8-2018

The Efficacy and Secondary Effects of Pharmaceutical Legislation

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THE EFFICACY AND SECONDARY EFFECTS OF PHARMACEUTICAL LEGISLATION

A Dissertation

Submitted to the Faculty

of

Purdue University

by

Justine A. Mallatt

In Partial Fulfillment of the

Requirements for the Degree

of

Doctor of Philosophy

August 2018

Purdue University

West Lafayette, Indiana

THE PURDUE UNIVERSITY GRADUATE SCHOOL
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In dedication to Jon Mallatt and Marisa de los Santos.

ACKNOWLEDGMENTS

The author would like to recognize Dr. Jillian Carr, Dr. Kevin Mumford, and Dr. Tim Bond for their advice and valuable guidance throughout her graduate career at Purdue. The author would also like to thank Dr. Kendall Kennedy, Andrew Compton, Dr. Jackie Kambič, Svetlana Beilfuss, Clint Harris, Dr. Michael Pesko, Dr. Rhet Smith, Dr. Michael Makowsky, Dr. Rosalie Liccardo Pacula, Dr. Daniel Grossman, Dr. Anita Mukherjee, and Dr. Jon Mallatt for their feedback on my papers. A special thanks to Marisa de los Santos for her medical experience and Sam Quinones for his book *Dreamland*, which together inspired the paper covered in the first chapter of my dissertation. This work uses data from the National Association for Public Health Statistics and Information Systems (NAPHSIS), The University of Chicago Booth School of Business Kilts Center for Marketing, and the Center for Disease Control's Wonder Data Query System.

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ABSTRACT

Mallatt, Justine A. PhD, Purdue University, August 2018. The Efficacy and Secondary Effects of Pharmaceutical Legislation. Major Professor: Jillian B. Carr and Kevin J. Mumford.

This dissertation examines the effects of pharmaceutical policies on various behavioral, health, and economic outcomes.

The first chapter is *The Effect of Prescription Drug Monitoring Programs on Opioid Prescriptions and Heroin Crime Rates*. In response to growing abuse of prescription opioid painkillers, 50 U.S. states have implemented electronic prescription drug monitoring programs (PDMPs) that record patients into a state-wide registry when a prescription opioid is received. This paper uses a difference-in-differences regression framework and interactive fixed effects factor models to identify the effect of PDMPs and two related programs on the types and strengths of opioid painkiller prescriptions filled and on rates of heroin crimes. This paper is the first to identify differing policy effects on opioid prescriptions by dosage of pill, and the first to find a large and significant link between PDMPs without usage mandates and heroin outcomes. The implementation of PDMP databases caused an 8% decrease in the amount of oxycodone shipments, with results from Medicaid prescription data pointing to larger decreases within high dosage pills. PDMPs have heterogeneous effects on heroin crime incidents across counties depending on the county's pre-policy level of prescription opioid milligrams per capita, with an 87% increase in heroin crime within the most opioid-dense counties. I find that non-Mandated PDMPs decrease access to high-dose oxycodone pills and cause an increase in heroin crime within the most opioid-dense counties.

The second chapter details a paper entitled *The Effect of Pharmacist Refusal Clauses on Contraception, Sexually Transmitted Diseases and Birthrates*. Emergency contraceptive drugs like Plan B are controversial, and there have been cases within at least 25 states of pharmacists refusing to provide the drug to patients. In response to pressure from activist groups on both sides of the debate, some states passed "Expand" laws which expand access to emergency contraception and protect patients' rights to receive prescribed drugs regardless of pharmacists' personal beliefs. Other states passed "Restrict" laws that restrict access to emergency contraception and favor pharmacists' rights of refusal. This paper emphasizes substitution behavior among contraception spurred by the policies, and is the first study to examine the effects of pharmacist refusal clauses on contraceptive outcomes, rates of sexually transmitted infections, and birthrates. I find that the laws cause a 12-26% increase in the prescibing rate of regular birth control pills purchased through Medicaid, and cause decreases in purchases of condoms as well as over-the-counter Plan B. There is not evidence that the policies have effects on rates of sexually transmitted diseases, however the states that pass the Restrict policy (favoring pharmacists' rights of refusal) realize a statistically significant and robust 1.16% decrease in the birthrate among black mothers. While I am not able to measure the effect of the policies on actual rates of pharmacist refusal, my findings suggest that thousands of cautious women change their behavior in response to the policies by adopting the birth control pill.

The third and final chapter is comprised of the paper *The Effect of Opioid Supply-Side Interventions on Opioid-Related Business Establishments*. In response to climbing opioid abuse and overdoses, states passed several types of programs that target the supply side of the prescription opioid market, including Prescription Drug Monitoring Programs (PDMPs) which track patient histories, mandates that doctors use the programs, "Pill Mill Bills" that target over-prescribing offices, and abuse-deterrent versions of prescription opioids. This paper is the first to investigate the effects of these policies on opioid-related business establishment counts nationwide, and examines how the policies affect rehabilitation facilities, doctors' offices and clinics, and

pharmacies. I find that Pill Mill crackdowns reduce the number of establishments in a widely-defined category which includes pain management clinics. States that implement the Pill Mill Bills notice a statistically significant 6-7% reduction in the rate of clinics per capita in this category. The Pill Mill Bills are associated with fewer pharmacies, a 2.6% decrease, but this result is only statistically significant within counties that receive a high concentration of opioids. “Must Access” mandates are associated with a 1.5-2.5% rise in the rate of residential rehabilitation establishments. The policies are not found to significantly affect inpatient rehabilitation hospitals, outpatient rehabilitation clinics, doctors’ offices, medical labs, or drug wholesalers. While the effect of opioid policies on patient and physician behavior has been well-investigated, this paper provides evidence that policies have spillover effects on medical business establishments.

1. THE EFFECT OF PRESCRIPTION DRUG MONITORING PROGRAMS ON OPIOID PRESCRIPTIONS AND HEROIN CRIME RATES

1.1 Introduction

The United States is in the midst of an opioid drug epidemic, which the Center for Disease Control has classified as a top public health concern, calling it "the worst drug epidemic in US history." An estimated 2 million Americans suffer from a prescription painkiller abuse disorder and 470,000 suffer from heroin abuse.[1] Skyrocketing overdose deaths have surpassed fatal car accidents as the leading cause of accidental death and have contributed to the recent historic reversal in mid-life mortality among non-Hispanic white Americans documented in Case and Deaton (2015).

In response to rising rates of opioid abuse and overdoses, lawmakers have legislated many interventions designed to limit the supply of prescription opioids to those who would abuse them while preserving access for legitimate users. Among these policies are prescription drug monitoring programs (PDMPs); statewide systems that record patient controlled substance prescription histories into an online database accessible to prescribers. Using PDMPs, doctors can identify patients who receive many overlapping prescriptions from several prescribers, a practice called doctor shopping. The non-mandated PDMPs were available to prescribers but did not legally require doctors to query them. A number of states later pass additional usage mandates (referred to as "Mandates" from here on) to existing PDMPs, which require practi-

[1]National Survey on Drug Use and Health: Summary of National Findings. Substance Abuse and Mental Health Services Administration 2013.

tioners to query the PDMPs in certain circumstances. This paper focuses primarily on the effects of PDMPs in general, and controls for mandates. [2]

Heroin is an inexpensive, chemically similar substitute for prescription opioid painkillers. When opioid-addicted patients face additional obstacles in obtaining prescription opioids, they may initiate heroin use. Heroin transition and substitution is an important secondary-effect of supply-side interventions for policymakers to consider because in recent years heroin is often laced with fentanyl, a powerful synthetic opioid which is the cause of many unexpected overdoses (Gladden, 2016). This paper examines the effect of the PDMPs on prescription opioids, disaggregating by dosage strength of pill and examines heroin transition caused by the PDMPs measured by heroin crime rates. I exploit staggered timing of PDMP implementation across states in a difference-in-differences framework to identify causal effects of the programs on prescription and heroin crime outcomes.

This paper contributes to the literature on opioid supply-side interventions by showing that PDMPs have large, significant effects on heavy opioid-abusers. I accomplish this by using more disaggregated data than has yet been used in the PDMP literature, which allows me to identify heterogeneous effects of the PDMPs on the dimension of dosage strength of opioid pill and on the dimension of finer geographic detail on heroin outcomes. First, I provide evidence that PDMPs significantly decrease access to strong prescription opioids. Past work has shown that PDMPs reduce prescription oxycodone, but this paper is the first to disaggregate prescription opioids by dosage of pill. I find that PDMPs decrease oxycodone in the Medicaid population by 25%, which is driven by a 35% decrease in oxycodone in the form of high-dose pills. Secondly, I show that heroin abuse, as measured by heroin crime rates, increases significantly due to the PDMP in counties with high rates of opioids per capita. While PDMPs dont have significant effects on heroin crime rates in the aggregate, they

[2]Much of the recent PDMP literature– Buchmueller and Carey (2018), Dave et al. (2017), Deza and Horn (2017), Meinhofer (2017)– focuses on usage mandates.

increase the rate of heroin crime incidents by 87% in counties within the top 10% of oxycodone per capita.

1.2 Background

Opioids are a class of natural and synthetic morphine-like drugs and include opium, morphine, oxycodone, hydrocodone, fentanyl, and heroin. Opioid molecules bind to opioid receptors in the body, relieving pain and sometimes creating a feeling of relaxation, well-being, or euphoria. Opioids also slow breathing and heart rate, sometimes to the point of respiratory failure in the event of an overdose. The most common prescription opioids are oxycodone (the active ingredient in Percocet, OxyContin, and MS Contin) and hydrocodone (the active ingredient in Vicodin and Lortab).[3]

1.2.1 History of the Opioid Crisis

The opioid crisis is commonly explained by increased access to prescription painkillers, beginning with the dramatic rise of Purdue Pharmaceutical's OxyContin in the mid-1990s. OxyContin was marketed to prescribers as safe and non-habit-forming due to its slow-release mechanism which prevented a sudden high and crash cycle that fosters withdrawal and dependence. OxyContin was also unique because of Purdue Pharmaceutical's aggressive marketing approach, which heralded massive revenue growth from $48 million in 1996 to $3.1 billion in 2012. Purdue painted Oxycontin as a miracle drug for the common American with chronic, non-cancer pain. Other opioid-producers followed suit, and the marketing was so effective that a medical field formerly characterized by "opiaphobia" that sometimes went so far as to deny opioid treatment to terminally ill patients now considers pain "the 5th vital sign," asking

[3]Oxycodone and hydrocodone make up the bulk of all opioid shipments in DEA's Automation of Reports and Consolidated Orders System (ARCOS) dataset, which tracks the universe of opioid shipments. Oxycodone and hydrocodone also have the highest reported rates of abuse within the NSDUH.

patients to rate their pain on a scale of one to ten after taking their blood pressure, temperature, breathing and pulse.[4]

OxyContin contains the active ingredient oxycodone and pills range anywhere from a low dose of 10 milligrams to a high dose of 80 milligrams (as well as the now-discontinued 160 milligram pill). The continuous-release mechanism of the pill was a patented wax coating, but determined opioid abusers could dissolve away the coating or crush the pills into powder in order to swallow, snort, smoke or inject a large immediate hit of the morphine-like drug.

With a rise in demand for opioids and doctors' increased willingness to prescribe these drugs, prescriptions for opioid pain killers increased as well. In 2012, 217 million opioid prescriptions were written in the US–a 150% increase from 1995, which realized 87 million opioid prescriptions.

1.2.2 Prescription Drug Monitoring Programs

As of 2017, 50 states have implemented PDMPs that track patients' prescription histories of controlled substances. Some states have tracked such histories for decades on paper, often for use by law enforcement agencies, but this paper focuses on the establisment of online, electronic drug histories that can be easily accessed by doctors. States set up online databases between 2004 and 2016, and Table 1.1 shows the precise dates when states allowed prescriber access. Many states began data collection 1-12 months before prescribers could access the electronic PDMPs, creating a possible announcement effect.[5]

[4] In 2001 the Joint Commission on Accreditation of Healthcare Organizations added the pain scale.

[5] Dates were obtained by searching the internet for effective dates of electronic, online PDMPs by state. Most dates were verified using several sources, including news articles, the Prescription Drug Monitoring Program Training and Technical Assistance Center website, the National Alliance for Model State Drug Laws website, state legislative laws and bills, government newsletters, various articles from peer reviewed journals, and pharmacy board websites.

Due to low prescriber use of the PDMPs, 12 states[6] implemented usage mandates on top of existing non-mandated PDMPs that require prescribers to query the PDMPs under certain circumstances. In addition, eight states[7] have passed packages of laws designed to stop over-prescribing at unscrupulous "pill mills": pain clinics that are typically cash-only and both prescribe and dispense opioid pills on site. These "Pill Mill Bills" often include requirements that prescribers of painkillers register with state Departments of Health, licensing requirements for pain clinics, or restrictions on in-office dispensing of painkillers.[8] I control for the usage mandates and "Pill Mill Bills" in all of my models. Table 1.1 displays dates of the usage mandates and "Pill Mill Bills." There is not evidence to suggest that states systematically implement both a PDMP and another policy like a Mandate or "Pill Mill Bill" in the same quarter.

1.2.3 Substitution to Heroin

Heroin and opioids are nearly identical at the chemical level[9] and produce similar effects in the body, acting as powerful pain suppressants and creating feelings of wellbeing and euphoria in large doses. Ways of taking heroin have changed, with an increasing prevalence for smoking and snorting because drug purity is now so high that injecting is not required for an intense euphoria. Since many prescription opioid users previously crushed and snorted or smoked oxycodone pills to get high, smoking or snorting heroin is an easy transition (Frank, 1999; Hines et al., 2017). The heroin of the 2010s is produced in Mexico and South America, is often nearly 100% pure, and costs $10 for a small 10 milligram capsule filled with white powder. Disconcertingly, to improve potency most heroin is now laced with a strong synthetic opioid called fentanyl, which is 50-100 times stronger than morphine. Inconsistent

[6]Delaware, Indiana, Kentucky, Louisiana, Maryland, Nevada, New Mexico, New York, Ohio Tennessee, Vermont, and West Virginia

[7]Florida, Kentucky, Louisiana, Mississippi, Ohio, Tennessee, Texas, and West Virginia

[8]For an excellent study on the Florida pill mill crackdown, see Meinhofer (2016).

[9]Different opioids have real chemical differences but have similar effects in the body, binding to the same mu-opioid receptors (Drewes et al., 2013).

amounts of fentanyl (or yet-more-potent fentanyl analogs) within heroin doses is the cause of many unexpected overdoses.

According to the Center for Disease Control, only 3% of prescription opioid abusers initiate heroin abuse, but 75-80% of heroin users report that they transitioned from abusing prescription drugs. Partially due to the prevalence of users who transition from opioids to heroin, the opioid crisis is now a socio-demographically wide-spread phenomenon, with the most concentrated effects among white non-Hispanic Americans (Cicero et al., 2014). In contrast, past drug crises like the heroin crisis of the 1970s and the crack epidemic of the 1980s and 1990s had been concentrated among urban and minority populations. Prescription opioid overdoses increased in the 2000s among middle-aged non-Hispanic white Americans, and heroin and fentanyl overdoses skyrocketed in the 2010s among non-Hispanic white Americans between ages 20 and 35 (Unick and Ciccarone, 2017). The opioid crisis is also geographically widespread, affecting suburban and rural areas nationwide.

The transition from opioids to heroin is widely documented in small-scale research samples and surveys in the health and addiction literature (Lankenau et al., 2012; Siegal et al., 2003), and wide-scale empirical studies linking prescription opioids and heroin have just recently emerged (Alpert et al., 2017; Evans and Power, 2017; Kilby, 2015; Meinhofer, 2017). This paper is unique among these in that I link non-mandated PDMPs to heroin transition, use heroin crime rates rather than heroin overdose deaths or treatment admissions as a measure of heroin abuse, and perform my heroin analysis at the county level instead of the usual coarser state level, with an emphasis on heterogeneous effects of the policy on heroin transition in different types of counties.

1.2.4 Related Literature

Existing studies in the health literature draw varying conclusions regarding the efficacy of PDMPs, with studies finding zero effects as often as significant reductions in opioid abuse measures. However, one typically corroborated result is that PDMPs decrease prescription oxycodone shipments (Kilby, 2015; Paulozzi et al., 2011; Reisman et al., 2009; Simeone and Holland, 2006). Several authors find PDMPs without mandates affect Schedule II opioids (oxycodone) and not Schedule III-V opioids (hydrocodone).[10] Few studies that examine the effect of the initial implementation of PDMPs use detailed prescription data, and most use aggregated opiate shipments tracked by the DEA. One exception is Kilby (2015), who uses a dataset of prescription claims from Truven Health Analytics that covers 59% of the U.S. population. She finds that non-mandated PDMPs cause a 10% reduction in oxycodone prescriptions, and also finds a 10% decrease in oxycodone shipments from the DEA's ARCOS dataset, which tracks aggregate shipments of opioids. Buchmueller and Carey (2018) utilize a claims-level subsample of the universe of Medicare claims, and find no effect of non-mandated PDMPs on abuse outcomes, likely because those 65 and up exhibit lower rates of opioid abuse than the younger general population.

Results for the effect of non-mandated PDMPs on outcomes outside of prescription oxycodone are mixed. Some studies find a reduction in overdoses or poisonings in response to PDMPs (Patrick et al., 2016; Reifler et al., 2012; Simoni-Wastila and Qian, 2012), whereas other studies find no response in opioid abuse outcomes. (Brady et al., 2014; Buchmueller and Carey, 2018; Dave et al., 2017; Bachhuber et al., 2016; Meara et al., 2016; Paulozzi et al., 2011)). Deza and Horn (2017) find that non-mandated PDMPs established between 2007 and 2012 reduce crime rates.[11] Because recent papers often find weak effects of non-mandated PDMPs, the opioid literature in economics has turned its attention to PDMP mandates that require doctors to access already-established PDMPs. Several recent studies find significant effects of PDMP

[10]Drugs receive a Schedule I-V rating based on medical usefulness and possibility of dependence, with higher numbers meaning more benign and lower numbers more dangerous. Illicit drugs like heroin and cocaine are Schedule I with little medical benefit and high potential for abuse. Some opiate painkillers (fentanyl, oxycodone, morphine) are Schedule II; hydrocodone was Schedule III in the time period relevant to this paper. Schedule III drug prescriptions can be refilled without making an appointment with a doctor; Schedule II drugs cannot be refilled.

[11]Deza and Horn (2017) finds the effects of PDMPs and their Mandates on crime rates, with an emphasis on violent crime and property crime. My paper focuses on drug crime, namely incidents involving the seizure of heroin or diverted opioids.

usage mandates that require doctors to check already-existent PDMPs (Buchmueller and Carey, 2018; Dave et al., 2017; Deza and Horn, 2017; Meinhofer, 2017). Mandates are effective at reducing many abuse outcomes, including doctor shopping through Medicare, substance abuse facility admissions, crime rates and fatal drug overdoses.

The economics literature has also begun to connect opioid abuse and heroin-substitution outcomes. Studies by Alpert et al. (2017) and Evans and Power (2017) examine heroin substitution in response to the 2010 reformulation of OxyContin. The reformulation made OxyContin more difficult to crush, which is a primary step to snorting, smoking, or injecting it to obtain a more intense high. Both sets of authors find dramatic increases in heroin overdose deaths in the most opioid-dense states consistent with the timing of the reformulation. In the PDMP literature, Kilby (2015), Meinhofer (2017), and Radakrishnan (2014) have studied the effect of PDMPs on heroin overdoses and treatment admissions. All three studies find limited effects of the non-mandated PDMP on heroin abuse outcomes, but do not account for the possibility of heterogeneous effects within the population.

In contrast to other PDMP papers that focus on effects of the added mandates, I focus on non-mandated PDMPs among high-abuse populations and geographical areas, and I find evidence that suggests that non-mandated PDMPs have large effects among high-abuse populations. In this paper I examine prescription outcomes in the Medicaid population, whereas other papers have focused on the general population or Medicare populations.[12] The CDC has long stated that the Medicaid population is at higher risk for opioid abuse disorders, and this paper is among the first to focus on Medicaid prescription outcomes in response to the PDMP. Past studies have shown that doctors who have patients from high-abuse populations access and query non-mandated PDMP databases at higher rates (Goodin et al., 2012; Irvine et al., 2014;

[12] A 2017 paper in the health policy literature by Wen et al. uses the same Medicaid dataset, using years 2011-2014. The authors do not include robustness checks or test different specification strategies of their difference-in-differences approach, nor do they provide evidence that parallel trends is supported. It is not clear if standard errors were cluster-bootstrapped, which is likely necessary due to few states implementing PDMPs between 2011 and 2014.

Ross-Degnan et al., 2004), and my results suggest these PDMPs have effects of a similar magnitude to mandated PDMPs among the Medicaid population.

This paper also contributes to the recent economics literature covering opioid-to-heroin substitution, by treating PDMPs as a source of exogenous variation in abusers' access to prescription opioids. Other studies estimate heroin use by admissions to substance abuse treatment facilities or by death rates from heroin. I use a more detailed and informative measure, namely an incident-level dataset of reported crimes, aggregated by county and month, to measure the effects of PDMPs on heroin crime rates. Since other recent studies only found weak or inconsistent links with heroin outcomes, I use more granular geographic data to examine heterogeneous effects across counties, using the counties' levels of pre-policy opioid abuse, proxied by oxycodone milligrams per capita. To the extent that residents in more opioid-dense counties are more likely to be heavy opioid users, an increase in heroin crime within these counties would suggest that PDMPs are highly influential in the transition to heroin use by those who heavily abuse prescription opioids.

1.2.5 Predictions of Policy Effects

PDMPs act as a negative supply shock for legally-obtained prescription opioids by making it more difficult for abusers to obtain prescriptions. Former doctor-shoppers may turn to the black market for diverted opioid prescriptions[13] because illegally diverted opioids are a substitute for legally prescribed opioids. The PDMP should therefore cause an increase in demand for diverted illegally-obtained opioids. However, the supply of diverted opioids available for purchase on the black market should also be affected by the PDMP because much of the supply of diverted opioids is obtained by doctor shopping, which the PDMP targets. Since the PDMP causes a decrease in supply as well as an increase in demand in the black market for illegally-

[13] In the NIBRS, an opioid is considered illegal or "diverted" when the individual in possession of the opioid does not possess a prescription.

diverted opioids, quantity effects are ambiguous and it is not clear whether police will encounter fewer or more illegal opioid crime incidents.

Heavy abusers who rely on doctor shopping to obtain their prescription opioids may turn to another substitute, heroin, in response to the additional obstacles to prescriptions posed by the PDMP. An increase in demand for heroin should mean police encounter more incidents where heroin is involved after the PDMP is passed.

1.3 Data

1.3.1 Prescription Data: Medicaid State Drug Utilization Data

Table 1.2 lists summary statistics on frequency of prescription opioid and heroin abuse from self reports in the National Survey on Drug Use and Health 1990-2014. The table is divided into non-Medicaid respondents and Medicaid-enrolled respondents. I further divided the data into all respondents, and respondents who report having ever used hydrocodone non-medically, used oxycodone non-medically, and used OxyContin non-medically. Hydrocodone, oxycodone, and OxyContin are presented in ascending order of potency and abuse potential. Hydrocodone is a relatively weak Schedule III opioid typically prescribed for acute temporary pain, and oxycodone is a stronger Schedule II substance used to treat moderate to severe chronic pain. Most opioid crackdowns have focused on limiting oxycodone. About a third (0.348) of oxycodone abusers report having used OxyContin, the slow-release formulation of oxycodone that comes in large doses.

Within the survey, Medicaid respondents are more likely to abuse opioids; and among groups of hydrocodone, oxycodone and OxyContin abusers, Medicaid enrollees use opioids more frequently than their non-Medicaid counterparts. The first column lists summary statistics for the entire Non-Medicaid and Medicaid subsets of the data, including respondents who do not abuse opioids. 11% of survey respondents not on Medicaid report having ever abused opioids, and the average respondent in the non-Medicaid group reports abusing opioids 2.029 times in the past year. Within the respondents who are Medicaid enrollees, 12.7% have ever abused opioids and the average respondent has abused opioids 3.30 times in the past year. The second column restricts both the Non-Medicaid and Medicaid groups to those who reported having ever abused hydrocodone. The average non-Medicaid abuser of hydrocodone has misused opioids 20.19 times in the past year, compared to 28.89 abuses for the average Medicaid counterpart. Abusers of oxycodone and OxyContin show the highest rates of reported abuse: oxycodone abusers report misusing opioids 22.82 and 32.41 times a year, in the non-Medicaid and Medicaid subsets respectively, and OxyContin abusers report using 40.45 and 52.10 times respectively. Medicaid have both higher rates and frequencies of reported heroin abuse than the non-Medicaid respondents. Those who abuse hydrocodone, oxycodone and OxyContin are much more likely to report heroin use as well, with increasing odds (8.4%, 11.4%, and 19.7% in the non-Medicaid population, and 10.8%, 14.6% and 23.4% in the Medicaid-enrolled population) across opioid-strength categories.

Since Medicaid enrollees are more likely to abuse opioids than the general population, and abuse increases across drug-strength categories, the Medicaid dataset used for this paper is advantageous in revealing the true effects of the PDMP. I expect PDMPs disproportionately affect heavy-abusers of opioids, so the Medicaid population provides a good chance of finding large and significant policy effects.

Medicaid tracks the universe of prescriptions the program pays for and compiles the information into aggregated reports on the Medicaid website in the Medicaid State Drug Utilization Data. The Medicaid dataset on opioid pills covers 7-15% of all prescription painkillers in the United States. The National Drug Code (NDC) is a unique product-identifier that identifies each drug by its manufacturer, active ingredient, and dosage amount, among other details. The Medicaid data report the state-by-quarter counts of each NDC prescribed. I use the NDC to merge the Medicaid data to detailed information from the Food and Drug Administration.[14] For

[14]Many of the NDCs for opioids found in the Medicaid data are outdated, so I manually searched for records by NDC and obtained dosage and strength information on outdated NDCs from many different websites.

my analysis, I restrict my observations to tablets[15] of oxycodone and hydrocodone painkillers, the most commonly abused opioids. Patients typically receive take-home opioid prescriptions in the form of tablets.[16]

Because the Medicaid data are reported at the NDC level, I aggregate milligrams by both drug type and strength, differentiating drug milligrams that come in the form of low-dose pills from those in high-dose pills. Opioid active ingredients have varying potencies, so I use different milligram cutoffs for hydrocodone and oxycodone drugs. Oxycodone is 1.5 times as strong as hydrocodone. I define a low-dose pill as a hydrocodone pill with 15 or fewer milligrams of hydrocodone or an oxycodone pill with 10 or fewer milligrams of oxycodone. A high-dose oxycodone pill contains greater than 10 milligrams of oxycodone, and a high-dose hydrocodone pill contains more than 15 milligrams of hydrocodone. Hydrocodone is typically not found in pills with more than 15 milligrams.[17] The 10 milligrams oxycodone/15 milligrams hydrocodone cutoffs were chosen because commonly-abused Percocet and Vicodin have 10 or fewer oxycodone milligrams and 15 or fewer hydrocodone milligrams, respectively. More dangerous pills like OxyContin, whose abusers exhibit more severe abuse characteristics, have more than 10 milligrams of oxycodone.[18]

1.3.2 Drug Enforcement Agency ARCOS Data

The Drug Enforcement Agency tracks aggregate shipped amounts of controlled substances through the Automation of Reports and Consolidated Orders System (AR-

[15]Tablets account for 79% of the NDCs in the opioid prescription dataset, and 69% of all quantities of opioids given out. In addition to tablets, opioids come as solutions, syrup, and patches, mostly in the form of codeine, a relatively weak form of opioid.

[16]Oxycodone and hydrocodone are the most commonly abused opioids (NSDUH) and the only opioids the Drug Enforcement Administration has tracked for the entire time period between 2000 and 2015. There is not evidence that PDMPs affect other less-commonly abused opioids like oxymorphone, hydromorphone, meperidine, tramadol, tapentadol, morphine, or methadone. The unresponsiveness of the more uncommon opioids is consistent with findings in Kilby (2015). Results available upon request.

[17]In the Medicaid data, only 0.2% of hydrocodone comes in higher-dose, extended release capsules.

[18]Effects disaggregated on pill strength are robust to using different milligram cutoffs for "strong" pill classification. Results are driven by 30, 40, and 80 mg oxycodone pills, as covered in C.

COS). These data are recorded by state and quarter and by zipcode and quarter. I use the shipped quantites of oxycodone and hydrocodone between 2000 and 2014 to supplement my Medicaid results with data from the general population, as well as for comparison to other studies in the literature that also use the ARCOS (Kilby, 2015; Reisman et al., 2009). The ARCOS data provides more fine-grained geographical information at the zipcode and county level than dos the Medicaid data, which is at the state level. I use ARCOS county oxycodone per capita to obtain a proxy measurement for pre-policy opioid abuse within counties. The ARCOS data are not at the NDC level of specificity, so I am not able to decipher dosage amounts (strong versus weak doses) nor dosage form (tablets versus solutions usually given under medical supervision) of the oxycodone and hydrocodone within the aggregate population data.

Table 1.3 displays Medicaid drug milligrams *in tablet form* per enrollee and ARCOS drug milligram shipments *in all forms* per population in the data. The oxycodone per capita rate from the ARCOS and the oxycodone *tablet milligrams* per Medicaid enrollee[19] from the Medicaid data appear similar at around 55 morphine units per quarter per person, which is approximately 6-8 low dose pills or 1-2 high-dose pills per capita. In the Medicaid data, where oxycodone can be broken down into high dose ($>$ 10 mg) and low dose ($\leq$ 10 mg), the bulk of prescribed oxycodone is dispensed in high dosage tablet form. Hydrocodone comes in nearly exclusively low-dose tablets, often in combination with acetaminophen, as is the case with brand name Vicodin. It is unknown wheter the proportions of weak dose versus strong dose tablets of oxycodone (or hydrocodone) in the Medciaid data is the same as in the general population because the ARCOS data lacks this information. I assume the Medicaid information is representative and explore it because policy effects on dosage strength are an interesting and potentially important contribution to the literature on opioid supply-side interventions.

[19] I classify capsules and tablets as tablets.

1.3.3 NIBRS

The National Incident-Based Reporting System (NIBRS) is an incident-level dataset of crimes committed in 6,251 law-enforcement jurisdictions across 38 states and 1,634 counties. For the purpose of this paper, I use a complete monthly panel of 735 counties in 26 states from 2004-2014. A map of the 735 counties is documented in Figure 1.1, which shows that coverage is nationally widespread, including some states with near-complete coverage. The NIBRS is a more-detailed subset of the FBI's Uniform Crime Reporting (UCR) system, and the 2004 NIBRS covered police districts in areas containing 20% of the United States population and accounted for 16% of the UCR crime statistics data collected by the FBI. Reported crimes include information about the location where the incident occurred, details about the nature of the crime, and demographic characteristics of the offender (among other information).

For my analysis, I focus on drug crimes involving the purchase, sale or possession of heroin or illegally obtained prescription opiates. I collapse the NIBRS incident-level data to obtain a panel of the number of crimes per 100,000 population per month in each covered county. Dependent variables include incidents where heroin or opiates are seized, and incidents involving possible drug dealers, as defined below.

I divide counties based on their density of oxycodone, revealed by the ARCOS data, for the year 2004, prior to the timing of most electronic PDMPs. My rationale is that PDMPs should have a larger impact and cause more opioid abusers to transition to heroin in areas with a larger stock of opioid abusers prior to the PDMP. I proxy the number of existing opioid abusers with the recorded numbers of oxycodone milligrams per capita, matching zipcode-level ARCOS data to county-level crime data in order to obtain fine geographic measures of oxycodone density. I use each county's mean per-quarter amount of oxycodone per capita in 2004 to proxy the initial stock of opioid abusers susceptible to the PDMP. The 2004 level is late enough that the opioid crisis was beginning to affect counties differently, but early enough that most PDMPs had not been implemented. The distribution of oxycodone density across different counties is plotted in Figure 1.2. Most counties receive 10-50 milligrams per person in oxycodone shipments, but the figure suggests that there are “outlier” counties that receive many more opioids per capita. I split the counties on the 90th percentile of oxycodone density, at 63.15 milligrams of oxycodone per capita. The 10% of counties that are above this cutoff are the “high oxycodone density” counties and the bottom 90% that are more centered around 25 mg/capita are classified as “low oxycodone density” counties.[20] Figure 1.3 shows oxycodone-density for the counties in the NIBRS data, with the most oxycodone dense counties appearing in New England, the Appalachian regions of Tennessee, Virginia, and West Virginia, and a few counties in Ohio, which are all known to be high-abuse areas.

Table 1.4 displays summary statistics of drug crimes from the NIBRS data. The table is split into 3 panels: crime rates across all 735 counties in the NIBRS, crime rates within the lower 655 (counties that make up the bottom 90%) of the oxycodone-per-capita distribution, and crime rates within the 80 counties (counties that make up the top 10%) with the highest oxycodone-per-capita. The typical county realizes 1.3 heroin incidents and 2.2 incidents of illegally diverted opioids per 100,000 population per month. The less oxycodone-dense counties experience a mean of 1.124 heroin incidents and 1.866 diverted-opioid incidents per month, whereas the highly-opioid-dense counties experience 2.342 and 4.009 heroin and diverted-opioid incidents, respectively. Thus, the rates in the most oxycodone-dense counties are twice as high.

To identify possible heroin dealers in the NIBRS dataset, I count the individuals per county and month who 1.) are carrying more than 2 grams[21] of heroin, 2.) Are

[20] Results are robust to different cutoffs. B includes figures that plot coefficient estimates when using cutoffs other than the 90th percentile, and suggest that the heroin results are significant among the top 30% of counties in terms of oxycodone density.

[21] 1 gram of heroin is 100 doses of 10 mg each. States have varying levels of heroin amounts that create the assumption of “trafficking,” with Idaho, Maine, Mississippi, South Carolina, and Vermont considering 2 grams an important cutoff for trafficking, assigning harsher punishments to those carrying above 2 grams of heroin. Other states typically have cutoffs ranging between 1 and 5 grams, but laws differ drastically across states.

carrying between 1 and 2 grams of heroin and a large amount of another drug[22], or 3.) Are carrying any heroin and were entered in the data as selling any drug. A probable opiate dealer is someone who 1.) is carrying more than 5 grams or 250 pills of opiates, 2.) is carrying between 2 and 5 grams or between 100 and 250 pills and are carrying a large amount of another drug, or 3.) is carrying opiates and are entered as selling any drug.

In Table 1.4, the average county realizes 0.502 incidents per month involving possible heroin dealers, and 0.523 involving possible dealers of diverted opioids. The low oxycodone counties experience about 0.4 incidents of each type per month, whereas the high oxycodone counties experience about 1 heroin and diverted-opioid incidents per month which involve a possible dealer. Again, the crime ratio for the two sets of counties is about two to one.

1.4 Empirical Methods

For the main analysis of this paper, I use a difference-in-differences regression framework on a state-quarter panel and a county-month panel weighted by population, using the different implementation dates by state of PDMPs, Mandates and Pill Mill Bills as a source of exogenous variation in treatment. The identifying assumption of the difference-in-differences specification is the parallel trends assumption that treated and untreated states follow similar growth paths prior to the treatment and would have continued to do so in the absence of treatment. This approach identifies changes in trends within the treated geographies that correspond to the timing of the implementation of the policy. I adapt the difference-in-differences models into an event-study framework with policy lags and leads to test the parallel trends assumption. I later supplement the analysis with interactive fixed effects factor models (IFE), as detailed in Bai (2009), which are explained later in the paper.

[22]More than 1 gram of crack cocaine, more than 1 gram of cocaine, more than 500 grams of marijuana (about 17 oz–enough to be charged with a felony in most states), more than 2 grams of opioids, or more than 1 gram of methamphetamine.

1.4.1 The Effect of PDMPs on Prescription Data and ARCOS Shipments

Models for finding the effect of the policies on the amount of opioids used by Medicaid recipients and ARCOS shipments are at the state and quarter level. The model is as follows:

$$RxOutcome_{it} = \alpha + \beta PDMP_{it} + \eta Mandate_{it} + \phi PillMillBill_{it} + \Psi X_{it} + \iota_i + \gamma_t + \epsilon_{it}$$

Where $RxOutcome_{it}$ is logged milligrams of Medicaid oxycodone or hydrocodone per Medicaid enrollee, or logged total ARCOS shipped amounts of oxycodone or hydrocodone per population in state i in quarter t or earlier.[23] $PDMP_{it}$ is an indicator that is equal to one if state i has established an electronic Prescription Drug Monitoring Program by quarter t. $Mandate_{it}$ is an indicator equal to one if a state has mandated that prescribers must check the PDMP under certain circumstances by time period t. $PillMillBill_{it}$ is an indicator equal to one if a state has passed a menu of laws targeting "Pill Mills."[24] γ_t is a set of time period fixed effects that flexibly capture the average national time path of the outcome variable. ι_i is a set of geography fixed effects that control for the average level of the outcome variable in a state and the effects of time-invariant state characteristics. ϵ_{it} is a stochastic, normally distributed error term.

Event-study graphs (for example, graphs in Figures 1.4 and 1.5) are based on the following models:

$$RxOutcome_{it} = \alpha + \Sigma_{p=-5}^{10} \beta_p PDMP_{i,t+p} + \eta Mandate_{it} + \phi PillMillBill_{it} + \Psi X_{it} + \iota_i + \gamma_t + \epsilon_{it}$$

$PDMP_{i,t+p}$ is an indicator equal to one if the policy started in state i in the time $t+p$. The coefficients β_p capture the measured effect of the PDMP at p periods

[23] Logged linear models are used for prescription outcomes, but results on Medicaid oxycodone, strong Medicaid oxycodone, and ARCOS oxycodone are robust to the removal of the log and are available upon request. Prescription results are also robust under a Poisson model, also available upon request to the author.

[24] A state with more than one policy, like Kentucky, which has a PDMP, a usage mandate, and a pill mill crackdown by July 2012 will have all three indicator variables equal to one, with the cumulative effect of the policies on the outcome equal to the sum of the variables' coefficients.

after passage. For example, if $p = 2$, $\beta_{i,t+2}$ would capture the effect of the policy on the outcome variable 2 periods after passage.[25] Negative values of p correspond to "leads," which capture the effect of the policy before it is implemented and should be zero under the parallel trends assumption of the difference-in-differences methodology.

X_{it} is a matrix of controls that capture changes within states over time in demographic characteristics and economic characteristics. State-level controls for the prescription outcome models are summarized in Table 1.5. The matrix includes the fraction of the population that is black, Hispanic, or of other non-white race, as well as the poverty rate, unemployment rate, average weekly wage rate, average income per capita, and the fraction of the population employed in the agriculture or manufacturing sectors. I include controls for the age composition of the population (fraction of population in age groups 10-19, 20-29, 30-39, 40-49, 50-59, 60-69 and 70 years or older) and the gender composition of the population. I control for the average number of pills of all drug types filled through Medicaid per Medicaid enrollee to capture variation in the overall Medicaid-prescribing behavior within states over time. I also control for the implementation of Medicare Part D, which increased elderly access to prescription drugs, by controlling for the fraction of the population enrolled in Medicare interacted with an indicator that turns on in 2006, when Medicare Part D began.[26] I control for state-varying Medicaid expansion under the Affordable Care Act, but the expansion occurs in 2014, 2015, and 2016 and is not driving results.[27]

Finally, I control for effects of the abuse-deterrent reformulation of OxyContin that became prevalent in 2010, because Alpert et al. (2017) and Evans and Power (2017) find a large impact of the OxyContin reformulation on heroin overdoses. Both studies find that states react differently to the OxyContin reformulation based on their pre-policy rate of reported OxyContin abuse (in the NSDUH) (Alpert et al.,

[25] Indicator variables $PDMP_{i,t+p}$ are only equal to one in the time p period after passage, and equal zero in all other time periods.

[26] Since many opioid abusers obtain their drugs from friends and relatives, increasing senior access to prescription drugs increases opioid abuse. See Pacula, Powell and Taylor (2015) for a time-study analysis.

[27] Regressions dropping data from 2013-2015 yield similar results, meaning the ACA is not driving coefficient estimates. Results available upon request.

2017) and oxycodone per capita in the ARCOS (Evans and Power, 2017). Their models control for heterogeneous effects of the reformulation across different states by multiplying a post-reformulation indicator variable by the pre-reformulation proxy for opioid abuse. Similarly, I control for differing effects of the reformulation across states by multiplying a post-reformulation indicator by a state's mean number of OxyContin milligrams per Medicaid enrollee (in the Medicaid data) in 2004.[28]

1.4.2 The Effect of the PDMPs on Crime Rates

Crime-rate models use the NIBRS panel data at the county and month level. The main analytic-weighted difference-in-differences models are in the form:

$$CrimeRate_{ct} = \alpha + \beta PDMP_{ct} + \eta Mandate_{ct} + \phi PillMillBill_{ct} + \Psi X_{ct} + \iota_c + \gamma_t + \epsilon_{ct}$$

$CrimeRate_{ct}$ is the number of crimes per 100,000 people in the NIBRS-covered population in county c in month t.[29,30] $PDMP_{ct}$, $Mandate_{ct}$, and $PillMillBill_{ct}$ are indicators equal to one if the PDMP, Mandate, or menu of "Pill Mill" legislation is in effect in county c's state in month t, and β, η, and ϕ capture the effect of the policies on the outcome crime-rate. X_{ct} is a matrix of county characteristics that vary over time, and γ_t and ι_c are time and county fixed effects.

Table 1.6 lists county controls in matrix X_{ct}. Controls include racial, age, and gender demographics like in the prescription section, but instead at the county level. I also control for the county-level unemployment rate and average weekly wage. I control for the fraction of the county's labor force that works in a manufacturing job and use pharmacies per capita to control for changing access to prescription drugs. I

[28]Alpert et al. (2017) use OxyContin abuse that is reported in the NSDUH as a measurement for how states will experience the effects of the OxyContin reformulation on heroin overdoses. When I instead use OxyContin prescribing rates in the Medicaid data on heroin crime outcomes, my result magnitudes are similar to the Alpert et al. (2017) effects of NSDUH OxyContin abuse reporting on heroin overdoses.

[29]Outcomes for crime rates are not logged because 86% of county-month pairs report zero heroin incidents. Heroin results are robust under a Poisson regression model, as documented in a later section.

[30]The NIBRS includes a variable that lists each reporting jurisdiction's covered population. Jurisdiction populations within the same county are summed when aggregated to the county level.

control for law enforcement officers per capita in each crime-reporting jurisdiction over time to account for any enforcement changes within counties that may correspond to the timing of the policies. I also control for the abuse-deterrent reformulation of OxyContin and the enactment of Medicare Part D as I did for the models in the prescription opioid models.[31] I adapt the approach in Alpert et al. (2017) and Evans and Power (2017) for measuring the effect of the OxyContin reformulation to the county level by multiplying a post-August 2010 indicator by counties' pre-reformulation oxycodone density in the ARCOS data.[32]

To identify the effect of the policies over time and support the identification assumption of parallel trends, I create graphs with coefficient estimates obtained from the event study (as seen in Figures 1.6):

$$CrimeRate_{ct} = \alpha + \Sigma_{f=-12}^{12} \beta_f PDMP_{i,t+f} + \eta Mandate_{ct} + \Psi X_{ct} + \iota_c + \gamma_t + \epsilon_{ct}$$

β_f captures the effect of the PDMP on the crime-outcome variable at f months after passage. For example, β_5 estimates the effect of the PDMP 5 months after passage. The β_f coefficients associated with negative, (pre-policy) time periods should equal zero and will capture pre-policy effects if the parallel trends assumption is not satisfied.

1.4.3 The Interactive Fixed Effects Factor Model

The interactive fixed effects (IFE) factor model as detailed in Bai (2009) accounts for (possibly non-linear) geography-specific time trends while nesting fixed effects of time and county (state), accomplished by adding a principal component analysis

[31] Medicare enrollment by year is available at the state level, but not at the county level. At the county level, I instead proxy by using fraction of the population who are aged 65 and up.

[32] Medicaid data are not available at the disaggregated county level. To measure a treatment intensity of the OxyContin reformulation at the county level, I use ARCOS oxycodone shipments per capita from each county interacted with a post-August 2010 indicator. This method is almost identical to the method in Evans and Power (2017), but at the county rather than state level. My estimates of the county-level effect of the reformulation (measured by ARCOS oxycodone density) on heroin abuse (measured by heroin crime rates) are similar in magnitude to those in Alpert et al. (2017), who also find the effect of the reformulation (measured by NSDUH OxyContin abuse reports) on heroin abuse (measured by heroin overdoses).

structure to the error term. The IFE factor model assumes that patterns in opioid and heroin abuse within counties (states) can be modeled as a function of R unobserved linear factors, F_{rt}. The optimal number of factors, R, are chosen using criteria in Bai and Ng (2002).

$$AbuseOutcome_{ct} = \alpha + \beta PDMP_{ct} + \Psi X_{ct} + \Sigma_{r=1}^{R} \lambda_{rc} F_{rt} + u$$

The above equation outlines the IFE factor model structure, where F_{rt} is an unobserved factor, common across all counties (states) in month (quarter) t, and λ_{rc} is a county (state) factor loading, constant over time.

The factors, F_{rt}, can be thought of as nationwide time trends in opioid or heroin abuse to which different counties (states) are either more or less susceptible, depending on unobservable characteristics of those counties (states). The basic difference-in-difference model accounts for national non-linear patterns in abuse, and the IFE factor model extends this by accounting for additional non-linear time trends that affect areas to varying degrees. For example, when I apply the factor model to heroin crime-rates, the factor model produces factors that plot out a gradual increase in heroin crime from 2004-2010, which then increases exponentially from 2010-2014. Counties experience the non-linear increase in heroin to differing degrees, which is accounted for in each county's factor loading. In the case of heroin crime incidents, a county's factor loading is correlated with its 2004 level oxycodone milligrams per capita, implying that more opioid-dense counties are more sensitive to the increase in heroin crime. This is consistent with the original hypothesis that restricting opioids causes more heroin use.

For factor model analysis on heroin incidents, the IFE factor model could in theory be approximated by adding linear, quadratic, and cubic geography-specific time trends to a difference-in-differences regression, but that comes at the cost of efficiency and statistical power. In practice, however, rather than adding a linear, quadratic, and cubic time trend for each of 735 counties, the factor model uses a matrix structure based on principle components analysis to account for several flexible time trends and assign factor loadings for each time trend by county. This factor approach uses fewer degrees of freedom while controlling for flexible time trends and therefore results in more precisely measured-estimates. The IFE factor model serves as a robustness check to my difference-in-differences model, and the point estimates are typically similar across both model specifications. Factor model results are covered in detail in the results section.

1.5 Results

1.5.1 Effect of the PDMP on Prescription Amounts

Table 1.7 shows the estimates for the coefficients of interest in Equation 1.4.1, measuring the effect of the PDMP and related policies on the Medicaid prescription and ARCOS shipments of oxycodone and hyrocodone amounts per capita. The model specification in Table 1.7 includes state and quarter fixed effects and state controls, and weights observations and standard errors by either state Medicaid enrollees for models run on Medicaid outcomes (Columns (1)-(4)) or state population for models run on ARCOS data (Columns (5) and (6)). Columns (1)-(4) contain coefficient estimates from the weighted difference-in-differences model run on Medicaid oxycodone, weak oxycodone, strong oxycodone, and hydrocodone, respectively. Columns (5) and (6) contain the estimates from the model run on ARCOS total oxycodone and hydrocodone, respectively.

The Medicaid outcome variables in Columns (1) through (4) are in logged morphine milligrams per Medicaid enrollee and the ARCOS outcome variables in Columns (5) and (6) are in logged morphine milligrams per capita, meaning that table entries are interpreted as proportional increases and decreases in the dependent variable in response to the PDMP, Mandates, and "Pill Mill Bills." Column (1) shows the PDMP reduces Medicaid oxycodone per Medicaid enrollee by 24.6%, which is significant at the 10% level. Column (2) shows neither a large nor significant reduction in oxycodone per Medicaid enrollee in the form of weak-dose ($\leq$10mg) oxycodone pills; however, Column (3) shows a significant 35% reduction in strong-dose (>10mg) oxycodone per Medicaid enrollee in response to the PDMP. In Column (5), the PDMP is found to reduce the aggregate amount of oxycodone shipped per capita by 8%, significant at the 10% level. Neither Columns (4) nor (6) suggest that the PDMP has an effect on hydrocodone use. See A for Medicaid prescription results across model specifications.

Figure 1.4 shows the accompanying event study graphs for the weighted difference-in-differences model in Columns (1), (2), and (3) from Table 1.7, in which the dependent variables are Medicaid total oxycodone, weak oxycodone, and strong oxycodone per enrollee. The vertical line in each graph marks the first quarter of the PDMP. Oxycodone begins trending downward at the time of the policy implementation, and this effect is driven by a reduction in strong oxycodone, which makes up the majority of all oxycodone amounts dispensed through Medicaid. The leads of the oxycodone and strong oxycodone graphs are close to zero until the policy takes effect at quarter zero, which supports the parallel trends assumption. The states with PDMPs had similar growth paths to states without PDMPs prior to the implementation of the policy. The parallel trends assumption seems to hold. The graphs show a break in trend among the treated states at the time of the policy implementation, lending evidence to the PDMP causing a decrease in oxycodone.

Figure 1.5 plots the event study coefficient of the model on aggregate shipment rates of oxycodone from the ARCOS data, and shows an 8% reduction among such shipments per capita over time. This result is consistent with much of the PDMP literature that uses ARCOS data as an outcome response to the systems, including Kilby (2015), who finds a 10% reduction in ARCOS oxycodone in response to the non-mandated PDMP. I find larger oxycodone reductions for the Medicaid population than for the aggregate population, which can be explained by several reasons. The CDC states that people enrolled in Medicaid are more prone to opioid and heroin abuse (see Table 1.2), meaning that if PDMPs affect all opioid abusers similarly, the effect will be greater in the Medicaid data because opioid abusers make up a larger fraction of the Medicaid population (Frank, 1999). Additionally, prescribers who interact with high-abuse populations are more likely to use a PDMP, even if it is not mandated (Goodin et al. (2012), Ross-Degnan et al. (2004), and Irvine et al. (2014)), so in areas with large abuse populations, PDMPs are perhaps effective in cutting usage despite not being mandated by law. In short, the Medicaid population may be specially positioned for the PDMP to work well on it.

Although many of the models in Table 1.7 show significant effects of the Mandate and Pill Mill Bill policies on drug amounts, all of the event study models fail the parallel trends assumption, and are not remedied by the addition of trends. Both Mandate and Pill Mill Bill results on prescription outcomes are volatile across model specifications.[33]

A novel contribution of this study is that I find the decrease Medicaid-prescribed oxycodone is driven by reductions in prescriptions for the high-dosage oxycodone pills (¿10mg). No other study has considered heterogeneous effects of the PDMP on oxycodone drugs of differing strengths. For additional detail, C includes an analysis of the PDMP effect on Medicaid oxycodone at a further level of disaggregation, and it finds that reductions in the 30, 40, and 80 milligram pills are driving the overall reduction in strong-dose pills. I also find that PDMP reductions among Medicaid prescriptions are only prevalent among generic oxycodone pills, and not brand-name OxyContin.[34]

1.5.2 Effect of the PDMP on Drug Crime Rates

Table 1.8 shows the effect of the PDMP on crime incidents in which heroin is seized per 100,000 NIBRS-covered population in a county and month. Entries in this table show the effect of the PDMP, Mandate and "Pill Mill Bills" on number

[33]See A for model estimates and graphs of Mandate event studies. This paper is restricted to examining 12 Mandates passed between 2007 and 2015. Since 2015, 15 more states have passed and or implemented Mandates to their PDMPs, and future work on the effectiveness of Mandates may benefit from the additional states.

[34]Hwang et al. (2015) and Meinhofer (2016) find that only generic oxycodone is responsive to the reformulation of OxyContin and Florida's crackdown on pill mills, respectively. Additional results on brand-name versus generic oxycodone are available upon request to the author.

of heroin crime incidents per 100,000 population. These entries interpretable as the change in heroin crime incidents per 100,000 per month caused by the policies. The table is broken up into three panels, for models run on A.) all 735 counties, B.) on the bottom 90% of counties by oxycodone density, and C.) on the top 10% of counties by oxycodone density (all as determined from the ARCOS dataset).

Panel A shows the effect of the policies across all counties in the NIBRS. Column (1) shows coefficient estimates from a simple ordinary least squares model of the heroin crime rate on the PDMP, Mandate and Pill Mill Bill. The significant estimate of 0.466 shows that PDMP-instigation is positively correlated with the rate of heroin incidents. This correlation is likely due to an overall upward trend in heroin incidents over time. Column (2) adds county and time fixed effects to the OLS specification, controlling for county levels and a national average trend in heroin incidents, and the point estimate falls to 0.155 additional heroin incidents after the passage of the policy, and this result is statistically insignificant. Column (3) adds county demographic and economic controls (as summarized in Table 1.6), and estimates do not substantially change from the fixed effects specification in Column (2). Column (4) adds county-specific time trends, and estimates become larger in magnitude (0.384) but remain insignificantly different from zero. Column (5) applies the IFE factor model, as outlined in Section 1.4.3, which nests difference-in-differences and time trends while controlling for unobserved confounding variables at the county level. The positive estimate and statistical significance of the factor model's estimate in Column (5) suggests there is some meaningful heterogeneity not being addressed in the difference-in-differences approach at the national level. However, this is only significant at the 10% level and demands confirmation, which will be given below.

As in the state-level models on prescription outcomes, the results for the Mandate and Pill Mill Bill effects on crime rates are volatile across model specifications. In Panels B and C in Table 1.8, Mandate effects on heroin incidents switch signs between the control and linear-time-trend model specifications. This is likely because the effect of the Mandate within the NIBRS-covered counties is identified using changes in the policy across only 8 states. The results for Pill Mill Bills also vary dramatically across specifications, likely because effects are identified using 6 treated states in the NIBRS data. The small sample sizes of too few treated states could be confusing results.

Panel B in Table 1.8 shows that the PDMP has an insignificant effect on the rate of heroin incidents in counties that had a low oxycodone density prior to the policy, and are therefore likely to be less susceptible to the policy. The IFE factor model finds a small significant increase (0.095 additional heroin incidents per 100,000 people per month) in the rate of heroin incidents among the bottom 90% of counties, equal to an 11% increase. Since the difference-in-differences estimates and the IFE factor model estimates are not consistent with one another, it is not certain that there was a change in heroin incidents in the less oxycodone-dense counties. B shows insignificant, near-zero effects of the PDMP in the bottom half of counties by oxycodone density when the more oxycodone-dense half of counties are excluded from the model.

In contrast to the less oxycodone-dense counties, the counties in the top 10% of the distribution, as shown in Panel C of Table 1.8, experience a statistically significant effect of 1.745, 1.69 or 0.972 additional heroin incidents per 100,000 population per month under the specifations with controls and linear time trends, and the IFE factor model specificatio, respectively. Police are encountering 47% to 84% more heroin incidents in these highly susceptible counties, which experience a baseline of 2.07 heroin incidents per 100,000 NIBRS-covered population per month in the year prior to the policy. This large, positive effect of the PDMP in high-density counties is robust across many different estimation specifications.[35]

Figure 1.6 shows the effect over time of the PDMP on the rate of heroin incidents in all counties in the top graph, and in the counties with high oxycodone density in the bottom graph. The event study graphs contain dashed vertical lines that allow for a possible announcement effect during a six month window leading up to

[35]This result is robust to the removal of analytic weights, though somewhat less precise. This result is also robust in poisson regressions and in the context of weighted and unweighted factor models, and results from all models are available upon request.

the effective date of the policy.[36] Consistent with Panels A and C of Table 1.8, the graphs show an increase in heroin incidents after the implementation of the PDMP. The leads on the graphs are close to zero, and support the identifying assumption of the differences-in-differences model that states that treated counties are trending similarly to untreated counties prior to the policy. Post-implementation, the graph line trends upwards, meaning PMDP is causing more heroin incidents in the counties with the highest oxycodone shipments per capita.

Table 1.9 contains estimated effects of the policies on several different drug-crime outcomes, split on high and low oxycodone density. This table contains results from the difference-in-differences model specification without county-specific linear time trends (the "Controls" model from Table 1.8). Again, Panels A-C distinguish types of counties by oxycodone density. Columns (1) through (4) document model coefficient estimates on the rates of heroin incidents (taken from Table 1.8), incidents that involved possible heroin dealers (Column (2)), diverted opiate incidents (Column (3)), and incidents involving possible dealers of diverted opiates (Column (4)).

Panel C shows that in the most oxycodone-dense counties, the incidents with possible heroin dealers increase significantly: 0.324 additional incidents per 100,000 population after the PDMP, equal to a 37% increase from the pre-policy, pre-announcement level of 0.880.

Figure 1.7 displays event studies of the PDMP effect on possible heroin dealers in all counties and in the most oxycodone-dense counties. There is a significant increase in possible heroin dealers in the most opioid-dense counties, but not across all counties.[37] Theory predicts an increase in demand for heroin and quantity traded of heroin, because heroin is a substitute for prescription opioids. I find a significant

[36]Many states began documenting controlled substances in the PDMP system months before the PDMP was accessible by prescribers (the effective date of the policy), perhaps resulting in a slight announcement effect.

[37]As shown in Panel C of Table 1.8 and discussed further B, the effect of the PDMP on heroin outcomes is driven by those counties in the top half of the oxycodone-per-capita distribution.

84% increase in heroin incidents in the most susceptible areas, equal to about 1.75 additional incidents per 100,000 population per month, consistent with predictions.[38]

A crime involving diverted opioids is an incident in the NIBRS in which an offender is carrying prescription opioids for which he or she does not have a prescription. The PDMP's effect on opiate incidents is noisy and has large standard errors, consistent with predictions. It remains noisy and insignificant, often with point estimates near zero, across different model specifications. Close examination of event study graphs of opiate incidents over time do not reveal consistent effects or anything of note for all counties or for the more oxycodone-dense counties. Figure 1.8 shows such graphs. The plotted coefficient points come from the IFE factor model this time because the difference-in-differences event studies do not satisfy the parallel trends identification assumption, even when accounting for linear county-specific time trends. That is, the linear time trends are not enough to capture trends in illegal opioid seizures in the data. Regardless of the model used, the PDMP does not produce significant effects on the rate of diverted opioid incidents. Results on possible opioid dealers are similarly noisy, insignificant, near zero and are not discussed.

Simple theory predicts PDMPs cause an increase in the demand for illegal prescription opiates, but a decrease in supply of illegal prescription opiates (diverted from the market of legal prescription opiates). These opposing market forces lead to a predicted increase in the street price of prescription opioids, but ambiguous effects on the predicted quantity traded. These imprecise, zero estimates of the effect of the PDMP on opiate incidents are not surprising in light of the uncertain theoretical predictions.

[38]The 84% increase estimate is obtained from the analytic-weighted difference-in-differences model with county and month fixed effects and controls. The result that the PDMP causes a large increase in heroin incidents in the most opioid-dense counties is robust across model specifications, including additional difference-in-differences specifications, factor model specifications, and a Poisson framework, all available upon request to the author.

1.5.3 Results from the Interactive Fixed Effects Factor Model

As explained in section 1.4.3, the IFE factor model from Bai (2009) flexibly accounts for nationwide time trends that affect different counties based on unobservable characteristics. Results calculated from the difference-in-differences models and IFE factor model are similar in regressions on prescription outcomes (as seen in A), likely because trends at the state-level are mitigated with aggregation. In contrast, difference-in-differences results and factor model results diverge more in the heroin models because of non-linear time trends at the more disaggregated county-level. When applied to the model on heroin incidents, the factor model produces time trends that appear to fit non-linear county-specific time trends that the difference-in-differences model with county-specific *linear* time trends is not able to capture.

The factor model nests nationwide time trends, and Figure 1.9 graphs a polynomial fit of the nationwide trend in race of heroin incidents by county. Difference-in-differences models are able to pick up this non-linear common time trend in the figure by including time fixed effects. The nationwide time trends in Figure 1.9 does not account for differences in time trends across counties.

Figure 1.10 shows the "Factor 1" time trend from the IFE factor model. Factor 1 is a nationwide time trend experienced differently by individual counties depending on county factor loadings. August 2010 is the month when Purdue Pharmaceutical released the abuse-deterrent reformulation of OxyContin. Notice that Factor 1 shows a non-linear pattern of heroin incidents over time, with a sudden acceleration after 2010. During time periods 0 through 80, which corresponds to the period between January 2004 and August 2010, the rate of heroin incidents increases modestly, and then dramatically after August 2010. In the county-level regressions, I control for county-specific level responses to the tamper-proof reformulation by multiplying a post-August-2010 dummy indicator by each county's pre-reformulation oxycodone density.[39] Controlling for a level shift allows the abuse-deterrent reformulation to

[39] Alpert et al. (2017) and Evans and Power (2017) use a similar method.

affect counties proportional to their likely abuse exposure. However, it appears that controlling for the reformulation in this way does not fit the curvature of heroin incidents after 2010 well, as the factor model's first factor and nationwide time fixed effects trends pick up a dramatic increase in heroin incidents beginning in August 2010.[40] Figure 1.11 contains a map of the NIBRS counties' Factor 1 loadings. The darkest-color counties in Delaware, Oregon, Ohio, West Virginia, and Virginia experience the steepest increases in heroin incidents after 2010.

Counties' Factor 1 loadings are correlated with their 2004 density of ARCOS oxycodone, meaning more opioid-dense counties experience greater heroin transition after 2010. As an illustrative example, I have chosen two example counties, and fit lines to their heroin incident rates over time. Figure 1.12 displays the rate of heroin incidents in Spotsylvania County, VA, which the IFE model had assigned a large factor loading (90th percentile) and Florence County, SC which the IFE model had assigned a typical factor loading (50th percentile). The rate in Spotsylvania County shows more of a non-linear incident pattern, realizing a dramatic increase in the 2010s. Figure 1.13 shows the heroin incident rate over time of the same counties, after removal of the controls and the county and time fixed effects. The figure approximates what the difference-in-differences model is left to fit with county-specific linear time trends after other covariates and fixed effects are controlled for. A linear trend fit to Spotsylvania's heroin incidents will provide a poor fit, and it biases the coefficient estimates of the PDMP upward.[41] The counties with large factor 1 loadings experience a sharp increase in heroin incidents in later time periods, and the difference-in-differences models with linear time trends will fit linear trends to counties partially based on the shallower slope in heroin incidents between 2004 and 2010. The increase in heroin incidents after 2010 will fall above the trend, and may be falsely attributed to the PDMP.

[40]Factor 1 is by construction orthogonal to the variable that proxies the OxyContin reformulation, and is perhaps picking up additional unexplained variation across counties not captured by the proxy.

[41]Virginia's PDMP was implemented in June 2016, corresponding to time period 30 and South Carolina's PDMP was implemented in June 2008, corresponding to time period 55.

Table 1.10 compares the results of various difference-in-difference models with those of the IFE factor model. The coefficients resulting from the difference-in-differences models under linear time trends is 0.384, larger than the model without time trends (0.239). Adding quadratic and cubic county-specific time trends for the regressions on all counties results in a PDMP coefficient estimate of 0.108 additional heroin incidents per 100,000 population per month, which is very close to the IFE factor model estimates (0.112) because the county-specific polynomials capture the curvature in heroin incidents within counties.

1.6 Additional Robustness Checks

1.6.1 Placebo Test and Wild Cluster Bootstrap

Due to concerns about autocorrelation and few treated states in the panel data, wild cluster bootstrapped p-values are used to draw inference for all main results. Coefficients on the PDMP remain significant for regressions on Medicaid oxycodone, Medicaid strong oxycodone, ARCOS oxycodone, and heroin incidents among oxycodone-dense counties.

Table 1.11 displays rejection rates from a placebo test as suggested in Bertrand et al. (2004). Concerns about autocorrelation are especially pertinent to difference-in-differences regressions on addictive opioid drugs, which have a highly correlated temporal pattern. Using the state-quarter Medicaid data and county-month crime rate data, I randomly assign fake PDMP, Mandate and Pill Mill Bill laws to states for any time between 2004 and 2014, with probability equal to the relative frequency of the real policies in the data. I then run my models on the data with the placebo policies to test rejection rates. Fictitious placebo laws should be significant at the 5% level 5% of the time. Table 1.11 shows that difference-in-differences over-rejects the null hypothesis of zero effect for all policies, to varying degrees. The problem is most acute for the Mandate policy and the Pill Mill Bill regulation, with rejection rates around 20% and 35%, respectively, likely because of few treated states for either policy. Rejection rates of the placebo PDMP policy range from 6% to 30%, with the main prescription results on oxycodone only slightly over-rejecting at the 6-8% level. This may mean that in this study, difference-in-differences estimates are overly lax in rejection.

To remedy the over-rejection problem, I use the Wild Cluster Bootstrap t-statistic-percentile procedure outlined in Cameron, Gelbach, Miller (2008).[42] P-values obtained from this procedure are included in brackets for key results in Table 1.7, Table 1.8 and Table 1.9. IFE factor model results are cluster-bootstrapped as well.

1.7 Conclusion

Opioids are highly addictive and foster dependence among individuals taking high doses. When abusers' supply of prescription opioids is cut off, some may turn to heroin or illegally diverted opioids to avoid the undesirable physical symptoms of opioid withdrawal.

Every state established electronic prescription drug monitoring programs between 2004 and 2017 to limit prescribing of opioids to those with patterns of abuse. Nationwide, PDMPs cause an 8% reduction in prescription oxycodone quantities, and an 11% increase in heroin crime, although this result is statistically insignificant. Prescription monitoring has larger effects on prescriptions in the Medicaid population and causes a statistically signficant 25% reduction in oxycodone prescribed, which is driven by an even larger 35% decrease in high-dosage pills. Heroin crime results are driven by the counties that have the highest pre-PDMP oxycodone per capita, which

[42]This procedure involves taking the residuals of a model run without the independent variables of interest (in my case, the PDMP, Mandate and Pill Mill Bill) and randomly reassigning them within treated clusters. The residual randomization disrupts the autocorrelation in the error term within clusters that causes over-rejection of the null. The procedure then runs the difference-in-differences regression model on the data with the randomly-ordered residuals, and, bearing similarities to a placebo test, obtains a distribution of t-statistics under the meaningless data. The real t-statistic is compared to the distribution of bootstrapped t-statistics and is assigned a p-value equal to its percentile within the distribution.

is consistent with substitution to heroin in response to the policy. The PDMP causes a 47% to 84% increase in heroin incidents within the most oxycodone-dense counties.

This paper contributes to the literature on the effects of legislation that reduces the supply of opioids, and finds evidence of substitution behavior in response to PDMPs. The results show heterogeneous effects of PDMPs within state populations, a possible explanation for the mixed, often statistically insignificant results in the PDMP literature. When focusing on the high abuse Medicaid enrollee subsection of the population and disaggregating oxycodone by pill strength, evidence here supports that PDMPs successfully limit the supply of opioids to the heaviest abusers.

Disaggregating Medicaid data on drug level allows me to identify heterogeneous policy effects on drugs with differing amounts of oxycodone. Using county-month level crime data, I am able to find heterogeneity of PDMP effectiveness within state populations. Disaggregating outcomes to the county level allows for a better examination of high-abuse populations, because of differences in opioid abuse across counties within states.

The opioid epidemic costs the U.S. an estimated $78.5 billion annually. Policymakers have primarily used supply-side policy levers in attempts to reduce the flow of new opioid addicts. However, supply-side policies haven't properly accounted for substitution responses among the stock of existing opioid-dependent individuals. Future supply-side interventions should provide alternative options for those already in the throes of addiction, or simultaneously target alternate sources of opioids.

Table 1.1.
Effective Dates of Electronic PDMPs, Mandates, and "Pill Mill" Legislation

State	PDMP Date	Mandate Date	"Pill Mill" Bill Date
Alaska	January 2012		
Alabama	August 2007		
Arkansas	March 2013		
Arizona	December 2008		
California	July 2009		
Colorado	February 2008		
Connecticut	July 2008		
Delaware	August 2012	March 2012	
Florida	October 2011		July 2011
Georgia	July 2013		
Hawaii	January 1982		
Iowa	March 2009		
Idaho	July 2008		
Illinois	Janurary 2008		
Indiana	July 2008		
Kansas	April 2011		
Kentucky	March 2005	July 2012	July 2011
Louisiana	January 2009	August 2014	July 2005
Massachusetts	December 2010	June 2013	
Maryland	January 2014		
Maine	January 2005		
Michigan	March 2011		
Minnesota	April 2010		
Missouri	July 2017		
Mississippi	March 2011		September 2011
Montana	October 2012		
North Carolina	October 2008		
North Dakota	January 2007		
Nebraska	April 2011		
New Hampshire	October 2014		
New Jersey	January 2012		
New Mexico	August 2005	September 2012	
Nevada	October 2004	October 2007	
New York	August 2013	August 2013	
Ohio	October 2006	November 2011	May 2011
Oklahoma	July 2006		
Oregon	September 2011		
Pennsylvania	August 2016		
Rhode Island	September 2012		
South Carolina	June 2008		
South Dakota	March 2012		
Tennessee	December 2006	January 2013	January 2012
Texas	August 2012		June 2009
Utah	January 2006		
Virginia	June 2006		
Vermont	April 2009	November 2013	
Washington	January 2012		
Wisconsin	May 2013		
West Virginia	January 2004	June 2012	September 2014
Wyoming	July 2004		

Dates obtained from the National Alliance for Model State Drug Laws, Brandeis University's Prescription Drug Monitoring Program Training and Technical Assistance Center, state legislative laws and bills, government newsletters, news articles, articles from peer reviewed journals, and pharmacy board websites.

Table 1.2.
Summary Statistics on Opioid Abuse of Individuals in the NSDUH

	All Respondents	Hydrocodone Abusers	Oxycodone Abusers	OxyContin Abusers
Non-Medicaid Population				
Fraction Abused Opioids	0.110	1	1	1
Past Year Frequency Opioids	2.029	20.190	22.822	40.453
Fraction Abused Heroin	0.011	0.084	0.114	0.197
Past Year Frequency Heroin	0.174	1.766	2.426	5.616
Fraction Abused Hydrocodone	0.077	1	0.663	0.897
Fraction Abused Oxycodone	0.056	0.481	1	1
Fraction Abused OxyContin	0.019	0.226	0.348	1
Observations	915,123	70,637	51,222	17,837
Medicaid Population				
Fraction Abused Opioids	0.127	1	1	1
Past Year Frequency Opioids	3.303	28.889	32.41	52.100
Fraction Abused Heroin	0.015	0.108	0.146	0.234
Past Year Frequency Heroin	0.289	2.636	3.847	7.143
Fraction Abused Hydrocodone	0.078	1	0.688	0.879
Fraction Abused Oxycodone	0.057	0.503	1	1
Fraction Abused OxyContin	0.022	0.257	0.400	1
Observations	163,528	12,756	9,323	3,725

The table displays summary statistics from the National Survey on Drug Use and Health 1990-2014. For the Non-Medicaid and Medicaid Population, indicators for and frequency of opioid abuse are reported for all survey respondents, survey respondents who report having ever abused hydrocodone, oxycodone or OxyContin. Medicaid enrollees report higher rates of abuse than those not enrolled in Medicaid, and respondents who report abusing OxyContin and oxycodone report more frequent misuse of opioids.

Table 1.3.
Summary Statistics of ARCOS and Medicaid Drug Amounts

	ARCOS Data		Medicaid Data	
	Morph. Units (Millions)	Morph. Units Per Capita	Morph. Units (Millions)	Morph. Units Per Capita
Oxycodone	312.5	55.54	25.90	52.24
Oxycodone: Weak Dose	–	–	9.083	17.53
Oxycodone: Strong Dose	–	–	16.81	34.71
Hydrocodone	149.4	24.68	7.377	11.44
Hydrocodone: Weak Dose	–	–	7.377	11.44
Hydrocodone: Strong Dose	–	–	–	–
Observations	5100	5100	5100	5100

Panel Data is by state and quarter. Data is in morphine-equivalent milligrams of oxycodone and hydrocodone. Strong dose pills are pills containing more than 15 morphine equivalent milligrams of the active opioid painkiller. Hydrocodone does not come in tablets containing more than 15 morphine equivalent milligrams. The ARCOS data contains information on aggregate shipped amounts of oxycodone and hydrocodone, and the Medicaid drug data contains information at the drug level, which is aggregated by strength.

Table 1.4.
Summary Statistics of Crime Rates Per 100,000 Population

	N	Mean	Std. Error
All 735 Counties			
Heroin Incidents	93,742	1.299	2.716
Opiate Incidents	93,742	2.175	4.533
Heroin Dealer	93,742	0.502	1.290
Opiate Dealer	93,742	0.523	2.604
655 Low Oxycodone Density Counties			
Heroin Incidents	86,232	1.124	2.481
Opiate Incidents	86,232	1.866	3.792
Heroin Dealer	86,232	0.426	1.199
Opiate Dealer	86,232	0.432	2.202
80 High Oxycodone Density Counties			
Heroin Incidents	10,548	2.342	3.655
Opiate Incidents	10,548	4.009	7.300
Heroin Dealer	10,548	0.949	1.663
Opiate Dealer	10,548	1.060	4.233

Panel Data is by county and month. 735 counties across 26 states have complete monthly coverage within the NIBRS dataset during the entire period of 2004 to 2014. Only counties with full coverage are used in the crime rate analysis.

Table 1.5.
Summary Statistics of Controls for State Level Models

	N	Mean	Std. Error
Data: Census Bridged Population Estimates			
Fraction Aged 10-19	3,204	0.1396	0.0090
Fraction Aged 20-29	3,204	0.1383	0.0093
Fraction Aged 30-39	3,204	0.1362	0.0112
Fraction Aged 40-49	3,204	0.1445	0.0101
Fraction Aged 50-59	3,204	0.1297	0.0115
Fraction Aged 60-69	3,204	0.0885	0.0150
Fraction Aged 70+	3,204	0.0916	0.0246
Fraction Female	3,204	0.509	0.0056
Fraction Black	3,204	0.1326	0.0866
Fraction Hispanic	3,204	0.1484	0.1271
Fraction Other Non-White	3,204	0.0627	0.0441
Data: BLS Quarterly Census of Employment and Wages			
Fraction Employed Manufacturing	3,204	0.1236	0.0441
Fraction Employed Agriculture	3,204	0.0116	0.0108
Data: BLS Local Area Unemployment Statistics			
Unemployment Rate	3,204	0.0817	0.0405
Data: Census Historical Poverty Tables			
Poverty Rate	3,204	0.1363	0.0293
Data: Bureau of Economic Analysis			
Income Per Capita	3,204	$38,867	$7,867
Data: Medicaid Drug Utilization Data			
OxyContin mgs per Enrollee (2004)	3,204	31.39	17.46
Medicaid Pills Per Enrollee	3,204	23.297	13.64
Data: Centers for Medicare and Medicaid Services			
Fraction Medicare Enrolled	3,204	0.157	0.0221

Panel Data is by state and quarter. Income per capita is per year, and OxyContin milligrams per capita and Medicaid pill per enrollee are quarterly.

Table 1.6.
Summary Statistics of Controls for County Level Models

	N	Mean	Std. Error
All 735 Counties			
Data: Census Bridged Population Estimates			
Fraction 10-19	92,292	0.1387	0.0139
Fraction 20-29	92,292	0.1348	0.0357
Fraction 30-39	92,292	0.1279	0.0167
Fraction 40-49	92,292	0.1437	0.0174
Fraction 50-59	92,292	0.1376	0.0160
Fraction 60-69	92,292	0.0955	0.0202
Fraction 70+	92,292	0.0925	0.0246
Fraction Female	92,292	0.5087	0.0127
Fraction Black	92,292	0.1181	0.1268
Fraction Hispanic	92,292	0.0687	0.0629
Fraction Other Non-White	92,292	0.0358	0.0370
Fraction 65+	92,292	0.1288	0.0389
Data: BLS Quarterly Census of Employment and Wages			
Fraction Employed Manufacturing	92,292	0.1479	0.0979
Average Week Wage	92,292	$790.70	$219.83
Pharmacies per 1,000 pop	92,292	1.64	0.738
Data: BLS Local Area Unemployment Statistics			
Unemployment	92,292	0.0551	0.0224
Data: Drug Enforcement Administration ARCOS Files			
Pre-2010 Oxycodone per capita	57,591	52.168	34.188
Data: FBI Uniform Crime Reporting LEOKA			
Officers per 1,000 pop	92,292	17.93	0.041

Panel Data is by county and month. 735 counties across 26 states have complete monthly coverage within the NIBRS dataset during the entire period of 2004 to 2014. Only counties with full coverage are used in the crime rate analysis.

Table 1.7.
The Effect of Policies on Logged Prescription Amounts per Capita

	Medicaid Data				ARCOS Data	
	(1)	(2)	(3)	(4)	(5)	(6)
	Oxycodone	Weak Oxycodone	Strong Oxycodone	Hydrocodone	Oxycodone	Hydrocodone
PDMP	-0.246*	-0.0813	-0.350**	-0.0530	-0.0814*	-0.0041
	(0.128)	(0.146)	(0.151)	(0.146)	(0.135)	(0.0263)
	[0.087]	[0.286]	[0.033]	[0.359]	[0.065]	[0.519]
Mandate	0.342**	-0.247	0.344***	-0.208*	0.157**	-0.165***
	(0.145)	(0.164)	(0.145)	(0.184)	(0.0589)	(0.0390)
	[0.989]	[0.844]	[0.992]	[0.123]	[0.99]	[0.001]
Pill Mill Bill	-0.190	-0.238	-0.185	0.0843	-0.176**	-0.0129
	(0.156)	(0.110)	(0.173)	(0.192)	(0.101)	(0.0506)
	[0.283]	[0.422]	[0.188]	[0.653]	[0.028]	[0.558]
Observations	2714	2713	2692	2714	3070	3066
Fixed Effects	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Linear Trends						
Medicaid Weights	X	X	X	X		
Population Weights					X	X

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data is by state and quarter. Standard errors in parentheses, clustered by state.
Wild cluster bootstrapped p-values in brackets.
The PDMP, Mandate, and Pill Mill rows contain coefficient estimates for variables indicating the timing of Prescription Drug Monitoring Programs, a Mandate that requires practitioners to check the PDMP, or a "Pill Mill" Bill that imposes many strict regulations on clinics that prescribe and dispense opioids on site.
Columns (1), (2), (3), and (4) show the effect of the PDMP on oxycodone, weak dose oxycodone, strong dose oxycodone, and hydrocodone per Medicaid enrollee in the Medicaid data. Columns (5) and (6) display the effect of the PDMP on ARCOS aggregate oxycodone and hydrocodone shipments per capita.
Weak dose oxycodone has 10 or fewer milligrams per pill; strong dose oxycodone has greater than 10 milligrams per pill.

Table 1.8.
The Effect of Policies on Heroin Incidents Per Capita, Across Model Specifications

	OLS	FE	Controls	LTT	Factor
Panel A: All 735 Counties					
PDMP	0.466***	0.155	0.239	0.384	0.112*
	(0.0382)	(0.230)	(0.288)	(0.361)	(0.059)
			[0.654]		[0.058]
Mandate	3.774***	1.337	0.945	0.0919	0.123
	(0.226)	(1.050)	(0.666)	(0.251)	(0.308)
			[0.881]		[0.689]
Pill Mill Bill	-1.597***	-0.519	-0.271	0.169	0.111
	(0.181)	(0.867)	(0.702)	(0.230)	(0.312)
			[0.365]		[0.722]
Observations	92292	92292	92292	92292	92292
Panel B: Bottom 90% of Oxycodone Density Counties					
PDMP	0.672***	-0.0767	-0.0306	-0.0256	0.095**
	(0.0359)	(0.0700)	(0.110)	(0.0889)	(0.045)
			[0.236]		[0.036]
Mandate	1.689***	0.178	-0.167	0.0674	-0.023
	(0.202)	(0.822)	(0.623)	(0.278)	(0.137)
			[0.449]		[0.869]
Pill Mill Bill	0.623***	0.752	0.976	0.333*	0.136
	(0.150)	(0.852)	(0.763)	(0.164)	(0.273)
			[0.794]		[0.618]
Observations	82704	82704	82704	82704	82704
Panel C: Top 10% of Oxycodone Density Counties					
PDMP	0.0462	1.249	1.745*	1.690**	0.972***
	(0.139)	(0.821)	(0.795)	(0.745)	(0.303)
			[0.915]		[0.001]
Mandate	5.545***	2.386*	1.115***	-0.497	2.003***
	(0.312)	(1.062)	(0.327)	(0.413)	(0.661)
			[0.999]		[0.002]
Pill Mill Bill	-6.104***	-3.189**	-1.928*	-0.606	-1.174**
	(0.301)	(1.295)	(0.858)	(0.726)	(0.551)
			[0.026]		[0.033]
Observations	9588	9588	9588	9588	9588
Fixed Effects		X	X	X	
Controls			X	X	
Linear Time Trends				X	$\hbar$
Population Weights	X	X	X	X	X
Factor Model					X
Cluster Bootstrap			X		X

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parentheses and are clustered on the treament level (state). Wild cluster boostrap p-values are listed in brackets.
Panel A shows coefficients on policies when models are run on all 735 counties. Panel B and Panel C show heterogeneity of policy effects across counties depending on pre-policy oxycodone milligrams per capita. Panel B shows the coefficients of the models run on a subsample of the data containing only the bottom 90% of oxycodone-dense counties, and Panel C shows results from models run on the top 10% most oxycodone-dense counties.
Data source: NIBRS 2004-2014.
$\hbar$: The IFE Factor Model nests fixed effects and county-specific linear time trends.

Table 1.9.
The Effect of the PDMP on Drug Crimes Per Capita

	Heroin		Opiates	
	Incidents	Possible Dealers	Incidents	Possible Dealers
Panel A: All 735 Counties				
PDMP	0.239	0.013	-0.162	-0.0174
	(0.288)	(0.0672)	(0.0956)	(0.0257)
	[0.654]	[0.430]	[0.243]	[0.246]
Mandate	0.945	0.160*	0.147	0.0781
	(0.106)	(1.050)	(0.195)	(0.0685)
	[0.881]	[0.925]	[0.589]	[0.721]
Pill Mill Bill	-0.271	-0.231*	-0.325	-0.124
	(0.702)	(0.110)	(0.344)	(0.0639)
	[0.365]	[0.062]	[0.622]	[0.385]
Observations	24780	24384	24384	24384
Panel B: Low Oxycodone Density Counties				
PDMP	-0.031	-0.0317	-0.651	0.014
	(0.288)	(0.0483)	(0.0774)	(0.0248)
	[0.236]	[0.237]	[0.441]	[0.514]
Mandate	-0.167	-0.224	0.437**	0.224**
	(0.623)	(0.136)	(0.347)	(0.0964)
	[0.449]	[0.257]	[0.983]	[0.964]
Pill Mill Bill	0.976	0.111	-0.284	-0.222
	(0.763)	(0.127)	(0.476)	(0.0911)
	[0.794]	[0.688]	[0.674]	[0.515]
Observations	21096	20964	20964	20964
Panel C: High Oxycodone Density Counties				
PDMP	1.745*	0.324*	-0.547	-0.248
	(0.795)	(0.140)	(0.213)	(0.0971)
	[0.915]	[0.918]	[0.131]	[0.150]
Mandate	1.115***	0.374**	-0.378	-0.237
	(0.327)	(1.050)	(0.208)	(0.103)
	[0.999]	[0.978]	[0.139]	[0.204]
Pill Mill Bill	-1.597**	-0.601**	-1.160*	-0.465*
	(0.181)	(0.235)	(0.249)	(0.329)
	[0.026]	[0.010]	[0.078]	[0.096]
Observations	3684	3420	3420	3420

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parentheses, clustered by state. Wild cluster p-values in brackets. Difference-in-differences regression model specification includes county and month fixed effects, county controls, and population weights.
In Panel B and Panel C, the data are subdivided into the bottom 90% of least oxycodone dense counties and the top 10% of most oxycodone dense counties. Crime data: NIBRS 2004-2014. Oxycodone density data: DEA ARCOS.

Table 1.10.
Effect of PDMP on Heroin Incidents: Comparison of Models

	Difference-In-Differences			IFE Factor Model	
	Controls	LTT	PTT	Factor	Wt. Factor
Panel A: All Counties					
PDMP	0.239	0.384	0.108	0.112*	0.138**
	(0.228)	(0.361)	(0.081)	(0.059)	(0.057)
Mandate	0.945	0.092	-0.036	0.123	0.485
	(0.666)	(0.666)	(0.143)	(0.308)	(0.402)
PillMill	-0.271	0.169	-0.036	0.111	0.114
	(0.702)	(0.230)	(0.131)	(0.312)	(0.461)
Observations	92292	92292	92292	92292	92292
Panel B: Top 10% Oxycodone Density Counties					
PDMP	1.745*	1.690**	0.412	0.927***	0.949***
	(0.795)	(0.745)	(0.496)	(0.303)	(0.304)
Mandate	1.115**	-0.497	-0.097	1.990***	2.003***
	(0.327)	(0.413)	(0.311)	(0.664)	(.661)
PillMill	-1.928*	-0.606	-0.598	-1.154***	-1.174***
	(0.858)	(0.726)	(0.383)	(0.547)	(0.551)
Observations	9588	9588	9588	9588	9588
Fixed Effects	X	X	X	$\hbar$	$\hbar$
Controls	X	X	X	X	X
Popln. Weight	X	X	X		X
Linear Time Trends		X	X	$\hbar$	$\hbar$
Quadratic Time Trends			X	$\hbar$	$\hbar$
Cubic Time Trends			X	$\hbar$	$\hbar$

$\hbar$: The IFE Factor Model nests fixed effects and county-specific polynomial time trends. The "controls" specification includes county demographic and economic controls, as well as county and time fixed effects. The "LTT" specification adds county-specific linear time trends, and "PTT" adds county-specific polynomial time trends by controlling for a quadratic and cubic time trend within counties.

Table 1.11.
Rejection Rates Under Placebo Test at 5% Level

Policy:	PDMP	Mandate	Pill Mill Bill
Medicaid and ARCOS Prescription Outcomes			
Medicaid Oxycodone	0.084	0.163	0.321
Medicaid Weak Oxycodone	0.118	0.236	0.352
Medicaid Strong Oxycodone	0.079	0.160	0.315
Medicaid Hydrocodone	0.137	0.227	0.389
ARCOS Oxycodone	0.058	0.155	0.317
ARCOS Hydrocodone	0.147	0.222	0.360
Drug Crime Outcomes			
Heroin Incidents	0.089	0.150	0.389
Heroin Dealers	0.083	0.119	0.334
Opiate Incidents	0.303	0.123	0.349
Opiate Dealers	0.091	0.082	0.380

The PDMP, Mandate, and Pill Mill Bill dates were randomly reassigned to take effect in a pre-PDMP time period The prescription regression model run includes state and quarter fixed effects, controls, Medicaid enrollment weights and linear time trends. The drug crime regression models include county and month fixed effects and cont and dont include county trends. Rejection rates are from regression models using cluster robust weighting.

Table 1.12.
Weighted Poisson Regression: Effect of PDMP on Prescription Outcomes

	Med Oxy	Med Weak Oxy	Med Strong Oxy	ARCOS Oxy
PDMP	-0.212***	-0.074***	-0.275***	-0.084**
	(0.058)	(0.024)	(0.073)	(0.029)
Mandate	-0.239*	-0.304**	-0.148	-0.052
	(0.134)	(0.135)	(0.118)	(0.037)
Pill Mill Bill	0.079	0.064	0.026	-0.051
	(0.110)	(0.073)	(0.109)	(0.122)
Observations	3070	3070	3070	3070

Table 1.13.
Poisson Regression: Effect of PDMP on Heroin Incidents

	Count	Rate per 100,000
Panel A: All Counties		
PDMP	0.123*	0.1833**
	(0.086)	(0.092)
Mandate	-0.047	0.070
	(0.104)	(0.138)
Pill Mill Bill	0.051	0.326
	(0.212)	(0.277)
Observations	67,092	66,948
Panel B: Top 10% Oxycodone Dense Counties		
PDMP	0.231**	0.380
	(0.118)	(0.278)
Mandate	0.131	0.497***
	(0.093)	(0.164)
Pill Mill Bill	-0.450	-0.734**
	(0.277)	(0.301)
Observations	8,088	8,076

Robust errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

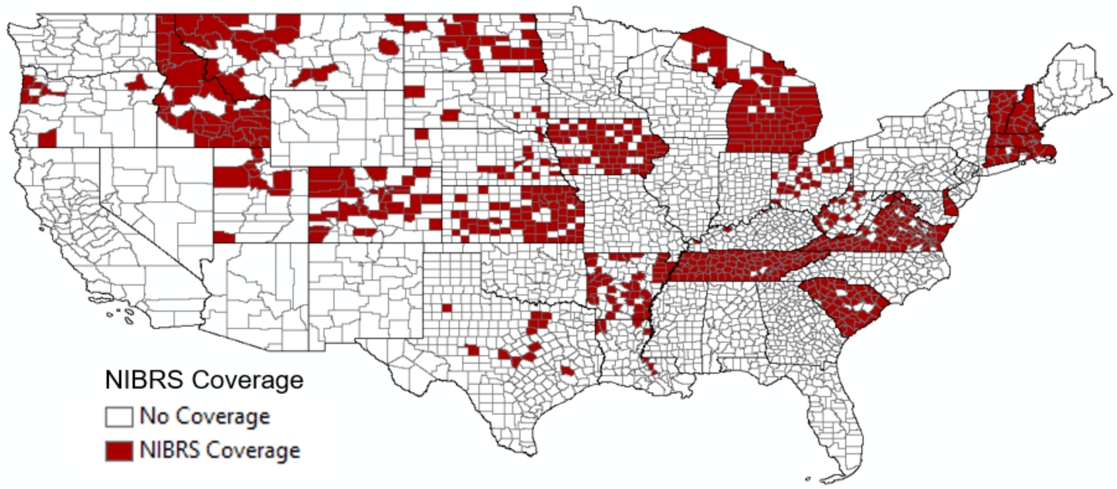

Fig. 1.1. A Map of NIBRS Data Coverage

Notes:The map shows the 735 counties for which there exists a complete monthly panel dataset of counts of crimes from 2004 to 2014 within the NIBRS dataset.

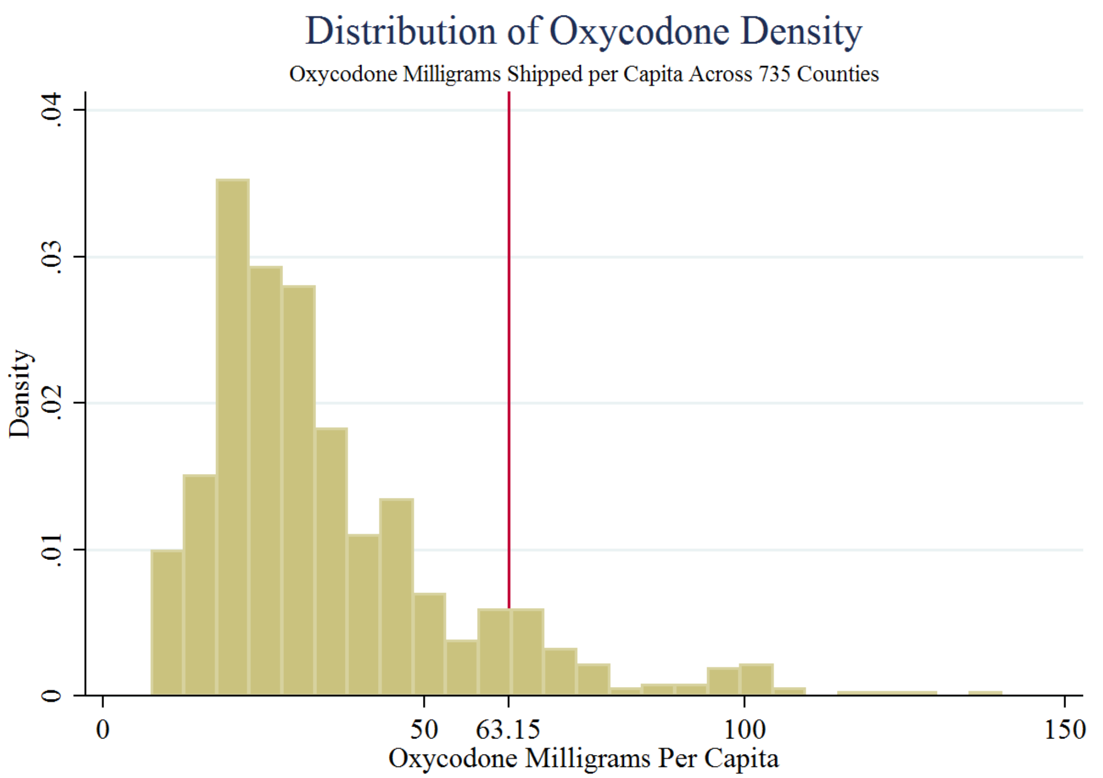

Fig. 1.2. The Distribution of Oxycodone Per Capita Across Counties

Notes: The figure plots the distribution of 2004 oxycodone density across 735 counties. The top 10% most oxycodone dense counties have greater than 63.15 milligrams of oxycodone per capita per month, equivalent to 6-12 weak dose pills or 2-3 strong dose pills per month for each resident. The PDMP has larger effects on counties that have higher pre-policy (year 2004) oxycodone density. Heroin incident data: NIBRS. Oxycodone density data: DEA ARCOS.

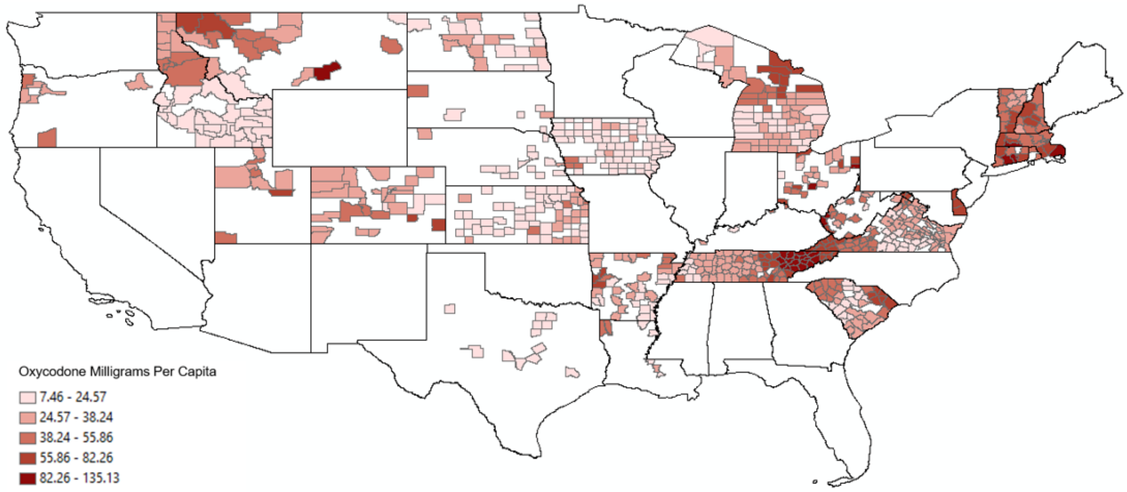

Fig. 1.3. NIBRS County Oxycodone Density

The figure displays the NIBRS-covered counties colored by oxycodone milligrams per person. Darker counties are more oxycodone dense. Oxycodone density data: DEA ARCOS.

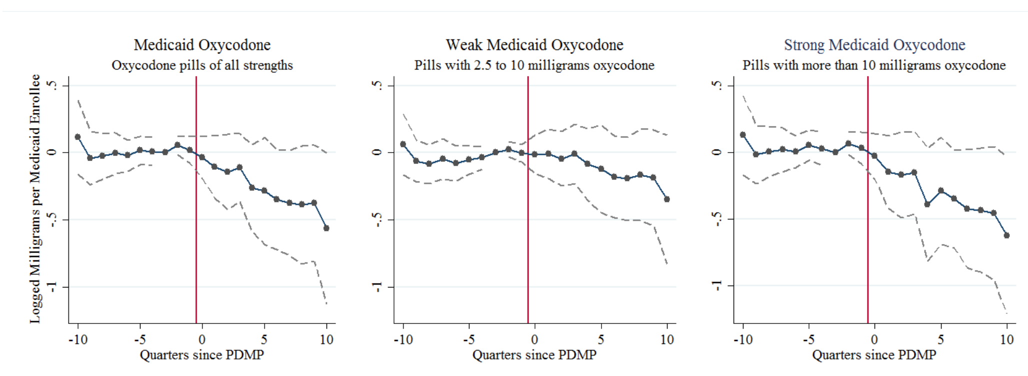

Fig. 1.4. PDMP on Medicaid Oxycodone Outcomes Over Time

Notes: The figures plot coefficients on lag and lead policy indicators from difference-in-differences models on logged amounts of oxycodone by Medicaid prescriptions (milligrams per capita). The dependent variable is restricted to weak dose oxycodone in the center graph and strong dose oxycodone in the right graph. The graphs correspond to event-study adaptations of Columns (1), (2) and (3) of Table 1.7 and models include state and time fixed effects, controls, population weights, and state-specific linear time trends. Data spans 50 states plus the District of Columbia quarterly from 2000-2015. Prescription Data: Medicaid SDUD

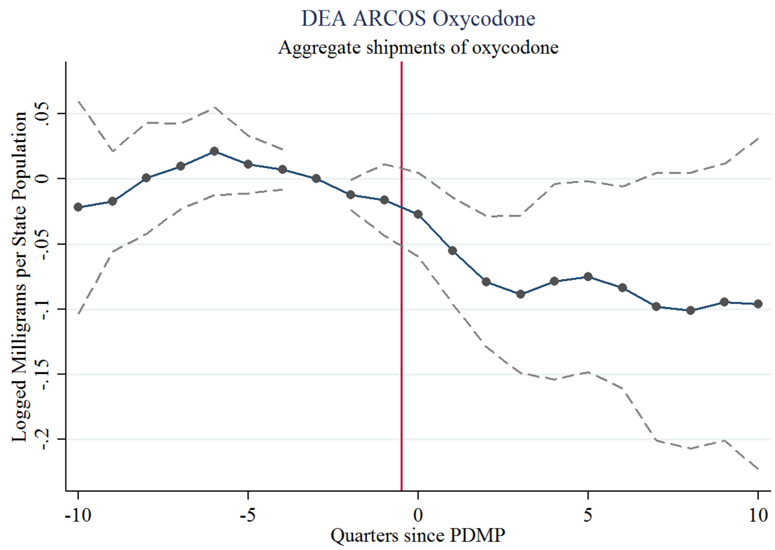

Fig. 1.5. PDMP on Aggregate Oxycodone Shipments

Notes: Same as Figure 4, except using aggregate shipments of oxycodone from ARCOS. The trends graphs correspond to Column (5) of Table 1.7 and includes state and time fixed effects, controls, population weights, and state-specific linear time trends. The dataset spans 50 states plus the District of Columbia quarterly from 2000-2015. Aggregate Shipment Data: DEA ARCOS

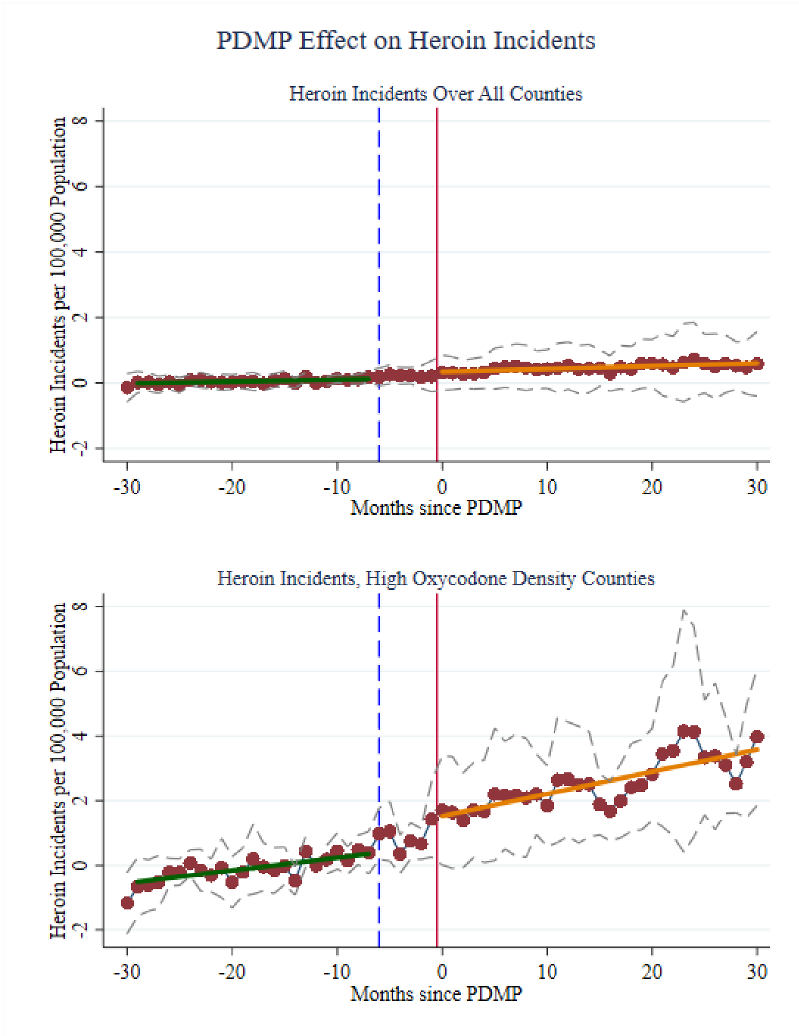

Fig. 1.6. PDMP on Heroin Incidents Over Time

Notes: Graphs plot the coefficients on PDMP lags and leads indicators in a difference-in-differences regression on heroin incidents per 100,000 in a county-month pair. The top graph shows the event study of the PDMP on heroin incidents across all counties. The lower graph shows the event study of the PDMP on heroin incidents in the most oxycodone-dense counties. Event study regressions include month and county fixed effects, controls, and county-specific linear time trends and population analytic weights. The county data spans 735 counties over 26 states monthly from 2004-2014. Heroin incident data: NIBRS. Oxycodone density data: DEA ARCOS.

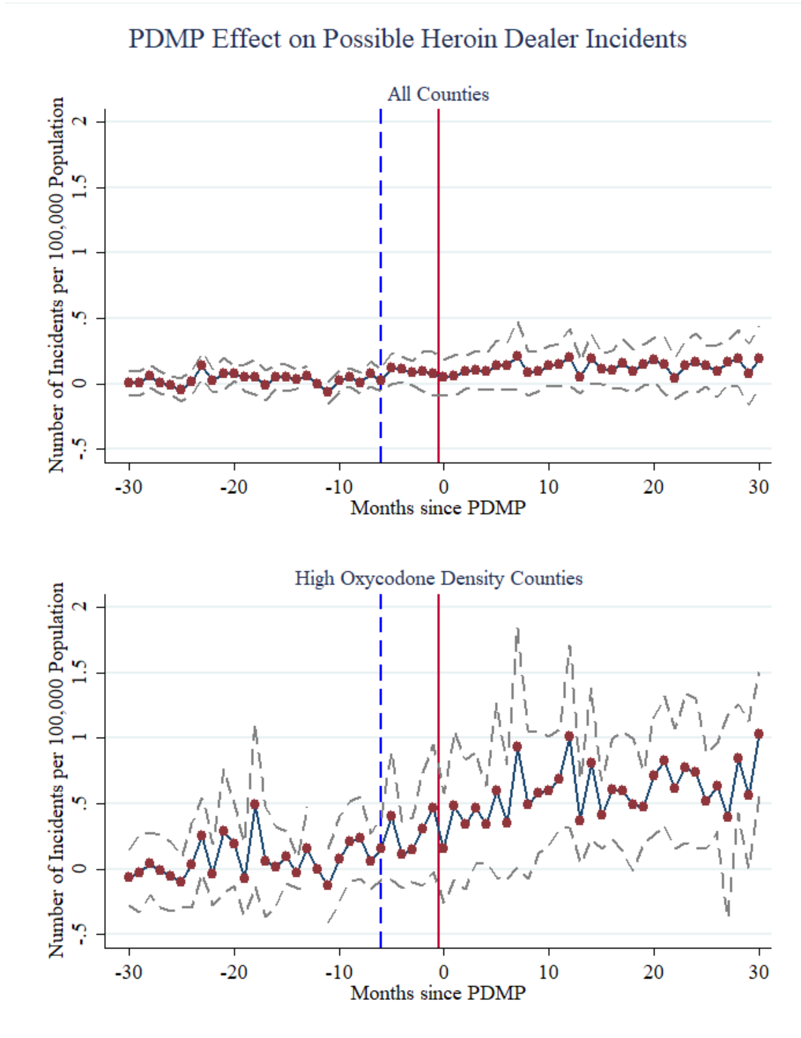

Fig. 1.7. The Effect of the PDMP on Possible Heroin Dealers

The event study graphs plot the effect of the PDMP on the rate over time of incidents involving possible heroin dealers in all counties and in counties with high oxycodone density. A possible heroin dealer incident is one where individuals 1.) are carrying more than 2 grams of heroin, 2.) Are carrying between 1 and 2 grams of heroin and a large amount of another drug, or 3.) Are carrying any heroin and were entered in the data as selling any drug. Weighted regressions include county and time fixed effects, controls, and county-specific linear time trends. Data source: NIBRS.

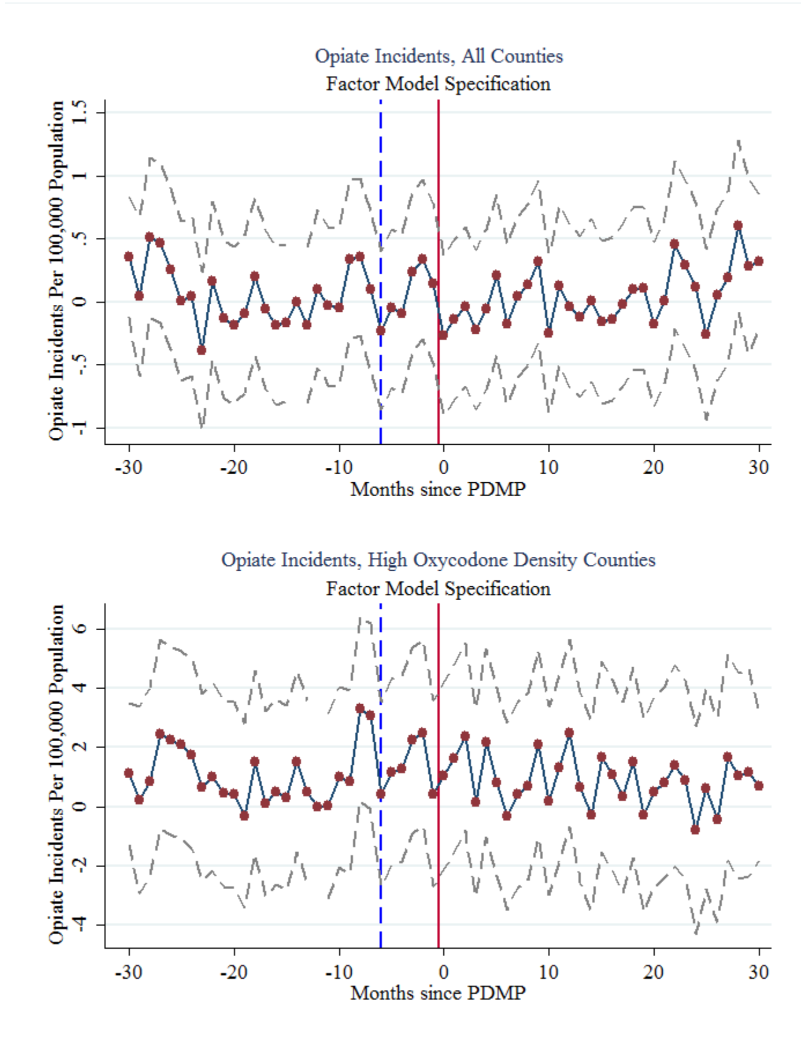

Fig. 1.8. The Effect of the PDMP on Opiate Incidents

The graphs display the event study of the PDMP on Opiate Incidents per 100,000 population. The factor model is used because difference-in-differences specifications do not pass the parallel trends test, due to non-linear county-specific time trends that are captured using the factor model.

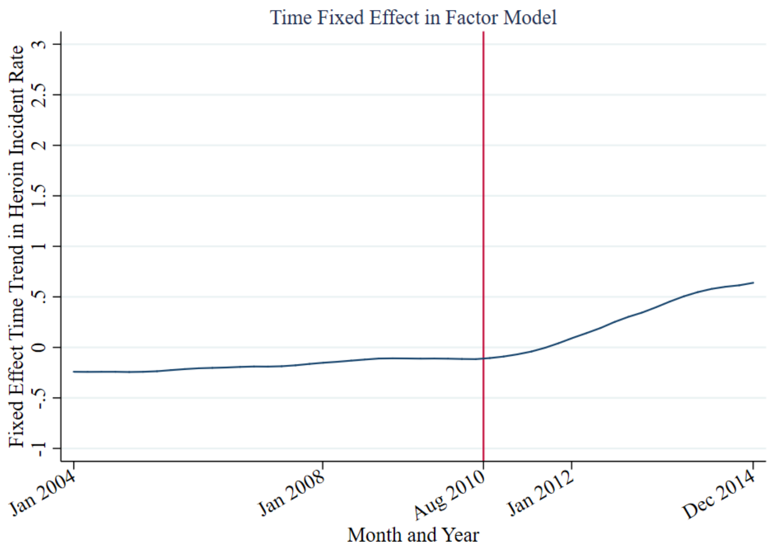

Fig. 1.9. The Nationwide Time Trend in Heroin Incidents, Obtained from the IFE Factor Model

Notes: The figure shows the average time trend (time fixed effects) in the heroin incident rate from the IFE factor model. Heroin incident data: NIBRS.

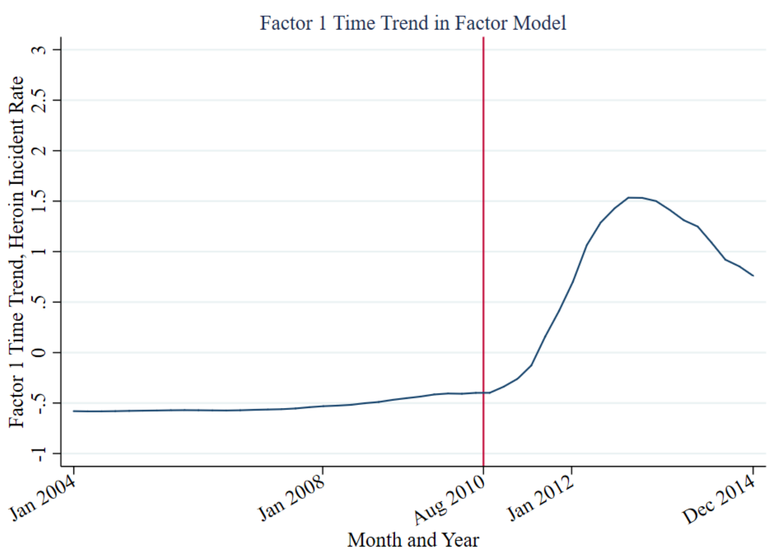

Fig. 1.10. Factor 1 From the Interactive Fixed Effect Factor Model on Heroin Incident Rate

Notes: The graph plots the IFE factor model's factor 1 time trend. The red line marks the OxyContin reformulation that made it harder to abuse. Within the IFE factor model, Factor 1 is the time trend that accounts for the most residual variance.

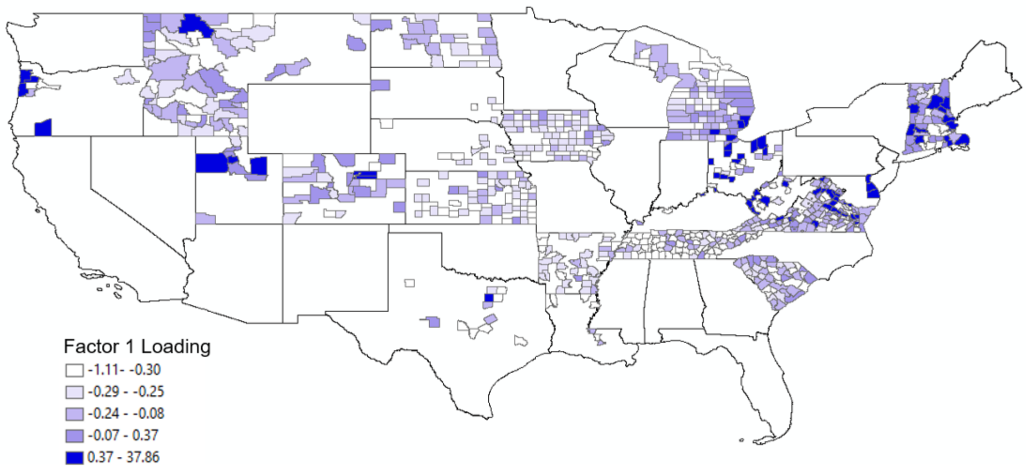

Fig. 1.11. Counties by Factor 1 Loadings

Notes: The map displays counties from the NIBRS data colored by each county's sensitivity to the Factor 1 time path from the interactive fixed effects factor model as shown in Figure 1.10. Factor 1 seems to pick up differences in county responses to the OxyContin reformulation, and the dark-colored counties perhaps have exceptional sensitivity to the reformulation.

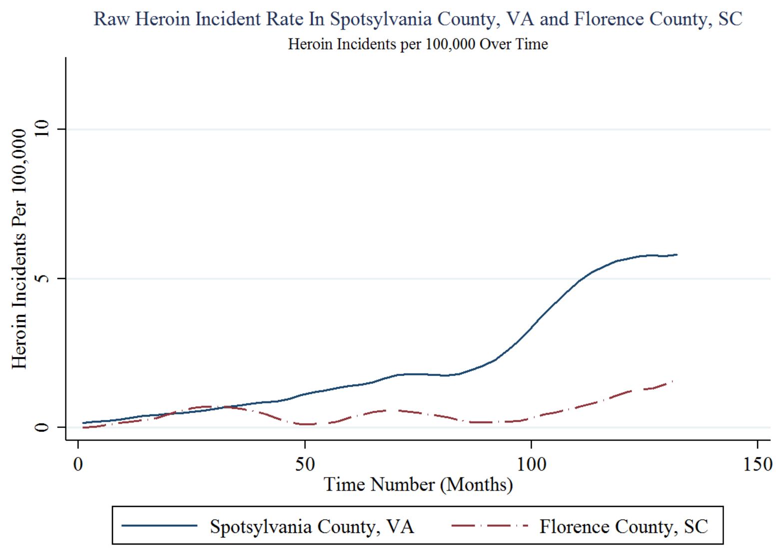

Fig. 1.12. Heroin Incident Rate in Two Example Counties

Notes: The graph compares the raw heroin incident rate over time in 2 counties with approximately 100,000 population. Spotsylvania County, VA is assigned a high factor 1 loading and Florence County, SC is assigned an average factor 1 loading under the IFE factor model. The factor 1 time trend captures a non-linear increase in the heroin incident rate over time, as seen in Figure 1.10, and Spotsylvania County's data corresponds with factor 1's more dramatic exponential growth in the heroin incident rate over time.

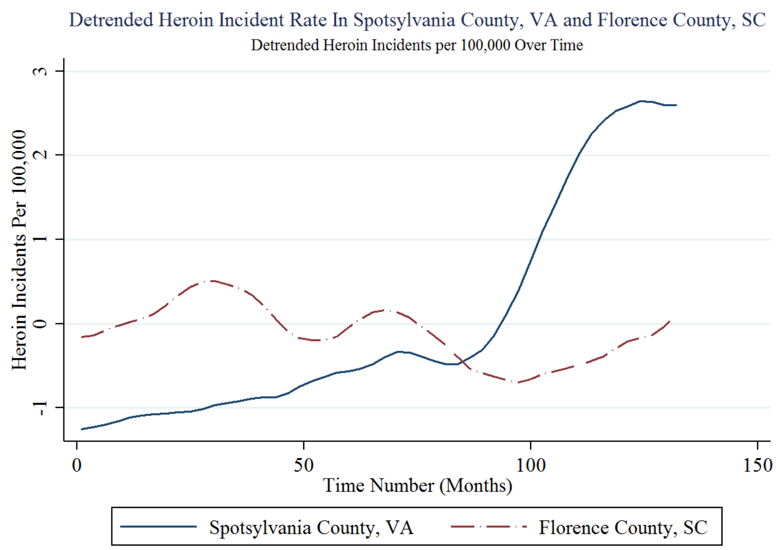

Fig. 1.13. The Detrended Heroin Incident Rate in Two Example Counties

Note: The figure shows the heroin incident rate with the national time trends, county fixed effects, and controls removed, for Spotsylvania County, VA and Florence County, SC, which both have approximately 100,000 residents. The figure suggests that the difference-in-difference specification alone does not capture the non-linear increase in the heroin incident rate in Spotsylvania County and counties like it. Spotsylvania and similar counties are assigned a high factor 1 loading under the IFE factor model, and factor 1 controls for a non-linear county-specific growth rate in heroin incidents. In contrast, Florence County, SC follows the national time trend more closely and is not assigned a high factor 1 loading.

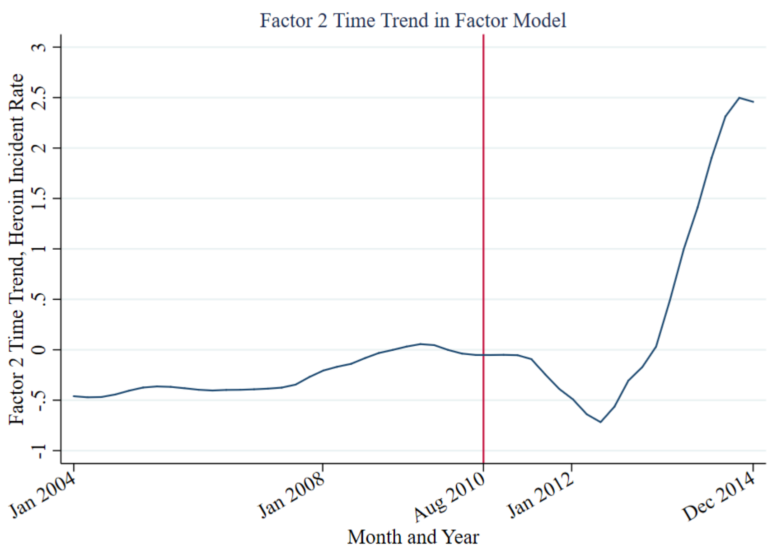

Fig. 1.14. Factor 2 From the Interactive Fixed Effect Factor Model on Heroin Incident Rate

The figure plots the second factor from the IFE factor model on the rate of heroin incidents. The red line marks the reformulation of OxyContin, which made it harder to abuse. Time periods 100-105 correspond to April to October 2012.

2. THE EFFECT OF PHARMACIST REFUSAL CLAUSES ON CONTRACEPTION, SEXUALLY TRANSMITTED DISEASES, AND BIRTHRATES

2.1 Introduction

Emergency contraceptive drugs, such as Plan B, are controversial because many groups equate taking the so-called "morning after" pill to a chemical abortion. Since Teva pharmaceuticals launched the emergency contraceptive drug Plan B, there have been instances in at least 25 states where pharmacists have refused to fill prescriptions for the drug or provide the drug over-the-counter to patients based on personal objections.[1]

In response to growing news coverage and public debate over patient access and pharmacist rights, many states passed laws that explicitly regulate the extent of medical professionals' rights to refuse to provide drugs and medical services. Some states have passed laws or ruled in court cases that pharmacists must fill valid prescriptions regardless of personal objections to the medications.[2] Other states have taken steps to protect pharmacists' rights to refuse without providing transfers or other accommodation to the patient. More states have lawed to allow refusal while requiring meaningful transfers to other pharmacies, balancing patients rights to drugs and pharmacists' personal beliefs. The effects of pharmacist refusal clauses have not been investigated. This paper is the first to consider potential effects of pharmacist refusal legislation on contraceptive purchasing outcomes, sexually transmitted diseases, and birthrates.

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[1]National Women's Law Center Pharmacist Refusal Fact Sheet

[2]For example, Wisconsin has specific legislation stating "A pharmacy shall dispense lawfully prescribed contraceptive drugs and devices and shall deliver contraceptive drugs and devices restricted to distribution by a pharmacy to a patient without delay." (Wis. Stat. Ann. 450.095)

This paper identifies the effect of “Expand” policies, which expand access to emergency contraception by prioritizing the rights of patients and require pharmacists to fill valid prescriptions, on multiple outcomes. I then investigate the effect of “Restrict” policies that restrict access to emergency contraception by emphasizing pharmacists’ rights of refusal without also providing patient protections in the case of a refusal. In addition, this paper looks at the effect of Collaborative Practice Agreements (CPAs), which expand access to emergency contraception by allowing pharmacists to write prescriptions for emergency contraception at the pharmacy counter.

This paper contributes to the literature on regulating contraception methods. It is the first to examine effects of pharmacist refusal clauses that favor either religious rights of pharmacists or patients’ rights to receive medication through the lens of legislation on emergency contraception. This paper also emphasizes substitution behavior between contraceptives induced by such policies, as well as more commonly-studied outcomes like sexually transmitted infections and birthrates. Collaborative Practice Agreements (CPAs) have been addressed by other papers, but this paper is unique because I consider possible substitution between contraceptive purchases and I find that CPAs increase birthrates, which is in contrast to other papers.

Even though Expand and Restrict pharmacist refusal policies favor different agents in the controversy surrounding emergency contraception, they have similar effects on behavior. I find evidence that both types of pharmacist refusal policy induce a small fraction of women (an estimated 1 out of 1,000 women, or 1 out of 200 women of child-bearing age) to adopt the regular birth control pill, which reduces purchases of both condoms and over-the-counter emergency contraception. There is not strong evidence to suggest that either policy has an impact on rates of sexually transmitted diseases, however there is evidence that the Restrict policy causes a drop in the birthrate among black mothers, beginning 9 months after passage. Since both the policies consistently have effects in the same direction, it is unlikely that the causal mechanism of the laws is creating or eliminating actual instances of pharmacist refusal, but that the passage of the laws draws the attention of some women who then respond by adopting the birth control pill.

2.2 Background

2.2.1 Policy Environment

In 1998 the first emergency contraceptive Preven was made available via prescription. Between 1998 and 2006, a prescription was needed to obtain emergency contraception, necessitating a doctor's visit. In 2006 the FDA ruled to make Plan B One Step available over the counter without a prescription to men and women 18 and older. Prior to the 2006 announcement making Plan B over-the-counter, 9 states passed Collaborative Practice Agreements (CPAs) to increase timely access to the drugs. Since emergency contraceptives must be taken as soon as possible after unprotected sex or contraceptive failure in order to be effective, states created agreements where a pharmacist collaborates with a doctors' office to write then fill prescriptions at the pharmacy counter. This allows women to receive both an official prescription for emergency contraceptives and the drug itself from an open pharmacy without having to make a doctor's appointment. Alaska, California, Hawaii, Massachusetts, New Hampshire and Washington passed Collaborative Practice Agreements before Plan B was made available over-the-counter. Maine and New Mexico passed protocols allowing pharmacists to dispense emergency contraception at the pharmacy counter without partnering with a doctor. For this paper, I classify Maine and New Mexico's state-approved protocols as CPAs because both types of legislation similarly expand access to emergency contraceptives.

Due to the controversy surrounding Plan B, there were many highly-publicized cases of pharmacists refusing to fill prescriptions for emergency contraceptives before and after Plan B was made over-the-counter. By 2005, twenty state legislatures introduced bills that would protect a pharmacist's right to refuse to fill prescriptions for contraceptives (Teliska, 2005). Starting in August 2006, women and men 18 years and older could purchase Plan B without a prescription, however the $50 drug was usually kept secured behind the pharmacy counter, necessitating communication with a pharmacist or pharmacy employee.[3] There were also publicized cases of pharmacists or pharmacy employees refusing to hand men and women Plan B from behind the counter.[4] Surveys of pharmacists suggest that between 5% and 15% of pharmacists report they would refuse to provide emergency contraception to a patient (Davidson et al., 2010; Richman et al., 2012).

These individual pharmacist refusal cases were often followed by lawsuits and demands from activist groups on both sides of the issue to draft pharmacist refusal legislation. Many states adopted a balanced approach, allowing pharmacist refusal but also protecting patient rights by requiring pharmacists to provide meaningful referrals to accommodating pharmacies or pharmacists. Other states still passed no significant legislation around pharmacist refusals. This paper explores the effects of policies that were passed that were imbalanced– that is, states that ruled to favor patient rights and prohibit pharmacist refusal, and states that ruled to favor pharmacists' rights to refuse without providing patient protections.

I define "Expand" policies as pharmacist refusal clauses that prioritize patient rights over the rights of pharmacists to refuse and thereby *expand* access to emergency contraceptives by requiring pharmacists to fill valid prescriptions. California, Illinois, Massachusetts, New Jersey, Nevada, Washington, and Wisconsin established these policies between 2004 and 2010, and are listed with start dates in Table 2.1. Illinois' law reads "Upon... lawful prescription for a contraceptive, a pharmacy must dispense the contraceptive... to the patient without delay."[5] The strongly-worded law also details what a pharmacy's responsibility is if the drug is out of stock, and

[3]A 2015 study by the American Society for Emergency Contraception found that as late as 2015, only 14% of stores have emergency contraceptives available for a customer to pick up in the aisle. The other 86% of stores either do not stock the drug or secure it with a plastic lock-box or security cord or store it behind a counter–all requiring assistance from an employee.

[4]A mystery shopper survey conducted by Bell et al. (2014) finds that 20% of men were denied over-the-counter Plan B at pharmacies in New York. There have also been news stories covering individual womens' experiences being denied OTC Plan B.

[5]Illinois Administrative Code Title 68, §1330.91 (j) 2005.

requires pharmacies to return unfilled prescriptions to patients if the patient asks for it. Washington passed a law prohibiting pharmacist refusal in July 2007, but in November 2007 several pharmacists opposed to the requirement that they provide emergency contraception sued the state, and the law was in limbo until December 2010, when a court ruled to uphold the Expand law.

I define "Restrict" policies as pharmacist refusal clauses that prioritize the rights of pharmacists to refuse to fill prescriptions for contraceptives or other drugs that violate personal beliefs, without also providing patient protections like the requirement of a meaningful transfer or even the return of the unfilled prescription to the patient.[6] Arizona, Arkansas, Georgia, Idaho, Kansas, Mississippi and South Dakota have laws on the books allowing pharmacists and other medical professionals to refuse to provide medical services that are in opposition to their beliefs.[7]

2.2.2 Emergency Contraception

Emergency contraception can prevent 75-90% of unintended pregnancies that otherwise would have occurred after unprotected intercourse or contraceptive failure. A regimen of emergency contraceptive pill consists of high doses of a progestin hormone, and prevents ovulation if it has not already occurred. Although the active ingredients vary across different brands of emergency contraception, most contain a larger dose of the same hormones that traditional oral contraceptive pills contain. Emergency contraceptives cost between $15 and $70 without insurance, and large doses of progestins have unpleasant side-effects like nausea, fatigue and vomiting, so there is reason to believe that women typically do not use emergency contraception as a primary contraceptive method.

[6] Arizona's pharmacist refusal clause requires the return of unfilled prescriptions to patients, and is the lone exception in this group of laws that allow pharmacist refusal without requiring transfers or other accommodations.

[7] South Dakota established an abortion-related refusal clause in 1998, but updated the wording of the law and the definition of an unborn child in 2006 in such a way that it could apply to the refusal of emergency contraception.

Between 1998 and 2006, the mechanism of action of the drugs were widely disputed. Plan B, the most common name-brand emergency contraceptive, stated that the drug prevents pregnancy in one of three ways: preventing the release of an ovum from the ovaries, preventing sperm from fertilizing an ovum, or preventing the implantation of a fertilized ovum onto the uterine wall. Religious organizations like the Catholic Church believe that human life begins at the moment of fertilization, and thus many viewed emergency contraception to be an abortificant because of it's third stated mechanism of action.

2.2.3 Related Literature

Much of the public health literature on access to emergency contraception concerns doctor and pharmacists' knowledge regarding the drugs. Multiple pharmacist surveys (Borrego et al., 2006; Golden et al., 2001; Ragland and West, 2009; Richman et al., 2012; Van, 2005) suggest that a large fraction of pharmacists in the early 2000s did not understand emergency contraception. In Richman et al. (2012), 46% of respondents said that Plan B can act as an abortificant, and 56% said that the drug can cause birth defects if taken by a pregnant woman. Similarly, Gorenflo and Fetters (2004) also finds that 44% of responding pharmacists believed emergency contraception could act as an abortificant, with some pharmacists unsure of the mechanism of action. Some of the above survey studies ask about pharmacists' moral attitudes concerning the drug, and between 5 and 15 percent of survey respondents say that they either do not or are unwilling to prescribe or dispense emergency contraception due to personal objections.

While this paper is the first to examine the effects of pharmacist refusal legislation on patient behavior and outcomes, there is an extensive literature on the effects of increasing access to emergency contraceptives more broadly. These studies examine the effect of expanding access to emergency contraception–for example, expanding over-the-counter access nationwide, expanding pharmacy access across different geographies, and conducting smaller-scale random control trials where women are given emergency contraceptives to keep at home–and do not address pharmacist refusal. In a review by Raymond et al. (2007), 14 randomized-control studies on emergency contraception worldwide between 1998 and 2006 find that increasing access to emergency contraceptives causes the treatment groups to use more emergency contraception, but has no effect on birthrates or abortion rates.

Literature on public policy regarding emergency contraceptives also find no effect on birthrates or abortion rates, but find increases in risky sexual behavior, like unprotected sex and rates of sexually transmitted diseases. Durrance (2013) finds that making Plan B available over the counter in 2006 increases STD rates and instances of unprotected sex and has no effect on abortion rates. Girma and Paton (2011) studies increased access to pharmacies that provide free emergency contraception in Britain, and finds that free emergency contraception causes increases in teenage STD rates.

There have been several studies on the effects of Collaborative Practice Agreements (Gross et al., 2014; Koohi, 2013; Zuppann et al., 2011), which study the effect of the CPAs on outcomes such as birth and abortion rates, reports of sexual assault, and marriage and dating patterns. Gross et al. (2014) find that CPAs do not change either birth rates or abortion rates, but find that provision of Plan B is shifted from emergency rooms to pharmacies, and rates of sexual assault reporting decrease due to the switch. Koohi (2013) studies effects of both CPAs passed by individual states and the nationwide over-the-counter expansion of Plan B in 2006. She finds significant birth reductions among single black women in response to the CPA, which provides low-income women official prescriptions (which can then be paid for by Medicaid) at the pharmacy counter, whereas making the $50 Plan B available over-the-counter reduces birthrates among older married women. She argues this difference is due to the out-of-pocket price difference between Plan B obtained through the CPA (and eligible to be paid for by Medicaid), and the $50 out-of-pocket price of Plan B to those who obtain it without a prescription. Koohi finds that outcomes of low-income women are responsive to pharmacy regulation of Plan B, and I also find that Medicaid-filled prescriptions are responsive to pharmacist refusal policies and the CPA. Zuppann et al. (2011) also finds that CPAs cause a decrease in the birthrates among young single white women.

2.3 Data

In 1990 Medicaid launched the Medicaid Drug Rebate Program, and required states to report drug utilization for covered outpatient drugs paid for by state Medicaid programs. The datasets are state-by-quarter drug counts by National Drug Code (NDC). Drugs paid for by Managed Care Organizations are dropped because Managed Care Organizations began reporting in 2010, whereas drugs purchased via Fee-For-Service Medicaid are recorded through the entire time period of 1990-2014. The Medicaid drug observations are matched to over 400 brand-name and generic birth control pill types, as well as matched to brand-name Plan B emergency contraceptive and generic emergency contraceptives.[8] As seen in Table 2.3, the typical state fills 2.08 Medicaid prescriptions for emergency contraceptives per 1,000 Medicaid enrollees per quarter, and 27 prescriptions for oral contraceptives per 1,000 enrollees. The Medicaid data covers birth control pills and suggests that about 2.89% of female Medicaid enrollees are on the birth control pill, whereas other studies (Hurt et al., 2012) suggest the real number is about 11% of women of reproductive age use the pill.[9]

The Nielsen Retail Scanner database tracks all purchases within participating retail establishments over time, by Universal Product Code. The data covers more than 35,000 grocery and drug stores across the U.S., containing more than half of

[8] Generics first appear in the Medicaid data in 2012 or 2013, with many brands becoming available in 2016 as the FDA made generic emergency contraceptives available over the counter. Prior to 2014 or so, the FDA only approved over-the-counter access for brand-name Plan B.

[9] This discrepancy is probably due to the fact that only fee-for-service Medicaid prescriptions are used, whereas many women are covered through Managed Care Medicaid, which is dropped due to poor data quality. Also, I used all Medicaid women in the denominator of the measurement instead of those restricted to between ages 15 and 44. In addition, I assume Medicaid women are on the pill for the full three months in a quarter, rather than adjusting for women starting and stopping the pill from month-to-month.

total sales in grocery stores across the country. A map of coverage is shown in Figure 2.1. There are sales of more than 2.4 million different products in the data. Nielsen categorizes products into groups, and for the purpose of this project I isolate sales of male contraceptives (condoms), female contraceptives (emergency contraceptives, female condoms, inserts and other female contraceptives), and pregnancy tests. Within the female contraceptive category, emergency contraception is identified by its UCR codes and separated from the rest of female contraceptives. I use only retail establishments that have full coverage in the Nielsen database from 2006 to 2015, and I then aggregate counts of products by county and month. Table 2.3 lists the average sales of contraceptive product by category per county and quarter. Each county sells about 0.77 over-the-counter doses of emergency contraceptives per 1,000 residents per quarter beginning in November 2006 after the FDA approval. This rate is less than half of the prescribing rate per Medicaid enrollee for emergency contraceptives within the Medicaid data, but the Medicaid enrollee population is more young and more female than the general population used in the denominator of Nielsen purchasing rates. The typical county realizes sales of 8.23 packages of condoms, 0.27 female non-prescription miscellaneous contraceptives (like female condoms), and 6.61 pregnancy tests per 1,000 residents per quarter.

Birthrates are obtained through the CDC Wonder query system, which draws from the universe of birth certificates to those born in recent years. For analyses at the state level, detailed birth counts from 1995 to 2014 are used. For the county level models, birth counts are available for years 1996-2014. At the county level, monthly birth counts are coded as zero if the number of births is less than 10, making models on birthrates among black mothers sometimes difficult and incomplete at the county level. The CDC sensors birth counts in counties with less than 100,000 population, but birth counts in these small counties are backed out by subtracting the sum of large county births from state births. Analyses are also performed on these aggregated small county birth counts. For these small county analyses, I obtain controls by taking a population-weighted average of controls within the small counties, aggregated at the state level.

Table 2.3 displays birthrates per 1,000 female population per quarter at the state-by-quarter level, at the county-by-quarter level within the available large counties with population greater than 100,000, and the birthrates from aggregated small counties at the state level. Birthrates are consistent across geographies, with an average birthrate of 6.7 births per 1,000 women per quarter. White women have similar birthrates to black women (5.6 and 7.6 births per 1,000 women per quarter), and Hispanic women have higher birthrates (11.2 births per 1,000 women per quarter).[10]

2.4 Empirical Methods

For the main analysis of this paper, I use a difference-in-differences regression framework on state-quarter and county-quarter panel data. The identifying assumption of the difference-in-differences specification is the parallel trends assumption that treated and untreated states follow similar growth paths prior to the treatment and would have continued to do so in the absence of treatment. This approach identifies changes in trends within the treated geographies that correspond to the timing of the implementation of the policy. I adapt the difference-in-differences models into an event-study framework with policy lags and leads to support the parallel trends assumption.

2.4.1 The Effect of the Policies on Medication Outcomes

Using state-by-quarter Medicaid data, I find the effect of the CPA, Expand, and Restrict policies on sales of prescription emergency contraceptives and prescription oral contraceptives using the following model framework:

[10]Birthrates to black mothers and Hispanic mothers differ at the state and county level because the CDC redacts the number of births if there are fewer than 10 births per month in a county in a category of mothers by race. That is, if there are between 1 and 9 births to either black or Hispanic women in a county in a month, the observation is coded as a zero.

$$Outcome_{it} = \alpha + \beta Policy_{p,i,t} + \Psi X_{it} + \iota_i + \gamma_t + \epsilon_{it}$$

$Policy_{p,i,t}$ is an indicator equal to one if policy $p \in \{CPA, Expand, Restrict\}$, is in effect within state i by quarter t. The coefficient of interest is β, and it captures the effect of the policy within the treated states, using untreated states as a counterfactual. ι_i represents a state fixed effect that captures the overall level of the outcome variable in a state and controls for characteristics of states that are not changing over the time period of interest. γ_t is the time fixed effect, which controls for the national trend in the outcome variables over the period of the panel data. X_{it} is a matrix of controls, capturing changes in covariates within counties over time which may otherwise confound estimates of the policy effect. Controls in X_{it} include the fraction of the population that is black, Hispanic, or of other non-white race, the age composition of the population, the poverty rate, average weekly wages, the unemployment rate, and number of pharmacies per capita.

To test the parallel trends assumption required for causal inference of the difference-in-differences framework, I adapt the above equation into a dynamic difference-in-differences model with policy lags and leads:

$$Outcome_{it} = \alpha + \Sigma_{\tau=-q}^{-1} \xi_\tau Policy_{p,i,\tau} + \Sigma_{\tau=0}^{m} \lambda_\tau Policy_{p,i,\tau} + \Psi X_{it} + \iota_i + \gamma_t + \epsilon_{it}$$

In the equation, $Policy_{p,i,\tau}$ are indicators equal to one if state i in time t has a policy take effect exactly τ time periods ago. The policy begins at time period $\tau = 0$. For example, $Policy_{i,t,2}$ is equal to one if the policy took effect two time periods ago and zero otherwise, and $Policy_{i,t,-3}$ is equal to one if the policy will take effect exactly three periods from time t and zero otherwise. The coefficients λ_τ measure the effect of the policy through time after passage. The coefficients ξ_τ capture the effect of the policy τ time periods before passage and should equal zero. If the coefficients ξ_τ do not equal zero, that is a sign that the outcome is trending differently within treated states than within untreated states. For the main analysis, the dynamic equation with lags and leads is presented in the form of graphs.

To find the effect of each policy on contraceptive outcomes, the outcomes within treated states are compared to outcomes within "control" states that do not experience any policy. Table 2.2 contains start dates by state for each of the policies in question (CPA, Expand and Restrict), as well as listing the set of control states that do not pass a strong pharmacist refusal policy. For each policy effect, a separate model is run; effects of the policies are obtained by dropping states that adopt the other two policies and comparing treated states with control states outlined in Table 2.2. For example, to find the effect of the CPA, only the CPA states and control states are used in the model, and the Expand and Restrict states are dropped.

Eight states – Hawaii, Illinois, Maryland, New Jersey, New York, Oklahoma, Oregon, Washington– expanded their Medicaid programs to cover over-the-counter Plan B in January 2007. I control for the Medicaid expansion in coverage in models with an indicator variable equal to one in these states starting in January 2007.

CPA coefficients are identified using policy changes in Alaska, California, Hawaii, Massachusetts, Maine, New Hampshire, New Mexico and Vermont. Models use analytic weighting, and California is weighted heaviest in this group of CPA policy states due to its population. Results associated with the Expand policy that favors patient rights are identified off of changes in Illinois, New Jersey, Nevada, and Wisconsin. California, Massachusettes, and Washington also adopt Expand policies during the time period of interest, but either already have CPAs in place or establish a CPA shortly after the expand policy is adopted. These states are dropped from the Expand policy models because the CPA is a more far-reaching policy than the Expand policy. [11] The effect of the Restrict policies that favor pharmacists' rights to refuse

[11] I was initially concerned that CPA results may confound or mask subtle responses from the Expand policy within states that have both policies. However, results from models run on CPA and the Expand policy simultaneously are similar to the main results where the policies are examined in the separate regression models.

are identified off changes in Arizona, Georgia, Idaho, Indiana, Kansas, Mississippi, and South Dakota.

2.4.2 Wild Cluster Bootstrapped Inference

Bertrand et al. (2004) show that the difference-in-differences approach suffers from over-rejection of the null due to unaccounted-for autocorrelation in the error term, and show that this issue is exacerbated under cases where there are few treated panel units. Since there are few treated states that adopt Expand, Restrict, and CPA policies, this issue is of concern for this study. To remedy the over-rejection problem, I use the Wild Cluster Bootstrap t-statistic-percentile procedure outlined in Cameron et al. (2008).[12] P-values obtained from this procedure are included in brackets for most of the key results in the paper. Inference is drawn from the resulting p-values.

2.5 Results

2.5.1 Medicaid Prescription Results

Table 2.4 lists the coefficients of interests on CPA, Expand, and Restrict policies from difference-in-difference models run on Medicaid Plan B and oral contraceptive rates. Each entry in the table corresponds to a separate model. Column (1) contains estimates of the effect of the CPA, Expand, and Restrict policies on the rate of Plan B prescribing through Medicaid. It should be noted that Plan B prescriptions in the Medicaid data only appear starting in 2004, and many states do not cover emergency contraceptives through Medicaid.[13] The CPA results are identified off of

[12]This procedure involves taking the residuals of a model run without the independent variables of interest (in my case, the Expand, Restrict and CPA) and randomly reassigning them within treated clusters. The residual randomization disrupts the autocorrelation in the error term within clusters that causes over-rejection of the null. The procedure then runs the difference-in-differences regression model on the data with the randomly-ordered residuals, and, bearing similarities to a placebo test, obtains a distribution of t-statistics under the meaningless data. The real t-statistic is compared to the distribution of bootstrapped t-statistics and is assigned a p-value equal to its percentile within the distribution.

[13]Some states that do not officially pay for emergency contraceptives still have emergency contraceptives paid for in the state drug utilization files, and these observations are used. For example,

changes within Massachusetts, Maine, New Hampshire, and Vermont. Expand results are identified off of Illinois, New Jersey, Nevada and Wisconsin. Restrict results are identified using changes in the already-uncommon Plan B prescribing within Georgia and Mississippi, and are likely not very informative. The CPA is associated with an increase in Plan B prescribing,

The Expand policy that ensures patient access to emergency contraceptives is associated with an increase in Plan B prescriptions, equal to between 1.3 and 2.6 additional emergency contraceptive prescriptions per 1,000 Medicaid enrollees per quarter per state, but these results are not statistically significant. The Restrict policy, which protects pharmacists' rights to refuse without offering patient protections, is very noisily measured and is not consistent across model specifications due to poor data on emergency contraception prescriptions through Medicaid in the treated states.

The Medicaid drug utilization data has much better data coverage for Medicaid prescriptions of traditional oral contraceptive pills. The CPA does not have a statistically significant effect on the rate of contraceptive pills per Medicaid enrollee, although point estimates suggest the CPA may slightly decrease the prescribing rate. The Expand policy is associated with a statistically significant increase in birth control pills per enrollee, equal to 0.423 additional pills per enrollee per quarter and state in the model specification without state-specific time trends in Column (3). The addition of state trends mitigates the result to 0.0881 additional pills per enrollee in the preferred specification in Column (4). 0.0881 additional pills per enrollee is equal to about 4,020 additional 30-packs of birth control pills in the average state with an Expand policy per quarter, or a 12% increase. Since a quarter is 3 months, the estimates suggest that 1,340 additional Medicaid-covered women obtain birth control prescriptions through Medicaid in the typical Expand state in response to the policy. This is a relatively small fraction of the 1,370,000 people in the average Expand state

Mississippi does not cover emergency contraceptives according to Princeton's Emergency Contraceptive Website, but the Medicaid data records 201 Plan B pills dispensed between 2004 and 2011.

enrolled in Medicaid. Figure 2.3[14] graphs the lag and lead effect coefficients from the dynamic difference-in-differences version of the models in 2.4. The figure displaying the effect of the Expand policy on the birth control prescribing rate graphs lead coefficients that are close to zero (albeit fairly noisy), meaning the parallel trends assumption is not obviously violated. The rate of prescribing seems to increase slightly after the Expand policy is put into place.

Results in Table 2.4 show that the Restrict policy is also associated with increases in the rate of birth control pills per enrollee, with the preferred specification in Column (4) showing a significant 0.0962 additional pills per enrollee. 0.0962 pills per enrollee is equal to a 26% increase in the rate of birth control pills per enrollee in the typical state that adopts the Restrict policy. This magnitude is equal to 2,973 additional 30-day pill prescriptions per quarter in the affected states, or 990 women adopting the pill out of an average of 930,000 Medicaid enrollees in these states. The lower right graph in Figure 2.3 plots the lags and leads of the adapted version of the model in Column (4). The lead coefficients are close to zero until 1 and 2 quarters before the policy is put into place. This may be indicative of an announcement effect; policies are passed in state legislatures and then go into effect at a later date. This finding helps to motivate my speculation on the mechanism of action; since there is a slight announcement effect, women may be hearing about pharmacist refusal clauses in the news and opt to start the birth control pill. The existence of the slight announcement effect does not support the possible mechanism of pharmacist refusal having an impact on the prescribing rate of oral contraceptives.

Figure 2.3 also graphs Google searches for Plan B in the treated states around the time of the implementation of the policy alongside the lags and leads coefficient estimates from the dynamic difference-in-differences models. With the CPA policy, the Expand policy, and the Restrict policy, there is a spike in Google searches for Plan B in the year leading up to the implementation of the policy. I speculate that when state legislatures introduce pharmacist refusal clauses and other policies related to

[14]Note that standard errors in the graphs are clustered at the state level, but are not corrected for autocorrelation and are most likely small because of few treated states in each policy group.

emergency contraceptives, there is an increase in public interest in Plan B and birth control, which is captured by relative increases in Google searches. The spike in Google searches for Plan B corresponds to a decrease in birth control within the CPA states and an increase in birth control within the states that establish Restrict policies within one year of policy implementation, however there is not much of a relationship between Google searches and birth control prescribing around the time of the Expand policies within states that adopt them.

Figures 2.4, 2.5 and 2.6 show the effect of the CPA, Expand and Restrict Policies, respectively, on the prescribing rate of birth control pills, with individual states dropped. These figures allow one to determine if one state in particular is driving the measured effects. This is important to explore in this paper, because there are few states treated with each policy, and population weights are used, meaning that large treated states may drive results. In the graphs of the effect of the CPA policy on birth control prescriptions in Figure 2.4, one can see that California is driving the decreasing trend in prescriptions around the timing of the policy. When California is dropped, the measured effect of the policy is a noisily-measured zero effect.

Illinois is the Expand state with the heaviest weight, and Figure 2.5 still shows an increase in the rate of birth control prescriptions around the timing of the Expand policy when Illinois is dropped. Expand states see an increase in birth control prescribing around the timing of the policy, with a possible lead effect due to announcement, and the effect is not driven by a particular state.

Georgia is the most populous state to adopt a Restrict policy, however Georgia is not driving the effect of the Restrict policy on birth control prescribing. Even when Georgia is dropped in Figure 2.6, there is an upward trend in birth control prescriptions after the Restrict policy is implemented, again with slight evidence of an announcement effect, as the increase in prescribing appears to begin in the quarter corresponding to 0-3 months prior to policy implementation.

2.5.2 Effects of Policies on Scanner Purchases

Table 2.5 lists the effects of the Expand and Restrict pharmacist refusal policies on contraceptive purchases and pregnancy test purchases from the Neilsen Scanner database. Since the Neilsen scanner data begins tracking purchases in January 2006, I am only able to analyze the effects of the Expand and Restrict pharmacist refusal policies. Models are at the county and quarter level, and outcome variables are logged purchases per population in a county, meaning entries in the table are interpreted as percentage changes. Columns (1), (3), (5), and (7) contain results from models run with county fixed effects and county controls, and Columns (2), (4), (6), and (8) add county-specific linear time trends to the model.[15] Once more, each of the eight entries in the table are from a separate model using either the states that adopt Expand or Restrict policies and the control states and dropping CPA states. Column (1) contains estimates of the effects of policies on over-the-counter emergency contraceptives, which first appear in the scanner data in November 2006, a few months after the August 2006 FDA ruling to make brand-name Plan B available over the counter. Column (3)-(4), (5)-(6) and (7)-(8) contain the model results run on rates of condom purchases, miscellaneous female contraceptives (e.g. female condoms and inserts), and pregnancy tests, respectively.

The Expand policy is associated with a 16.9% decrease in the purchase rate of over-the-counter Plan B. Since the data runs from 2006 to 2015, this result is identified off of changes in Washington and Wisconsin law. Figure 2.7 graphs the effect of the Expand policy on the scanner outcomes, but only identifies off of changes within Nevada, New Jersey, Washington and Wisconsin because of the relatively late time period coverage. In the Emergency Contraceptives graph, changes are identified off of Washington and Wisconsin alone because data coverage in this category begins in November 2006, leaving no pre-policy results to identify off of in Nevada or New Jer-

[15]Models run on scanner outcomes are very sensitive to the addition of trends, likely due to few treated states. The preferred model specifications are the models without time trends, because introducing trends also introduces pre-trends into the dynamic difference-in-differences graphs.

sey because these states established the laws in 2006 prior to the start of Plan B data coverage. Consequently, the measured effect of the Expand policy on Emergency Contraceptive scanner purchases is noisy in the dynamic difference-in-differences graphs, although there appears to be a decrease in over-the-counter purchases in the first year of passage, which does not persist through later quarters of the post-implementation period.

The Expand policy is also associated with a 5.87% decrease in the rate of condom purchases. A 5.87% decrease in the rate of condom purchases is equal to 0.09 fewer condoms per 1,000 population, or about 55 fewer condom purchases per month in retailers covered by the scanner data in the average county that adopts the Expand policy. The dynamic difference-in-differences graph for the effect of the Expand policy on condoms in Figure 2.7 shows that the policy is associated with a slight decrease in condom purchases after passage, although this effect is not entirely clear because the lead coefficients have a noisy seasonality to them. The graph is identified off of the four states that adopt an Expand policy after the start of the data in January 2006; Nevada, New Jersey, Washington and Wisconsin.

The Expand policy does not have a large or significant effect on other female contraceptives like female condoms. Column (5) shows an imprecisely-measured 8.36% decrease in female contraceptives in response to the policy. The policy also does not have an effect on purchases of pregnancy tests, with the point estimate of a 2.2% decrease in pregnancy tests, which is not statistically significant. Looking at the lag and lead coefficients in Figure 2.7, there is a downward trend in miscellaneous female contraceptives prior to the passage of the policy, and after the policy the treated counties seem to return to the trend of the control counties. There is a noticeable upward trend in purchases of pregnancy tests in the treated counties leading up to the time of passage, where purchases even out. It is not possible to tell if the return of the treated counties to the trend of the control counties that serve as counter-factuals has anything to do with requirements that pharmacists must fill valid prescriptions.

The effect of the Restrict policy on scanner outcomes is measured off of changes to the policy within Arizona, Idaho, Kansas, and South Dakota. Under the preferred model specification, the Restrict policy reduces the rate of over-the-counter emergency contraceptive purchases in the stores covered in the scanner data, and does not have statistically significant effects on purchases of condoms, other female contraceptives, or pregnancy tests. Lag and lead coefficients of the Restrict policy are plotted in Figure 2.8 and display the measured effect of the policy on scanner outcomes over time. The Restrict policy is consistent with a gradual decrease in Emergency Contraceptive purchases that begins within the first year of the policy, matching a -12.5% estimate in Table 2.5, or about 0.042 fewer emergency contraceptive purchases per 1,000 population in a county in a quarter; 5.33 fewer over-the-counter emergency contraceptive purchases within the covered stores in the average county within a state that adopts a Restrict policy. There does not appear to be a pre-trend in the emergency contraceptives graph, but the lead coefficient estimates are a bit noisily measured.

The Restrict policy decreases the rate of condom sales in affected counties by 4.7% when controlling for county-specific linear time trends. In Figure 2.8, there is a downward trend in condom sales before the passage of the policy, but there is a clear downward shift (even within the overall downward trend) that corresponds to the first quarter of effect. This downward shift matches the estimate of -4.7%. The effect is equal to 0.20 fewer condom purchases per month per 1,000 population in a county. This is about 190 fewer condom purchases per month for the average affected county.

There does not appear to be an effect of the Restrict policy on condom purchases or pregnancy test purchases in either the coefficient estimates in Table 2.5 nor the time paths graphed in Figure 2.8. The graph depicting the effect of the Restrict policy on other female contraceptives appears to show a decrease in the rate of female contraceptive purchases for about 5 quarters after the policy is passed, but the effect does not persist past a year and a half. Neither does the decrease correspond to an overall effect as measured in the models in Table 2.5, which estimates a non-significant 4.53% decrease in other female contraceptives.

2.5.3 Effect of Policies on STD Rates

Tables 2.7 and 2.8 display estimates of the effect of the CPA, Expand and Restrict policies on logged annual STD rates among women broken down into race and age categories. The CPA increases the gonorrhea rate among all women by a marginally significant 20%, and the increase in STI rates increases around the first year that the policy is in effect, as seen in 2.9. This is equal to about 11 additional cases per 100,000 population per year, or about 860 more cases in the average adopting state. This is similar in magnitude to estimates in Mulligan (2016), who finds that passing over-the-counter access to Plan B causes a 5% increase in combined cases of chlamydia, gonorrhea and syphilis. Durrance (2013) also finds that the CPA in Washington state increased the gonorrhea rate by 16% within the state. This increase in the female gonorrhea rate looks to be driven by cases among white and black women, and increases within women across all age groups, but especially in women ages 20 and older as seen in Table 2.8. Women aged 14-19 and 20-24 make up the bulk of gonorrhea cases and are weighted heavily in the overall STI rate, but the estimates of the CPA within the 14-19 and 20-29 columns are not statistically significant. Increases in the STI rate of women 30 and older are large in relative magnitude, but STI cases are much more rare among women in these age groups, as seen in the summary statistics in Table 2.3.

Both the Expand and Restrict policies are associated with decreases in the STI rates, with marginally significant decreases in the overall gonorrhea rate, and rates among black and Hispanic women. However, Figures 2.10 and 2.11 do not suggest a large effect corresponding to the timing of the policies. The states that implement Restrict policies appear to have a downward trend in STI rates beginning two years prior to the policy, and the states that implement Expand policies do not appear to realize a decrease in STI rates that correspond to the policy. For the expand state, the measured effect is 8 fewer cases per 100,000 population per year, or 564 fewer cases per state. The Restrict policy is associated with 7 fewer cases per 100,000 population, or 96 fewer cases in the typical Restrict policy state.

2.5.4 Effects of Policies on Birthrates

Table 2.9 displays coefficient estimates of the effect of the policies on state-level birthrates among all women, white women, black women, and Hispanic women in Columns (1)-(4). Each coefficient is the result of a separate model of treatment states compared with control states. The preferred model specification in Table 2.9 includes state and time fixed effects, controls, state-specific polynomial time trends, additional state-specific polynomial trends past 2007 to better fit state-varying drops in birthrates after the recession, and corresponding population analytic weights.

The CPA is associated with a slight increase in birthrates, and the point estimates of the effect of the CPA is in the same positive direction for all racial groups of mothers. The measured 1.24% increase in the overall birthrates is statistically insignificant, but appears to be driven by a statistically significant 2.35% increase in the birthrate among white mothers.[16] Looking at the dynamic difference-in-differences graphs in Figure 2.12, birthrates appear to be on an upward trend for white mothers in the treated states prior to the policy taking effect. However, there appears to be an additional level increase even within the upward trend among white mothers at the time 3 quarters or 9-12 months (marked by the second line) after the CPA goes into effect. This estimate is fairly robust across model specifications. Table 2.10 shows the effect of the CPA on the birthrate among white mothers across several model specifications, and the positive direction of the result is consistent across models, with additional state-specific trends making the estimates more precise. This effect is equal to 774 additional births to white mothers in the average CPA state, which is home to 5 million white women of child-bearing age.

[16]Finding a positive effect of the CPA on birthrates is in contrast to other papers, which find either no effect on birthrates (Gross et al., 2014) or a negative effect on births among black mothers (Koohi, 2013).

The Expand policy that prioritizes patient rights over pharmacist refusal is associated with a slight decrease in birthrates, as is observable in Table 2.9 in Columns (1) amd (2). Upon inspection of dynamic difference-in-differences graphs plotted in Figure 2.13, one can see that the negative coefficient estimate is merely capturing a downward trend in white birthrates within states that adopt the Expand policy. There is also no change in the downward trends at the timing of the policy, leading me to conclude that there is no discernible effect of the Expand policy on birthrates.

On the other hand, the Restrict policy that prioritizes pharmacist refusal rights without offering patient protections is associated with a marginally significant decrease in the birthrate among black mothers. In Figure 2.14, there is a noisily-measured drop in the overall birthrate beginning in the time period 3 quarters or 9-12 months after the policy goes into effect, marked by the second vertical line. This drop is not statistically significant, but appears consistent across demographic groups. The birthrate among black mothers experiences a marginally significant 1.16% decrease in birthrates due to the Restrict policy. It should be noted that this result is sensitive to model specification. Table 2.11 displays estimates from different model specifications of the effect of the Restrict policy on black birthrates. The point estimates are negative in Columns (1), (2) and in the preferred specification in Column (5), but are positive in Columns (3) and (4). The model with state-specific linear and quadratic trends in addition to state-specific linear and quadratic trends after the Great Recession is chosen as the preferred specification. A 1.16% decrease in the birthrate among black mothers is equal to 76 fewer births to black mothers per quarter (compared to a mean of 5,967 births to black mothers per quarter in the treated states).

In Table 2.12, birthrates are examined at the county level across the large counties for which birth counts are available. Table 2.13 contains estimates of the effects of the policies on birthrates within small counties, aggregated to the state level. The CPA no longer appears to be causing an increase in the birthrate within large counties. However, the Restrict policy is still causing a small and statistically significant 3.63%

drop in the black birthrate. Small counties also realize an increase in birthrates after the passage of the CPA, matching the state-level results. Aggregated small counties also experience a 2.25% decrease in the birthrate among black mothers after the Restrict policy, although it is not statistically significant, likely due to few black births within small counties, which adds additional noise.

The model that includes state-specific linear time trends, state-specific quadratic time trends, and state-specific post-regression linear and quadratic time trends is the preferred specification because of overall trends in birthrates within states. I am identifying small changes in birthrates that correspond to the timing of policies that I expect to have either limited or no effect on birth outcomes, whereas overall patterns of birthrates vary across states by a much larger degree. These additional flexible trends help the model fit the data. To illustrate the reason why this specification was chosen, Figures 2.15 and 2.16 display the white birthrate within California (which is driving the CPA results because of population weighting) and the black birthrate within Georgia (which is driving the Restrict policy results on black women). Both graphs show curvature within the birthrate patterns over time, in addition to a drop in birthrates throughout the great recession. Patterns in birthrates over time across different demographic groups and across states vary quite a bit, which means that national time fixed effects do not capture pattern variation across states. Flexible state-specific trends are added to the models to account for these patterns that are not correlated with the timing of the policies.

2.5.5 Discussion of Substitutions and Magnitudes

Table 2.15 summarizes the relative and absolute effects of each of the policies on different outcomes. The CPA increases prescriptions for emergency contraceptives through Medicaid by 20%, and may cause a 7% drop in the rate of prescriptions for the birth control pill, but these effects are not statistically significant. The CPA causes a 20% increase in the rate of STIs, which is in line with other papers that explore the effect of the CPA or other large expansions of access to emergency contraception. The

CPA does not have a clear effect on birthrates, but the point estimates are positive and small, which is in contrast to findings in Koohi (2013) and Zuppann et al. (2011).

The Expand and Restrict policies both increase the rate of birth control pill prescriptions through Medicaid, by 12% and 26%, respectively. The policies also cause a 1-6% decrease in condom purchases and a 12-17% decrease over-the-counter emergency contraceptive purchases, perhaps due to substitution onto the birth control pill. The policies do not affect STI rates in a robust or consistent way, but the Restrict policy is associated with a small decrease in the rate of births among black mothers.

Table 2.16 gives estimates for the overall magnitudes of the Expand and Restrict policies. Each estimated effect is a calculation using the population of child-bearing-aged females in the group of states that pass either Expand or Restrict policies. The magnitudes in Column (1) are the aggregate effect of the Expand policy across the states that implement the policy within the time frame of interest: Illinois, New Jersey, Nevada, and Wisconsin. This accounts for about 7.1 million women of child-bearing-age within these states, and their typical rates of prescriptions, condom purchases, STI rates, and births. The magnitudes in Column (2) are estimates of the effect of the Restrict policy on women between ages 14-44 within Arizona, Georgia, Idaho, Indiana, Kansas, Mississippi and South Dakota. Restrict states are home to about 6.4 million women of child-bearing-age.

The estimates of the effects of the Expand and Restrict policies are small in relative terms. However, tens of thousands of women begin birth control prescriptions in response to the policy when the rate of effect within the Medicaid population (5 additional quarterly prescriptions per 1,000 child-bearing-aged women) is applied to the aggregate population of women ages 14-44. This leads to a few thousand fewer over-the-counter Plan B purchases and condom purchases per quarter, and 280-380 fewer births per quarter. The policies may be associated with a small decrease in STI rates (about 100 fewer cases per year) but these effects are not precisely measured.

2.5.6 Conclusion

The use of emergency contraception is controversial, and instances of pharmacists refusing to fill prescriptions for drugs like Plan B were highly publicized in the early 2000s. States passed laws strongly favoring either the refusal rights of pharmacists or the rights of patients to receive the drugs. These policies had effects on women's contraceptive choices. There is reason to believe the effects on womens' choices are not due to rare cases of pharmacist refusal, but are more likely due to press coverage and womens' concerns that they may be denied emergency contraception.

Both the Expand policy (which emphasizes patients' rights) and Restrict policy (which allows pharmacist refusal without key patient protections) both cause small increases in the rate of birth control prescribing in the states that pass them, equal to about 1 in 1,000 women on Medicaid responding to the policies by adopting the pill. The pharmacist refusal clauses also cause decreases in over-the-counter emergency contraceptive purchases, condom purchases, and non-pill female contraceptives, perhaps because women who are sensitive to the policies have substituted onto the birth control pill. In response to the Expand pharmacist refusal policy, covered grocery stores are experiencing 165 fewer condom purchases per county per quarter (or 55 fewer per month); in response to the Restrict policy, covered grocery stores are experiencing 570 fewer purchases per county per quarter (or 190 fewer purchases per month).

Since more women adopt the birth control pill and fewer condoms are sold due to the pharmacist refusal policies, one may expect that rates of risky sex would increase. However, there is not evidence that rates of STIs are increasing in response to the policies. If anything, there may be a slight negative effect of the policies on STI rates, although the measurements are imprecise. To speculate, this may be because women who respond to new knowledge of pharmacist refusal clauses are more informed, prepared and cautious.

Although birthrates are mostly unresponsive to the policies, there is evidence that the Restrict policy is associated with a small decrease in pregnancies among black mothers. This result is statistically significant at the 10% level, and is robust under many models at the state, large-county, and aggregated small-county levels, as well as across model specifications, albeit with varying noise in measurement. This robust measured effect of the Restrict policy is small in magnitude, with a 1.16% decrease corresponding to 76 fewer births to black mothers in the average quarter within the typical state that adopts the Restrict policy. There is not evidence that the Expand policy has an effect on birthrates.

The Expand and Restrict policies were passed in various states, typically along political party lines, in order to please different groups of people. Pharmacists who were against the drug wanted to express their religious beliefs at work and refuse to provide drugs that were viewed as an abortificant. Reproductive rights activists and women who had been refused the drugs wanted to preserve access to FDA-approved medication, and protect the doctor-patient relationship. Strangely, the ultimate effect of the policies was to cause concerned women to adopt the birth control pill, forgoing the controversial drug and causing a decrease in purchases of over-the-counter Plan B.

Table 2.1.
The Effect of Emergency Contraceptive Policies on Medicaid Plan B and Birth Control Pills

Policy	State	Date	Law
CPA	Alaska	April 2002	
	California	January 2002	
	Hawaii	June 2003	
	Maine	July 2004	
	Massachusetts	September 2005	
	New Hampshire	June 2005	
	New Mexico	December 2002	
	Vermont	March 2006	
	Washington	February 1998	
Expand	California	September 2005	Cal. Bus&Prof. Code §733
	Illinois	April 2005	Ill. Admin. Code tit. 68, §1330.500
	Massachusetts	May 2004	Pharmacy board interpretation (2004) of Mass. Gen. Laws ch. 94C, 19(a)
	Nevada	May 2006	Nev. Admin. Code §639.753
	New Jersey	November 2006	N.J. Stat. Ann. §45:14-67.1
	Washington	July 2007-November 2007	Wash. Admin. Code §246-869-010
	Washington	December 2010 – present	
	Wisconsin	August 2008	Wis. Stat. Ann. §450.095
Restrict	Arizona	September 2009	
	Arkansas	1973	Ark. Code Ann. §20-16-304(4)
	Georgia	September 2001	GA Comp. R.& Regs. r. 480-5-.03(n)
	Idaho	March 2010	ID Code §18-611
	Kansas	March 2012	
	Mississippi	June 2004	MS Code Ann. §41-107-1 to 13
	South Dakota	July 2006	SD Codified Laws §36-11-70

Table 2.2.
Effective Dates of Expand, Restrict, CPA, and Medicaid Policies

State	CPA	Expand	Restrict	Medicaid '07	Control
Alaska	Apr 2002				
Alabama					Control
Arkansas			1973		
Arizona			Sep 2009		
California	Jan 2004	Sep 2005			
Colorado					Control
Connecticut					Control
Delaware					Control
Florida					Control
Georgia			Sep 2001		
Hawaii	June 2003			Jan 2007	
Iowa					Control
Idaho			Mar 2010		
Illinois		Apr 2005		Jan 2007	
Indiana					
Kansas			Mar 2012		
Kentucky					Control
Louisiana					Control
Massachusetts	Sep 2005	May 2004			
Maryland				Jan 2007	
Maine	Jul 2004				
Michigan					Control
Minnesota					Control
Missouri					Control
Mississippi			Jun 2004		
Montana					Control
North Carolina					Control
North Dakota					Control
Nebraska					Control
New Hampshire	June 2005				
New Jersey		Nov 2006		Jan 2007	
New Mexico	Dec 2002				
Nevada		May 2006			
New York				Jan 2007	
Ohio					Control
Oklahoma				Jan 2007	
Oregon				Jan 2007	
Pennsylvania					Control
Rhode Island					Control
South Carolina					Control
South Dakota			Mar 1998		
Tennessee					
Texas					Control
Utah					Control
Virginia					Control
Vermont	Mar 2003				
Washington	Feb 1998	Jul 2007-Nov 2007, Dec 2010		Jan 2007	
Wisconsin		Aug 2008			
West Virginia					Control
Wyoming					Control

CPAs or Collaborative Practice Agreements allow pharmacists to write and dispense official emergency contraceptive prescriptions at the pharmacy counter. Expand require pharmacists to fill valid prescriptions. Restrict policies legally allow pharmacists to refuse to fill emergency contraceptive prescriptions on the basis of their personal values or beliefs without offering patient protections. Refuse-Accommodate policies allow pharmacists and pharmacy employees to refuse to fill prescriptions for emergency contraceptives, but legally require the pharmacy to make accommodations for the patient.

Table 2.3.
Summary Statistics of Outcome Variables

Outcome	Mean and Std. Dev.
Medications and Contraceptives	
Medicaid Prescriptions Per 1,000 Enrollees Per State Per Quarter	
Plan B	2.08 (3.89)
Oral Contraceptives	27.01 (17.05)
Scanner Purchases Per 1,000 Population Per County Per Quarter	
Emergency Contraceptives	0.77 (0.93)
Condoms	8.23 (7.33)
Female Non-Rx Contraceptives	0.27 (0.28)
Pregnancy Tests	6.61 (4.10)
Birthrates – Births Per 1,000 Female Population* Per Quarter	
State Level Birthrate	
Total	6.74 (0.92)
White	5.60 (0.76)
Black	7.63 (1.44)
Hispanic	11.19 (3.03)
County Level Birthrate – Large Counties	
Total	6.68 (1.41)
White	5.48 (1.22)
Black	4.94 (3.68)
Hispanic	8.49 (5.31)
County Level Birthrate – Aggregate Small Counties	
Total	6.25 (0.81)
White	5.67 (0.70)
Black	5.58 (2.68)
Hispanic	11.12 (3.64)
Female STI Rate Per 100,000 Population	
State Level Female STI Rate Per 100,000 Population* Per Year	
Total	116.3 (59.9)
White	28.4 (14.0)
Black	450.1 (211.9)
Hisp	53.9 (29.96)
Age 10-19	619.5 (350.1)
Age 20-29	577.5 (291.5)
Age 30-39	114.9 (52.4)
Age 40+	8.5 (4.1)

Standard deviation of variables in parentheses, weighted by population.
* Each white/black/Hispanic birthrate is calculated using the white/black/Hispanic female population or the fema
population in each age category.

Table 2.4.
The Effect of Emergency Contraceptive Policies on Medicaid Plan B and Birth Control Pills

	(1)	(2)	(3)	(4)
	Plan B Rate Pills Per 1000 Enrollees		Birth Control Rate Pills Per Enrollee	
CPA	1.239	0.420	-0.0261	-0.143
	(1.068)	(0.869)	(0.104)	(0.0501)
	[0.780]	[0.570]	[0.410]	[0.124]
Expand	2.626	1.273	0.423**	0.0881
	(1.469)	(0.800)	(0.183)	(0.0670)
	[0.880]	[0.875]	[0.953]	[0.879]
Restrict	-1.473	0.175	0.0708	0.0962*
	(1.113)	(0.274)	(0.135)	(0.0536)
	[0.152]	[0.770]	[0.760]	[0.909]
FE	X	X	X	X
Controls	X	X	X	X
Trends		X		X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inference is drawn from the Wild cluster bootstrap T-test proposed in Cameron et al. (2008) and bootstrapped p-values are in brackets.

Table 2.5.
The Effect of Emergency Contraceptive Policies on Purchased Contraceptives, at County Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emergency Contraceptives		Condoms		Female Contraceptives		Pregnancy Tests	
Expand	-0.169***	-0.0347*	-0.0613**	0.009	-0.0577	-0.005	-0.0457	0.0235***
	(0.0476)	(0.0179)	(0.0199)	(0.0184)	(0.0565)	(0.0733)	(0.0172)	(0.00810)
	[0.010]		[0.025]		[0.171]		[0.377]	
Restrict	-0.125**	0.0036	-0.0109	-0.047**	-0.0453	-0.111***	0.0336	-0.0371*
	(0.0401)	(0.0373)	(0.0387)	(0.0218)	(0.0313)	(0.0159)	(0.0976)	(0.0215)
	[0.046]		[0.458]		[0.371]		[0.617]	
FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
Trends		X		X		X		X

Table contains estimates from models run at the county and month level, with logged rates of contraceptive purchases as the outcome variable.
Standard errors in parentheses, clustered by state.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inference is drawn from the Wild cluster bootstrap T-test proposed in Cameron et al. (2008) and bootstrapped p-values are in brackets.

Table 2.6.
The Effect of Emergency Contraceptive Policies on Purchased Contraceptives, at County Level: Large Counties With Population Greater Than 100,000

	(1) Emergency Contraceptives	(3) Condoms	(5) Female Contraceptives	(7) Pregnancy Tests
Expand	-0.137***	-0.0628*	-0.0247	-0.0600***
	(0.0397)	(0.0278)	(0.0537)	(0.0166)
	[0.010]	[0.052]	[0.377]	[0.005]
Restrict	-0.1327**	-0.0458	-0.0174	-0.0466
	(0.0402)	(0.0211)	(0.0385)	(0.0401)
	[0.018]	[0.106]	[0.395]	[0.239]
FE	X	X	X	X
Controls	X	X	X	X
Trends				

Models in the above table only use counties with population greater than 100,000 for which birthrate counts are available.
Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inference is drawn from the Wild cluster bootstrap T-test proposed in Cameron et al. (2008) and bootstrapped p-values are in brackets.

Table 2.7.
The Effect of Emergency Contraceptive Policies on STD Rates by Race

	(1) All	(2) White	(3) Black	(4) Hispanic
CPA	0.201*	0.117	0.107	-0.0396
	(0.108)	(0.138)	(0.139)	(0.153)
	[0.915]	[0.717]	[0.681]	[0.450]
Expand	-0.0978*	-0.0238	-0.0305	-0.0640
	(0.059)	(0.098)	(0.084)	(0.162)
	[0.068]	[0.412]	[0.392]	[0.373]
Restrict	-0.0495	-0.124	-0.119*	-0.311*
	(0.0108)	(0.097)	(0.0542)	(0.143)
	[0.252]	[0.145]	[0.086]	[0.057]

Table includes models run on state logged yearly female gonorrhea rates per 100,000 females.
Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8.
The Effect of Emergency Contraceptive Policies on STD Rates by Age Group

	(1) All	(2) 10-19	(3) 20-29	(4) 30-39	(5) 40+
CPA	0.201	0.0834	0.166	0.339**	0.345***
	(0.108)	(0.126)	(0.100)	(0.118)	(0.107)
	[0.915]	[0.701]	[0.879]	[0.977]	[0.995]
Expand	-0.0978*	-0.0578**	-0.134***	-0.195	-0.232**
	(0.059)	(0.074)	(0.047)	(0.062)	(0.093)
	[0.068]	[0.211]	[0.015]	[0.004]	[0.015]
Restrict	-0.0495	-0.044	-0.0114	-0.140	-0.153*
	(0.070)	(0.0642)	(0.069)	(0.101)	(0.106)
	[0.252]	[0.266]	[0.433]	[0.115]	[0.095]

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inference is drawn from the Wild cluster bootstrap T-test proposed in Cameron et al. (2008) and bootstrapped p-values are in brackets.

Table 2.9.
The Effect of Emergency Contraceptive Policies on Birthrates by Race

	(1) All	(2) White	(3) Black	(4) Hispanic
CPA	0.0124	0.0235**	0.0097	0.0171
	(0.007)	(0.010)	(0.021)	(0.010)
	[0.751]	[0.959]	[0.628]	[0.825]
Expand	-0.0062*	-0.0116**	0.0111	0.0032
	(0.004)	(0.005)	(0.010)	(0.007)
	[0.075]	[0.032]	[0.861]	[0.668]
Restrict	-0.0060	-0.0051	-0.0116*	0.0065
	(0.005)	(0.006)	(0.006)	(0.019)
	[0.128]	[0.301]	[0.067]	[0.392]

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Inference is drawn from the Wild cluster bootstrap T-test proposed in Cameron et al. (2008) and bootstrapped p-values are in brackets.

Table 2.10.
The Effect of the CPA on White Birthrates, Across Model Specifications

	(1) FE	(2) Controls	(3) LTT	(4) PTT	(5) Recession
CPA	-0.0151	0.0125	0.0329**	0.0230**	0.0235**
	(0.0191)	(0.0271)	(0.0156)	(0.0109)	(0.0103)
FE	X	X	X	X	X
Controls		X	X	X	X
State Linear Trends			X	X	X
State Polynomial Trends				X	X
Recession Trend Controls					X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11.
The Effect of the Restrict Policy on Black Birthrates, Across Model Specifications

	(1) FE	(2) Controls	(3) LTT	(4) PTT	(5) Recession	(6) 1996-2010	(7) Factor
Restrict	-0.0380	-0.0044	0.0196	-0.0046	-0.0166*	-0.0095	-0.0490
	(0.0291)	(0.0292)	(0.0135)	(0.0152)	(0.0059)	(0.0117)	(0.0316)
FE	X	X	X	X	X	X	$\hbar$
Controls		X	X	X	X	X	X
State Linear Trends			X	X	X	X	$\hbar$
State Polynomial Trends				X	X	X	$\hbar$
Recession Trend Controls					X	X	$\hbar$
Drop 2011-2014						X	
IFE Factor							X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
$\hbar$: The IFE Factor Model nests fixed effects, and state-specific curvature over time.

Table 2.12.
The Effect of Emergency Contraceptive Policies on Birthrates by Race Within Large Counties, at County Level

	(1) All	(2) White	(3) Black	(4) Hispanic
CPA	-0.0086	-0.0644	0.0904	-0.0018
	(0.0067)	(0.0417)	(0.0216)	(0.0261)
Expand	-0.0072	-0.0061	-0.0166	-0.0035
	(0.0057)	(0.0068)	(0.0355)	(0.0094)
	[0.253]	[0.222]	[0.365]	[0.520]
Restrict	-0.0036	-0.0010	-0.0363**	-0.0011
	(0.0047)	(0.0056)	(0.0338)	(0.0432)
	[0.371]	[0.505]	[0.018]	[0.570]

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13.
The Effect of Emergency Contraceptive Policies on Birthrates by Race within Aggregated Small Counties

	(1) All	(2) White	(3) Black	(4) Hispanic
CPA	0.0291	0.0348	–	0.265
	(0.0230)	(0.0223)	(–)	(0.0232)
	[0.856]	[0.830]	[–]	[0.999]
Expand	-0.0059	-0.0081	-0.0103	-0.0134
	(0.0063)	(0.0049)	(0.0167)	(0.0079)
	[0.265]	[0.118]	[0.425]	[0.164]
Restrict	-0.0061	0.0077	-0.0225	0.0028
	(0.0108)	(0.0080)	(0.0163)	(0.0379)
	[0.382]	[0.224]	[0.318]	[0.532]

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14.
The Effect of Emergency Contraceptive Policies on Purchased Contraceptives, at County Level: IFE Factor Model

	(1) Emergency Contraceptives	(3) Condoms	(4) Other Female Contraceptives	(7) Pregnancy Tests
Expand	0.0255	-0.0192**	0.0559	0.00778
	(0.0265)	(0.0097)	(0.0197)	(0.0099)
Restrict	-0.0220	-0.0767	-0.104	0.0531
	(0.0151)	(0.0104)	(0.0186)	(0.0090)
FE	X	X	X	X
Controls	X	X	X	X
Trends	$\hbar$	$\hbar$	$\hbar$	$\hbar$

Standard errors in parentheses, clustered by state.
Standard errors are not bootstrapped, and inference is not drawn.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
$\hbar$: The IFE Factor Model nests linear time trends.

Table 2.15.
The Effect of Emergency Contraceptive Policies on Purchased Contraceptives, at County Level: IFE Factor Model

	CPA		Expand		Restrict	
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid Rx Pills Per Quarter						
Rx EC	+20%	+0.42 /1,000	+52.6%	+1.27 /1,000	+416.6%	+0.175/1,000
Rx Birth Control	-6.9%	-143 /1,000	+12%*	+88 /1,000	+26%*	+96 /1,000
Scanner Purchase Outcomes Per Quarter						
OTC EC			-16.9%**	-0.21 /1,000	-12.5%**	-0.09 /1,000
Condoms			-6.13%**	-0.27 /1,000	-1.1%**	-0.60 /1,000
Misc. Female BC			-5.77%	-0.03 /1,000	-4.5%	-0.02 /1,000
Preg. Tests			-4.57%	-0.44 /1,000	+3.36%	+0.29 /1,000
STI Rates Per Year						
STI Rate	+20.1%*	+11.4 /100,000	-9.78%*	-8.12 /100,000	-4.95%	-7.14 /100,000
White STI Rate	+11.7%	+1.3/100,000	-2.38%	-0.52/100,000	-12.4%	-4.0 /1,000
Black STI Rate	+10.7%	+27.3 /100,000	-3.05%	-18.5 /100,000	-11.9%*	+70.0 /100,000
Hispanic STI Rate	-3.96%	-1.4 /100,000	-6.40%	2.7 /100,000	-31.1%*	-22.6 /100,000
Birthrates Per Quarter						
All Births	+1.24%	+.086 /1,000	-0.62%	-0.042 /1,000	-0.6%	-0.043 /1,000
White Births	+2.35%	+0.122 /1,000	-1.16%	-0.061 /1,000	-0.51%	-0.31 /1,000
Black Births	+0.97%	+0.068 /1,000	+1.11%	+0.083 /1,000	-1.16%**	-0.093 /1,000
Hispanic Births	+1.71%	+0.184 /1,000	+0.32%	+0.035 /1,000	+0.65%	+0.084 /1,000

Standard errors in parentheses, clustered by state.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.16.
A Comparison of the Magnitudes of Effects from the Expand and Restrict Policies

Outcome	(1) Effect of Expand Policy	(2) Effect of Restrict Policy
Birth Control Pill	+5 quarterly prescriptions /1,000 Women 35,615 women affected out of 7.1 million	+5 quarterly prescriptions /1,000 Women 32,013 women affected out of 6.4 million
OTC Plan B	-0.5 quarterly purchases /1,000 Women 3,561 fewer purchases per quarter out of 55,400	-1 quarterly purchases /1,000 Women 6,402 fewer purchases per quarter out of 42,400
Condom Purchases	-1.3 quarterly purchases /1,000 Women 9,260 fewer purchases per quarter out of 478,000	-1.3 quarterly purchases /1,000 Women 20,488 fewer purchases per quarter out of 320,000
STI Rates	-0.016 annual cases /1,000 Women 112 fewer cases per year out of 17,622	-0.014 annual cases /1,000 Women 111 fewer cases per year out of 21,392
Births	-0.04 quarterly births /1,000 Women 281 fewer births per quarter out of 94,330	-0.04 quarterly births /1,000 Women 318 fewer births per quarter out of 116,799

Rates are per 1,000 women of child bearing age; ages 10-44. Magnitudes are obtained by scaling the rates of the effect to the population of females of child bearing age across all states that adopt Expand policies or Restrict policies; about 7 or 8 million women within the affected states.

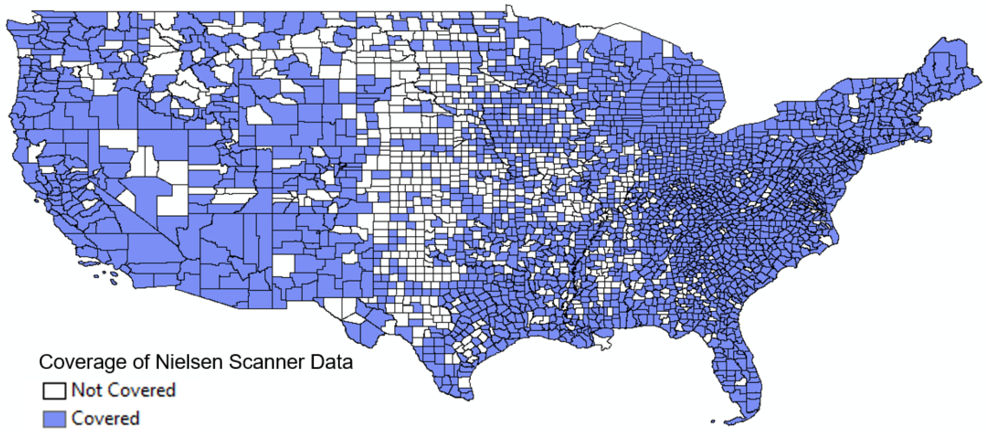


Fig. 2.1. Counties with Grocery Stores Tracked in the Nielsen Scanner Database

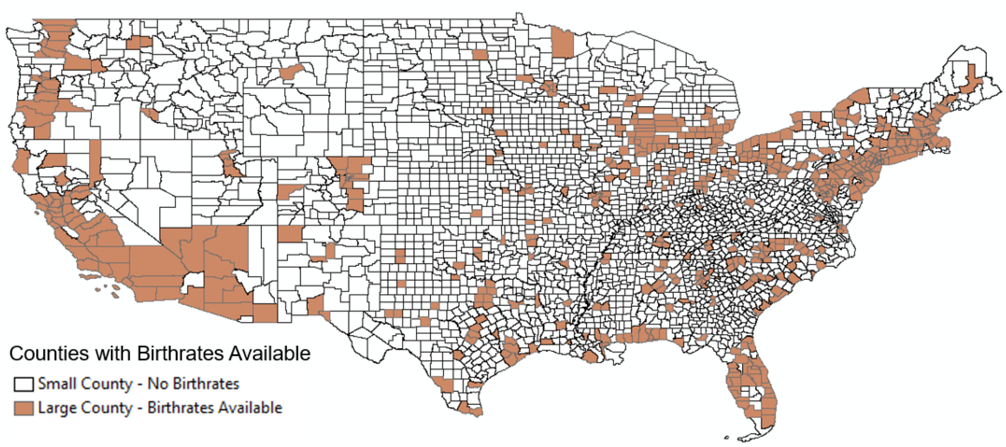


Fig. 2.2. Counties with Birth Counts Available

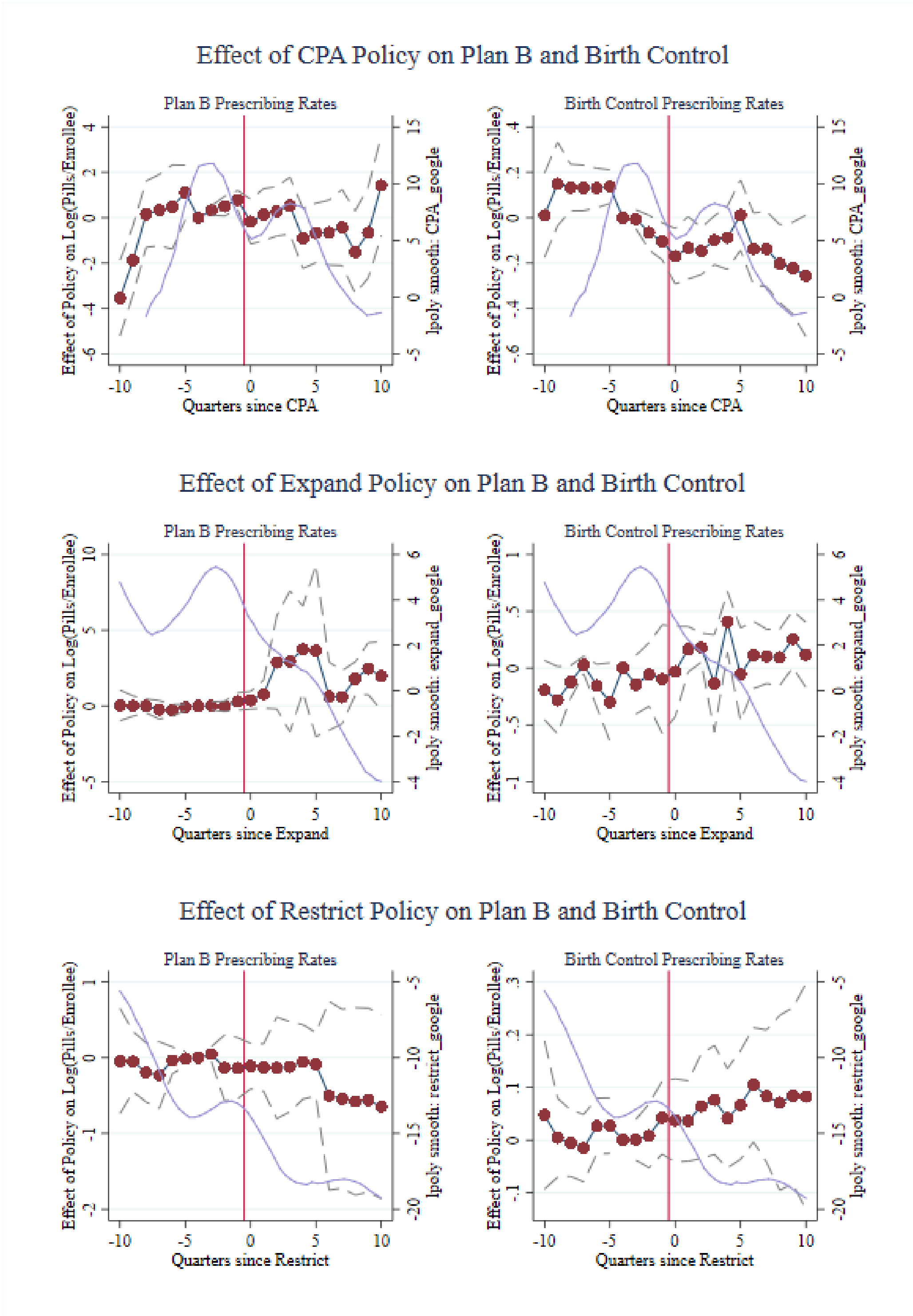


Fig. 2.3. The Effect of the Policies on Medicaid Prescription Plan B and Oral Contraceptives

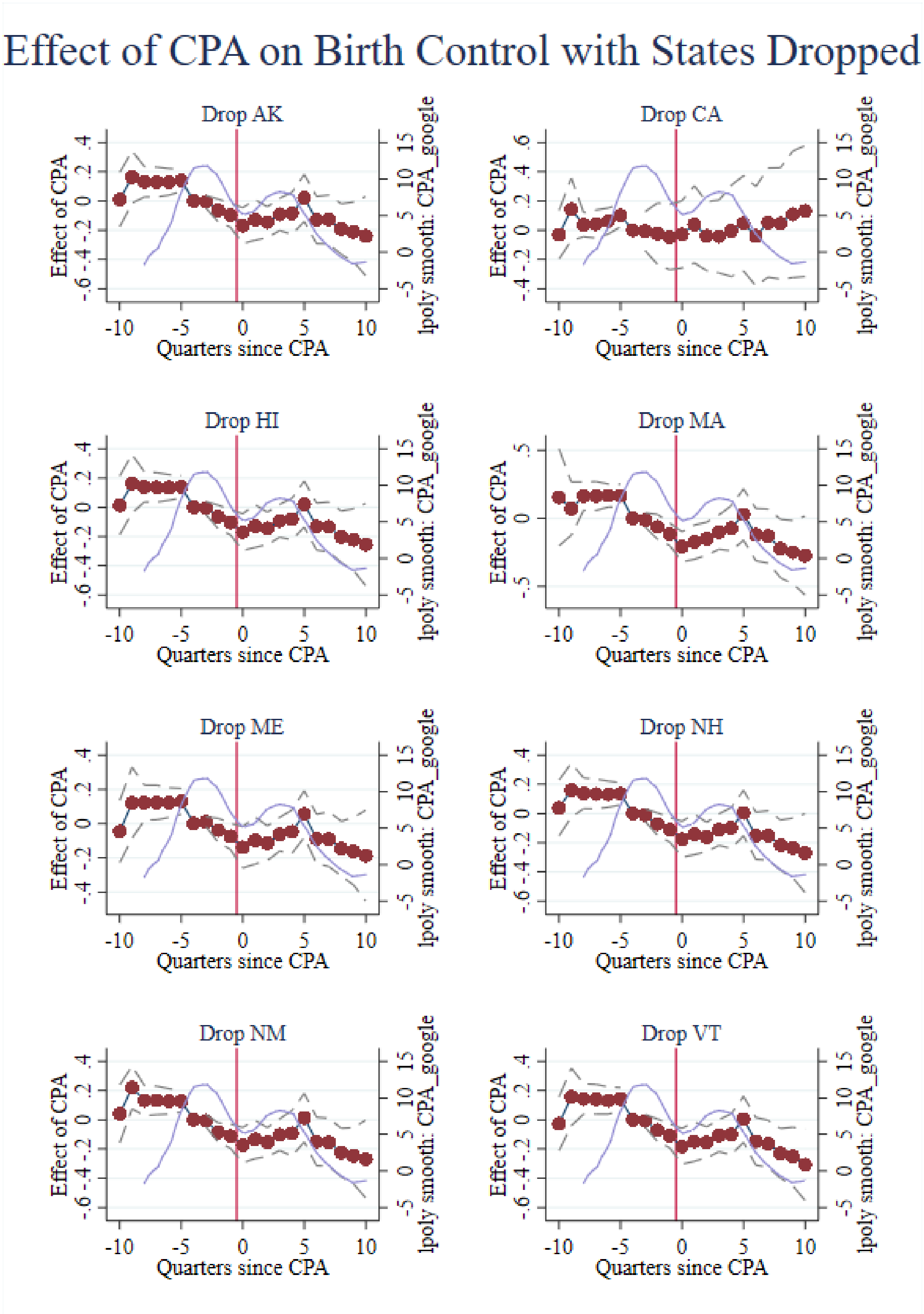


Fig. 2.4. The Effect of the CPA on Medicaid Oral Contraceptives, Dropping Individual States

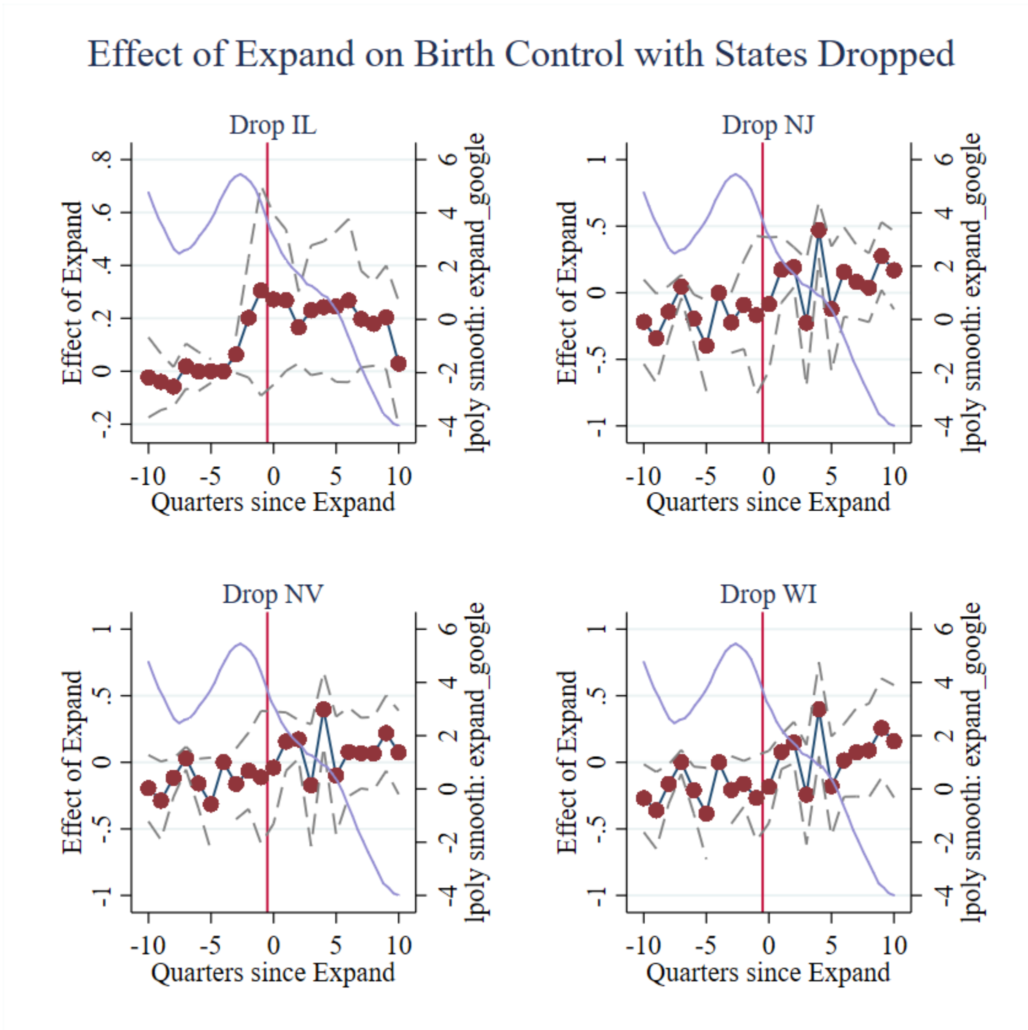


Fig. 2.5. The Effect of Expand on Medicaid Oral Contraceptives, Dropping Individual States

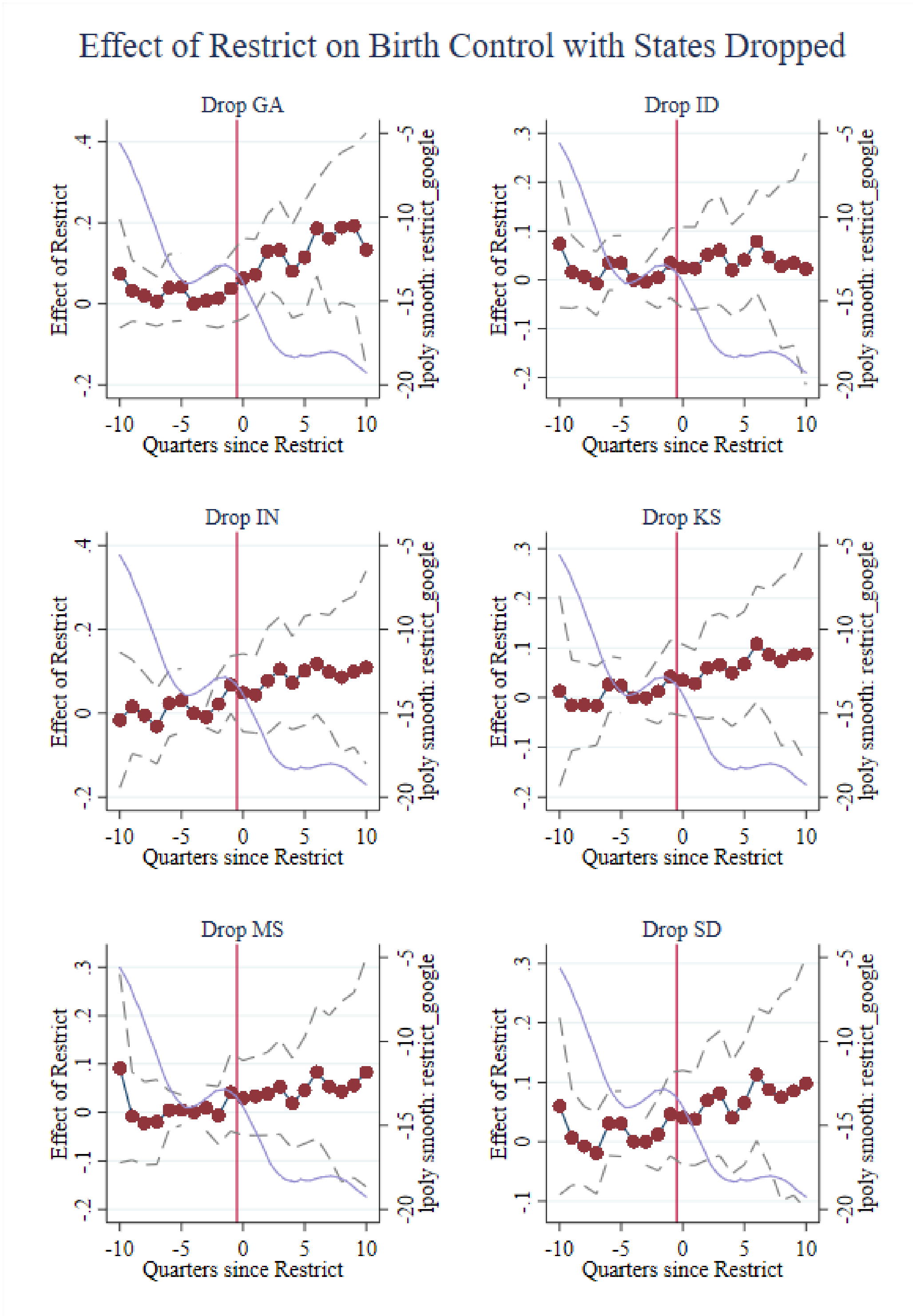


Fig. 2.6. The Effect of Restrict on Medicaid Oral Contraceptives, Dropping Individual States

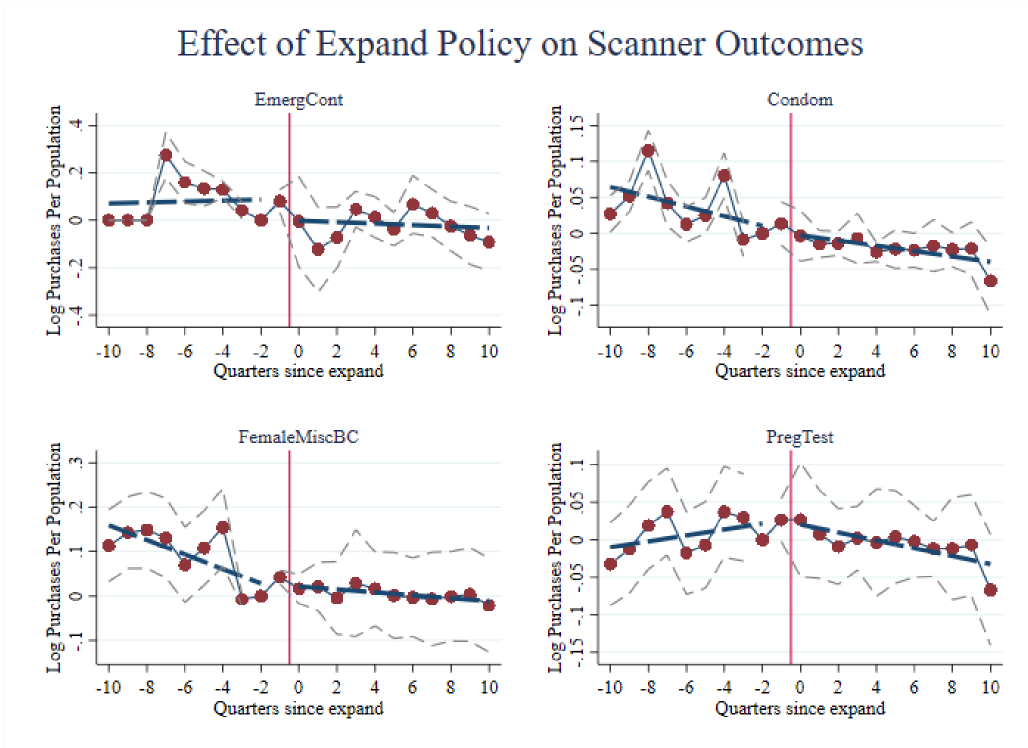


Fig. 2.7. The Effect of Expand on Scanner Outcomes

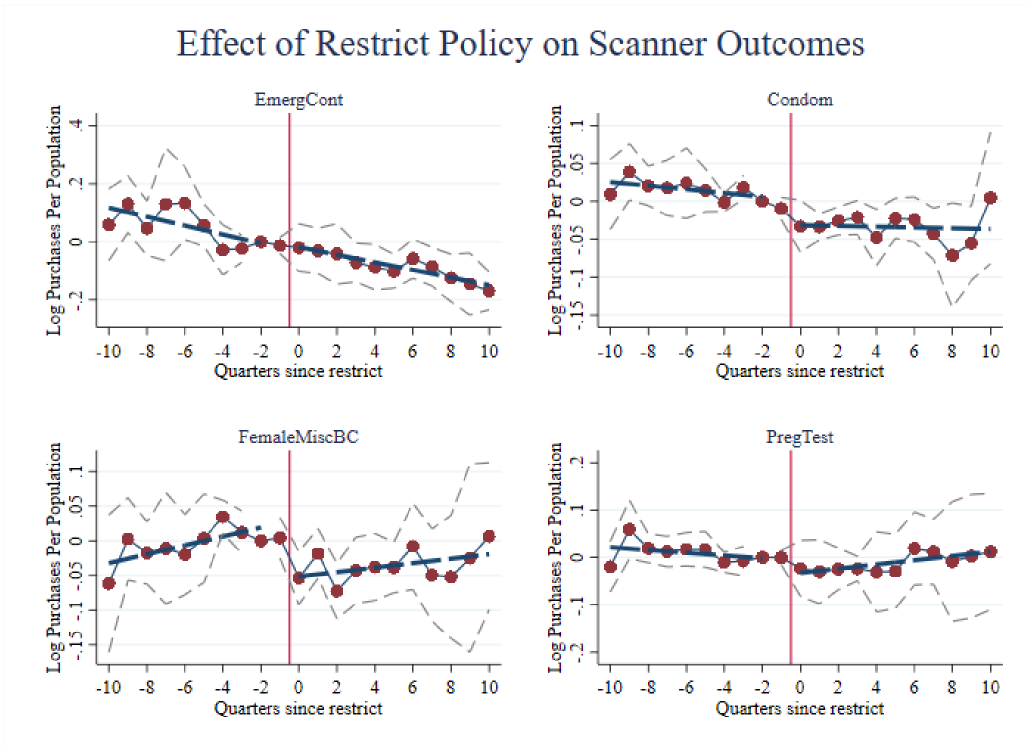


Fig. 2.8. The Effect of Restrict on Scanner Outcomes

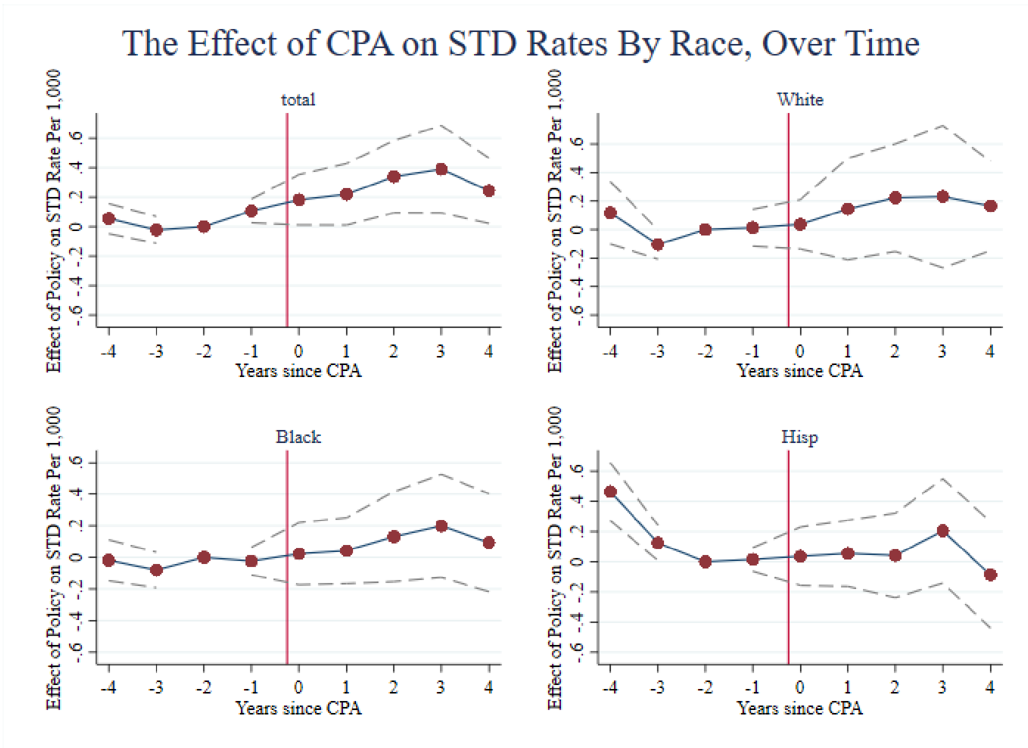


Fig. 2.9. The Effect of Restrict on Scanner Outcomes

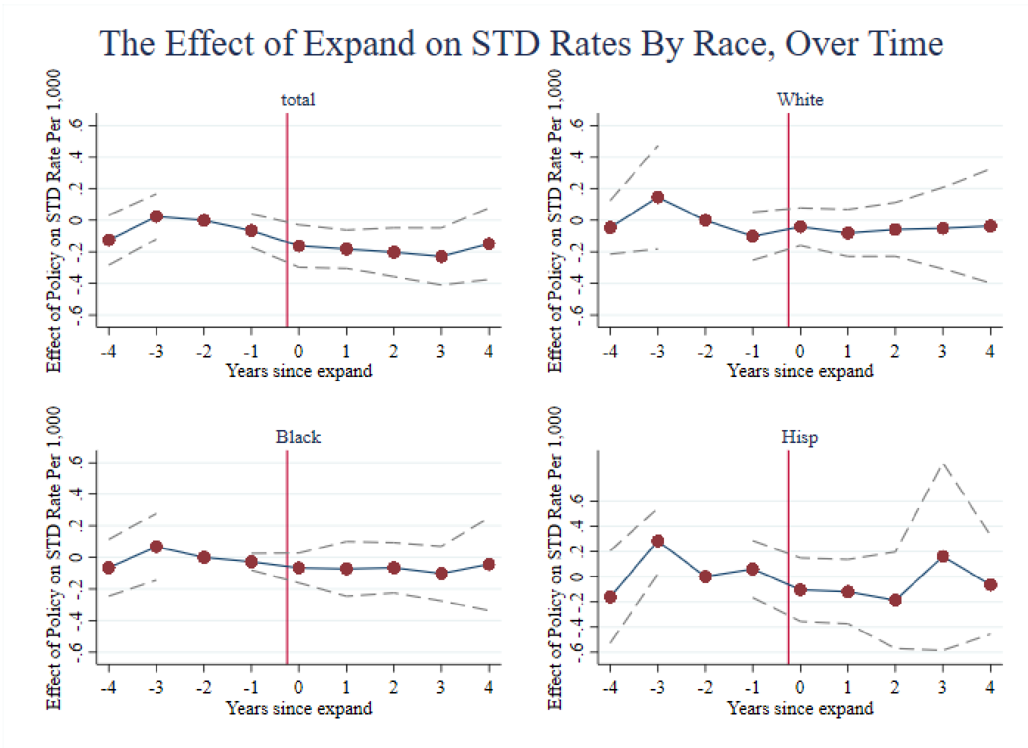


Fig. 2.10. The Effect of Restrict on Scanner Outcomes

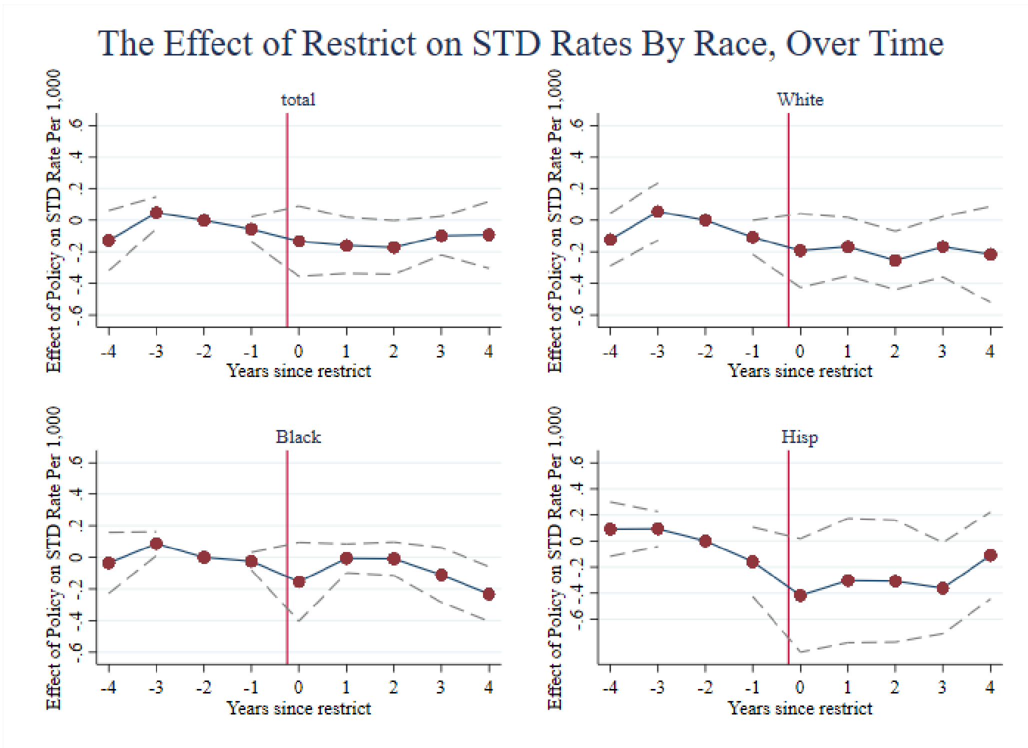


Fig. 2.11. The Effect of Restrict on Scanner Outcomes

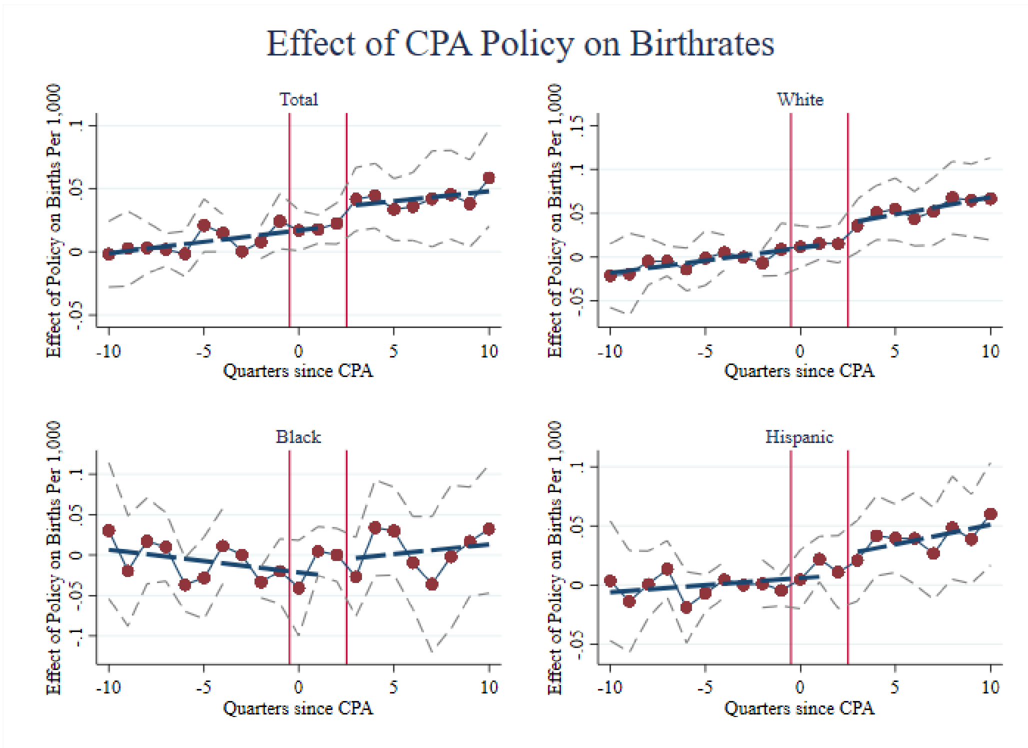


Fig. 2.12. The Effect of CPA on Birthrates

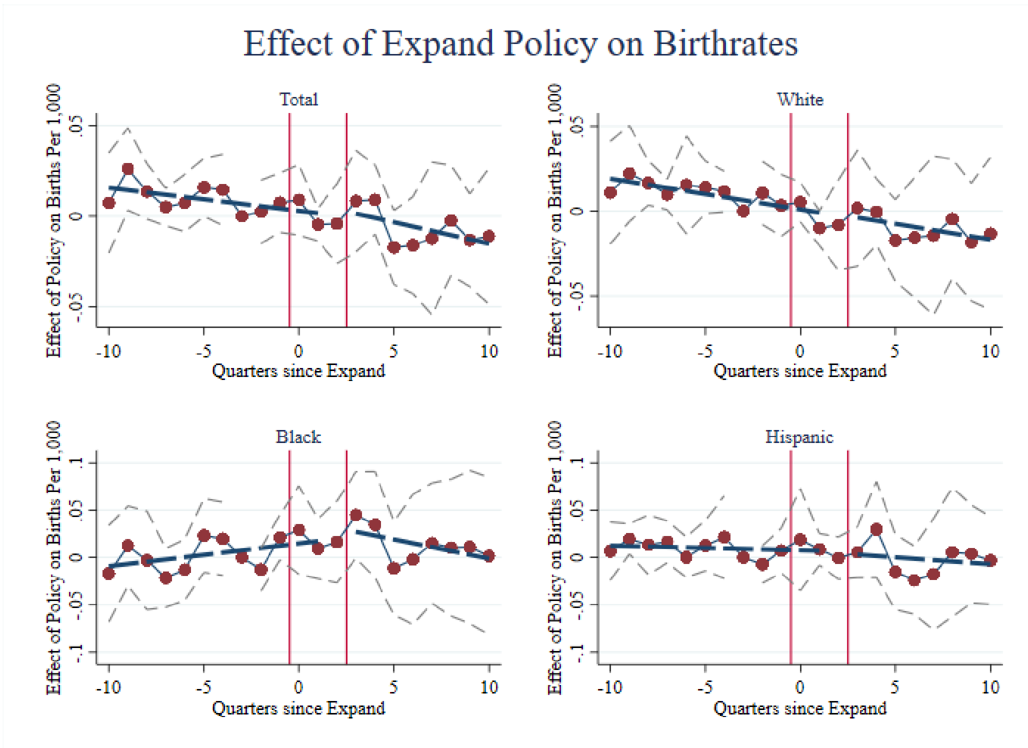


Fig. 2.13. The Effect of Expand on Birthrates

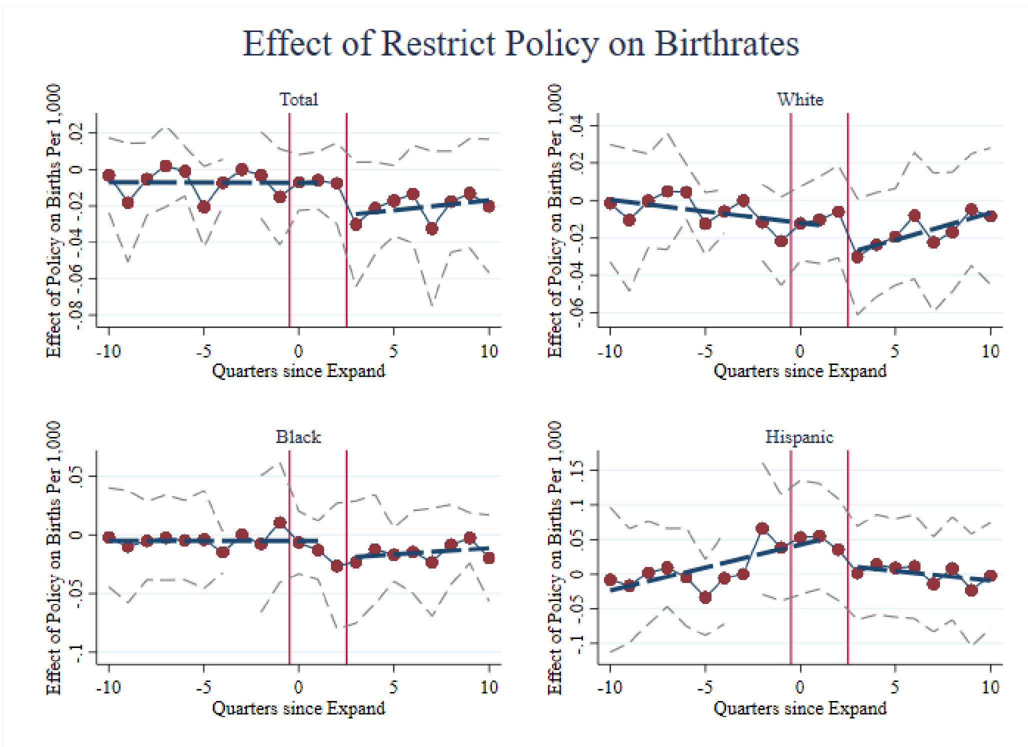


Fig. 2.14. The Effect of Restrict on Birthrates

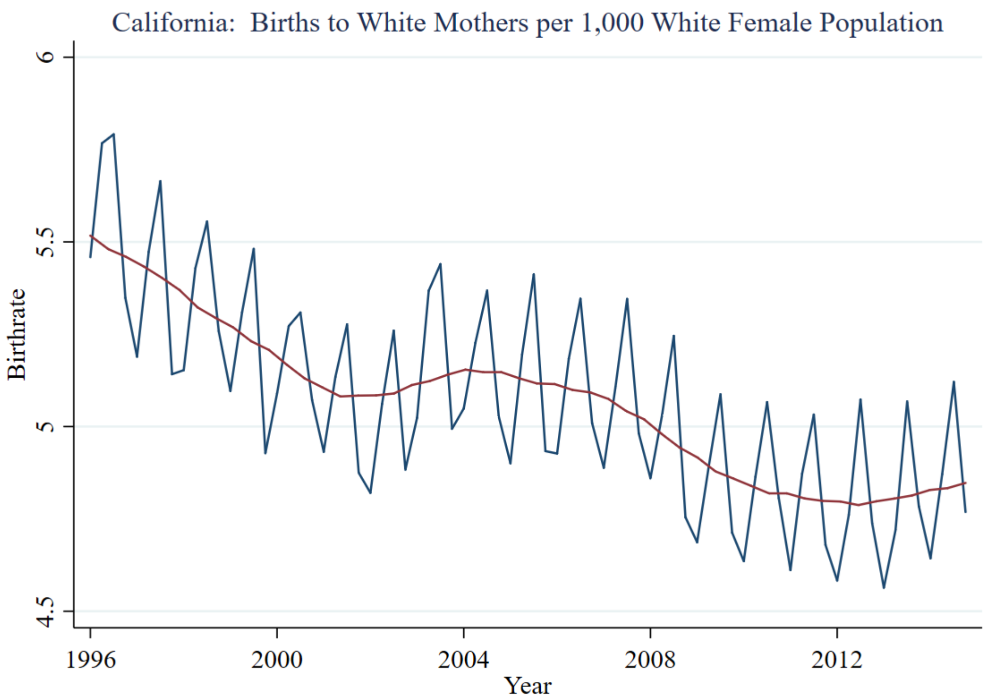


Fig. 2.15. Rate of Births to White Mothers in California Over Time

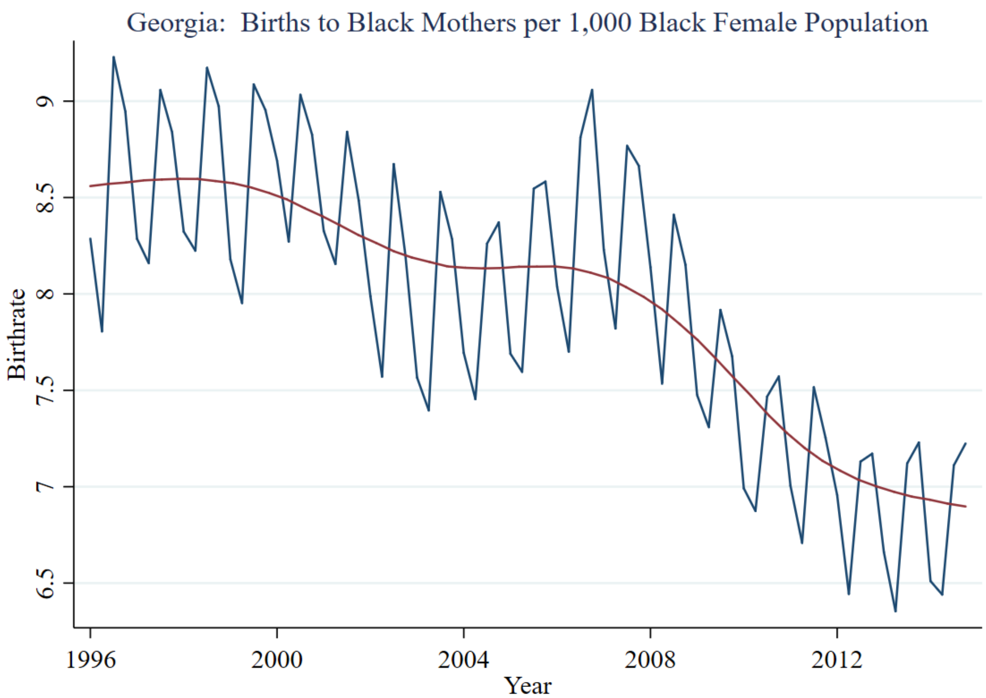


Fig. 2.16. Rate of Births to Black Mothers in Georgia Over Time

3. THE EFFECT OF OPIOID SUPPLY-SIDE INTERVENTIONS ON OPIOID-RELATED BUSINESS ESTABLISHMENTS

3.1 Introduction

Currently in the U.S., the number of deaths from drug overdoses surpasses the number of deaths from car accidents and gun homicides combined. The drug overdose death rate doubled between 1999 and 2014, and 75 percent of this increase is due to rising deaths from prescription opioids and their close substitute, heroin. Sales of prescription opioids in the U.S. quadrupled over the same period, with health care providers writing 259 million prescriptions for opioid painkillers in 2012 (Centers for Disease Control and Prevention Report 2014). Prescription opioid painkillers are morphine-like drugs effective for treating acute pain, but are habit-forming and cause breathing to slow at high dosages.

State lawmakers have passed many types of policies targeting the supply side of the market for prescription opioids to curb abuse and overdose rates. Because of reports of drug-seeking patients visiting many doctors to obtain several overlapping prescriptions at once (a practice called "doctor-shopping") all 50 states have passed prescription drug monitoring programs (PDMPs) that track patient prescription histories. PDMPs only track prescription histories for drugs classified by the Drug Enforcement Administration as controlled substances, and doctors are not required to query the system. After the passage of the initial PDMPs, 12 additional states implemented usage mandates ("Mandates") applying to the PDMP that require health providers to query patient controlled substance histories in certain circumstances. 8 states have also cracked down on over-prescribing doctors and their pain clinics, where the doctors over-prescribe opioids for profit. They often dispense prescription opioids through an on-site pharmacy, prompting legislators to pass “Pill Mill Bills” designed to limit excessive opioid prescribing. Purdue Pharma, the makers of brand-name OxyContin, reformulated the oxycodone drug in August 2010 to be more difficult to crush and dissolve, deterring the main avenues for its abuse.

While the economics literature on opioid policies is expanding, almost all papers have focused on policy effects on patients or abusers. This paper is one of the first to focus primarily on four policies' effects on businesses, and is the first paper to consider business establishments at the national level. The policies examined are PDMPs, Mandates, “Pill Mill Bills”, and the Reformulation of Oxycontin. This paper uses a difference-in-differences identification strategy to study the effect of the aforementioned supply-side intervention policies on the rate of opioid-related medical establishments at the county and state levels. I examine the effect of the policies on rates of three categories of opioid related businesses: rehabilitation facilities, doctors' offices, laboratories, and clinics, and prescription drug retailers and wholesalers.

I find that “Pill Mill Bills” decrease establishments in the industry category of “all other outpatient care centers,” (“All Other Centers and Clinics” or “AOC clinics” from here on) which includes pain therapy centers and clinics in addition to sleep therapy centers and clinics and community centers and clinics. The legislation leads to a statistically significant 6.54% decrease in AOC clinics (including pain clinics), which is about 1.7 fewer AOC clinics in the average county covered by the data, and 17 fewer of these centers at the state level. Pill Mill legislation also may decrease the number of pharmacies, equal to about 5 fewer pharmacies (out of 188 total) in the average county that passes the law, but this result is not statistically significant.

I also find that adding a “must access” Mandate to existing PDMPs is associated with an increase in residential rehabilitation facilities, like sober living homes, in the states that pass them. After the Mandate, the typical treated state realized 4.3 additional facilities.

3.2 Descriptions of Policies and Related Literature

To curb opioid abuse, lawmakers in each state have passed various types of supply-side opioid interventions. This paper examines the effects of PDMPs and their Mandates, "Pill Mill Bills," and the OxyContin reformulation on counts of opioid-related business establishments. Research on supply-side policies have shown the laws reduce oxycodone prescriptions and amounts dispensed (Bao et al. (2016), Buchmueller and Carey (2018), Kilby (2015), Mallatt (2017)) and have many other effects on opioid users. The policies decrease many measures of opioid abuse, including opioid admissions to substance abuse facilities (Dave et al., 2017), reduce doctor shopping behavior in Medicare recipients Buchmueller and Carey (2018), reduce prescription opioid overdoses (Meinhofer, 2017), and reduce violent crime (Deza and Horn, 2017). On the other hand, supply-side policies have been shown to increase heroin overdoses (Alpert et al., 2017; Evans and Power, 2017) and heroin drug crime (Mallatt, 2017).

All 50 states have implemented prescription drug monitoring programs (PDMPs)– statewide electronic systems that track patient controlled substance prescription histories. A patient is entered into the statewide system each time he or she receives an opioid at the pharmacy, and prescribers have access to the system. This allows doctors, dentists, and other prescribers within the state to search for a patient if they suspect opioid misuse. Due to low doctor usage rates, 12 states[1] passed additional usage mandates on top of existing PDMPs (referred to as "Mandates" from here on) requiring doctors to query the databases under certain circumstances.[2] PDMPs and their Mandates have been shown to effect the quantity of opioids dispensed (Kilby 2015, Mallatt 2017) and abuse outcomes such as overdoses (Kilby, 2015; Meinhofer, 2016)(Kilby 2015, Meinhofer 2017B), admissions to substance-abuse treatment facilities (Dave, Grecu and Saffer 2017, Radarkrisnan 2014), heroin crime rates (Mallatt 2017), and non-drug crime rates (Deza and Horn 2017).

[1]Delaware, Indiana, Kentucky, Louisiana, Maryland, Nevada, New Mexico, New York, Ohio, Tennessee, Vermont, and West Virginia

[2]Mandates legally require doctors to check when they're suspicious that a patient is abusing, to check with new patients or new opioid regimens, or to check before every opioid prescription.

Whereas PDMPs and their Mandates provide practitioners with additional information regarding patients, eight states[3] have passed menus of legislation, referred to as "Pill Mill Bills," to regulate and prosecute unscrupulous opioid prescribing. "Pill mills" are doctors offices, clinics and pain management centers that dispense opioids and other scheduled drugs inappropriately or for non-medical reasons. These clinics are often cash-only, dispense opioid painkillers on site, and write prescriptions with few questions asked. "Pill Mill Bills" specifically target over-prescribing practices. Details of the bills vary; some legislations require new state licensing for clinics that dispense pain medication, some require establishments to register with state department of health, others require physicians to have official pain management certification from reputable agencies, and many require physician-owners to be on site at least half the time, or limit quantities of opioids permitted to be dispensed on site.

In the case of Florida, the "Pill Mill Bill" was accompanied by law enforcement action and additional prosecution of over-prescribing doctors and practitioners. "Pill Mill Bills" have not been widely studied with the exception of Meinhofer (2017A) who examines the effect of Florida's pill mill crackdown. Florida was widely known as the epicenter of the opioid crisis in the late 2000s. Drug-seekers from around the nation traveled to Florida with confidence that they could find doctors willing to prescribe painkillers to them. The legislation in Florida's "Pill Mill Bill" of July 2011 prohibited doctors from dispensing painkillers on the site of the pain clinic and revoked or suspended state medical licenses and DEA registrations of many prescribers. As a result, the number of active pain clinic licenses dropped from 988 in 2010 to 407 in 2012. Oxycodone quantities decreased 59%, opioid substance abuse treatment facilities increased by 33%, and opioid overdose rates fell. Besides controlling for Florida's Pill Mill Bill, this paper also accounts for similar (but smaller-scale) crackdowns in Kentucky, Louisiana, Mississippi, Ohio, Tennessee, Texas, and West Virginia.

[3]Florida, Kentucky, Louisiana, Mississippi, Ohio, Tennessee, Texas, and West Virginia

This paper also examines the effect of the OxyContin reformulation. In August 2010, Purdue Pharmaceuticals reformulated its best-selling OxyContin to be more difficult to crush into a powder. OxyContin contains the active ingredient oxycodone, a powerful and widely-abused opioid. OxyContin is a 12-hour "continuous release" drug due to its patented wax coating, which Purdue claimed prevented a cycle of euphoria and subsequent crash that fosters addiction. Because of this continuous-release mechanism, OxyContin was approved in extremely large doses of oxycodone, including 80 milligram and discontinued 160 mg pills.[4] Prior to the reformulation, determined opioid abusers could crush OxyContin, circumventing the wax coating, in order to snort, smoke or inject the resulting powder. Crushing the reformulated OxyContin turns it into mush-like chunks, and mixing it with water creates a viscous gel that clogs up syringe needles. The reformulation had serious ramifications among the prescription-opioid addicted population. Studies by Alpert, Pacula and Powell (2017) and Evans, Lieber, and Power (2017) find that the August 2010 reformulation of OxyContin explains much of the increase in heroin overdoses in the 2010s.

PDMPs and their Mandates, "Pill Mill Bills" and the OxyContin reformulation have all been shown to have had significant effects on opioid abusers' behavior. Since these three types of supply-side legislative policies had significant effects on opioid abusers, there is reason to believe there might be spillover effects onto opioid-related business establishments, including rehabilitation centers and clinics, doctors' offices, and pharmacies. Meinhofer (2016) finds that pain clinic establishments in Florida plummet after the Pill Mill crackdown, and in this paper I show that Pill Mill legislation causes clinics to close in the other states that implement them as well.

3.3 Data

The outcomes in question are opioid-related business establishments. State and county counts of businesses, employees and wages by NAICS-code industry are taken

[4]In contrast, commonly-prescribed, non-continuous release Percocet contains 5-10 mg of oxycodone. Quick release pills are available in a maximum of 30 milligrams per pill.

from the Bureau of Labor Statistics Quarterly Census of Employment and Wages (QCEW) dataset from 2004 to 2015. The QCEW is reported at the quarterly level and contains counts of establishments by NAICS 6-digit industries. While the QCEW has good coverage of establishment counts, it only contains employee counts and wages for US counties containing 75,000 employees or more. NAICS codes of interest cover businesses pertaining to the opioid crisis, including drug wholesalers, pharmacies, doctors offices, outpatient mental health and substance abuse centers (excluding hospitals), AOC clinics, medical laboratories, inpatient mental health and substance abuse hospitals, and residential mental health and substance abuse facilities. AOC clinics and medical laboratories are included in the analysis because some pain clinics and pain management centers fall under the umbrella of these broad categories. The drug wholesaler outcome is included because its covers prescription drug wholesalers. Analyses using the QCEW are restricted to more populated counties with complete panel counts throughout the 2004 to 2015 period.[5]

Table 3.13 lists the 6-digit NAICS codes used to obtain counts of the relevant businesses, as well as the number of counties that have complete data of establishment counts from 2004-2015, and provides examples of businesses that fall into each industry category. The category of residential mental health facilities includes smaller residential drug addiction rehabilitation facilities as well as halfway homes and sober living homes. Inpatient mental health and substance abuse hospitals applies to large medical facilities dedicated to drug addiction and mental health treatment. Outpatient mental health and substance abuse hospitals encompass outpatient treatment centers like methadone clinics where patients do not reside at the establishment. Doctors' offices is a wide category, including offices of physicians, specialists, and surgeons. Medical laboratories include pain management centers, but also many other medical labs. The industry category of all other outpatient clinics (AOC clinics) includes out-

[5]There are missing observations in the QCEW and only counties without any missing observations of the NAICS-code of interest between 2004 and 2015 are used in each separate model. For example, 761 counties contain complete panel information on counts of residential rehabilitation facility establishments, so only those 761 counties are used to calculate the effect of the policies on residential rehab rates.

patient pain clinics, which are most likely to include Pill Mills, however this category also includes sleep disorder clinics and community health clinics which are not directly tied to the opioid crisis. The pharmacy business category captures apothecaries, drug stores and pharmacies, whereas the category of drug wholesalers covers a wide range of businesses from the relevant prescription drug merchant wholesalers and the less relevant razor blade merchant wholesalers.

Table 3.2 includes mean counts and counts per 100,000 population among each outcome's complete county panel. Covered counties for each NAICS code of interest are mapped in Figures 3.1 through 3.8. Note that complete county panels are restricted to more populous counties that have complete information on counts of establishments in each NAICS category between 2004 and 2014.[6] Medical facilities within the categories of interest are fairly uncommon; for reference, the there are about 36 gas stations per 100,000 population and 12 supermarkets or grocery stores per 100,000 across the entire US. Within the sample of counties that have residential rehab clinics, the average county has about 4.8 clinics, or a rate of 5.54 clinics per 100,000. The average county of the 53 counties containing a inpatient rehabilitation hospital has 10.49 hospitals on average, with most smaller counties containing one or two hospitals and large metro counties in California, Illinois and Texas driving up the average. 391 counties nationwide contain outpatient rehab centers, with an average of 5 centers per county, equaling 7.23 establishments per 100,000 in those counties. Pharmacies and doctors offices are more widespread, with the majority of US counties having complete panel information on the outcomes. Within this larger subsample of the US, there are an average of 20.6 pharmacies per county (18.23 pharmacies per 100,000 population) and 72 doctors offices per county (42.30 doctors offices per 100,000). Drug wholesalers, AOC clinics and medical labs are less widespread.

Table 3.14 contains analyses using County Business Patterns (CBP) data. The CBP has wider geographic coverage of businesses by NAICS code, but interpolates establishment counts when data is not available and is known to be less reliable than

[6]More populous counties have higher concentrations of the medical establishments than the country as a whole, which includes smaller counties in the denominator.

the QCEW for business counts. The CBP is collected through surveys conducted by the Business Registrar, whereas the QCEW is constructed using actual business counts via unemployment insurance information. A later section contains results of models using CBP data rather than QCEW data and explains the difference between the QCEW and CBP in more detail.

To differentiate counties' levels of oxycodone over time, I use the Drug Enforcement Administration's Automation of Reports and Consolidated Orders System (ARCOS) data. The ARCOS tracks all shipments in kilograms of oxycodone to each 3-digit zipcode by quarter. Each county's/state's 2009 level of oxycodone milligrams per capita is used to find the magnitude of the OxyContin reformulation on business establishments. In addition, models of heterogeneous effects across counties depending on their 2004 level of oxycodone milligrams per capita use data from the ARCOS.

3.4 Methodology

3.4.1 Differences-In-Differences

County level models run on each outcome variable only use data from counties with a complete panel of the outcome variable between 2004-2015, and state level models include all 50 states. I also include results from models run on the entire panel of 3,200 counties, which codes missing observations as zero. The main results are in the same direction and similar magnitudes under each approach.

To find the effect of the Prescription Drug Monitoring Programs on local opioid-related industries, I implement a difference-in-differences model. The parallel trends identifying assumption of differences-in-differences assumes treated and non-treated counties are trending similarly before the implementation of the PDMP, and would have continued to do so in the absence of the policy treatment. The models are as follows:

$$
\begin{aligned}
Outcome_{it} =& \alpha + \omega PostReformulation_{it} + \tau PostReformulation_{it} * PreReformOxycodone \\
& + \beta PDMP_{it} + \eta Mandate_{it} + \phi PillMillBill_{it} \\
& + \Psi X_{it} + \iota_i + \gamma_t + \iota_i * t + \epsilon_{it}
\end{aligned}
\tag{3.1}
$$

$Outcome_{it}$ is county (state) i's logged number of Quarterly Census of Employment and Wages residential rehabs, inpatient rehabs, outpatient rehabs, drug wholesalers, pharmacies, doctors offices, AOC clinics, or medical labs per 100,000 population in quarterly time period t.[7]

$PostReform_{it}$ and $PostReform_{it} * PreReformOxycodone$ control for changes in the outcome variables in response to the OxyContin reformulation, which has been shown to have a large impact on abusers' outcomes (Evans and Power, 2017; Alpert et al., 2017). $PostReform_{it}$ is an indicator equal to one if the OxyContin reformulation has occurred–August 2010 or later.[8] $PostReform_{it} * PreReformOxycodone$ is an interaction term, multiplying a Post-August-2010 indicator by the county's (state's) normalized mean 2009 level of quarterly oxycodone per capita from the ARCOS data.[9] τ captures a level shift in the data after the OxyContin reformulation that is proportional to a county's (state's) pre-reformulation oxycodone density, measuring intensity of treatment. τ is interpreted as the additional effect on outcomes that a county one standard deviation above the mean 2009 oxycodone level experiences compared to a county with the mean 2009 oxycodone per capita level. $PDMP_{it}$, $Mandate_{it}$ and $PillMillBill_{it}$ are dummy indicator variables equal to one if county (state) i has a PDMP, Mandate, or Pill Mill legislation bill in place at quarterly time

[7] Logged establishments per 100,000 population plus one is used to account for zeros.

[8] If not included, this would be nested in the time fixed effects and would not affect results, but is included in the model to see the overall level shift in outcome variables at the timing of the reformulation.

[9] The mean county (state) 2009 oxycodone per capita is subtracted from each county's (state's) 2009 level of oxycodone per capita and then divided by the standard deviation of the distribution of county oxycodone per capita.

period t. The coefficients of interest are ω, τ, β, η, and ϕ which measure effects of the supply-side policies on the outcome variable.

X_{it} is a county (state) and quarter set of controls, including the fraction of the population in the county (state) in the age groups: 10-19, 20-29, 30-39, 40-49, 50-59, 60-69 and 70+. I also control for the fraction of the county (state) population that is black, Hispanic, or of other non-white race, as well as the gender ratio, unemployment rate, average weekly wage rate, fraction of the workforce working in manufacturing, and fraction of the workforce working in agriculture within each county (state). ι_i is a county (state) fixed effect, which controls for each county's average level of outcome variable. γ_t is a time fixed effect, controlling for national trends in the outcome variable. $\iota_i * t$ accounts for county (state) linear time trends. Models use analytic county (state) population weights.

I extend the equation above into an event study difference-in-difference model with policy lags and leads, in the form:

$$\begin{aligned} Outcome_{it} =& \alpha + \omega PostReform_{it} + \tau PostReform_{it} * PreReformOxycodone \\ &+ \Sigma_{p=-5}^{10} \beta_p PDMP_{i,t+p} + \eta Mandate_{it} + \phi PillMillBill_{it} \\ &+ \Psi X_{it} + \iota_i + \gamma_t + \iota_i * t + \epsilon_{it} \end{aligned} \tag{3.2}$$

$PDMP_{i,t+p}$ is an indicator equal to one if the policy started in county (state) i in the time $t+p$, and is zero for all other time periods. The coefficients β_p capture the measured effect of the PDMP p periods after passage. For example, if $p = 2$, $\beta_{i,t+2}$ would capture the effect of the policy on the outcome variable 2 periods after passage. Negative values of p correspond to "leads", which capture the effect of the policy before it is implemented and should be zero under the parallel trends assumption of the difference-in-differences methodology. Event studies for the Mandate and Pill Mill Bills are adapted from Equation 3.2, replacing lags and leads of $PDMP_{i,t+p}$ with $Mandate_{i,t+p}$ or $PillMillBill_{i,t+p}$.

3.5 Results

Table 3.4 displays coefficient results from the model in equation 3.1 on logged opioid-related establishments per 100,000 population conducted at the state level. The model specification includes state and quarter fixed effects, state controls, and state-specific linear time trends. Table 3.5 displays coefficient results from the model in equation 3.1 on logged establishments per 100,000 population plus one at the county level, accounting from county and quarter fixed effects, controls and county-specific linear time trends. Table 3.6 list results at the county level using the full panel of data, where missing observations have been coded as zeros. The main findings are robust across these differing methods. Each column in Tables 3.4 and 3.5 correspond to a separate model.

Columns (1) through (8) display policy effects on residential substance abuse facilities, inpatient mental health and substance abuse centers, outpatient mental health and substance abuse centers, doctors offices, medical laboratories, AOC clinics (including pain therapy clinics), pharmacies, and drug wholesalers, respectively.

Each column displays coefficient estimates of the OxyContin reformulation. The average effect across counties of the reformulation is nested in the time fixed effects. The models allow for heterogeneous intensity-of-treatment effects in "Post-Reformulation x OxyDense," which is interpreted as the additional effect of the reformulation on a state (county) with oxycodone density one standard deviation above the mean. For example, Column (2) of Table 3.4 shows a "Post-Reform x OxyDense" coefficient of 0.0302, which means a state with oxycodone density one standard deviation above the mean level experiences a 3.02% increase in inpatient mental health and drug abuse hospitals after the OxyContin reformulation in comparison to states with the mean level of oxycodone per capita. The "PDMP," "Mandate," and "Pill Mill Bill" list coefficients on policy indicators.

Column (1) of Tables 3.4 and 3.5 list the estimates of the results for residential rehab establishments, which include "sober living" homes, substance abuse homes, and drug addiction rehabilitation facilities that are not hospitals. Neither the OxyContin reformulation nor the PDMPs without mandates appear to have a significant effect on residential rehabilitation facilities. The Mandate is associated with a 1.5% increase at the state level and a 2.5% increase at the county level in the rate of residential rehabs, but the estimated effects are not statistically significant. Examining Figure 3.9, there is not a clear effect of the PDMP, but there is an increase in the rate of facilities around the timing of the Mandate, and there is not evidence that the parallel trends assumption is violated. The treated states and counties are experiencing an increasing rate of residential facilities even before the Pill Mill Bill is passed, and there is not visual evidence that the trend is changing in response to the policy. The Mandate may be causing a 2.5% increase in residential facilities at the county level, which is equivalent to 0.065 additional residential rehabs per 100,000 population, or about 1 additional facility in the (typically more populous) counties that have a complete panel on residential rehabilitation facilities. The 1.5% increase at the state level is equal to 0.033 additional facilities per 100,000 population or 4.3 extra businesses for the typical state. In Table 3.6, the Mandate is associated with a 0.99% increase in the rate of residential rehab facilities across the full panel of 3,200 counties. This is equal to 0.24 additional facilities in the typical affected county.

In Column (2), the results for inpatient hospital rehabilitation centers are listed. This industry category includes drug addiction rehab hospitals, mental health hospitals, and detoxification hospitals. These facilities are very uncommon and only consistently recorded in 346 populous counties. None of the policies consistently effect rehabilitation hospitals. Coefficients for the Mandate and Pill Mill Bill are statistically significant on hospitals, Figure 3.10 shows violations in the parallel trends assumption. The post-policy effect appears to follow trends already present in the treated states. The interaction between the OxyContin reformation and pre-reformulation levels of oxycodone is statistically significant at the state and county level. The coefficients of 0.0302 and 0.0154 imply that states or counties with an oxycodone density one standard deviation above the mean experience 3.02% or 1.54% more rehabilitation hospitals after the reformulation in comparison to a county with an average level of oxycodone. The average county has 68 milligrams of oxycodone per capita shipped to it per quarter, and a county one standard deviation above the mean has 114 milligrams per capita in oxycodone shipments.[10] 1.5% or 3% is equivalent to 0.013-0.026 additional hospitals per 100,000 population, or 0.24-0.48 additional hospitals in the counties with complete panel data.

Column (3) contains estimates of the policy effects on outpatient rehab centers, which include methadone clinics and other alcohol and drug rehabilitation clinics with outpatient treatment. There are neither consistent nor large effects of the policies on the rate of outpatient clinics. Examining Figure 3.11, one can see that the PDMP is implemented amidst an upward trend in the rate of clinics, and Mandates and Pill Mill Bills also have trends in the outcome variable prior to the policies taking effect. Effects are noisily measured. I cannot conclude that outpatient treatment clinics are responsive to supply-side policies.

Column (4) lists policy coefficients from the model run on the rate of doctors' offices, including those of physicians, specialists and surgeons. The policies do not affect the rate of doctors' offices. The PDMP is associated with positive and significant coefficient estimates, but upon viewing Figure 3.12, one can attribute this estimate to an overall downward trend in the rate of doctor's offices within treated states that does not appear to have anything to do with the timing of the PDMP.

Column (5) displays estimates of effects on medical laboratories, which is a broad category of medical facilities, but can include pain management centers. The coefficient on the PDMP policy is consistent at the state and county level, but appears to violate the parallel trends assumption across all policies as seen in Figure 3.13. I cannot conclude that any policy has an effect on the rate of medical labs because treated states and counties are trending differently than untreated states and counties. Even within the trend, there does not appear to be a change in the downward trends around the timing of any of the policies.

[10] A Percocet contains 5-10 milligrams of oxycodone per pill, and OxyContin comes in doses ranging from 5 milligrams to 160 milligrams per pill.

Column (6) shows results for "All Other Clinic" (AOC) clinics, which include outpatient pain therapy centers and clinics as well as sleep disorder clinics and community health centers. These clinics respond strongly to the implementation of the Pill Mill Bills, which specifically target doctors' offices that prescribe opioids on-site. At both the state and county levels, the Pill Mill Bill is causing a statistically significant and robust 6-7% decrease in the rate of these clinics.[11] This is equivalent to 1.7 fewer AOC clinics in the average county and 17 fewer AOC clinics in the average state. 17 fewer AOC clinics in the average state is a relatively small estimate when compared to the effect of the Florida pill mill crackdown in 2011. The number of active pain clinic licenses fell from 988 to 407 (Meinhofer 2017A), a decrease of 581 clinics. The PDMP is associated with a slight 2.05% increase in the rate of AOC clinics, but this result is less precisely measured. This is equal to 0.74 additional clinics in the average county, or 6-7 additional clinics in the typical state. In Figure 3.14, the policy effects of the PDMP and Pill Mill Bill seem to take effect at the start of the implementation of the policies. The graphs for the PDMP at the state and county level show a level shift in the rate of clinics consistent with the start of the policy. The graphs for the Pill Mill Bill show a slight downward trend in clinics before the policy goes into effect, but the trend steepens sharply after the policy takes effect. In 3.6, Column (6) also shows a significant decrease in the rate of AOC clinics in response to the Pill Mill Bill. The coefficient -5.16% is equal to 0.95 fewer clinics in the typical affected county, slightly smaller than the estimate from the models run on the selection of counties with a complete panel.

Column (7) lists the coefficients measuring the effect of policies on the rate of pharmacies within the covered counties. Neither models at the state nor county levels find statistically significant effects of the policies on the rate of pharmacies. Figure 3.15 suggests that the PDMP and Pill Mill Bill may have a slight negative effect on pharmacies (equal to a 2.6% and 2.9% decrease, respectively) at the state and county

[11]This result is robust under an interactive fixed effects model specification, robust to bootstrapping standard errors, and robust to the removal of any one Pill Mill Bill state from the models (including the removal of Florida).

levels, but these effects are noisily measured and cannot be statistically differentiated from a zero effect. This is equivalent to 0.43 fewer pharmacies per 100,000 population or about 5 fewer pharmacies (out of 188 pharmacies) in the average covered county. When all counties are included in Table 3.6, a similar 2.65% decrease in pharmacies is equal to 4.8 fewer pharmacies (out of 187) in the typical county.

Column (8) contains results for the models on drug wholesalers, which covers prescription drug merchant wholesalers which may be influenced by the policies, as well as vitamin merchant wholesalers, razor blade merchant wholesalers, and many other businesses that are not likely affected by the policies. In both Tables 3.4 and 3.5, estimates of policy effects are around zero. The exception is the Mandate policy; at both the state and county levels, the Mandate policy is associated with a decreasing rate of drug wholesalers, but this is the product of an overall downward trend in drug wholesalers within the treated states and counties, as can be seen in Figure 3.16. There is not a notable effect of the policies on this broadly defined category of businesses.

In summary, the Mandate causes an increase in the rate of residential rehabilitation facilities (which include sober homes and substance abuse halfway homes), however this result is not statistically different from zero. The Pill Mill Bill is causing AOC clinics (a category that contains counts of pain clinics) to close, and has a statistically significant effect that is consistent across many specifications. Robustness tests of these results across different model specifications are listed in Tables 3.8 and 3.7.

3.5.1 Robustness of Main Results

Table 3.7 lists coefficients of the Pill Mill Bill on AOC clinics across different model specifications. The Pill Mill Bill effect on the rate of AOC clinics is robust, but under the model specifications without county-specific linear time trends, the point estimate reverses signs. Figure 3.17 graphs the effect of the Pill Mill Bills on AOC clinics over time under the model with fixed effects and controls but no linear trends, and suggests the positive point estimate is due to an upward trend in AOC clinics over time within treated states that is filtered out with the addition of county-specific trends. The rate of AOC clinics decreases as soon as the Pill Mill legislation goes into effect.

The effect of a Mandate added to existing PDMPs on the rate of residential rehabilitation facilities is more noisy, as seen in Table 3.8. Point estimates are typically between 0.01 and 0.03, with Column (6) containing a notable exception. Figure 3.18 graphs the lags and leads of a model of the effect of the Mandate on rehabs over time under the specification which includes fixed effects and controls while leaving out county-specific linear time trends. The rate of residential rehab centers begins a noisy upward trend after the Mandate goes into effect.

While from Tables 3.4 and 3.5, the Pill Mill Bills have a negative but insignificant effect on the rate of pharmacies, the measured effect is sensitive to model specification. Results in Table 3.9 display coefficients of Pill Mill Bills under different models. The measured effect is sensitive to the addition of county-specific time trends and switches from a positive effect to a negative effect once linear trends are accounted for. Event study graphs of the effect of the Pill Mill Bill excluding and including county-specific linear time trends are plotted in Figure 3.19. When linear time trends are not included, the treated counties experience an upward trend in the rate of pharmacies per capita, and the upward trend flattens out around the time the Pill Mill Bill goes into effect. The point estimate of the difference-in-differences model corresponding to the left graph is significant and positive because of the pre-existing trend. Adding county-specific trends in the right graph accounts for the upward trend in treated counties prior to treatment, and the rate of pharmacies decreases after the passage of the law. Under both specifications, the Pill Mill Bill is associated with a decrease in the rate of pharmacies.

3.5.2 Heterogeneity of Policy Effects Across Counties

Previous studies that examine the effects of supply side policies on opioid-related outcomes find the interventions have concentrated effects within counties with a higher rate of oxycodone milligrams per capita (Alpert et al., 2017; Evans and Power, 2017; Mallatt, 2017). To investigate whether or not supply-side policies have stronger effects on business establishments within more opioid-dense counties, I perform analyses on establishments within the top 25% of counties in terms of oxycodone milligrams per capita in 2004, obtained from the ARCOS dataset. Table 3.11 lists coefficients from the model including fixed effects, controls, and county-specific linear time trends within oxycodone-dense counties in each outcome's complete panel selected sample. Table 3.12 lists coefficients for models run across the top 25% of oxycodone dense counties across the entire sample of 3,200 counties, using 800 counties in each model. Results are similar across samples.

Column (1) shows that within the most opioid-dense counties, the Mandate significantly increases the rate of residential rehabilitation facilities by 4.76%. This effect is both more statistically significant and larger than the 1.5-2.5% increase in residential rehabs in the main result tables. Column (6) suggests that the Pill Mill Bill is associated with a 6.7% decrease in AOC clinics, which is similar to the 6-7% decrease in clinics in the main analyses across all counties. Column (7) shows a statistically significant 5.9% drop in the rate of pharmacies within opioid-dense counties, compared to the insignificant 2.15-2.85% drop in pharmacies across all counties.

3.5.3 Analyses Performed on County Business Patterns Establishment Counts

At the county level, the Quarterly Census of Employment and Wages (QCEW) and County Business Patterns (CBP) have a few differences. The QCEW has more accurate counts of establishments, but more missing counts than the CBP, requiring incomplete counties to be dropped from the main analysis. In addition, the QCEW and CBP list different counts of businesses by industry at the county level for complete counties. The CBP conducts censuses through the Business Registrar, and suffers from nonsampling errors. The QCEW, in contrast, is constructed from unemployment insurance claims and includes actual business counts rather than estimates obtained from census surveys. The QCEW is more reliable at the county level, but counties do not have complete panels of counts of establishments.[12]

Table 3.13 displays summary statistics of establishments per 100,000 population in the QCEW data used in the main text and in the CBP data. The CBP rate covers more counties and draws from fewer establishments, which helps explain why the CBP rate is always lower than the QCEW rate of establishments. The most dramatic difference between the datasets is across inpatient rehabilitation hospitals, likely because only 346 highly-populated counties contain inpatient establishments. The QCEW drops many counties containing zero inpatient facilities.

Table 3.14 lists the estimates for the effect of the supply-side policies on business establishments in the CBP. Consistent with the results in the main text, Pill Mill Bills are found to decrease the rate of AOC clinics, a category which encompasses pain therapy clinics, by a significant 3.1%. This is equivalent to 1.13 fewer AOC clinics within the average county, which is a similar magnitude to the 1.7 fewer AOC clinics in the average QCEW-covered county. It's reasonable that the CBP estimate is closer to zero because many small unaffected counties are included in the CBP model that never have AOC clinics and do not respond to the policies.

In contrast to the results in the main text, Table 3.14 does not show significant effects of the PDMP on doctors' offices nor significant effects of the Mandate on inpatient hospitals. These discrepancies may be due to the under-sampling errors within the CBP.

[12]Different statistical disclosure limitation methods are used in the QCEW and CBP as well, with CBP censoring individual establishments then aggregating and QCEW aggregating establishments and then censoring certain aggregate counts. QCEW workplace is an establishment in the Quarterly Workforce Indicator (QWI) data, produced by the Longitudinal Emplyoer-Household Dynamics program at the US Census Bureau, which tabulates measures from UI wage records. The CBP are published by the Census Bureau from inputs based on its employer Business Registrar.

3.6 Conclusion

While there is a considerable and rapidly-expanding literature on the effects of opioid supply policies on health and behavioral outcomes of patients and abusers, little is known about the effect of such policies on opioid-related industries. This paper measures the effects of PDMPs, “must access” Mandates, “Pill Mill Bills,” and the 2010 reformulation of OxyContin on the number of businesses in several industry categories, covering rehabilitation centers, doctors’ offices and clinics, and drug retailers and wholesalers. Opioid-related business establishment counts per 100,000 population by industry are obtained from the Quarterly Census of Employment and Wages. Using a difference-in-differences strategy, I show that Pill Mill Bills cause a significant decline the number of “All Other Clinics” (which encompasses pain therapy centers) per capita, causing an estimated 17 clinics to close per treated state. In addition, the Pill Mill Bills are associated with a more noisily-measured 2.9% decrease in the rate of pharmacies per capita, equal to 5 fewer pharmacies (out of 188 total pharmacies) in the average county.

Adding “must-access” Mandates to PDMPs may cause an increase in the rate of residential rehabilitation facilities in the affected states, to the tune of 4 additional facilities in the average treated state, a 1.5%-2.5% increase. This effect is more pronounced and more precisely measured within counties that are more opioid-dense prior to the policies.

The sweeping Pill Mill Bill in Florida was found to be highly effective at closing down doctors’ offices licensed to dispense pain medication (Meinhofer, 2017), causing the number of licensed facilities to fall from approximately 900 to less than 400 over two years. I find that Pill Mill Bills passed in Florida as well as Kentucky, Louisiana, Mississippi, Ohio, Tennessee, Texas, and West Virginia were effective at reducing the number of establishments in the NAICS6-classified industry covering “All Other Clinics,” and may also cause some pharmacies to close. Since Pill Mill Bills specifically target over-prescribing doctors and pharmacies, my findings suggest that these policies are effective at shutting down or deterring business establishments within relevant, but somewhat broadly-defined industry sectors across several affected states.

Table 3.1.
Outcome Variables from Quarterly Census of Employment and Wages

NAICS Code	Outcome	Complete Counties	Example List of Business
Panel A: Rehabilitation Facilities			
623220	Residential Mental Health and Substance Abuse Facilities	761	• Alcoholism and drug addiction rehabilitation facilities (except licensed hospitals) • Psychiatric convalescent homes or hospitals • Substance abuse halfway homes • “Sober living” homes • Residential group homes for the emotionally disturbed
622210	Inpatient Mental Health and Substance Abuse Hospitals	346	• Drug addiction rehab hospitals • Mental health hospitals • Detoxification hospitals
621420	Outpatient Mental Health and Substance Abuse Centers (excludes hospitals)	1165	• Outpatient drug addiction treatment centers and clinics • Outpatient alcoholism treatment centers and clinics
Panel B: Doctor’s Offices, Laboratories, and Clinics			
621111	Doctors Offices	2,342	• Physicians’ Offices • Specialists’ Offices • Surgeons’ Offices
621511	Medical Laboratories	662	• Pain Management Centers • Blood analysis laboratories • Laboratory testing services, medical
621498	All Other Outpatient Clinics	708	• Outpatient pain therapy centers and clinics • Outpatient sleep disorder centers and clinics • Outpatient community health centers and clinics
Panel C: Prescription Drug Retailers and Wholesalers			
446110	Pharmacies	2,404	• Apothecaries, drug stores, and pharmacies
424210	Drug Wholesalers	589	• Prescription drug merchant wholesalers • Vitamins merchant wholesalers • Deodorants, personal merchant wholesalers • Blades, razor merchant wholesalers

c

Table 3.2.
Mean Number of Establishments by NAICS Type in Covered Counties

Outcome	Counties	Establishment Rate Per 100,000 in Covered Counties	Establishments in Average Covered County
Panel A: Rehabilitation Facilities			
Residential Rehabilitation	761	4.814	25.81
Inpatient Rehab Hospitals	346	2.270	14.54
Outpatient Rehab Centers	1,165	6.056	22.00
Panel B: Doctor's Offices, Laboratories, and Clinics			
Doctors Offices	2,342	47.041	910.09
Medical Labs	662	3.967	50.31
Other Outpatient Clinics	708	4.663	20.86
Panel C: Prescription Drug Retailers and Wholesalers			
Pharmacies	2,404	19.429	187.46
Drug Wholesalers	589	3.865	38.36

Table 3.3.
Summary Statistics Of Control Variables By County

Control	Observations	Mean	Standard Deviation
Fraction Age 10-19	150,792	0.138	0.016
Fraction Age 20-29	150,792	0.139	0.031
Fraction Age 30-39	150,792	0.132	0.018
Fraction Age 40-49	150,792	0.142	0.016
Fraction Age 50-59	150,792	0.134	0.015
Fraction Age 60-69	150,792	0.093	0.021
Fraction Age 70+	150,792	0.092	0.027
Fraction Female	150,792	0.508	0.013
Fraction Black	150,792	0.128	0.129
Fraction Hispanic	150,792	0.160	0.164
Fraction Other Non-White Race	150,792	0.061	0.074
Fraction Workforce Manufacturing	150,792	0.122	0.087
Fraction Workforce Agriculture	150,792	0.014	0.073
Average Weekly Wage	150,792	$834.28	$233.56
Fraction Unemployed	150,792	0.056	0.023
Oxycodone Density 2004	150,768	35.47	23.67

Age, race, and gender data from Census Bridged Population Estimates, workforce and wage data from QCEW, unemployment statistics from BLS Local Area Unemployment Statistics, and oxycodone density from DEA ARCOS.

Table 3.4.
Effect of Policies on Establishments Per 100,000 Population, State Level

	(1) Res. Rehab	(2) Inpatient	(3) Outpatient	(4) Doc. Office	(5) Med. Labs	(6) AOC Clinics	(7) Pharmacies	(8) Drug Wholesale
Post-Reformulation	-0.0366**	0.0302*	0.0214*	0.00183	-0.0251	0.0229	0.00566	0.0260
x OxyDense	(0.0156)	(0.0158)	(0.0122)	(0.00411)	(0.0181)	(0.0154)	(0.0226)	(0.0163)
PDMP	-0.000140	0.0137	0.0128	-0.00668*	-0.0279*X	0.0205*	-0.0300	-0.000870
	(0.0108)	(0.0146)	(0.00993)	(0.00358)	(0.0166)	(0.0110)	(0.0191)	(0.0165)
Mandate	0.0156	0.108***	0.00389	0.00758	-0.0368	0.00437	-0.0146	-0.0384
	(0.0182)	(0.0321)	(0.0149)	(0.00677)	(0.0322)	(0.0314)	(0.0220)	(0.0235)
Pill Mill Bill	0.00878	-0.0247*X	0.00316	-0.00850	-0.0254	-0.0764***	-0.0215	-0.0243
	(0.0222)	(0.0147)	(0.0159)	(0.00560)	(0.0354)	(0.0220)	(0.0140)	(0.0204)
Observations	2163	2163	2163	2163	2163	2163	2163	2163
FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
LTT	X	X	X	X	X	X	X	X

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; *X, **X, or ***X indicates significance with a failure of the parallel trends assumption.

Table 3.5.
Effect of Policies on Establishments Per 100,000 Population, County Level

	(1) Res. Rehab	(2) Inpatient	(3) Outpatient	(4) Doc. Office	(5) Med. Labs	(6) AOC Clinics	(7) Pharmacies	(8) Drug Wholesale
Post-Reformulation	0.00166	0.0154*	0.0001	0.0010	-0.00964	0.0145*	-0.00597	0.0001
x OxyDense	(0.00513)	(0.00641)	(0.0045)	(0.0016)	(0.00687)	(0.00817)	(0.00544)	(0.0078)
PDMP	-0.00854	-0.00634	0.0223*	-0.00767*	-0.0319	0.0206	-0.0261	0.0154
	(0.0110)	(0.0111)	(0.0122)	(0.0040)	(0.0204)	(0.0166)	(0.0215)	(0.0151)
Mandate	0.0248	0.0716**	-0.0156	0.00331	-0.00912	-0.0144	0.0168	-0.0539**
	(0.0169)	(0.0310)	(0.0152)	(0.00897)	(0.0328)	(0.0399)	(0.0216)	(0.0245)
Pill Mill Bill	0.0191	-0.0424*	0.0150*	-0.00373	-0.00045	-0.0654***	-0.0285	0.0051
	(0.0152)	(0.0223)	(0.0089)	(0.00584)	(0.0317)	(0.0220)	(0.0215)	(0.0183)
Observations	35,631	16,159	54,170	108,760	30,936	33,053	111,629	27,589
Counties Used	761	346	1,165	2,342	662	708	2,404	589
FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
LTT	X	X	X	X	X	X	X	X

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6.
Effect of Policies on Establishments Per 100,000 Population, County Level Using Full Panel

	(1) Res. Rehab	(2) Inpatient	(3) Outpatient	(4) Doc. Office	(5) Med. Labs	(6) AOC Clinics	(7) Pharmacies	(8) Drug Wholesale
Post-Reformulation x OxyDense	-0.0000592	0.00888*	0.00240	0.00142	-0.00565	0.0108	-0.00610	0.00513
	(0.00531)	(0.00526)	(0.00458)	(0.00164)	(0.00586)	(0.00735)	(0.00507)	(0.00660)
PDMP	-0.00888	-0.00506	0.0193	-0.0133**	-0.0142	0.0196	0.0138	0.0149
	(0.0128)	(0.0144)	(0.0127)	(0.00608)	(0.0288)	(0.0146)	(0.0158)	(0.0141)
Mandate	0.00993	0.0882**	0.00136	0.00465	-0.0227	0.0151	0.0155	-0.0453**
	(0.0150)	(0.0427)	(0.0160)	(0.0101)	(0.0289)	(0.0330)	(0.0211)	(0.0182)
Pill Mill Bill	0.0257	-0.0172	0.00892	-0.00245	-0.0130	-0.0516**	-0.0265	0.00467
	(0.0176)	(0.0290)	(0.0116)	(0.00556)	(0.0354)	(0.0239)	(0.0281)	(0.0171)
Observations	121419	121419	121419	121419	121419	121419	121419	121419
Counties Used	3,200	3,200	3,200	3,200	3,200	3,200	3,200	3,200
FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X
LTT	X	X	X	X	X	X	X	X

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7.
Testing Robustness of Pill Mill Bill on AOC Clinic Facilities

	(1) FE	(2) Controls	(3) LTT	(4) No Wt	(5) Drop FL	(6) No Log	(7) Drop Zeros
Pill Mill Bill	0.0323	0.0145	-0.0656***	-0.0207	-0.0544*	-0.0207	-0.102***
	(0.0313)	(0.0274)	(0.0219)	(0.0187)	(0.0296)	(0.0187)	(0.0341)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models include fixed effects, controls, analytic weights and county-specific linear time trends.

Table 3.8.
Testing Robustness of the Mandate on Residential Rehabilitation Facilities

	(1) FE	(2) Controls	(3) LTT	(4) No Wt	(5) Drop FL	(6) No Log	(7) Drop Zeros
Mandate	0.0171	0.0161	0.0248	-0.002	0.0316	-0.124	0.0102
	(0.0272)	(0.0290)	(0.0169)	(0.0158)	(0.0162)	(0.143)	(0.0196)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models include fixed effects, controls, analytic weights and county-specific linear time trends.

Table 3.9.
Testing Robustness of Pill Mill Bill on the Rate of Pharmacies

	(1) FE	(2) Controls	(3) LTT	(4) No Wt	(5) Drop FL	(6) No Log	(7) Drop Zeros
Pill Mill Bill	0.0811*	0.0771*	-0.0284	-0.0378*	-0.0092	-0.0378*	-0.0293
	(0.0379)	(0.0290)	(0.0216)	(0.0145)	(0.0207)	(0.0145)	(0.0235)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models include fixed effects, controls, analytic weights and county-specific linear time trends.

Table 3.10.
Effect of Policies on Establishments per 100,000 Population within Top 25% of Counties by Oxycodone Milligrams Per Capita

	(1) Res. Rehab	(2) AOC Clinics	(3) Pharmacies
Post-Reformulation	0.0022	0.0217**	-0.0079
x OxyDense	(0.0077)	(0.0106)	(0.0075)
PDMP	-0.0036	0.0169	0.0163
	(0.0213)	(0.0243)	(0.0181)
Mandate	0.0476**	-0.0064	0.0114
	(0.0222)	(0.0492)	(0.0293)
Pill Mill Bill	0.0409	-0.0670	-0.0592***
	(0.0312)	(0.0459)	(0.0147)
N	14261	12744	33188

Table 3.11.
Effect of Policies on Establishments per 100,000 Population within Top 25% of Counties by Oxycodone Milligrams Per Capita

	(1) Res. Rehab	(2) Inpatient	(3) Outpatient	(4) Doc. Office	(5) Med. Labs	(6) AOC Clinics	(7) Pharmacies	(8) Drug Wholesale
Post-Reformulation	0.00186	0.0147^{**}	-0.00336	0.000455	-0.0231^{***}	0.0248^{**}	-0.00785	-0.0193^{*}
x OxyDense	(0.00754)	(0.00580)	(0.00648)	(0.00277)	(0.00750)	(0.00996)	(0.00756)	(0.00999)
PDMP	-0.00152	-0.0336^{*}	0.0294^{**}	-0.0180^{***}	-0.0349	0.0274	0.0116	0.0240
	(0.0205)	(0.0168)	(0.0130)	(0.00445)	(0.0346)	(0.0265)	(0.0199)	(0.0273)
Mandate	0.0346	0.0522^{**}	-0.0324	0.00783	0.0232	0.00634	0.0224	-0.0651^{***}
	(0.0218)	(0.0230)	(0.0255)	(0.00868)	(0.0378)	(0.0481)	(0.0316)	(0.0219)
Pill Mill Bill	0.0500	-0.0518^{*}	0.0156	0.00848	0.0363	-0.0805	-0.0742^{***}	0.00226
	(0.0328)	(0.0275)	(0.0155)	(0.00874)	(0.0603)	(0.0507)	(0.0238)	(0.0376)
N	15744	7776	23712	39744	14496	13872	40176	11616

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12.
Effect of Policies on Establishments per 100,000 Population within Top 25% of Counties by Oxycodone Milligrams Per Capita, Full Panel of Counties

	(1) Res. Rehab	(2) Inpatient	(3) Outpatient	(4) Doc. Office	(5) Med. Labs	(6) AOC Clinics	(7) Pharmacies	(8) Drug Wholesale
Post-Reformulation	0.00716	0.00719^{*}	-0.00368	0.000828	-0.0234^{***}	0.0108	-0.00775	-0.00635
x OxyDense	(0.0102)	(0.00404)	(0.00643)	(0.00273)	(0.00554)	(0.00875)	(0.00761)	(0.00756)
PDMP	0.0232	-0.0168	0.0360^{**}	-0.0188^{***}	-0.0375	0.0345	0.0112	0.0263
	(0.0174)	(0.0204)	(0.0152)	(0.00454)	(0.0333)	(0.0223)	(0.0200)	(0.0233)
Mandate	0.0207	0.0517^{*}	-0.0232	0.00838	0.0203	0.0210	0.0223	-0.0657^{***}
	(0.0216)	(0.0276)	(0.0276)	(0.00859)	(0.0351)	(0.0406)	(0.0316)	(0.0203)
Pill Mill Bill	0.0233	-0.0339	0.0158	0.00863	0.00290	-0.0639	-0.0748^{***}	-0.00448
	(0.0308)	(0.0342)	(0.0189)	(0.00881)	(0.0572)	(0.0480)	(0.0235)	(0.0306)
N	42816	42816	42816	42816	42816	42816	42816	42816

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13.
Information on Counties in the Top Quartile of Oxycodone Milligrams Per Capita

Outcome	QCEW Covered Counties	QCEW Rate Per 100,000 in Covered Counties	CBP Rate Per 100,000 in 3,187 Counties
Panel A: Rehabilitation Facilities			
Residential Rehabilitation	761	4.814	1.648
Inpatient Rehab Hospitals	346	2.270	0.173
Outpatient Rehab Centers	1,165	6.056	3.543
Panel B: Doctor's Offices, Laboratories, and Clinics			
Doctors Offices	2,342	47.041	42.49
Medical Labs	662	3.967	1.060
All Other Outpatient Clinics	708	4.663	4.185
Panel C: Prescription Drug Retailers and Wholesalers			
Pharmacies	2,404	19.429	16.916
Drug Wholesalers	589	3.865	1.191

The CBP covers more counties, but the QCEW covers more establishments within covered counties.

Table 3.14.
The Effect of Supply Side Policies on Opioid-Related Establishments, County Business Pattern Data

	(1) Res. Rehab	(2) Inpatient	(3) Outpatient	(4) Doc. Office	(5) Med. Labs	(6) AOC Clinics	(7) Pharmacies	(8) Drug Wholesale
PostReform	-0.0376	0.0186	0.0206	-0.0477**	0.267***	0.209***	-0.0540**	-0.0680
	(0.0408)	(0.0197)	(0.0703)	(0.0215)	(0.0526)	(0.0437)	(0.0250)	(0.0501)
PostReformulation x OxyDense	0.000727	-0.000205	-0.000110	0.00409**	-0.0162***	0.00984*	-0.000917	0.00398
	(0.00910)	(0.00218)	(0.00636)	(0.00166)	(0.00465)	(0.00548)	(0.00429)	(0.00478)
PDMP	-0.00112	0.000986	0.0223	-0.00115	0.0217*	-0.00256	0.00937*	0.0232*
	(0.0142)	(0.00561)	(0.0175)	(0.00421)	(0.0121)	(0.0126)	(0.00501)	(0.0118)
Mandate	0.0159	0.00617	-0.0391	0.00115	0.0146	0.0119	-0.00171	-0.0295**
	(0.0143)	(0.0102)	(0.0245)	(0.0115)	(0.0163)	(0.0188)	(0.00744)	(0.0144)
Pill Mill Bill	0.0200*	0.00744	0.0103	0.000449	-0.00343	-0.0308**	0.0175*	-0.0103
	(0.0109)	(0.00881)	(0.0211)	(0.00387)	(0.0172)	(0.0150)	(0.00898)	(0.0269)
N	119393	119393	119393	119393	119393	119393	119393	119393

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

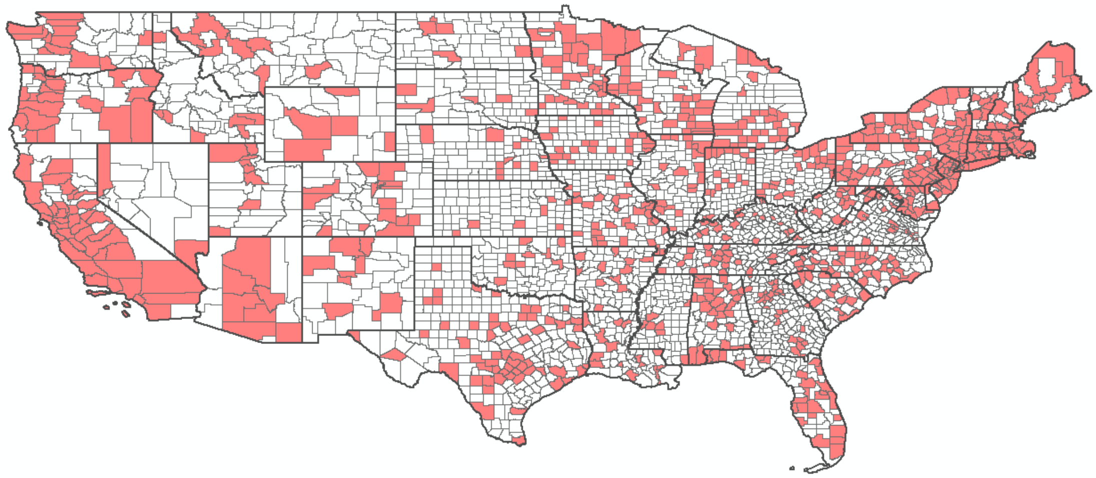


Fig. 3.1. Counties with Complete Panel of Residential Rehabilitation Facilities

The figure displays the location of the 761 counties used to identify the effect of the PDMP on the rate of residential rehabilitation facilities per 100,000 population. Data: QCEW 2004-2015.

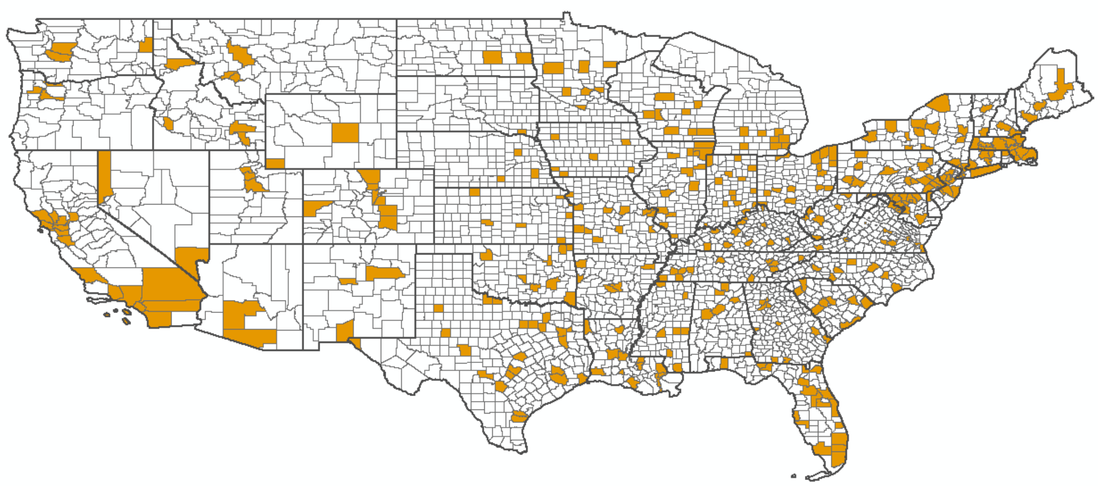


Fig. 3.2. Counties with Complete Panel of Inpatient Rehabilitation Hospitals

The figure displays the location of the 346 counties used to identify the effect of the PDMP on the rate of inpatient rehabilitation hospitals per 100,000 population. Data: QCEW 2004-2015.

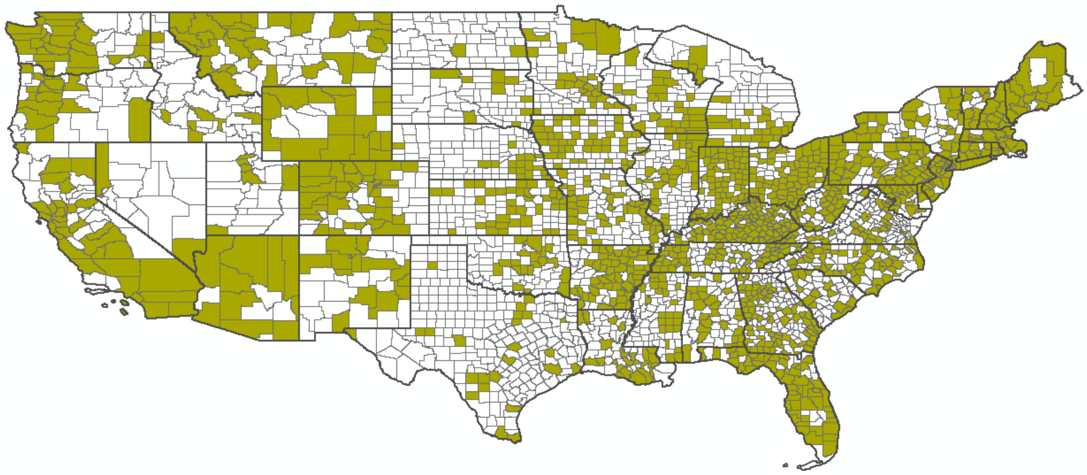


Fig. 3.3. Counties with Complete Panel of Outpatient Rehabilitation Centers

The figure displays the location of the 1,165 counties used to identify the effect of the PDMP on the rate of outpatient rehabilitation centers per 100,000 population. Data: QCEW 2004-2015.

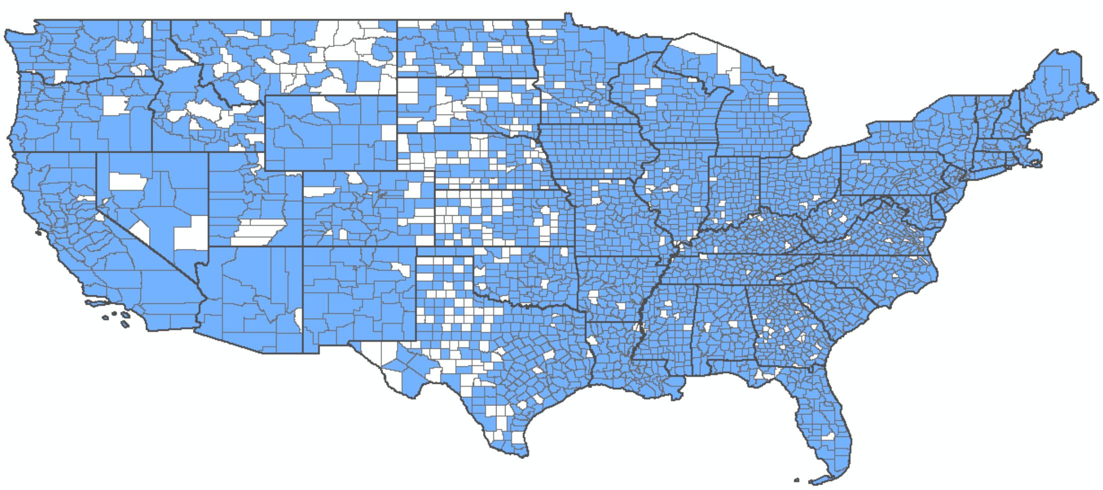


Fig. 3.4. Counties with Complete Panel of Doctors Offices

The figure displays the location of the 2,342 counties used to identify the effect of the PDMP on the rate of doctors offices per 100,000 population. Data: County Business Patterns 2004-2015.

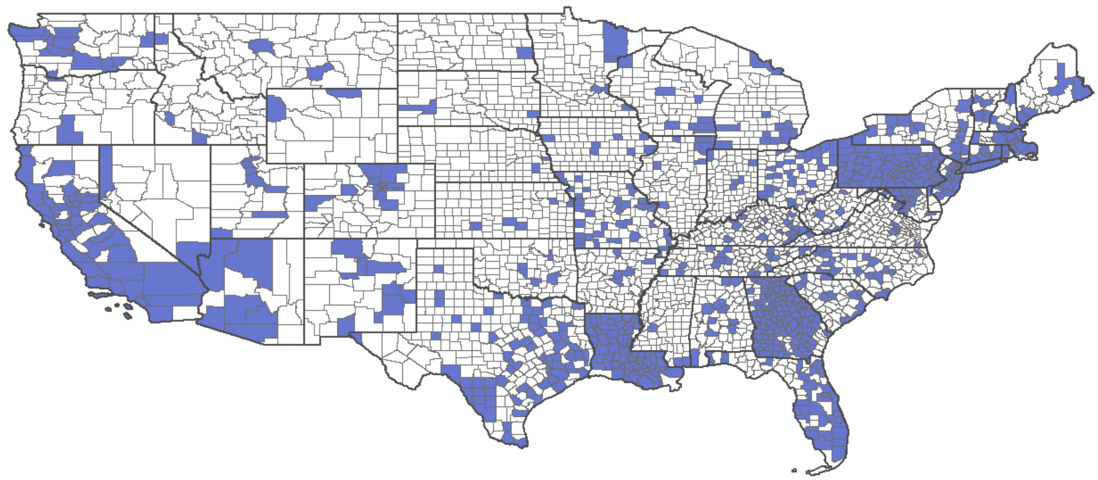


Fig. 3.5. Counties with Complete Panel of All Other Outpatient Care Centers

The figure displays the location of the 662 counties used to identify the effect of the PDMP on the rate of AOC clinics per 100,000 population. Data: QCEW 2004-2015.

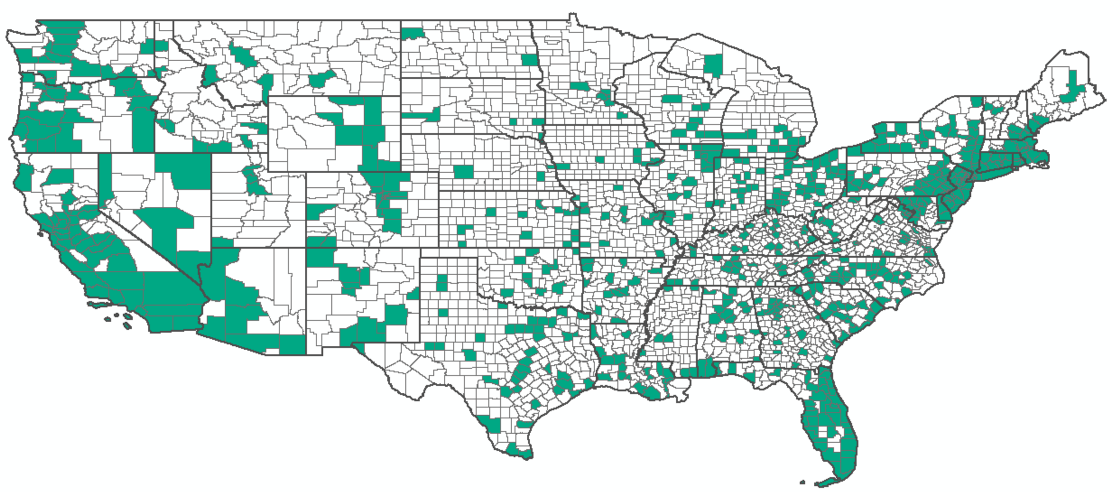


Fig. 3.6. Counties with Complete Panel of Medical Laboratories

The figure displays the location of the 7708 counties used to identify the effect of the PDMP on the rate of medical laboratories per 100,000 population. Data: QCEW 2004-2015.

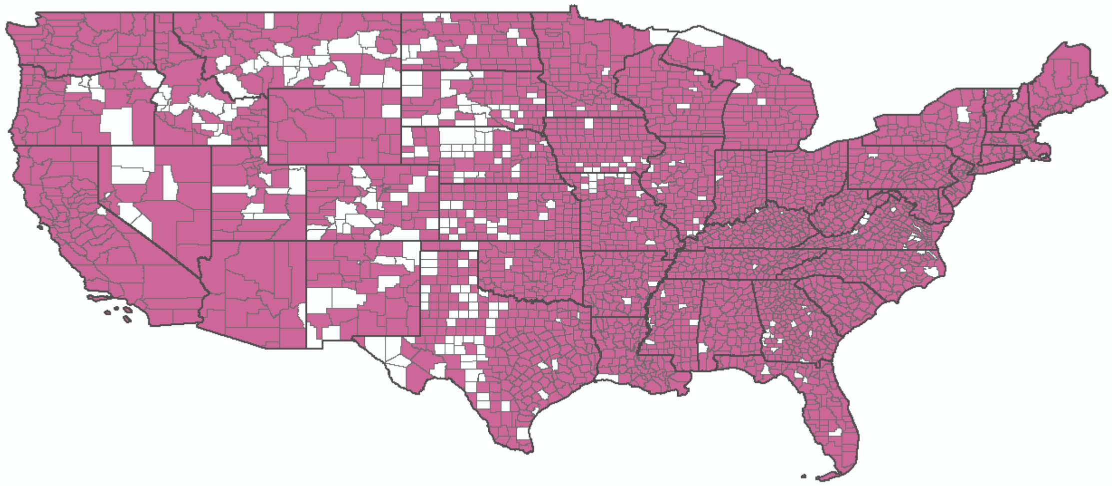


Fig. 3.7. Counties with Complete Panel of Pharmacies

The figure displays the location of the 2,404 counties used to identify the effect of the PDMP on the rate of pharmacies per 100,000 population. Data: QCEW 2004-2015.

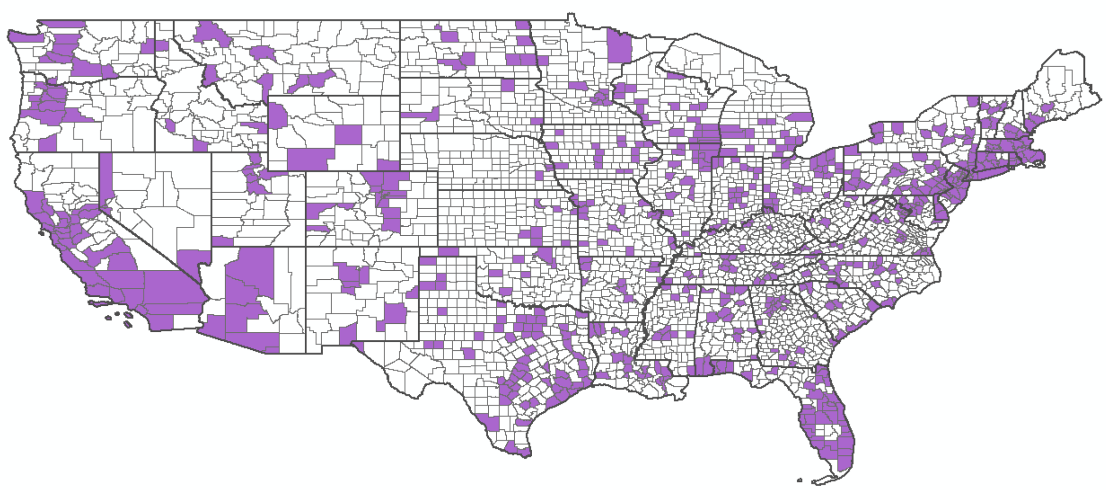


Fig. 3.8. Counties with Complete Panel of Drug Store Wholesalers

The figure displays the location of the 589 counties used to identify the effect of the PDMP on the rate of drug store wholesalers per 100,000 population. Data: QCEW 2004-2015.

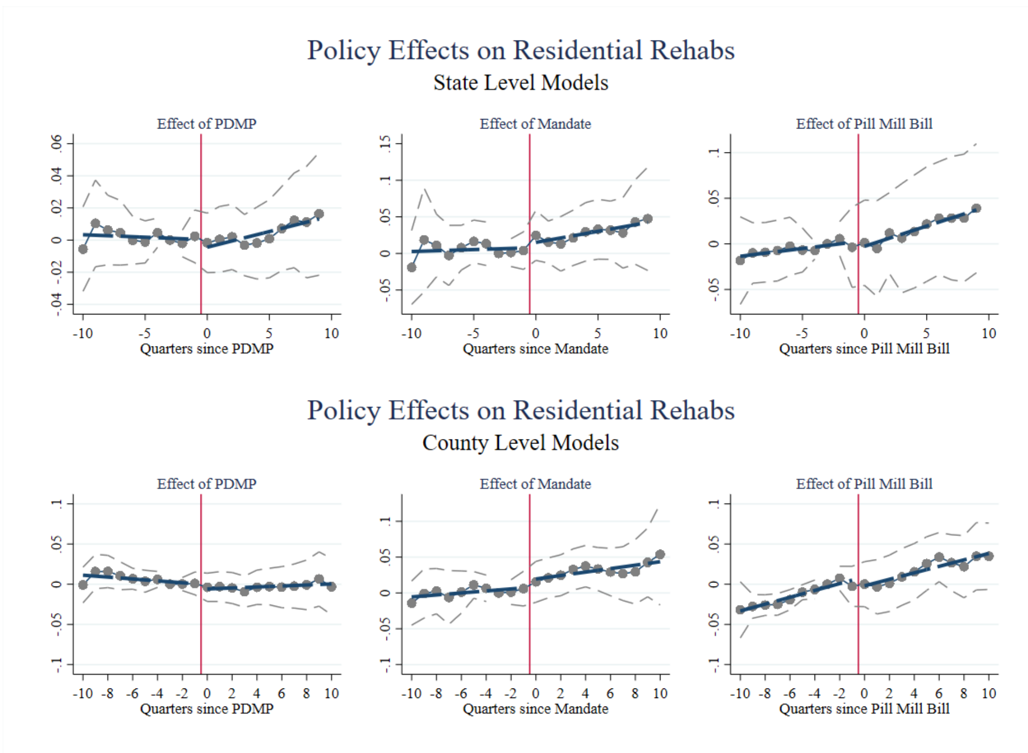


Fig. 3.9. Supply-Side Intervention Policy Effects on Residential Rehabilitation Establishments

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

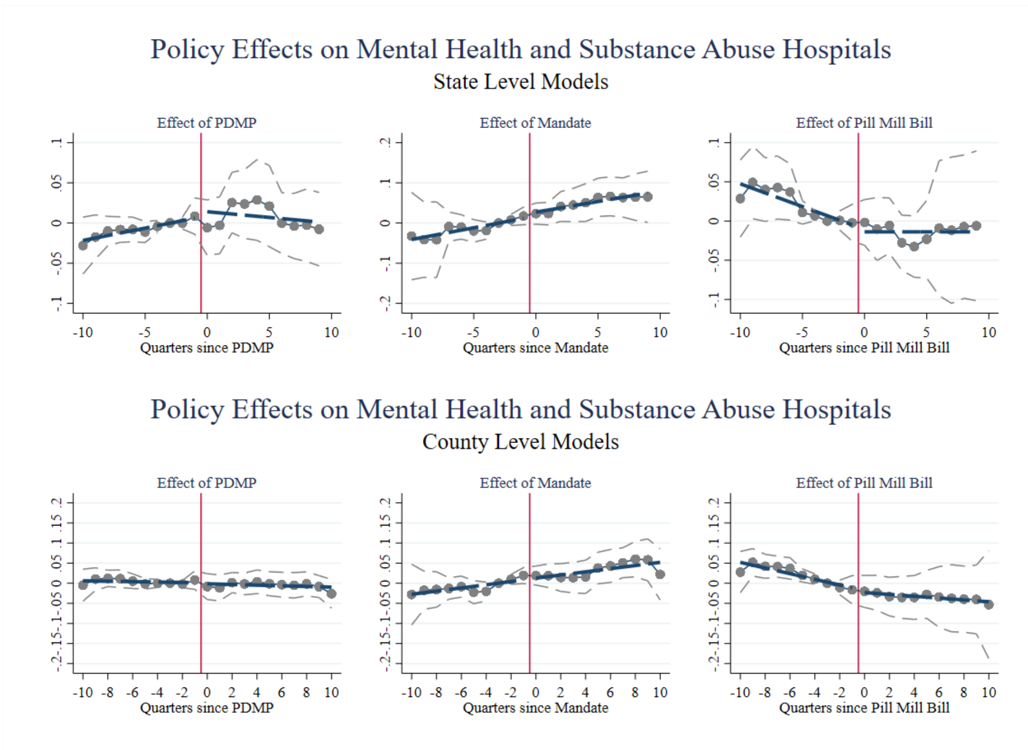


Fig. 3.10. Supply-Side Intervention Policy Effects on Inpatient Mental Health and Substance Abuse Hospitals

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

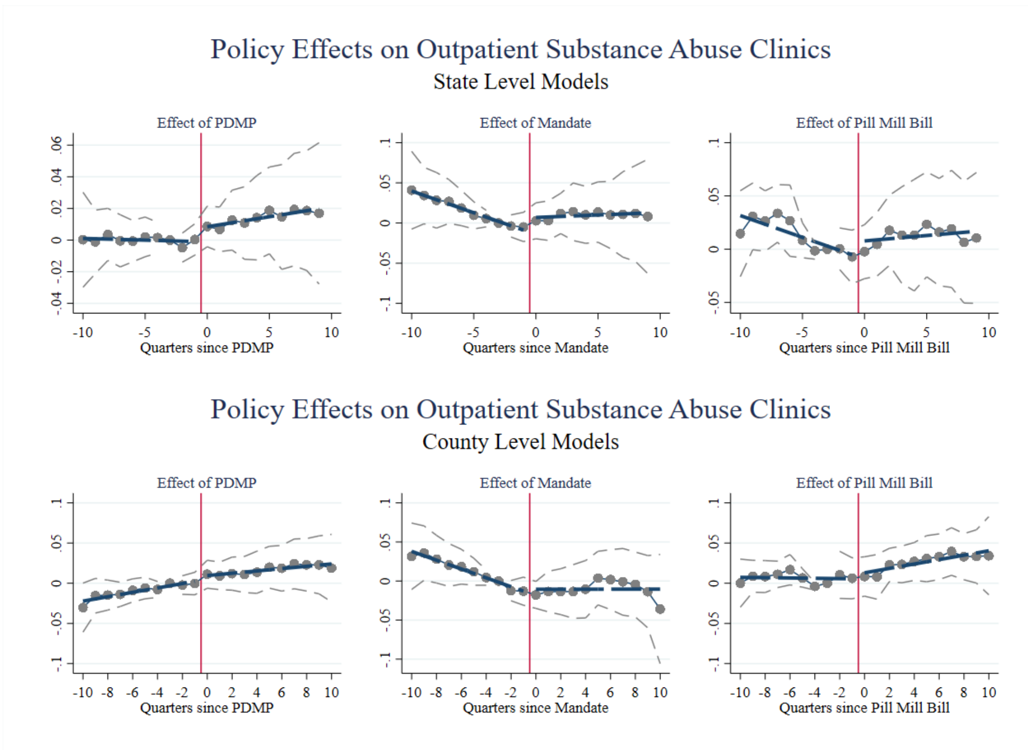


Fig. 3.11. Supply-Side Intervention Policy Effects on Outpatient Substance Abuse Rehabilitation Establishments

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

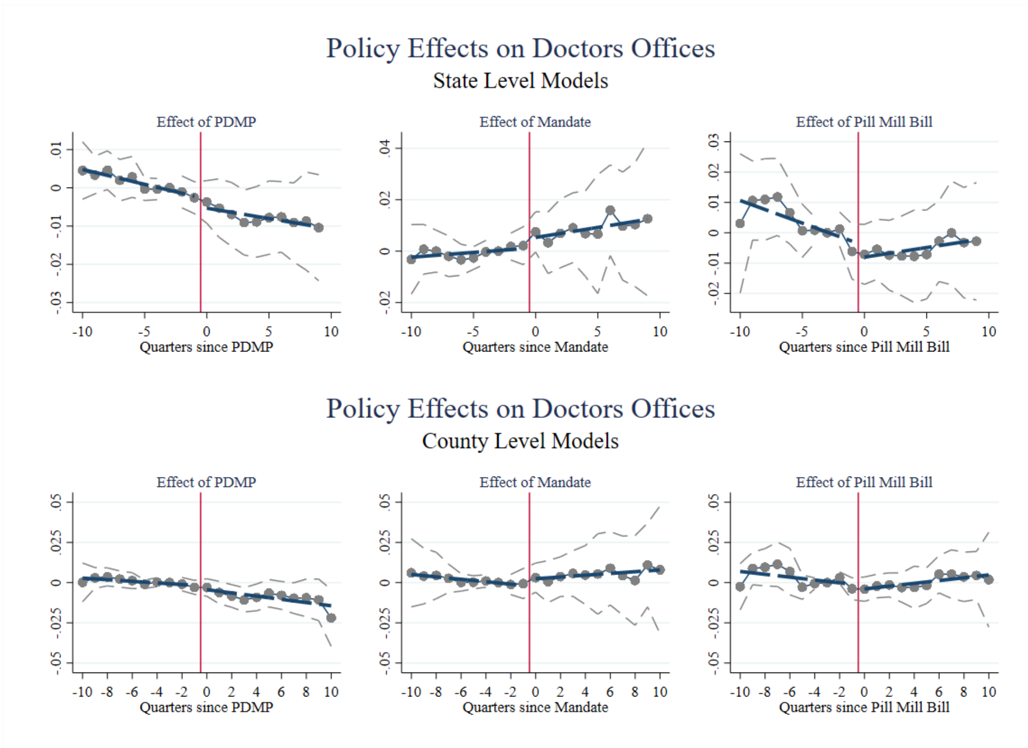


Fig. 3.12. Supply-Side Intervention Policy Effects on Doctors' Offices

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

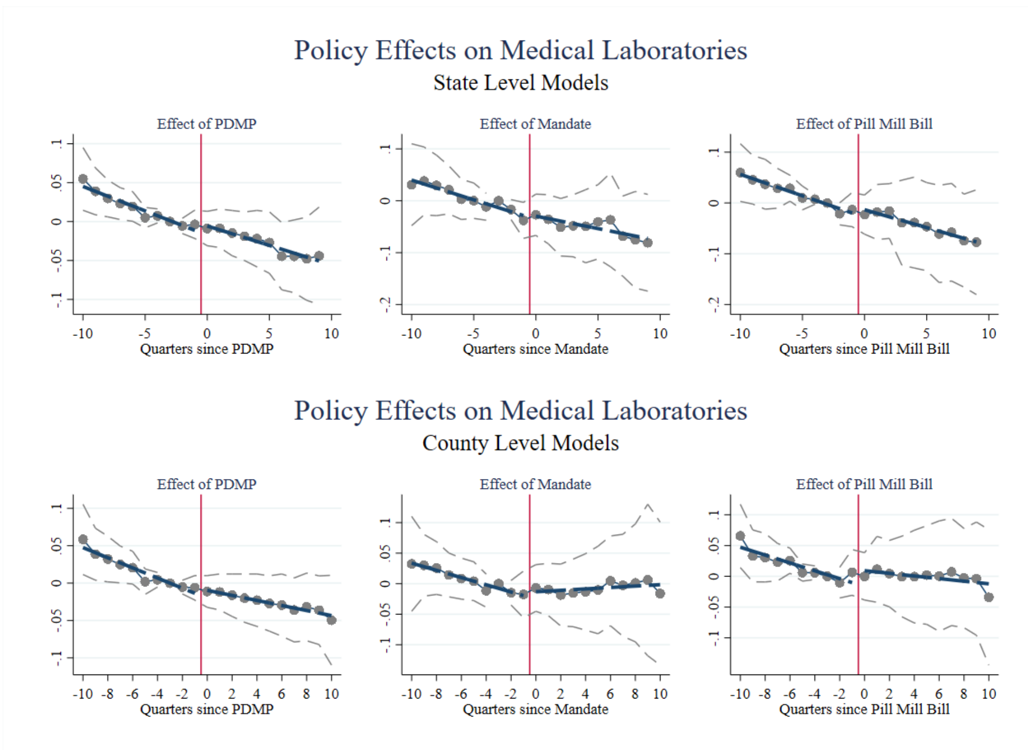


Fig. 3.13. Supply-Side Intervention Policy Effects on Medical Laboratories

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

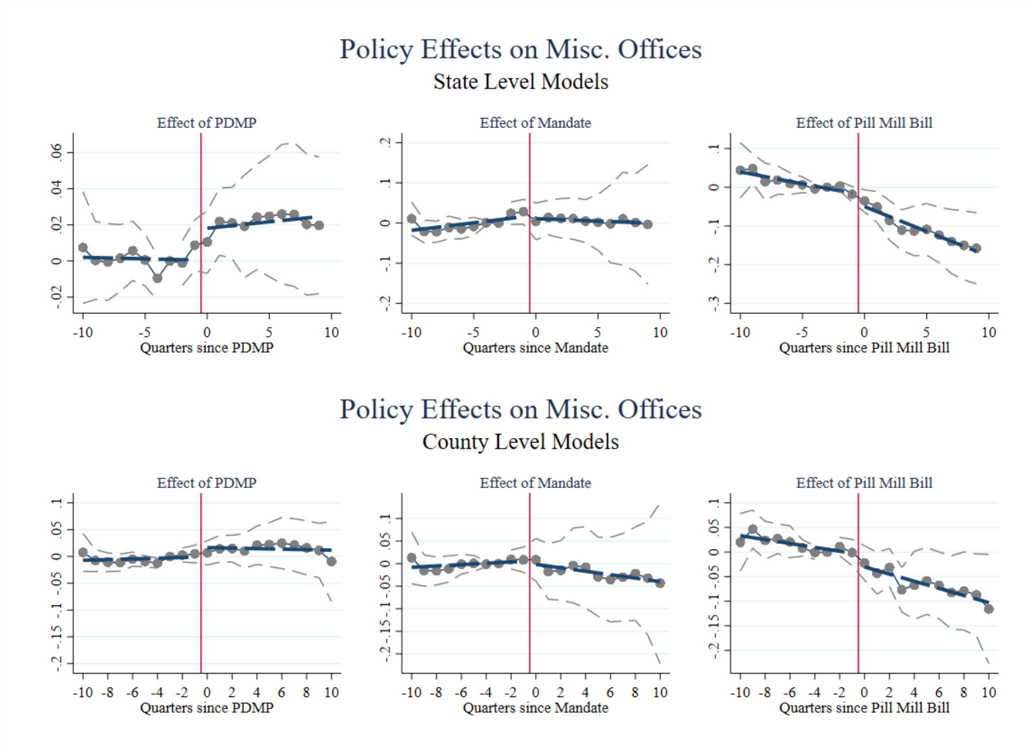


Fig. 3.14. Supply-Side Intervention Policy Effects on All Other Outpatient Care Centers, Including Pain Therapy Clinics

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

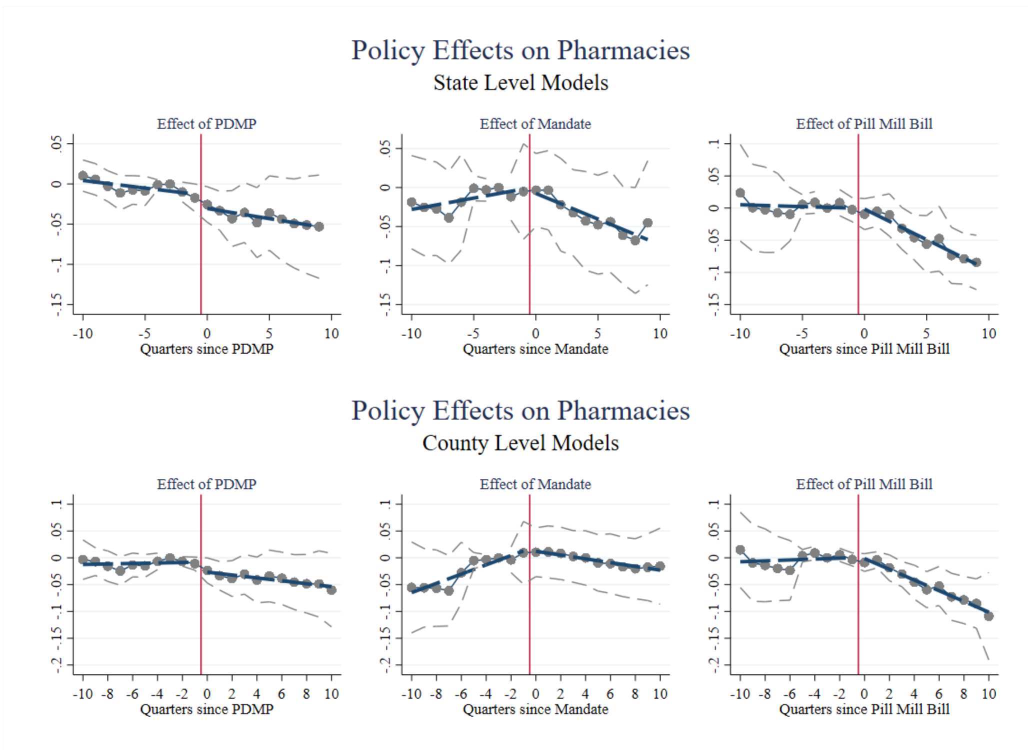


Fig. 3.15. Supply-Side Intervention Policy Effects on Pharmacies

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

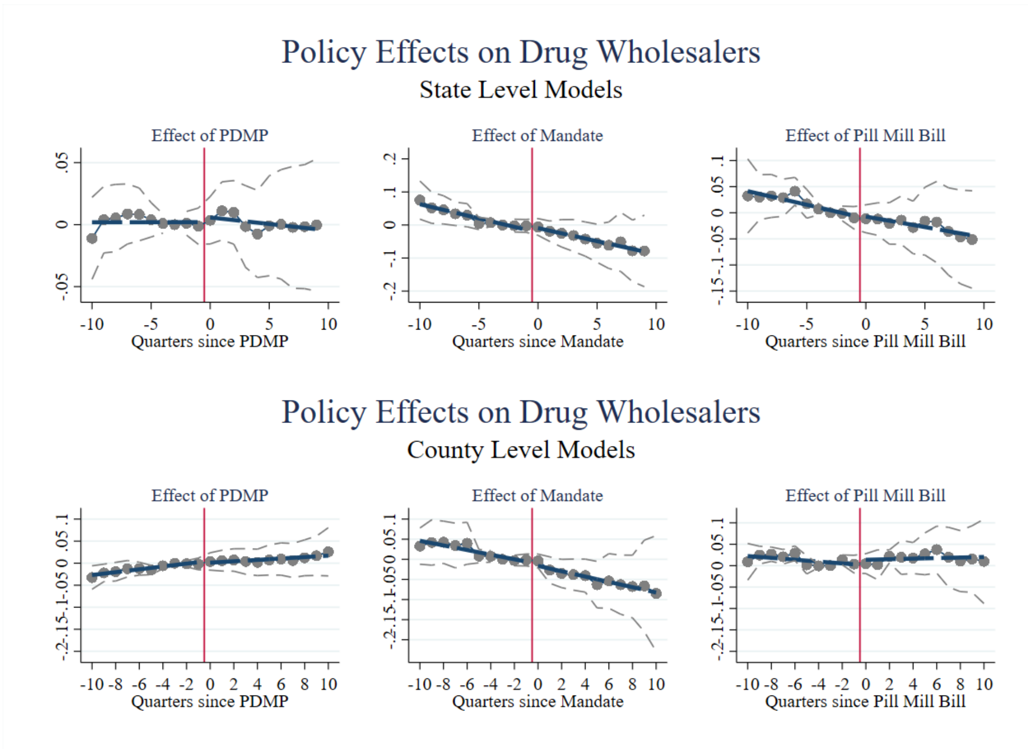


Fig. 3.16. Supply-Side Intervention Policy Effects on Drugstore Wholesaler Establishments

The figure displays event study graphs of the effect of the supply-side policies on logged establishments per 100,000 population. The top three graphs plot coefficients from models at the state level, and the bottom three graphs show results at the county level. Data: Quarterly Census of Employment and Wages 2004-2015.

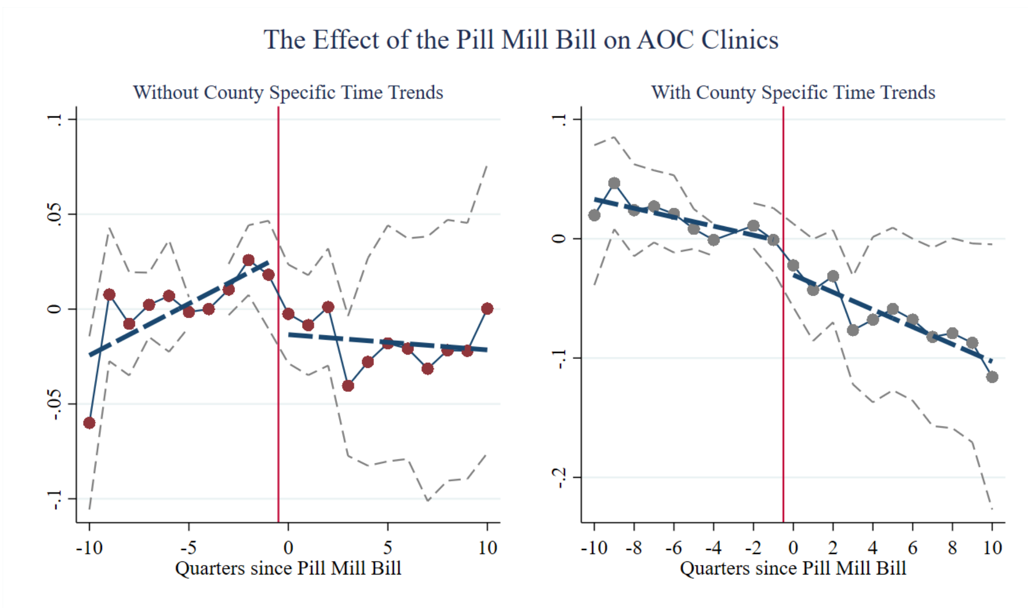


Fig. 3.17. The Effect of the Pill Mill Bill on AOC Clinics, No LTT

The figure displays event study graphs of the effect of the Pill Mill Bill on logged AOC clinics per capita. The model depicted in the left graph includes fixed effects and county controls, but not county-specific linear time trends. The graph on the right adds county-specific linear time trends. Data: Quarterly Census of Employment and Wages 2004-2015.

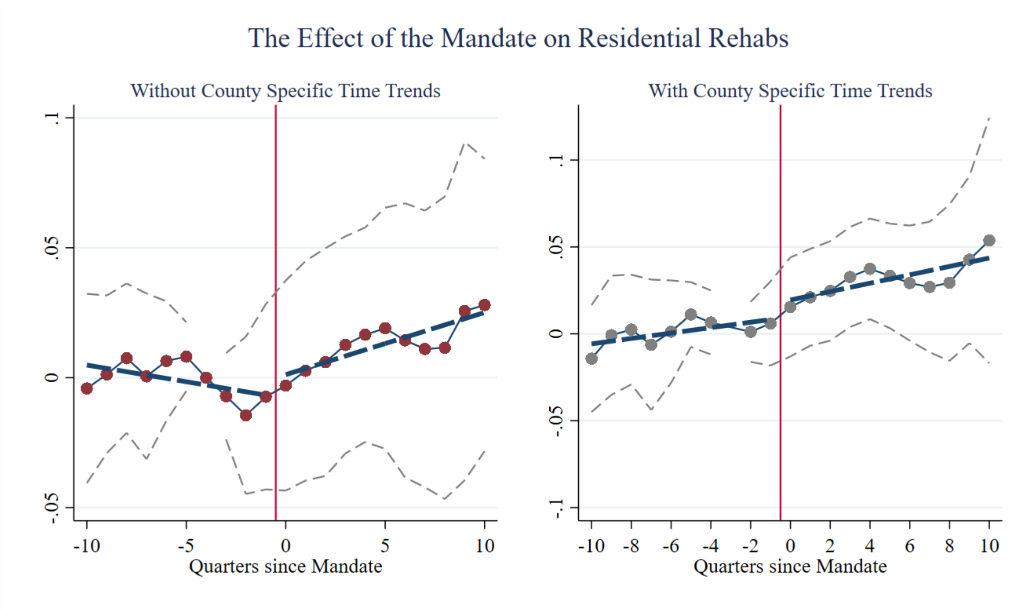


Fig. 3.18. The Effect of the Mandate on Residential Rehab Centers, No LTT

The figure displays event study graphs of the effect of the Mandate on logged residential rehabs per 100,000 population. The model depicted in the left graph includes fixed effects and county controls, but not county-specific linear time trends. The graph on the right adds county-specific linear time trends. Data: Quarterly Census of Employment and Wages 2004-2015.

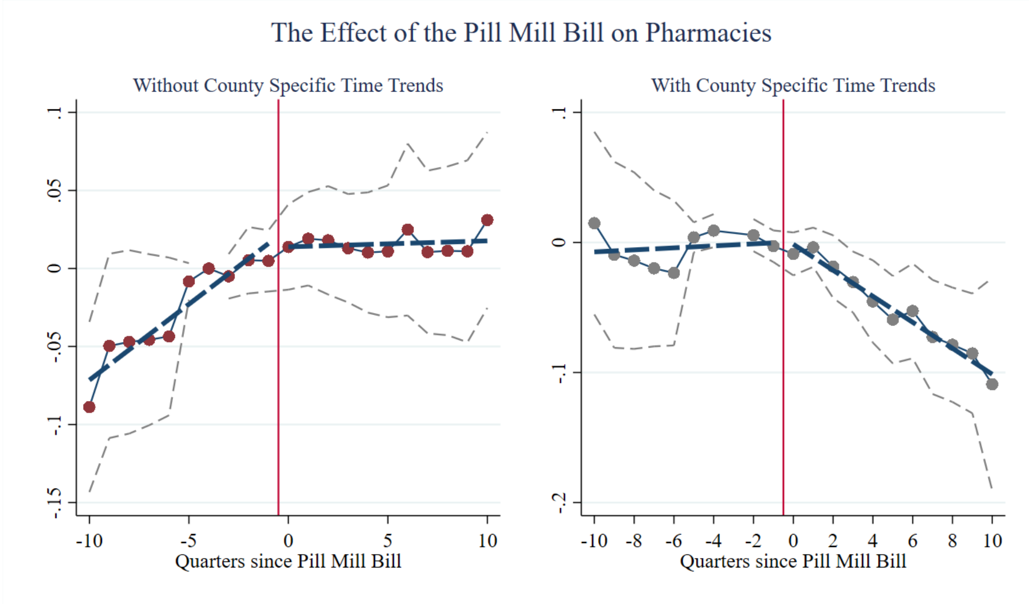


Fig. 3.19. The Effect of the Pill Mill Bill on Pharmacies, Excluding and Including County Trends

The figure displays event study graphs of the effect of the Pill Mill Bill on logged pharmacies per 100,000 population. The model depicted in the left graph includes fixed effects and county controls, but not county-specific linear time trends. The graph on the right adds county-specific linear time trends. Data: Quarterly Census of Employment and Wages 2004-2015.

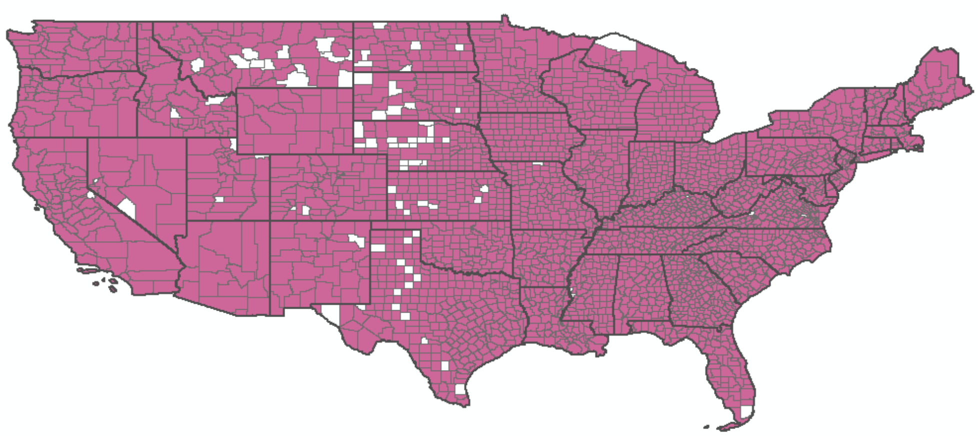


Fig. 3.20. Counties with Complete Panel of County Business Patterns Outcomes

The figure displays the location of the 3,187 counties used to identify the effect of the policies on the rate of business establishments using the CBP data. Data: County Business Patterns 2004-2015.

4. APPENDIX

A Additional Robustness and Model Specifications: Prescription Outcomes

Tables A1, A2, A3, A4, A5, and A6 list the effects of the PDMP, Mandate and Pill Mill Bills on Medicaid oxycodone, Medicaid weak oxycodone, Medicaid strong oxycodone, Medicaid hydrocodone, ARCOS oxycodone, and ARCOS hydrocodone usage, respectively, under different model specifications. In each table, the specifications include simple ordinary least squares in Column (1) in each of the tables, then the addition of fixed effects, controls, and linear time trends in Columns (2) through (4). Column (5) in each table drops analytic weights from the models, Column (6) drops data past 2012 to eliminate any possible confounding influences posed by the implementation of the Affordable Care Act, Column (7) excludes Florida (the state that was considered the "pill mill capital" of the US in the 2000s) from the model, and Column (8) lists coefficients from the interactive fixed effects factor model applied to prescription outcomes. Results are fairly consistent across model specifications, with Medicaid oxycodone, strong oxycodone, and ARCOS oxycodone responding to the policies across specification. However, PDMP estimates lose both power and magnitude when Florida is excluded from models, although magnitudes of coefficients are still negative. Results of the PDMP on heroin incidents in the NIBRS do not use Florida for identification.

Turning to the Mandate policy, Figure A1 graphs its effects on prescription outcomes under a difference-in-differences specifications with fixed effects and controls but not including state-specific linear time trends. Non-zero lead coefficients characterize all six graphs, which is a problem. The addition of linear time trends does not bring the lead coefficients to zero. Therefore each of the graphs in Figure A1 suggest a violation of the parallel trends assumption required for causal inference in difference-in-differences models. As the lead coefficients are statistically significantly different from zero, the treated counties did not trend similarly to untreated counties in the time prior to the mandate. Because of this failure of the parallel trends assumption, I cannot draw causal inferences regarding the effects of the Mandates on outcomes.

Table A1.
PDMP on Log Medicaid Oxycodone Across Model Specifications

	(1) OLS	(2) FE	(3) Controls	(4) LTT	(5) NoWt	(6) NoACA	(7) DropFL	(8) Factor
PDMP	-0.257***	-0.223	-0.246*	-0.188	-0.236	-0.221	-0.116	-0.148*
	(0.0413)	(0.160)	(0.128)	(0.144)	(0.152)	(0.147)	(0.142)	(0.0858)
Mandate	0.915***	0.431**	0.342**	0.141	0.133	0.133	0.0718	0.217
	(0.111)	(0.170)	(0.145)	(0.153)	(0.278)	(0.134)	(0.143)	(0.141)
Pill Mill Bill	-0.666***	-0.366*	-0.190	-0.186	0.0258	-0.118	-0.0589	-0.031
	(0.131)	(0.206)	(0.154)	(0.165)	(0.223)	(0.152)	(0.160)	(0.253)
Observations	2791	2791	2783	2783	2783	2582	2727	2714
Fixed Effects		X	X	X	X	X	X	X
Controls			X	X	X	X	X	X
Linear Trends				X	X	X	X	$\hbar$
Weights	X	X	X	X		X	X	
Drop 2014 on						X		
Drop Florida							X	
Factor Model								X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\hbar$: The interactive fixed effects factor model flexibly nests time trends.

Table A2.
PDMP on Log Medicaid Weak Dose Oxycodone, Across Model Specifications

	(1) OLS	(2) FE	(3) Controls	(4) LTT	(5) NoWt	(6) NoACA	(7) DropFL	(8) Factor
PDMP	-0.289***	-0.0438	-0.0813	-0.0341	-0.0760	-0.0523	-0.0240	-0.050
	(0.0417)	(0.167)	(0.146)	(0.153)	(0.171)	(0.163)	(0.147)	(0.047)
Mandate	-0.253***	-0.348**	-0.350**	-0.247	-0.282*	-0.300*	-0.160	0.0891
	(0.165)	(0.164)	(0.164)	(0.159)	(0.272)	(0.165)	(0.157)	(0.114)
Pill Mill Bill	-1.042***	-0.359**	-0.115	-0.0462	-0.00389	-0.0132	-0.0307	-0.007
	(0.174)	(0.158)	(0.110)	(0.137)	(0.190)	(0.119)	(0.159)	(0.177)
Observations	2790	2790	2782	2782	2782	2581	2726	2713
Fixed Effects		X	X	X	X	X	X	X
Controls			X	X	X	X	X	X
Linear Trends				X	X	X	X	$\hbar$
Weights	X	X	X	X		X	X	
Drop 2014 on						X		
Drop Florida							X	
Factor Model								X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\hbar$: The interactive fixed effects factor model flexibly nests time trends.

Table A3.
PDMP on Log Medicaid Strong Dose Oxycodone, Across Model Specifications

	(1) OLS	(2) FE	(3) Controls	(4) LTT	(5) NoWt	(6) NoACA	(7) DropFL	(8) Factor
PDMP	-0.253***	-0.348**	-0.350**	-0.247	-0.282*	-0.300*	-0.160	-0.172**
	(0.0417)	(0.167)	(0.146)	(0.153)	(0.171)	(0.163)	(0.147)	(0.077)
Mandate	0.790***	0.409**	0.344**	0.120	0.0807	0.0301	0.0390	0.106
	(0.0953)	(0.177)	(0.145)	(0.155)	(0.234)	(0.145)	(0.138)	(0.166)
Pill Mill Bill	-0.572***	-0.341	-0.238	-0.226	0.110	-0.157	-0.0831	-0.072
	(0.121)	(0.212)	(0.173)	(0.190)	(0.249)	(0.172)	(0.184)	(0.225)
Observations	2766	2766	2758	2758	2758	2557	2702	2692
Fixed Effects		X	X	X	X	X	X	X
Controls			X	X	X	X	X	X
Linear Trends				X	X	X	X	$\hbar$
Weights	X	X	X	X		X	X	
Drop 2014 on						X		
Drop Florida							X	
Factor Model								X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\hbar$: The interactive fixed effects factor model flexibly nests time trends.

Table A4.
PDMP on Log Medicaid Hydrocodone, Across Model Specifications

	(1) OLS	(2) FE	(3) Controls	(4) LTT	(5) NoWt	(6) NoACA	(7) DropFL	(8) Factor
PDMP	0.0833	-0.0740	-0.0530	-0.111	-0.0817	-0.0618	-0.150	0.067
	(0.0544)	(0.216)	(0.135)	(0.115)	(0.101)	(0.107)	(0.111)	(0.090)
Mandate	-0.582**	-0.380	-0.208	-0.308*	-0.297	-0.471*	-0.266	-0.355*
	(0.243)	(0.344)	(0.184)	(0.184)	(0.195)	(0.242)	(0.185)	(0.194)
Pill Mill Bill	-0.0384	-0.187	0.0843	-0.0575	-0.156	-0.0165	-0.160	-0.121
	(0.117)	(0.300)	(0.192)	(0.142)	(0.232)	(0.133)	(0.179)	(0.208)
Observations	2782	2782	2782	2782	2782	2581	2726	2714
Fixed Effects		X	X	X	X	X	X	X
Controls			X	X	X	X	X	X
Linear Trends				X	X	X	X	$\hbar$
Weights	X	X	X	X		X	X	
Drop 2014 on						X		
Drop Florida							X	
Factor Model								X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\hbar$: The interactive fixed effects factor model flexibly nests time trends.

Table A5.
PDMP on Log ARCOS Oxycodone, Across Model Specifications

	(1) OLS	(2) FE	(3) Controls	(4) LTT	(5) NoWt	(6) NoACA	(7) DropFL	(8) Factor
PDMP	0.124***	-0.0894*	-0.0814	-0.0847**	-0.0584**	-0.124**	-0.0256	-0.032*
	(0.0436)	(0.0499)	(0.0509)	(0.0401)	(0.0291)	(0.0510)	(0.0221)	(0.017)
Mandate	0.428***	0.193**	0.157**	-0.0862	-0.0935**	-0.0591	-0.145***	-0.037
	(0.0593)	(0.0785)	(0.0589)	(0.0556)	(0.0445)	(0.0549)	(0.0376)	(0.041)
Pill Mill Bill	-0.197***	-0.290***	-0.276***	-0.210**	-0.115	-0.173	-0.0575	-0.024
	(0.0760)	(0.107)	(0.101)	(0.0970)	(0.117)	(0.105)	(0.0495)	(0.063)
Observations	3264	3264	3153	3153	3153	2594	3090	3070
Fixed Effects		X	X	X	X	X	X	X
Controls			X	X	X	X	X	X
Linear Trends				X	X	X	X	$\hbar$
Weights	X	X	X	X		X	X	
Drop 2014 on						X		
Drop Florida							X	
Factor Model								X

Standard errors in parentheses, clustered by state
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\hbar$: The interactive fixed effects factor model flexibly nests time trends.

Table A6.
PDMP on Log ARCOS Hydrocodone, Across Model Specifications

	(1) OLS	(2) FE	(3) Controls	(4) LTT	(5) NoWt	(6) NoACA	(7) DropFL	(8) Factor
PDMP	0.414***	0.0580	-0.00409	0.0180	0.0264	0.0121	0.0247	-0.021
	(0.0274)	(0.0354)	(0.0263)	(0.0183)	(0.0159)	(0.0200)	(0.0184)	(0.014)
Mandate	-0.372***	-0.148	-0.165***	-0.125***	-0.0905***	-0.0936*	-0.119***	-0.060**
	(0.0735)	(0.0940)	(0.0390)	(0.0355)	(0.0309)	(0.0471)	(0.0345)	(0.028)
Pill Mill Bill	0.534***	0.000350	-0.0129	-0.00830	-0.0198	-0.00362	0.0175	-0.0225
	(0.0561)	(0.102)	(0.0506)	(0.0297)	(0.0298)	(0.0171)	(0.0343)	(0.031)
Observations	3260	3260	3149	3149	3149	2590	3086	3066
Fixed Effects		X	X	X	X	X	X	X
Controls			X	X	X	X	X	X
Linear Trends				X	X	X	X	$\hbar$
Weights	X	X	X	X		X	X	
Drop 2014 on						X		
Drop Florida							X	
Factor Model								X

Standard errors in parentheses, clustered by state

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\hbar$: The interactive fixed effects factor model flexibly nests time trends.

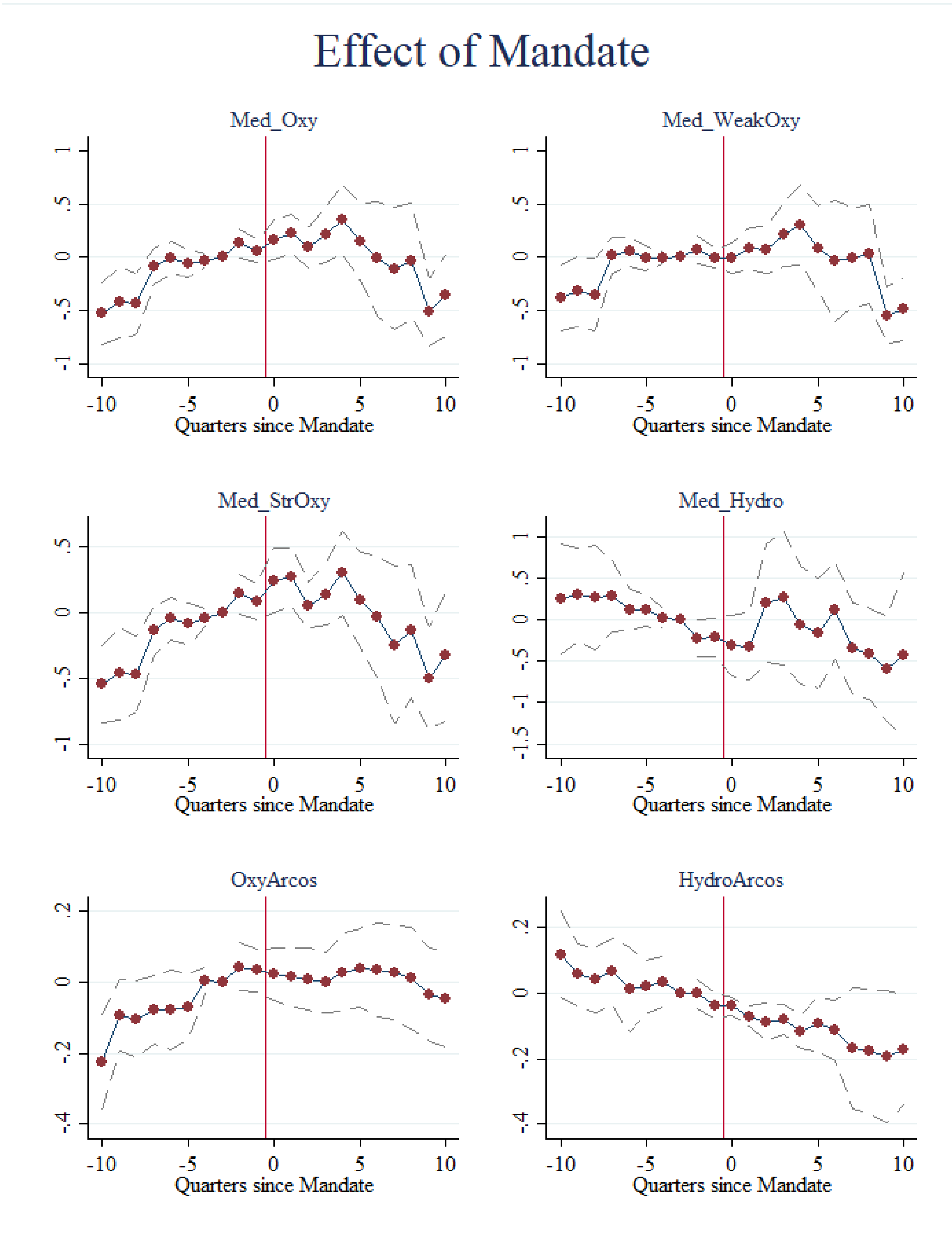


Fig. A1. The Effect of Mandated PDMPs on Medicaid and ARCOS Prescription Outcomes

The graphs display coefficients on Mandate lag and lead indicators in a difference-in-differences model including state and quarter fixed effects and controls, but not including state-specific trends. Note the non-zero lead coefficients.

B Additional Model Robustness: Heroin Results

The main text divides counties into "high oxycodone density" and "low oxycodone density" by cutting on the 90th percentile of the distribution of oxycodone per capita. Figure B2 tests the robustness of the significant increase in heroin incidents using different high density and low density cutoffs (other than the 90th percentile). To clarify, the top graph plots policy coefficient estimates on the bottom percentage of counties classified as "low" oxycodone-density counties, with the horizontal axis plotting which percentage cutoff was used to determine which counties were classified as "low" oxycodone-dense counties. The measured effect of the PDMP on the bottom 80-90% (excluding more oxycodone-dense counties) of the data is about zero. The lower graph plots the PDMP effect on heroin incidents within "high" oxycodone-density counties. The PDMP coefficients become significant at the 95% confidence level at about the 70th percentile, so using the top 30% of counties as "high density". These show 1-1.75 additional heroin incidents per 100,000 population each month in the top 30% of oxycodone-dense counties.[1]

Figure B3 tests the robustness of the PDMP effect on heroin incidents on counties with low versus high oxycodone, under the IFE factor model specification. Similarly to Figure B2, the top graph plots coefficients on the bottom 5 through 50 percent of counties cut on oxycodone density, measuring a zero effect of the PDMP. The bottom graph plots the coefficients for IFE factor models run on the top 50 to 95 percent of counties cut on oxycodone density, and measures an increase of 0.2 to 0.6 additional heroin incidents per 100,000 population per month as a result of the PDMP.

Figure B4 plots the event study of the PDMP on heroin incidents across all counties and in the top 10% of counties based on oxycodone-density using the IFE factor model. This graph is the IFE factor model analog to Figure 1.6 (which plots coefficients from a difference-in-differences model) in the main text. The IFE factor model graphs show similar results to the difference-in-differences graphs in the main text,

[1]Each plotted point is from a different model run on a different subset of counties in the data, depending on the high/low oxycodone cutoff.

but display lead coefficient points closer to zero, with less of a possible pre-trend. Figures still display an increase in the heroin incident rate within the most opioid-dense counties after implementation of the PDMP.

C The Effect of PDMPs on Oxycodone by Strength of Pill

The Medicaid drug data comprises state-by-quarter counts of drugs, classified by NDC code. The NDC code specifies the strength of drug by dosage units. Oxycodone comes in pills ranging from 2.5 milligrams to 100 milligrams in the Medicaid data. Table C7 gives summary statistics of oxycodone amounts dispensed through Medicaid, specified by strength of pill. The table lists the mean amount of oxycodone per enrollee by pill strength. It also lists number of pills per enrollee by pill strength. The 5-milligram pills are most common, making up 44.6% of dispensed pills, but only makes up 17.5% of active-ingredient oxycodone dispensed through Medicaid. The 30, 40, and 80 milligram pills make up a small fraction of dispensed pills by number of pills (6.5%, 5.1%, and 3.7% of pills, respectively); however the large-dose pills make up 14.2%, 12.5%, and 17.3% of oxycodone dispensed.

Figure C5 graphs PDMP coefficients from separate difference-in-differences models on logged milligrams of oxycodone per Medicaid enrollee as grouped pills of each strength. The size of each plotted circle is determined by how much of the total dispensed oxycodone comes in each form of pill. The largest circles–associated with 5, 10, 30, 40, and 80 milligram pills–correspond to pills that each make up 10% or more of the oxycodone dispensed, medium-sized points correspond to pills that each make up between 5 and 10% of oxycodone dispensed, and small points are for pills that each make up less than 5% of dispensed oxycodone milligrams. The points are different sizes so that a viewer can determine which pill strengths are most responsible for the aggregated coefficient estimates in Table 1.7 in the main text. That table shows a 24.6% reduction in overall oxycodone dispensed, and a 35% decrease in strong-dose oxycodone (pills with $>$ 10 milligrams of oxycodone) in response to the PDMP. The large reduction in pills with more than 10 milligrams is driven by large, marginally

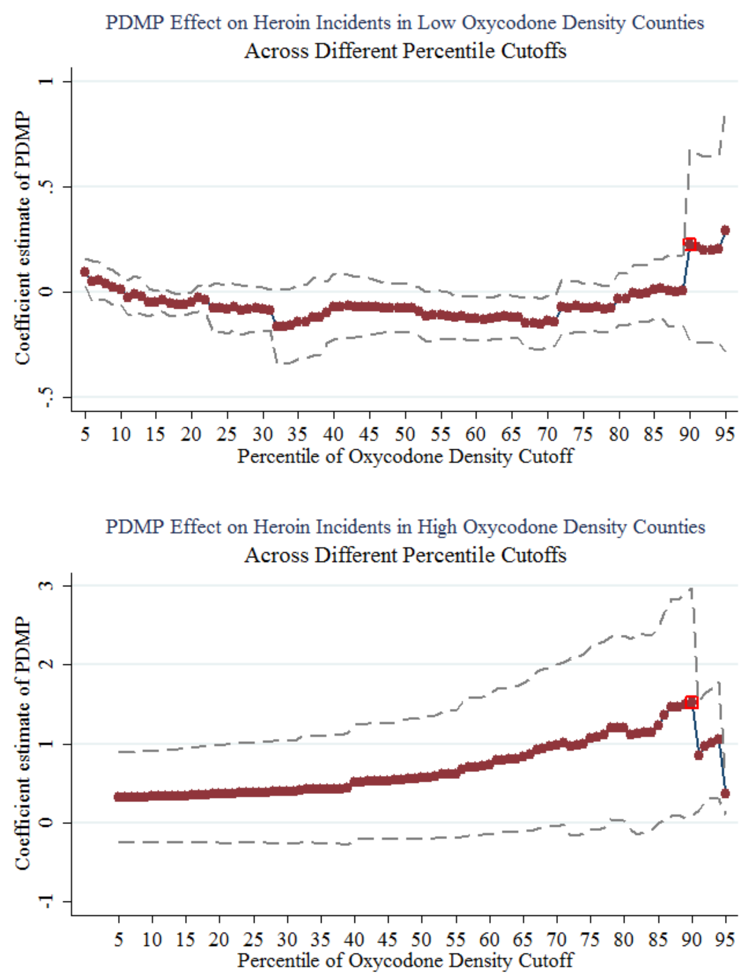


Fig. B2. Sensitivity of the Estimated Effects on Heroin Incident Rate Using Different Thresholds to Define High/Low Oxycodone Density Counties

The top figure plots the PDMP estimated coefficients in the less oxycodone dense counties, depending on the threshold (in oxycodone per capita distribution percentile) used to classify counties as “low oxycodone dense” counties. The bottom figure plots the PDMP coefficients for the more oxycodone dense counties, depending on the threshold (in oxycodone per capita distribution percentile) used to classify counties as “high oxycodone dense” counties. The main tables use the 90th percentile as the cutoff. Coefficients are obtained by running a difference-in-differences regression (including county and month fixed effects, controls and analytic weights) on heroin incidents on subsets of counties that are below or above the thresholds.

significant decreases in the prescription rate of oxycodone dispensed in the form of 30, 40, and 80 milligram pills in response to the PDMP.

Table C7.
Summary Statistics on Medicaid Oxycodone Pills by Milligrams

Pill Strength	Mean Mg Per Enrollee	Mean Pills Per Enrollee	Fraction of Oxycodone Mg	Fraction of Oxycodone Pills
2.5 mg	0.004	0.0016	0.0001	0.000
	(0.008)	(0.003)		
5 mg	6.14	1.228	0.1746	0.446
	(5.03)	(1.006)		
7.5 mg	1.02	0.137	0.0295	0.052
	(1.14)	(0.153)		
10 mg	5.05	0.505	0.1638	0.218
	(4.70)	(0.470)		
15 mg	2.47	0.164	0.0684	0.0592
	(3.91)	(0.261)		
20 mg	3.05	0.153	0.0775	0.061
	(3.31)	(0.166)		
30 mg	5.10	0.170	0.1421	0.065
	(8.69)	(0.290)		
40 mg	5.41	0.135	0.1248	0.051
	(5.68)	(0.142)		
50 mg	0.356	0.007	0.0113	0.003
	(0.751)	(0.015)		
60 mg	0.760	0.013	0.0223	0.006
	(0.929)	(0.154)		
80 mg	7.29	0.091	0.1727	0.0365
	(7.10)	(0.089)		
100 mg	0.498	0.005	0.0122	0.002
	(1.03)	(0.010)		

Oxycodone comes in pills of varying strength. The table contains summary statistics on the mean milligrams of oxycodone per Medicaid enrollee within each pill strength, the mean number of pills per Medicaid enrollee in each pill strength, the fraction of total Medicaid oxycodone milligrams administered in each pill strength, and the fraction of total oxycodone pills given out in each strength. For example, the average Medicaid enrollee receives 6.14 milligrams of oxycodone in the 5 milligram pill form, equal to 1.228 5-mg-pills per Medicaid enrollee. 5 milligram pills make up 17.5% of oxycodone *milligrams of active ingredient* and 44.6% of oxycodone *pills* covered by Medicaid.
Standard errors are in parentheses. Data source: Medicaid prescription data.

D The Effect of PDMPs on Heroin Crimes: Offender Characteristics

Results in Table 1.8 of the main text show that PDMPs increase the rate of heroin incidents in the top 10% of counties in the distribution of oxycodone per capita. The increase of 1.745 additional heroin incidents per 100,000 population per month is equal to an 87% increase. This appendix section uses additional detail from the NIBRS incident-level dataset to identify characteristics of the heroin offenders affected by the policies. The NIBRS dataset shows that at the baseline, the most common locations for heroin incidents are discount and department stores, parking lots and garages, homes and residences, and roads including highways, alleys, streets and sidewalks. These four location categories make up 84% of heroin incident locations, whereas the broad category of "other locations" accounts for the other 16% of heroin incidents.[2] Table D8 suggests that the PDMPs are causing heroin incidents that occur mainly in parking lots and garages (an 84% increase) and on roads (a 71% increase). Anecdotally, heroin sales take place in parking lots and on roadways, often with a simple drive-by transaction or a hand-off exchange between vehicles, and the increase in parking-lot and roadway incidents in response to the PDMP may be a sign of police encountering more heroin transactions in these locations. Also, police may also be encountering more erratic driving as a result of heroin use and may possibly be pulling over greater numbers of under-the-influence offenders in parking lots and on roadways.

Table D9 breaks down the heroin incidents in the most opioid-dense counties by race. It appears the increase in heroin incidents is driven by increases in the rate of heroin incidents among white and black offenders, but the measured increases in heroin incidents split up by race of offender are not individually statistically significant. Table D10 divides heroin incidents into those committed by male offenders and those committed by female offenders, and shows a statistically significant increase in the male heroin incident rate in the most oxycodone-dense counties in response to the PDMP. The point estimate of the increase in female offender heroin incidents is large in magnitude but is not statistically significant. Table D11 classifies heroin incidents by age of offender, and shows that PDMPs affect heroin incidents involving offenders between the ages of 20 and 29, 30 and 39, and 40 and 49. The increases in heroin incidents among 30-39 year-olds and 40-49 year-olds are statistically significant at the

[2]The "other locations" category includes 54 other types of location categories in the NIBRS and are not listed here.

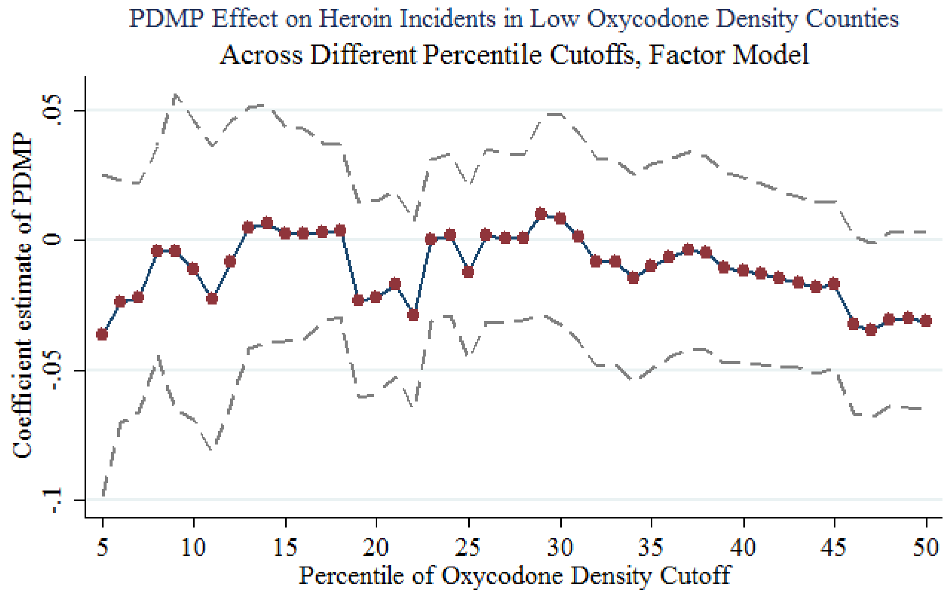
5% level. Overall, white males of a fairly wide range of ages are responding to the PDMP.

Finally, Table D12 breaks down heroin incident rates by both race of offender and across the four most common locations of heroin incidents. This is to examine the effect of the PDMP on offenses by race and location. The PDMP causes an increase in heroin incidents with white offenders occurring in parking lots, within homes, and on roadways. The 0.256 and 0.487 increase in parking lot and roadway incidents add to a combined 0.743 additional heroin incidents by white offenders, which makes up the bulk of the increase of 1.114 additional white-offender incidents, recorded in previous Table D9. Heroin incidents involving black offenders in parking lots and roadways also increase in response to the PDMP, with a combined effect of 0.457 additional heroin incidents, accounting for the bulk of the measured 0.667 additional black-offender incidents in Table D9. In addition, there is a small but statistically significant increase in the rate of heroin incidents involving Hispanic offenders in parking lots.

Table D8.
Effect of PDMP on Heroin Incidents in High Oxycodone Density Counties: By Location of Offense

	(1) Disc. Store	(2) Parking Lot	(3) Home	(4) Road	(5) Other
PDMP	-0.00228	0.504*	0.0579	0.724**	0.121
	(0.0102)	(0.255)	(0.0621)	(0.331)	(0.123)
Mandate	0.0330***	0.484***	0.142***	0.562***	0.313***
	(0.00709)	(0.112)	(0.0424)	(0.147)	(0.0827)
Pill Mill Bill	-0.0557***	-0.764**	-0.249**	-0.999**	-0.537***
	(0.0114)	(0.284)	(0.107)	(0.360)	(0.153)
Observations	9588	9588	9588	9588	9588
Fixed Effects	X	X	X	X	X
Fixed Effects	X	X	X	X	X
Controls	X	X	X	X	X
Popln. Weight	X	X	X	X	X
Linear Time Trends					
Mean Rate Per 100,000 Pop	0.0347	0.600	0.1702	1.014	0.278

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
The most common locations of crimes in the NIBRS are residences/homes, highways/roads/alleys, department/discount stores, and parking lots/garages, which make up 71% of offenses, and 84% of heroin incidents. The "other" location category makes up the remaining 29% of offenses or 16% of heroin incidents, respectively.



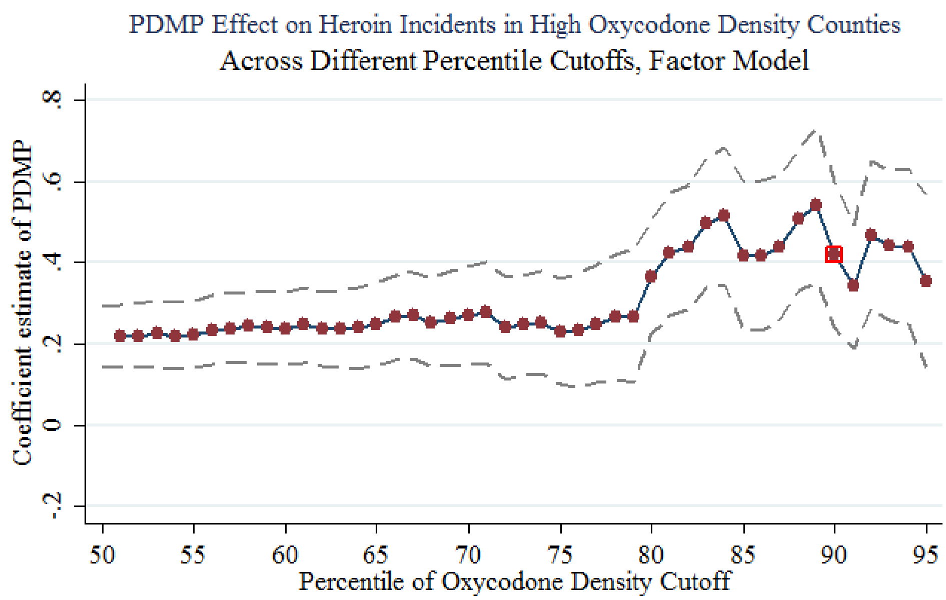


Fig. B3. Sensitivity of the Estimated Effects on Heroin Incident Rate Using Different Thresholds to Define High/Low Oxycodone Density Counties: Unweighted Factor Model

Notes: Graphs plot the coefficients on PDMP lags and leads indicators in an interactive fixed effects factor model on heroin incidents per 100,000 in a county-month pair. The top graph shows the event study of the PDMP on heroin incidents across all counties. The lower graph shows the event study of the PDMP effects in the most oxycodone-dense counties. These event study models include controls and fixed effects by month and county. The county data span 735 counties over 26 states monthly from 2004-2014. Heroin incident data: NIBRS. Oxycodone density data: DEA ARCOS.

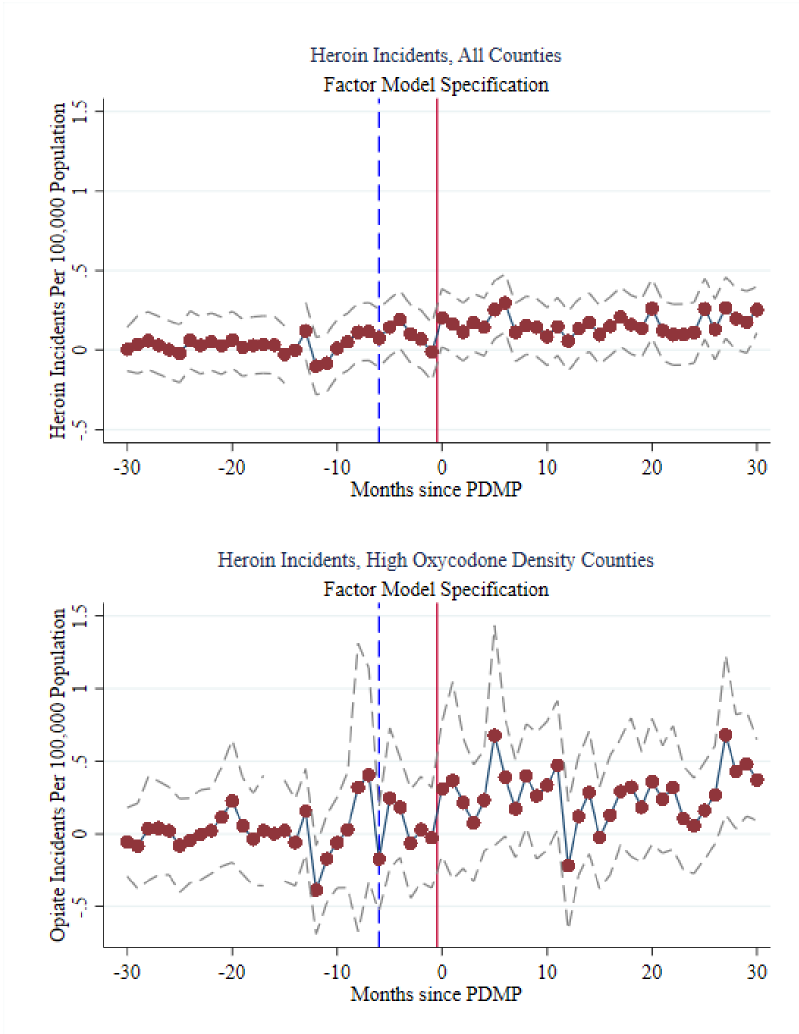


Fig. B4. The Effect of the PDMP on Heroin Incidents Across All Counties and in Most Oxycodone Dense Counties: Factor Model

Notes: The figure plots event studies of the PDMP on the rate of heroin incidents per 100,000 population per month across all counties (top graph) and across the most oxycodone-dense counties (bottom graph) under the IFE factor model specification.

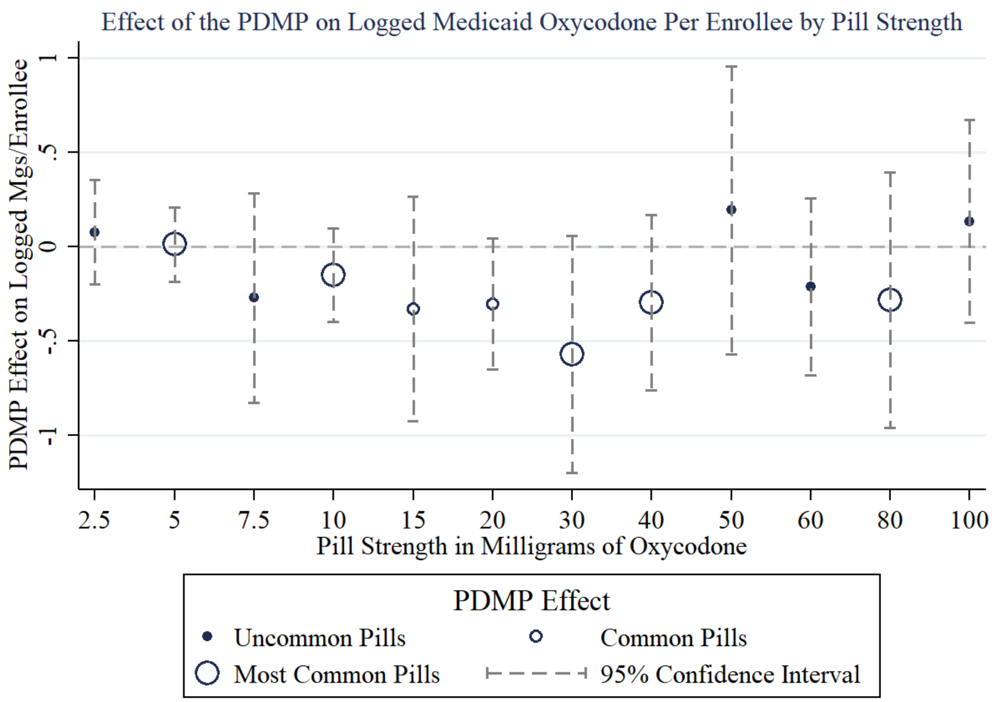


Fig. C5. The Effect of the PDMP on Medicaid Oxycodone by Strength

Notes: The figure plots the effect of the PDMP on logged Medicaid oxycodone per enrollee disaggregated by pill strength. Each plotted point is associated with a separate regression on milligrams per enrollee restricted to pills of each strength. Points are sized by the relative frequency of pills in the Medicaid data, which corresponds to the fraction column in Table C7. "Uncommon Pills" are pills that make up less than 5% of oxycodone, "Common Pills" make up between 5% and 10% of oxycodone, and "Most Common Pills" are pills that make up for greater than 10% of oxycodone.

Table D9.
Effect of PDMP on Heroin Incidents in High Oxycodone Density Counties: By Race of Offender

	(1) White	(2) Black	(3) Hispanic	(4) Other
PDMP	1.114	0.667	0.156	0.0161
	(0.666)	(0.469)	(0.101)	(0.0115)
Mandate	1.472***	0.496*	0.0412	0.0355**
	(0.278)	(0.240)	(0.0947)	(0.0160)
Pill Mill Bill	-2.432***	-1.080**	-0.109	-0.0484*
	(0.788)	(0.472)	(0.138)	(0.0241)
Observations	9588	9588	9588	9588
Fixed Effects	X	X	X	X
Controls	X	X	X	X
Popln. Weight	X	X	X	X
Linear Time Trends				
Mean Rate Per 100,000 Pop	1.595	0.781	0.379	0.126

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D10.
Effect of PDMP on Heroin Incidents in High Oxycodone Density Counties: By Sex of Offender

	(1) Male	(2) Female
PDMP	1.432*	0.508
	(0.753)	(0.293)
Mandate	1.559***	0.444***
	(0.307)	(0.132)
Pill Mill Bill	-2.634**	-1.002**
	(0.907)	(0.334)
Observations	9588	9588
Fixed Effects	X	X
Controls	X	X
Popln. Weight	X	X
Linear Time Trends		
Mean Rate Per 100,000 Pop	2.208	0.574

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D11.
Effect of PDMP on Heroin Incidents in High Oxycodone Density Counties: By Age of Offender

	(1) 10-19	(2) 20-29	(3) 30-39	(4) 40-49	(5) 50-59	(6) 60+
PDMP	0.143	1.067	0.479**	0.174**	0.0213	0.0124
	(0.0900)	(0.614)	(0.218)	(0.0735)	(0.0179)	(0.00770)
Mandate	0.0519	1.131***	0.454***	0.202***	0.0808***	0.0222***
	(0.0411)	(0.255)	(0.110)	(0.0510)	(0.0161)	(0.00378)
Pill Mill Bill	-0.212*	-2.141**	-0.854**	-0.219***	-0.0855***	-0.0226**
	(0.115)	(0.720)	(0.293)	(0.0702)	(0.0220)	(0.0101)
Observations	9588	9588	9588	9588	9588	9588
Fixed Effects	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Popln. Weight	X	X	X	X	X	X
Linear Time Trends						
Mean Rate Per 100,000 Pop	0.232	1.252	0.668	0.393	0.130	0.026

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D12.
Effect of PDMP on Heroin Incidents by Race and Location Of Offender: Across High Oxycodone Density Counties

	(1) Disc. Store	(2) Parking Lot	(3) Home	(4) Road	(5) Other
PDMP on White	-0.00474	0.256**	0.0591**	0.487***	0.0604
	(0.0101)	(0.0982)	(0.0269)	(0.146)	(0.0599)
PDMP on Black	0.00180	0.230**	0.00467	0.227***	0.0655*
	(0.00368)	(0.0895)	(0.0239)	(0.0846)	(0.0373)
PDMP on Hispanic	0.000133	0.0490*	0.000709	0.0580	0.00482
	(0.00184)	(0.0255)	(0.00437)	(0.0508)	(0.00598)
PDMP on Other/Unrecorded	-0.000961	0.00444	0.000218	0.00387	0.00615
	(0.000923)	(0.00469)	(0.00298)	(0.00238)	(0.00727)
N	9588	9588	9588	9588	9588

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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