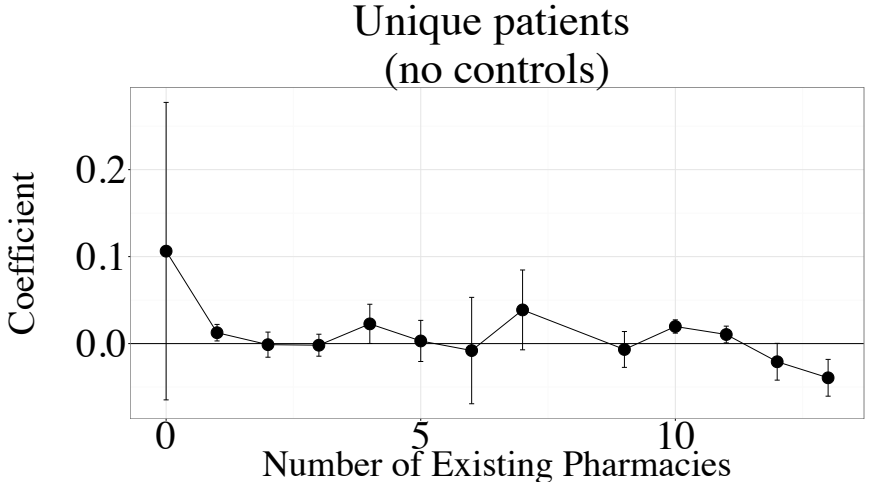
Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events, limited to zip codes where, prior to the opening, there were no pharmacies in the zip code. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.13: Opening Effect by Number of Existing Pharmacies - Total Pills



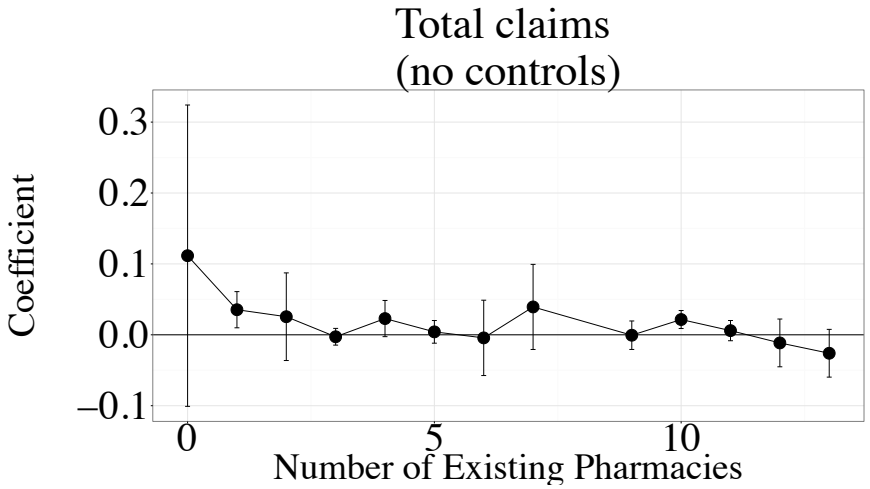
Note: This figure shows the coefficient results for the pharmacy opening events, split by the number of pharmacies in the zip code in the month prior to the opening.

Figure 2.14: Opening Effect by Number of Existing Pharmacies - Unique Patients



Note: This figure shows the coefficient results for the pharmacy opening events, split by the number of pharmacies in the zip code in the month prior to the opening.

Figure 2.15: Opening Effect by Number of Existing Pharmacies - Total Claims

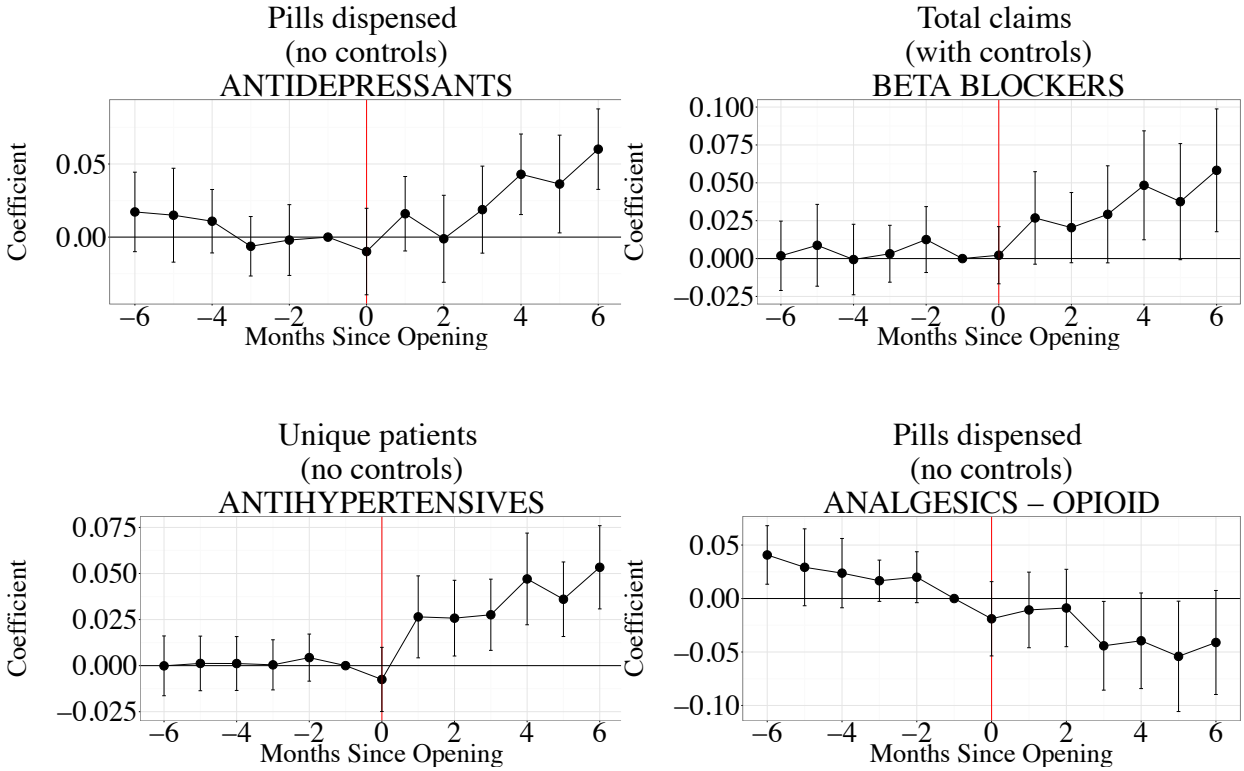


Note: This figure shows the coefficient results for the pharmacy opening events, split by the number of pharmacies in the zip code in the month prior to the opening.

2.5.2 Drug Type

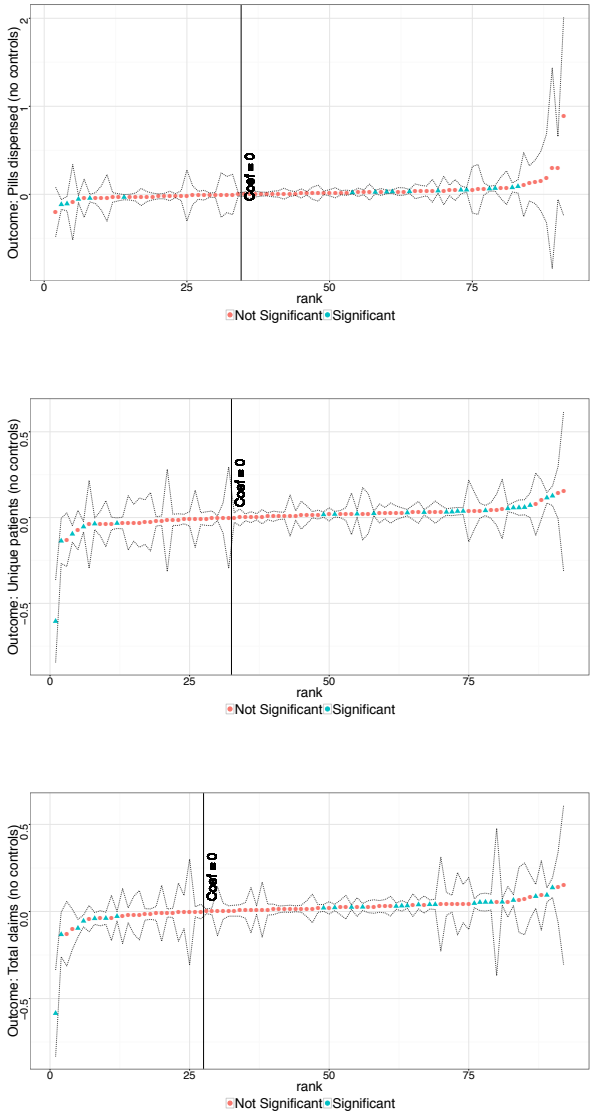
I show a selection of the plots of the $\delta_{d\tau}$ from the drug selected regressions in figure 2.16. This figure shows a sample of the different patterns in the effects for different types of drugs. For example, antidepressants did not change for the first few months after the opening, but then increased dramatically in months 3 through 6. Beta blockers and anti-hypertensives, important heart medications, had slightly larger effects than the overall effects, but followed the general overall effect pattern. Opioid prescriptions did not increase after openings, but, though not statistically significant, may have *decreased* after opening.

Figure 2.16: Pharmacy Opening Output Regression Coefficients - Split by Drug Type



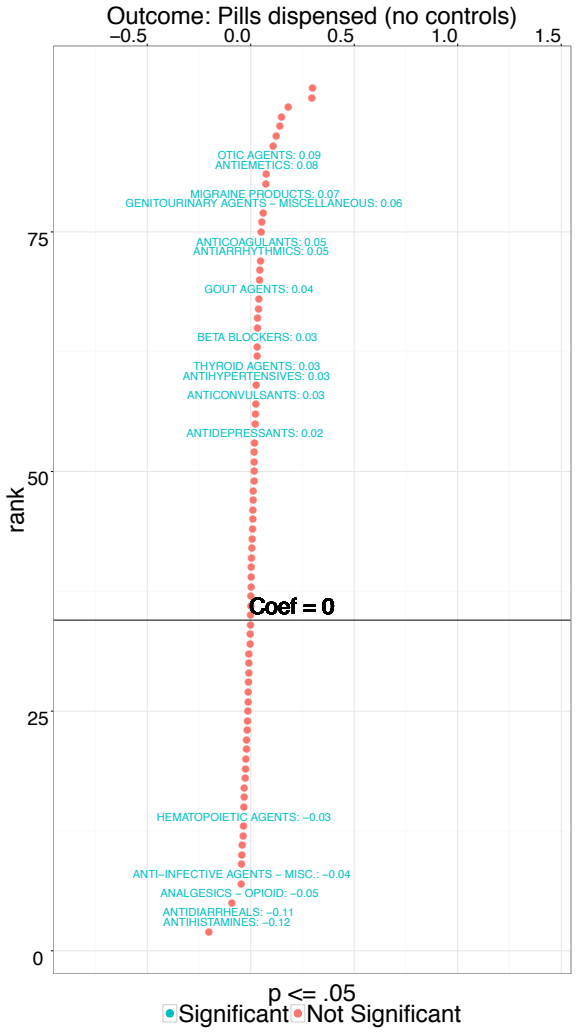
Note: This figure shows the coefficient results for the pharmacy opening events, split by drug type.

Figure 2.17: Rank Effects by Drug Type



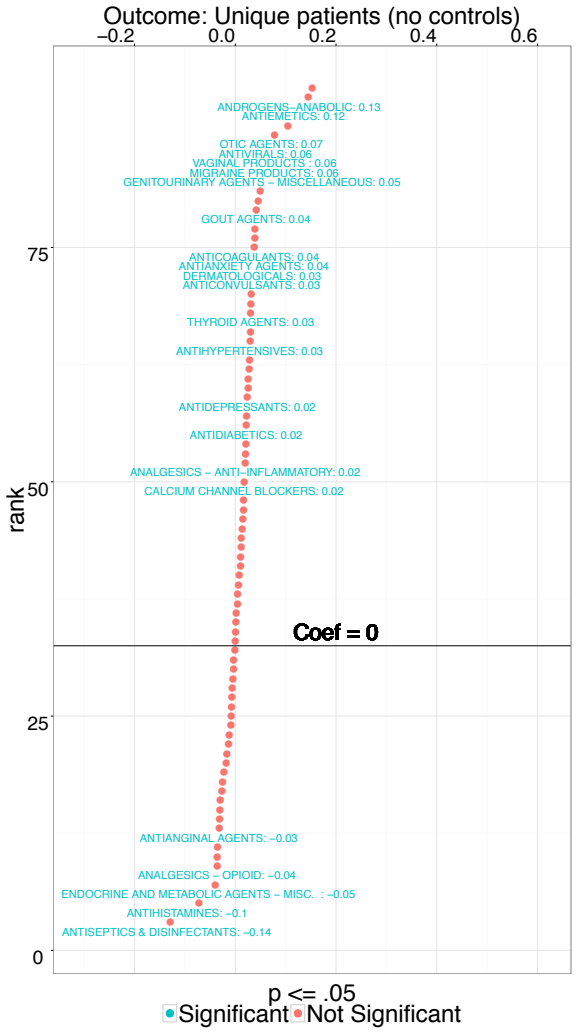
Note: This figure shows the coefficient results for the pharmacy opening events, split by drug type, ranked by coefficient magnitude. Ninety-five percent confidence intervals are shown with the dotted lines. The color of the points indicate whether the coefficient was statistically significant.

Figure 2.18: Drug type labels



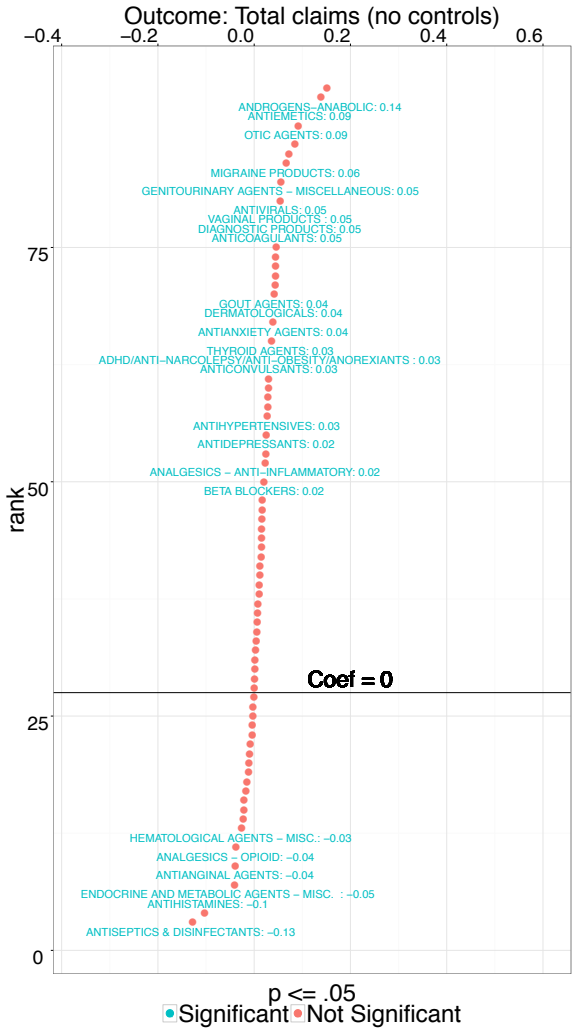
Note: This figure shows the coefficient results for the pharmacy opening events, split by drug type, ranked by coefficient magnitude. Ninety-five percent confidence intervals are shown with the dotted lines. The color of the points indicate whether the coefficient was statistically significant. Drugs with significant coefficients are labeled with their type.

Figure 2.19: Drug type labels



Note: This figure shows the coefficient results for the pharmacy opening events, split by drug type, ranked by coefficient magnitude. Ninety-five percent confidence intervals are shown with the dotted lines. The color of the points indicate whether the coefficient was statistically significant. Drugs with significant coefficients are labeled with their type.

Figure 2.20: Drug type labels

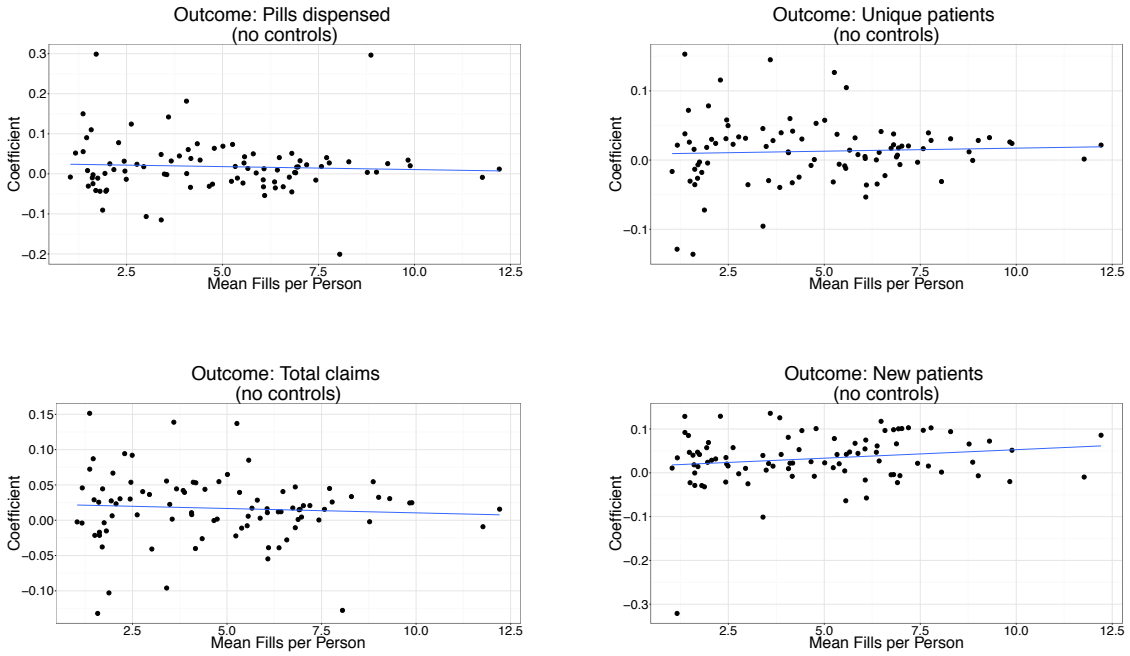


Note: This figure shows the coefficient results for the pharmacy opening events, split by drug type, ranked by coefficient magnitude. Ninety-five percent confidence intervals are shown with the dotted lines. The color of the points indicate whether the coefficient was statistically significant. Drugs with significant coefficients are labeled with their type.

When I estimated the overall effect for each drug type in the data, I found that several important drugs were significantly affected by the openings: antidepressants, antihypertensives, thyroid agents, beta blockers, migraine products, antidiabetics,

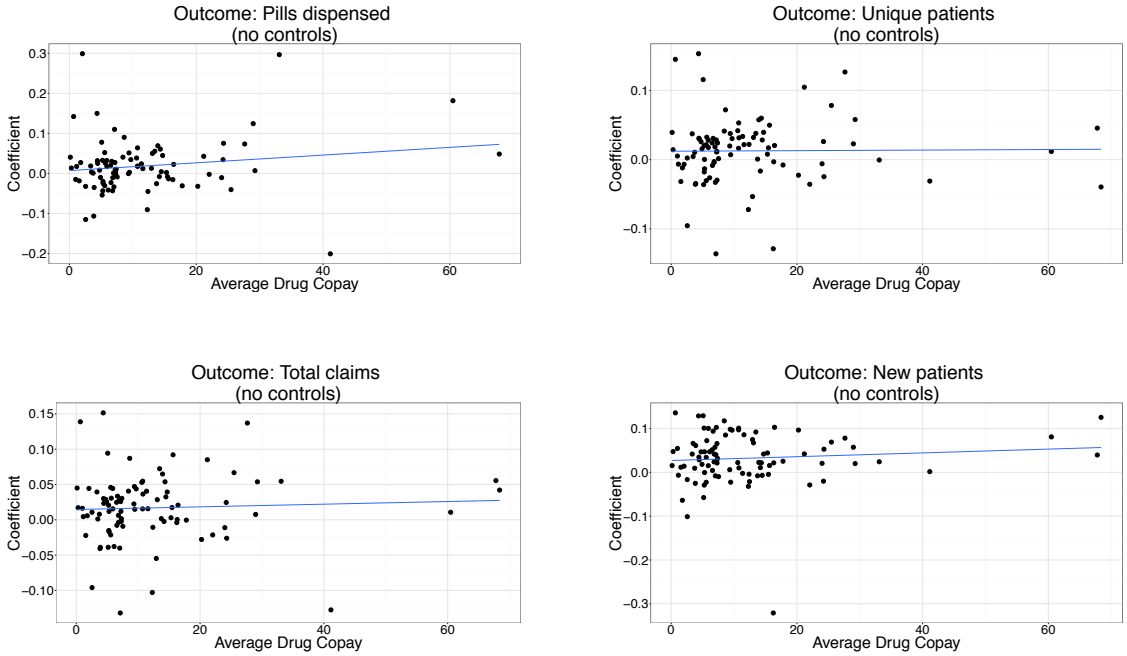
and calcium channel blockers. Thus, openings did not only induce patients to fill more prescriptions for luxury, non-essential drugs, but also induced more claims for important, maintenance drugs.

Figure 2.21: Effect of Opening and Mean Fills per Patient



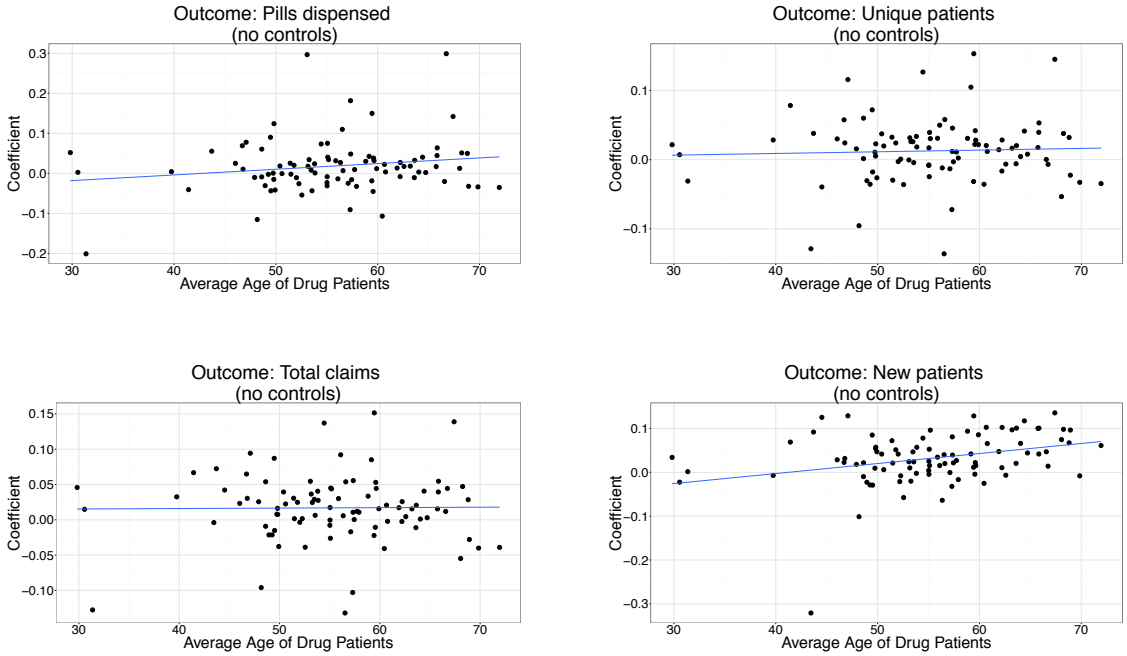
Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on mean total fills per patient.

Figure 2.22: Effect of Opening and Mean Drug Copay



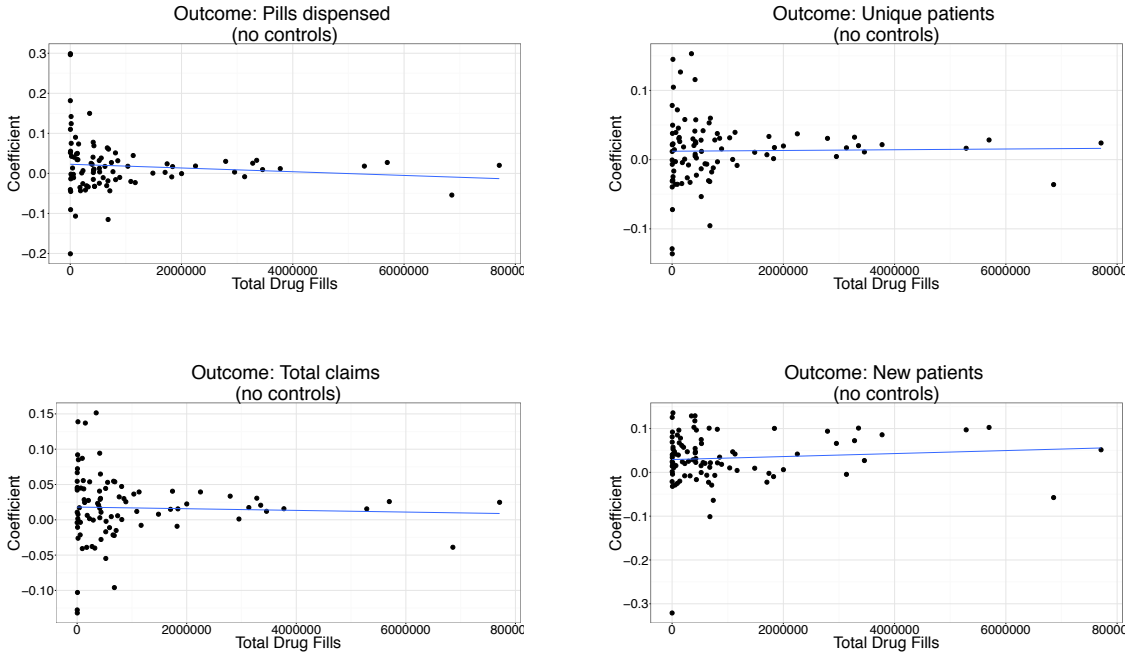
Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on mean drug type copay.

Figure 2.23: Effect of Opening and Mean Patient Age



Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on mean patient age.

Figure 2.24: Effect of Opening and Total Drug Fills



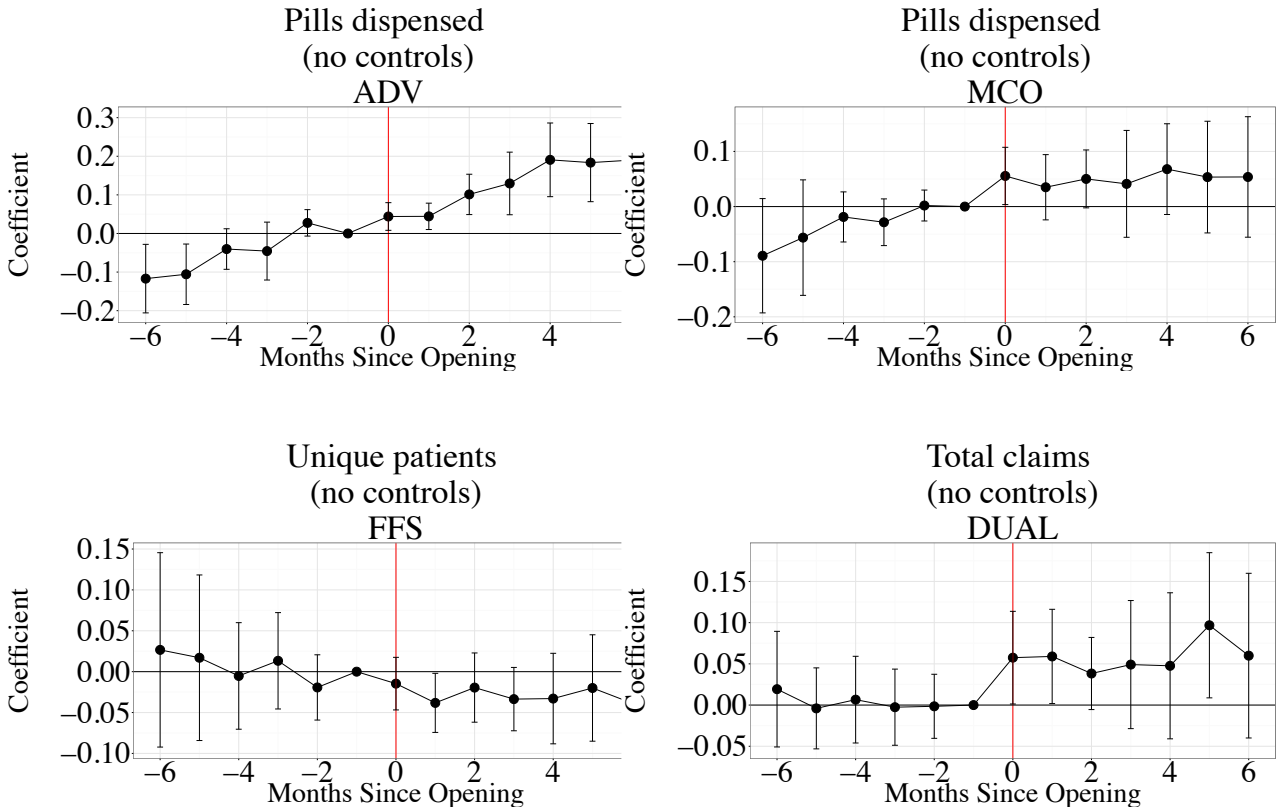
Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on total fills per patient.

2.5.3 Insurance Type

In figure 2.25, I split the baseline regressions into insurance-specific results, using the "APAC Payer" variable that has 8 insurance type categories. Table 2.9 shows the names of the insurance types corresponding to the abbreviations. As with the drug types, this figure only shows a selected sample of the different insurance types. I repeated the exercise with various combinations of the three available insurance type variables, finding similar results. The largest effects appear in the Medicare Advantage category, with large effects also in the private insurance, Medicaid Managed Care and the Dual Eligible Insurance groups. The Medicaid Fee-for-service group

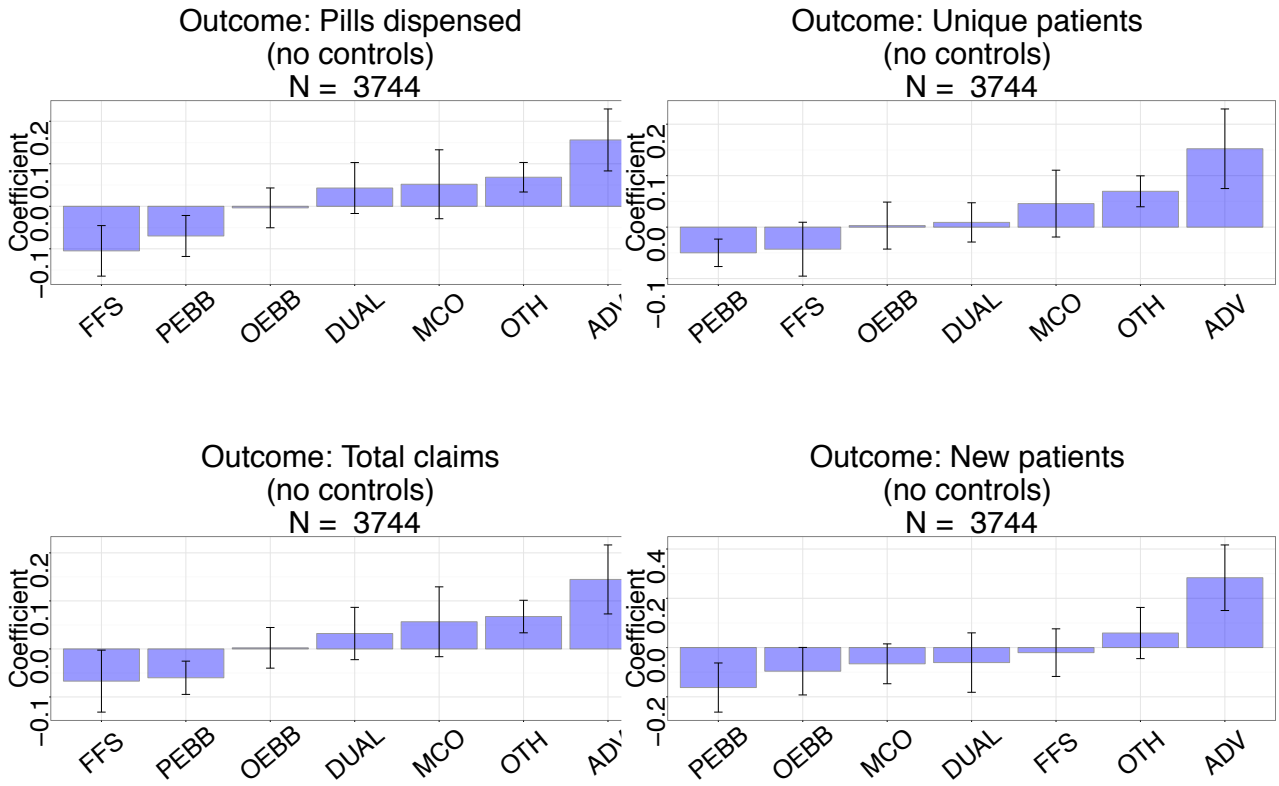
had no positive effects from opening and potential decreases.

Figure 2.25: Pharmacy Opening Event Study - Split by Insurance Type



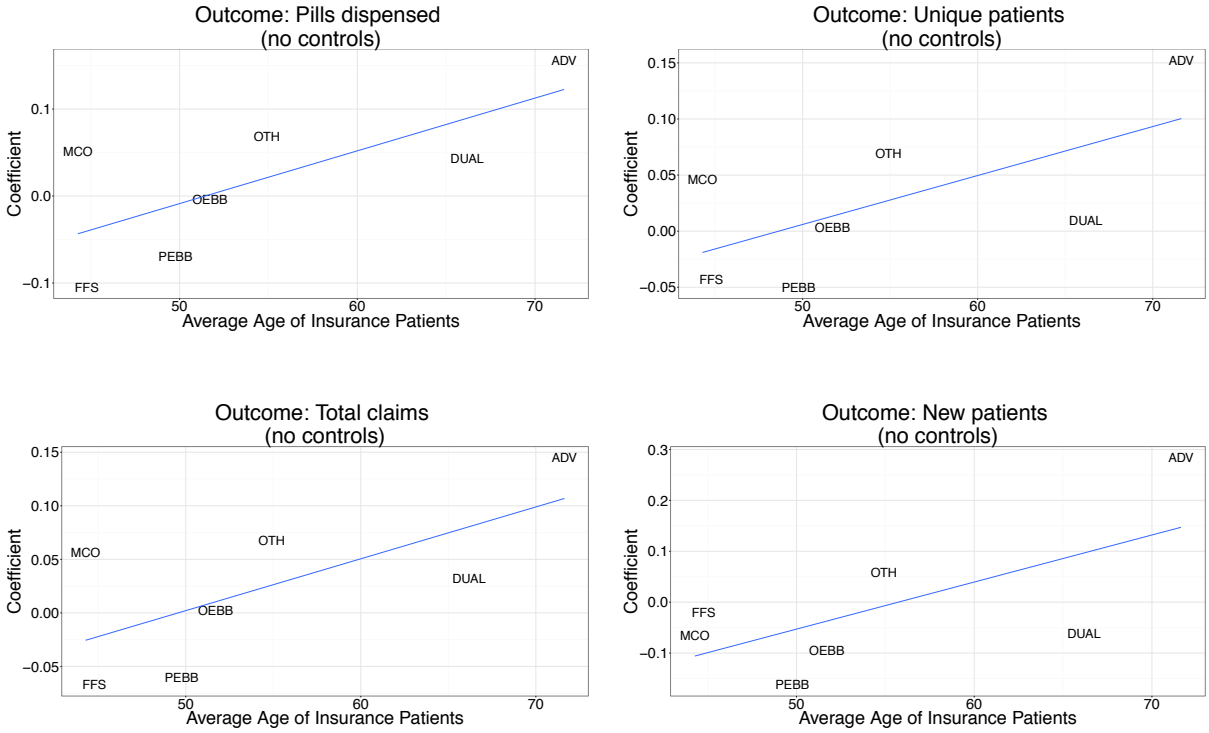
Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events, split by insurance type. The vertical axis shows the coefficient on the *MonthsSince* dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.26: Pharmacy Opening Output Regression Coefficients - Split by Insurance Type



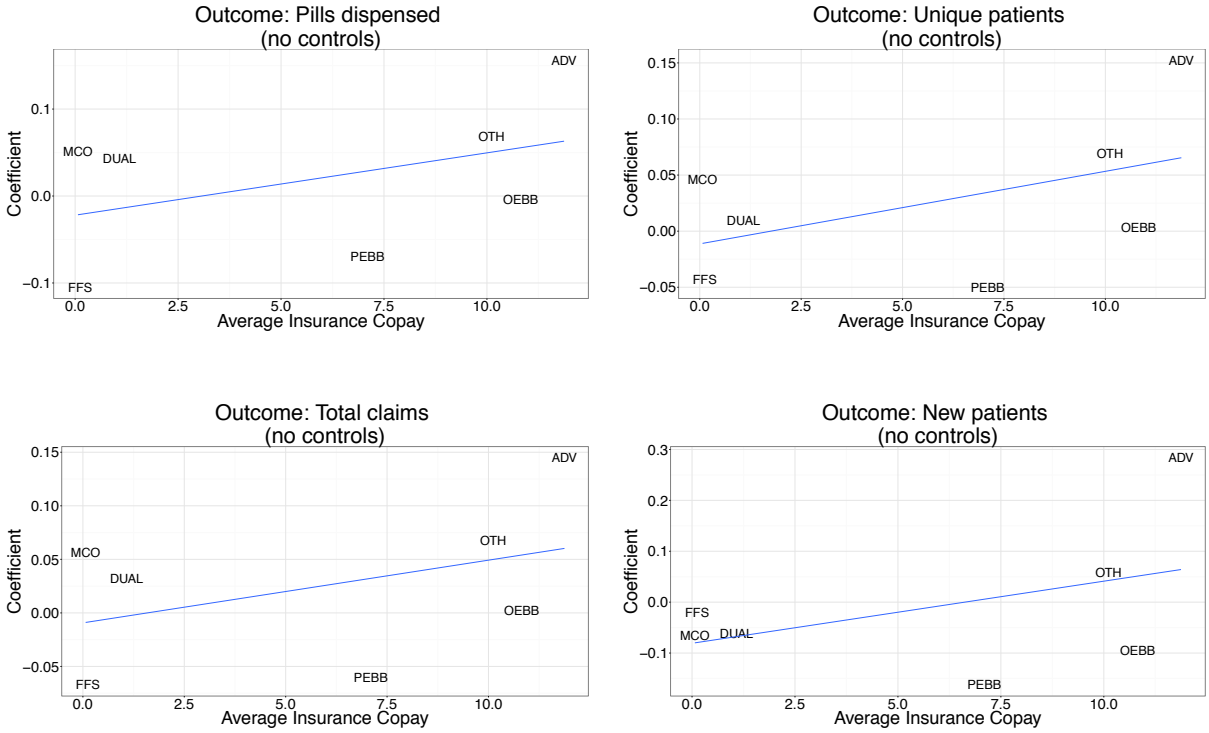
Note: This figure shows the event study plots in the four outcome variables for the pharmacy opening events, split by insurance type. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the opening. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.27: Opening Effect and Insurance Age



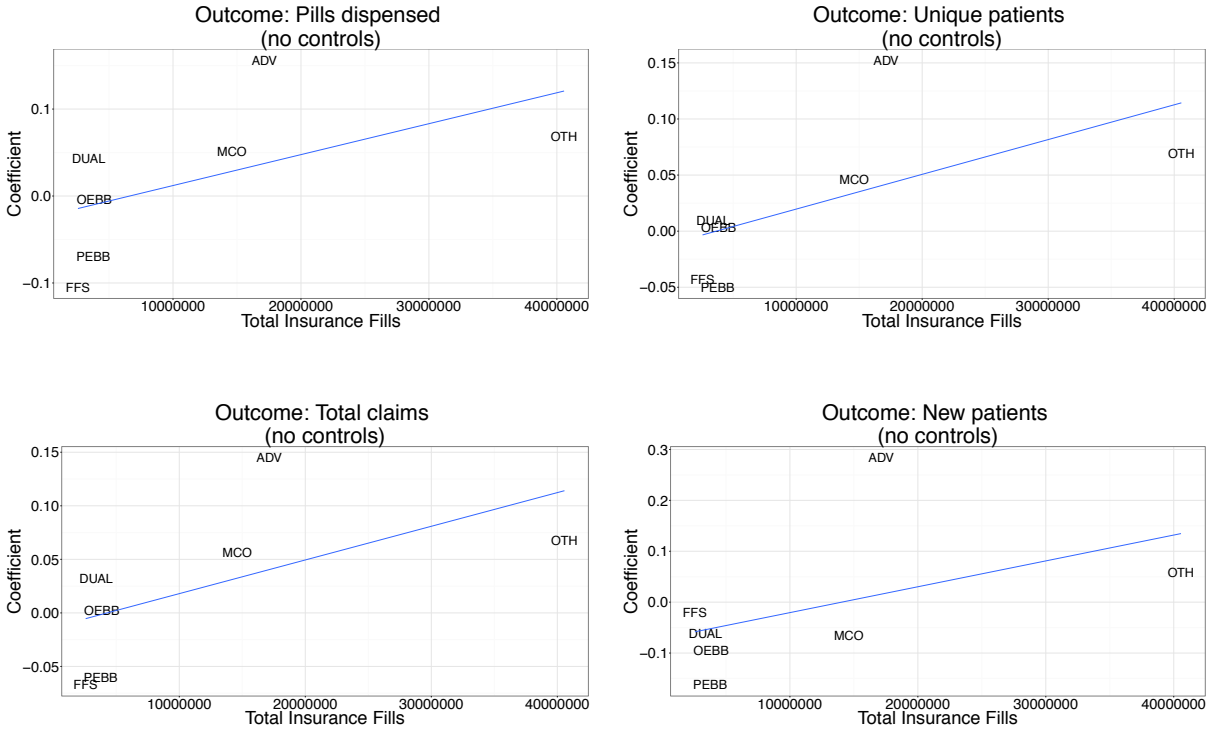
Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on mean patient age in each insurance type.

Figure 2.28: Opening Effect and Insurance Copay



Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on mean copay in each insurance type.

Figure 2.29: Opening Effect and Insurance Total Fills



Note: This figure shows the coefficient results for the pharmacy opening events, from regressions split into bins based on total fills in each insurance type.

Table 2.9: APAC Payer Codes

FFS	Medicaid Fee for Service
PEBB	Oregon Public Employee Benefits
OEBB	Oregon Educators Benefits
DUAL	Dual (Medicaid + Medicare) Eligibles
MCO	Medicaid Managed Care Organization
OTH	Other Commerical Payer (Private Insurance)
ADV	Medicare Advantage

Note: This table shows the meaning of the insurance payer codes indicated in the OAPCD.

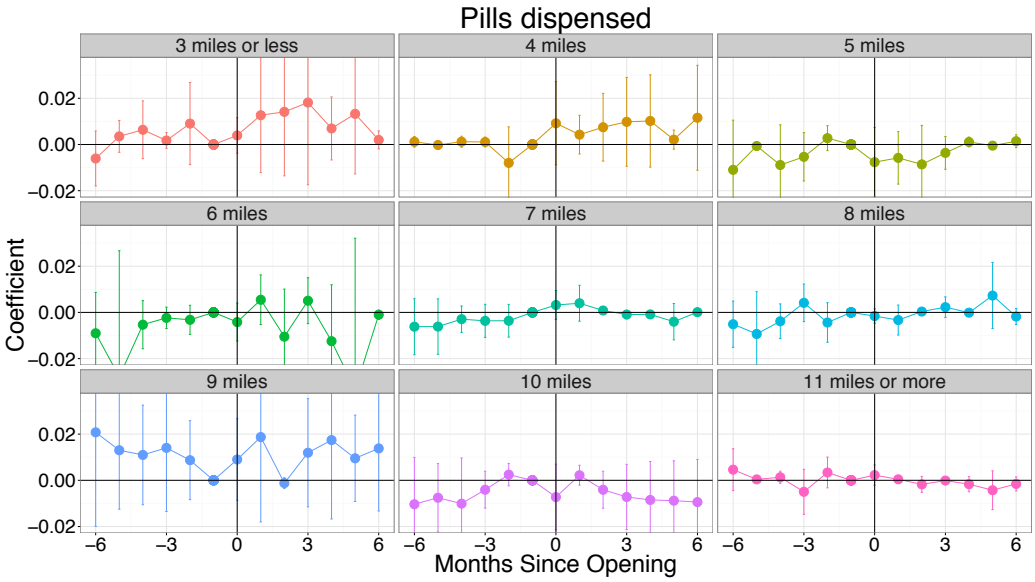
I show the "post" effect sizes split by the 8 insurance groups in figure 2.26. This figure shows that the effect size range from roughly negative 5 percent (for the Oregon Public Employees Benefit plan) to around 15 percent (for the Medicare Advantage plans). Private insurance plans ("OTH") also have large positive effects of around 7 percent.

2.5.4 Adjacent Zip Codes

Figure 2.30 shows the time series effects of the opening events on the residents of zip codes adjacent to the opening zip codes. The figure is split into 9 panels, each showing the effects for a different distance group, starting with 3 miles or less in the top left panel and ending with 11 miles or more in the bottom right panel. This figure

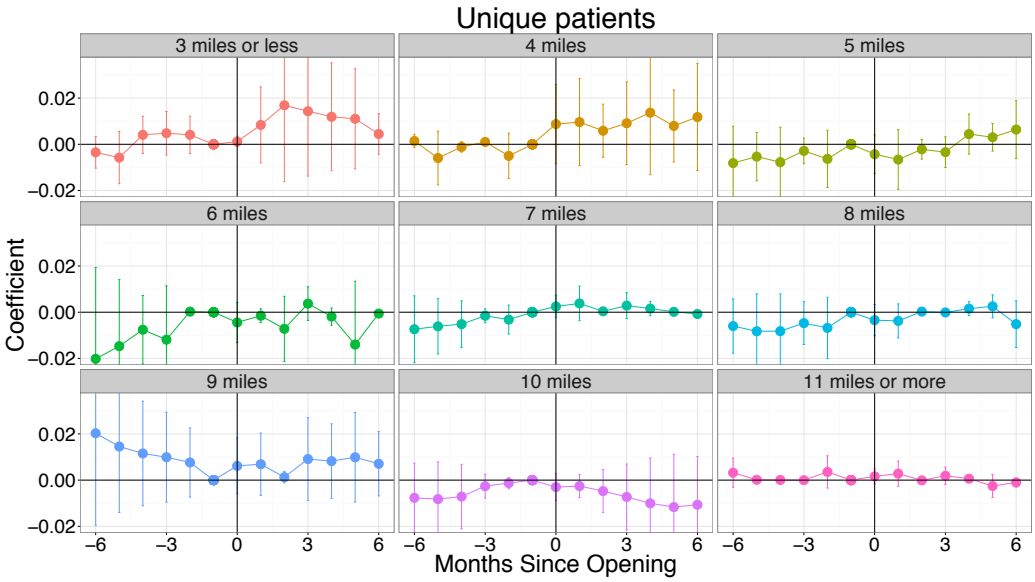
shows that the zip codes nearest to the opening zip code have the largest effect from the opening, and that the effect appears to fade out for zip codes about 5 or more miles away from the opening zip code. The clustered standard errors are relatively large, but the pattern is still suggestive of the effects of distance costs.

Figure 2.30: Time Series Effect of Openings on Adjacent Zip Codes - Total Pills



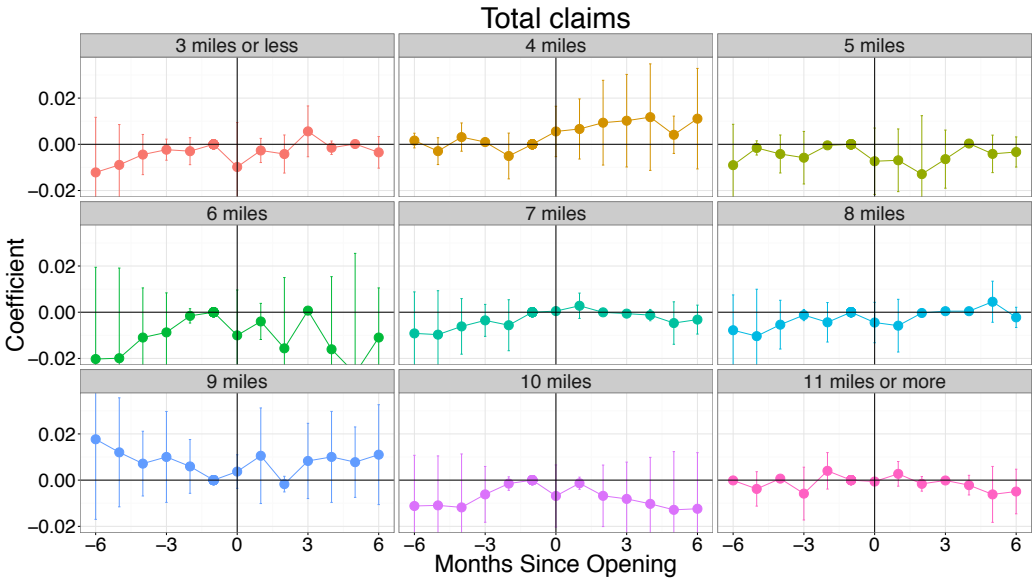
Note: This figure shows the event study results, split into groups of increasing distance from the opening zip code.

Figure 2.31: Time Series Effect of Openings on Adjacent Zip Codes - Unique Patients



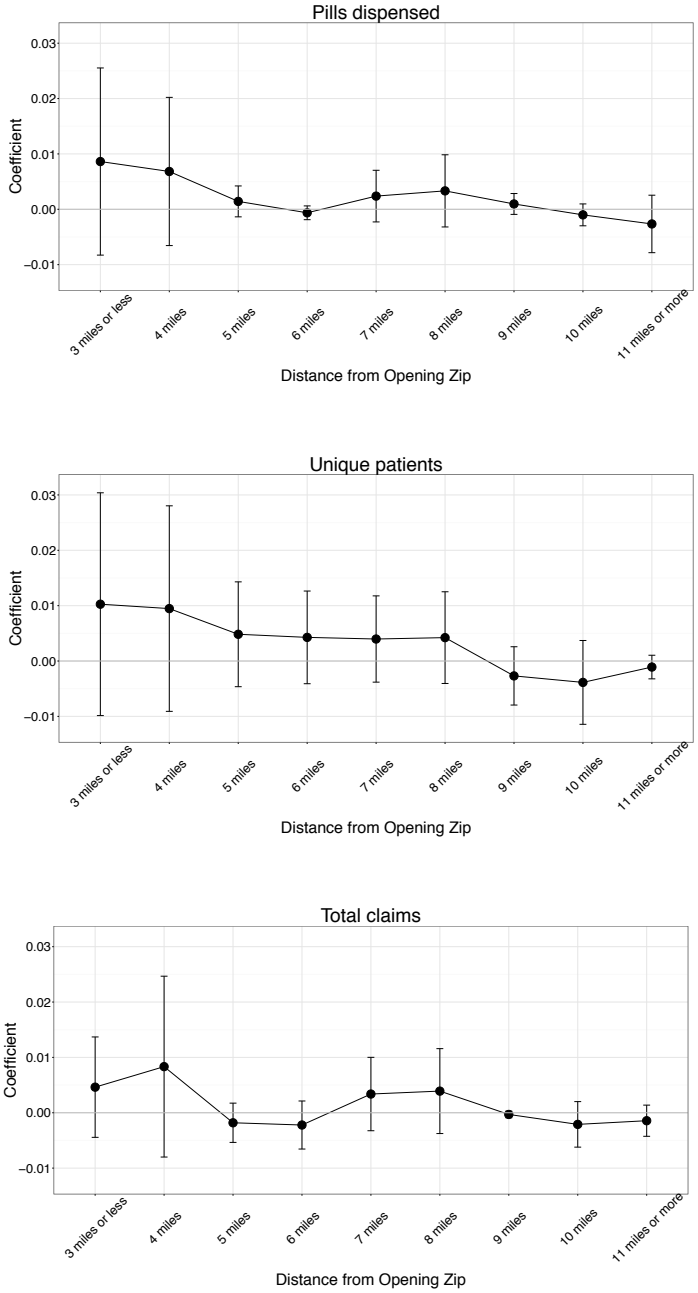
Note: This figure shows the event study results, split into groups of increasing distance from the opening zip code.

Figure 2.32: Time Series Effect of Openings on Adjacent Zip Codes - Total Claims



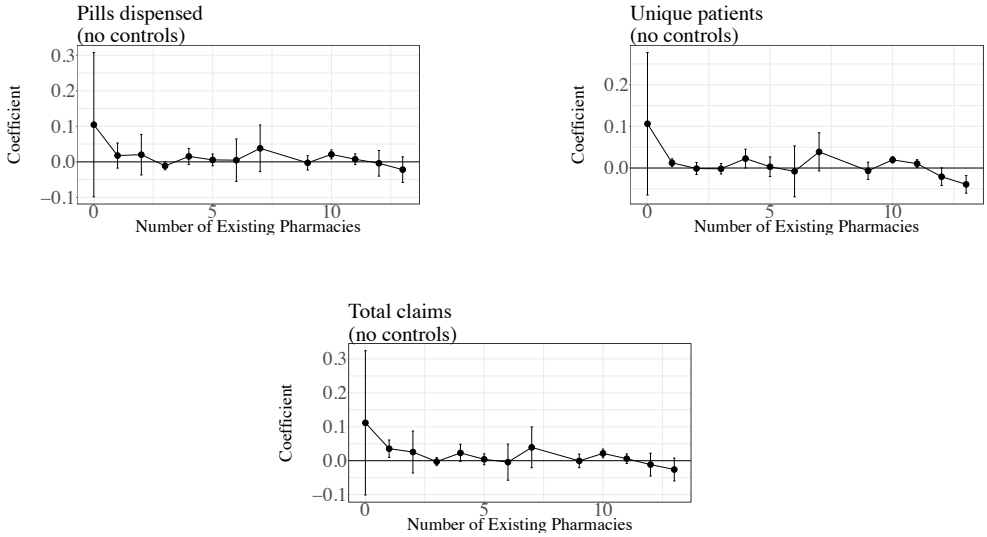
Note: This figure shows the event study results, split into groups of increasing distance from the opening zip code.

Figure 2.33: Effect of Openings on Adjacent Zip Codes



Note: This figure shows the effects of openings split into groups of increasing distance from the opening zip code.

Figure 2.34: Effect of Openings by Number of Existing Pharmacies



Note: This figure shows the estimated effect of an additional pharmacy opening in a zip code with the specified number of existing pharmacies, for the three adherence outcomes. The vertical axis shows the coefficient on the *Post* dummy variables in the regressions. The horizontal axis shows the number of existing pharmacies in the zip code of the opening in the month prior to the opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

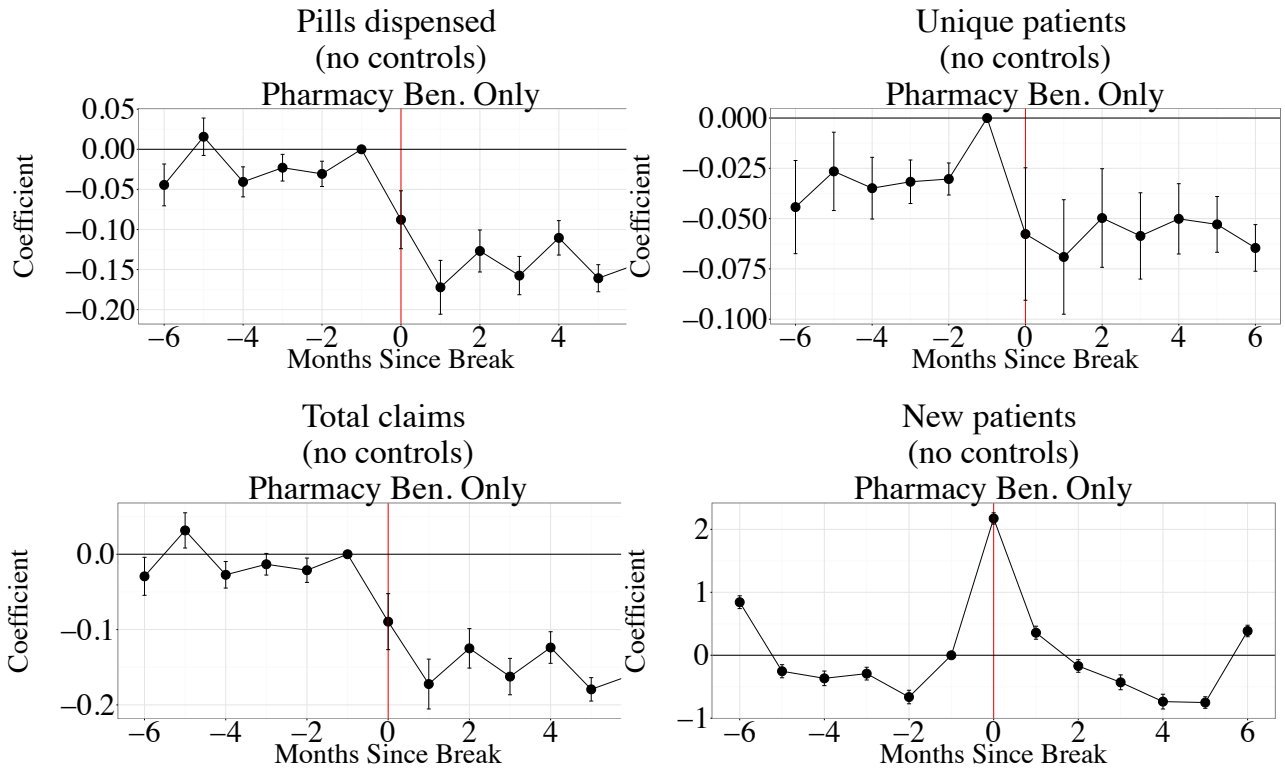
Figure 2.34 shows the effects of the openings on adjacent zip codes, summarized into one coefficient by the *Post* indicator variable. This figure show a general fade out effect of the openings as the distance to the opening zip code increases. Large standard errors prevent strong conclusions, but the pattern is again suggestive of distance as a main driver in the opening effect.

2.5.5 Walgreens and Express Scripts

Figure 2.35 plots the effects from the Walgreens - Express Scripts separation, limiting to the 6 months around the separation (July 2011 to July 2012), patients from

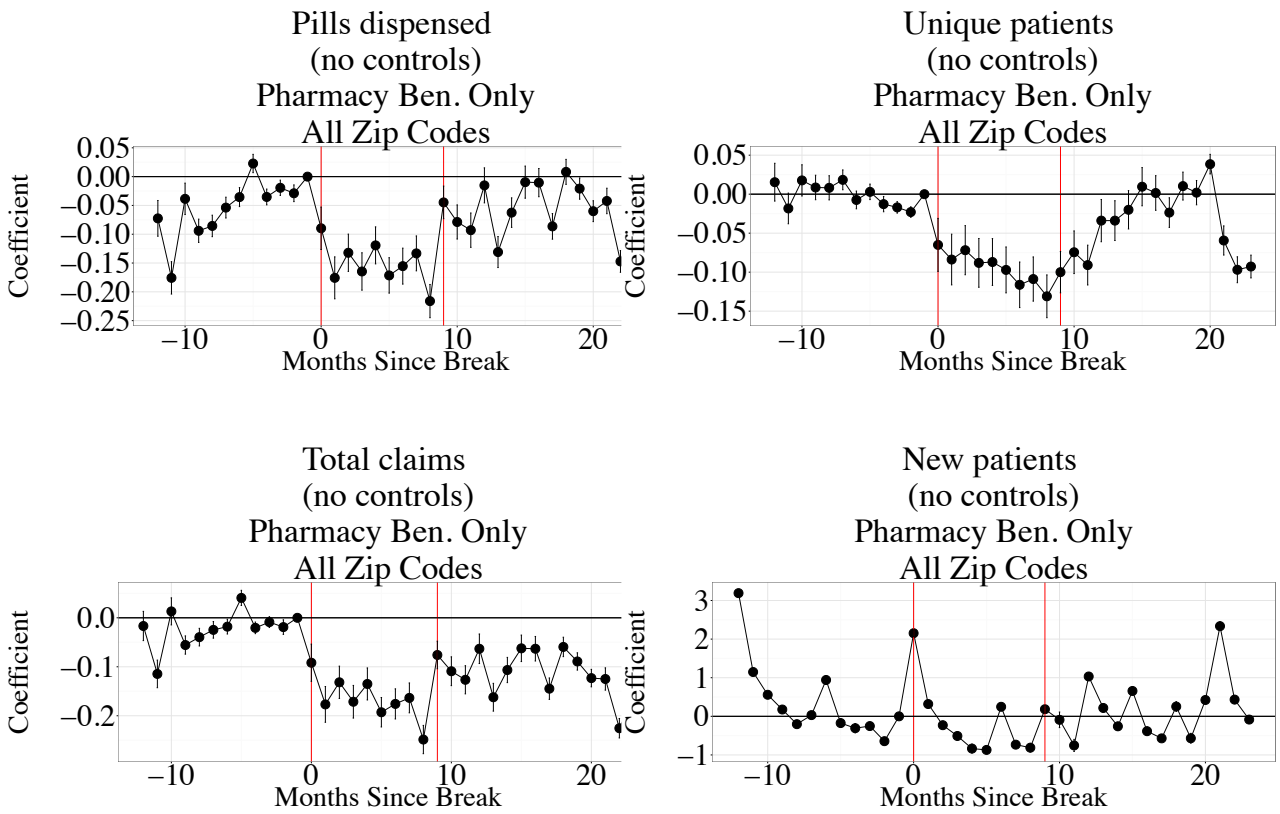
the Walgreens zip codes who were covered by pharmacy benefits management companies only. This figure shows a large decrease of about 10 to 15 percent in each of the outcome variables (except new patients) after the separation. The new patients outcome has a spike in January, suggesting that many patients begin filling prescriptions in January. This fact is true across insurance plans as illustrated in figure 2.40, which shows the outcome variables for the Walgreens zip codes, limited to Medicaid enrollees only as a control group.

Figure 2.35: Walgreens leaving Express Scripts Network (Jan 2012) - Walgreens Zip Codes



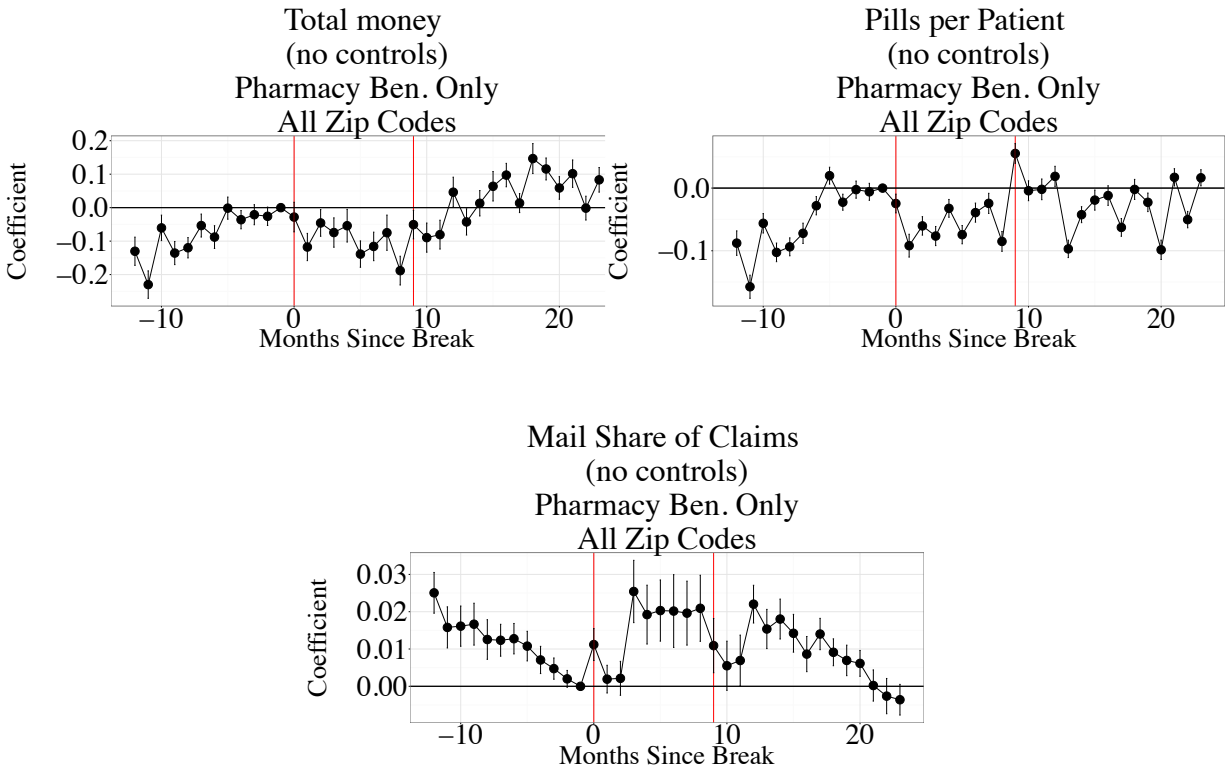
Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events. The vertical axis shows the coefficient on the *MonthsSince* dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.36: Walgreens leaving Express Scripts Network - All Zip Codes, All Months



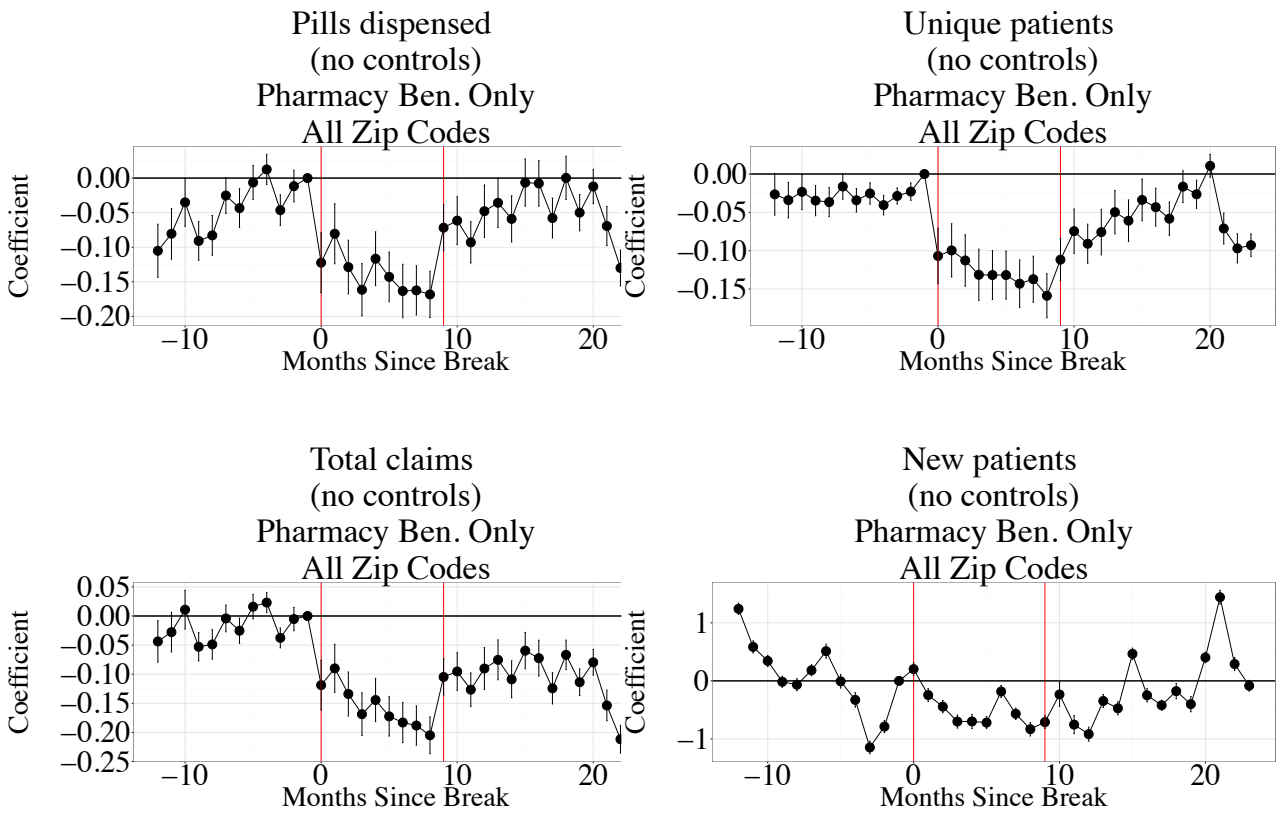
Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events, including all zip codes as control zip codes and expanding the event time frame to include all months in the data. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.37: Walgreens leaving Express Scripts Network - All Zip Codes, All Months



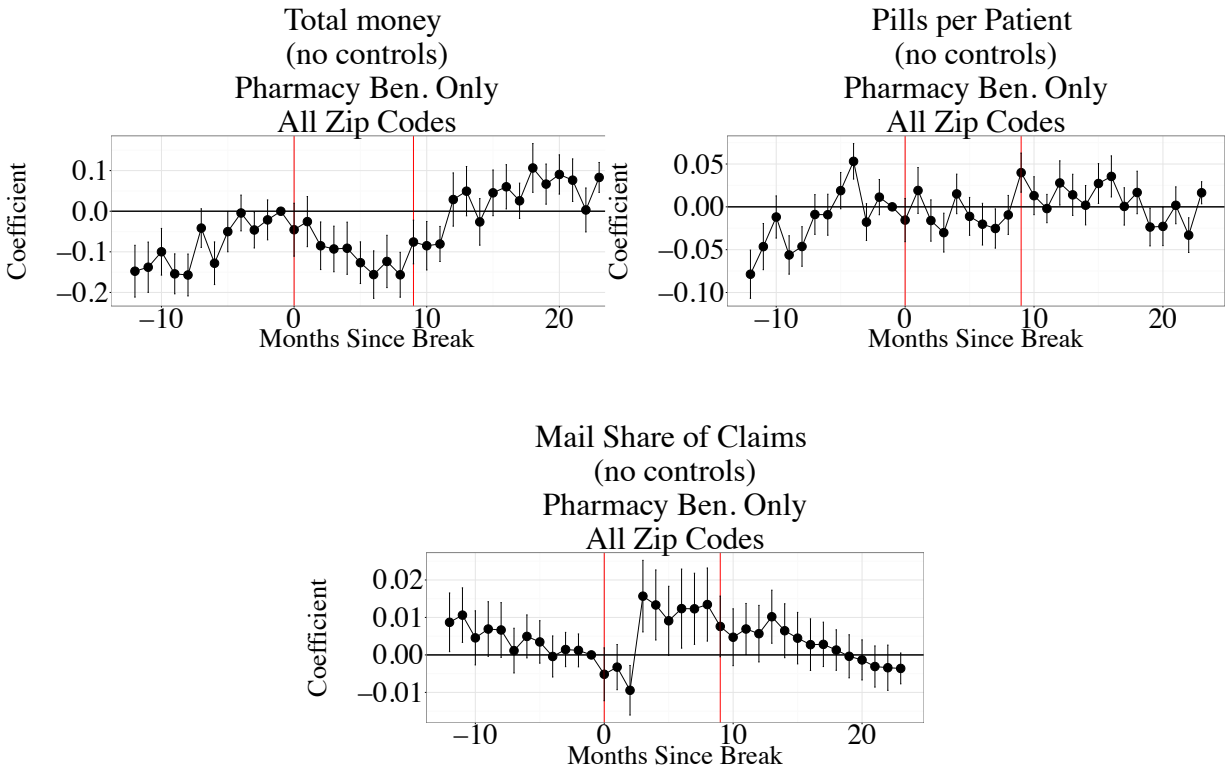
Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events, including all zip codes as control zip codes and expanding the event time frame to include all months in the data. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.38: Walgreens leaving Express Scripts Network - All Zip Codes, Month FE



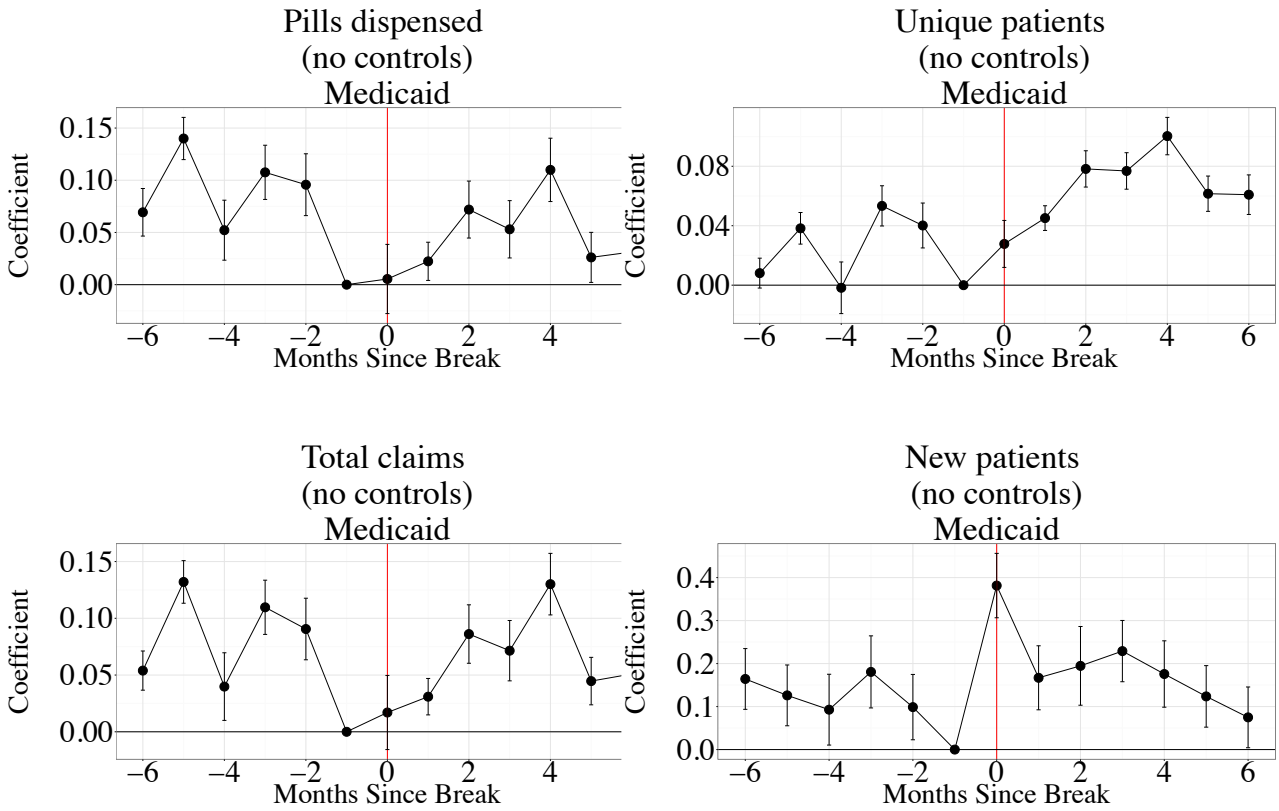
Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events, including all zip codes as control zip codes and expanding the event time frame to include all months in the data. These regressions also include fixed effects for the month of the year. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.39: Walgreens leaving Express Scripts Network - All Zip Codes, Month FE



Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events, including all zip codes as control zip codes and expanding the event time frame to include all months in the data. These regressions also include fixed effects for the month of the year. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.40: Walgreens leaving Express Scripts Network - Medicaid, Walgreens Zip Codes



Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events, limiting to patients with Medicaid insurance. These regressions also include fixed effects for the month of the year. The vertical axis shows the coefficient on the $MonthsSince$ dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

Figure 2.36 expands both the time horizon and the geographical limits, showing the effects of separation from January 2011 to December 2013. The re-entry point, when Walgreens returned to the Express Scripts network in late September of 2012,

is evident in a rise of the outcome variables nearly back to the baseline level. The January spike of new patients is again visible in each of the three January months in the data, along with an unexplained jump in new patients around October 2013.

Table 2.10 shows the post coefficients the Walgreens specifications. Each column represents a different outcome variable, with panel A showing the regressions limiting to the six months before and after the separation, and panel B showing the effects for the whole time frame. The first row of panel A limits to the Walgreens zip codes only, while the second row includes all zip codes in Oregon. The two rows of panel B (within each column) come from the same regression that includes an indicator variable for the 9 months Walgreens spent out of the network (first row, "Out of Network") and an indicator for when Walgreens returned to the network in late September 2012 (second row, "Returned").

With the exception of the new patient outcome variable, each of the coefficients is negative, showing the impact of the separation on individuals living in the Walgreens zip codes. Focusing first on the panel A, it is clear that adding all zip codes has little effect on the coefficients or their standard errors. The network separation reduced pills dispensed, total claims, total patients, total money spent, and total pills per patient for patients in Walgreens zip codes by around 4 percent to 11 percent depending on the outcome variable. Panel B shows similar effects during the time Walgreens was out of the Express Scripts network, and then moderate increases after Walgreens rejoined the network, though not quite to the baseline levels.

As mentioned above, figure 2.40 shows the same plots as figure 2.35, but for Medicaid enrollees only. This figure acts as a control group for patients with insurance

Table 2.10: Post Walgreens Separation Effects

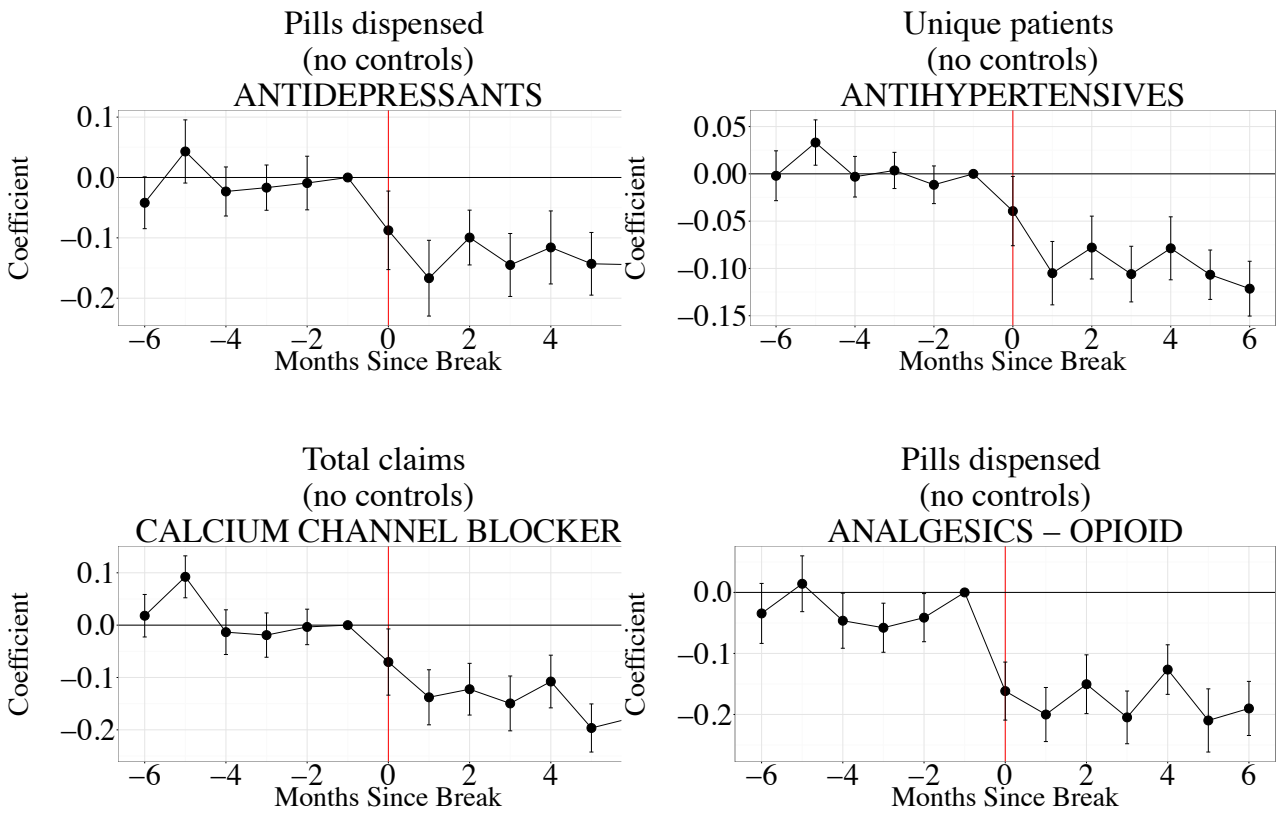
	Total Pills	Unique Patients	New Patients	Total Claims
Panel A: Jul 2011 - Jul 2012				
Post	-0.106	-0.046	1.788	-0.103
N = 1,014	(0.019)	(0.018)	(0.053)	(0.020)
Post × Walgreens	-0.106	-0.046	1.788	-0.103
N = 6,283	(0.019)	(0.019)	(0.053)	(0.020)
Panel B: Jan 2011 - Dec 2013, Walgreens Zip Codes Only				
Out of Network	-0.118	-0.089	0.179	-0.114
N = 2,808	(0.020)	(0.021)	(0.041)	(0.021)
Returned	-0.046	-0.026	1.301	-0.028
N = 2,808	(0.024)	(0.025)	(0.086)	(0.025)
Controls	N	N	N	N

Note: This table shows the effects from the Walgreens - Express Scripts separation and reunion in 2012. In panel A, each cell represents a separate regression, with outcome variables defined in column headings. The second row of panel A shows the coefficients for regressions including all zip codes as controls. Panel A only includes months from July 2011 to July 2012 (six months before and after the separation). In panel B, each column represents a separate regression, with outcome variables in column headings. Each row of panel B shows a different variable, the first row corresponding to the effect of the separation, and the second row to the effect of the reunion. Panel B includes all months available in the data (2011-2013). None of these regressions include month-of-year fixed effects.

through pharmacy benefit managers only. It shows that Medicaid enrollees in Walgreens zip codes did not have the same decreases as patients with insurance through pharmacy benefit managers only after the separation. This illustrates that the effect was centered in patients who were affected by the treatment - the Express Scripts enrollees.

To illustrate that the separation affected important drugs, I show a selection of the drug specific effects in figure 2.41. This figure shows decreases in various outcomes for four important drugs: antidepressants, anti-hypertensives, calcium channel blockers, and opioids. Interestingly, opioid prescriptions decreased after the Walgreens separation, but did not increase after pharmacy openings. Thus, the separation impacted necessary drugs that have substantial impacts on patient health.

Figure 2.41: Walgreens leaving Express Scripts Network - Drug Type Split



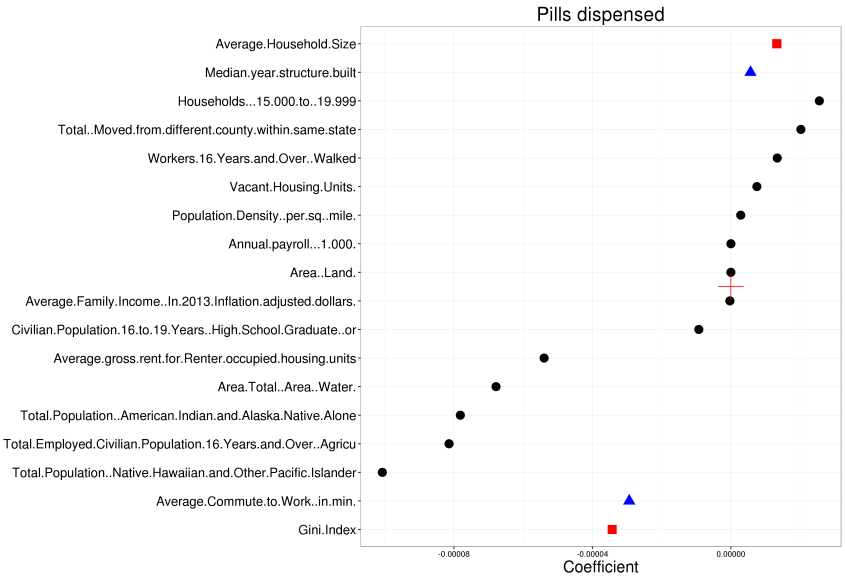
Note: This figure shows the event study plots in the four outcome variables for the Walgreens-Express Scripts separation events, split by the type of drug filled. The vertical axis shows the coefficient on the *MonthsSince* dummy variables in the event study regressions, relative to the month before the split. The horizontal axis shows the number of months before or after the local pharmacy opening. Vertical bars show 95 percent confidence intervals based on standard errors clustered at the zip code level.

2.5.6 National Prediction

I show the list of significant predictors from the national prediction of opening effects in figure 2.42. In this figure, I scale the largest outlier coefficients so to make the

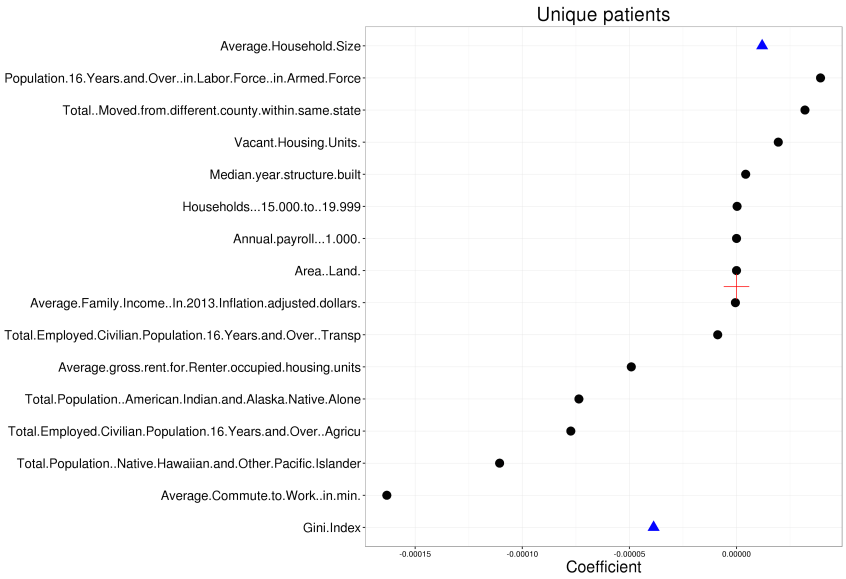
figure meaningful for all coefficients. The variables are ranked by coefficient size, with all variables above the red cross having positive coefficient values. The red triangles represent the furthest outlier coefficient values divided by 1000, and the blue squares represent the second wave of coefficient outlier values divided by 100. The black points show unscaled coefficient values. Though the predictors are different for the different outcome variables, there are patterns that emerge in which types of zip codes have the largest predicted effects. Average household size is the largest positive coefficient in each of the outcome variables, and gini index is the most negative coefficient. Thus, zip codes with many individuals per household and low inequality are predicted to have the largest effects. Based on the other predictors, larger effects are generally predicted in zip codes with lower incomes, higher populations, more households, lower minority populations, lower rent, and higher population density.

Figure 2.42: Significant Predictors: Pills Dispensed



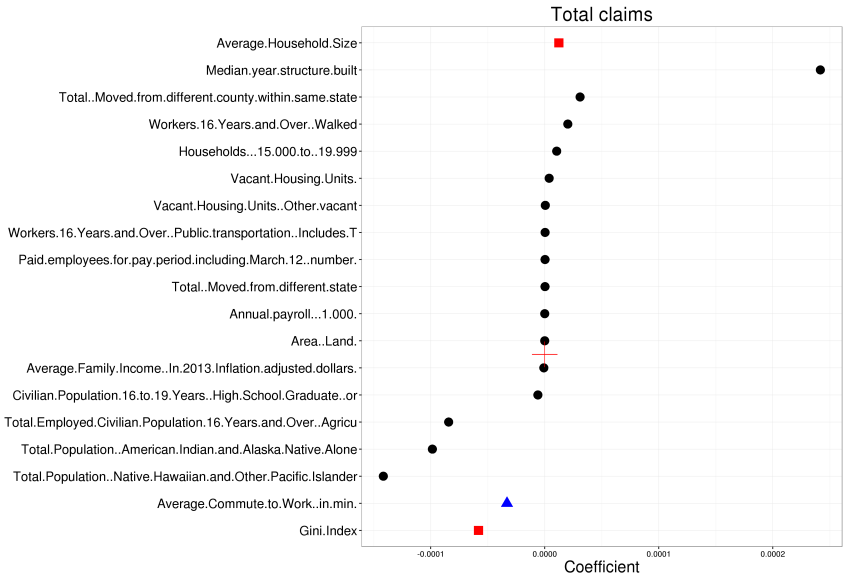
Note: This figure shows the significant predictors of the pharmacy opening effects. The predictors are ordered from top to bottom based on their significance, and the different symbols represent different scales.

Figure 2.43: Significant Predictors: Total Patients



Note: This figure shows the significant predictors of the pharmacy opening effects. The predictors are ordered from top to bottom based on their significance, and the different symbols represent different scales.

Figure 2.44: Significant Predictors: Total Claims



Note: This figure shows the significant predictors of the pharmacy opening effects. The predictors are ordered from top to bottom based on their significance, and the different symbols represent different scales.

When I expand the prediction to the national level, find a wide range of predicted effects. The summary of these effects is presented in table 2.11, which shows the mean of the predicted effect, as well as its standard deviation, median, maximum value, and minimum value. It also reports these statistics for the subset of zip codes that are within 1 standard deviation of the mean predicted effect to show the large impact outlier zip codes have on the summary statistics. The median predicted effects from opening a new pharmacy in each zip code in the United States range from 3.5 percent for claims filed to 4.9 percent for number of patients. These estimates do not take into account the costs of pharmacy openings, nor the spillover effects on neighboring zip codes.

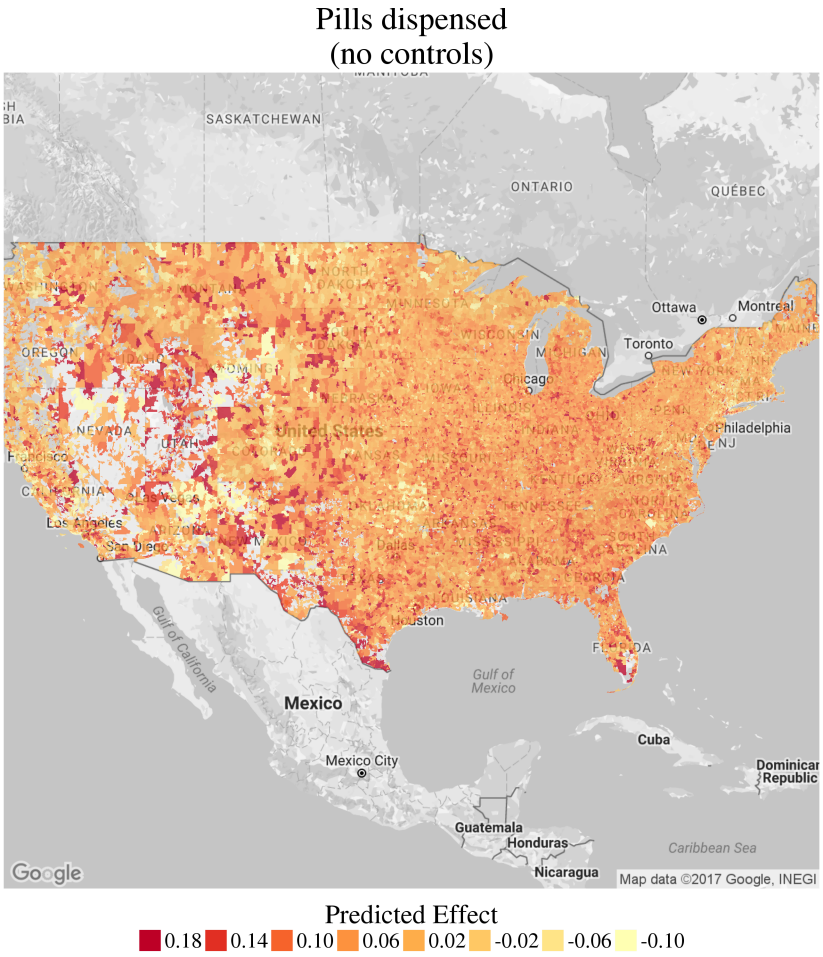
Table 2.11: Predicted Effects Summary

	Extrapolated Effects				
	Mean	SD	Median	Min	Max
Pills Dispensed	-0.16	0.466	0.037	-1.547	1.827
Pills Dispensed (within 1 SD)	0.052	0.072	0.051	-0.628	0.303
Unique Patients	0.049	0.082	0.049	-1.648	2.205
Unique Patients (within 1 SD)	0.038	0.048	0.044	-0.033	0.130
Total Claims	0.016	0.182	0.035	-2.223	2.407
Total Claims (within 1 SD)	0.039	0.062	0.046	-0.205	0.160

Note: This table shows the summary statistics for the predicted effects of opening an additional pharmacy in each zip code in the country on the three listed outcome variables. The summary statistics are reported for all zip codes, and for th zip codes with predicted effects that are within 1 standard deviation of the mean effect.

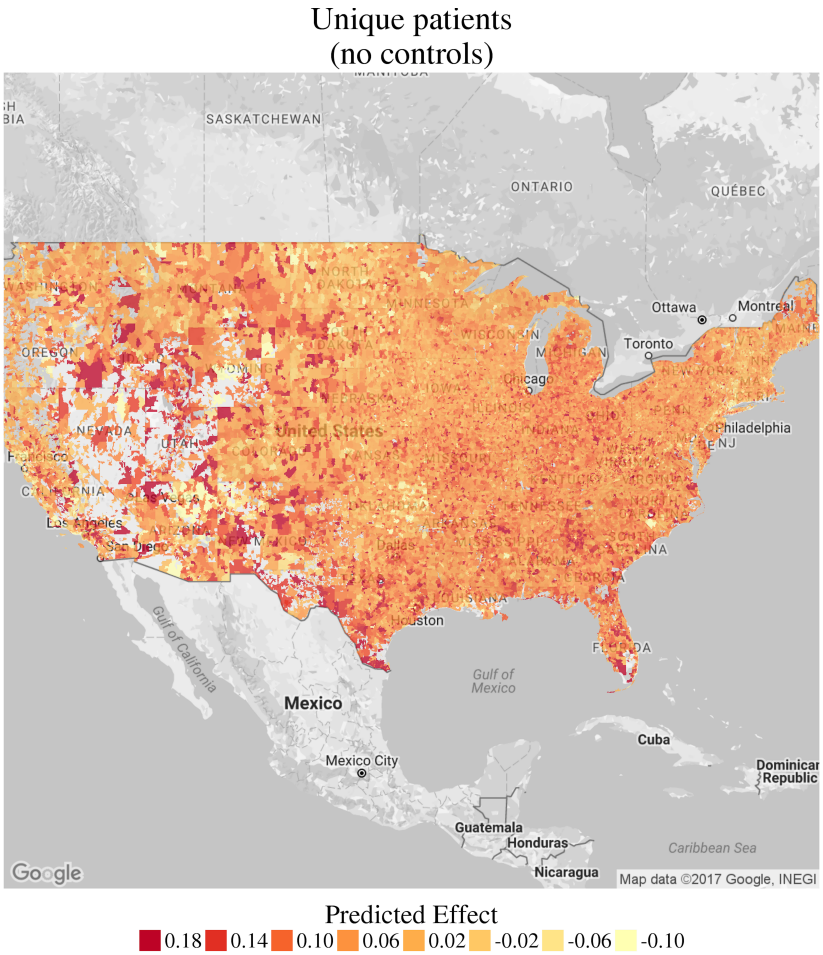
Finally, I provide a map (in figure 2.45) of the predicted effects in each zip code in the United States. One pattern visible on the map is that the predicted effects are largest in areas where access to pharmacies may be very limited. These areas may also have very few people, with potentially low baseline adherence rates, so any increased access may only improve adherence for a few individuals, driving the predicted percentage increase to high levels.

Figure 2.45: National Predicted Effects of Pharmacy Openings - Pills



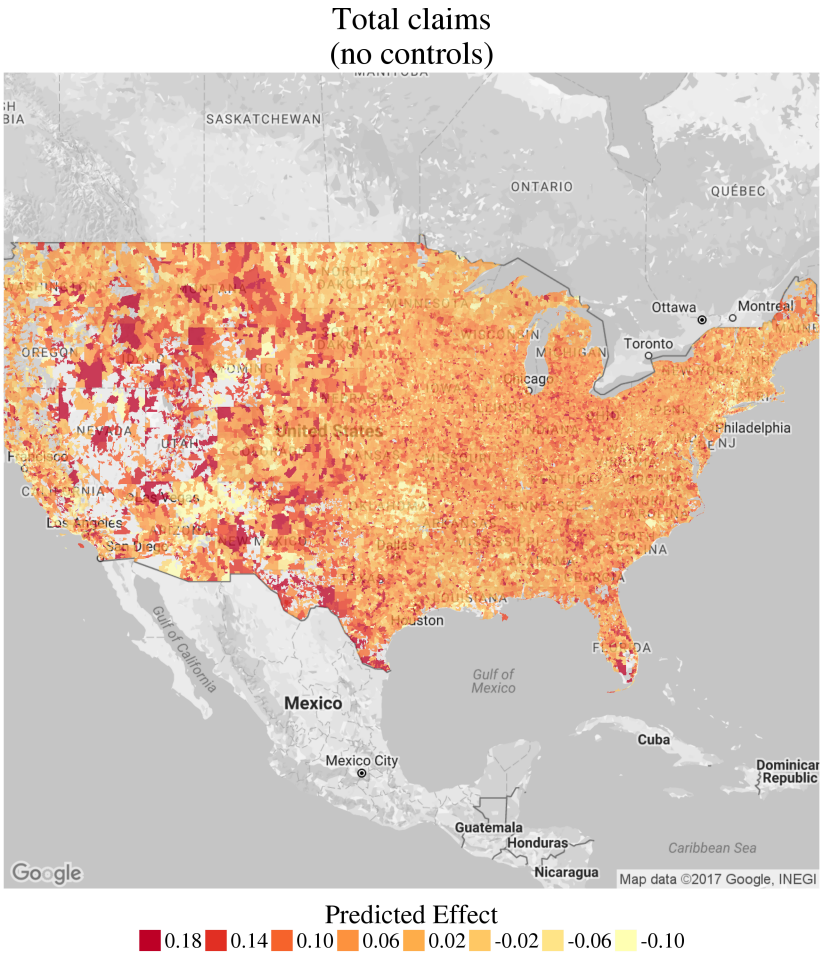
Note: This figure shows predicted effect of an additional pharmacy opening in each zip code in the United States. The shade of the zip code represents the magnitude of the predicted effect.

Figure 2.46: National Predicted Effects of Pharmacy Openings - Patients



Note: This figure shows predicted effect of an additional pharmacy opening in each zip code in the United States. The shade of the zip code represents the magnitude of the predicted effect.

Figure 2.47: National Predicted Effects of Pharmacy Openings - Claims



Note: This figure shows predicted effect of an additional pharmacy opening in each zip code in the United States. The shade of the zip code represents the magnitude of the predicted effect.

2.6 Conclusion

I have shown that pharmacy access affects prescription drug behavior, by using pharmacy openings, closings, and the network status of Walgreens in the Express

Scripts network. Pharmacy openings increase the total number of pills dispensed, the total number of patients, and the total number of claims filed by roughly 2 percent. Openings increase the number of patients filling claims for the first time by 7-8 percent in the short months following the opening. Openings do not appear to change the composition of patients in terms of the share of patients on Medicaid or the average age of patients. The effect of openings is heterogeneous across drug types and insurance types. The drug types that are affected include drugs that are crucial for patient health such as heart medications, cholesterol reducers, anti-diabetics, and antidepressants. The largest opening effects are on Medicare Advantage enrollees and patients with private insurance. The observed closings do not appear to reduce the outcome measures significantly.

When Walgreens left the Express Scripts network, the patients living in the same zip codes where Walgreens were located reduced their prescription drug filling behaviors. Total pills dispensed to patients from Walgreens zip codes decreased by 10 percent initially after the separation, then increased back to 5 percent below the baseline level after Walgreen's re-entered the Express Scripts network. The total number of claims filed by patients from Walgreens zip codes and the total number of patients from Walgreens zip codes had similar patterns, both decreasing around 9 percent to 11 percent initially, then climbing back to roughly the baseline level.

I use the opening effects combined with data from the American Community Survey and the Census Zip Code Business Patterns to predict the effects of opening an additional pharmacy in each zip code in the United States. Though this is an unlikely policy, the results are informative about where additional pharmacies may

have the largest impact. I find that the predicted effects on the total number of pills dispensed, the total number of patients, and the total number of claims filed have medians of roughly 3 percent to 5 percent. The largest predicted effects are in locations with lower incomes and low access to any businesses, including pharmacies.

This paper is valuable for understanding the effects of access to pharmacies on medication adherence. Future work could examine the effects of policy changes regarding mail order prescriptions, leveraging the distance patients must travel to their local pharmacy. A major conclusion of this paper is that there are non-monetary costs that are significant drivers in medication adherence, and dealing with these costs have potential to significantly improve prescription drug behavior.

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