Three Essays in Applied Health Economics

by

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For my family: Liz, Valdo, Pico, and Emily.

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ABSTRACT

The US healthcare system faces numerous challenges. In this dissertation I study issues of access to care, healthcare costs, and responses to the opioid epidemic. I take an applied economic approach, using causal inference methods to examine the effects of recent policies and changes to landscape of healthcare providers.

In the first chapter, I study urgent care centers (UCCs), which provide timely care for nonchronic, low-severity health conditions. Over the past decade, UCCs have disrupted the market for outpatient healthcare. The entry of these new providers may reduce healthcare spending by diverting care from higher cost emergency departments. Alternatively, if UCC entry increases healthcare utilization, total spending may increase. I use administrative insurance claims data from Massachusetts to estimate the effect of UCC entry on healthcare utilization and spending. The data span 2012 to 2015, during which the number of UCCs increased by 88 percent. In the months immediately following UCC entry, patients substitute away from other outpatient providers. Patients substantially reduce visits to physician offices and outpatient clinics, and slightly reduce visits to emergency departments. Overall, UCC entry increases the efficiency of the healthcare system. Aggregate spending appears to modestly decline, while in areas with few primary care providers, UCC entry increases the total number of healthcare visits.

The second chapter examines the effect of insurance coverage on utilization of prescription drugs that treat ADHD. It uses a regression discontinuity design that exploits the change in eligibility for dependent insurance coverage at age 26. From 2014-2017, the probability of insurance coverage decreased by 5 percentage points at this threshold. I examine the effect on central nervous system stimulant expenditures using an administrative database that captures all prescriptions filled at Kentucky pharmacies. At the eligibility threshold, the probability of purchasing a prescription drops by 5-7 percentage points and expenditures fall by 18-27 percent. Only 30 percent of the decrease in prescriptions purchased with insurance is offset by an increase in prescriptions purchased out-of-pocket. People also decrease expenditures by switching from branded medications to a category of similar generics that costs $104

(43 percent) less per prescription. The probability of filling a prescription recovers as people regain insurance, but decreases in expenditures persist longer-term.

The third chapter studies opioid control policies that target the prescribing behavior of health care providers. In this chapter, (co-authored by Thomas Buchmueller and Colleen Carey), we study the first comprehensive state-level policy requiring providers to access patients' opioid history before making prescribing decisions. We compare prescribers in Kentucky, which implemented this policy in 2012, to those in a control state, Indiana. Our main difference-in-differences analysis uses the universe of prescriptions filled in the two states to assess how the information provided affected prescribing behavior. We find that a significant share of low-volume providers stopped prescribing opioids altogether after the policy was implemented, though this change accounted for a small share of the reduction in total volume. The most important margin of response was to prescribe opioids to fewer patients. While providers disproportionately discontinued treating patients whose opioid histories showed the use of multiple providers, there were also economically-meaningful reductions for patients without multiple providers and single-use acute patients.

CHAPTER I

Do Urgent Care Centers Decrease Spending and Increase Access to Care?

1.1 Introduction

In recent years urgent care centers (UCCs) have disrupted the market for primary care. These facilities provide timely care for nonchronic, low-severity conditions such as sinus infections, ear infections and sprains. Unlike traditional physician offices, they treat walk-in patients and offer extended hours. Compared to emergency departments (EDs), they treat patients at a fraction of the price. According to the *Massachusetts Health Policy Commission* (2018) the average price of a low-severity UCC visit was 80% less than a low-severity ED visit. From 2013-2019 the industry grew quickly, increasing by 57% from 6,100 facilities to 9,616 (*Urgent Care Association*, 2020).

UCCs may address key policy concerns. US citizens face long wait times for family medicine appointments (*Hawkins and Associates*, 2017), and they report the highest financial barriers to care compared to people in similarly wealthy countries (*Osborn et al.*, 2016). Thus, an increase in the number of providers offering low-cost walk-in appointments may improve access to care. The US also struggles with wasteful healthcare spending, and emergency department visits for low-severity conditions contribute to the problem (*Cutler*, 2018). In 2010, 30% of ED visits were for non-emergent conditions and they accounted for $64 billion—2.5% of US health spending (*Galarraga and Pines*, 2016). UCCs may reduce such spending by providing care at lower prices.

I empirically evaluate how UCC entry affects healthcare utilization and spending. Basic demand theory predicts that UCCs increase utilization because they increase the convenience of obtaining care relative to traditional physician offices, and copays tend to be lower than EDs. But the effect of UCCs on spending is theoretically ambiguous. While substitution from EDs to UCCs may reduce spending, increases in overall healthcare utilization may increase spending.

To examine the effect of UCCs, I build a comprehensive database that tracks the year and month of all UCC openings and closures in Massachusetts from 2012-2015. Because no data vendors or government agencies comprehensively track UCCs, I draw on a wide array of sources, such as the Urgent Care Association, the National Provider Registry, the Massachusetts Department of Public Health, and individual press releases from social media and company websites. During the years studied, the UCC industry quickly expanded in Massachusetts and the number of facilities increased by 88% from 66 to 124.

I employ a staggered difference-in-differences design to estimate the effects of UCC entry in Massachusetts. The design compares changes in utilization and spending for people who live at various distance levels from UCC openings. I control for fixed differences between zip codes, and estimate the effect of UCCs based on changes in visits and spending for zip codes close to new UCCs openings. My main results estimate the effect of entry on visits and spending for a set of nonchronic primary care treatable conditions.

In the months that immediately follow a UCC opening, utilization of UCCs greatly increases in nearby zip codes. At the same time, other outpatient visits decline sharply and ED visits also slightly decline. For the sample of all Massachusetts zip codes the total number visits for primary care treatable conditions does not appear to increase. However, this result masks heterogeneous effects related to the primary care capacity in a local area. In zip codes with few primary care physicians per capita, total visits increase when UCCs open nearby.

The changes in utilization increase spending at UCCs and decrease spending at EDs and other outpatient providers. The estimates for total spending are imprecise, but suggest a modest reduction. In zip codes closest to UCC openings, the increase spending at UCCs and the decrease at other outpatient providers offset. Thus patient and insurers realize the savings in ED spending. Such savings amount to an 8% reduction in ED spending on primary care treatable conditions and a 1% reduction in total ED spending. Compared to a counterfactual scenario in which there were no UCCs in Massachusetts, if every zip code was located within 2 miles of a UCC, spending would decrease by $18 million per year. Extending the results to the broader US suggests savings of $800 million per year. In contrast, estimates from the prior literature, such as *Weinick et al.* (2010) and *Galarraga and Pines* (2016), suggest that 15-30% of ED visits could be treated by primary care providers, saving up to

20% of overall ED costs. While my estimates of savings are non-negligible, they fall substantially short of the prior estimates.

The results are robust to a variety of specifications—the increase in UCC visits and decrease in other outpatient visits is plain in event study graphs and multiple difference-in-differences specifications. Estimates of the average cohort treatment effect proposed by *Sun and Abraham* (2020) demonstrate that the results are robust to heterogeneity based on the timing of the treatment. I also conduct a placebo test to help validate the research design. I do not observe significant changes in utilization of care for emergent conditions that are rarely treated at UCCs.

My paper contributes to the literature that examines how new types of healthcare delivery models affect patients and providers. Several studies examine retail clinics, which are located in pharmacies and staffed by nurse practitioners (*Alexander et al.*, 2019; *Ashwood et al.*, 2016; *Parente and Town*, 2009). While the evidence is mixed, *Alexander et al.* (2019) convincingly show that retail clinics generate large savings by averting ED visits. Relative to retail clinics, UCCs differ in important ways and make up a substantially larger fraction of the primary care market.[1] But despite the importance of new healthcare delivery models and the differences between UCCs and retail clinics, only one other paper uses econometric methods to study UCCs. *Allen et al.* (2019) find that visits to EDs decrease during the time of day that UCCs are open.

My paper is the first to examine how UCC entry affects the totality of the primary care market. Existing work on UCCs shows that they reduce ED visits (Allen, Cummings and Hockenberry, 2019), but does not assess substitution across other outpatient providers or increases in UCC utilization. Understanding the effect of UCCs on the broader market is important for evaluating policy outcomes. Because my paper uses data on all primary care providers, I am able to generate the first estimates of the effects of UCCs on aggregate utilization and spending.

Overall, my results show that UCC openings lead to some increases the efficiency of the US healthcare system. People in areas with many primary care physicians substitute from traditional providers to UCCs, and people in areas with few primary care physicians increase the total number of visits. I find no evidence that UCCs

[1]UCCs differ from retail clinics in important ways. Retail clinics are are located in pharmacies and staffed by nurse practitioners. Thus they treat lower severity conditions compared to UCCs, which employ physicians and typically have X-ray machines and laboratory services on site. Additionally, the UCC industry is larger and continues to grow faster. In 2019 the UCCs outnumbered retail clinics by five to one. The number of UCCs increased by 38% between 2015-2019, while the number of retail clinics slightly declined (*Fein*, 2019).

provide lower quality of care relative to traditional primary care providers. Rather, the openings appear to increase visits by addressing capacity constraints and expanding access to care. Reductions in ED use are small in magnitude and the evidence suggests that UCCs only modestly decrease spending.

UCCs may also improve patient welfare in other ways. The large shift of patients to UCCs reflects high demand for their services. Future research should study the effects on convenience and other spillovers for patient welfare, such as decreases in ED wait times and increases in the availability of primary care appointments.

1.2 Institutional Background and Conceptual Framework

UCCs treat walk-in patients with low-severity conditions. They specialize in nonchronic conditions because patients cannot predict when they will contract such conditions making it impossible to schedule appointments in advance. UCCs optimize their operations in several ways to treat nonchronic low-severity conditions. They employ a generalist physician (specialized in family medicine, internal medicine, or emergency medicine) who is supported by nurses and physician assistants. UCCs offer extended hours and typically have an X-ray machine and laboratory services on site.

Market entry by UCCs may increase or decrease healthcare utilization and spending. The effects largely depend on three factors: the number of medical episodes[2] treated in the health system, the number of visits per episode, and substitution from EDs to UCCs.

When a UCC enters a market the patient choice set for primary care providers expands, which may increase the number of medical episodes treated in the health system and shift patients away from other providers. Before UCCs enter a market, some people may not seek medical treatment because of convenience factors such as long wait times or cost factors such as high copays at EDs. If UCCs reduce these barriers some people may opt to visit a UCC rather than treating their condition at home, increasing the number of medical episodes treated in the health system. Other people may already obtain treatment at traditional physician offices or EDs before UCCs enter the market. Such people may substitute to UCCs.

The overall effect on utilization depends on the quantity of medical episodes treated in the healthcare system and the amount of visits needed to treat each episode.

[2] An episode of care consists of all visits to treat a patient for a specific condition, from the onset of symptoms until the treatment is complete.

UCCs may positively or negatively affect the number of visits needed to treat each episode. For example, some physicians have voiced concerns that the quality of care at UCCs may be lower relative to the quality of care at other providers (*Chang et al.*, 2015). If UCCs provide lower quality of care, patients may require more visits to treat a given medical episode. On the other hand, if lower wait times allow patients to treat conditions before they worsen, the average number of visits per episode may decrease.

UCC entry may increase or decrease aggregate health spending per person. Substitution from EDs to UCCs decreases spending per visit because the price of UCC visits is substantially lower. Spending may also decrease if prices fall because of increased competition. However, increases in utilization may increase spending. The net effect on spending must be determined empirically.

UCC use and substitution patterns likely vary by type of condition. I study low- and high-severity nonchronic conditions. As discussed, UCCs specialize in treating low-severity nonchronic conditions, known as "primary care treatable conditions". Thus entry should most affect utilization for such conditions. UCC entry should only minimally affect visits for high-severity nonchronic conditions, known as "emergent conditions". Patients self-triage and few visit UCCs for emergent conditions because such conditions require more specialized treatment. To the extent that patients present to UCCs with such conditions, they will be referred to EDs or specialists. Thus for emergent conditions, UCC entry should not affect the number of visits to other types of providers.

The effects of UCCs on utilization may also vary based on the availability of primary care physicians (PCPs). All else equal, people who live in with few PCPs have a more limited choice set and UCC openings in such areas should increase utilization more than openings in areas that have many PCPs. Additionally, in areas with fewer PCPs people rely more heavily on EDs (*Greenwood-Ericksen and Kocher*, 2019; *Richman et al.*, 2007). Therefore, people such areas may be more likely to substitute from EDs to UCCs, lowering the average cost per visit. Because patients have fewer options, they may also be willing to travel further, and UCC openings may affect patients located further away.

1.3 Data and Descriptive Analysis

1.3.1 UCCs and Other Providers in Massachusetts

My study uses data from 2012-2015, a relatively short period during which the urgent care industry grew rapidly in Massachusetts. For many years the industry in Massachusetts lagged behind the rest of the US because Blue Cross Blue Shield, the dominant insurer in the state, reimbursed UCCs at a low rate. According to industry executives, Blue Cross Blue Shield of Massachusetts increased reimbursement for UCCs in 2009 leading to significant entry over the following years. During the years studied, the number of UCCs in Massachusetts increased by 88% from 66 to 124. Today the number of urgent care centers in Massachusetts mirrors the broader United States. In January 2020 the Urgent Care Association counted 26 UCCs per million people in Massachusetts and 27 UCCs per million people in the US.

Figure 1.1 maps UCCs that operated in 2012 and in 2015. (For details on the procedure for identifying UCC locations dates of operation see Appendix Section A.0.1). From 2012-2015, new UCCs opened in many regions that previously did not have UCCs, including along the coast, and in the metro areas surrounding Boston, Worcester, and Springfield.

Table 1.1 shows changes in access to UCCs using the Census' Zip Code Tabulation Areas. These areas broadly correspond to zip codes, but combine very small zip codes with larger ones. I use the Zip Code Tabulation Areas as the primary unit of analysis and for simplicity I refer to them as "zip codes" throughout paper.

For each of the 538 zip codes in Massachusetts, I construct several measures of access to UCCs based on the distance between the population-weighted centroid and the set of open UCCs. Across nearly all measures reported in Table 1.1 access to UCCs substantially increased between 2012 and 2015. During these years, the average distance to the closest UCC decreased from 6.7 miles to 5.4 miles (20%). The percentage of zip codes with a UCC within 2 miles increased from 22% to 30%, and the percentage within 5 miles increased from 46% to 58%.

When weighting by population, access to UCCs was higher but there was still a substantial increase between 2012 and 2015. During these years the population-weighted average distance to the closest UCC decreased from 4.4 miles to 3.2 miles (28%). The percentage of the population living in a zip code within 2 miles of a UCC increased from 35% to 51%, and the percentage within 5 miles increased from 69% to 80%.

I also gather data on other types of providers to control for other reasons why utilization of healthcare changed in Massachusetts. According to the American Medical Association, in the years studied the number of primary care physicians per capita was approximately stable in Massachusetts, increasing by 1%. The number of emergency departments (EDs) also remained stable. Based on data from the Massachusetts Department of Public Health, all 75 EDs in the state operated continuously between 2012-2015.

While the supply of most providers remained stable in Massachusetts, the number of retail clinics increased. As mentioned earlier, these providers also treat low-severity conditions, but they are staffed by nurse practitioners and operate in pharmacies. In Massachusetts CVS owns and operates all retail clinics under the name MinuteClinic.[3] In January 2012, CVS operated 46 retail clinics, and by December 2015 they operated 58. In all analyses I control for the number of retail clinics located near each zip code.

As discussed in the conceptual framework, utilization of UCCs based on the availability of primary care physicians (PCPs). Thus, I use data from the 2011 National Plan and Provider Enumeration System to categorize zip codes based on the number of PCPs working nearby. For each zip code I count the number of providers that specialized in family medicine, general medicine, internal medicine, and pediatric medicine. I then sum the number of PCPs in all zip codes within five miles and divide by the corresponding population.

Figure A.3 maps the number of PCPs per capita at the zip code level. The zip color represents the level of PCPs, with zip codes divided by quartile and darker colors representing areas with more PCPs per capita. As expected, zip codes with few PCPs tend to be located in more rural areas. For my analyses I split the sample based on whether zip codes fall below or above the median. Approximately half of the zip codes in the low group meet the US government's criteria for primary care shortage areas because they have fewer than 1 PCP per 3,500 people.

1.3.2 Insurance Claims Data

The Massachusetts All Payer Claims Database (APCD) collects all claims of Massachusetts residents with private insurance and Medicaid. For this project, Massachusetts and the University of Michigan IRB approved a dataset that included the patient's 5 digit zip code, the exact initial date of service, and the facility National

[3]For each retail clinic I obtained the opening date based on social media and Google searches. If it was not possible to find the opening date, then I estimated it based on the date that the retail clinic registered with the Massachusetts Department of Public Health. To estimate the opening date, I add two months to the registration date, which is the average time between the registration and opening dates in cases where I observe both.

Provider Identifier associated with each claim. The APCD also includes a unique patient ID code that is consistent across insurance plans. The patient ID prevents visits from being double counted for patients with coverage from multiple insurance plans.

I construct several outcome variables. I categorize visits and spending based on the type of provider visited: urgent care centers (UCCs), emergency departments (EDs), inpatient hospital facilities, and other outpatient facilities. I consider insurer spending (payments made by the insurer) and patient charges. (For details on the classification of visit types and construction of the spending variables see Appendix Section A.0.2.)

The analysis sample covers claims from all privately insured Massachusetts residents under 65 and spans January 2012 to September 2015. While Massachusetts also provided data for 2016 and 2017, those years are not directly comparable because a substantial proportion of self-insured groups declined to provide data after the US Supreme Court ruled that they were exempt from reporting requirements of all payer claims databases. I exclude October-December 2015 because providers switched from the ICD-9 condition coding system to ICD-10 in October 2015. I exclude people 65 and older because I do not observe Medicare claims.

As discussed in the conceptual framework, I study two types of nonchronic conditions: low-severity "primary care treatable conditions" and high-severity "emergent conditions". The condition categories build on *Alexander et al.* (2019), which mapped a representative set of conditions to categories that followed the spirit of *Billings et al.* (2000). *Alexander et al.* (2019) developed their categories to study retail clinics and I adapt them to better study UCCs. I include the top ten conditions seen in UCCs in the primary care treatable conditions, (adding cellulitis, eczema, and superficial injuries). To increase the number of emergent conditions I also add acute myocardial infarctions (heart attacks), which are commonly studied in the literature on emergent conditions. Additionally, I categorize closed limb fractures as primary care treatable conditions rather than emergent conditions because UCCs have physicians on staff and X-ray machines that they can use to treat or triage uncomplicated fractures.

Ultimately, the primary care treatable condition category includes urinary tract infections, upper respiratory infections (like sinus infections and bronchitis), pharyngitis, otitis media and externa, cellulitis, eczema, superficial injuries, sprains and strains, and uncomplicated fractures. The emergent condition category includes births, heart attacks, high-severity fractures, and drug poisonings.

Table 1.2 shows the conditions included in the analysis sample. It lists the corresponding ICD-9 codes, and the percentage of visits that the conditions account for. Though the list of conditions is meant to be representative and not exhaustive, primary care treatable conditions account for over half (52%) of UCC visits. They also account for a substantial proportion of ED visits (27%), and a relatively smaller percentage (10%) of all visits. Upper respiratory conditions account for the plurality of UCC visits 15%, followed by pharyngitis (sore throat), which accounts for 10% of UCC visits. Emergent conditions are uncommonly treated at UCCs. They account for only 0.3% of UCC visits, and make up a larger share (2.3%) of ED visits. I exclude all other conditions from the analysis.

Panel A of Table 1.3 shows changes over time in the number of visits and the type of provider visited. In each month there were approximately 250,000 visits for primary care treatable conditions. Between 2012-2015 the proportion treated in UCCs increased markedly from 1.2% to 5.6%. At the same time the share treated in EDs declined from 7.3% to 6.3% and the share treated by other outpatient providers declined from 91.4% to 87.8%.

Panel B shows the corresponding statistics for emergent conditions. In each month there were approximately 10,000 emergent visits and less than 1% were treated at UCCs. While the proportion of emergent conditions treated at UCCs rose, it remained below 1% in all years. The minimal rise in the number of UCC visits for emergent conditions did not meaningfully change the percentage of emergent conditions treated in other settings.

Throughout the sample period, inpatient visits accounted for less than 0.5% of visits for primary care treatable conditions and 4% of emergent visits. Because I focus on primary care treatable conditions and inpatient visits account for such a small proportion of such conditions, I exclude inpatient visits from all analyses that follow.

Figure 1.2 shows the average spending per visit for primary care treatable conditions. ED visits were substantially more expensive than UCC visits or other outpatient visits. The average ED visit cost $631 for insurers and $153 for patients. In contrast, the average UCC visit cost $141 for insurers and $48 for patients. Prices at EDs may exceed those at UCCs because EDs face higher labor and capital costs, treat more severe (and therefore more resource-intensive) versions of the disease, or provide more services. The price of UCC visits was approximately in line with the price of other outpatient visits. For insurers, the average UCC visit was $43 cheaper, while for patients it was $9 more expensive. On average emergent visits were approximately ten times as expensive, and I provide the statistics for those conditions in

Table A.1.

1.4 Econometric Analysis

The location of UCCs is clearly not random. Figure 1.1 shows that UCCs are clustered in urban and suburban areas. Thus, I use a staggered difference-in-differences model to control for location fixed effects, and identify the effect of UCC entry based on changes in utilization patterns after entry.

Per Equation (1.1) below, I model utilization and spending in zip code z and month t as function of zip code fixed effects, time fixed effects, a treatment variable *Treat*, time-varying zip code level covariates X, and an error term. In the baseline specification, the outcome variable Y is the number of visits or total spending per thousand people under 65 with private insurance. The treatment variable *Treat* is an indicator for any UCCs within 5 miles. The variables in X control for the number of retail clinics and emergency departments within 2 miles and within 2-5 miles of each zip code. I report the effect of interest β, cluster standard errors at the zip code level, and apply analytical weights based on the number of privately insured people under 65 in each zip code.

$$Y_{zt} = \alpha_z + \tau_t + \beta * Treat_{zt} + \gamma * X_{zt} + \epsilon_{zt} \tag{1.1}$$

For robustness, I report a total of three specifications based on alternate definitions of the treatment variable. The second specification uses a categorical treatment variable that denotes the number of UCCs within 5 miles. The third specification uses a categorical treatment variable for the minimum distance to the nearest UCC. It compares zip codes within 2 miles, 2-5 miles, 5-10 miles, and over 10 miles away from the nearest UCC.

The effects identified by the difference-in-differences models are unbiased if the parallel trends assumption holds. It states that in the absence of UCC openings, changes in utilization and spending would have been the same in control and treatment zip codes. The assumption would be violated if there were other contemporaneous changes that were correlated with the timing of UCC entry and affected utilization.

Studying Massachusetts should minimize changes to confounding variables. My study examines the 2012-2015 because the urgent care industry in Massachusetts grew exceptionally quickly during this period. Studying this relatively short period maximizes changes to the proportion of people living close to a UCC and minimizes changes to other confounding factors such as demographics.

Additionally, Massachusetts was insulated from many of the changes that occurred because of the Affordable Care Act. At a national level, the Affordable Care Act changed insurance coverage and numerous studies show that it affected health care utilization for primary care and emergency departments (*Garthwaite et al.*, 2017; *Sommers et al.*, 2017; *Miller and Wherry*, 2017). In contrast, Massachusetts reformed its health insurance system in 2006 and it experienced only minimal changes to insurance coverage over the period studied. As Figure A.2 shows, during period studied (2012-2015) insurance coverage for the population under 65 in Massachusetts remained remarkably stable, increasing from 94% to 95%.

Despite the advantages to studying UCCs in Massachusetts, the difference-in-differences results could still be biased. For example the parallel trend assumption could be violated if UCCs opened in areas where visits for primary care treatable conditions were growing particularly quickly.

To evaluate the plausibility of the parallel trends assumption, I also model the effects of UCC openings using the event study model in Equation (1.2). For the event study model, the treated group TG is an indicator variable that is one for zip codes where there were no UCCs within 5 miles in January 2012, but there was at least one by September 2015. For such zip codes, I define $t_z^\star$ as the month that the first UCC opened within 5 miles.

$$Y_{zt} = \alpha_z + \tau_t + TG_z * \sum_e \beta_e * \mathbb{1}(t - t_z^\star = e) + \gamma X_{zt} + \epsilon_{zt} \tag{1.2}$$

The event study model empirically evaluates the difference between control and treatment groups at each point in time relative to the first UCC opening within 5 miles. I graph the β_e coefficients to explore whether utilization and spending had similar trends in control and treatment zip codes prior to the first UCC opening. I also examine whether changes in β_e coincide with the first UCC opening within 5 miles.

Recent advances in econometrics show that heterogeneous treatment effects may bias the estimates from staggered difference-in-differences models (*Borusyak and Jaravel*, 2017; *de Chaisemartin and D'Haultfœuille*, 2020; *Goodman-Bacon*, 2018; *Sun and Abraham*, 2020). Such problems arise if the magnitude of the treatment effect is correlated with the timing of treatment. Strategic entry by UCCs could potentially create this problematic pattern. For example, if UCCs open first in areas with greater demand, then the treatment effects for zip codes where UCCs opened in 2012 could be larger than the treatment effects for zip codes where UCCs opened in 2015.

In Appendix Section A.0.3 I evaluate whether the results are robust to heterogeneous treatment effects. The results rely on statistical tests and estimators developed by *Sun and Abraham* (2020). They provide further support for the parallel trends assumption and suggest that the results are broadly robust to heterogeneity based on the timing of treatment.

1.5 Results

1.5.1 Event Study Estimates for Visits

The first set of results explore the effect of UCC openings on visits for primary care treatable conditions. As described above, in the event study the treatment group corresponds to zip codes where the first UCC within 5 miles opened between 2012-2015. Figure 1.3 shows the event studies, which plot the β_e coefficients from Equation 1.2. The x-axis represents the number of months relative to the first UCC opening within 5 miles of a zip code, and the coefficients represent the difference in visits between treatment and control zip codes. All graphs normalize the difference between treated and control zip codes to zero in the month before the UCC opening.

Panel A shows the effect on the number of UCC visits. Before the first UCC opens, the event study coefficient estimates are not statistically significant and very close to zero, suggesting that UCC visits followed approximately the same trend in treatment and control zip codes. Immediately after the first UCC opens, the trends in visits diverge; the event study shows a statistically significant increase in UCC visits in treated zip codes relative to control zip codes.

Panels B and C provide evidence that UCC openings reduce ED visits and other outpatient visits for primary care treatable conditions. In both cases the pre-period event study coefficients are not significant or trended, and in the post-period the coefficients immediately decline. The evidence is most compelling for other outpatient visits. All of the post-period coefficients are negative and the majority are statistically significant. For ED visits, nearly all of the post-period coefficients are negative, but the magnitude of the coefficients is smaller and most are not statistically significant.

Total visits, shown in Panel D, do not appear to change after UCC openings. Both in the pre- and post-periods the coefficient estimates are not statistically significantly and there is no evidence of a change in visits that coincides with UCC openings.

1.5.2 Difference-in-Differences Estimates for Visits

The event study graphs showed parallel pre-trends among control and treatment zip codes, and that UCC, ED, and other outpatient visits began to diverge immediately following UCC openings. These findings suggest that the parallel trends assumption holds and a difference-in-differences model is appropriate. The following results estimate such a model using Equation 1.1. Each reported coefficient corresponds to the estimate of β from a regression on the number of monthly visits per thousand people with private insurance.

Table 1.4 shows the estimates for primary care treatable visits. Specification 1 corresponds to a traditional difference-in-differences model in which the treatment variable is an indicator for whether the zip code has an open UCC within five miles. In each month UCC openings are associated with an increase of 2 UCC visits (77%), a decrease of 0.3 ED visits (-5%), and a decrease of 1.3 other outpatient visits (-2%) per thousand people with private insurance. The increase in UCC visits translates to a substantial 77% increase relative to the average number of UCC visits per month, and a more modest 2% increase relative to the total number of visits per month. Substitution away from other outpatient visits accounts for the majority (65%) of the increase in UCC visits. The reduction in the level of ED visits is much smaller, but it still amounts to a 5% reduction in ED visits for primary care treatable conditions. The combined reduction in ED and other outpatient visits offset 80% of the increase in UCC visits. Though the point estimate for the effect on the total number of visits is positive, it is small and not statistically significant. The upper bound for the 95% confidence interval suggests that total visits increase by no more than 1%.

Specification 2 allows the effect to vary based on the number of UCCs within five miles. The treatment variable is categorical so the estimates indicate the total effect given a certain number of UCCs (not the incremental effect). As the number of UCCs increases, UCC visits increase and other outpatient visits decrease. In zip codes with four or more UCCs in each month the number of UCC visits increases by 3.8 (148%), and the number of other outpatient visits decreases by 2.5 (-6%) per thousand people with private insurance. The size of these effects is approximately double the size of the effect in zip codes with only one UCC. In contrast, the decline in ED visits stays constant even as the number of UCCs increases. The effect on the total number of visits is not significant for any number of UCCs.

Specification 3 allows the effect to vary based on the distance to the nearest UCC. I estimate the effect at three distance levels: within 2 miles, 2-5 miles, and 5-10 miles. Because 99% of zip codes have a UCC within 20 miles the omitted category is zip codes where the nearest UCC is 10-20 miles away.

In zip codes where the nearest UCC is 5-10 miles away, UCC visits increase and ED visits slightly decrease. The magnitude of the effects roughly doubles for zip codes within 2-5 miles, and other outpatient visits also decline significantly. The magnitude increases even more in zip codes within 2 miles. In such zip codes in each month UCC visits increase by 3.6 (140%), ED visits decrease by 0.58 (-11%), and outpatient visits decrease by 2.2 (-3%) per thousand people with private insurance.

The effects estimated in Specification 3 are larger than the effects estimated in Specification 1 because they exclude zip codes within 5-10 miles of a UCC from the control group. In such zip codes UCC openings have statistically significant effects, though they are smaller in magnitude. Therefore, it is ideal to exclude such zip codes from the control group. For this reason, in the remainder of the paper I emphasize the results from Specification 3.

Overall, each specification shows the same pattern of effects for UCC openings. Decreases in other outpatient visits offset the majority the increase in UCC visits. ED visits also decline, but by substantially less. The effect on total visits is not significant.

1.5.3 Heterogeneity by PCPs per Capita

Tables 1.5 and 1.6 explore heterogeneity based on the number of primary care physicians (PCPs) per capita. The results measure the effect of UCC openings in two samples: zip codes above and zip codes below the median number of PCPs per capita in Massachusetts.

Table 1.5 shows the estimates for zip codes with many primary care physicians per capita. Like in the full sample, in such zip codes UCCs entry is associated with a large increase UCC visits, a large decrease in other outpatient visits, and a small decrease in ED visits. Specification 3 estimates that in zip codes within 2 miles of a UCC, in each month UCC visits increase by 2.5 (83%), ED visits decrease by -0.24 (-5%), other outpatient visits decrease by 3.6 (-5%) per thousand people with private insurance. In all specifications the effect on total visits is negative and not statistically significant at the 5% level.

Compared to the main results, zip codes with many PCPs differ in two respects. In these zip codes substitution away from other outpatient visits is particularly large. In all specifications the decrease in outpatient visits fully offsets the increase in UCC visits. Additionally, there appears to be no effect in zip codes that are 5-10 miles from a UCC. This suggests that people are not willing to travel to a UCC more than

5 miles away if they are in an area with many PCPs.

Table 1.6 shows the estimates for zip codes with few primary care physicians per capita. In such zip codes UCC openings increase UCC visits, decrease ED visits, and increase the total number of visits. Specification 3 estimates that in zip codes within 2 miles of a UCC, in each month UCC visits increase by 3.9 (170%), ED visits decrease by -0.7 (-15%), and total visits increase by 1.8 (3%) per thousand people with private insurance. In all specifications the effect on other outpatient visits is not significant at the 5% level.

Relative to zip codes with many PCPs, UCC openings differ in several dimensions. The increase in UCC visits is larger, the decrease in other outpatient visits is small and not statistically significant, and the effect on the total number of visits is positive and significant. Additionally, in areas with few PCPs per capita people travel farther to visit UCCs. In zip codes 5-10 miles away from the nearest UCC the number of UCC visits increases and ED visits decrease. For this reason, it is especially important to consider Specification 3, which uses zip codes greater than 10 miles away as the control group. This specification indicates that ED visits fall by 0.74 per month, three times as much as in areas with many PCPs. This translates to a substantial 15% reduction in ED visits for primary care treatable conditions.

The results broadly align with economic theory, as the time and hassle costs of scheduling an appointment may be higher in areas with fewer PCPs per capita. Per the predictions discussed in the conceptual framework, in such areas UCC openings visits increase by more, people are willing to travel farther, and ED visits decrease more. Across the different specifications, the increase in total visits suggests that 45-75% of incremental UCC visits would not have occurred without a nearby UCC.

1.5.4 Placebo tests for Emergent Conditions

Table 1.7 reports the effect of UCC entry on visits for emergent conditions. UCCs rarely treat such conditions. In every specification market entry only minimally increases monthly UCC visits by 0.01-0.02 visits per thousand people with private insurance. This amounts to 0.3-0.6% of the average number of monthly emergent visits.

In nearly all of the estimates the effect on ED visits, other outpatient visits, and total visits is not statistically significant. The results are sensible: a small increase in UCC visits should not cause meaningful substitution away from EDs or other outpatient providers. Rather, any UCC visits for emergent conditions would need to be referred to an ED or specialist.

The placebo test suggests that the timing of UCC entry is not correlated with changes in population or insurance coverage. If either of these problems arose, the number of observed insurance claims would increase in markets that UCCs entered. This would yield positive estimates for ED, other outpatient, and total visits despite minimal use of UCCs to treat emergent conditions. Reassuringly, the estimates suggest that UCC entry is not correlated with emergent visits.

1.5.5 Effects on Spending

The visits results suggest that UCCs only modestly affect spending. On aggregate visits do not appear to rise, and even in areas with few PCPs the total number of visits only increases by 3%. Substitution away from EDs could reduce spending, but the number of averted ED visits is small.

Figure A.4 shows a back-of-the-envelope calculation based on the estimated change in visits. It applies the average price per visit to the estimated change in the number of visits in zip codes within 2 miles of a UCC (Specification 3 of Table 1.4). The estimate suggests that total spending decreases by $268 (-1.5%).

Though the back-of-the-envelope calculation provides a helpful estimate, it may be biased if the visit case mix changes. For example, even within the set of primary care treatable conditions, people who substitute from EDs to UCCs may have less severe conditions than the average ED case. If so, then applying the average price of an ED visit may overstate the savings from UCC openings.

To address this problem, I directly estimate the effect of UCCs on spending using the difference-in-differences regressions. Table 1.8 shows the effect of UCC openings on insurer spending. The estimates follow a similar pattern to patient visits, but they are less precise and only the increase in UCC spending is statistically significant across all specifications. The estimates for other outpatient spending are negative in all specifications, but they are only statistically significant in Specification 3. Similarly, nearly all of the estimates for ED spending are negative, but they are only statistically significant at the 10% level for zip codes within 2 miles. The estimates for total spending vary across each specification and are never significant.

In Specification 3 the estimates suggest that UCC spending increases, and both ED and other outpatient spending decrease as the distance to the nearest UCC falls. For zip codes within 2 miles of the nearest UCC, monthly spending per thousand people with private insurance increases by $519 (98%) at UCCs, and it decreases by $264 (-7%) at EDs and $577 (-5%) at other outpatient providers. On net, spending in such zip codes decreases by $322 (-2%), but the effect is not statistically significant and in other specifications it is close to zero.

Figure 1.9 shows the effect of UCC entry on patient spending. Entry increases patient spending at UCCs and decreases spending at EDs. In most specifications, the effects on other outpatient spending and total spending are not significant.

In Specification 3, UCC spending increases and ED spending decreases as the distance to the nearest UCC falls. For zip codes within 2 miles of the nearest UCC, monthly spending per thousand people with private insurance increases by $158 (124%) at UCCs, and it decreases by $92 (-11%) at EDs. The estimates for patient spending at other outpatient providers and total spending are negative, but not statistically significant.

Overall the results show that UCC entry increases spending at UCCs and decreases spending at EDs and other outpatient providers. In line with the estimated changes in visits, UCC entry has the largest effects on zip codes within 2 miles. In such zip codes the point estimates suggest that total spending decreases by 2%, but the estimates are not statistically significant and vary widely across specifications. Even in such zip codes, the 95% confidence interval rules out decreases in spending greater than 5%.

1.6 Discussion

1.6.1 Sizing the Total Effect of UCCs

Between 2012-2015, the number of Massachusetts zip codes within 2 miles of a UCCs increased by 60. The privately insured population under 65 living within 2 miles of a UCC increased by 870,000, while the population living 2-5 miles away decreased by 230,000, the population living 5-10 miles away decreased by 360,000, and the population living more than 10 miles away decreased by 290,000. Applying the estimates for primary care treatable conditions to the affected population suggests that in 2015 there were approximately 26,000 more UCC visits, 3,400 fewer ED visits, and 16,000 fewer other outpatient visits because of UCCs that opened between 2012-2015.[4] Similarly, applying spending estimates suggests that ED spending decreased by $2.3 million and net spending decreased by $2 million per year.

The total number of privately insured people under 65 in Massachusetts was 4.3 million, substantially more than the population living in markets where UCCs entered. There is no guarantee that effects would be the same for the wider population, but it is

[4]The reported estimates use Specification 3, but they are similar if they apply the estimates from Specification 1.

still helpful to get a ballpark estimate of the potential savings from UCCs. If we apply the same estimates to the larger population, if all zip codes had a UCC within 2 miles there would be 190,000 more UCC visits, 30,000 fewer ED visits, and 110,000 fewer other outpatient visits compared to a scenario without any UCCs. The incremental savings would be $18 million per year, approximately 8% of the average annual ED spending on such conditions for privately insured people under 65 in Massachusetts.

1.6.2 Implications of Visits and Spending Analyses

This paper shows that UCC entry substantially changes utilization patterns for primary care treatable conditions. For such conditions, increases in UCC visits are largely offset by substitution away from other outpatient providers, and ED visits modestly decline. The total number of visits does not appear to increase, except in areas with few primary care physicians per capita. The results rule out meaningful increases in spending, and suggest that it slightly declines.

The substantial substitution from traditional primary care providers to UCCs should not affect spending, but may have meaningful effects on patient welfare. The substitution suggests that many patients prefer to visit UCCs. Because UCCs visits cost patients approximately the same amount if not slightly more, patients presumably prefer them because they are more convenient. Patients may perceive UCCs as more convenient because openings decrease travel costs or waiting times.

Decreases in wait times are especially important in the US healthcare system. *Hawkins and Associates* (2017) finds that the average wait for new family medicine appointments was 29 days in large metropolitan areas and 54 days in mid-sized metropolitan areas. Thus, people who do not have an established patient relationship with a family medicine physician may reap particularly large benefits from UCC openings. Decreases in wait times may also improve the welfare of the broader population. *Osborn et al.* (2016) finds that approximately half of US citizens reported waiting two or more days for primary care appointments, and one quarter of the population reported waiting six or more days. Thus, substitution from traditional primary care to UCCs may dramatically reduce wait times.

Relative to the effect on traditional primary care providers, UCCs entry reduces ED visits by less, but this margin of substitution remains important. In zip codes within two miles ED visits for primary care treatable conditions fall by approximately 8%. This is a consequential reduction because UCC visits are substantially cheaper, and ED visits for primary care treatable conditions contribute to high levels of wasteful spending in the US. Reductions in ED visits for such conditions may also lower ED wait times, crowding, and exposure to diseases. Overall, UCCs make a meaningful contribution to reducing ED visits, but are not a silver bullet. Policymakers should consider UCCs as a part of a broader set of strategies to reduce ED visits.

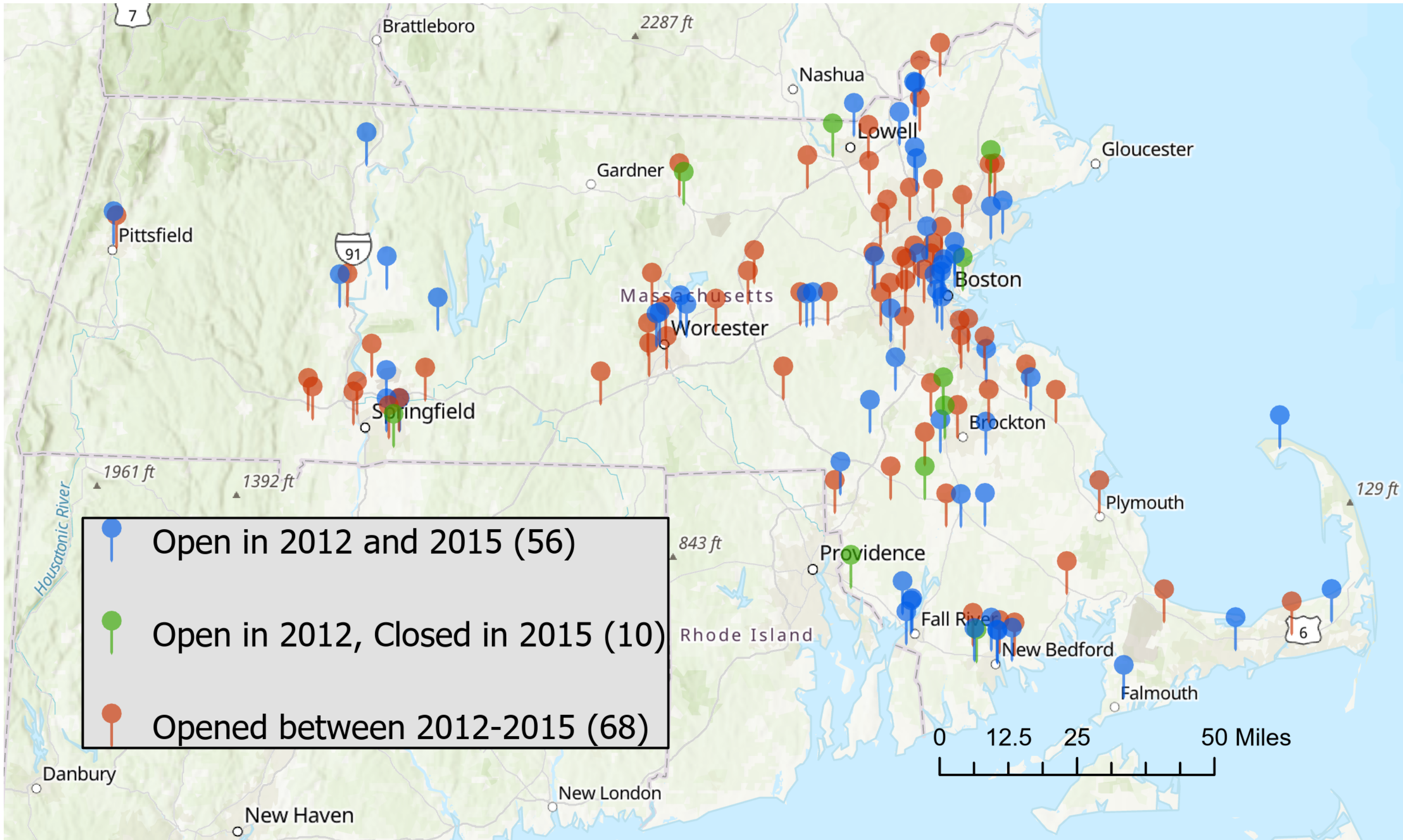
Heterogeneous responses to UCC openings have implications about quality of care and capacity constraints. Some physicians have expressed concerns that the quality of care at UCCs may be low and that substitution from traditional primary care providers may increase the total number of visits needed to treat a condition (*Chang et al.*, 2015). In zip codes with high numbers of primary care physicians per capita, I observe substitution from traditional primary care to UCCs, but the total number of visits does not increase. Thus, I find no evidence of substantial differences in quality of care at UCCs relative to traditional primary care providers.

Responses to UCC openings differ in areas with few primary care physicians. In such areas, people only minimally substitute between traditional primary care and UCCs, but the total number of visits increases. This pattern suggests that increases in visits are not caused by differences in quality of care at UCCs. Rather, it suggests UCC openings in areas with few primary care physicians address capacity constraints and increase access to care.

The results also shed light on the importance of convenience as barrier to obtaining care. As discussed earlier, in areas with many primary care physicians many people switch from traditional primary care to UCCs, presumably because of convenience. But market entry of UCCs does not increase overall utilization in such areas. This suggests that in areas with many primary care physicians, convenience factors like wait times and travel distances do not deter people from obtaining care. In contrast, convenience factors *do* appear to deter people from obtaining care in areas with fewer primary care physicians per capita.

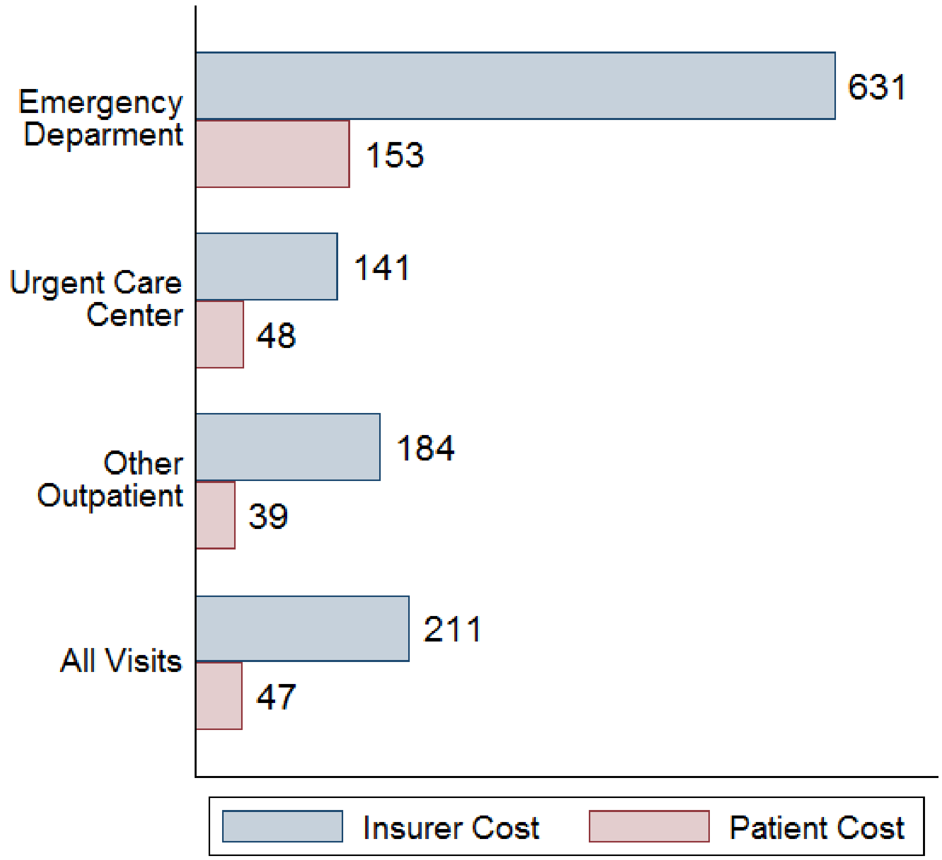
Many questions remain about the effects of UCCs on the US healthcare system. Future studies should quantify the effects on patient welfare, such as reductions in travel distances and wait times. They should also study spillovers on the broader healthcare system, such as reductions in ED wait times and wait times at traditional physician offices.

Figure 1.1: Massachusetts Urgent Care Centers, 2012 and 2015



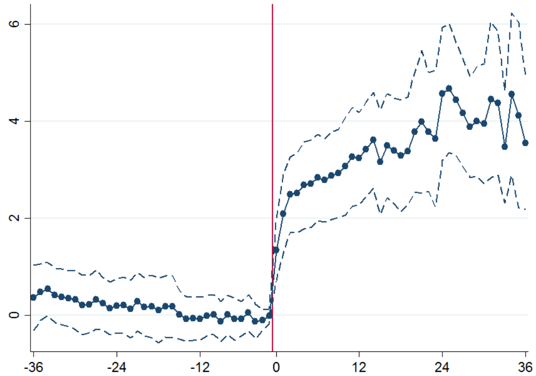
Note: UCC locations based on data compiled from the Massachusetts Health Policy Commission, ReferenceUSA, the Urgent Care Association, and the National Plan and Provider Enumeration System. For details on the construction of the UCC database see the Appendix.

Figure 1.2: Average Visit Prices for Primary Care Treatable Conditions

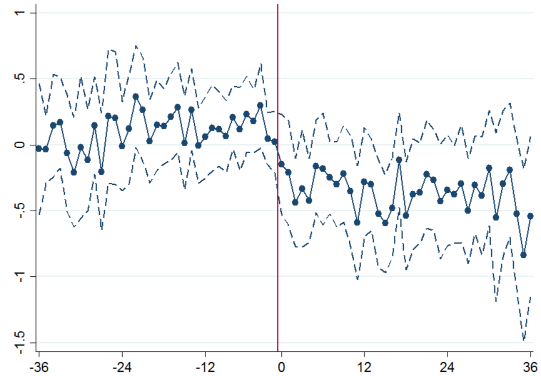


Note: The figure shows average spending per visit based on author calculations from the Massachusetts All Payer Claims Database using records from 2012-2015.

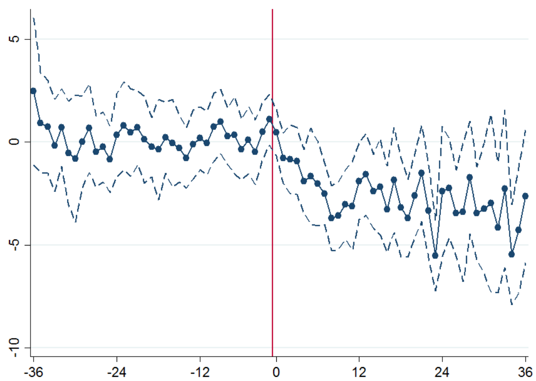
Figure 1.3: Event Studies for Visits for Primary Care Treatable Conditions



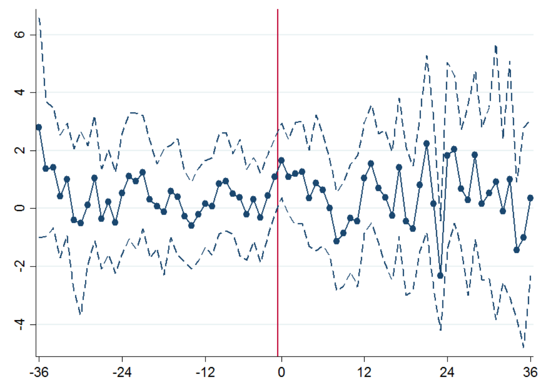
(a) UCC



(b) ED



(c) Other Outpatient



(d) Total

Note: The figure shows the effect of UCC entry on visits to different types of providers using zip code level data. Visits measure the number of monthly visits by privately insured people under 65 per thousand. The event study is estimated using Equation 1.2. The x-axis denotes the number of months relative to UCC entry. The red vertical line is placed immediately before the month that the UCC opened. The difference between control and treatment zip codes is normalized to zero for the month immediately before the UCC opened.

Table 1.1: Access to UCCs by Zip Code

Year	Distance to Closest UCC	UCC w/in 2mi	UCC w/in 5mi	UCC w/in 10mi	UCC w/in 20mi
		Panel A: Unweighted Average			
2012	6.7	0.22	0.47	0.76	0.99
2013	6.2	0.25	0.51	0.79	0.99
2014	5.9	0.27	0.54	0.80	0.99
2015	5.4	0.30	0.58	0.83	0.99
		Panel B: Population-Weighted Average			
2012	4.3	0.35	0.70	0.90	1.00
2013	4.0	0.40	0.73	0.92	1.00
2014	3.6	0.46	0.76	0.93	1.00
2015	3.1	0.50	0.81	0.95	1.00

Note: The table shows the average distance in miles from the zip code centroid to the nearest UCC and the percent of zip codes with a UCC within a given distance. Panel A provides the raw averages for Massachusetts zip codes. Panel B provides the population-weighted averages for Massachusetts zip codes.

Table 1.2: Visits by Condition Type

Conditions	ICD-9 Codes	UCC	ED	All
Primary Care Treatable		51.8%	26.4%	9.8%
UTI	599, 595	4.1%	1.7%	0.8%
Conjunctivitis	372	2.1%	0.5%	0.3%
Sinusitis/Bronchitis/URTI	460-461, 465-466, 473, 490	14.5%	2.9%	1.8%
Phayringitis	462-463, 034	9.9%	1.9%	1.4%
Otitis Media/Externa	3801, 3802, 381-382	4.1%	1.1%	0.9%
Cellulitis/Eczema	681, 682, 692	5.5%	3.4%	1.0%
Superficial Injuries	AHRQ Clinical Classification Software	4.5%	5.1%	0.5%
Sprains/Strains	AHRQ Clinical Classification Software	5.2%	6.1%	2.2%
Fracture, uncomplicated	Closed arm or leg fractures, excluding femur	1.7%	3.5%	0.8%
Emergent		0.3%	2.3%	0.4%
Drug Poisoning	9090, 9095, 960-979, 9952, E85, E9800-E9805	0.1%	0.7%	0.1%
Birth	V27, 650	0.0%	0.0%	0.1%
Fracture, serious	Other fractures	0.2%	1.2%	0.2%
AMI	410	0.0%	0.4%	0.1%
Other		47.9%	71.3%	89.8%

Note: For each type of provider the table displays the percent of visits that fall in each condition category. E.g., primary care treatable conditions account for 52% of all UCC visits. In each column primary care treatable, emergent, and other conditions sum to 100%. UTI corresponds to urinary tract infections, URTI corresponds to upper respiratory tract infections, and AMI corresponds to acute myocardial infarction.

Table 1.3: Visits over Time by Provider Type and Condition

Year	Monthly Visits	UCC	ED	Inpatient	Other
		Panel A: Primary Care Treatable			
2012	267,469	1.2%	7.3%	0.2%	91.4%
2013	260,925	2.5%	6.9%	0.2%	90.4%
2014	245,193	4.3%	6.3%	0.3%	89.2%
2015	241,399	5.6%	6.3%	0.3%	87.8%
		Panel B: Emergent			
2012	10,445	0.2%	13.3%	3.7%	82.8%
2013	11,068	0.3%	12.8%	3.8%	83.0%
2014	10,139	0.5%	12.9%	3.8%	82.8%
2015	10,329	0.7%	13.3%	3.8%	82.2%

Note: The table shows the number of monthly visits in each year by provider type and condition. The sample consists of all privately insured patients in Massachusetts under 65. The conditions in each category are reported in Table 1.2.

Table 1.4: Difference-in-Differences Regressions for Primary Care Treatable Visits

VARIABLES	(1) UCC	(2) ED	(3) Other	(4) Total
		Specification 1		
Ind UCC <5 mi	1.96***	-0.25***	-1.28***	0.43
	(0.37)	(0.08)	(0.45)	(0.43)
		Specification 2		
1 UCC <5 mi	1.97***	-0.24***	-1.28***	0.45
	(0.40)	(0.09)	(0.46)	(0.41)
2 UCCs <5 mi	2.53***	-0.23**	-1.69***	0.61
	(0.45)	(0.09)	(0.54)	(0.62)
3 UCCs <5 mi	3.37***	-0.25***	-2.80***	0.32
	(0.47)	(0.09)	(0.61)	(0.67)
4+ UCCs <5 mi	3.84***	-0.18*	-2.76***	0.90
	(0.47)	(0.10)	(0.77)	(0.82)
		Specification 3		
Ind closest UCC <2 mi	3.63***	-0.58***	-2.24***	0.81
	(0.52)	(0.16)	(0.71)	(0.71)
Ind closest UCC 2-5 mi	2.68***	-0.50***	-1.71***	0.48
	(0.47)	(0.17)	(0.52)	(0.58)
Ind closest UCC 5-10 mi	1.12***	-0.31**	-0.65	0.15
	(0.40)	(0.14)	(0.40)	(0.46)
Observations	23,850	23,850	23,850	23,850
Number of zip codes	530	530	530	530
Mean of outcome in levels	2.6	5.2	66.1	73.9

Note: The table shows the estimates for the effect of UCCs on primary care treatable visits. The reported coefficients correspond to β from Equation 1.1. Standard errors clustered at the zip code level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Heterogeneity for Primary Care Treatable Visits: Zip codes with Many Primary Care Physicians per Capita

VARIABLES	(1) UCC	(2) ED	(3) Other	(4) Total
			Specification 1	
Ind UCC <5 mi	1.74**	-0.21**	-2.03**	-0.50
	(0.70)	(0.09)	(0.95)	(0.96)
			Specification 2	
1 UCC <5 mi	1.58**	-0.20**	-1.81*	-0.42
	(0.75)	(0.09)	(1.00)	(0.88)
2 UCCs <5 mi	2.30***	-0.22**	-2.71***	-0.63
	(0.80)	(0.10)	(1.02)	(1.18)
3 UCCs <5 mi	3.04***	-0.26**	-3.95***	-1.17
	(0.79)	(0.11)	(1.11)	(1.20)
4+ UCCs <5 mi	3.40***	-0.20*	-4.18***	-0.99
	(0.79)	(0.12)	(1.24)	(1.32)
			Specification 3	
Ind closest UCC <2 mi	2.58***	-0.24*	-3.62***	-1.28
	(0.82)	(0.15)	(1.36)	(1.18)
Ind closest UCC 2-5 mi	1.50*	-0.17	-2.99**	-1.66
	(0.78)	(0.14)	(1.20)	(1.14)
Ind closest UCC 5-10 mi	-0.01	0.02	-1.10	-1.09*
	(0.47)	(0.11)	(0.74)	(0.66)
Observations	11,925	11,925	11,925	11,925
Number of zip codes	265	265	265	265
Mean of outcome in levels	3.0	5.4	73.2	81.5

Note: The sample includes zip codes where the number of primary care physicians was above the median for Massachusetts. The table shows the estimates for the effect of UCCs on primary care treatable visits. The reported coefficients correspond to β from Equation 1.1. Standard errors clustered at the zip code level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Heterogeneity for Primary Care Treatable Visits: Zip codes with Few Primary Care Physicians per Capita

VARIABLES	(1) UCC	(2) ED	(3) Other	(4) Total
		Specification 1		
Ind UCC <5 mi	1.94***	-0.22**	-0.42	1.30***
	(0.40)	(0.11)	(0.48)	(0.44)
		Specification 2		
1 UCC <5 mi	2.05***	-0.22*	-0.54	1.29***
	(0.43)	(0.12)	(0.49)	(0.46)
2 UCCs <5 mi	2.51***	-0.19	-0.22	2.11***
	(0.51)	(0.13)	(0.54)	(0.56)
3 UCCs <5 mi	3.55***	-0.16	-1.08*	2.31***
	(0.59)	(0.13)	(0.60)	(0.71)
4+ UCCs <5 mi	4.33***	-0.09	-1.04	3.20***
	(0.57)	(0.17)	(0.84)	(0.96)
		Specification 3		
Ind closest UCC <2 mi	3.92***	-0.74***	-1.37	1.81*
	(0.58)	(0.19)	(0.97)	(1.05)
Ind closest UCC 2-5 mi	3.00***	-0.57***	-0.14	2.28***
	(0.44)	(0.19)	(0.68)	(0.81)
Ind closest UCC 5-10 mi	1.67***	-0.50***	-0.15	1.02
	(0.44)	(0.16)	(0.55)	(0.64)
Observations	11,925	11,925	11,925	11,925
Number of zip codes	265	265	265	265
Mean of outcome in levels	2.2	5.1	59.0	66.3

Note: The sample includes zip codes where the number of primary care physicians was below the median for Massachusetts. The table shows the estimates for the effect of UCCs on primary care treatable visits. The reported coefficients correspond to β from Equation 1.1. Standard errors clustered at the zip code level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Difference-in-Differences Regressions for Emergent Visits

VARIABLES	(1) UCC	(2) ED	(3) Other	(4) Total
		Specification 1		
Ind UCC <5 mi	0.01***	-0.01	0.01	0.02
	(0.00)	(0.01)	(0.08)	(0.09)
		Specification 2		
1 UCC <5 mi	0.01***	-0.01	0.01	0.01
	(0.00)	(0.01)	(0.08)	(0.09)
2 UCCs <5 mi	0.01***	-0.02	0.05	0.04
	(0.00)	(0.01)	(0.10)	(0.10)
3 UCCs <5 mi	0.01***	-0.00	0.00	0.01
	(0.00)	(0.01)	(0.10)	(0.11)
4+ UCCs <5 mi	0.01***	-0.01	0.06	0.07
	(0.00)	(0.02)	(0.11)	(0.11)
		Specification 3		
Ind closest UCC <2 mi	0.02***	0.01	0.19	0.22
	(0.01)	(0.03)	(0.13)	(0.14)
Ind closest UCC 2-5 mi	0.02***	0.01	0.15	0.19
	(0.01)	(0.02)	(0.11)	(0.12)
Ind closest UCC 5-10 mi	0.01**	0.02	0.17**	0.21**
	(0.00)	(0.02)	(0.09)	(0.09)
Observations	23,850	23,850	23,850	23,850
Number of zip codes	530	530	530	530
Mean of outcome	0.0	0.4	2.5	3.0

Note: The table shows the estimates for the effect of UCCs on emergent visits. The reported coefficients correspond to β from Equation 1.1. Standard errors clustered at the zip code level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.8: Regressions for Insurer Spending on Primary Care Treatable Conditions

VARIABLES	(1) UCC	(2) ED	(3) Other	(4) Total
		Specification 1		
Ind UCC <5 mi	262***	-100	-171	-9
	(51)	(75)	(112)	(141)
		Specification 2		
1 UCC <5 mi	264***	-94	-175	-4
	(54)	(77)	(117)	(146)
2 UCCs <5 mi	333***	-84	-180	68
	(60)	(79)	(138)	(167)
3 UCCs <5 mi	442***	-17	-289*	135
	(61)	(87)	(161)	(194)
4+ UCCs <5 mi	510***	5	-259	257
	(61)	(99)	(195)	(233)
		Specification 3		
Ind closest UCC <2 mi	519***	-264*	-577***	-322
	(92)	(143)	(172)	(227)
Ind closest UCC 2-5 mi	390***	-189	-377***	-177
	(83)	(146)	(144)	(204)
Ind closest UCC 5-10 mi	187**	-125	-298**	-237
	(72)	(121)	(135)	(188)
Observations	23,850	23,850	23,850	23,850
Number of zip codes	530	530	530	530
Mean of outcome in levels	383	3473	11434	15290

Note: The table shows the estimates for the effect of UCCs on insurer spending for primary care treatable conditions. The reported coefficients correspond to β from Equation 1.1. Standard errors clustered at the zip code level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Regressions for Patient Spending on Primary Care Treatable Conditions

VARIABLES	(1) UCC	(2) ED	(3) Other	(4) Total
		Specification 1		
Ind UCC <5 mi	86***	-33*	78	131*
	(17)	(19)	(63)	(74)
		Specification 2		
1 UCC <5 mi	84***	-30	96	150**
	(18)	(20)	(62)	(73)
2 UCCs <5 mi	113***	-41**	9	81
	(20)	(19)	(66)	(81)
3 UCCs <5 mi	145***	-51**	34	129
	(21)	(20)	(75)	(87)
4+ UCCs <5 mi	164***	-45*	13	132
	(21)	(24)	(76)	(91)
		Specification 3		
Ind closest UCC <2 mi	158***	-92***	-112*	-46
	(25)	(31)	(63)	(75)
Ind closest UCC 2-5 mi	117***	-72**	3	48
	(22)	(31)	(54)	(67)
Ind closest UCC 5-10 mi	49**	-51**	-122***	-124**
	(20)	(25)	(40)	(50)
Observations	23,850	23,850	23,850	23,850
Number of zip codes	530	530	530	530
Mean of outcome in levels	127	804	2446	3377

Note: The table shows the estimates for the effect of UCCs on patient spending for primary care treatable conditions. The reported coefficients correspond to β from Equation 1.1. Standard errors clustered at the zip code level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

CHAPTER II

How Does Insurance Affect Prescription Drug Utilization? Evidence from ADHD Medications.

2.1 Introduction

Because of high prices, US spending on prescription drugs is among the highest in the world (*OECD*, 2015) and access is mediated by insurance coverage. As a result, insurance expansions affect both prescription drug access and spending. Between 2013-2015, when much of the Affordable Care Act (ACA) was implemented, spending on prescriptions grew over 10% per year, more than any other health care category (*Martin et al.*, 2016). Over the same period, utilization of prescription drugs by low-income people increased substantially in states that expanded Medicaid (*Cher et al.*, 2019; *Ghosh et al.*, 2019; *Wen et al.*, 2016).

The ACA also increased insurance coverage among young adults, but few studies have examined the effects on prescription drug utilization for this age group. Between 2007 and 2016 the probability of insurance coverage for people aged 18-29 increased by 13 percentage points, more than any other age group. Further expansions may also disproportionately affect young adults because they remain the age group with the lowest rate of insurance coverage (83% in 2016). Understanding the effect of insurance on prescription drug utilization by young adults should be considered a policy priority.

I study the effect of insurance on young adults with attention-deficit/hyperactivity disorder (ADHD), a common chronic mental health condition. Physicians frequently prescribe central nervous system (CNS) stimulants to treat ADHD, (they are rarely prescribed to treat other conditions). I focus on CNS stimulants for several reasons. People aged 18-29 spend $1.3 billion on CNS stimulants per year, which is second only to spending on contraceptives.[1][2] Additionally, data on CNS stimulants has several advantages relative to data on other medications. Because they are classified as Schedule II substances all purchases of CNS stimulants are tracked by state prescription monitoring drug programs (PDMPs). This study uses data from Kentucky's PDMP, KASPER.[3] The PDMP tracks all DEA-scheduled prescriptions filled in Kentucky, which had a population of 4.5 million in 2018. The data is more complete and accurate than survey data, (which is subject to reporting error and small sample sizes), and claims data, (which does not capture purchases made by uninsured people). It is one of the only administrative sources with data on utilization of prescriptions that can follow people who lose insurance coverage or switch policies.

I examine the effects of insurance on two outcomes: access (represented by the probability of filling a prescription) and prescription spending. Insurance improves access by increasing the quantity of prescriptions consumed, which also increases spending. But insurance may increase spending more than access. With prescriptions, insurance may induce people to purchase more expensive drugs, such as branded medications. In this paper, I explore the overall effect of insurance on access and spending, and then measure the effects on the types of drugs people purchase.

My study uses a regression discontinuity (RD) design based on the maximum age children are eligible for dependent coverage. It builds on a small set of studies that use this method to examine the effects of insurance coverage on healthcare utilization by young adults. *Anderson et al.* (2012, 2014) find that hospitalizations and emergency department use decline at the age cutoff for dependent coverage. And more recently, three studies use the same strategy to examine health outcomes and utilization for young adults in survey data (*Lee*, 2018; *Lee and Kim*, 2020; *Nguyen and Yörük*, 2020). The RD methodology is particularly well suited to analyze CNS stimulants. To reduce the risks of misuse, a variety of DEA regulations restrict prescribing of CNS stimulants and limit the ability of people to stockpile the drugs. For example, approximately 99% of the prescriptions supply no more than 31 days of medications. Though people may anticipate losing dependent coverage, they are limited in their ability to stockpile such drugs.

I begin by examining the probability of insurance coverage using data from the

[1]The spending figure was calculated from the Medical Expenditure Panel Survey years 2014-2016.

[2]Overall, spending on CNS medications totals \$9 billion per year in the US. It is 12^{th} highest among all drug categories.

[3]Kentucky was chosen because its PDMP regulations allow it to share data with researchers and it has the resources to fulfill customized data requests. KASPER was able to share the birth month of people who purchased prescriptions, which was necessary for the empirical design used in this paper.

Medical Expenditure Panel Survey. The eligibility criteria for dependent coverage decreases the probability of insurance coverage by approximately 5 percentage points at 26. The decrease is similar in size to the much more commonly studied change at 65 (*Barcellos and Jacobson*, 2015; *Card et al.*, 2008, 2009; *Decker*, 2005). Among the population with chronic conditions, insurance coverage falls by 2-4 percentage points, and a similar fraction of the population switches their insurance coverage.

The month that dependent eligibility expires, use of CNS stimulants falls sharply. Among consumers of CNS stimulants, the probability of purchasing a prescription decreases by 2-3 percentage points (10-13%) and average monthly spending decreases by $5-7 (15-19%). Only one third of the decrease in prescriptions purchased with insurance coverage is offset by out-of-pocket purchases, and there is no immediate change Medicaid purchases.

Relative to the probability of purchasing a prescription, spending falls by a larger amount because people switch from branded medications to similar generics. At age 26, the probably of switching from branded to generic extended-release (XR) medications increases by 1-2 percentage points. Though the fraction of the population that switches is relatively small, it substantially affects spending because the average generic XR prescription costs $104 (43%) less than the average branded prescription.

Purchases with private insurance and Medicaid quickly increase in the months following the eligibility threshold and by age 27 access to prescriptions recovers. While spending also rebounds, it does not return to the level observed at age 25. The persistent decrease in spending appears to be driven by switches to generics. Among consumers who primarily purchased branded medications prior to age 26, use of generics increases discontinuously at 26 and continues to grow thereafter. Over the longer-term, switches to generics restore access to prescriptions, while containing spending.

My study is closely related to the literature that examines the effect of the Affordable Care Act's dependent coverage provision on young adults. There is broad agreement that the dependent coverage provision increased insurance coverage (*Sommers et al.*, 2013; *Akosa Antwi et al.*, 2013; *Cantor et al.*, 2012), and improved measures of self-reported mental and physical health among young adults (*Barbaresco et al.*, 2015; *Chua and Sommers*, 2014; *Wallace and Sommers*, 2015). However, it is unclear why self-reported health increased, as there is disagreement regarding the effects of the dependent coverage provision on mediating factors, such as healthcare utilization. With respect to prescriptions, *Shane et al.* (2016) and *Kotagal et al.* (2014) find no effect on access and utilization, while *Breslau et al.* (2019) finds an increase in utilization concentrated among young adults with mental health conditions.

I provide conclusive evidence that dependent insurance coverage increases use of prescriptions for young adults with ADHD. Previous studies rely almost exclusively on surveys with small sample sizes. Such studies are subject to reporting error and small sample sizes, which limit the ability to assess parallel trends and measure potentially large effects for subsets of the population. I use administrative data, with many more observations for the set of young adults that have chronic conditions, which allows me to measure the effects of insurance coverage with more power. Furthermore, I rely on a regression discontinuity approach, with simple identification assumptions that may be more reasonable than the parallel trends assumptions. The large amount of data even allows me to drop the months right before and after the threshold for dependent coverage, showing that the results are robust to corrections for anticipatory behavior.

The study is also one of the first to examine the effect of insurance coverage on generic drugs. For many years policy efforts to contain prescription spending have promoted generics because they are much cheaper than branded medications (*Morton and Kyle*, 2011). In 2016 branded prescriptions accounted for 11% of prescription drugs fills, but 77% of spending (*Generic Pharmaceutical Association*, 2016). Previous literature shows that generic utilization responds to cost-sharing and formularies (see *Howard et al.* [2018] for a review). Yet my literature review only identified one study that measures the effect of insurance coverage on generic utilization. *Ghosh et al.* (2019) find that the Medicaid expansion increased generic utilization more than branded utilization (24% versus 17%). In contrast, I find that dependent coverage disproportionately affect branded prescriptions, presumably because dependent insurance is private.

2.2 Background

Among all age groups in the United States, young adults have the lowest rate of insurance coverage. Figure B.1 shows the evolution of the uninsured population by age group from 2007-2017. Before 2010, the rate of insurance coverage for people 18-25 and 26-29 was nearly identical. In each group 29% of the population was uninsured. In contrast, in every other age group less than 20% of the population was uninsured. Overall, 12% of rest of the population was uninsured, less than half the rate of 18-29 year-olds.

The Afforcable Care Act (ACA) directly addressed the high rate of unisurance among young adults. Its dependent coverage provision was one of the first policies to take effect; it was implemented on September 23, 2010. The provision extended the age that children of policy-holders were eligible for dependent insurance coverage from 18 to 26. In practice, from this point forward, private insurers allowed children to stay on their parents' insurance plans until the month after their 26^{th} birthday. In conjunction, a special enrollment period was introduced, which allowed people to purchase other forms of insurance within 60 days of their 26^{th} birthday.

The dependent coverage provision substantially decreased the proportion of young adults without insurance coverage. Figure B.1 shows that from 2011-2013 the rate of uninsurance decreased to 25% among people 18-25, even as it slightly increased and reached 30% among people 26-29. As discussed in the introduction, numerous studies evaluated the effects of the provision. The consensus is that insurance increased by 5-7 percentage points among the affected population (*Sommers et al.*, 2013; *Akosa Antwi et al.*, 2013; *Cantor et al.*, 2012). But studies disagree regarding the effects on utilization, including utilization of prescription drugs (*Breslau et al.*, 2019; *Shane et al.*, 2016; *Kotagal et al.*, 2014).

In this study, I compare people immediately before and after their 26th birthday using a regression discontinuity design. The transactions studied cover all purchases of CNS stimulants in Kentucky from 2015-2017. During these years all other coverage provisions of the ACA were fully in effect. Major ACA policies included the Medicaid expansion, (which extended coverage to people under 138% of the poverty line in expansion states), income-based subsidies for purchasing individual insurance on the exchanges, and a tax penalty for people without insurance coverage.[4] On net, the proportion of young adults without insurance substantially decreased after the additional ACA policies were implemented. Figure B.1 shows that in 2014-2017, the proportion of young adults without insurance coverage dropped by 10 percentage points relative to 2011-2013. The rate of uninsurance for young adults eligible for dependent coverage (age 18-25) decreased to 15%, close to the rate for 30-54 year-olds. The rate of uninsurance remained 4.5 percentage points higher for young adults who were not eligible for dependent coverage (age 26-29).

The ACA also included several provisions relevant to people with chronic health conditions, such as ADHD. Foremost among these, it prohibited price discrimination

[4] The 2014 ACA policies also made small changes to the dependent coverage provision. Originally, insurers were not required to cover children who were offered insurance coverage by their employer. Beginning in 2014, this exemption was removed; insurers were required to cover all children regardless of access to employer-sponsored coverage. Children were also eligible for dependent coverage on policies purchased on the exchanges, but these policies do not contribute the discontinuity that I study. Children may stay on a parent's exchange policy until the end of the calendar year of their 26^{th} birthday.

based on pre-existing conditions. The prohibition eliminated the price differential in the individual market between young adults with mental health conditions, like ADHD, and healthy young adults.[5] Other regulations increased the generosity of insurance plans. Under the ACA, insurance plans must cover "essential benefits", including prescription drug coverage and mental health and substance abuse services. Individual and small-group insurance must cover essential benefits and, beginning in 2015, large employers that did not cover essential benefits faced tax penalties.

Kentucky, which provided the data on prescription purchases was affected by many aspects of the ACA. Like the majority of US states, it expanded Medicaid in 2014. The Medicaid expansion and accompanying ACA policies substantially reduced the proportion of uninsured Kentuckians. According to the American Community Survey, between 2013 and 2016 the proportion of people without insurance in Kentucky decreased more than any other state, falling by 11.3 percentage points to 5.4%. In the third quarter of 2016, there were 650,867 Medicaid enrollees in Kentucky. Medicaid covered 14.5% of the Kentucky population and three-quarters of the enrollees were eligible because of the increase in the qualifying income threshold (*Foundation for a Healthy Kentucky*, 2016). From 2014-2017, 11.6% of Kentuckians aged 18-25 were uninsured and 14.6% of Kentuckians aged 25-29.

2.3 Econometric Analysis

I compare outcomes for people slightly above and slightly below the age cutoff for dependent insurance eligibility. I use a regression discontinuity design with a cutoff the month after a person turns 26. Y_{ia}, the outcome of interest for person i at age a (in months), is a function of age, which may differ above and below the threshold:

$$Y_{ia} = \alpha + f(Age_{ia}) + \mathbb{1}(Age_{ia} > 26) * (\beta + f(Age_{ia}) + \epsilon_{ia} \tag{2.1}$$

The coefficient of interest, β, measures the difference in the potential outcomes at age 26 and 1 month. In the baseline specification, $f(Age_{ia})$ is a quadratic function of age. The outcomes are modeled with a quadratic function to account for nonlinearities generated by the special enrollment period at age 26. During this period, which begins 60 days before and ends 60 days after an persons' 26^{th} birthday, it is possible

[5] Prior to the ACA applicants for individual insurance were required to complete a survey that asked for information on all doctor visits and prescription medication taken. Applicants also authorized insurers to review all medical records and pharmacy database information.

to change insurance policy. Gradual changes in insurance coverage during the special enrollment period may diverge from the trend before and after the special enrollment period. The quadratic function captures the resulting nonlinearities in outcomes, such as the probability of insurance coverage and utilization of prescriptions.

For robustness, I estimate several specifications. For each set of outcomes I calculate the optimal bandwidths using the *rdrobust* Stata package (*Calonico et al.*, 2017). Based on these estimates, I show 6 and 12 month bandwidths for the insurance outcomes, and 9 to 12 months for the utilization outcomes. For the insurance outcomes, I also estimate a specification that includes a person fixed effect, α_i. The person fixed effects may affect the estimate of β because the set of people observed at each age is not constant.[6] In some graphs of the utilization outcomes it appears that a quadratic polynomial may overfit the data, so I also report results where $f(Age_{ia})$ is linear.

All regressions weight the data with a triangular kernel to increase the efficiency of the local RD estimate (*Cheng et al.*, 1997), and cluster standard errors at the person level to account for serial correlation over time. I estimate the regressions using the *reghdfe* command in Stata, which absorbs the person fixed effects in relevant specifications.

The measured effect β is unbiased if the potential outcomes, (utilization with insurance and utilization without insurance), are continuous at age 26 and 1 month. Manipulation of the running variable, age, is not possible. The sample is identical on each side of the discontinuity because it is composed of a monthly panel of people who are observed before and after their 26^{th} birthdays. Appendix B.0.4 shows that labor outcomes, such as the probability of employment, do not discontinuously change at the threshold. Appendix B.0.2 conducts a placebo test. It shows that discontinuities in insurance coverage and utilization did not exist from 2007-2009, before the dependent coverage provision was enacted.

As discussed earlier, DEA regulations should prevent patients from stockpiling CNS stimulants. I empirically assess whether anticipatory stockpiling drives the prescription utilization results by using a "donut RD" approach, which drops the months immediately before and after the discontinuity (*Barreca et al.*, 2011). I report the results from the donut RD in Appendix B.0.3. For nearly all outcomes they they do not differ in direction or statistical significance.

[6]MEPS respondents are followed for 2 calendar years. Therefore, in both cases, the set of ages observed differs by respondent. Set of people observed at each age also changes in the KASPER data, but I do not show estimates with fixed effects because they are nearly identical for those outcomes.

2.4 Medical Expenditure Panel Survey (MEPS) Data and Analysis

The MEPS follows respondents for two calendar years. I primarily use it to examine the "first stage" of the dependent coverage eligibility threshold, i.e. the effect on insurance coverage. In contrast to other outcomes, the MEPS reports the insurance coverage status of respondents in every month. I use the monthly data on insurance coverage to conduct a regression discontinuity analysis at age 26. The MEPS also reports prescription drug utilization for intervals that span multiple months (4.8 on average). Because the data on prescription drug utilization is not reported on a monthly basis, it is too coarse for a regression discontinuity analysis. Instead, I compare the mean number of prescriptions for people just below and above the threshold. This comparison adds additional context and helps motivate the analysis of CNS stimulants in Kentucky.[7]

2.4.1 Insurance Coverage

Panel A of Table 2.1 shows the effect of turning 26 and 1 month on the probability of dependent insurance coverage and the probability of any insurance coverage. For robustness, I report 4 specifications. Graphs of the RD estimates are presented in Figure 2.1, Panels A and B. The graphs overlay the RD estimates from the third specification (no fixed effects and 12 month bandwidth) on the raw means of the outcome variables.

The results show a decline in the probability of dependent insurance coverage at the eligibility threshold. Over the year before MEPS respondents turn 26, the probability of dependent insurance coverage decreases gradually from 25% to 18%. This appears to accelerate in the months leading up to the 26^{th} birthday, which correspond to the special enrollment period for young adults. In the month after the 26^{th} birthday, the probability of dependent coverage decreases sharply by 5-6 percentage points. The rate of dependent coverage remains below 10% thereafter.

The probability of any insurance coverage also decreases at the threshold for dependent coverage eligibility. During the two years before MEPS respondents turn 26, the probability of any insurance coverage is flat at 72%. The month after a respondent's 26^{th} birthday, the probability of dependent insurance coverage drops by 4-5 percentage points. Respondents gradually regain insurance coverage over the next year.

[7]The labor market outcomes analyzed in Appendix B.0.4 also come from the MEPS.

The drop in dependent coverage is only one percentage point larger than the drop in insurance coverage. This suggests that the vast majority of people who lose dependent coverage at 26 do not immediately replace it with another form of insurance coverage. Presumably the price of alternative options, such as individual insurance on the exchanges, is higher than the price that most respondents are willing to pay.

2.4.2 Insurance Coverage for People with Chronic Conditions

Demand for insurance coverage and the resultant discontinuity at age 26 may vary across different segments of the population. In particular, demand theory suggests that people with chronic conditions such as ADHD have higher demand for insurance. I categorize MEPS respondents to test for heterogeneity among the subgroup with chronic health conditions. Respondents are categorized as Healthy or Chronic based on the medical conditions reported during their first interview. I only use conditions reported in the first interview to avoid a change in the probability of reporting a condition if a person gains or loses insurance coverage.[8] I categorize people as Chronic if they report at least one chronic condition on their first interview date.[9]

Table B.1 displays sample statistics for MEPS respondents aged 18-29. Between 2014 and 2017 there were 14,671 young adults in the MEPS and 19% of them qualified as Chronic. The percentage of young adults with chronic conditions may seem large, but the definition includes mental health conditions, which are pervasive among young adults. Approximately 44% of the Chronic subgroup (8% of all young adults) reported a mental health condition. Other sources such as the National Survey of Drug Use and Health (NSDUH), actually report higher rates of mental health conditions among young adults. From 2014-2017, among NSDUH respondents age 19-29, 22% reported a mental health condition and 6% reported a serious mental health condition. The comparison suggests that the MEPS may only capture serious mental health conditions.

[8] Respondents report their conditions at one of three stages of the interview process. In the condition enumeration section of MEPS, respondents are asked whether they have each of 15 common "priority" conditions. The survey also notes the condition associated with each medical event such as a hospital stay or prescribed medicine. Finally, respondents are asked to report any condition that bothers them during each reference period.

[9] To protect patient confidentiality, publicly available conditions are reported using 3 digit ICD-9 codes instead of 5 digit ICD-9 codes. I classify the 3 digit codes as "chronic" based on the percentage of underlying five digit codes considered chronic by the HCUP Chronic Condition Indicator Algorithm. The distribution of this percentage is bimodal; 86% of clinical classification codes are over 90% chronic or under 10% chronic. If the percentage is more than 25% chronic, then I consider the clinical classification code chronic. I use a low threshold to reduce the probability of contaminating the Healthy group with people who have chronic conditions.

There are substantial demographic differences between the Healthy and Chronic populations of young adults in the MEPS. People with chronic conditions are 11 percentage points more likely to be women and 6 percentage points more likely to be white. Labor outcomes appear to be broadly similar across the two groups, but average annual income is 9% lower for people with chronic conditions. People with chronic conditions are also 11 percentage points more likely to be covered by any type of insurance. Total health spending is more than twice as large ($3,118 higher) for the Chronic population and prescription spending accounts for a substantial portion ($954) of the difference.

Panel B of Table 2.1 presents the regressions results for the Chronic subgroup and Panels C and D of Figure 2.1 present the accompanying graphs. Results for the Healthy subgroup are shown in Appendix Table B.2 and Figure B.2. I focus my discussion on the Chronic subgroup because the results for the Healthy subgroup are very similar to the full sample of young adults.

Relative to the full sample, the Chronic subgroup has substantially fewer observations, which increases the standard errors in the regressions. As described above, only 20% of respondents are classified as Chronic and the analysis only includes those who are observed before their 26^{th} birthday. Because respondents are categorized based on the conditions reported in their first interview, these restrictions ensure that the probability of reporting a chronic condition does not change if a respondent loses insurance coverage. Few people who are observed at age 26 are also observed at 24 and 27. For this reason, I do not include the means at age 24 and 27 in the accompanying graphs.[10]

At age 26, insurance outcomes also decrease for the Chronic subgroup. In the 12 months before the eligibility threshold, the rate of dependent coverage for the Chronic subgroup declines gradually from 27% to 22%. The month after respondents turn 26, the probability of dependent coverage decreases sharply by 8 percentage points. It remains at approximately 10% thereafter. Similarly, in the 12 months before the eligibility threshold, the rate of any insurance coverage for the Chronic subgroup declines gradually. The month after respondents turn 26, the probability of dependent coverage decreases by 2-5 percentage points. Unlike previous results, which are all significant at the 1% level, the drop in the probability of any insurance coverage is only statistically significant at the 5% level in specifications with person fixed effects.

[10]MEPS respondents are only followed for 2 years so few respondents in this analysis are observed at ages 24 or 27.

The results for the Chronic subgroup conform to classical models of adverse selection. The Affordable Care Act outlawed price discrimination based on pre-existing conditions. Thus, it is reasonable to assume that people with chronic conditions face similar prices as healthy people, but have a higher willingness to pay for insurance coverage. If demand for insurance coverage is less elastic for people with chronic conditions, then they should be more likely to obtain dependent coverage and less likely to lose insurance coverage when the price rises at age 26. The data bears this out.

2.4.3 Prescription Drug Utilization

The MEPS does not collect monthly data on prescription drug utilization, but it does collect data in longer intervals that span 4 to 5 months. Using this data it is possible to compare utilization of prescriptions for people close to age 26. I categorize these people into three groups based on the start and end dates for the interview reference period. For the before 26 group, the interview reference period ends slightly before they turn 26 (between 25.5 and 26). For the including 26 group, the interview reference period includes the month they turned 26 (it begins before age 26 and ends after age 26). For the after 26 group, the interview reference period begins slightly after they turn 26, (after 26 and before 26.5). The before 26 group is eligible for dependent coverage, while the after 26 group is not. On average, the including 26 group is eligible for dependent coverage for half of the time that it is observed.

Table B.3 shows the mean number of monthly prescriptions for people who were interviewed slightly before age 26. For the including group and the after 26 group, it shows the difference in means relative to the group interviewed slightly before 26. The table includes the total number of prescriptions and the types of medications most commonly used by young adults.

Utilization of prescriptions appears to decline substantially at age 26. Relative to the before 26 group, the number of prescriptions declines by 14% for the including 26 group, and 10% for the after 26 group. With the exception of antidepressants, prescription utilization appears to fall for all categories of medications.

The comparisons suggest that a large fraction of people who lose insurance coverage may forgo prescriptions. The magnitude of the decrease is similar to the RD estimates for insurance coverage, which estimate that 8% of insured young adults lose dependent coverage at 26. However, because of the relatively small sample size in the MEPS, most of decreases in utilization are measured inaccurately and are not statistically significant. Furthermore, medications such as birth control are not regulated by the DEA and the apparent decrease may reflect stockpiling from by people before they turn 26. Insurance coverage appears to affect utilization for a wide range of prescriptions, but statistically robust inference requires larger sample sizes and monthly data.

2.5 Kentucky All Schedule Prescription Electronic Reporting (KASPER)

2.5.1 Data and Descriptive Analysis

The data limitations in the MEPS make it impossible to study all type of prescriptions using an regression design at 26. Thus I turn to KASPER, an administrative dataset that reports all sales of Schedule II-V prescriptions from pharmacies in Kentucky. I focus on central nervous system (CNS) stimulants. Among the medication classes reported in KASPER, they represent the category of prescriptions most commonly used by young adults. (Overall, among young adults, spending on CNS stimulants is second only to birth control, which is not reported in KASPER). As discussed earlier, CNS stimulants also have other advantages, such as being difficult to stockpile.

KASPER records the national drug code and quantity of medications for each prescription filled, the date it was filled, the date of birth (year and month) of the patient, and the patient's zip code. It also contains a unique id for each patient. Beginning in 2015, KASPER recorded the form of payment for nearly all transactions.[11] For this reason, I limit the RD analysis to records from 2015-2017. I use previous years to characterize the prescription history of each person.

KASPER does not collect information on prices. I generate an estimate of spending by merging price data from the CMS Retail Price Survey. Each month, the survey selects a national random sample of 2,500 retail community pharmacies, of which 450-600 voluntarily provide price data. The price data is referred to as the NADAC (national average drug acquisition cost). It represents the average manufacturer or wholesale price of each drug and is derived from pharmacy invoices. Medicaid agencies commonly use the NADAC as a metric for setting their reimbursements levels. For example, Kentucky Medicaid reimburses pharmacies the minimum of the NADAC and several other benchmarks plus a $10.64 dispensing fee. The spending

[11] The form of payment is available in earlier years, but is only reported for a very small fraction of transactions before 2014. In 2014, the field was missing for approximately 5% of transactions, and from 2015-2017 it was missing for less than 1% of transactions.

figures generated from NADAC prices represent the value of purchases based on the manufacturer price. They are an underestimate of the actual prices paid by consumers, but the two are closely tied.

The resulting dataset measures monthly purchases and spending on CNS stimulants for each person in Kentucky from 2010-2017. I limit the sample for the RD analysis to people who are Kentucky residents (based on zip code) and turn 26 in the years studied, 2015-2017. For these people, I observe all CNS stimulants purchased since age 21. I restrict the sample to people who purchased a CNS prescription between 21 and 28. Use of CNS stimulation by all other people does not change at 26, since it is zero both before and after. The criteria yield a panel of 11,800 people observed in each month.

The primary outcome of interest is monthly utilization of CNS stimulants. Purchases can be broken down by three broad categories of CNS stimulant medications. Generic immediate-release (generic IR) medications are the cheapest, ($39 per prescription[12]), and are immediately metabolized by the body. They are effective for 3-5 hours (*Stevens et al.*, 2013). Generic extended-release medications are substantially more expensive, ($137 per prescription). They are metabolized slower and are effective for 10-12 hours. Branded medications are the most expensive category of medication, ($241 per prescription). Over 99% of branded medications sold were extended release and 80% were Vyvanse. A generic version of the active ingredient in Vyvanse, lisdexamfetamine, was not available in during the years studied. Despite this limitation, the closest substitute for branded drugs are generic XR drugs.

Table 2.2 compares monthly CNS stimulant purchases at age 25 and 26. As discussed earlier, the sample is composed of Kentucky residents who turned 26 between 2015-2017 and purchased a CNS stimulant between ages 21-28. The top portion of the table shows purchases by three types of mutually exclusive payment methods: private insurance, public insurance, and out-of-pocket. Though it isn't possible to directly observe purchases with dependent coverage, they are a subset of purchases with private insurance. In my sample of young adults, Medicaid accounts for 80% of purchases made with public insurance.

At age 25, the vast majority of transactions were paid with private insurance. In each month 22% of the sample purchased a prescription with private insurance, 1.5% purchased a prescription with public insurance, and less than 1% purchased a prescription out-of-pocket. Similarly, in each month spending with private insurance averaged $32, spending with public insurance averaged $2, and out-of-pocket spending

[12]The statistics in this paragraph are calculated based on the sample of interest.

averaged \$1.

At age 26, when young adults were no longer eligible for dependent coverage, transactions paid with private insurance decreased and there was a small increase in other transactions. The probability of making a purchase with private insurance decreased by 2.1 percentage points. About half of the decrease in private insurance purchases was offset by increases in the probability of making a purchase with Medicaid and out-of-pocket. Average spending followed a similar pattern, falling by \$3.65. Only one quarter of the decrease in spending paid with private insurance coverage was offset by increases in Medicaid and out-of-pocket spending.

The second portion of Table 2.2 shows the types of drugs purchased. At age 25, in each month 8.6% of the sample purchased a branded drug, 6.3% purchased a generic XR drug, and 10.5% purchased a generic IR drug. Monthly purchases averaged \$22 for branded drugs, \$9 for generic XR drugs, and \$4.20 for generic IR drugs. At age 26, purchases of branded drugs decreased substantially, while purchases of other drugs were less affected. Branded medications accounted for nearly all of the decrease in the probability of purchasing a prescription and two thirds of the decrease in spending.

2.5.2 Overall Utilization of CNS Stimulants

The first set of KASPER results establish that monthly utilization of CNS stimulants decreases at the threshold for dependent coverage eligibility. As discussed in the methods section, in the main text I show four specifications, varying the bandwidth and polynomial order. The graphs overlay the RD estimates from Specification 3, (which uses a 12 month bandwidth and second order polynomial), on the raw means of the outcome variables. Appendix Section B.0.3 shows variety of "donut RD" specifications that drop the months immediately before and after people turn 26. The donut RD specifications demonstrate that the results are robust to anticipatory effects. They are discussed in the appendix.

Table 2.3 displays the results from the RD regressions and Figure 2.2 displays the accompanying graphs. The pattern of prescription utilization broadly mirrors the pattern of insurance coverage observed in the MEPS. The probability of filling a prescription is approximately flat before age 26, decreases substantially at the threshold for insurance coverage, and rebounds over the next year as the rate of insurance coverage rebounds. Spending follows a similar pattern, but does not fully recover to the level observed before age 26.

Ageing out of dependent insurance coverage reduces probability of a monthly CNS stimulant purchase by 2.3-2.9 percentage points (10-13%). This is a large decrease.

In comparison, among MEPS respondents with insurance coverage, the probability of dependent coverage decreased by 8%. The larger decrease in purchases of CNS stimulants suggests that the population studied in the KASPER data is more likely to lose dependent coverage at age 26. Additionally, purchases of CNS stimulants by the KASPER population appear to be very sensitive to insurance coverage.

Spending on CNS stimulants also decreases at age 26. It falls by $5.35-6.63 (15-19%). On a percent basis the decrease in spending is larger than the decrease in the probability of purchasing a prescription. This suggests that people may decrease both the probability of purchasing a prescription and their spending conditional on purchasing a prescription. I explore this possibility in the analyses that follow.

Both the probability of purchasing a prescription and spending rebound in the 9 months after people turn 26. While the probability of purchasing a prescription returns to the levels observed at age 25, spending remains approximately $2 (6%) lower. The change in insurance status is associated with long-lasting changes in spending.

2.5.3 Purchases by Insurance Status

Table 2.4 and Figure 2.3 show the RD estimates for CNS stimulant use by method of payment. Turning 26 decreases purchases made with insurance coverage substantially more than it increases out-of-pocket purchases. It also changes the trajectory of purchases made with public insurance, (mostly Medicaid), which quickly increase the year after people turn 26.

At age 25, in each month 21% of the sample purchases a prescription with private insurance, 1.5% purchases a prescription with public insurance, and 0.8% purchases a prescription out-of-pocket. The proportions are relatively flat in the year before people turn 26. The month after, the probability of purchasing a prescription with private insurance decreases, while the probability of purchasing a prescription out-of-pocket increases by a substantially smaller amount. The probability of purchasing a prescription with private insurance decreases by 3.3-4.3 percentage points (16-21%). Out-of-pocket purchases increase by 0.9-1.3 percentage points, roughly doubling off of a small base. The probability of purchasing a prescription with public insurance does not immediately change.

Spending follows a similar pattern. At age 25, in each month private insurance spending averages $32.23, public insurance spending averages $1.94, and out-of-pocket spending averages $0.85. The figures are relatively flat in the year before people turn 26. The month after, spending with private insurance decreases, while out-of-pocket spending increases by a substantially smaller amount. Private insurance spending decreases by \$6.55-8.22 (20-26%). Out-of-pocket spending more than doubles, increasing by \$1.07-1.62. Increases in out-of-pocket purchases offset up to one third of the decrease in purchases with private insurance.

The decrease in access to prescriptions is short-lived. By age 27, the probability of purchasing a prescription with private insurance is only 1.5 percentage points lower than it was at age 25, and the probability of purchasing a prescription with public insurance is 1 percentage point higher. Though purchases with public insurance do not immediately shift to a higher level, they do immediately shift to a steady upward trajectory. Medicaid appears to play an important role for maintaining access to CNS stimulants.

The decrease in spending diminishes over time, but is persistent. By age 27, private insurance spending is \$4 lower than at age 25, public insurance spending is \$1 higher, and out-of-pocket spending returns to the same level. Despite the rebound in access to prescriptions, spending remains lower.

2.5.4 Heterogeneity by Type of Prescription

Patients may reduce spending by reducing the probability of purchasing a prescription or switching to less expensive medications. Changes to a patient's insurance status do not immediately affect their underlying conditions, thus doctors are unlikely to change prescription dosages. Instead, it is more sensible that patients may decrease spending by switching from branded to generic medications.

Prices vary substantially among the three types of CNS stimulants. Over the years studied, the average price was \$39 for a generic immediate release (IR) prescription, \$137 for a generic extended release (XR) prescription, and \$241 for a branded prescription. Given the large price differences, the effects of insurance coverage on utilization may vary based on the type of prescription. Heterogeneity may be driven by people who lose insurance coverage or by people who switch insurance coverage. For example, increased cost sharing or formularies of less generous plans may incentivize people to switch from branded to generic medications.

Table 2.5 and Figure 2.4 show the RD estimates by type of CNS stimulant. The graphs and regression estimates clearly show that people reduce utilization of expensive branded drugs. In the year before people turn 26, the probability of purchasing a branded drug and spending on branded drugs are approximately flat. Immediately after the cutoff for dependent coverage, utilization of branded drugs shifts downward and only slowly recovers. The results suggest that the probability of purchasing a branded drug declines by 1.3-1.6 percentage points (15-19%) and that spending on branded drugs declines by 18-22%.

Reductions to generic utilization and spending are smaller in magnitude. The probability of purchasing either type of generic decreases by 8-12%, approximately half of the decrease in branded purchases. Because generics are cheaper, reductions in generic purchases account for only a small portion (25%) of the overall change in spending.

The RD graphs shows that in addition to being small in magnitude, the decline in generic utilization is short-lived. Withing a few months of the dependent coverage threshold, generic utilization and spending return to the original trend. In contrast, the reduction in branded purchases is longer lasting. Branded purchases and spending take more than one year to return to the original trend.

2.5.5 Switching Across Drug Types

I also examine whether there is direct evidence of switching from branded to generic medications. To assess the evidence, I categorize people based on the type of prescription filled before age 26. I present results for people who primarily purchased branded prescriptions before age 26. Approximately one third of the sample (3,663 people) fulfill this criteria. The results for people who primarily purchased generics before age 26, (not shown for parsimony), do not show discontinuities.

Table 2.6 and Figure 2.6 display the probability of purchasing a given type of prescription conditional making a purchase. The RD estimates indicate that, among people who primarily purchased branded medications, the probability of purchasing a branded medication decreased by 4.5-5.8 percentage points (4-5%) at the dependent eligibility threshold. The decrease is mostly offset by an increase in the probability of purchasing a generic XR medication by 3-3.5 percentage points (40-50%). Point estimates for the probability of purchasing a generic IR prescription are also positive, but they are smaller in magnitude and only statistically significant in some specifications. The graphs show that the switch to generic XR prescriptions appears to be permanent.

Substitution between branded and generic XR drugs is sensible because they are the most similar from a pharmacological point of view. Overall, one third of people primarily purchase branded prescriptions before age 26, and conditional on making a purchase the probability of the drug being branded prescription declines by 4-5 percentage points. Thus, about 1-2 percent of prescriptions are affected by switching. Switches from branded to generic drugs helps explain why the reduction in spending is larger than the reduction probability of purchasing a prescription. However, the magnitude of the switching effect is substantially smaller than the reduction in the probability of purchasing a prescription.

2.6 Discussion

The evidence from CNS stimulants demonstrates that young adults with ADHD respond to changes in insurance coverage by adjusting prescription drug purchases along multiple margins. When they age out of dependent insurance coverage, the vast majority of young adults respond by reducing prescriptions. The probability of filling a prescription falls by 10-13%. A small fraction the population (1-2%) also adjusts the type of prescriptions they purchase. Spending decreases more than the probability of filling a prescription because people switch from branded to cheaper generic medications.

While access to prescriptions rebounds as young adults regain insurance, spending remains below the levels observed just before age 26. The longer-term decrease in spending is driven by the switch to generic medications. The switch is immediate, which rules out that it is due to the gradual shift to Medicaid, and it appears to be permanent. Among people who primarily purchased branded prescriptions before they turned 26, the percentage of prescriptions that are branded decreases by 20 percentage points from age 26 to age 27. The shift from branded to generic prescriptions substantially affects spending, because on average a generic prescription costs $104 (43%) less.

The results also demonstrate the importance of Medicaid for access to prescription medications. In a related paper, *Ghosh et al.* (2019) find that in states that expand Medicaid there is no offsetting decline in prescriptions purchased out-of-pocket or with other forms of insurance. I find that up to 30% of the decline in purchases with insurance coverage is offset by an increase in out-pocket purchases; but the out-pocket purchases quickly decline. Moreover, the probability of purchasing a prescription with private insurance does not fully return to the levels observed at age 25. Rather, in the year after people lose access to dependent coverage there is a steep increase in Medicaid purchases, which offset the decrease in purchases with private insurance.

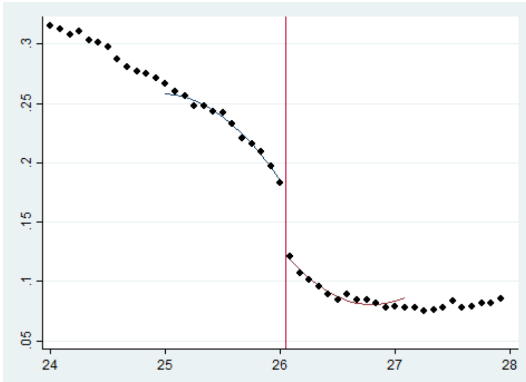
I estimate that at least two-thirds of people who would have purchased a prescription with dependent coverage do not purchase a prescription when they lose their original insurance. Relative to the literature, this estimate of the treatment on the treated is large. By comparison *Finkelstein et al.* (2012) estimate that people who received Medicaid in the Oregon Health Experiment increased prescription drug utilization by 35%. The larger effect documented in my study emphasizes the importance of studying prescription utilization for young adults. This subgroup of the population has low levels of income and wealth and appears to be particularly sensitive to insurance coverage.

Though the main study results are specific to people with ADHD, statistics from the MEPS suggest use of other prescriptions may also decline after people lose access to dependent coverage. Approximately 20% percent of young adults have a chronic condition such as ADHD, and they may substantially change health care utilization based on insurance coverage. Future work on young adults should examine the effects of insurance on other classes of drugs, such as birth control, (which is substantially cheaper), and medications for depression, (which affects a large segment of the population). Given the difficulty of estimating the effects using national surveys, future studies on may be most successful if they also use administrative data and focus on the segments of the population that are most likely to be affected.

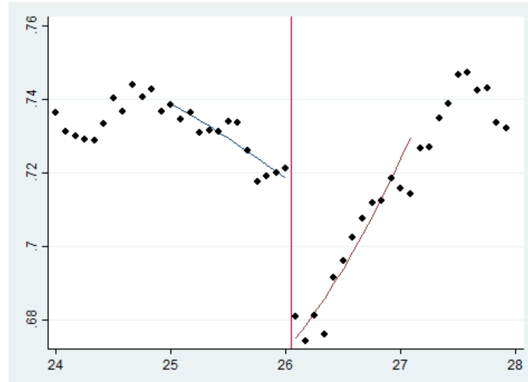
Use of prescriptions is an important behavior, that may help to explain why several papers find that the dependent coverage provision increased self-assessed health (*Barbaresco et al.*, 2015; *Chua and Sommers*, 2014; *Wallace and Sommers*, 2015). Many papers in the prior literature found no effect on healthcare utilization, thus it was unclear why self-reported health increased. My study suggests that increases in prescription utilization may be concentrated among young adults with chronic conditions. Similarly, three studies found increases in healthcare utilization for young adults with mental health conditions (*Saloner and Lê Cook*, 2014; *Antwi et al.*, 2015; *Breslau et al.*, 2019). This group may have benefited the most from the dependent coverage provision.

One important limitation is that I am not able to distinguish between the effects of losing insurance coverage and the effects of transitioning to a less generous insurance plan. The MEPS results suggest that, among people with chronic conditions, about half lose insurance coverage at 26 while the other half transition to another form of insurance coverage. Many young adults may transition to high deductible plans that are less generous than dependent insurance from a parent's plan. Given the large change in use of CNS stimulants, both types of changes in insurance coverage may have significant effects. Future work should attempt to disentagle the effects and understand the consequences of transitioning to less generous plans.

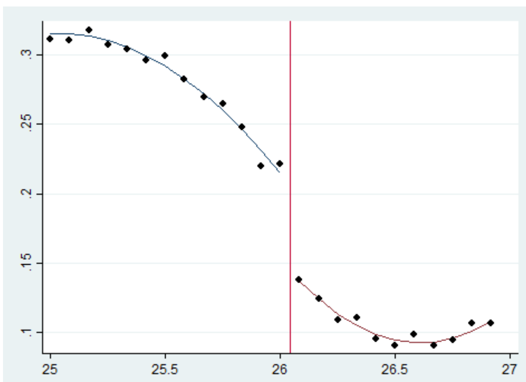
Figure 2.1: RD for Insurance Coverage



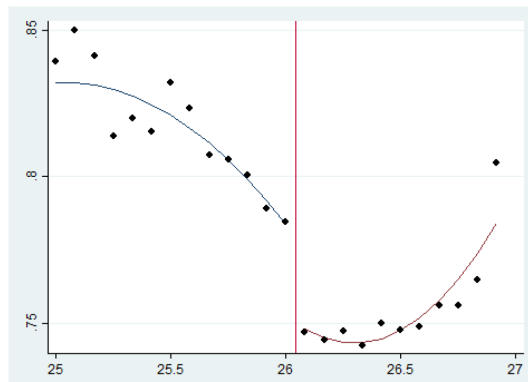
(a) Dependent Insurance, All Respondents



(b) Any Insurance, All Respondents



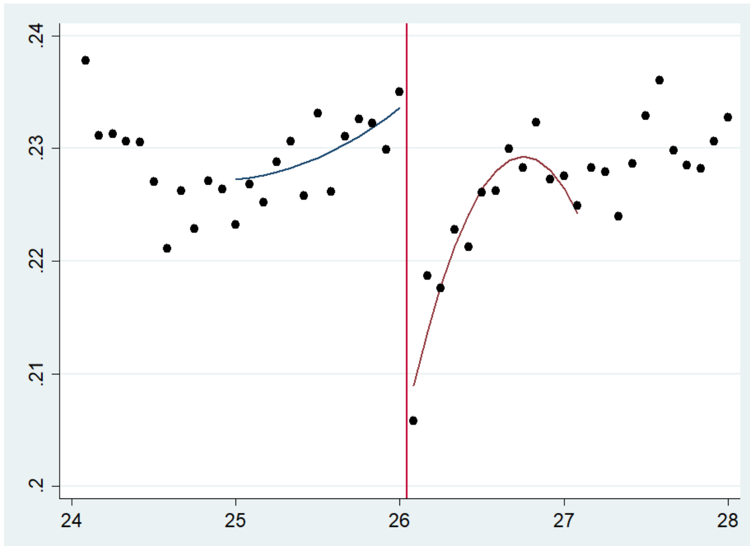
(c) Dependent Insurance, Chronic Group



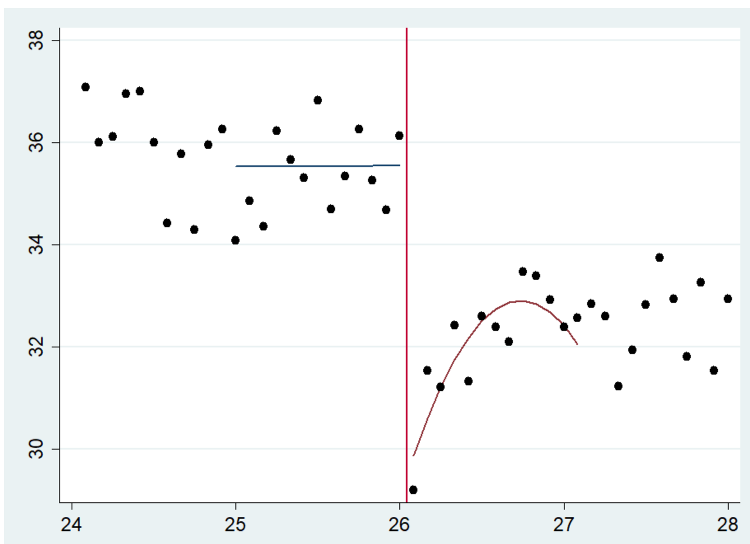
(d) Any Insurance, Chronic Group

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

Figure 2.2: RD for CNS Stimulant Prescriptions



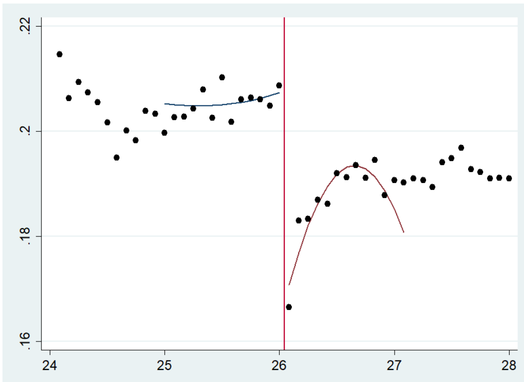
(a) Pr Buy



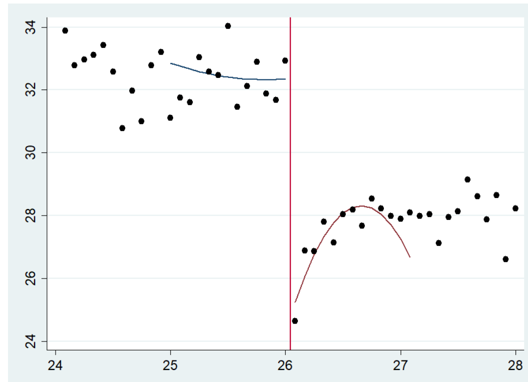
(b) Spending

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

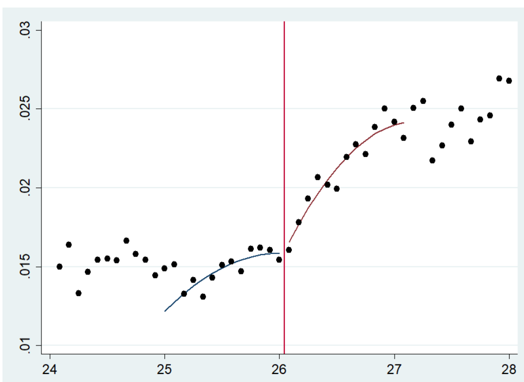
Figure 2.3: RD for Prescriptions by Insurance Status



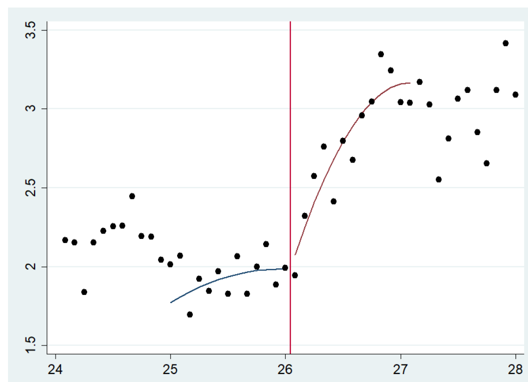
(a) Pr Buy with Private Insurance



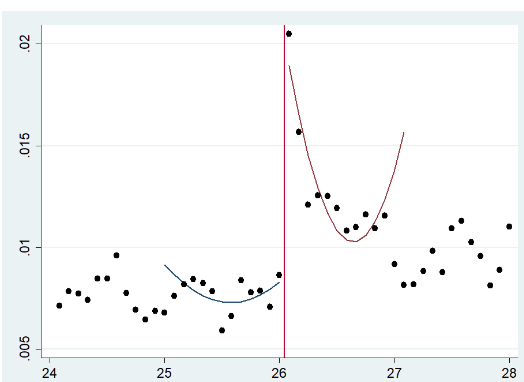
(b) Spend with Private Insurance



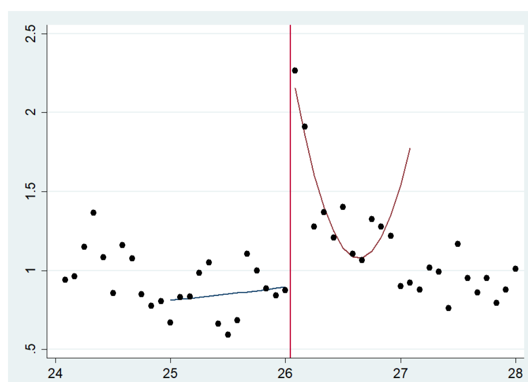
(c) Pr Buy with Public Insurance



(d) Spend with Public Insurance



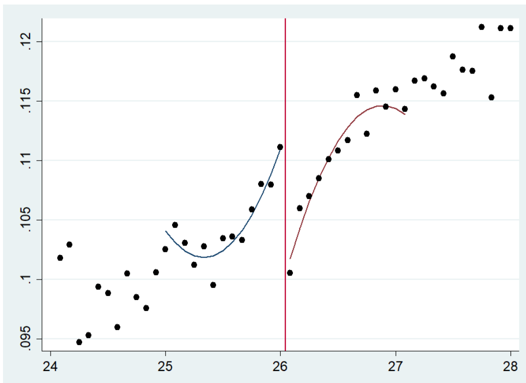
(e) Pr Buy Out-of-pocket



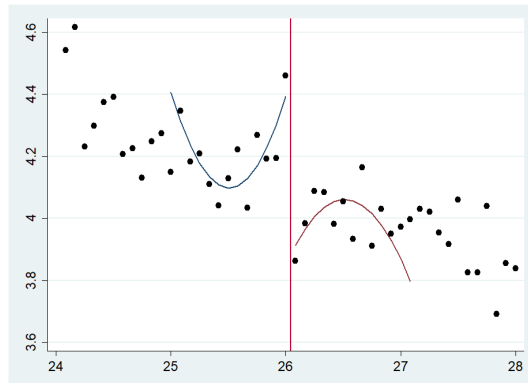
(f) Spend Out-of-pocket

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel. Medicaid accounts for approximately 80% of public insurance in the sample.

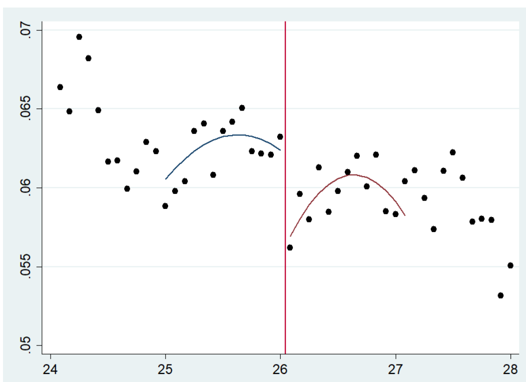
Figure 2.4: RD for Prescriptions by Type



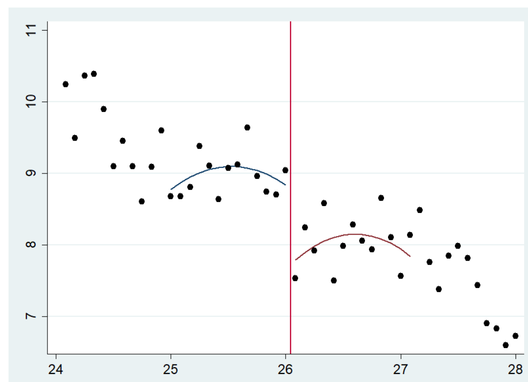
(a) Generic IR, Pr Buy



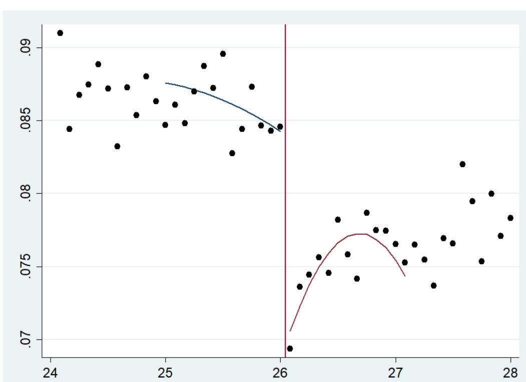
(b) Generic IR, Spending



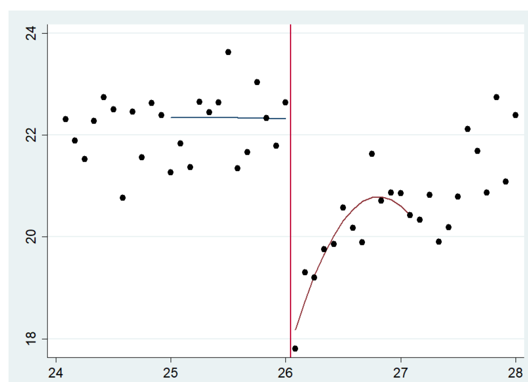
(c) Generic XR, Pr Buy



(d) Generic XR, Spending



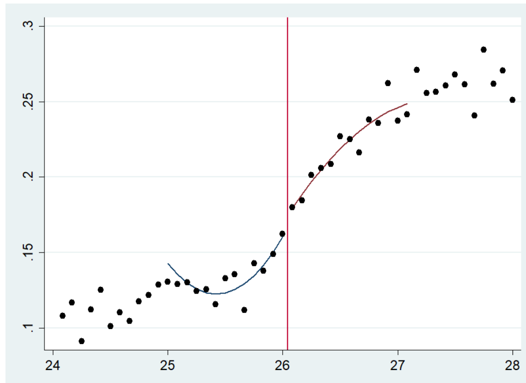
(e) Branded, Pr Buy



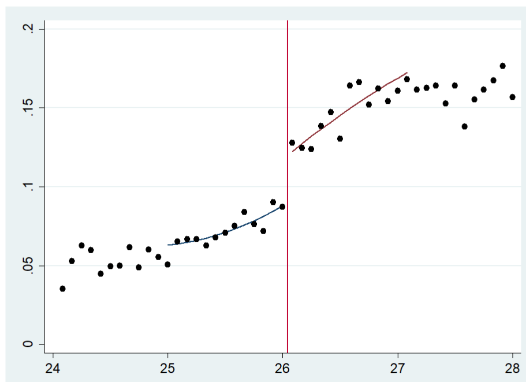
(f) Branded, Spending

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

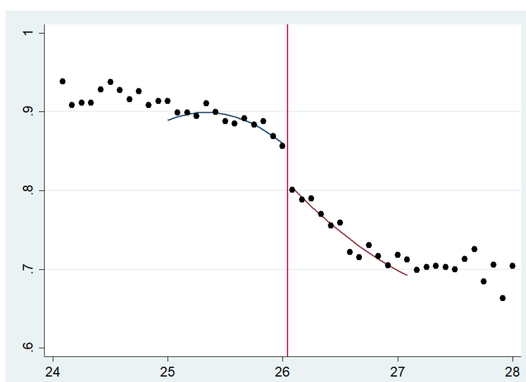
Figure 2.5: RD for Type of Prescription Purchased by Branded-Type Consumers



(a) Generic IR



(b) Generic XR



(c) Branded

Note: Consumers are considered "Branded-Type" if the plurality of their purchases between ages 21 and 26 were branded prescriptions. Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

Table 2.1: Change in Probability of Insurance Coverage at 26 and 1 Month

	(1)	(2)	(3)	(4)
Panel A: All Respondents				
Dependent Insurance	-0.0501***	-0.0525***	-0.0558***	-0.0590***
	(0.00716)	(0.00652)	(0.00761)	(0.00694)
Observations	23,758	23,571	44,310	44,120
Mean: Age 25	0.236	0.236	0.236	0.236
Any Insurance	-0.0413***	-0.0454***	-0.0437***	-0.0477***
	(0.00834)	(0.00720)	(0.00885)	(0.00767)
Observations	23,758	23,571	44,310	44,120
Mean: Age 25	0.729	0.729	0.729	0.729
Panel B: Respondents with Chronic Conditions				
Dependent Insurance	-0.0657***	-0.0697***	-0.0615***	-0.0678***
	(0.0178)	(0.0169)	(0.0177)	(0.0171)
Observations	4,098	4,092	6,508	6,504
Mean: Age 25	0.287	0.287	0.287	0.287
Any Insurance	-0.0338*	-0.0456***	-0.0290	-0.0377**
	(0.0176)	(0.0162)	(0.0180)	(0.0163)
Observations	4,098	4,092	6,508	6,504
Mean: Age 25	0.818	0.818	0.818	0.818
Respondent FE	No	Yes	No	Yes
Window	6 mo	6 mo	12 mo	12 mo

Note: Regression specification includes 2^{nd} degree polynomial and is estimated with triangular kernel. Standard errors clustered at respondent level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.2: CNS Stimulant Utilization by People Close to their 26th Birthday

	25	26	Difference
Pr Buy with Private Insurance	0.205	0.187	-0.019***
	(0.404)	(0.390)	(0.003)
Pr Buy with Public Insurance	0.015	0.021	0.006***
	(0.121)	(0.143)	(0.001)
Pr Buy Out-of-pocket	0.008	0.013	0.005***
	(0.087)	(0.112)	(0.001)
Private Insurance Spending	32.37	27.41	-4.96***
	(85.53)	(78.39)	(0.57)
Public Insurance Spending	1.94	2.72	0.78***
	(21.77)	(24.95)	(0.18)
Out-of-Pocket Spending	0.86	1.39	0.53***
	(13.61)	(17.53)	(0.10)
Pr Buy Generic IR	0.105	0.110	0.005**
	(0.306)	(0.313)	(0.002)
Pr Buy Generic XR	0.063	0.060	-0.003*
	(0.242)	(0.237)	(0.002)
Pr Buy Branded	0.086	0.075	-0.011***
	(0.280)	(0.264)	(0.002)
Generic IR Spending	4.20	4.00	-0.20**
	(15.36)	(13.57)	(0.10)
Generic XR Spending	8.99	8.03	-0.97***
	(42.02)	(37.88)	(0.26)
Branded Spending	22.29	19.96	-2.33***
	(78.82)	(75.00)	(0.55)

Note: 25 group includes people 25 and 1 month to 26 and 0 months old. 26 group includes people 26 and 1 month to 27 and 0 months old. MEPS data for 2014-2016. KASPER outcomes for 2015-2017. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Overall Changes in Utilization of CNS Stimulants at 26 and 1 month

	(1)	(2)	(3)	(4)
Pr Buy	-0.028***	-0.023***	-0.029***	-0.024***
	(0.0032)	(0.0026)	(0.0035)	(0.0026)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.229	0.229	0.229	0.229
Spending	-6.07***	-5.35***	-6.63***	-5.55***
	(0.707)	(0.557)	(0.801)	(0.587)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	35.32	35.32	35.32	35.32
Window	12 mo	12 mo	9 mo	9 mo
Polynomial order	2	1	2	1

Note: Regression specifications include 2^{nd} degree polynomial and are estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Changes in Utilization by Insurance Status at 26 and 1 month

	(1)	(2)	(3)	(4)
Pr Buy with Private Insurance	-0.040***	-0.033***	-0.043***	-0.035***
	(0.0032)	(0.0026)	(0.0036)	(0.0027)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.205	0.205	0.205	0.205
Pr Buy with Public Insurance	0.000	0.000	0.000	0.000
	(0.0010)	(0.0009)	(0.0011)	(0.0009)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.015	0.015	0.015	0.015
Pr Buy Out-of-Pocket	0.012***	0.009***	0.013***	0.010***
	(0.0013)	(0.0010)	(0.0014)	(0.0011)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.008	0.008	0.008	0.008
Private Insurance Spending	-7.57***	-6.55***	-8.22***	-6.87***
	(0.712)	(0.565)	(0.801)	(0.595)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	32.23	32.23	32.23	32.23
Public Insurance Spending	-0.00	0.09	-0.07	0.07
	(0.172)	(0.149)	(0.191)	(0.149)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	1.94	1.94	1.94	1.94
Out-of-Pocket Spending	1.42***	1.07***	1.62***	1.18***
	(0.210)	(0.155)	(0.238)	(0.170)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.85	0.85	0.85	0.85
Window	12 mo	12 mo	9 mo	9 mo
Polynomial order	2	1	2	1

Note: Regression specification includes 2^{nd} degree polynomial and is estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Changes in Utilization by Type of Medication at 26 and 1 month

	(1)	(2)	(3)	(4)
Pr Buy Generic IR	-0.012***	-0.008***	-0.012***	-0.009***
	(0.0022)	(0.0018)	(0.0025)	(0.0019)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.104	0.104	0.104	0.104
Pr Buy Generic XR	-0.006***	-0.005***	-0.006***	-0.005***
	(0.0020)	(0.0016)	(0.0022)	(0.0017)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.062	0.062	0.062	0.062
Pr Buy Branded	-0.014***	-0.013***	-0.016***	-0.013***
	(0.0021)	(0.0018)	(0.0023)	(0.0019)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	0.086	0.086	0.086	0.086
Generic IR Spending	-0.56***	-0.36***	-0.61***	-0.43***
	(0.111)	(0.088)	(0.130)	(0.091)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	4.17	4.17	4.17	4.17
Generic XR Spending	-1.07***	-1.04***	-1.18***	-1.03***
	(0.330)	(0.265)	(0.373)	(0.275)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	8.96	8.96	8.96	8.96
Branded Spending	-4.44***	-3.95***	-4.84***	-4.09***
	(0.644)	(0.521)	(0.718)	(0.544)
Observations	252,876	252,876	204,315	204,315
Mean: Age 25	22.19	22.19	22.19	22.19
Window	12 mo	12 mo	9 mo	9 mo
Polynomial order	2	1	2	1

Note: Regression specifications include 2^{nd} degree polynomial and are estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Changes in the Type of Medication Purchased by Branded-Type Consumers at 26 and 1 month

	(1)	(2)	(3)	(4)
Generic IR | Buy	0.009	0.024***	0.005	0.019*
	(0.0111)	(0.0094)	(0.0124)	(0.0095)
Observations	19,140	19,140	15,616	15,616
Mean: Age 25	0.131	0.131	0.131	0.131
Generic XR | Buy	0.030***	0.033***	0.035***	0.031***
	(0.0102)	(0.0090)	(0.0110)	(0.0090)
Observations	19,140	19,140	15,616	15,616
Mean: Age 25	0.072	0.072	0.072	0.072
Branded | Buy	-0.045***	-0.058***	-0.048***	-0.052***
	(0.0122)	(0.0103)	(0.0133)	(0.0105)
Observations	19,140	19,140	15,616	15,616
Mean: Age 25	0.892	0.892	0.892	0.892
Window	12 mo	12 mo	9 mo	9 mo
Polynomial order	2	1	2	1

Note: Consumers are considered "Branded-Type" if the plurality of their purchases between ages 21 and 26 were branded prescriptions. Coefficients do not sum to 1 because consumers may buy multiple types of medication each month. Regression specifications include 2^{nd} degree polynomial and are estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

CHAPTER III

How Well do Doctors Know their Patients? Evidence from a Mandatory Access Prescription Drug Monitoring Program.

3.1 Introduction

The number of prescription opioids filled in the U.S. increased by roughly 300% in the first decade of the twenty-first century (*Kunins et al.*, 2013), contributing to a similarly dramatic increase in overdose deaths (*Chen et al.*, 2014; *Dart et al.*, 2015; *Rudd et al.*, 2016). Although the total volume of prescriptions has declined since 2010, nearly 2 million Americans had an opioid addiction in 2015 (*Han et al.*, 2017). While illicitly imported or manufactured narcotics are now a significant contributor, the epidemic has its roots in the misuse of prescriptions legally obtained from medical professionals. Thus, a number of policy responses to the opioid epidemic have targeted prescribing behavior.

Among the most significant policy responses to this public health crisis are state-level Prescription Drug Monitoring Programs (PDMPs). These systems track all purchases of Drug Enforcement Agency (DEA) scheduled drugs in the state and were originally designed to identify inappropriate or suspicious utilization, often for law enforcement. Current programs are designed to influence the behavior of health-care practitioners by providing comprehensive and timely information about patients' prescription histories. Forty-nine states have established a PDMP and many have strengthened their programs over time.

Historically, most PDMPs relied on providers to take the initiative to access patient prescription histories. Evidence from several states suggests that when PDMPs are voluntary, provider engagement is low (*Haffajee et al.*, 2015). This may explain results from several studies, which find no effect of PDMPs on a variety of opioid-related outcomes (*Paulozzi et al.*, 2011; *Jena et al.*, 2014; *Brady et al.*, 2014; *Li et al.*, 2014). Since 2007, 17 states have increased provider engagement via "mandatory access" laws. These policies require prescribers to consult the PDMP in certain circumstances before prescribing opioids and other DEA-scheduled drugs. Recent studies suggest that such mandates reduce the volume of opioids prescribed and indicators of misuse (*Dowell et al.*, 2016; *Wen et al.*, 2017; *Buchmueller and Carey*, 2018; *Meinhofer*, 2017; *Bao et al.*, 2018; *Haffajee et al.*, 2018; *Grecu et al.*, 2019; *Wen et al.*, 2019; *Strickler et al.*, 2019).

The first comprehensive "mandatory access" policy was enacted and implemented in Kentucky in 2012. It required all providers in the state, with limited exceptions, to check the PDMP before prescribing opioids to new patients and at intervals for continuing patients. In contrast, previous mandates in other states applied only to certain types of providers or circumstances. Since it implemented its mandate in 2012, Kentucky's PDMP has been held up as a model program (*Shatterproof*, 2016). Subsequently, several other states (including New Mexico, New York, Tennessee and West Virginia) enacted similar laws. Thus, Kentucky represents an excellent case study for investigating the impact of comprehensive "mandatory access" legislation on opioid prescribing.

We examine how this policy altered the prescribing behavior of Kentucky providers compared to providers in neighboring Indiana, which represents a good counterfactual for Kentucky for several reasons. Both states were among the top ten in opioid prescriptions per capita in 2012 (*Paulozzi et al.*, 2014). The two states are also very similar in terms of demographics, economic conditions, health systems, and health insurance coverage during the period of our analysis. Furthermore, until Kentucky's 2012 reform, the two states' PDMPs and other opioid policies were quite similar, as detailed in Section 3.2.

We estimate the policy's effect on the total morphine-equivalent dosage (MED) prescribed by each provider in each quarter. We find that after the policy went into effect, Kentucky providers significantly decreased MED prescribed relative to Indiana providers. To shed light on the changes providers made, we estimate the effect of the policy on four distinct margins: (1) whether the provider writes any opioid prescriptions; (2) the number of patients to whom they prescribe opioids; (3) the number of days supplied per patient; and (4) the average MED per day.

Our results suggest that providers primarily responded along the first two margins. After the policy went into effect, there was a 3.8 point decline in the percentage of providers writing opioid prescriptions in Kentucky (relative to the change in Indiana). Among providers that wrote any opioid prescriptions in a quarter, there was a roughly 16% decline in the number of patients. Decreases in the days per prescription and MED per day were smaller in magnitude and sensitive to specification.

There is substantial variation in provider practice style related to opioid prescribing (*Makary et al.*, 2017; *Thiels et al.*, 2017; *Schnell and Currie*, 2018). To test for heterogeneous policy effects, we sort providers into quartiles based on their prescribing in the six months prior to the mandatory access policy. Like previous research, we observe substantial variation in opioid prescribing across providers. In our data, prior to the policy change, providers in the top quartile account for 97% of total MED supplied, while, conditional on prescribing, the modal provider in the lowest quartile had only one patient with an opioid prescription. The heterogeneity analysis reveals that the decrease in the percentage of providers writing any opioid prescriptions was largely limited to low-volume providers. Low-volume providers experience a 5 percentage point (41 percent) reduction in the probability of any opioid prescribing. Among high-volume providers, the main response to the policy was to prescribe opioids to fewer patients.

A key question is what type of patients were affected. Ideally, increasing provider PDMP engagement will not simply reduce opioid prescribing, but will result in more appropriate prescribing. Providers who access PDMP records will be alerted to patients with utilization patterns that are consistent with misuse or diversion. We thus investigate whether providers targeted their reductions on patients with histories suggestive of high-risk use. We characterize patients using three mutually-exclusive categories. The first consists of "single use" patients who fill a single prescription in a quarter and none in the following quarter. This utilization pattern suggests post-surgical acute care and is generally considered low risk. We divide patients who fill multiple prescriptions into two groups, depending on whether or not they exhibit behaviors consistent with "shopping," which we define as obtaining opioids from three or more prescribers or pharmacies in a quarter.

The results from this part of the analysis suggest that providers target their reductions on those who meet the "shopping" criteria. The average provider reduced the total number of patients with opioid prescriptions by 19% and the number of "shoppers" by one-third. Providers reduce opioid supply to other patient types by a smaller but statistically significant amount. Prescriptions to single-use patients fell by 12%. Thus, while prescriptions to "shoppers" were most affected, the mandatory access policy may have induced a broader "chilling" effect on opioid prescribing.

An important strength of our analysis is that it is based on the *universe* of DEA scheduled prescriptions in Kentucky and Indiana. By comparison, most of the research on PDMPs uses insurance claims datasets or state-level aggregate data. Administrative claims datasets are limited to the enrollees of a particular payer, such as Medicare (*Buchmueller and Carey*, 2018), Medicaid (*Wen et al.*, 2017) or a private insurer (*Haffajee et al.*, 2018). Our PDMP dataset includes prescriptions purchased with all insurance types plus cash purchases. The use of cash correlates with measures of misuse such as doctor-shopping (*Cepeda et al.*, 2013). Not only is our our data representative of the entire population, but the completeness of the PDMP data allows us to account for all of a provider's patients and to conduct a provider-level analysis. This is important given that the policy is designed to affect provider behavior. With our provider-level analysis, we are able to not only estimate the overall effect of the policy on opioid prescriptions, but to investigate in more detail the provider behavior driving the results.

We explore the impact of the provision of a particular type of information – opioid prescribing histories – across the full distribution of providers. This research complements other recent field experiments that examine the change in prescribing by providers in response to information. One such experiment provided "peer comparison" information to providers prescribing opioids at extremely high levels in Medicare; this experiment concluded that such information had no effect on prescribing and ruled out meaningfully-sized impacts (*Sacarny et al.*, 2016). However, a similar experiment targeting prescribing of antipsychotics to dementia and Alzheimer patients found large decreases in prescribing, including substantial effects on clinically appropriate psychiatric patients (*Sacarny et al.*, 2018). This finding echoes our observation that the provider response, while larger among shopper patients, also reduced prescribing to single-use patients. Finally, a small experiment found that providers who were informed of the overdose death of an opioid patient prescribed 10% less MED in the following three months compared to matched prescribers who were not informed (*Doctor et al.*, 2018).

3.2 PDMP and Other Opioid Policies in Kentucky and Indiana

By 2012 both Kentucky and Indiana had well-established PDMPs. The Kentucky All Schedule Prescription Electronic Reporting (KASPER) system first became operational, with its data available to providers and dispensers, in July 1999. Indiana's

PDMP, known as the INSPECT system, was established one year earlier, but access was initially limited to state regulators; providers and dispensers gained access in March 2009. Law enforcement agencies in both states are allowed to access the data in connection with ongoing investigations. Both systems capture data on prescriptions for DEA Schedule II - V drugs. During our entire sample period, physicians in both Indiana and Kentucky could delegate access to the PDMP to a nurse or other employee.

Kentucky's mandatory access requirement was established by House Bill 1 (HB 1), which was passed in April 2012 and went into effect that July. The law requires all providers who are licensed to prescribe DEA-scheduled drugs to register with the PDMP and refers non-compliers to the Kentucky Board of Medical Licensure. With limited exceptions, providers are also required to query the PDMP the first time they order any Schedule II prescription (and Schedule III containing hydrocodone) for a patient and at least every three months thereafter. During the period we analyze, providers in Indiana faced no such requirements.

It is important to note that the Kentucky mandatory access requirement did *not* change any aspect of the reporting of controlled substance prescriptions to the Kentucky PDMP. Over the entire sample period, pharmacies reported all fills of DEA-scheduled prescriptions to KASPER using established procedures, which are unrelated to provider registration or querying behavior.

Indiana makes a useful comparator for Kentucky due to its similarity on numerous dimensions, including other policies that might affect the demand for and supply of prescription opioids, such as demographics, income, and physicians per capita. The states have similar demographics, income, and employment over the time period (see Appendix Table C.1). They ranked 35^{th} (KY) and 37^{th} (IN) in physicians per capita (*Center for Workforce Studies*, 2013). Neither state allowed the medical or recreational use of marijuana. During the period of our analysis, Indiana did not have a law allowing third-party prescribing and lay administration of naloxone. In Kentucky, such a law went into effect in June 2013. Neither state had a "good samaritan law" providing immunity from prosecution for drug possession to anyone who seeks emergency medical assistance in the event of a drug overdose.

One difference between the two states is that HB 1 also included a provision regulating pain clinics.[1] The law limits ownership of pain clinics to physicians (though

[1] The law defines pain clinics as facilities where a majority of patients receive pain medications and either the primary practice component is the treatment of pain or the facility advertises any type of pain management services. The regulations do not apply to hospital-owned facilities, hospices or long-term care facilities.

there is a grandfather provision for facilities established prior to July 2012) and a physician with appropriate board certification must be on-site practicing medicine at least 50 percent of the time. The law also requires that clinics accept private insurance and prohibits payments from parties other than a patient, their spouse, parent/guardian, or insurer. Pain clinic physicians are required to complete 10 hours of continuing medical education in pain management during each registration period.

According to KASPER, between mid-2012, when the policy went into effect, and mid-2015, 24 pain clinics closed. While we cannot definitively disentangle the effect of the mandatory access policy from these pain clinic provisions, previous research using national data suggests that the independent effect of pain clinic laws is minimal and that the estimated effect of a PDMP mandate is not sensitive to how the analysis controls for such laws (*Dowell et al.*, 2016; *Buchmueller and Carey*, 2018). Later, we provide several pieces of evidence that suggest that Kentucky's pain clinic regulations are not the predominant cause of the decline in opioid prescribing in the state.

Horwitz et al. (2018) argue that research on the impact of PDMPs is often hampered by ambiguity about when exactly programs became operational. From Figure 3.1 it should be clear that there is no such problem dating when Kentucky's access mandate went into effect. The figure presents raw administrative data on the monthly number of requests for KASPER records between 2010 and 2016. The gray bar is at 2012q3, when the mandatory access policy was implemented. Prior to that, there were 60,000-70,000 requests made per month. Apart from a slight increase in the two months before implementation, there was very little trend in the number of monthly requests. The figure shows a five-fold increase in KASPER requests coincident with the policy implementation. Similarly, the number of providers registered with KASPER rose from 37% of DEA registrants in June 2012 to 97% a year later (*Freeman et al.*, 2015). Available evidence suggests that during the period of our analysis PDMP queries in Indiana were comparable to pre-period usage in Kentucky (*Allain*, 2012).

3.3 Data and Descriptive Analysis

3.3.1 Aggregate State-Level Data

Data from Indiana's PDMP begins in the first quarter of 2012, only two quarters before mandatory access went into effect in Kentucky. Thus, it is not possible with our main data sources to test for parallel pre-trends in the two states. We address this issue by considering other data sources. Figure 3.2 presents annual data from the Centers for Disease Control (CDC) on opioid prescriptions per capita and quarterly data from the DEA's Automation of Reports and Consolidated Orders System (ARCOS) database on MED[2] per capita for the years 2006 to 2016.

Both sources indicate that opioid prescriptions in Kentucky and Indiana were trending in a roughly parallel fashion from 2006 through 2011. The ARCOS series, but not the CDC series, shows a level shift up in MED beginning with 2010q2. This level shift is due entirely to oxycodone, which jumps up in Kentucky and continues a near-linear trend in Indiana. The timing of the shift in oxycodone shipments to Kentucky coincides with two important events related to this commonly abused drug.

One is the reformulation of the extended release version of the drug, Oxycontin. Although this change had a large effect on the demand for oxycodone products and their substitutes, there is little reason to expect a large positive effect on shipments to Kentucky relative to Indiana. *Alpert et al.* (2016) show that Kentucky and Indiana had the same rates of Oxycontin misuse before the reformulation and thus were similarly exposed to it.

The other change that occured around this time is a major crackdown on pill mills in Florida (*Kennedy-Hendricks et al.*, 2016). These clinics were widely reported to be a source of drugs sold in other states. Indeed, Interstate 75, which runs through both Florida and Kentucky, was dubbed the "Oxy Express." *Evans et al.* (2018) develop a measure for cross-state comparisons that suggests Kentucky was obtaining considerably more opioids from Florida than Indiana was prior to the crackdown. Thus, it is very plausible that as the supply from Florida was reduced, the demand for oxycodone from in-state providers increased more in Kentucky than Indiana.

Table 3.1 examines trends in aggregate quarterly per capita MED consumption in Kentucky and Indiana using ARCOS data from 2006q1 to 2012q2. The models in columns 1 and 2 include just an indicator for Kentucky, a linear trend and the interaction of the two. In columns 3 and 4, we include a second Kentucky intercept to capture the level shift in oxycodone shipments after the Florida pill mill crackdown.

Using the logged outcome (column 1), the results indicate that in both states the volume of opioids grew by roughly 8% per quarter over the seven year period. When the dependent variable is specified in levels, the simpler specification suggests stronger growth in Kentucky in the pre-period. However, when we include a second intercept for Kentucky from 2010q2 onwards, we no longer find a difference in the time trends

[2]We converted ARCOS and PDMP opioid prescriptions to their morphine equivalents using conversion factors from the following three sources: *Palliative.org* (2016); *CMS* (2015); *Ohio Bureau of Workers' Compensation* (2016).

between Kentucky and Indiana. This suggests that indeed there was simply a level shift in Kentucky rather than different trends over the full pre-policy period.

Table 3.2 reports difference-in-differences estimates using quarterly MED per capita for 2006q1 to 2013q4, before and after Kentucky's implementation of the mandatory access policy (but prior to the Affordable Care Act expansions). Because the implementation quarter (2012q3) is partially treated we allow it its own dummy. In these results we again consider the impact of allowing Kentucky a second intercept for the period beginning in 2010q2. Including this additional variable does not alter the results in a qualitative sense. The first column suggests that the PDMP mandate reduced the volume of opioids in Kentucky by 11%. Allowing Kentucky to have a second intercept (similar to beginning the analysis in 2010q2) increases our estimate of the policy impact to 14%. Similarly, when the dependent variable is specified in levels both specifications indicate that the effect of Kentucky's PDMP mandate was statistically and economically significant.

3.3.2 Prescription Records

Via data use agreements with KASPER and INSPECT, we obtained the states' complete PDMP records. Each record contains the following fields: encrypted identifiers for patients, providers and pharmacies, National Drug Code (from which we derive ingredient, strength, and route of administration), number of units, days supply, patient zip code, and provider location. For most physicians with prescriptions recorded in KASPER, we also know their specialty.[3]

Our analysis period begins in 2012q1. Though PDMP data for both states are available through 2016, we end our sample period in 2013q4 to avoid a possible confounding effect of the Affordable Care Act. Kentucky implemented the ACA Medicaid expansion in January 2014 and also established its own marketplace. Indiana did not expand Medicaid until 2015 and participated in the Federal Healthcare.gov marketplace. Whereas in 2013 a similar percentage of each state's population was uninsured (14.3% in Kentucky, 14.0% in Indiana), between 2013 and 2014, the percent uninsured declined by 5.8 percentage points in Kentucky compared to only 2 points in Indiana (*Smith and Medalia*, 2015).

A key advantage of PDMP administrative data over other opioid utilization data is that we observe all or substantially all of an in-state provider's outpatient opioid

[3]Physician specialty is observed for all physicians who registered with KASPER between 2010 and 2018. In our sample, physician specialty is known for 96% of Kentucky prescriptions.

prescribing,[4] which allows us to conduct a provider-level analysis. By comparison, analyses of aggregate opioid supply (e.g., ARCOS data) do not report data at the provider level, and claims from a subsample of patients (e.g., Medicare data) do not fully capture a provider's prescribing behavior. In particular, our PDMP data include all cash purchases, which is predictive of other suspicious behaviors (*Cepeda et al.*, 2013).[5] Therefore, analyses based on PDMP data yield the maximal insight on how providers respond to mandatory access policies.

We limit our sample to providers who practiced in Kentucky or Indiana. Prescriptions filled in Kentucky and Indiana but written by out-state providers are disregarded because those providers are subject to other states' opioid regulations. Our main analyses are done on a balanced panel consisting of quarterly observations of providers who wrote at least one opioid prescription in any quarter between 2012q1 and 2013q4.

3.3.3 Hypotheses and Outcome Measures

There are several possible provider responses to a PDMP use mandate. The requirement that providers have an active account and check the database before prescribing opioids introduces fixed compliance costs. Some providers may cease prescribing opioids altogether rather than bear the cost associated with learning to navigate the system. Therefore, we test whether the policy induced a change on the extensive margin of writing any opioid prescriptions in a quarter.

Fundamentally, PDMPs are designed to alert providers to possible doctor shopping and other suspicious patterns of patient behavior. Previous research based on Medicare claims data finds that mandatory access policies significantly reduce the number of patients receiving prescriptions from multiple providers and the number of "new patient" visits (*Buchmueller and Carey*, 2018). Thus, we hypothesize that among providers who continue to prescribe opioids, the strongest effect of a mandatory access policy will be on the number of patients to whom they prescribe.

[4]KASPER and INSPECT capture about 95% of the total MED shipped to a state (as reported by ARCOS) or about 95% of all prescriptions filled in the state (as reported by the CDC). We expect the PDMP to capture less than 100% of the ARCOS volume, which includes opioids administered to hospital inpatients (not reported to PDMPs). The CDC data is based on a sample of retail pharmacies; the CDC does not give detailed information about its methods.

[5]While our PDMP datasets always report prescriptions purchased with cash, information on the source of payment is not available in Kentucky until 2015. In 2015, after both states had expanded their Medicaid programs, 8% of prescriptions in KASPER and 10% of INSPECT prescriptions were purchased with cash. Prior to Medicaid expansion, 14% of Indiana's prescriptions were purchased with cash.

It is possible that a provider encountering a PDMP report that suggests a patient is overusing opioids may not refuse to prescribe to that patient, but rather will write a weaker prescription in hopes of weaning the patient off high-dosage or chronic opioid use. Additionally, the mandate may indirectly affect prescribing intensity. In a survey of Kentucky prescribers, roughly three-quarters of respondents said they believed they were being more closely monitored after the policy went into effect (*Freeman et al.*, 2015). This perception may have led some to prescribe more conservatively. And checking the PDMP more often may affect prescribing intensity by raising the salience of safe prescribing practices. We analyze two measures of prescribing intensity: days supplied per patient and the average MED per day.

Table 3.3 presents summary statistics for these outcomes aggregated to the provider by quarter level. In the pre-period (2012q1 and 2012q2) the percentage of providers prescribing any opioids was identical in Indiana and Kentucky (74%). Conditional on prescribing opioids, the average Kentucky provider prescribed to more patients (60.2 vs. 54.5). MED per provider are higher in Kentucky because of the difference in the number of patients; the baseline means for days/patient and MED/day were essentially identical in the two states. Figure 3.3, which presents the provider-level distribution of log MED prescribed for the pre-period, also indicates that prescribing patterns were quite similar in the two states before Kentucky's policy change.

After the policy change, the number of Indiana providers writing any opioid prescriptions increased by 2 percentage points, while the percentage in Kentucky fell by 2 points. The number of patients per provider fell in both states, but more so in Kentucky (-6.1 vs. -2.4). There was essentially no change in either intensity measure in either state. Overall, the mean MED per provider fell by 9.4% in Kentucky and by 0.4% in Indiana.

3.4 Econometric Analysis

3.4.1 Overall Impact of Kentucky's Mandatory Access Provision

We estimate the impact of Kentucky's mandatory access policy in a difference-in-difference framework. Our estimating equation is

$$Y_{it}^{j} = \alpha^{j} KYpost_{it} + \beta^{j} KY \times 2012q3_{it} + \delta_{t}^{j} + \delta_{i}^{j} + \varepsilon_{it}^{j}, j = 1, 2, 3, 4 \qquad (3.1)$$

where i indexes providers, t indexes calendar quarters, and the j superscript indexes the four distinct decisions that providers make regarding opioids: whether to write any opioid prescriptions; the number of patients; days supply per patient; and MED per day. The product of these four outcomes is the total MED prescribed by a provider in a quarter.

The policy variable, $KYpost$, equals 1 for Kentucky providers beginning in 2012q4 and 0 elsewhere; Kentucky's partially-treated implementation quarter, 2012q3, is accounted for with its own dummy variable. Because the data from both states include encrypted provider identifiers, we are able to condition on provider fixed effects (δ_i). Our models also include quarter fixed effects (δ_t). We cluster ε_{it} at the provider level. With only two states, asymptotics for consistency will not apply if standard errors are clustered at the state level. However, clustering at the provider level will account for much of the serial autocorrelation in errors, which is the main concern that *Bertrand et al.* (2004) identify for difference-in-differences models. We explore inference under two alternative models in Section 3.4.2.

Y^1 is an indicator variable that equals one if a provider wrote at least one prescription in the quarter and zero otherwise. Because of the fixed effects we specify this equation as a linear probability model. For the continuous outcomes, we report two sets of models, one with the dependent variable in levels, the other in natural logs. As shown in Figure 3.3, the distribution of MED prescribed by providers in the pre-period appears to be approximately lognormal. Intuitively, we expect the policy to have a proportional effect on the number of patients, which points to the log model as the preferred specification. For the other continuous outcomes, it is less clear *a priori* whether logs or levels should be preferred. Tests for model specification recommended by *Deb et al.* (2017) suggest that the log model fits our data better for all three continuous outcomes.

In addition to the basic difference-in-differences specification, we also estimate an event study version of the model in which an indicator variable for Kentucky is interacted with each time dummy. Since we have a very short pre-period, we rely on the previous analysis of ARCOS and CDC data to provide evidence on pre-period trends. We primarily use this specification to confirm that the estimated treatment effect coincides with the quarter when HB 1 was implemented and to examine the dynamics of the treatment effect in the post-period.

The event study results are presented graphically in Figure 3.4. Each of the four variables exhibits a sharp decline in 2012q3 relative to the previous quarter, with the full impact realized by 2012q4. The fact that the movement in the variables is so tightly linked to the policy timing is reassuring. The pattern in these event studies –

a sharp change in prescribing behavior followed by parallel trends in the post-period – is well-captured by a difference-in-differences framework.

Table 3.4 reports difference-in-differences regressions for each of our four outcomes. The first column indicates that mandatory access reduced the probability of any opioid prescribing by nearly 4 percentage points. This significant effect on the extensive margin is consistent with the hypothesis that fixed compliance costs may have led some providers to stop prescribing scheduled drugs altogether. We provide further support for the fixed-cost hypothesis in the next section.

We hypothesize that requiring providers to check a PDMP before prescribing opioids will have the strongest effect on the number of patients receiving prescriptions. The regression results indicate a large provider response along this margin. The log specification implies that among providers writing any opioid prescriptions in a quarter, Kentucky's mandatory access policy reduced the number of patients by 16% (exp(-.177) – 1 = -.162). Specifying the model as linear in levels also yields a significant policy effect, though, relative to the sample mean, the percent effects are slightly smaller (-11%). The results for Y^1 and Y^2 can be combined to estimate the total effect on the number of patients treated using standard methods for two-part models (*Deb et al.*, 2017). Because the change in the extensive margin was concentrated among very low volume providers, the reduction in the number of patients among providers writing any opioid prescriptions in a quarter accounts for roughly 95% of the total decline in the number of patients per provider.

Estimated effects for the average days per patient and MED per day are smaller and more sensitive to specification. In the log model, the mandatory access policy is estimated to reduce days per patient by 4%, whereas the levels model implies essentially no change. Similarly, for MED per day, the log model implies a statistically significant 3 to 4% reduction, whereas the corresponding levels model implies smaller and less precisely estimated reductions. As noted, any effects on these margins are likely to be indirect. The changes observed for these outcomes may also reflect a change in the composition of patients receiving opioids after the policy change. As we show below, the effect of the policy on the patient margin was strongest for patients who filled multiple prescriptions. Reducing the number of high-use patients will have the effect of also reducing measures of prescribing intensity. Because of this and the sensitivity of the estimates to specification, we are reluctant to conclude that providers responded to the policy by reducing the number of days or MED per day.

We calculate the overall effect on MED supplied by combining the estimates from our four outcomes, which can be multiplied to yield total MED. When the outcome variables are in levels, we find a 10.2% reduction in MED after the policy in Kentucky relative to Indiana. Our estimate is in line with the simple comparison of sample means, which implies a 9% reduction, and the ARCOS regressions, which imply a 10-14% reduction. We find a somewhat larger impact when we specify the dependent variables in logs, but the log model is not ideal for calculating the overall effect on MEDs. The calculation with the log model requires retransformation to levels, which can be inaccurate if there is treatment effect heterogeneity because of the Duan smearing factor (*Deb et al.*, 2017).[6]

3.4.2 Robustness and Specification Checks

Since Kentucky implemented its mandatory access policy near the time when it implemented regulations on pain clinics, our difference-in-difference estimates could be driven by the pain clinic regulation. To examine this possibility, we isolate the group of physicians most likely to be affected by pain clinic regulation using the information on specialties that we observe for Kentucky physicians. In the third set of columns in Table 3.3, we report the pre- and post-period averages for Kentucky physicians in general and excluding the roughly 1% reporting a pain management specialty. The pre-post differences for each variable are weakly greater in the sample that excludes pain management specialists. This is the opposite of the pattern we would expect if pain clinic regulation is the cause of our estimates.

One advantage of the fact that the two states we analyze are neighbors is that they are more likely to be subject to similar economic shocks. At the same time, there are possible disadvantages. A significant population center, Louisville, lies close to the Indiana border, giving rise to a region known as "Kentuckiana." To the extent that some Kentucky patients respond to the PDMP mandate by seeking prescriptions in Indiana, our estimates will be biased away from zero, representing the combined effect of a decrease in prescriptions in the treatment state and an increase in the control state.

To test the sensitivity of our results to border-crossing, we re-estimated all models on two subsets. The first excludes all counties in either state along the Kentucky-Indiana border, and the second further excludes all counties along any Kentucky border. Results from this robustness check are reported in Table 3.5. The results

[6] We note that the 10% overall reduction is not inherently inconsistent with the 16% decrease in the number of patients estimated by the log model. The log model measures the average change in logs, which corresponds to the average percentage change across providers. The log model weights the percent change from a low volume prescriber the same as the percent change from a high volume prescriber.

are statistically indistinguishable from the full sample result and nearly the same to the hundredth place. While cross-border contamination need not be limited to the counties that lie along the border, we are reassured by the fact that the results are so similar when excluding the individuals for whom border effects are likely to be largest.

Another issue with analyzing only two states (neighbors or not), is that asymptotics for consistency will not apply if standard errors are clustered at the state level. As a robustness check to test the sensitivity of our inferences, we follow the suggestion of *Bertrand et al.* (2004) and collapse the data to create a single pre- and post-period observation for each provider, to account for residual serial correlation in the errors. Results from this exercise are reported in Appendix Table C.2. Estimating the model on this data set yields similar point estimates and standard errors that are roughly 30% larger than those obtained using quarterly data. For the extensive margin of prescribing any opioids (Y^1) and the number of patients (Y^2), this difference does not qualitatively change our inferences: the estimates remain statistically significant at the 1% level.

In addition, we conduct an exercise recommended by *Donald and Lang* (2007) to assess whether we are overrejecting the null hypothesis of no effect. They suggest estimating the difference-in-differences coefficient for every consecutive two periods within the sample period. If standard errors are severely underestimated, these placebo tests will return statistically signifcant results. Appendix Table C.3 implements this exercise, reporting the coefficients and t-statistics for all four outcomes across all seven possible two-quarter intervals. Our implementation period is 2012q3, and the two regressions that include that period are bolded for reference. The placebo regressions are generally, though not always null. However, the t-statistics for the implementation period average more than five times the t-statistics for the placebo tests. This suggests that even if the standard errors are somewhat too large, inference is likely to be robust to smaller standard errors.

3.4.3 Heterogeneity Across Providers by Pre-Period Prescribing Volume

We hypothesize that the Kentucky law had different impacts for higher and lower volume providers. Low-volume providers may be most reluctant to pay the costs of mandatory access compliance, since opioid prescribing is not critical to their practice. Additionally, low-volume providers may not be sufficiently familiar with opioid prescribing histories to confidently interpret a PDMP record (*Carey et al.*, 2018). Thus, we expect that low-volume providers are more likely than high-volume providers to stop prescribing.

It is less obvious which types of providers will reduce the number of patients the most. High-volume providers treat hundreds of patients every quarter and are more likely to be pain specialists. Since chronic pain patients are at high risk for opioid misuse, pain specialists may be most likely to learn of suspicious behavior when they begin using the PDMP. On the other hand, these providers may *already* use the PDMP prior to the mandatory access provision. And of course, some high-volume providers may engage in illicit opioid distribution, and thus may be insensitive to the information contained in the PDMP.

To estimate heterogeneous policy effects by volume, we divide the provider sample into quartiles based on the total MED prescribed in the six months before Kentucky's policy went into effect. Table 3.6 provides summary statistics on providers in each quartile. All of the outcome variables that we analyze increase monotonically across the quartiles, with the differences being most pronounced for the number of patients treated. The first quartile is made up of infrequent prescribers. Only 12% wrote an opioid prescription in each quarter in the pre-period and the modal provider who did so had only one patient. Conditional on prescribing, mean days per patient and MED per day are low for providers in quartile 1 relative to other providers. This is consistent with lower-volume providers treating opioid-naive patients with short-term pain. Quartiles 2 and 3 differ mainly in terms of the number of patients to whom opioids are prescribed. Quartile 4 appears to include many pain specialists. The average provider in this quartile prescribes to a high number of patients and writes prescriptions with longer durations.

For each quartile, we estimate a separate set of regressions. These results are reported in Table 3.7, the first column of which repeats the full sample results from Table 3.4. For brevity, we report only the log models for the continuous outcomes, and show the level models in Appendix Table C.4.

The first panel reports the effect of mandatory access on the probability of writing any opioid prescriptions in a quarter. We find that the lowest-volume prescribers are 5 percentage points less likely to write a prescription due to the policy change. Relative to the pre-period mean, this is a 41% decline. The estimated coefficient is slightly larger for quartile 2, though in percentage terms the effect is smaller. The vast majority of providers in quartile 3 and 4 continue to prescribe after the policy change.

The results by provider quartile support the hypothesis that low-volume providers view the fixed costs of mandate compliance as excessive relative to the benefits. As a further test of the fixed cost hypothesis we also examine whether the number of providers who never again write an opioid prescription increases. The results, reported in Appendix Table C.5, suggest that Kentucky's access mandate did lead low-volume providers to "exit the market". We find that high-volume Kentucky prescribers are *not* more likely to cease prescribing than their Indiana counterparts. This is further evidence that the closure of pain clinics is not the main driver of our results.

Among providers who continue to prescribe opioids, there is a substantial decline in the number of patients by between 15% and 17% for providers in quartiles 2 through 4. For providers in the first quartile, the log-linear model implies a smaller percent decline (8%) and the linear model indicates no significant change. A weak effect for the lowest volume providers is not surprising given that for this group, the bulk of the variation is at the extensive margin.

Results for the two measures of prescribing intensity, days per patient and MED per day, are reported in the bottom two panels. As in the full sample analysis, these results are mixed, providing less clear evidence for an effect of the policy. Effects on log days per patient are absent for the lowest volume providers, who already write very short duration prescriptions, and small for the highest volume providers. When measured in levels, none of the estimates are statistically significant. The magnitude of the effect sizes for the natural log of MED per day are monotonically decreasing. Among providers in the fourth quartile, who account for the vast majority of all opioid prescriptions, we see no significant reduction in MED per day.

3.4.4 Prescribing Reductions by Patient Type

The goal of PDMPs is to alert providers to possible drug-seeking and other indicators of high-risk use. We now examine whether providers target reductions on patients with suspicious opioid histories that would be revealed in PDMP records. We are also interested in whether providers reduce prescribing to patients *without* suspicious behaviors. Such a finding would suggest that mandatory access was associated with a general chilling effect, which potentially could have inhibited clinically appropriate prescribing.

To provide insight on how different types of patients were affected by the policy, we define three mutually exclusive patient types. In contrast to our prescriber volume quartiles, which are defined using only pre-period data, we categorize patients contemporaneously because many obtain a prescription in only a single quarter. "Single use" patients fill a single prescription in a quarter and none in the following quarter. As shown in Table 3.6, in the pre-period, 29% of the average provider's patients (about 12 patients) were single-use patients. Among patients observed filling multiple prescriptions we distinguish between "shoppers" and "non-shoppers." Shoppers are defined as patients who receive prescriptions from three or more providers or fill prescriptions at three or more pharmacies in a given quarter.[7] In the pre-period, 6% of patients in either state meet our definition of "shoppers". However, because these patients appear in the patient pools of multiple providers, roughly 14% of each provider's patients meet the standard, as is reported in Table 3.6. Individuals exhibiting shopping behavior comprised a similar share of providers' patient set across the volume quartiles. However, since high-volume providers account for the bulk of all prescriptions, most shoppers (more than two-thirds) obtain opioids from these prescribers. The most common consumption pattern was filling multiple prescriptions but not meeting our shopping criteria. Presumably, many of these individuals are chronic pain patients.

We use our categorization to examine whether providers targeted reductions in opioid prescriptions on high risk patients. For simplicity, we report for each patient type an overall effect on the number of patients that combines the effect on any prescribing (extensive margin) and the effect on the number of patients (intensive margin) (*Deb et al.*, 2017).

Table 3.8 reports estimated policy effects by patient type, as well as bootstrapped standard errors (*Belotti et al.*, 2015). Consistent with the goal of the policy, the effect of the policy was largest in percentage terms for shoppers and smallest for single-use patients. The average provider prescribed to 2.6 fewer shoppers, which represents a 34% effect. Reductions in prescribing to shopping patients is exactly what we expect from the provision of PDMP information; without a PDMP it is difficult for a provider to observe prescriptions written by other providers.

However, there are meaningful declines for the other patient types. A 17% decline in non-shopping patients and a 12% decline in single-use patients is consistent with providers imperfectly targeting the reductions in patients. It is possible these patients were adversely affected by a chilling effect. At the same time, the PDMP may include other information suggesting that an opioid prescription would be contraindicated, such as prescriptions for benzodiazepines (*Dasgupta et al.*, 2016). The

[7] Most analyses of opioid misuse define measures of multiple use of providers and pharmacies over a longer period, such as a six months or a year (*Buchmueller and Carey*, 2018; *GAO*, 2011; *Jena et al.*, 2014). We use a quarterly measure to investigate whether patterns consistent with provider and pharmacy shopping change shortly before compared to after the policy change. Of the patients who obtain a prescription from 3+ providers in a quarter, roughly three-quarters obtain prescriptions from 4 or more providers in a year.

number of patients with overlapping claims for opioids and benzodiazepines fell by 5% in Kentucky after the mandatory access policy went into effect, while there was no change in Indiana.

3.5 Discussion: Study Limitations and Strengths

It is important to acknowledge several possible limitations of our analysis. With any analysis that focuses on a single state, questions can be raised about the external validity of the particular "case study". We first consider whether the policy that is being examined representative of an approach that other states might adopt. Kentucky was the first state to implement a comprehensive PDMP mandate and shortly after its law went into effect other states enacted similar legislation. Whether the other states explicitly based their PDMP mandates on the Kentucky policy or they just adopted best practices, the subsequent legislation followed Kentucky's model and we expect that if more states implement mandates they will look like Kentucky's. Our one-state case study also has an advantage compared to studies that use data on all states. Such studies define the "treatment group" based on multiple states with policies of varying strength (for example, *Buchmueller and Carey* (2018)). The average effects estimated using such an approach may not correspond directly to specific policies that states are considering.

A second threat to external validity is special circumstances that would affect replicability. As we have noted, the legislation that established the PDMP mandate also introduced new regulations governing pain clinics, raising the possibility that our results represent the combined effect of these two reforms. Although it is not possible to precisely disentangle the separate effects of these two policies, several patterns we observe suggest that our results are not merely the effect of the pain clinic law. First, the overall prescribing reductions in Kentucky are the same or greater if we exclude the pain management specialists most likely to be affected by the pain clinic closures. Secondly, if opioid prescriptions fell because of pain clinic closures, we would expect to see a significant reduction in the number of high-volume providers writing any opioid prescriptions in Kentucky relative to Indiana. We do see a policy effect on the extensive margin, but it is low-volume providers who "exit the market." The largest effects we find are on the number of patients to whom opioid prescriptions are written. Again, if this effect was driven by stricter pain clinic laws it would be concentrated among the highest volume providers, as *Rutkow et al.* (2015) find in an analysis of Florida pain clinic closures. Yet, for this outcome we find roughly comparable effects for all but the lowest volume providers.

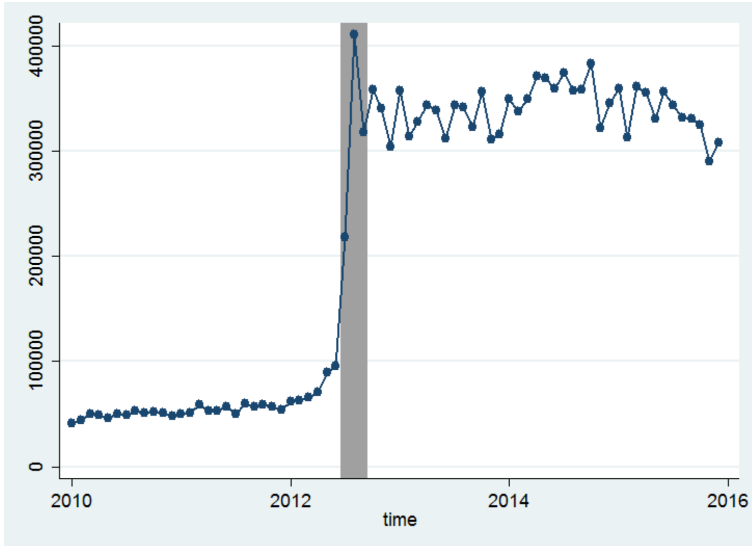
Our analysis also has limitations that have possible implications for internal validity. With any analysis using a difference-in-differences research design, the internal validity of the results depends on whether the "control group" represents a plausible counterfactual. It is standard practice to assess the plausibility of this assumption by testing for parallel trends in the period before the policy went into effect. If outcomes trended similarly in treatment and control states during the pre-period, the assumption that control states are a good counterfactual is more plausible. Because we have limited PDMP data prior to the implementation of Kentucky's mandate, we cannot assess pre-trends in that data. However, we are able to assess pre-trends using other state-level data. The results are generally supportive of the parallel trends assumption. Moreover, the fact that the two states are so similar not only in terms of prescribing characteristics right before Kentucky's mandate but also in terms of demographic and economic characteristics, provides further support for the use of Indiana as a control group.

3.6 Conclusion

Prescription drug monitoring programs have the potential to decrease inappropriate prescribing of opioids. But PDMPs will only be effective if healthcare providers access the data. In an effort to increase provider engagement, several states have recently enacted policies requiring providers to query the state's PDMP before prescribing opioids. This paper evaluates the first comprehensive PDMP mandatory access policy, which was enacted by Kentucky in 2012. We find that providers responded to this policy in two main ways. Some, who prescribed low volumes of opioids before the policy went into effect, stopped prescribing the drugs altogether. This is consistent with the idea that the policy introduced fixed compliance costs that low-volume providers were not willing to bear. Higher volume providers continued to prescribe, but wrote prescriptions to fewer patients.

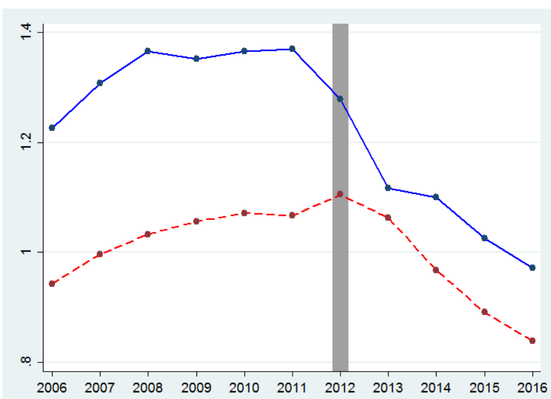
We also assess what types of patients were affected. Ideally, PDMP data will help providers identify doctor shoppers and other high risk patients. Our results suggest that providers reduced prescriptions to patients whose prescription histories suggest possible doctor or pharmacy shopping. We find large reductions (in percentage terms) in the number of such patients receiving opioid prescriptions. At the same time, we find economically significant reductions in the number of patients without suspicious prescribing histories. These decreases suggest there may also be patients with a clinically-justified need for pain relief who lose access to treatment as a result of the policy.

Figure 3.1: Requests for KASPER reports, 2010-2015

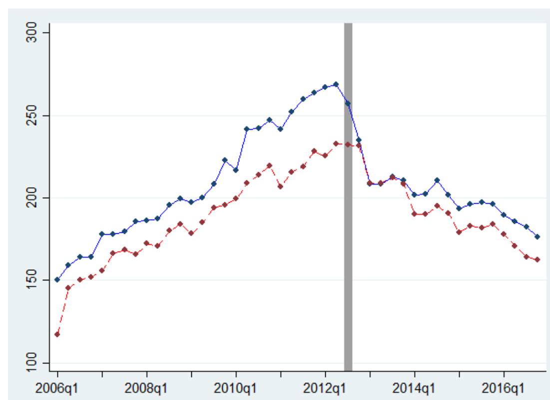


Note: Monthly administrative count, obtained via personal communication with KASPER staff. Shaded area represents implementation quarter 2012q3.

Figure 3.2: Opioid Utilization Per Capita in Indiana and Kentucky, 2006-2016



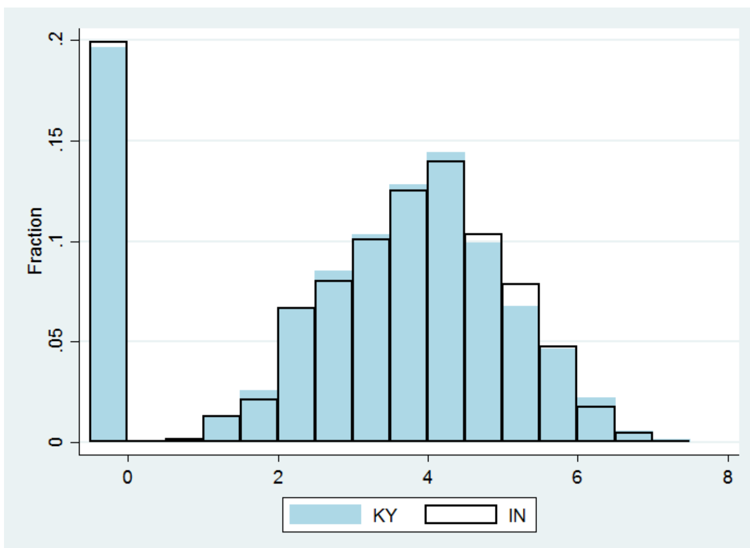
(a) Annual Prescriptions Per Capita
Source: CDC



(b) Quarterly MED Per Capita
Source: ARCOS

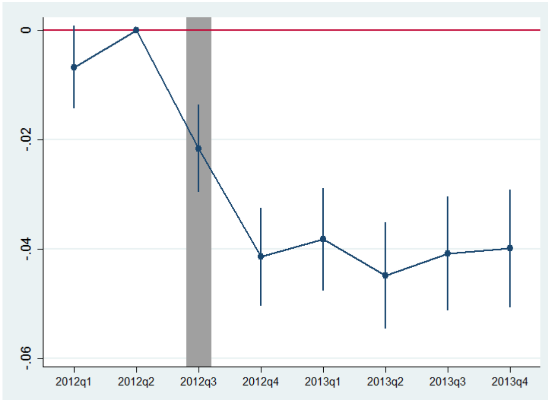
Note: Shaded area represents implementation period (2012 for CDC and 2012q3 for ARCOS.) Blue solid line represents KY and red dashed line represents IN.

Figure 3.3: Distribution of Logged (Base 10) Total MED Prescribed by Provider, 2012h1 (0 mapped to -0.5)

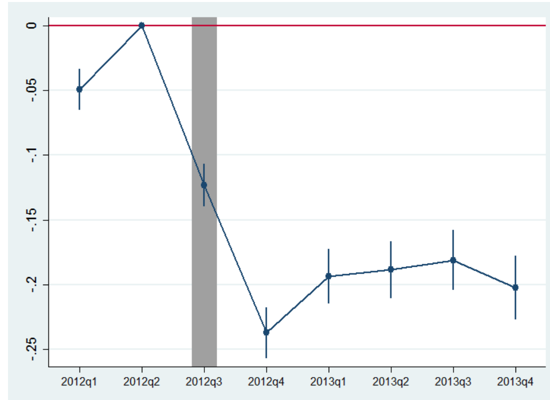


Note: Figure depicts distribution of providers in Kentucky (blue) and Indiana (outline) based on the logged (base 10) total MED they prescribe in 2012h1, with those who prescribe zero mapped to -0.5.

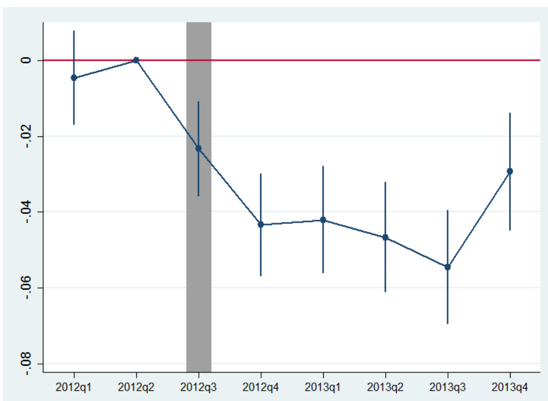
Figure 3.4: Provider-Level Prescribing Behavior: Event Study, 2012q1-2013q4



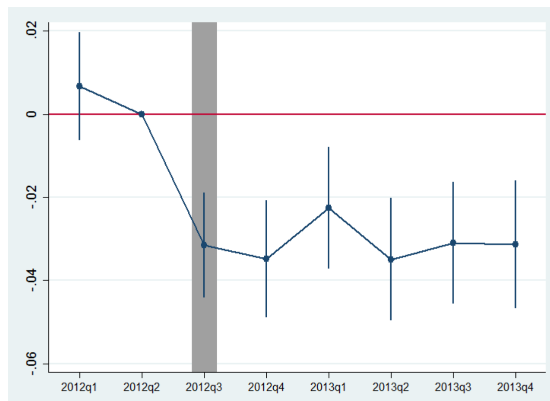
(a) Any Prescription Written



(b) Ln Unique Patients



(c) Ln Days Per Patient



(d) Ln MED Per Day

Note: In these event study figures, coefficients represent the deviation from the mean difference between Kentucky and Indiana in each quarter 2012q1 to 2013q4, with 2012q2 normalized to zero. Regressions include provider and quarter fixed effects. Standard errors clustered at provider level. Shaded area represents implementation quarter 2012q3. Bar represents 95 percent confidence interval.

Table 3.1: Pre-Period Trends in Aggregate MED Per Capita (ARCOS data), 2006q1-2012q2

	Log	Level	Log	Level
KY	0.143***	37.35***	0.0917***	19.45***
	(0.0232)	(3.466)	(0.0320)	(5.067)
KY#Post2010q2			0.0462**	16.21***
			(0.0185)	(3.366)
Time	0.0840***	15.17***	0.0840***	15.17***
	(0.00740)	(0.899)	(0.00747)	(0.909)
KY#Time	0.00748	3.884***	-0.00219	0.492
	(0.00774)	(0.997)	(0.00908)	(1.220)
2nd intercept for KY	No	No	Yes	Yes
Observations	52	52	52	52

Note: Table reports pre-trends analysis of quarterly aggregates MED per capita for Kentucky and Indiana from ARCOS. Models include a constant (not shown) and end in the quarter prior to Kentucky's implementation of a mandatory access policy. Columns 1 and 3 use logged outcomes. Columns 3 and 4 include a binary variable for Kentucky 2010q2 to 2012q2. Robust Huber-White standard errors reporting throughout. $**$ p<0.01, $*$ p<0.05, + p<0.1

Table 3.2: Difference in Difference Analysis of Aggregate MED Per Capita (ARCOS data), 2006q1-2013q4

	Log	Level	Log	Level
KY	0.116**	23.27**	0.0987**	17.13**
	(0.00871)	(2.027)	(0.0108)	(1.423)
KY#Post2010q2			0.0491**	17.73**
			(0.0123)	(2.120)
KY#2012q3	-0.0141	1.560	-0.0462**	-10.04**
	(0.00871)	(2.027)	(0.00588)	(1.572)
KY#Post2012q3	-0.111**	-22.20**	-0.143**	-33.80**
	(0.00929)	(2.155)	(0.00672)	(1.737)
2nd intercept for KY	No	No	Yes	Yes
Quarterly fixed effects	Yes	Yes	Yes	Yes
Observations	64	64	64	64

Note: Table reports a difference-in-difference analysis comparing aggregate quarterly MED per capita for Kentucky and Indiana from ARCOS before and after Kentucky's implementation of a mandatory access policy in 2012Q3. KY#2012Q3 corresponds to implementation period. Robust Huber-White standard errors. $**$ p<0.01, $*$ p<0.05, + p<0.1

Table 3.3: Sample Means of Quarterly Provider-Level Outcomes

	Indiana All Prescribers			Kentucky All Prescribers			Kentucky Excl. Pain Mgmt.		
Outcome	Pre	Post	Change	Pre	Post	Change	Pre	Post	Change
MED (1000s)	56.1	55.9	-0.2	62.6	56.7	-5.9	49.2	41.8	-7.5
Any Prescription	0.74	0.76	0.02	0.74	0.72	-0.02	0.74	0.72	-0.02
Unique Patients | Any	54.5	52.1	-2.4	60.2	54.2	-6.1	56.4	49.5	-6.9
Days/Patient | Any	18.5	18.6	0.1	18.9	19.3	0.4	18.4	18.8	0.4
MED/Day | Any	35.6	35.5	-0.1	34.6	34.3	-0.3	34.4	34.1	-0.3

Note: This table reports means of quarterly provider-level measures based on PDMP data. The column marked ”pre” refers to 2012q1 & 2012q2; the column marked ”post” refers to 2012q4 through 2013q4. The first set of columns reports on all prescribers in Indiana, the second on all prescribers in Kentucky, and the third on Kentucky prescribers excluding pain management specialists.

Table 3.4: Difference-in-Differences Estimates of the Effect of Kentucky's Mandatory Access PDMP Law

VARIABLES	(1) Any Prescription	(2) Patients	(3) Days/Patient	(4) MED/Day
	Results with Dependent Variables 2-4 in Logs			
KYPost	-0.0377**	-0.177**	-0.0409**	-0.0343**
	(0.00379)	(0.00913)	(0.00536)	(0.00513)
Observations	290,464	215,409	215,328	215,328
Number of providers	36,308	36,308	36,294	36,294
Mean in KY pre-period	0.739	60.20	18.85	34.61
Treatment Effect in Levels	-0.0377	-10.73	-0.805	-1.211
	Results with Dependent Variables 2-4 in Levels			
KYPost	-0.0377**	-6.466**	-0.0334	-0.418*
	(0.00379)	(0.607)	(0.113)	(0.187)
Observations	290,464	215,409	215,328	215,328
Number of Providers	36,308	36,308	36,294	36,294

Note: Table reports difference-in-difference coefficients from estimation of Equation 3.1 on a panel of quarterly provider-level measures 2012q1 to 2013q4. Outcomes in second, third, and fourth column are conditional on the provider having any prescribing in the quarter. Outcomes in the third and fourth column are further conditional on having information on days supply. Means of outcomes reported in levels for Kentucky in the pre-period. Standard errors clustered at provider level.
$**$ p<0.01, $*$ p<0.05, + p<0.1

Table 3.5: Impact of Excluding Providers in Border Counties

	(1) Any Prescription	(2) Ln Patients	(3) Ln Days/Patient	(4) Ln MED/Day
All providers				
KYPost	-0.0377**	-0.177**	-0.0409**	-0.0343**
	(0.00379)	(0.00913)	(0.00536)	(0.00513)
Observations	290,464	215,409	215,328	215,328
Number of providers	36,308	36,308	36,294	36,294
No KY-IN border counties				
KYPost	-0.0415**	-0.177**	-0.0497**	-0.0293**
	(0.00446)	(0.0107)	(0.00637)	(0.00592)
Observations	228,672	171,221	171,155	171,155
Number of providers	28,584	28,584	28,572	28,572
No KY-IN border counties, no KY border counties				
KYPost	-0.0398**	-0.186**	-0.0491**	-0.0303**
	(0.00523)	(0.0130)	(0.00749)	(0.00696)
Observations	203,304	152,736	152,692	152,692
Number of providers	25,413	25,413	25,408	25,408

Note: The first panel repeats the analysis of Tabel 3.4. The next panel excludes all providers located in a county on the Indiana-Kentucky border (in either state). The next panel excludes all providers located in a county on the Indiana-Kentucky border as well as any other Kentucky border county.
** p<0.01, * p<0.05, + p<0.1

Table 3.6: Provider-Level Outcomes by Pre-Period Provider Volume

	All	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Total MED in 2012H1 (000s):					
minimum	0	0	0.136	3.652	31.024
maximum	14,639	0.135	3.652	31.022	14,639
Average Quarterly Outcomes in Pre-Period:					
Any prescriptions in quarter: mean	73.9%	12.1%	85.0%	98.7%	99.8%
Number of patients: mean	41.9	0.17	4.7	35.4	127.4
Percent Single Use	29%	61%	51%	47%	23%
Percent Multiple Use Non-Shoppers	57%	29%	36%	38%	63%
Percent Multiple Use Shoppers	14%	9%	12%	15%	13%
Days per patient	18.6	5.83	9.9	12.3	33.7
MED per day	35.2	14.73	28.9	34.7	43.5

Note: This table reports means of pre-period provider-level measures by provider volume quartile, based on MED prescribed in pre-period (2012h1.) Sample statistics correspond to quarterly averages in in the pre-period.

Table 3.7: Difference in Differences Estimates by Provider Quartile

Sample	(1) All	(2) Quartile 1	(3) Quartile 2	(4) Quartile 3	(5) Quartile 4
			Any Prescription		
KYPost	-0.0377**	-0.0533**	-0.0644**	-0.0232**	-0.00957**
	(0.00379)	(0.00892)	(0.00729)	(0.00472)	(0.00311)
Observations	290,464	72,696	72,536	72,616	72,616
No. of providers	36,308	9,087	9,067	9,077	9,077
Mean in KY pre	0.739	0.129	0.850	0.989	0.998
			Ln Patients		
KYPost	-0.177**	-0.0824+	-0.180**	-0.190**	-0.167**
	(0.00913)	(0.0470)	(0.0189)	(0.0150)	(0.0148)
Observations	215,409	24,264	51,585	68,426	71,134
No. of providers	36,308	9,087	9,067	9,077	9,077
Mean in KY pre	60.20	1.329	5.251	35.85	142.1
			Ln Days/Patient		
KYPost	-0.0409**	-0.00134	-0.0616**	-0.0505**	-0.0183**
	(0.00536)	(0.0460)	(0.0133)	(0.00844)	(0.00683)
Observations	215,328	24,217	51,559	68,421	71,131
No. of providers	36,294	9,073	9,067	9,077	9,077
Mean in KY pre	18.85	5.659	10.04	12.50	34.98
			Ln MED/Day		
KYPost	-0.0343**	-0.167**	-0.0542**	-0.0340**	-0.00802
	(0.00513)	(0.0580)	(0.0134)	(0.00738)	(0.00553)
Observations	215,328	24,217	51,559	68,421	71,131
No. of providers	36,294	9,073	9,067	9,077	9,077
Mean in KY pre	34.61	16.69	29.95	35.03	40.68

Note: Table reports difference-in-difference coefficients from estimation of Equation 3.1 on a panel of quarterly provider-level measures 2012q1 to 2013q4 by quartile of provider MED in 2012h1. Outcomes in second, third, and fourth panel are conditional on the provider having any prescribing in the quarter. Means of outcomes reported in levels for Kentucky in the pre-period. Standard errors clustered at provider level. ** p<0.01, * p<0.05, + p<0.1.

Table 3.8: Change in Number of Patients Seen by a Provider by Patient Type

	(1)	(2)	(3)	(4)
	All	Single-Use	Multiple Use	
			Non-Shoppers	Shoppers
Total Effect in Levels	-9.483**	-1.689**	-5.523**	-2.584**
	(0.510)	(0.127)	(0.334)	(0.0905)
Implied Effect in Percent	-19.4%	-12.2%	-17.1%	-33.9%

Note: Single-use patients defined as patients who receive one prescription in current period and none in next period. Multiple-use shoppers defined as patients who fill a prescription from 3+ providers or at 3+ dispensaries in a quarter. Remaining patients are multiple-use nonshoppers. Estimates based on a two-part model combining a linear probability model of any prescribing to the patient type with an OLS regression of the log number of patients of the given type. For ease of interpretation, the combined effect is reported in both percentage change and levels. All regressions include provider fixed effects. Bootstrapped standard errors based on 1000 replications, which are sampled with replacement at the provider level. ** p<0.01, * p<0.05, + p<0.1

APPENDICES

APPENDIX A

Chapter I supporting material

A.0.1 Procedure for Gathering Data on UCC Locations and Dates of Operation

Historical data on UCC locations must be gathered from a variety of sources because no centralized database exists. The primary challenge cited by multiple data vendors such as Merchant Medicine, is that UCCs are owned by a wide array of small and medium sized businesses. While there are some national UCC chains such as American Family Care and Concentra, physician and hospital owned businesses typically operate less than five facilities and account for a substantial fraction of the of the market. For example, in Massachusetts in 2015 there were 125 UCCs and American Family Care owned 15, more than any other company. By contrast, 58 retail clinics operated during the same year and CVS owned all of them.

I draw on a wide array of public and private sources to create a comprehensive database of UCCs. My search included all UCCs in Massachusetts and UCCs within 20 miles of the Massachusetts border. I start with a dataset created by *Massachusetts Health Policy Commission* (2018), which collected information on all UCCs that operated in Massachusetts in 2018. I then add UCCs identified via a search of historical business listings from ReferenceUSA that included the words "urgent", "walk-in", or "walk in" in the company name. I also add records from the Urgent Care Association's 2017 annual census of UCCs (previous years were not available). Finally, I include facilities in the National Plan and Provider Enumeration System that indicated their specialty was urgent care.

For each UCC, I verified the address, opening date, closure date (if applicable), and the National Provider Identifier (NPI). Each of the above sources reported the address. I manually searched for press releases that indicated the opening and closure dates on company websites, Facebook, and Google. I also manually searched for each UCC's National Provider Identifier (NPI) using the National Plan and Provider Enumeration System. The manual search identified NPIs even in cases where records have discrepancies in the business name or address.

The search identified 75 Massachusetts UCCs in 2012, and 134 in 2015. It also identified 105 UCCs within 20 miles of the Massachusetts border and 32 UCCs within 5 miles. I obtained the exact month of opening and closure for 98% of UCCs. The remaining UCCs are excluded from the analysis. I obtained a relevant NPI for 70% of UCCs. I do not use the NPI of the other UCCs because they billed using the identifier for their owner, which was a hospital, community health center, or larger physician group that offered wider array of services. When available, the NPI makes it easier to identify UCC visits in the insurance claims, but UCCs without an NPI are still included in the analysis.

A.0.2 Procedure for Classifying the Type of Visit

I categorize visits into four types based on the provider visited: urgent care centers (UCCs), emergency departments (EDs), inpatient hospital facilities, and other outpatient facilities. If a claim is associated with a National Provider Identifier from the UCC database I constructed, or if the site of service code shows that the visit took place at a UCC, then I categorize the claim as a UCC visit. If I observe an ED revenue code, then I categorize the claim as an ED visit. I follow the guidance of the Massachusetts government and identify inpatient claims based on the type of bill, several admission variables, and the discharge date. I assume that all claims with the same type of provider, date of service, and patient ID code correspond to a single visit. In this way I avoid double counting visits from providers that submit multiple claims (for example the majority of ED visits generate a facility claim and professional claim). If I observe medical claim on a date of service that is not associated with a UCC, ED, or inpatient visit, I categorize it as an other outpatient visit.

My classification may undercount visits to UCCs because not all UCCs have a distinct NPI.[1] This issue should be minimized because I also classify UCC visits based on the site of service claim variable. However, estimates of the effect of UCC openings on UCC visits may be attenuated. The classification of inpatient claims and ED claims was validated via discussions with the Center for Health and Information Analysis (CHIA), which maintains the APCD. CHIA determined the set of variables used to code inpatient claims based on a comparison of APCD claims and data from hospital case mix files. It also provided a count of ED visits from the case mix files, and my count of visits based on revenue codes was within 1%.

I construct two types of spending variables. The total payment by the insurer sums the amount paid after the visit and the prepaid amount. The total charge to the patient sums the copayment amount, the deductible amount, and the coinsurance amount. I use the same procedure described above to categorize insurer and patient spending by type of visit.

[1] As described in the previous section, some UCCs bill under the NPI of the owner, which may be a hospital or another type of provider that offers a wide range of services. I only identify UCCs based on NPI if they bill using an NPI that primarily corresponds to urgent care services.

A.0.3 Robustness to Heterogeneous Treatment Effects

Recent advances in econometrics show that heterogeneous treatment effects may bias estimates from staggered difference-in-difference models (*Borusyak and Jaravel*, 2017; *de Chaisemartin and D'Haultfœuille*, 2020; *Goodman-Bacon*, 2018; *Sun and Abraham*, 2020). Such problems arise if the magnitude of the treatment effect is correlated with the timing of treatment. Strategic entry by UCCs could potentially create this problematic pattern. For example, if UCCs open first in areas with greater demand, then the treatment effects for zip codes where UCCs opened in 2012 could be larger than the treatment effects for zip codes where UCCs opened in 2015.

I evaluate whether the results are robust to heterogeneous treatment effects using an estimator developed by *Sun and Abraham* (2020). Following the literature, I categorize zip codes into cohorts based on the timing of treatment. I define each cohort based on the year that the first UCC opened within five miles (2012, 2013, 2014, or 2015). I then estimate Equation A.1, a modified event study regression that allows the treatment effect to vary for each cohort, c. For each time period relative to treatment date, I report the average cohort treatment effect by taking the average across cohorts (Equation A.2). I calculate the average cohort treatment effects using Stata's postestimation command *lincom*.

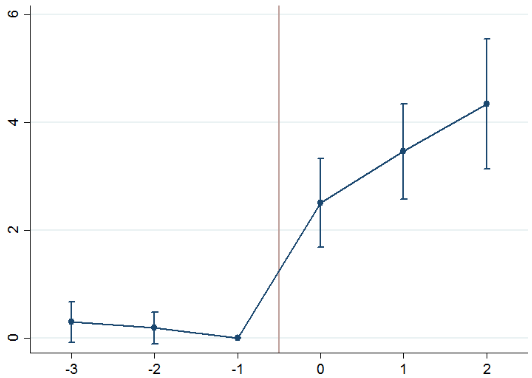
$$Y_{zt} = \alpha_z + \tau_t + \sum_c \mathbb{1}(t_z^\star = c) * \sum_e \beta_{ce} * \mathbb{1}(t - t_z^\star = e) + \gamma X_{zt} + \epsilon_{zt} \tag{A.1}$$

$$\beta_e = \frac{1}{|C|} \sum_c \beta_{ce} \tag{A.2}$$

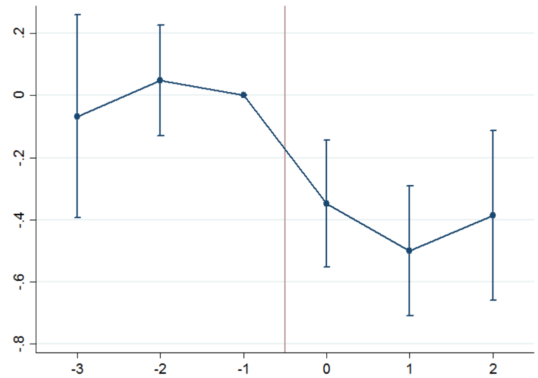
Figure A.1 graphs the event study of the average cohort treatment effects. The results are very similar to the traditional event study estimates in the main text. They show an increase in UCC visits, and declines in ED visits and other outpatient visits. The magnitudes are also similar to the event study estimates from the main text. The estimates for the total number of visits are not statistically significant. For each outcome, there is no evidence of differential pretrends.

Overall, the estimates based on *Sun and Abraham* (2020) suggest that the main results are robust to heterogeneity in treatment effects over time. This is broadly sensible since the sample period only spans four years.

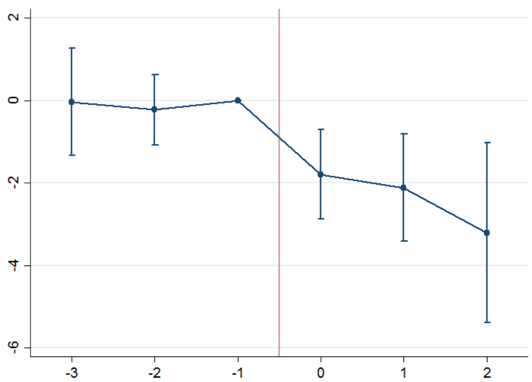
Figure A.1: Event Studies for Visits for Primary Care Treatable Conditions based on *Sun and Abraham* (2020)



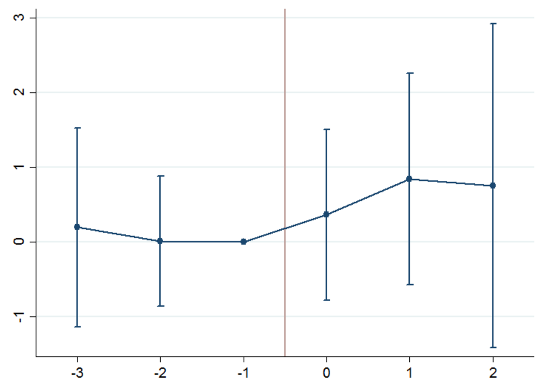
(a) UCC



(b) ED



(c) Other Outpatient

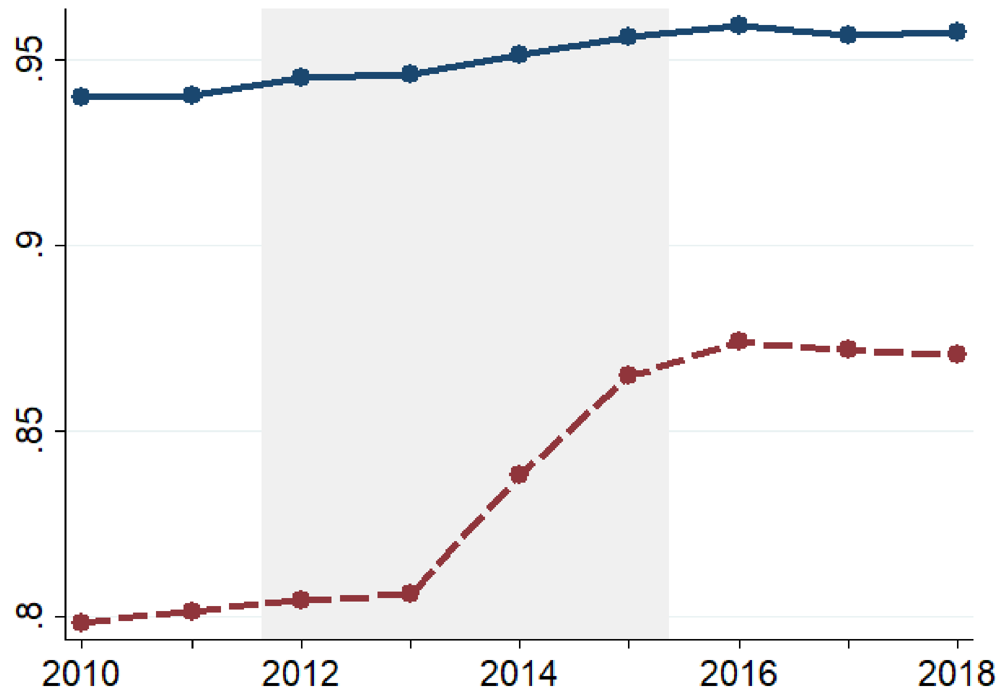


(d) Total

Note: The figure shows the effect of UCC entry on visits to different types of providers using zip code level data. Visits measure the number of monthly visits by privately insured people under 65 per thousand. The event study is estimated using Equations A.1 and A.2, which were adapted from *Sun and Abraham* (2020). The x-axis denotes the number of years relative to UCC entry. The red vertical line is placed immediately before the year that the UCC opened. The difference between control and treatment zip codes is normalized to zero for the year immediately before the UCC opened.

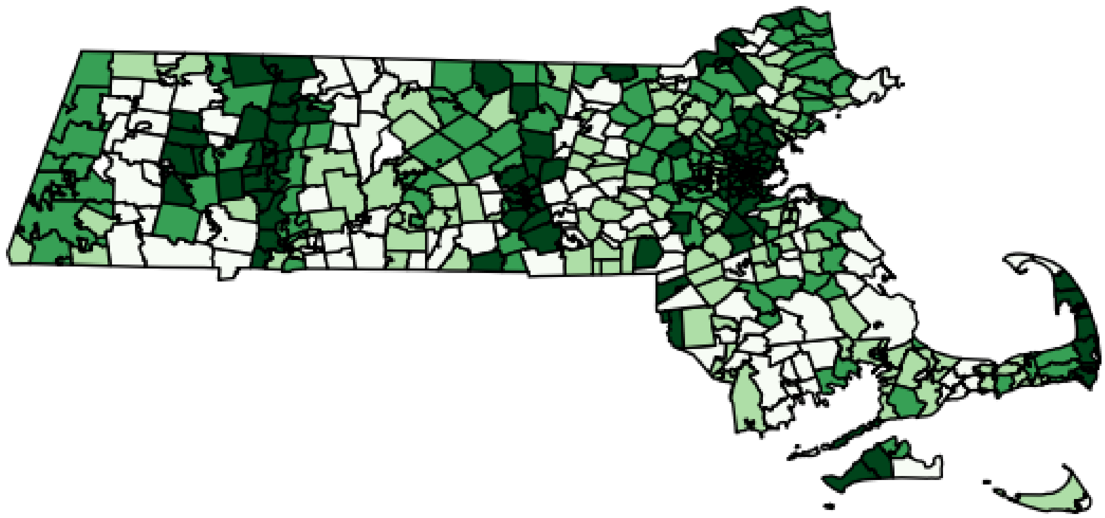
A.0.4 Additional Tables and Figures Referenced in Main Text

Figure A.2: Share of Population under 65 with Private Insurance or Medicaid in Massachusetts and US



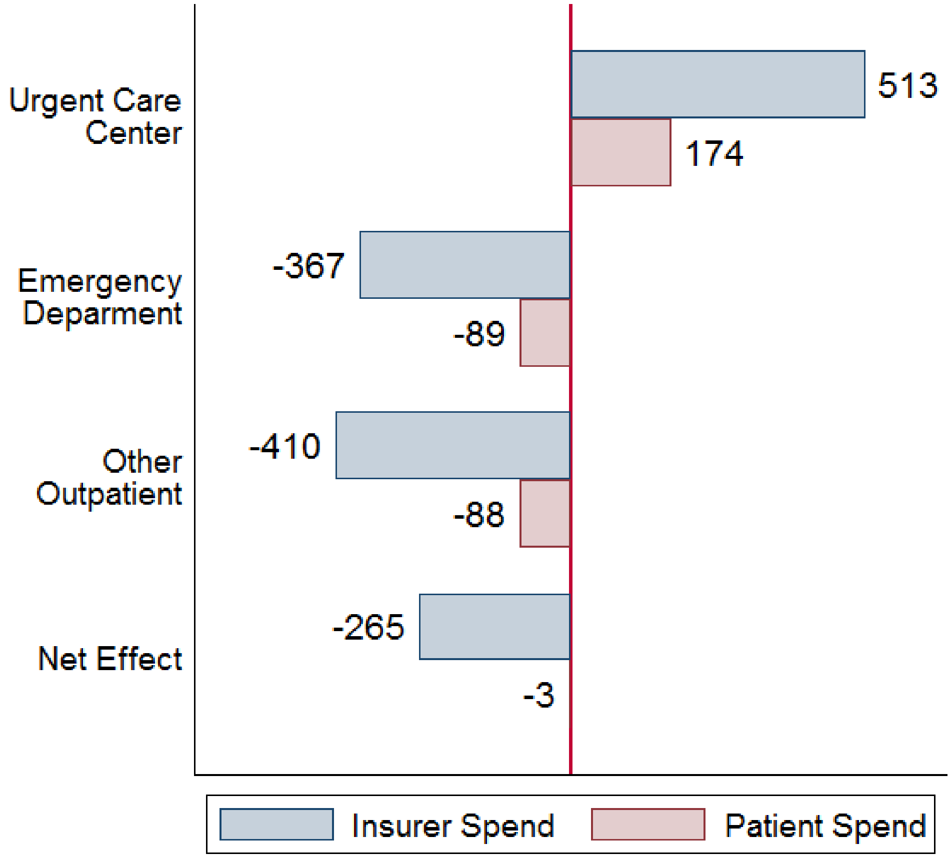
Note: Blue solid line shows Massachusetts. Red dashed line shows the United States. Grey shaded area corresponds to 2012-2015, the years included in the analysis.

Figure A.3: Number of Primary Care Physicians per Capita



Note: Zip codes categorized by quartile, with darker colors representing more primary care physicians per capita. Based on author calculations from the 2011 National Plan and Provider Enumeration System.

Figure A.4: Back-of-the-Envelope Estimates for Spending in Zip Codes within 2 miles of a UCC



Note: Back-of-the-envelope estimates are calculated by applying the average price per visit displayed in Figure 1.2 to the estimated change in the number of visits displayed in Specification 3 of Table 1.4.

Table A.1: Average Spending for Emergent Conditions by Location of Visit

Condition Category	Visit Location	Insurer Spend	Patient Spend
Primary Care Treatable	Urgent Care Center	141	48
	Emergency Dept	631	153
	Other Outpatient	184	39
	All	211	47
Emergent	Urgent Care Center	291	71
	Emergency Dept	2,094	168
	Other Outpatient	1,025	67
	All	2,059	102

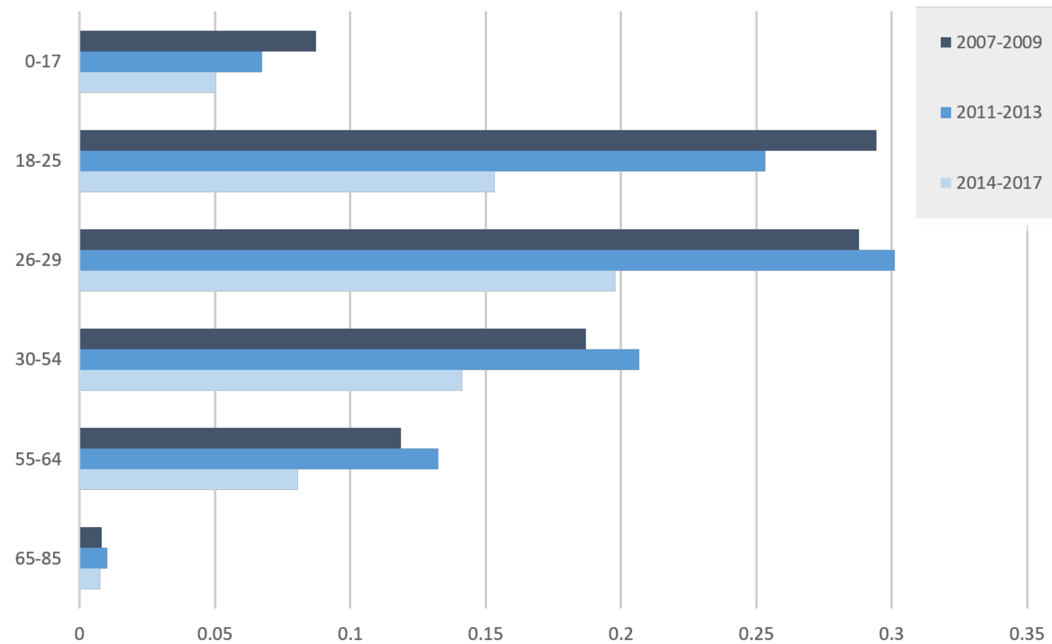
Note: The table shows average spending per visit based on author calculations from the Massachusetts All Payer Claims Database using records from 2012-15.

APPENDIX B

Chapter II supporting material

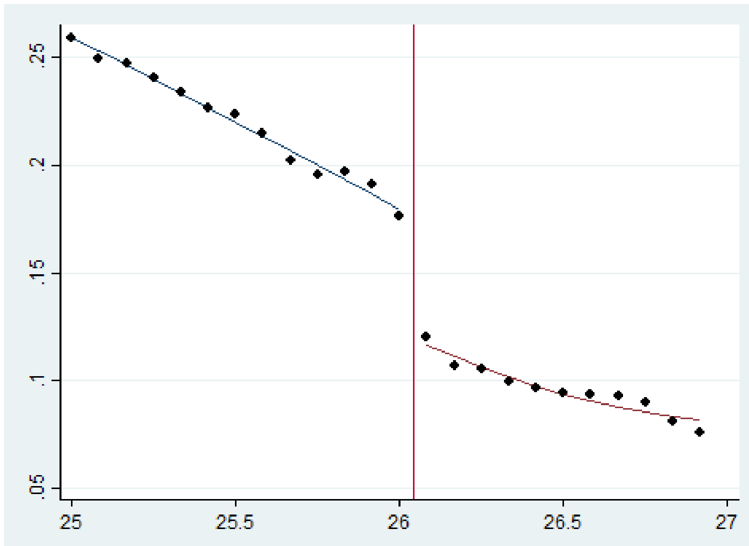
B.0.1 Figures and Tables Referenced in Main Text

Figure B.1: Proportion of Population Uninsured by Age Group, Historical Comparison

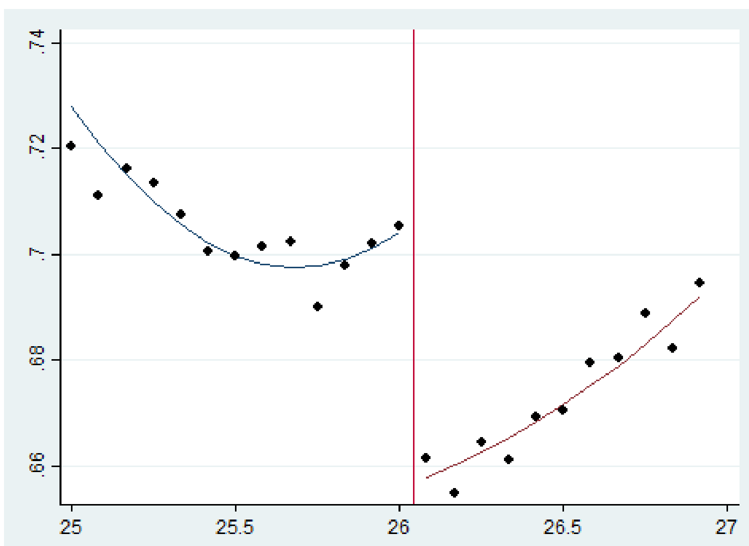


Note: Source: National Health Interview Survey. 2010 not included because it is an implementation year.

Figure B.2: RD Insurance, Healthy Subgroup



(a) Dependent Insurance



(b) Any Insurance

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

Table B.1: Demographics and Outcomes of MEPS Respondents aged 18-29, 2014-2017

Variable	Healthy	Chronic	Difference
Age	23.9	24.1	0.246***
Female	0.487	0.600	0.113***
Asian	0.092	0.064	-0.028***
Black	0.240	0.215	-0.024**
White	0.685	0.746	0.061***
Hispanic	0.377	0.257	-0.12***
Mental health condition	0.001	0.444	0.443***
Family income	59,359	54,059	-5,300***
Total health spending	1,435	4,553	3118***
Of which out of pocket	204	535	332***
Total prescription spending	124	1078	954***
Of which out of pocket	16	126	110***
Any insurance coverage	0.717	0.828	0.110***
Private insurance coverage	0.491	0.526	0.035***
Of which dependent coverage	0.258	0.298	0.040***
Of which employer coverage	0.202	0.196	-0.006
Of which exchange coverage	0.016	0.016	0
Public insurance coverage	0.248	0.337	0.089***
Employed	0.650	0.616	-0.034***
Temp job	0.084	0.076	-0.008
Multiple jobs	0.049	0.065	0.016***
Self-employed	0.025	0.022	-0.003
Hours worked if employed	34.7	33.8	-0.9**
Total individuals observed	11,926	2,745	
Proportion of respondents	0.813	0.187	

Note: Inference for difference based on standard errors clustered at respondent level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.2: Change in Probability of Insurance Coverage at 26 and 1 Month, Healthy Subgroup

	(1)	(2)	(3)	(4)
Dependent Insurance	-0.0505***	-0.0499***	-0.0571***	-0.0576***
	(0.00779)	(0.00734)	(0.00816)	(0.00766)
Observations	16,037	15,995	25,604	25,587
Mean: Age 25	0.224	0.224	0.224	0.224
Any Insurance	-0.0484***	-0.0473***	-0.0489***	-0.0478***
	(0.00937)	(0.00838)	(0.0100)	(0.00893)
Observations	16,037	15,995	25,604	25,587
Mean: Age 25	0.708	0.708	0.708	0.708
Respondent FE	No	Yes	No	Yes
Window	6 mo	6 mo	12 mo	12 mo

Note: Regression specification includes 2^{nd} degree polynomial and is estimated with triangular kernel. Standard errors clustered at respondent level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Number of Monthly Prescriptions for People Close to Age 26

	Before 26		Change Including 26		Change After 26	
	Mean	Std Err	Level	Percent	Level	Percent
All prescriptions	0.285	(0.021)	-0.040*	-13.9	-0.029	-10.2
CNS stimulants	0.014	(0.003)	-0.001	-3.7	-0.005	-33.6
Analgesics	0.040	(0.013)	-0.017	-41.6	-0.006	-15.4
Birth control	0.038	(0.004)	-0.001	-3.4	-0.010*	-26.1
Antidepressants	0.022	(0.003)	0.000	0.9	0.003	13.8
Other	0.213	(0.016)	-0.032**	-15.2	-0.015	-7.2

Note: People were categorized based on the interview reference period. For the before 26 group, the interview reference period ended between age 25.5 and 26. For the including 26 group, the interview reference period began before age 26 and ended after age 26. For the after 26 group, the interview reference period began after age 26 and before age 26.5. The average interview reference period spans 4.8 months. Based on MEPS data for 2014-2017.
*** p<0.01, ** p<0.05, * p<0.1.

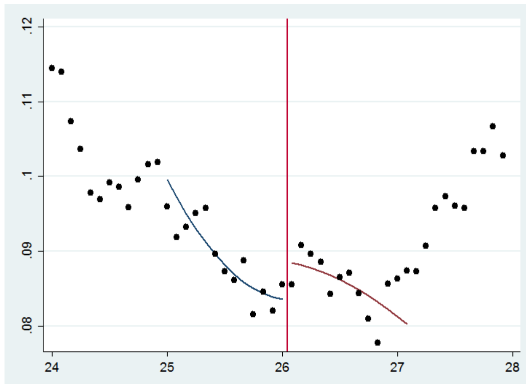
B.0.2 Placebo Tests Using Historical Data

The identification strategy employed in this paper assumes that insurance coverage at age 26 changes as a result of the dependent coverage eligibility threshold. In the analyses that follow, I conduct a placebo test with historical data. The dependent coverage provision was enacted in September of 2010. Therefore, I run the regression discontinuity on three historical periods to determine whether the emergence of the discontinuity aligns with the implementation of the policy. The placebo period spans 2007-2009. In this period, children were eligible to stay on their parent's insurance plans until their 18^{th} birthday or their 23^{rd} birthday if they were enrolled in college. I omit 2010, which was the implementation year of the dependent coverage provision. The post period is divided into two periods: 2011-2013 (prior to the majority of ACA policies) and 2014-2017 (post-ACA).

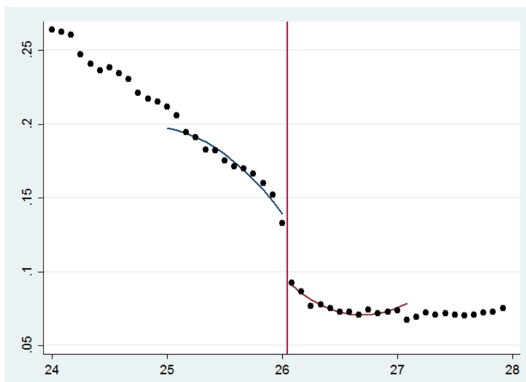
Table B.4 and Figure B.3 display the results on dependent insurance coverage using the MEPS dataset. In the placebo period, there is no discontinuity the probability of depedent coverage at 26. The emergence of the discontinuity occurs in 2011-2013, after the dependent coverage provision was implemented.

Table B.5 and Figure B.4 display the results for CNS stimulant prescriptions. For the historical analyses, the person id is not available. Therefore, I group people based on their birth cohort (birth month and year). These cohorts cross the threshold for dependent insurance eligibility at the exact same time. The outcome variable is the log number of prescriptions per month purchased by people within a given cohort. Similarly to the insurance results, in the placebo period, there is no discontinuity. The emergence of the discontinuity occurs in 2011-2013, after the dependent coverage provision was implemented.

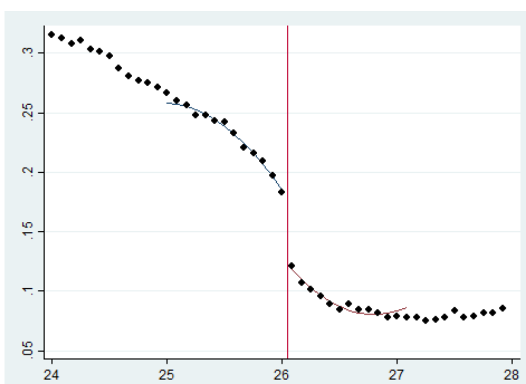
Figure B.3: RD Dependent Insurance, Historical



(a) 2007-2009



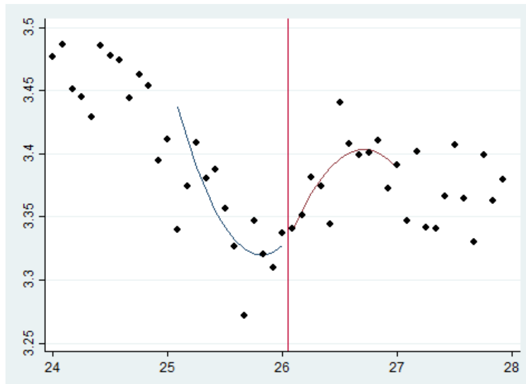
(b) 2011-2013



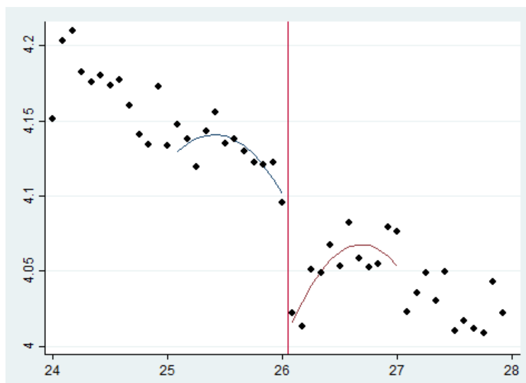
(c) 2014-2017

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

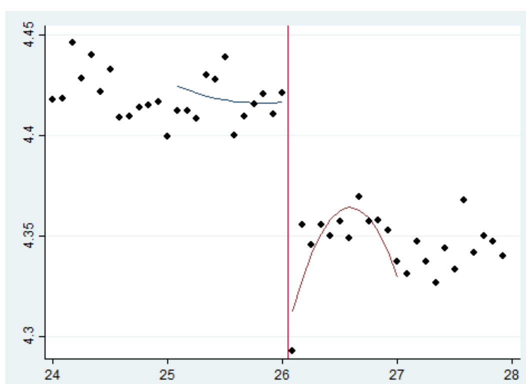
Figure B.4: RD Ln Prescriptions by Cohort, Historical



(a) 2007-2009



(b) 2011-2013



(c) 2014-2017

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

Table B.4: Change in Probability of Dependent Coverage at 26 and 1 Month, Historical Comparisons

	(1)	(2)	(3)	(4)
2007-2009	-0.000362	0.000149	0.00509	0.00169
	(0.00411)	(0.00219)	(0.00461)	(0.00264)
Observations	16,699	16,584	31,254	31,136
Mean: Age 25	0.0893	0.0893	0.0893	0.0893
2011-2013	-0.0291***	-0.0281***	-0.0414***	-0.0384***
	(0.00640)	(0.00547)	(0.00679)	(0.00588)
Observations	20,466	20,301	38,004	37,853
Mean: Age 25	0.180	0.180	0.180	0.180
2014-2017	-0.0501***	-0.0525***	-0.0558***	-0.0590***
	(0.00716)	(0.00652)	(0.00761)	(0.00694)
Observations	23,758	23,571	44,310	44,120
Mean: Age 25	0.236	0.236	0.236	0.236
Respondent FE	No	Yes	No	Yes
Window	6 mo	6 mo	12 mo	12 mo

Note: Regression specification includes 2^{nd} degree polynomial and is estimated with triangular kernel. Standard errors clustered at respondent level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: Change in Log Monthly Prescriptions by Cohort at 26 and 1 Month, Historical Comparisons

	(1)	(2)	(3)	(4)
2007-2009	-0.00148	-0.00522	-0.00138	-0.0137
	(0.0657)	(0.0403)	(0.0410)	(0.0309)
Observations	432	427	864	859
Mean: Age 25	29.58	29.58	29.58	29.58
2011-2013	-0.0782*	-0.0506	-0.0867***	-0.0731**
	(0.0437)	(0.0390)	(0.0308)	(0.0290)
Observations	432	427	864	859
Mean: Age 25	63.38	63.38	63.38	63.38
2014-2017	-0.141***	-0.151***	-0.113***	-0.117***
	(0.0311)	(0.0230)	(0.0216)	(0.0167)
Observations	576	571	1,152	1,147
Mean: Age 25	83.88	83.88	83.88	83.88
Respondent FE	No	Yes	No	Yes
Window	6 mo	6 mo	12 mo	12 mo

Note: Dependent variable defined at cohort level (group of people with same birth month and birth year). Regression specification includes 2^{nd} degree polynomial and is estimated with triangular kernel. Standard errors clustered at respondent level. *** p<0.01, ** p<0.05, * p<0.1.

B.0.3 Regressions Excluding Months Close to the Threshold

Because CNS stimulants are DEA Schedule II drugs, it is difficult to stockpile them. Prescriptions cannot be refilled, and in 99% of cases they contain a 31 day supply or less. Physicians commonly write multiple prescriptions for their patients, but each prescription cannot be filled until the date noted by the physician.

Despite these safeguards it is possible that patients may attempt to stockpile CNS prescriptions. In this section I empirically test whether the results differ if the months immediately before and after the threshold for dependent coverage are excluded. I show donut RD specifications that drop one or two months on both sides of the threshold. Because I exclude these months, I only use a window of 1 year. Given the restrictions on filling prescriptions, it seems unlikely that patients can stockpile for more than 1-2 months.

Table B.6 shows the results for overall utilization and Table B.7 shows the results for purchases by insurance status. The results broadly align with the estimates presented in the main text, but the magnitudes tend to be smaller. The smaller magnitudes may reflect non-linearities in the probability of insurance coverage at age 26 and 1 month or they could reflect limited stockpiling. In the donut RD estimates, the probability of purchasing a prescription falls by 2-3 percentage points (compared to 3 percentage points in the main specifications). Spending falls by \$3-5 (compared to \$6-8 in the main specifications). Similarly, the magnitudes estimated for purchases by insurance status are 30-50% smaller relative the effects reported in the main specifications. The results by type of medication suggest that branded purchases decline the most.

B.0.4 Employment Outcomes at Age 26

Employers are the primary source of insurance coverage in the United States. Additionally, young adults who lose insurance coverage may need more income to cover medical costs. These considerations may lead young adults to seek additional employment opportunities at the threshold for dependent coverage. I use the MEPS employment data, which measures employment status at the time of the MEPS interviews, to investigate whether labor outcomes change at the threshold for dependent coverage. These outcomes are only collected during the five interview dates (not monthly). As a result, the graphs are noisier than the insurance data and it is not appropriate to use the bandwidth of 6 months because the sample size is substantially smaller than the insurance data.

Across the four labor outcomes considered, none of the point estimates are significant at the 5% level. I do not find any evidence that labor outcomes increase discontinuously at age 26. The findings are reasonable given that searching for a job can take multiple months and young adults know in advance that they will lose dependent coverage at age 26. People who search for a job because they expect to lose insurance likely start their search in the months leading up to the threshold.

Table B.6: Overall Changes in Utilization of CNS Stimulants at 26 and 1 month, Alternate Specifications

	(1)	(2)	(3)	(4)	(5)
Pr Buy	-0.028***	-0.017***	-0.017***	-0.021***	-0.027***
	(0.0032)	(0.0036)	(0.0049)	(0.0048)	(0.0078)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.229	0.229	0.229	0.229	0.229
Spend	-6.07***	-3.71***	-3.13***	-4.55***	-4.43***
	(0.707)	(0.738)	(1.004)	(1.011)	(1.627)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	35.32	35.32	35.32	35.32	35.32
Months Excluded	0	1	1	2	2
Window	12 mo	9 mo	12 mo	9 mo	12 mo
Polynomial order	2	1	2	1	2

Note: Regression specifications include 2^{nd} degree polynomial and are estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.7: Changes in Use by Insurance at 26 and 1 month, Alternate Specifications

	(1)	(2)	(3)	(4)	(5)
Pr Buy w/ Pvt Ins	-0.040***	-0.025***	-0.027***	-0.027***	-0.032***
	(0.0032)	(0.0036)	(0.0049)	(0.0048)	(0.0076)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.205	0.205	0.205	0.205	0.205
Pr Buy w/ Pub Ins	0.000	0.001	0.000	0.001	0.001
	(0.0010)	(0.0012)	(0.0016)	(0.0017)	(0.0027)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.015	0.015	0.015	0.015	0.015
Pr Buy Out-of-Pocket	0.012***	0.008***	0.009***	0.005***	0.003
	(0.0013)	(0.0013)	(0.0018)	(0.0016)	(0.0025)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.008	0.008	0.008	0.008	0.008
Spend w/ Pvt Ins	-7.57***	-5.00***	-4.81***	-5.23***	-5.14***
	(0.712)	(0.734)	(0.987)	(0.997)	(1.592)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	32.23	32.23	32.23	32.23	32.23
Spend w/ Pub Ins	-0.00	0.28	0.32	0.25	0.27
	(0.172)	(0.210)	(0.276)	(0.281)	(0.446)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	1.94	1.94	1.94	1.94	1.94
Spend Out-of-Pocket	1.42***	0.86***	1.12***	0.30	0.13
	(0.210)	(0.217)	(0.315)	(0.251)	(0.393)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.85	0.85	0.85	0.85	0.85
Months Excluded	0	1	1	2	2
Window	12 mo	9 mo	12 mo	9 mo	12 mo
Polynomial order	2	1	2	1	2

Note: Regression specification includes 2^{nd} degree polynomial and is estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.8: Changes in Use by Medication at 26 and 1 month, Alternate Specifications

	(1)	(2)	(3)	(4)	(5)
Pr Buy Generic IR	-0.012***	-0.006**	-0.009**	-0.007*	-0.013**
	(0.0022)	(0.0027)	(0.0037)	(0.0036)	(0.0057)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.104	0.104	0.104	0.104	0.104
Pr Buy Generic XR	-0.006***	-0.003	-0.002	-0.005	-0.004
	(0.0020)	(0.0022)	(0.0030)	(0.0029)	(0.0047)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.062	0.062	0.062	0.062	0.062
Pr Buy Branded	-0.014***	-0.010***	-0.011***	-0.010***	-0.010**
	(0.0021)	(0.0025)	(0.0032)	(0.0033)	(0.0051)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	0.086	0.086	0.086	0.086	0.086
Spend on Generic IR	-0.56***	-0.20*	-0.29*	-0.15	-0.25
	(0.111)	(0.121)	(0.167)	(0.158)	(0.255)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	4.17	4.17	4.17	4.17	4.17
Spend on Generic XR	-1.07***	-0.47	-0.01	-0.77*	-0.20
	(0.330)	(0.354)	(0.498)	(0.456)	(0.727)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	8.96	8.96	8.96	8.96	8.96
Spend on Branded	-4.44***	-3.03***	-2.83***	-3.62***	-3.98***
	(0.644)	(0.688)	(0.914)	(0.950)	(1.503)
Observations	252,876	180,998	229,559	158,305	206,866
Mean: Age 25	22.19	22.19	22.19	22.19	22.19
Months Excluded	0	1	1	2	2
Window	12 mo	9 mo	12 mo	9 mo	12 mo
Polynomial order	2	1	2	1	2

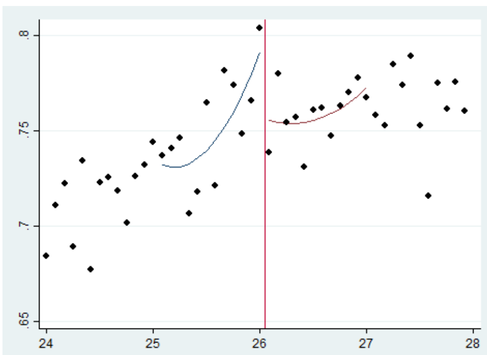
Note: Regression specifications include 2^{nd} degree polynomial and are estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9: Changes in the Type of Medication Purchased by Branded-Type Consumers at 26 and 1 month, Alternate Specifications

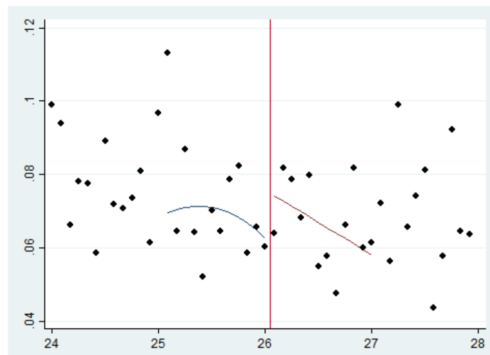
	(1)	(2)	(3)	(4)	(5)
Generic IR	0.009	0.027**	0.012	0.042**	0.030
	(0.0111)	(0.0138)	(0.0186)	(0.0174)	(0.0274)
Observations	19,140	13,860	17,384	12,104	15,628
Mean: Age 25	0.131	0.131	0.131	0.131	0.131
Generic XR	0.030***	0.025**	0.017	0.032**	0.026
	(0.0102)	(0.0121)	(0.0153)	(0.0157)	(0.0235)
Observations	19,140	13,860	17,384	12,104	15,628
Mean: Age 25	0.072	0.072	0.072	0.072	0.072
Branded	-0.045***	-0.059***	-0.048***	-0.061***	-0.042
	(0.0122)	(0.0140)	(0.0183)	(0.0183)	(0.0277)
Observations	19,140	13,860	17,384	12,104	15,628
Mean: Age 25	0.892	0.892	0.892	0.892	0.892

Note: Consumers are considered "Branded-Type" if the plurality of their purchases between ages 21 and 26 were branded prescriptions. Coefficients do not sum to 1 because consumers may buy multiple types of medication each month. Regression specifications include 2^{nd} degree polynomial and are estimated with triangular kernel. Standard errors clustered at person level. *** p<0.01, ** p<0.05, * p<0.1.

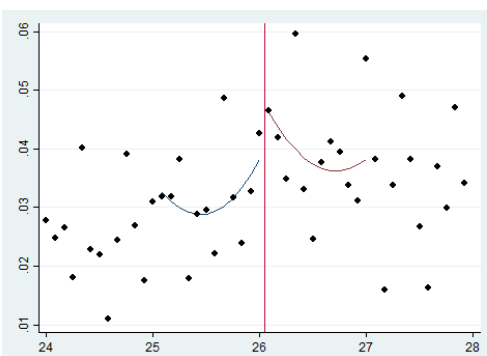
Figure B.5: RD Labor Market Outcomes



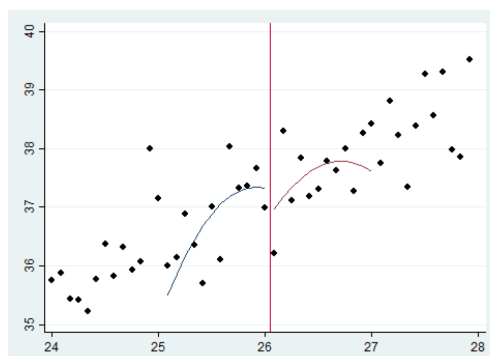
(a) Employed



(b) Temp Job



(c) Self-Employed



(d) Number of Hours
Conditional on Employed

Note: Red vertical line indicates eligibility for dependent coverage, which expires the month after an individual's 26^{th} birthday. Polynomial lines represent estimates from a local regression discontinuity estimate that includes a second degree polynomial and triangular kernel.

Table B.10: Change in Labor Outcomes at 26 and 1 Month, 2014-2017

OUTCOMES	(1)	(2)	(3)	(4)
Employed	-0.0418	-0.0225	-0.0436*	-0.0266
	(0.0273)	(0.0171)	(0.0243)	(0.0167)
Observations	8,906	8,006	8,906	8,006
Mean: Age 25	0.746	0.746	0.746	0.746
Temp Job	0.0134	-0.00168	0.00653	-0.000866
	(0.0165)	(0.0104)	(0.0150)	(0.01000)
Observations	8,830	7,930	8,830	7,930
Mean: Age 25	0.0746	0.0746	0.0746	0.0746
Self-Employed	0.00783	0.00712	0.0105	0.00690
	(0.0133)	(0.00653)	(0.0118)	(0.00617)
Observations	8,889	7,988	8,889	7,988
Mean: Age 25	0.0307	0.0307	0.0307	0.0307
Hours Worked	0.0134	-0.00168	0.00653	-0.000866
	(0.0165)	(0.0104)	(0.0150)	(0.01000)
Observations	8,830	7,930	8,830	7,930
Mean: Age 25	0.0746	0.0746	0.0746	0.0746
Kernel	Triangular	Triangular	Uniform	Uniform
Respondent FE	No	Yes	No	Yes

Note: Regression specification includes 2^{nd} degree polynomial. Bandwidth of 12 months is used. Bandwidth of 6 months not included because of smaller sample size relative to insurace data. Standard errors clustered at respondent level. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX C

Chapter III supporting material

Table C.1: Demographic Statistics for Indiana and Kentucky

	Indiana	**Kentucky**
Total population	6514516	4364627
DEMOGRAPHIC CHARACTERISTICS		
Male	49.2%	49.2%
Median age (years)	37.1	38.2
65 years and over	13.2%	13.6%
married (15 years and older)	50.1%	50.3%
Race/Ethnicity		
single race	97.9%	98.2%
white	86.5%	89.6%
black	9.2%	9.0%
asian	1.7%	1.2%
hispanic	6.2%	3.1%
EDUCATION (25 years and older)		
Less than High School	12.7%	17.1%
High School Degree	35.5%	34.1%
College Degree or More	23.1%	21.2%
veteran (18 years and older)	9.4%	9.5%
INCOME, POVERTY, HEALTH INSURANCE		
Per capita income (dollars)	$ 24,048	$ 22,796
Percent in Poverty		
under 18 years	22.4%	26.8%
18 to 64	14.6%	17.9%
65 and over	7.1%	11.8%
Health insurance coverage (civilian noninstitutionalized pop.)		
private health insurance	68.1%	64.3%
public coverage	28.9%	33.0%
uninsured	14.4%	14.5%
EMPLOYMENT STATUS (16 years and over)		
in labor force	64.2%	60.0%
employed	57.8%	53.4%
unemployed	6.3%	6.1%
INDUSTRY (civilian employed, 16 years and over)		
Goods producing	25.8%	22.3%
Wholesale, Retail	14.2%	14.5%
Other Services, Private	56.5%	58.5%
Public Administration	3.6%	4.6%
OCCUPATION (employed, 16 years and over)		
Management, professional	31.9%	32.5%
Service occupations	17.2%	16.9%
Sales and office occupations	24.0%	24.3%
Natural resources, construction, and maintenance	8.8%	10.0%
Production, transportation, and material moving	18.1%	16.4%

Note: Source: U.S. Census Bureau, 2010-2012 American Community Survey, accessed via American FactFinder factfinder.census.gov

Table C.2: Comparison of Baseline (Quarterly) Regressions to 2-period Regressions

VARIABLES	(1) Any Prescrip	(2) Ln Patients	(3) Ln Days/Pat	(4) Ln MED/Day
		Baseline (Quarterly) Regressions in Logs		
KYpost		-0.177**	-0.0409**	-0.0343**
		(0.00913)	(0.00536)	(0.00513)
Observations		215,409	215,328	215,328
No. of providers		36,308	36,294	36,294
		2-Period Regressions in Logs		
KYPost		-0.216**	-0.0398**	-0.0398**
		(0.0119)	(0.00651)	(0.00668)
Observations		62,835	62,812	62,812
No. of providers		35,897	35,882	35,882
		Baseline (Quarterly) Regressions in Levels		
KYPost	-0.0377**	-6.466**	-0.0334	-0.418*
	(0.00379)	(0.607)	(0.113)	(0.187)
Observations	290,464	215,409	215,328	215,328
No. of providers	36,308	36,308	36,294	36,294
		2-Period Regressions in Levels		
KYPost	-0.0246**	-6.076**	-0.0366	-0.392
	(0.00520)	(0.593)	(0.124)	(0.244)
Observations	72,6165	62,835	62,812	62,812
No. of providers	36,308	35,897	35,882	35,882

Note: Table reports difference-in-difference coefficients from estimation of Equation 3.1. Outcomes in second, third, and fourth column are conditional on the provider having any prescribing in the quarter. Baseline regressions are run on a panel of quarterly outcomes. 2-Period regressions are run on a panel of two outcomes (pre and post) for each provider. Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table C.3: Coefficients from Difference in Differences Regression Including Only Pairs of Adjacent Quarters. (Implementation Period Bolded.)

Time Period	Any	Patients	Days/Patient	MEDs/Day
2012Q1-2012Q2	0.00675+	0.0503**	0.00172	-0.00326
	(1.750)	(6.543)	(0.291)	(-0.530)
2012Q2-2012Q3	**-0.0216****	**-0.130****	**-0.0274****	**-0.0336****
	(-5.279)	**(-16.28)**	**(-4.583)**	**(-5.620)**
2012Q3-2012Q4	**-0.0198****	**-0.117****	**-0.0186****	**0.00534**
	(-4.897)	**(-13.92)**	**(-3.078)**	**(0.841)**
2012Q4-2013Q1	0.00323	0.0397**	0.000220	0.0134*
	(0.815)	(5.154)	(0.0381)	(2.070)
2013Q1-2013Q2	-0.00660+	0.00170	-0.00129	-0.0100
	(-1.666)	(0.216)	(-0.221)	(-1.593)
2013Q2-2013Q3	0.00398	0.0115	-0.000757	0.00834
	(0.959)	(1.449)	(-0.133)	(1.369)
2013Q3-2013Q4	0.000927	-0.0167*	0.0236**	0.00181
	(0.230)	(-2.080)	(4.082)	(0.289)

Note: Each coefficient corresponds to one regression. T-stats in parentheses. Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table C.4: Difference in Differences Regressions by Provider Quartile Using Levels

	(1)	(2)	(3)	(4)	(5)
Sample	All	Quartile 1	Quartile 2	Quartile 3	Quartile 4
			Patients, Conditional on Any		
KYPost	-6.466**	0.0169	-1.059**	-4.548**	-13.00**
	(0.607)	(0.754)	(0.355)	(0.489)	(1.587)
Observations	215,409	24,264	51,585	68,426	71,134
No. of providers	36,308	9,087	9,067	9,077	9,077
Treatment Effect (%)	-9.7	1.3	16.8	-11.3	-8.4
			Days/Patient		
KYPost	-0.0334	0.0662	-0.280	-0.157	0.274
	(0.113)	(0.613)	(0.224)	(0.180)	(0.196)
Observations	215,328	24,217	51,559	68,421	71,131
No. of providers	36,294	9,073	9,067	9,077	9,077
Treatment Effect (%)	-0.2	1.2	-2.7	-1.2	0.8
			MEDs/Day		
KYPost	-0.418*	-4.416**	-0.742+	-0.338	0.0938
	(0.187)	(1.049)	(0.422)	(0.316)	(0.270)
Observations	215,328	24,217	51,559	68,421	71,131
No. of providers	36,294	9,073	9,067	9,077	9,077
Treatment Effect (%)	-1.2	-20.9	-2.4	-1.0	0.2

Note: Table reports difference-in-difference coefficients from estimation of Equation 3.1, but with level outcomes instead of logged outcomes. A panel of quarterly provider-level measures 2012q1 to 2013q4 by quartile of provider MED in 2012h1 is used. Outcomes in each panel are conditional on the provider having any prescribing in the quarter.
Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table C.5: Percent of Providers That Don't Prescribe in 2012q4-2013q4 Among Providers who Wrote a Prescription in 2012h1

	IN Mean	KY Mean	Difference
All	0.067	0.088	0.0213**
Quartile 1	0.296	0.372	0.076**
Quartile 2	0.124	0.157	0.033**
Quartile 3	0.025	0.031	0.006+
Quartile 4	0.007	0.010	0.003

Note: This table reports the share of providers prescribing in 2012h1 who did not not prescribe at all in the post-period (2012q4-2013q4), respectively for Indiana and Kentucky. The third column reports the difference, where asterisks signify the mean differs from zero at ** $p<0.01$, * $p<0.05$, + $p<0.1$.

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