Pharmacy Deserts and Medication Adherence

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Abstract

Poor medication adherence is responsible for large health care costs. In this paper, 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.

Keywords: Medication adherence; pharmacy access; prescription drugs

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 OA-PACD, 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 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.

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 appendix table A1.

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 appendix figures A1 and A2. 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.

Appendix figures A3 and A4 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.

Table 1 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 1: 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.

Appendix table A3 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, appendix table A3 also reports that there are roughly 156,000 patients living in Oregon Walgreens zip codes with "pharmacy benefit manager only" insurance. In appendix table A4, 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.

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.

4 Methods

4.1 Pharmacy Openings and Closings

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

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

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.

4.1.1 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.

4.1.2 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 appendix table A6. 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.

4.1.3 "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 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.

4.1.4 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:

$$log(Y_{azt}) = \gamma_z + \gamma_z t + \lambda_t + \theta_a + \sum_{a} \sum_{\substack{\tau=-6 \\ \tau\neq-1}}^{6} \delta_{a\tau}(MonthsSince_\tau \times \theta_a) + X'_{zt}\beta + \epsilon_{azt} \tag{2}$$

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.

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 3), I expand the time window to include all months in the data to estimate rebound effects from re-entry:

$$log(Y_{zti}) = \gamma_z + \gamma_z t + \delta_1 Out_t + \delta_2 Returned_t + X'_{zt}\beta + \epsilon_{zti} \tag{3}$$

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 3 gives the effect of Walgreens leaving the network. The effect of Walgreens returning to the network, δ_2 in equation 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.

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:

$$log(Y_{zt}) = \gamma_z + \gamma_z t + \lambda_t + \sum_z \delta_z (Post_{zt} \times \gamma_z) + X'_{zt}\beta + \epsilon_{zt} \tag{4}$$

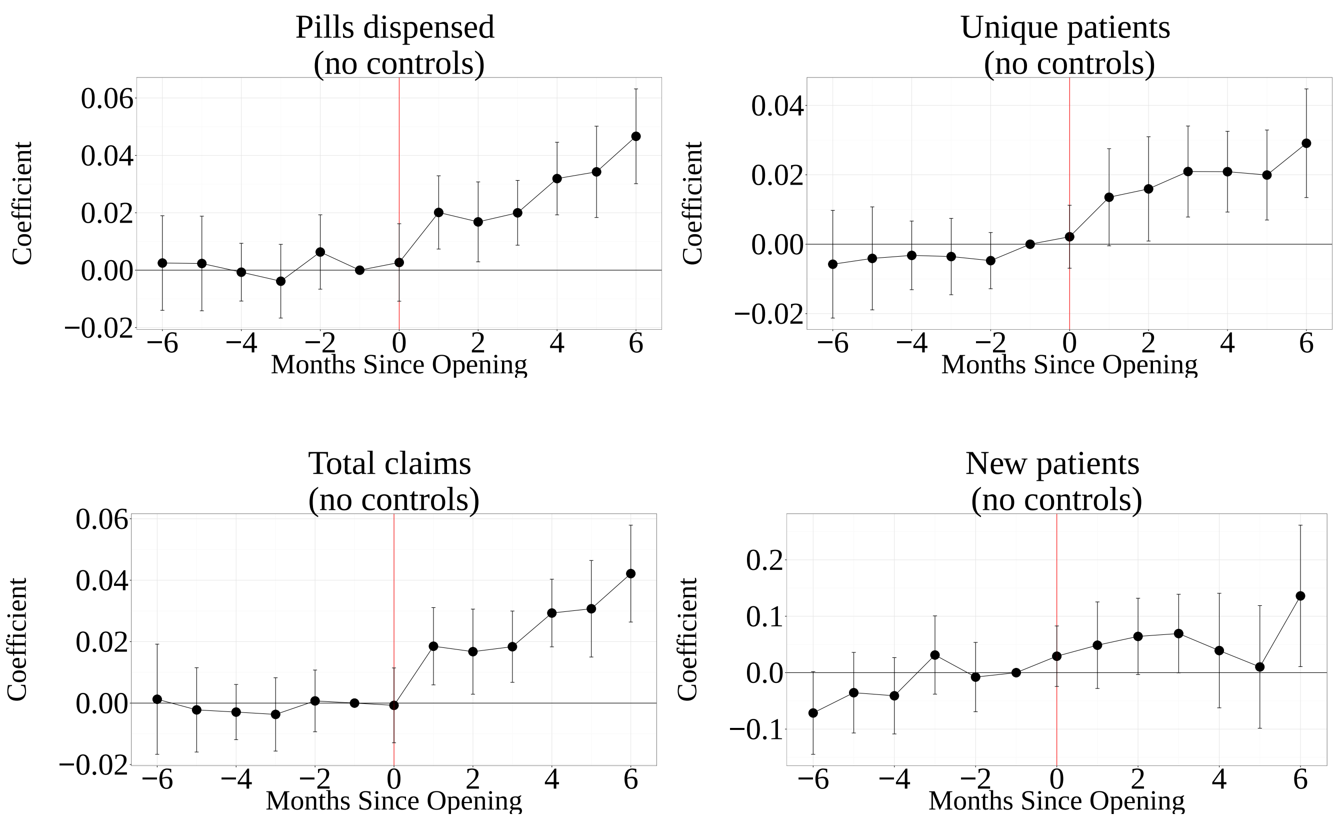
This approach gives an opening effect, $\widehat{\delta_z}$, for each zip code in Oregon in which there was an opening event. Then, I use the estimated effect as the outcome variable in a variable selection regression (LASSO) including zip code measures from the American Community Survey (ACS) and Census 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.

5 Results

5.1 Baseline Specification

Figure 1 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.

Figure 1: 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.

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 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 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, 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 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.

Table 2: 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.

5.1.1 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. Appendix figure A5 shows the analogous results to figure 1, but for closings. There does not appear to be any effect from closings on the outcome variables.

5.1.2 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 appendix figure A6, which shows that there are no effects of openings on mail-order prescriptions only.

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 appendix figure A7). This is essentially limiting to the patients who have the highest elasticity of adherence with respect to distance.

5.1.3 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.

Appendix figure A8 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.

5.1.4 Deserts

In appendix figure A9, 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.

5.2 Drug Type

I show a selection of the plots of the $\delta_{d\tau}$ from the drug selected regressions in appendix figure A10. 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.

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.

5.3 Insurance Type

In appendix figure A11, I split the baseline regressions into insurance-specific results, using the "APAC Payer" variable that has 8 insurance type categories. Appendix table A7 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.

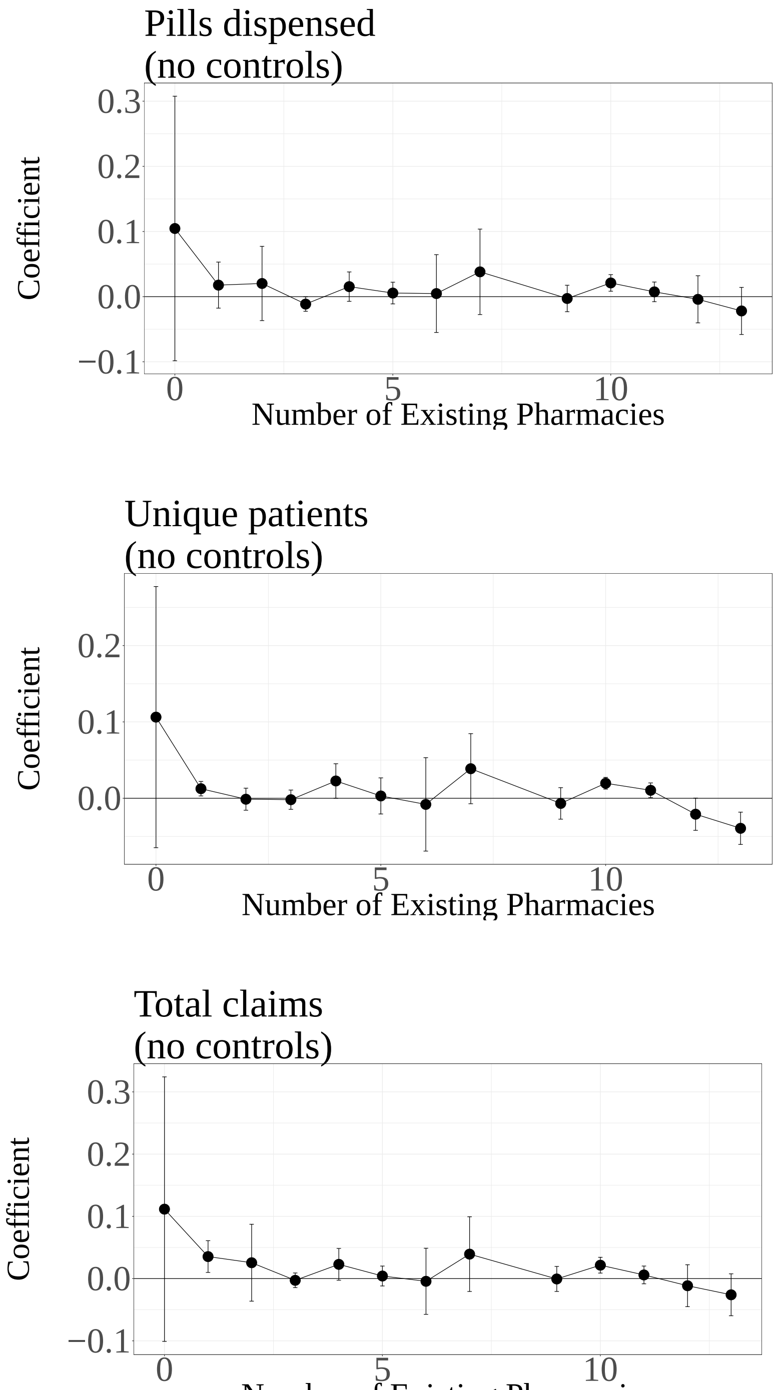
I show the "post" effect sizes split by the 8 insurance groups in appendix figure A12. This figure shows that the effect size range from roughly negative 5 percent (for the Oregon Public Employees Benefit plan) to around 15 percent (for the Medicare Advantage plans). Private insurance plans ("OTH") also have large positive effects of around 7 percent.

5.4 Adjacent Zip Codes

Appendix figure A13 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 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.

Figure 2: Effect of Openings by Number of Existing Pharmacies

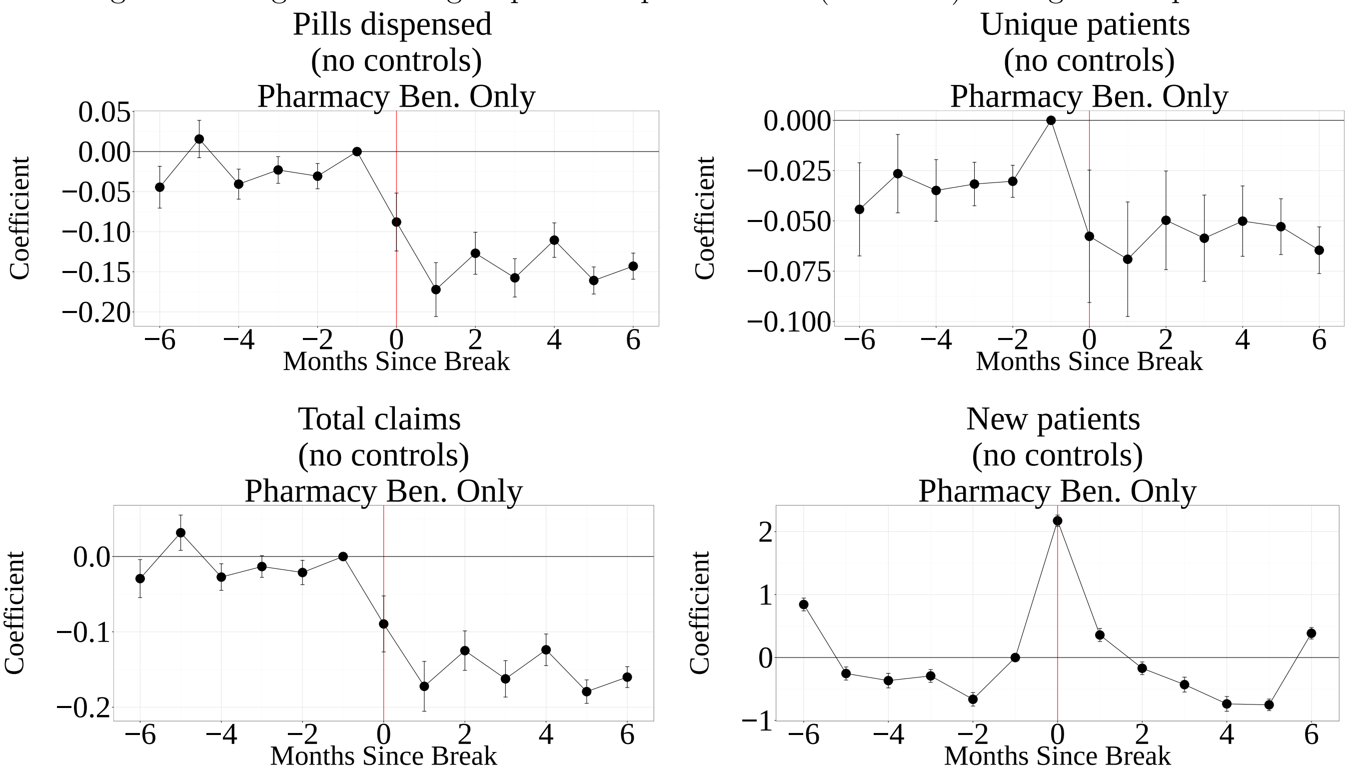


Note: This figure shows the estimated effect of an additional pharmacy opening in a zip code with the specified number of existing pharmacies, for the three adherence outcomes. The vertical axis shows the coefficient on the *Post* dummy variables in the regressions. The horizontal axis shows the number 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.

5.5 Walgreens and Express Scripts

Figure 3 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 appendix figure A15, which shows the outcome variables for the Walgreens zip codes, limited to Medicaid enrollees only as a control group.

Figure 3: 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.

Appendix figure A14 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 3 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").

Table 3: 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 $\times$ 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.

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, appendix figure A15 shows the same plots as figure 3, but for Medicaid enrollees only. This figure acts as a control group for patients with insurance 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 appendix figure A16. 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.

5.6 National Prediction

I show the list of significant predictors from the national prediction of opening effects in appendix figure A17. 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.

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 4, 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 4: 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 appendix figure A18) 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.

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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