Disrupting Drug Markets:
The Effects of Crackdowns on Rogue Opioid Suppliers*

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Abstract

More than 564,000 Americans have died from an opioid-related overdose since 1999. In this paper, I estimate the impacts of enforcement actions taken against rogue doctors on the supply of prescription opioids, black-market prices, and health outcomes. Exploiting plausibly exogenous variation in the timing and location of controlled substance license audits, I find that cracking down on a single doctor decreases city-level opioid dispensing by 10% within three months and 25% after two years. This decline in legal supply persists across space and time and results in a 44% increase in the black-market pill price. Significant heroin substitution also occurs, yet for each additional heroin overdose death, there are two fewer non-heroin opioid overdose deaths. The mortality declines are strongest among young and prime-aged males. These results highlight a novel tradeoff policymakers should consider when attempting to address drug abuse through supply-side interventions: reductions in the flow of new users must be balanced against the harm that arises when existing users substitute to more dangerous drugs.

JEL Codes: H12, I10, K42

Keywords: enforcement, drug epidemic, overdose, spatial crime dynamics

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1 Introduction

Since 1999, over 60% of all drug overdose deaths in the United States involved opioids (CDC, 2021). Reducing drug abuse is an important domestic policy goal, where the majority of federal drug control spending is on enforcement efforts targeting supply (Dobkin and Nicosia, 2009). These supply-side crackdowns are intended to reduce drug availability and use, but two types of demand-side spillovers can reduce their efficacy: 1) substitution to different suppliers, and 2) substitution to more harmful substances (Caulkins and Reuter, 2010; Evans et al., 2022). This is especially concerning for controlled substances, which have a large pool of potential users and where initial use is often driven by medical need (Volkow and McLellan, 2016). For example, a nationwide supply disruption of prescription opioids in 2010 caused significant increases in heroin use (Alpert et al., 2018; Evans et al., 2019). Although four of the last five drug epidemics began with legally-produced drugs, there is almost no research on the impacts of targeted supply-side enforcement in a controlled substance market (Moore and Pacula, 2020).[1]

In this paper, I identify the impacts of individual opioid supplier crackdowns on legal drug supply, black-market prices, and health outcomes. I do this by first scraping the Federal Register to construct a new dataset on the universe of Drug Enforcement Administration (DEA) investigations of doctors and pharmacies from 2006 to 2014, which I then merge with information on all opioid pills dispensed at US pharmacies, all deaths that occur within the US, and black-market opioid pill prices. This novel dataset allows me to study not only the spatial and temporal dynamics associated with supply-side enforcement, but how these actions reverberate into the illicit market. Such high-quality administrative data is unique within the literature on drug enforcement, as its focus is often on illegally-produced drugs.

My primary research design exploits plausibly exogenous variation in the timing and location of DEA audits of controlled substance license holders in a difference-in-differences framework.[2] I find that these individual DEA investigations, which can lead to a controlled substance license revocation, significantly decrease prescription opioid dispensing relative to cities or counties without such investigations. I also find increases in black-market opioid pill prices and heroin overdose deaths, but decreases in net mortality: for each additional heroin overdose death, there are two fewer non-heroin opioid overdose deaths.

For context, non-medical use of prescription opioids is the primary cause of our current opioid crisis (Cutler and Glaeser, 2021).[3] The DEA attempts to prevent diversion of controlled substances out of the medical system by requiring medical professionals to obtain a controlled substance license.[4] They also detect and investigate diversion, such as doctors prescribing to patients that do not have a medical need, where enforcement actions range from fines to loss

[1] Legally-produced drugs that fueled nationwide public health crises include morphine, heroin, amphetamine, and prescription opioids (DeShazo et al., 2018); see Figure A.1 for a timeline of these epidemics.

[2] The identifying variation traditionally used in the research on opioid epidemic management is generated by state- or federal-level policies attempting to address illicit behavior or misuse through preventative measures.

[3] The misuse of prescription opioids has been linked to higher health care costs (White et al., 2005), lower worker productivity and employment (Harris et al., 2020), more suicides (Borgschulte et al., 2018), increased use of other drugs (Grecu et al., 2019), and worse outcomes for children (Evans et al., 2022).

[4] The controlled substance license is in addition to any state license requirements. Doctors with this license should only prescribe them if there is a legitimate medical purpose and if it is within the usual course of professional practice. Pharmacies have a corresponding responsibility.

of license and prison time. The DEA often targets doctors and pharmacies, as they are the source of over 80% of all controlled substance diversion (Inciardi et al., 2007). This is especially relevant in the prescription opioid market: 1% of the doctors who prescribe opioids account for almost 50% of all domestic opioid doses prescribed and 60% of abused opioids begin with a prescription (Lembke, 2012; Kiang et al., 2020).

I begin with the impacts of rogue doctor crackdowns on the supply of prescription opioids, black-market pill prices, and substitution to heroin. These doctors often run high-volume practices with little patient interaction or concern for patient health (Quinones, 2015). They are colloquially known as "drug dealers in lab coats," working directly with addicts and exchanging prescriptions for cash or sexual favors (Kristof, 2017). I find that cracking down on a single doctor decreases city-level opioid dispensing by 10% after three months and 25% after two years. The declines are larger for smaller cities, where the increasing effects over time are likely due to dynamic deterrence for non-sanctioned doctors that prescribe more conservatively after a local colleague is sanctioned. These enforcement actions are relatively infrequent, with less than one in ten thousand doctors found in violation in any year of my sample.

This persistent decline in legal opioid supply leads to over 40% increases in both heroin overdose deaths and black-market opioid pill prices. To contextualize these findings, I interviewed current and former DEA agents, who suggest that it is often difficult to find a doctor willing to write a prescription for a non-medical purpose in one's local community. Therefore, while the flow of new addicts may slow due to a doctor crackdown, the increase in heroin overdose deaths is likely due to existing users substituting from prescription opioids to heroin. Black-market prices increase when the supply of legal prescriptions decreases, the demand for diverted pills in the black market increases, or both.[5]

Having established the local impacts of crackdowns on doctors, I then examine their effects at a larger geographic scale. Caulkins (1992) indicates that enforcement either displaces illegal activity or reduces its overall frequency. To determine the extent of spatial displacement, I approximate larger markets around a crackdown by generating different size buffers and then study how dispensing behavior changes within them. I find that doctor crackdowns lead to persistent declines in dispensing across space and time, which suggests that targeted enforcement is a deterrent for non-sanctioned doctors.

This persistent decline in dispensing following a doctor crackdown is especially important, as I find evidence of spatial displacement when the DEA separately cracks down on pharmacies. More specifically, while the effects are similar between doctor and pharmacy crackdowns at the city- and county-level, namely prescription opioid supply decreases, heroin overdose death increases, and net mortality decreases, I find large increases in dispensing in areas farther from a sanctioned pharmacy. I investigate this further using outlier detection methods from forensic accounting and find that several small independent pharmacies in the same market become "suspicious" for the first time soon after a pharmacy crackdown. On average, the rapid increases from their normal ordering patterns negate the reduction achieved from removing the rogue pharmacy. I provide suggestive evidence that this is not mechanical and may be

[5]Virtually all opioid pills found in the black market begin in the legal market during my sample period (Powell and Pacula, 2017).

driven by drug traffickers diversifying their supply along interstate highways. Note that spatial displacement is a common finding in the literature on drug enforcement in purely illicit markets.

In addition to substance substitution and crime displacement, an important consideration is the type of user impacted by a supply-side intervention. This is relevant because drug epidemics occur when a sufficient number of light users create feedback loops that encourage further initiation and escalation and then a "tipping point" is passed (Moore and Pacula, 2020).[6] I find that young and prime-aged male mortality decreases by almost 10% following a doctor crackdown; this demographic is the most susceptible to drug abuse (Grecu et al., 2019). The effects are smaller for females, likely due to differences in risk-taking behavior and occupational choice, and the reductions are driven by decreases in non-heroin opioid overdoses and non-opioid drug overdoses (Kloos et al., 2009). While the goal is to prevent drug use altogether, these interventions can delay initiation and prevent people from becoming regular drug users (Strang et al., 2012).

This paper contributes to several literatures. My primary contribution is providing some of the first empirical evidence on the effectiveness of individual supplier crackdowns in a legal drug market. To the best of my knowledge, the only other paper to study enforcement actions within a controlled substance market is by Meinhofer (2016). Meinhofer studies a coordinated enforcement effort to shut down pain clinics in Florida and finds striking state-level opioid supply decreases with limited aggregate effects on crime and drug substitution. A related paper by Dobkin and Nicosia (2009) studies the impacts of a federal effort to reduce the supply of methamphetamine precursors in 1995. Using data from California, they find that enforcement causes state-level prices to rise, drug abuse and purity to fall, and methamphetamine arrests to drop. Notably, all of these effects are temporary. These seminal papers focus on broad interventions and leverage aggregate measures of drug supply and usage. I instead focus on local supply shocks and outcomes using administrative data on the universe of dispensed opioid pills, enforcement actions, and mortality. This allows me to study not only the more granular direct impacts of enforcement, but the potential spillovers across space, substance, and time.

This paper builds on the economic literature studying the causes and consequences of the opioid epidemic by focusing on individual rogue actor interventions. Related research often examines the role of preventative state- or federal-level policies, which can affect both illegitimate *and* legitimate actors. One of the most popular of these supply-side interventions is a prescription drug monitoring program (PDMP), which is a state-based electronic database that tracks controlled substance prescriptions. PDMPs are intended to identify or deter misuse and diversion, but evidence on their effectiveness is mixed (Meinhofer, 2018). Doctor access in most states is voluntary and participation rates are often low, but broad interventions that restrict supply may still cause harm to patients with a legitimate medical need (Islam and McRae, 2014; Kim, 2021). Researchers have also examined changes in Medicare coverage (Alpert et al., 2015; Powell et al., 2020), the reformulation of OxyContin (Alpert et al., 2018; Evans et al., 2019), improved access to opioid antagonists (Doleac and Mukherjee, 2021), triplicate prescription programs (Alpert et al., 2022), and pharmaceutical marketing (Arteaga and Barone, 2022).

[6] The rate at which current users introduce or initiate new "recruits" is proportional to the number of light users and is moderated by the reputation of the drug (Behrens et al., 1999).

I also compliment a growing body of work examining the role of physician behavior in contributing to the opioid epidemic by focusing instead on a single doctor's removal from the medical system. Eichmeyer and Zhang (2021) find that patients randomly assigned to high-prescribing doctors are more likely to be addicted to opioids, underscoring the role of a doctor in initiating drug abuse. These authors also highlight significant heterogeneity in opioid prescribing patterns across doctors, which Schnell and Currie (2018) find is partly driven by differences in training. Schnell (2017) estimates a model of physician behavior in the presence of an illegal secondary market, which shows that the potential for diversion will make strict doctors prescribe more conservatively and lenient doctors to loosen their prescription thresholds.

Lastly, this paper contributes to a large literature on drug enforcement, which generally focuses on the effectiveness of traditional policing methods in purely illicit markets and often finds evidence of displacement. For example, Dell (2015) finds large increases in drug-related violence following close mayoral elections for a conservative party in Mexico that was tougher on drug traffickers. Additional papers examining this type of enforcement include Caulkins (1992), Caulkins and Reuter (2010), Dobkin et al. (2014), Pollack and Reuter (2014), Castillo et al. (2020), and Blattman et al. (2021). The purely illicit nature of these markets and data constraints have limited the ability of researchers to granularly test for both spatial and substance substitution following targeted enforcement actions. In particular, there is limited evidence on the second margin of a potential demand-side response to crackdowns.

Similar to the literature on drug enforcement in purely illicit markets, I find evidence of spatial displacement, but only for pharmacy crackdowns. The persistent decline in prescription opioid dispensing across space and time for doctor crackdowns suggests that certain supply-side interventions can be quite effective at reducing diversion and generating deterrence.[7] Furthermore, given the net mortality decreases, my results suggest that there is scope for complementary demand-side interventions to address the unintended consequence of heroin substitution, such as targeted treatment programs or harm reduction strategies.

The remainder of this paper is organized as follows. Section 2 provides background information that helps contextualize and interpret the main empirical findings. Section 3 describes the primary data sources. Section 4 determines the extent of substance and spatial substitution following doctor crackdowns. Section 5 examines pharmacy crackdowns. Section 6 concludes.

2 Background Information

This section helps frame and interpret the empirical results found in Sections 4 and 5. Section 2.1 provides information on controlled substances and the role of doctors and pharmacies. Section 2.2 presents facts associated with the opioid epidemic. Section 2.3 describes diversion in the prescription opioid market and contains a conceptual framework for each substitution margin. Section 2.4 highlights relevant institutional details regarding DEA enforcement. Section 2.5 presents an outlier detection method from forensic accounting which I use to identify suspicious pharmacy behavior.

[7] In addition to differences in the opportunity cost of crime between doctors and pharmacists and their respective customers, interviews with DEA agents suggest that rogue pharmacies are often part of a larger criminal enterprise, while rogue doctors are generally not.

2.1 Controlled Substances

Prescription drugs are produced and distributed in a highly regulated medical system. The Food and Drug Administration (FDA) decides first whether a pharmaceutical can be sold based on whether the pharmaceutical company has provided adequate scientific evidence of its efficacy and therapeutic benefit. If approved, the Controlled Substances Act (CSA) states that controlled substances are subject to additional rules and regulations from the DEA. Only authorized veterinarians, dentists, optometrists, physicians, and advanced practice nurses can prescribe such drugs. While there are state-specific requirements to practice, medical professionals who want to prescribe or dispense controlled substances must also receive a special federal license from the DEA (Powell and Pacula, 2017).

The DEA assigns controlled substances a 'schedule' of I through V. Scheduling is determined by considering both the medical benefits of a drug and the potential for misuse or addiction. Schedule I drugs are prohibited and considered to have no medical benefits and a high potential for misuse and addiction; there are fewer restrictions on access to drugs in lower schedules. For example, heroin is a schedule I drug while prescription opioids, such as oxycodone, fentanyl, and hydrocodone, are schedule II pharmaceuticals (Maclean et al., 2022).

The Code of Federal Regulations (21 CFR 1306.04) states that

> [a] prescription for a controlled substance to be effective must be issued for a *legitimate medical purpose by an individual practitioner acting in the usual course of his professional practice* [emphasis added]. The responsibility for the proper prescribing and dispensing of controlled substances is upon the prescribing practitioner, but a *corresponding responsibility rests with the pharmacist who fills the prescription* [emphasis added]. An order purporting to be a prescription issued not in the usual course of professional treatment or in legitimate and authorized research is not a prescription within the meaning and intent of section 309 of the Act (21 U.S.C. 829) and the person knowingly filling such a purported prescription, as well as the person issuing it, shall be subject to the penalties provided for violations of the provisions of law relating to controlled substances.

The mission of DEA's Diversion Control Division is then to "prevent, detect, and investigate the diversion of controlled pharmaceuticals and listed chemicals from legitimate sources while ensuring an adequate and uninterrupted supply for legitimate medical, commercial, and scientific needs." Penalties for violating any aspect of the DEA's regulations range from fines and the loss of their prescribing license to prison time.

Unlike other addictive substances, controlled substances serve an important medical purpose. For example, prescription opioids effectively treat severe post-operative pain (Wiffen et al., 2016). Initiation, therefore, may be driven by a medical need, not just an intention to "get high." This means that the pool of potential users is larger than it is for illegally-produced drugs (Volkow and McLellan, 2016). Of primary concern is non-medical use and misuse, where the former is when the user is not the one originally prescribed the medication and the latter is when it is used in a manner inconsistent with the doctor's orders. Individuals can obtain prescription drugs for non-medical use through several channels including theft, street purchases, from a friend or relative, and "doctor shopping" (Grecu et al., 2019).

The legal standard described in 21 CFR 1306.04 would necessitate a doctor to conduct a medical examination and record the patient's history, as well as the patient to be experiencing severe pain (Cutler and Glaeser, 2021). If a prescription is written, it is often filled at a pharmacy. The pharmacist has a responsibility, among other things, to ensure that the prescription is valid and that the patient is not doctor shopping. In my sample period, rogue doctors were often running high-volume practices that did not see patients for more than a few minutes (Quinones, 2015). Rogue pharmacies often filled forged prescriptions and thus were not always linked to a doctor (Eyre, 2020).

2.2 The Opioid Epidemic

In 2018, an estimated 10.1 million people aged 12 and older misused opioids. Figure 1 shows the change in the distribution of prescription opioids and the associated mortality rate. OxyContin, the pain reliever produced by Purdue Pharma at the heart of the opioid epidemic, was introduced in 1996. Its main ingredient is OxyCodone, which sees steady increases over time that correspond to increases in the mortality rate. There are several potential explanations for the dramatic rise in opioid dispensing and abuse over time, which include aggressive marketing, changes in willingness to prescribe opioids, over-prescribing, declining costs to customers, and diversion (Powell and Pacula, 2017; McCance-Katz, 2020).

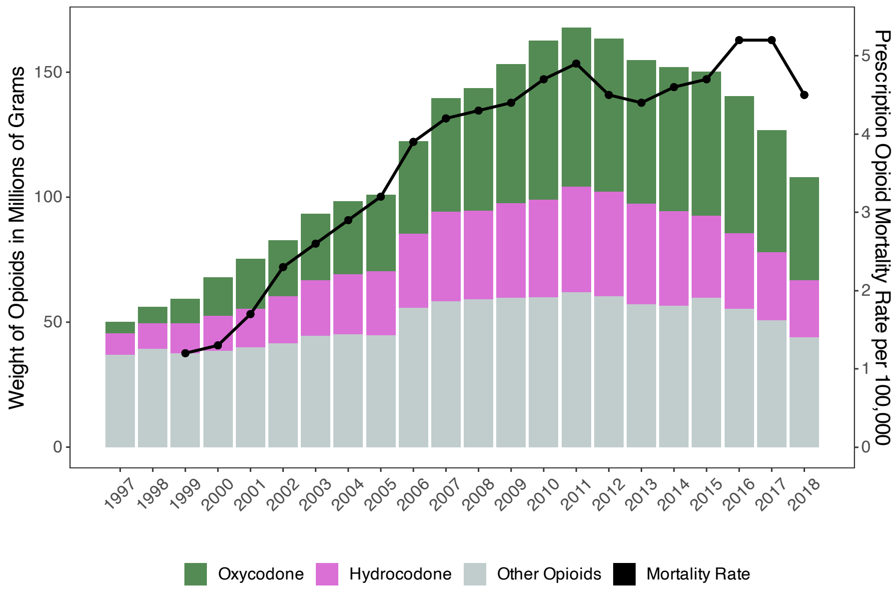

Figure 1: Evolution of Prescription Opioid Distribution and Mortality in the United States

Notes: Oxycodone is the active ingredient in OxyContin. Shipments of prescription opioids are expressed in morphine-equivalent doses on the left axis. The mortality rate from prescription opioids is constructed using data from the National Vital Statistic System and plotted in a black line on the right axis.

A unique aspect of prescription drug markets is the relatively low cost consumers pay for the intoxicating substance. Opioid pain relievers are now included in all prescription drug plans, so individuals getting these drugs through medical markets pay a small fraction of the drug's total cost. One implication is that patients have an incentive to overconsume the product, i.e., moral hazard (Ketcham and Simon, 2008). Patients also have the potential to profit from selling unused drugs. Quinones (2015) finds that for a $3 Medicaid copay, an addict receives

OxyContin pills priced at $1000 and could sell them for $10,000 on the street.[8]

Another contributing factor is (the lack of timely) enforcement. The Office of the Inspector General recently examined the regulatory activities and enforcement efforts of the DEA and found that it was slow to respond to the opioid crisis (Department of Justice, 2019). Eyre (2020) provides additional insight from a May 2018 hearing involving the DEA director and the five largest drug distributors held by the Subcommittee on Oversight and Investigations of the Committee on Energy and Commerce:

> Distributors set limits on pharmacy drug orders but failed to enforce them. They flagged excessive shipments but did not stop them. They sold drugs to pharmacies they knew were breaking the law. They willfully ignored red flags and stark warning signs that the drugs they sold were being diverted to the black market…"Your agency needs to be turned upside down," Rep. Chris Collins, R-N.Y., said. "There is no doubt there is an abject failure in the DEA going back 10 years."

2.3 Opioid Diversion and Potential Spillovers from Enforcement

Within a market-oriented complex of criminal activity, there are important interrelations that involve suppliers, middlemen, and customers (Cook, 1980). For example, with heroin, this includes poppy growers and heroin processors, importers and retailers, and users. In a market for controlled substances, it is manufacturers and distributors, doctors and pharmacies, and patients, where Figure 2 summarizes information gathered on rogue doctors and pharmacies involved in the prescription opioid market. According to the Office of National Drug Control Policy, disrupting a market that supports illegal activity is one of three core priorities for federal drug policy.[9] The motivation for disrupting these markets is that while they operate outside of the law, they do not operate outside of social and economic forces. The implicit assumption is therefore that disrupted markets will be less able to supply customer demand for their product (Brownstein and Taylor, 2007). Figure A.2 shows that if a substitute is available, a supply disruption may lead to worse health outcomes: heroin is more lethal than prescription opioids, and for some users, it is a close substitute (Alpert et al., 2018).

Note that while legally- and illegally-produced drugs are pharmacologically similar, the controlled substance market is more like the gun market than an illegal drug market: their market structure, supply chain, and enforcement agency are similar. Enforcement actions in markets for illegal drugs include source-country eradication, interdiction, and those related to traditional policing, while those for controlled substances and guns include regulation and administrative action. Especially relevant for gun violence and opioid abuse is that virtually all guns and opioid pills in the illegal market begin in the legal market.

Determining the extent of displacement is important not only to evaluate the overall effectiveness of a market disruption approach, but because the answer has implications for our understanding of criminal behavior (Blattman et al., 2021). In economic models of criminal decision-making, such as Becker (1968), potential offenders weigh the returns from engaging in illegal activity against the risk of capture and expected sanction. Supply-side interventions

[8] A study from 2013 found that a single prescription of generic oxycodone filled at a pharmacy would cost a patient $200 and would command a black-market price between $1100 and $2400 (Office of Inspector General and Services, 2013).

[9] The other two core priorities are stopping drug use before it begins and healing drug users.

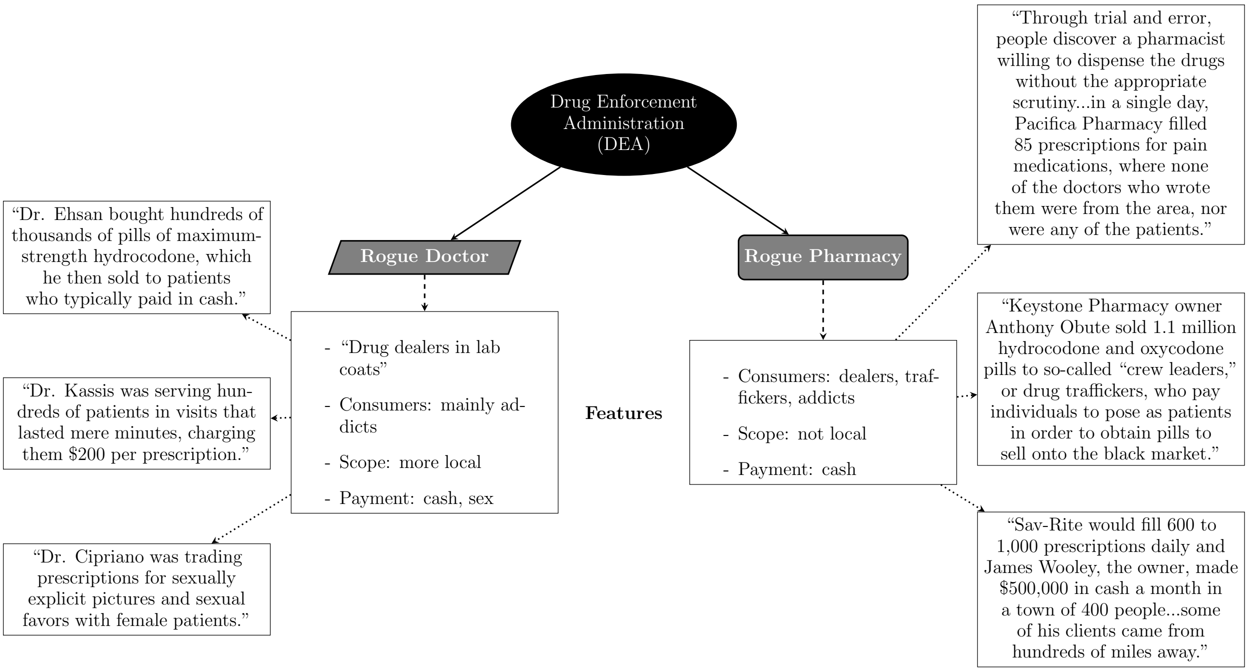

Figure 2: Excerpts from reports and articles on rogue doctors and pharmacies

Sources: interviews with DEA agents, news media articles, and DEA Diversion Control Division reports

should increase the risk of capture in a given area and thus reduce the likelihood of crime commission there. According to Johnson et al. (2014), the degree to which displacement occurs depends on how invested an offender is in committing a crime, the degree to which the extra effort of finding and exploiting an alternative is worthwhile, and the degree to which offenders know of other potential situations that they can manipulate.

If displacement does not occur, Blattman et al. (2021) suggest that at least one of the following hypotheses are true: criminal rents are highly concentrated and unequally distributed within a given area, some offenders are resistant to moving crime locations, and/or the supply of crime is elastic to the actual or perceived risk of apprehension in a small number of areas. To formalize the spatial substitution analysis, consider a market m that has two cities $c \in \{1, 2\}$ and that the probability of apprehension is p_c. If we assume that the market production function for opioids is

$$\text{opioids}_m = \text{opioids}_1(p_1, p_2) + \text{opioids}_2(p_1, p_2),$$

where opioids_c is the amount of opioids dispensed in a given city, then the total effect of an increase in enforcement in one city can be decomposed into

$$\frac{\partial \text{opioids}_m}{\partial p_1} = \underbrace{\frac{\partial \text{opioids}_1}{\partial p_1}}_{\text{local effect}} + \underbrace{\frac{\partial \text{opioids}_2}{\partial p_1}}_{\text{spatial spillover}} .$$

Existing literature suggests that the local effect is likely negative, as prescribing is relatively concentrated and thus a single doctor can play a large role in local drug supply, but I test this empirically. The spatial spillover effect could be zero/negative (market-level deterrence) or positive (market-level displacement) depending on various margins of substitutability, such as the opportunity cost of crime for a supply chain actor, distance to the nearest rogue actor, reputation costs, or informational asymmetries for the user.

The primary underlying consideration, however, is the distribution of rogue behavior within each of these two groups of actors. Assume that there is a threshold of perceived profitability above which a given supply chain actor decides to participate in illegal activity. The crackdown may increase the perceived revenue, as when a competitor is removed, more customers are available. Simultaneously, it may increase the perceived probability of getting caught. These competing forces will affect expected profits and thus the marginal doctor or pharmacy. However, the point of indifference and the size of the shift in potential profit following a crackdown may be different for doctors and pharmacies.

As described in Section 2.1, the primary responsibility of a patient's health lies with the doctor. They also have extensive training and a lucrative legal option for work. Therefore, the threshold needed to participate in illegal activity is likely high at baseline. The saliency of the crackdown combined with their legal liability likely means that if the shift in potential profit is not very large, the intervention may not entice marginal doctors to participate. There also may be little mass near the threshold or doctors do not have much scope for additional capacity.

The opportunity cost to commit crime, or the personal cost of illegal activity, is potentially lower for a pharmacist than for a doctor. They can go out of business and only have a corresponding legal responsibility. The threshold of perceived profitability needed to participate in illegal activity is therefore likely lower. Additionally, the clientele of each actor is often different. Rogue pharmacies often work with drug dealers and criminal organizations, who have a significant incentive to search for another rogue pharmacy to meet their trafficking needs. Given the scale of such operations, the shift in perceived revenue following a crackdown may be quite large. They may also have more excess capacity than doctors. Taken together, this suggests that a pharmacy crackdown is more likely to induce spatial displacement.

2.4 DEA Enforcement during the Opioid Epidemic

To obtain causal estimates for the impacts of crackdowns, I exploit plausibly exogenous variation in the timing and location of DEA audits. Figure A.3 provides a flow chart summarizing the DEA investigation process following a CSA violation. For doctors, actions that would lead to a violation include prescribing controlled substances that were either not for a legitimate medical purpose or not in the usual course of professional practice. The DEA conducts audits of controlled substance license registrants, and conditional on DEA field division, the timing of these audits are conducted at random.[10] If a given registrant is found to be in violation, the audit report can be sent to the Office of Administrative Law Judges to conduct a formal hearing and adjudication. There are two Administrative Law Judges, appointed for life, that hear cases from all 23 DEA field divisions. The Deputy Administrator of the DEA then issues the final Agency decision after the hearing is complete. There are relatively few cases, with less than one in ten thousand doctors found in violation in any year of my sample, and Department

[10]The exact algorithm that predicts an audit is not currently known, but for the analysis, all that is needed is plausibly exogenous variation in the timing of the audit, conditional on DEA division, which I test for in Section 4. Note that the DEA also pursues anonymous tips to determine CSA compliance. They lead to less than 15% of all subsequent enforcement actions during my sample and these treated units are removed from the analysis.

of Justice (2019) states that

> ...the DEA was slow to respond to the significant increase in the use and diversion of opioids since 2000. We also found that DEA did not use its available resources, including its data systems and strongest administrative enforcement tools, to detect and regulate diversion effectively. Further, we found that DEA policies and regulations did not adequately hold registrants accountable or prevent the diversion of pharmaceutical opioids. Lastly, we found that while the Department and DEA have recently taken steps to address the crisis, more work is needed.

2.5 Suspicious Behavior Detection

In addition to submitting information on all controlled substance shipments to the DEA, the CSA requires drug distributors to "design and operate a system to disclose all suspicious orders of controlled substances." Given the richness of the data that was available to the DEA and that their data systems were cited as a primary issue in Department of Justice (2019), I use their data to flag suspicious behavior of non-sanctioned pharmacies. I will then use this to generate control groups for the main analysis and to examine how non-sanctioned pharmacies respond to a crackdown.

Suspicious orders are considered deviations from a normal ordering pattern, and I flag suspicious behavior using the "twice the trailing 12-month average pharmacy dosage units for each pharmacy" method. This detection method is referenced in court documents from Ohio lawsuits against opioid distributors (Ohio MDL 2804).[11] Figure 3 shows what this method looks like graphically, where a pharmacy is suspicious any time one of their monthly orders (gray bar) exceed two times the rolling average (red line). The pharmacy in the left panel has their first suspicious order in the beginning of 2009. Their ordering continues to increase until around August 2010. The pharmacy in the right panel does not have any significant deviations in its ordering and thus would not be flagged as suspicious. For reference, the total share of pharmacies that are suspicious in any given month is approximately 0.5%.

3 Data

In order to examine both the direct and spillover effects of supplier crackdowns in a prescription opioid market, I combine four sources of data. First, as part of the CSA, distributors and manufacturers of controlled substances are required to report all transactions to the DEA. This Automation of Reports and Consolidated Orders System (ARCOS) database contains the record of every controlled substance sold in the United States. The complete database for opioids, from 2006 to 2014, was recently released by a federal judge as a result of a trial in Ohio against manufacturers and distributors. The ARCOS database has been previously used to study opioids, but generally only using the publicly available quarterly aggregate weight of drugs sold or via special request to the DEA (Zhang and Guth, 2021). The newly released database allows me to examine supply dynamics at any level of geographic or temporal aggregation. For example, Figure A.4 shows that there is significant variation in both the levels and trends in

[11] Such a method does not capture, for example, pharmacies that always dispense a very high amount of opioids. Therefore, I also explore high per-capita dispensing (99th percentile for the year) and other outlier detection methods from forensic accounting. They show qualitatively similar results and are in the appendix.

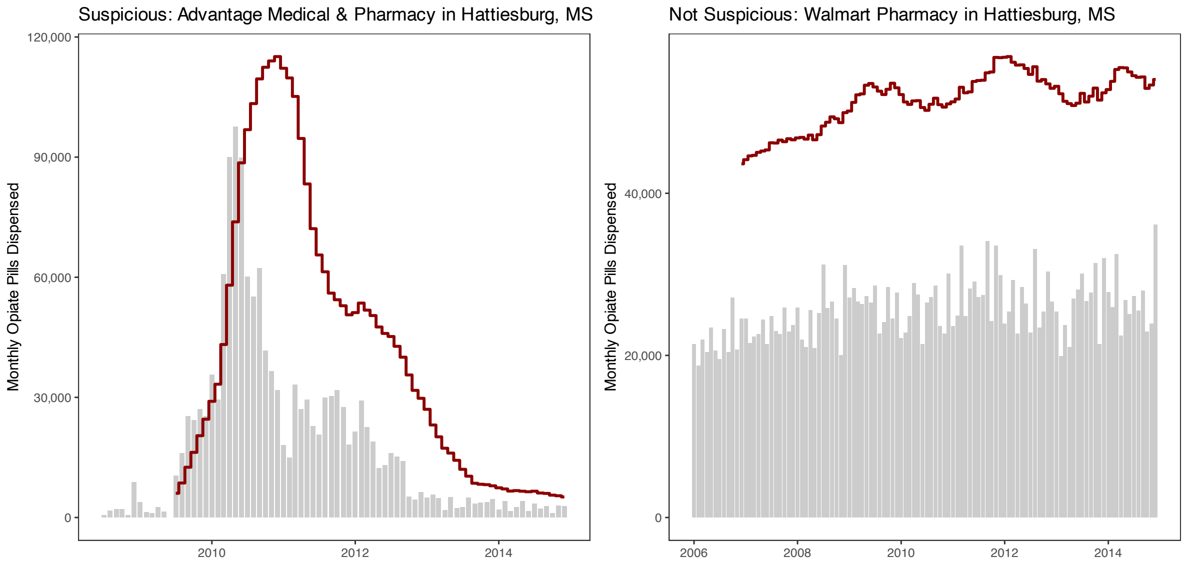

Figure 3: Examples of Implementing Criteria for Flagging Suspicious Orders

Notes: gray bars show total monthly opioid pill orders and the red line captures the "twice the trailing 12-month average pharmacy dosage units" outlier detection threshold for each pharmacy. The left pharmacy would be flagged as suspicious, as monthly orders during 2009 and 2010 exceed the red line, while the right pharmacy would not be.

yearly per capita opioid dispensing across states. Figure A.5 provides a heat map of per capita dispensing at the county-level.

For each of these over 180 million transactions, I have the DEA number, address, name, and business type for both the reporter and the buyer, as well as information on each transaction, such as the transaction date, opioid drug name, quantity, strength, and Morphine Milligram Equivalent (MME) conversion factor. I consider only retail pharmacies, where approximately 90,000 unique chain and independent pharmacies are in operation during my sample period. An observation in the raw data is therefore a single transaction between a retail pharmacy and a distributor or manufacturer. I aggregate these data to the pharmacy-month level and convert the dosage into MME so that different opioid pills can be compared. For the primary analysis, I aggregate this further to a certain unit of geography in a given month or year, which includes city, county, or market depending on the outcome.

For the second source, I construct a dataset on every DEA investigation of a doctor or pharmacy using Federal Register notices and the DEA's Diversion Control Division reports. These notices and reports contain information on the registrant's DEA number, operating location, details of the investigation, and the outcome of the investigation. They represent the universe of investigations with a disposition. I examined all reports and notices from 2000 until 2019 involving doctors or pharmacies and focus on potential revocations of the registrant's DEA license. This includes the doctor voluntarily surrendering their DEA license or the state revoking their medical license before the investigation is completed.[12] Figure 4 shows that few investigations occurred and that they were not spatially concentrated.

For the third source, I utilize information on black-market opioid pill prices from StreetRx,

[12] I exclude DEA license applications, which are the remainder of the notices, as they unrelated.

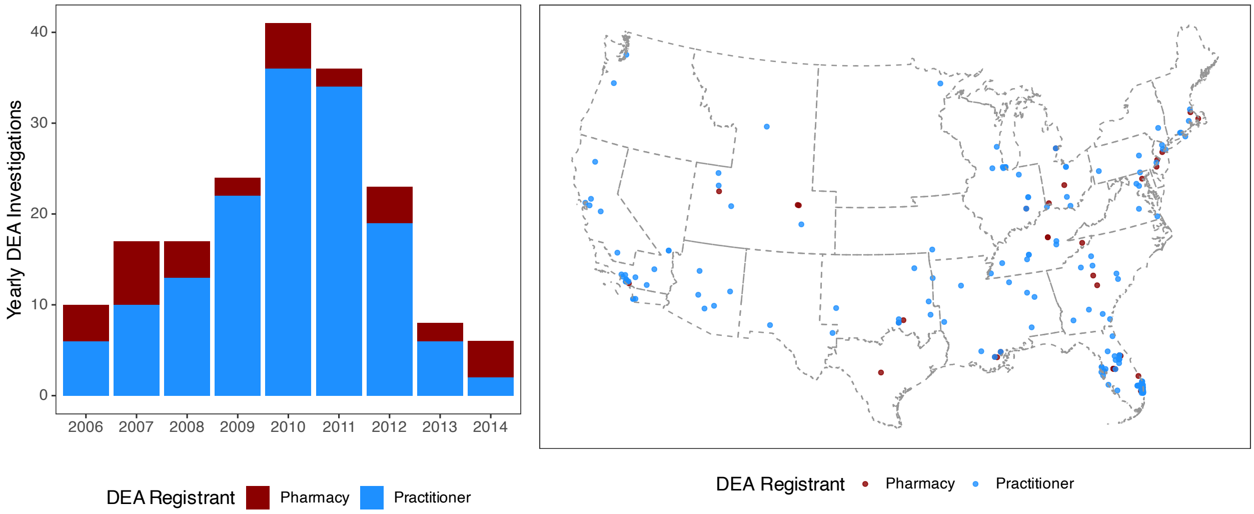

Figure 4: Yearly DEA Investigations from 2006 until 2014

Notes: The left panel shows the number of times when a registrant type was found to be in violation of the Controlled Substance Act and when it entered the Federal Register. The right panel shows where these actions occur; dashed grey lines are DEA jurisdiction boundaries.

which is a partnership between the Denver Health and Hospital Authority and Lexigraph. It is an on-going observational public health surveillance study based on crowdsourcing, where their website gathers user-submitted information on black-market prices of diverted prescription and illicit drugs (Denver Health and Hospital Authority, 2022). Visitors can anonymously view, post, and rate submissions in a format that offers transparency to an otherwise opaque market. Variables in the data include the city and date of purchase, source, product name, drug classification, and dosage since 2000. Other than occasional cross-sectional surveys for a single geographic area, the only reliable data on illicit drug prices are single yearly nationwide ones for marijuana, cocaine, heroin, and methamphetamine from the Office of National Drug Control Policy. These data correlate well with these yearly averages, but are able to go considerably further both temporally and spatially. Figure A.6 shows cross sectional county-level variation in pill prices.

For the fourth data source, I obtain micro-data from the CDC's Restricted-Use Vital Statistics Data, which are also known as the Multiple Cause of Death (MCOD) research files. They provide a comprehensive collection of information on all deaths in the United States. They include details on the decedent, the county of occurrence, the county of residence, the underlying cause of death, and multiple conditions (if applicable) since 2000. This allows me to isolate opioid-related overdose deaths, heroin overdose deaths, homicides, suicides and other relevant classifications, which can be broken down by gender and age group. Figure A.7 shows significant cross-sectional variation in opioid-related overdose death rates across counties, as well as that the vast majority of counties experience an increases in these rates over time.[13] Tables A.1 to A.3 provide county-level summary statistics.

[13]Figure A.8 shows the distribution of county-level changes in opioid overdose deaths, where the average was an increase of 74%, while Figure A.9 shows the age profiles of those that overdose from opioids by gender.

4 The Impacts of Crackdowns on Rogue Doctors

4.1 Substance Substitution

The potential for substitution to more harmful substances following a supply-side crackdown is a primary concern when taking this approach to reduce drug abuse. To examine whether this occurs and/or if legal opioid dispensing is impacted, my primary research design exploits plausibly exogenous variation in the timing and location of DEA enforcement actions in a difference-in-differences framework. I utilize the data sources described in Section 3 and begin by estimating a two-way fixed effects model of

$$Y_{it} = \beta \text{Post\_DEA\_Action}_{it} + \lambda_i + \gamma_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of interest in geographic unit i in time t, $\text{Post\_DEA\_Action}_{it}$ is a dummy variable equal to one after a treated unit experiences a DEA enforcement action, λ_i are unit fixed effects, γ_t are time fixed effects, and ε_{it} is the error term. The geographic unit depends on data availability for the outcome and can be either a city or a county; standard errors are clustered at the geographic unit. β is the coefficient of interest, which captures the average impact of a doctor crackdown on legal opioids dispensed, black-market pill prices, or heroin overdose deaths relative to control units. To examine whether using a two-way fixed effects approach is appropriate, I begin by implementing the difference-in-difference decomposition found in Goodman-Bacon (2021). Figure A.10 shows that all weights are non-negative and that the vast majority of the weighting comes from the treated versus untreated comparison.

While the full sample includes every geographic unit in the contiguous United States, I utilize three additional approaches to ensure that the estimates are unbiased. First, as treatment effect heterogeneity is a primary concern when using a two-way fixed effects approach, I utilize the dynamic aggregation method from Callaway and Sant'Anna (2021) with both not-yet-treated and never treated control units.[14] The second utilizes outlier detection methods from forensic accounting described in Section 2.5 to generate control units that are similarly "suspicious" but did not experience a crackdown. The third uses the synthetic difference-in-differences method from Arkhangelsky et al. (2021). The results are robust to these additional approaches and I present the two-way fixed effects estimates and the event studies from Callaway and Sant'Anna (2021) in the main text of the paper. The results from the additional specifications and those with different control groups are referenced in footnotes and located in the appendix.

The identifying assumption is that the average untreated potential outcomes for the group of units first treated with a violation in a given time period would have followed parallel trends in outcomes to the control group for all post-treatment periods. As this is not directly testable, I examine the implications of this assumption in three different ways. First, Table A.4 suggests that this assumption holds for observable characteristics: when I regress a dummy variable for whether the violation occurred in the first half of my sample on characteristics of counties that experienced a crackdown, I find no significant predictors and the effect sizes are small.

[14] An additional test from Jakiela (2021) is to examine the relationship between the residualized treatment and each residualized outcome. In Figure A.11, I find that this relationship is linear, which suggests that the treatment effects are homogeneous.

Table A.5 repeats this exercise with a continuous treatment timing measure at the city-level and arrives to the same conclusion. These tests suggest that the DEA does not appear to select or target certain areas earlier, such as those with higher levels of opioid-related overdose deaths. Lastly, there are no pre-trends in the event study analysis of this section and Section 5. For the primary treatment variable, $\text{Post\_DEA\_Action}_{it}$, I use the date a case is first registered in the Federal Register, which is a standardized date for when the doctor definitively knows of their CSA violation and when it is public record. The results should therefore be interpreted as an intent-to-treat (ITT) effect, as this is before any potential formal punishment.

I begin with the legal opioid supply impacts of crackdowns on rogue doctors in an event study framework. Figure 5 shows how monthly city-level opioid dispensing evolves, where we see statistically significant decreases of approximately 10% within 3 months of the initial enforcement action.[15] Refills are not allowed for schedule II controlled substances and the supply limit for any single prescription is 90 days. Additionally, pharmacists should not fill a prescription from a doctor if their registration was revoked or suspended. Therefore, any prescriptions filled after 3 months should not be attributed to the sanctioned doctor.

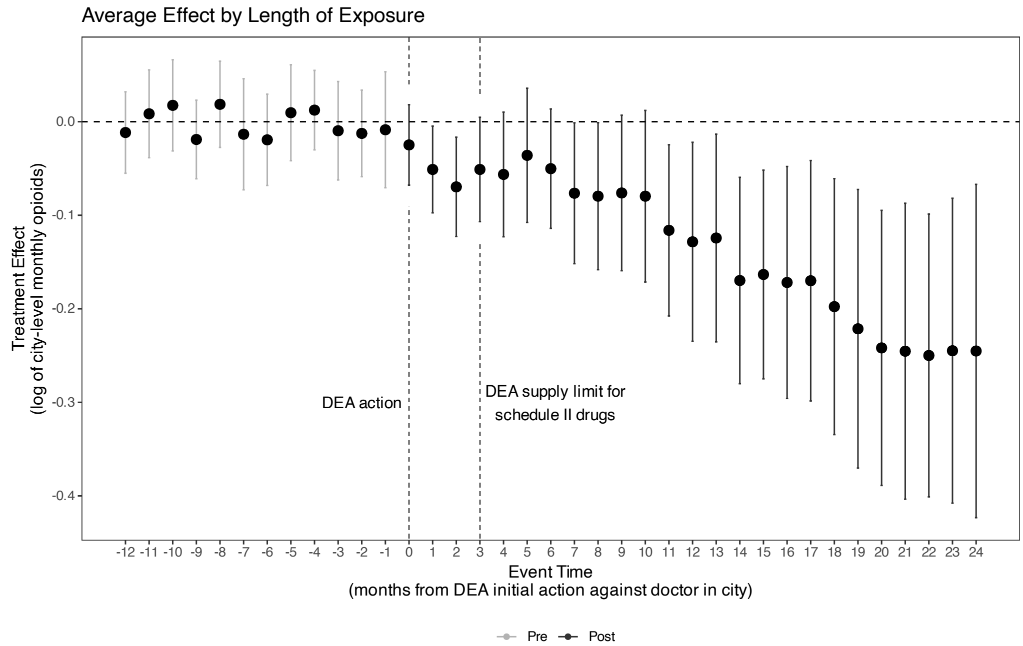

Figure 5: City-Level Impact of DEA Action against a Doctor

Notes: The outcome is the logarithm of monthly city-level opioids, where the treatment is the DEA action against taken against a doctor. The control group consists of never treated cities. Figure A.13 instead uses not-yet-treated cities and limits the sample to only cities that were treated. Standard errors are clustered at the city-level and computed using a multiplier bootstrap, and the estimation method is doubly robust from Callaway and Sant'Anna (2021). The first dotted line captures the treatment date, while the second captures the 90-day supply limit of any schedule II controlled substance prescription. Prescriptions filled after the 90-day limit should not be attributable to the sanctioned doctor.

The effect continues to grow to approximately 25% after two years, which is likely due to

[15] Figure A.12 provides examples of opioid dispensing trends for treated cities. Figure A.13 instead uses the not-yet-treated cities as the control group, where I also limit the sample to cities where a crackdown eventually occurs; the results are similar.

two factors.[16] Interviews with DEA agents suggest it is dynamic deterrence for non-sanctioned doctors, who likely are "scared straight" or prescribe more conservatively after a local colleague is sanctioned. There is often media coverage surrounding crackdowns on rogue doctors, which are generally from local news outlets or in DEA press releases.[17] The second factor involves substitution to other substances or death. I examine this in the following paragraph, but conceptually, rogue doctors want to both maximize the number of prescriptions written to current patients, as well as initiate new addicts. Sanctioning them affects both margins. In other words, if we assume that the counterfactual flow of consumers into addiction would be very high, past reduction in equilibrium quantity could also change consumer tastes and eventually lower demand for legal opioids. Additionally, Figures A.14 and A.15 show that the decline is larger for smaller cities. This is likely because a single doctor can contribute a larger share of dispensing in a smaller city.

Given the large and persistent negative legal supply shock following a crackdown on a rogue doctor, I examine their impacts on black-market opioid pill prices and heroin substitution. The results for estimating equation (1) at the county-year level are in Table 1.[18] In my sample, treated cities are smaller than their associated counties, and column (1) provides the point estimate for the average county decline in yearly dispensing: 9.4%. Columns (2) and (3) show 44% increases in both average street pill prices per milligram and heroin overdose deaths. Note that all opioid pills entering the illegal market begin in the legal market, and a single rogue doctor can play a significant role in supplying addicts directly and the black market indirectly. Supply to both channels decreases once a rogue doctor is removed. However, I cannot determine whether the rise in street prices is due to reduced supply to the black market, increased black market demand from addicts because they no longer have a valid prescription, or both.[19]

The increase in heroin overdose deaths is likely due to end user substitution following the negative supply shock. This aligns with the findings of Alpert et al. (2018) and Evans et al. (2019): states with higher initial rates of opioid misuse experienced a greater increase in heroin overdoses after a nationwide shortage of opioids. Additionally, Cicero et al. (2015) find that the incidence of heroin initiation is 19 times higher among those who reported prior non-medical pain reliever use than among those who did not. This represents a significant shift from historical trends. Of people entering treatment for heroin addiction who began abusing opioids in the 1960s, more than 80 percent started with heroin. Of those who began abusing opioids in the 2000s, 75 percent reported that their first opioid was a prescription drug; nearly 80 percent of

[16] To ensure that the dynamics are not due to changes in the composition of the control group, I examine only enforcement actions that occurred before 2013. This provides at least two years of post-treatment dispensing data for each treated city. The results are qualitatively similar if I use the entire sample.

[17] Note that the raw data is at the pharmacy-level, not doctor, and thus I cannot determine how individual doctor prescribing habits change.

[18] The corresponding event studies for county dispensing, overdoses, and prices (Figures A.16 to A.18) all show dynamic changes.

[19] To examine stability of the main results, I first randomize the treatment date and plot the resulting t-statistics and coefficients from estimating equation (1) 1000 times in Figures A.19 and A.20, respectively. This exercise suggests that the results in Table 1 are not likely due to chance. Note I randomize 1) treatment years across only treated units and 2) first which units get treated, then conditional on receiving treatment, I randomize the treatment year; the two approaches produce qualitatively similar results. Two additional checks include generating alternative control groups with outlier detection methods described in Section 2.5 and utilizing the synthetic difference-in-differences approach from Arkhangelsky et al. (2021). The results for these approaches are in Table A.6, Figure A.21, and Table A.7, and they all align with the findings of Table 1.

Table 1: Impacts of Rogue Doctor Crackdowns on Dispensing, Street Prices, and Substitution

	Opioids Dispensed	Pill Price	Heroin Deaths
	(1)	(2)	(3)
Post-DEA Action	-0.094^{***}	0.444^{***}	0.439^{***}
	(0.017)	(0.110)	(0.087)
Control Mean	3.218	1.441	1.386
Observations	26,765	5,057	26,765
R^2	0.990	0.430	0.800
Notes:	*p<0.1; **p<0.05; ***p<0.01		
	Outcomes are county-year and log transformed		
	County and year FEs in all specifications		
	Standard errors are clustered at county level		

heroin users reported using prescription opioids prior to heroin (Cicero et al., 2014).

Table 2 shows that for each additional heroin overdose death per 100,000 residents (2), there are two fewer non-heroin opioid overdoses (3). There also are reductions in non-opioid drug overdoses and suicides, yet they are smaller in magnitude and insignificant. This suggests that there is a tradeoff between existing users substituting to more harmful substances and stopping the flow of new users. Decomposing this further, Table A.8 finds a reduction in young male mortality, which suggests that heroin substitution is limited to heavy users, while the number of new recreational users is reduced. I begin with young males because they are the group that is most likely to be exposed to drugs and thus initiate use (Grecu et al., 2019). I then transition to middle-aged men, who see a larger decline in overall mortality. In additional to risk-taking behavior and occupational choice, Kloos et al. (2009) suggest that cultural ideas of masculinity may contribute to male substance use. Women of the same age groups also experience declines in overall mortality, but the effects sizes are smaller; substance abuse in women often starts later and involves lighter and/or less frequent use. As a placebo test, I examine mortality of those 85 and older and find a small and insignificant effect on death rates.

Table 2: Impacts of Rogue Doctor Crackdowns on County Mortality Rates by Type

	Per Capita Pill Volume	Heroin	Non-Heroin Opioids	Non-Opioid Drugs	Suicide
	(1)	(2)	(3)	(4)	(5)
Post-DEA Action	-3.889^{***}	0.531^{***}	-1.033^{***}	-0.441	-0.169
	(0.808)	(0.200)	(0.386)	(0.429)	(0.307)
Control Mean	39.541	0.633	11.356	10.409	14.846
Observations	26,765	26,765	26,765	26,765	26,765
R^2	0.939	0.458	0.517	0.535	0.271
Notes:	*p<0.1; **p<0.05; ***p<0.01				
	Mortality outcomes are in yearly rates per 100,000				
	County and year FEs in all specifications				
	Standard errors are clustered at county level				

The results in this section suggest that a single rogue doctor's reach is quite far: sanctioning them not only leads to large city-level declines in opioid dispensing, but to a county-level decline of almost 10% (Table 1). The dynamic declines found in Figure 5 further suggest that other doctors are at least partially deterred by the enforcement action. Moreover, while substitution to more dangerous substances is especially concerning, net mortality in these communities decreases. These mortality reductions are strongest among young and prime-aged males.

4.2 Spatial Substitution

Displacement of illicit activity following a crackdown is a critical consideration when evaluating the effectiveness of a supply-side intervention. To examine the extent of spatial displacement following a doctor crackdown, I study their pharmacy-level impacts within larger geographic areas. More formally, I estimate

$$Y_{pt} = \sum_{d} \phi_d (\text{Post\_DEA\_Action}_{pt} \times \text{Distance\_Group}_{p \in d}) + \delta_j + \lambda_t + \mu_{pt}, \tag{2}$$

where Y_{pt} is the log of legal opioid dispensed by pharmacy p in time t, $\text{Post\_DEA\_Action}_{pt}$ is a dummy variable equal to one after a treated market is exposed to a crackdown, $\text{Distance\_Group}_{p \in d}$ is a categorical variable that splits up each pharmacy p's distance to the rogue doctor into 10-mile bins d, δ_j are market fixed effects, λ_t are time fixed effects, and μ_{pt} is the error term. The reference category are pharmacies that are greater than 100 miles from the crackdown.[20]

Figure 6 presents the estimates of ϕ from equation (2), where we observe large decreases within 10 miles of the doctor crackdown: the average decline in pharmacy dispensing in this distance bin is 17%. The effect becomes less negative until 40 miles, crossing the zero at 20 to 30 miles from the crackdown. It then remains insignificant and small thereafter. The overall effect is a small but significant decrease in dispensing. This suggests that deterrence plays a significant role in the behavior of nearby doctors following a crackdown, as the decline in dispensing persists across space and time with no negating increases.

5 Pharmacy Interventions and Spatial Dynamics

Doctors are the primary focus of this paper, as they are the source of all legal prescriptions, but pharmacies also play a critical role in circulating opioids. While there are considerably fewer enforcement actions taken against pharmacies than doctors, which limits statistical power if repeating the analysis found in Section 4, I can conduct a more granular examination of crackdowns because the data is at the pharmacy-level. For the sake of comparison, I repeat the exercises from the previous section, which show similar results between doctor and pharmacy crackdowns at the city- and county-levels; these are in the appendix and include Figures A.22 to A.29 and Tables A.9 to A.14.

To examine the extent of spatial displacement, I re-estimate equation (2) for pharmacies in Figure 7. We see large declines for pharmacy crackdowns until 30 to 40 miles, then significant increases thereafter. I cannot reject the null hypothesis that any of the first five coefficients are equal to each other, but they are each statistically different than the last five, and the overall

[20]The results are not sensitive to the choice of buffer size within 15 miles and so I use 100 miles to align with the literature and to ensure there is no overlap of buffer zones. I also separately replace market fixed effects with market-by-distance bin fixed effects and pharmacy fixed effects, and the results are qualitatively similar. Lastly, I cannot estimate equation (2) for prices or overdoses, as these data sources are only available at the county-level.

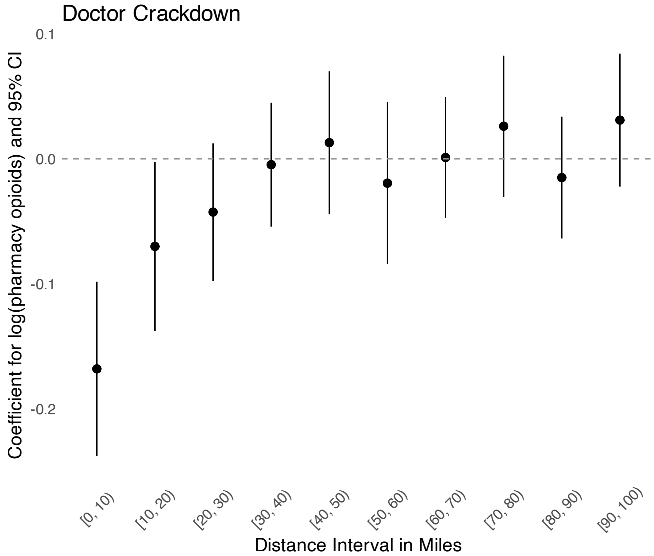

Figure 6: The Pharmacy-Level Impacts of Crackdowns on Rogue Doctors

Notes: Market and year fixed effects are included, and standard errors are clustered at the DEA action. The plotted coefficients are the interaction between each 10-mile distance bin and the DEA enforcement action. Pharmacies farther than 100 miles from the enforcement action are the omitted category.

effect is a small and insignificant decrease. This spatial displacement contrasts to Figure 6, which finds persistent and significant declines following a doctor crackdown. As an alternative test of displacement, I aggregate the data to the market-level, remove markets that did not experience a crackdown, and then estimate

$$Y_{jt} = \pi \text{Post\_DEA\_Action}_{jt} + \delta_j + \lambda_t + \nu_{jt}, \tag{3}$$

where Y_{jt} is the outcome of interest in market j in month t, $\text{Post\_DEA\_Action}_{jt}$ is a dummy variable equal to one after a given market is exposed to a sanctioned actor, δ_j are market fixed effects, λ_t are year fixed effects, and ν_{jt} is the error term.[21] The parameter of interest, π, captures the average impact of a crackdown at the market-level relative to markets that have yet to have a crackdown.

Table 3 shows the results from estimating equation (3) in a market that is within 100 miles of a crackdown.[22] In columns (1) and (3), we see that there is no meaningful change in overall opioid dispensing after a pharmacy crackdown and that the share of suspicious pharmacies increased by approximately 8%. The typical 100-mile area has approximately 300 pharmacies, with 0.5% of them being suspicious in any given month, so this represents a large increase relative to the baseline. For doctor crackdowns, columns (2) and (4) of Table 3 show no meaningful change in suspicious behavior and instead a persistent decline in overall opioid dispensing. Taken together, the results of this section suggest that spatial displacement is a concern for pharmacy crackdowns, while non-sanctioned doctors are at least partially deterred by a crackdown against

[21] I cannot estimate equations (2) or (3) for prices or overdoses, as these data sources are only available at the county-level.

[22] Similar to estimating equation (2), the results are not sensitive to the choice of buffer size within 15 miles and so I use 100 miles to align with the literature and to ensure there is no overlap of buffer zones.

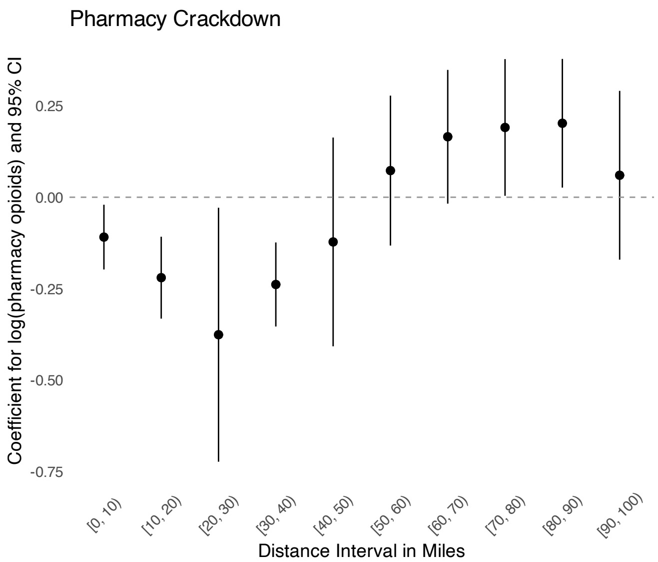

Figure 7: The Pharmacy-Level Impacts of Crackdowns on Rogue Pharmacies

Notes: The outcome is the log of pharmacy-level opioid dispensing. Market and year fixed effects are included in all models, and standard errors are clustered at the DEA action. The plotted coefficients are the interaction between each 10-mile distance bin and the DEA enforcement action. Pharmacies farther than 100 miles from the enforcement action are the omitted category.

another doctor.

Table 3: The Impacts of Crackdowns on Rogue Actors at a 100-Mile Market Level

	Monthly Opioids		Share Suspicious	
	Pharmacy	Doctor	Pharmacy	Doctor
	(1)	(2)	(3)	(4)
Post-DEA Action	-0.015	-0.056^{***}	0.083^{***}	0.010
	(0.050)	(0.021)	(0.016)	(0.028)
Control Mean	67.343	51.408	0.605	0.576
Observations	3,672	13,608	3,672	13,608
R^2	0.988	0.983	0.109	0.249
Notes:	*p<0.1; **p<0.05; ***p<0.01			
	DEA crackdown and year FEs in all specifications			
	Standard errors are clustered at the DEA crackdown			
	Outcomes are market-month and log transformed			

For pharmacy crackdowns, Figure 8 shows that the average decrease in prescription opioids achieved from the supply-side intervention is almost fully negated by several initially low-volume independent pharmacies that significantly increase their post-crackdown dispensing. More specifically, on average 8 pharmacies per crackdown become suspicious within the first 3 months and 100 miles of an enforcement action; the average number of suspicious pharmacies per 100 miles is 1.6. The average increase in dispensing the year after the crackdown for each of these 8 newly suspicious pharmacies is 70,000 opioid doses, or a 110% increase above their pre-crackdown dispensing. The total dose increase of 560,000 from these 8 pharmacies almost completely negates the 740,000 dose decrease achieved from sanctioning the rogue pharmacy.

This displacement is not likely due to legitimate users moving to these newly suspicious

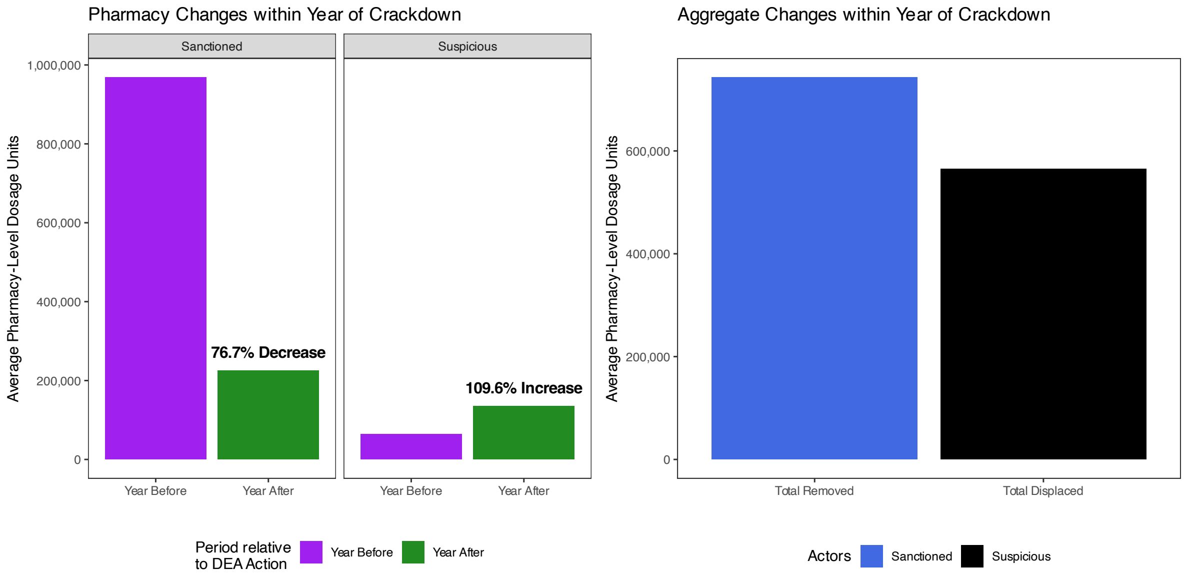

Figure 8: Displacement following a Pharmacy Crackdown

Notes: The left panel shows the average change in pharmacy-level dispensing one year before and after a pharmacy crackdown for both the average sanctioned pharmacy and the average newly suspicious pharmacy. The right panel multiplies the average increase from a newly suspicious pharmacy by the average number of newly suspicious pharmacies.

pharmacies, as they are on average 40 miles from the sanctioned pharmacy. A newly suspicious pharmacy is never the next closest pharmacy to the sanctioned one, as there is always at least one other pharmacy operating within 6 miles of the crackdown. They are also always close to an interstate highway: in the US, the average distance from a pharmacy to an interstate highway is 8.9 miles, yet newly suspicious pharmacies are on average 4.5 miles from an interstate. This is suggestive evidence that the spatial displacement is not mechanical and is following some economic rationale. Interviews with the DEA agents suggest that one possibility is that drug traffickers are attempting to diversify their supply or spread their risk across multiple pharmacies. Given that these pharmacy crackdowns are effective at reducing prescription opioid supply locally, the suspicious behavior detection methods used in this section may be helpful to the DEA in order to minimize displacement.

6 Conclusion

Supply-side interventions are the primary policy tool for curbing drug abuse in the United States. When evaluating the effectiveness of this approach, there are two important demand-side spillovers to consider, namely substitution to other suppliers and substitution to more harmful substances. Despite the substantial costs associated with drug epidemics, which often originate with legally-produced drugs, we have little sense of the impacts of targeted enforcement in legal drug markets. This is especially relevant because regulation has either fallen short or been subverted, and drug treatment has not been a policy priority.

This paper is one of the first to examine the effectiveness of supply-side crackdowns on individual actors within a market for controlled substances. Exploiting plausibly exogenous variation in the timing and location of controlled substance license audits, I find that cracking down on a single rogue doctor causes large decreases in local opioid dispensing that persist across space and time. This sustained reduction in legal drug supply increases both street pill prices and heroin overdose deaths but decreases net mortality. These results suggest that substitution to heroin is limited to existing heavy users and that targeted enforcement reduces the number of new recreational users. Given that these interventions are effective at reducing local drug supply, the policy implication is that complementary demand-side interventions should be employed to minimize substance substitution. These may include treatment programs and harm reduction strategies.

Future research should focus on supply-side factors that may have accelerated the opioid epidemic, as changes in demand-side factors alone can only explain a small fraction of the increase in opioid use and deaths over time (Currie and Schwandt, 2021; Cutler and Glaeser, 2021). However, our current public health crisis involving opioids is not unique. The failure to consider the addictive potential of legally-produced drugs is a common theme across domestic drug epidemics since the Civil War (Moore and Pacula, 2020). The findings of this paper suggest that using only supply-side interventions after a certain threshold of community-level addiction is reached can be problematic. Moving forward, quantifying the optimal mix of enforcement and treatment is critical for properly addressing drug abuse.

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A Additional Figures and Tables

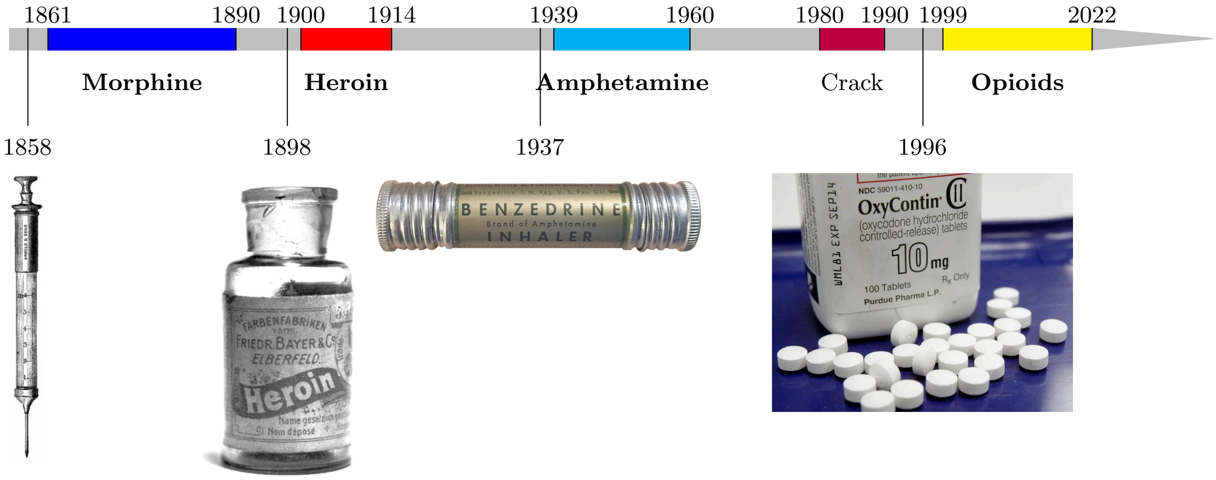


Figure A.1: History of Nationwide Drug Epidemics since the US Civil War

Note: Substances in bold were legally produced at the time.

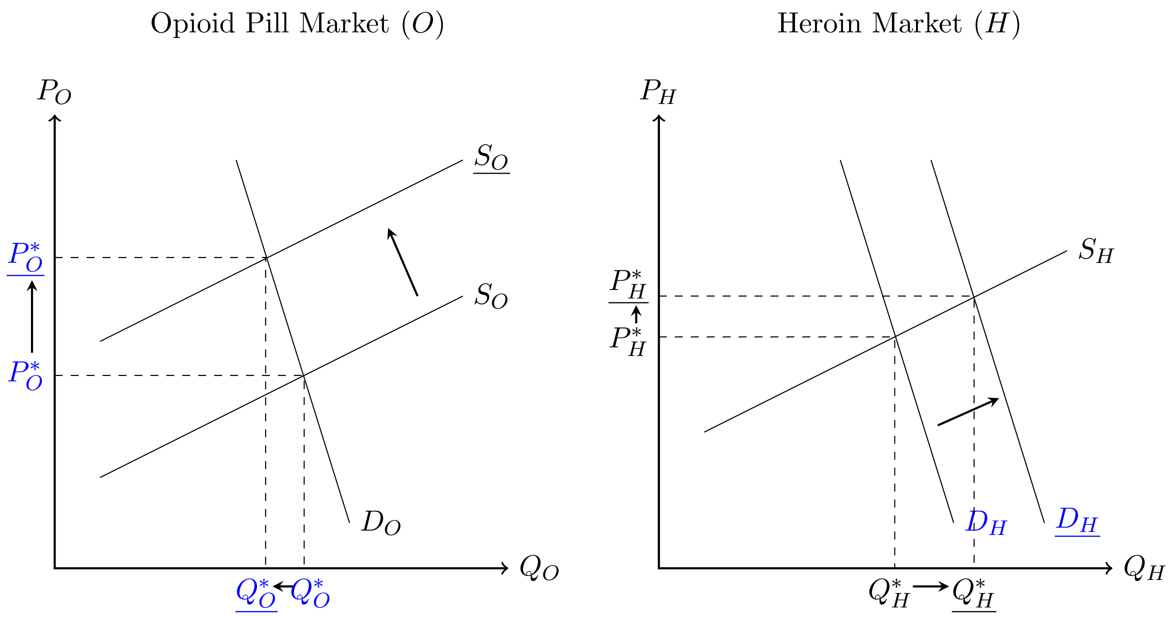


Figure A.2: Supply-Side Drug Policy in the Presence of Substitutes

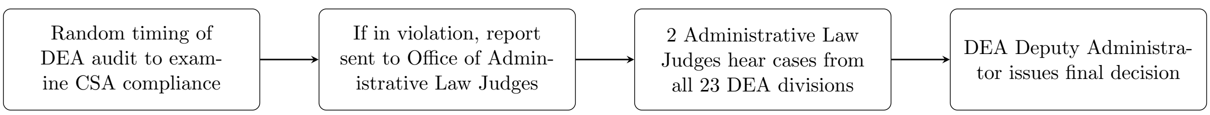


Figure A.3: DEA Investigation Process for Violations with Controlled Substances Act

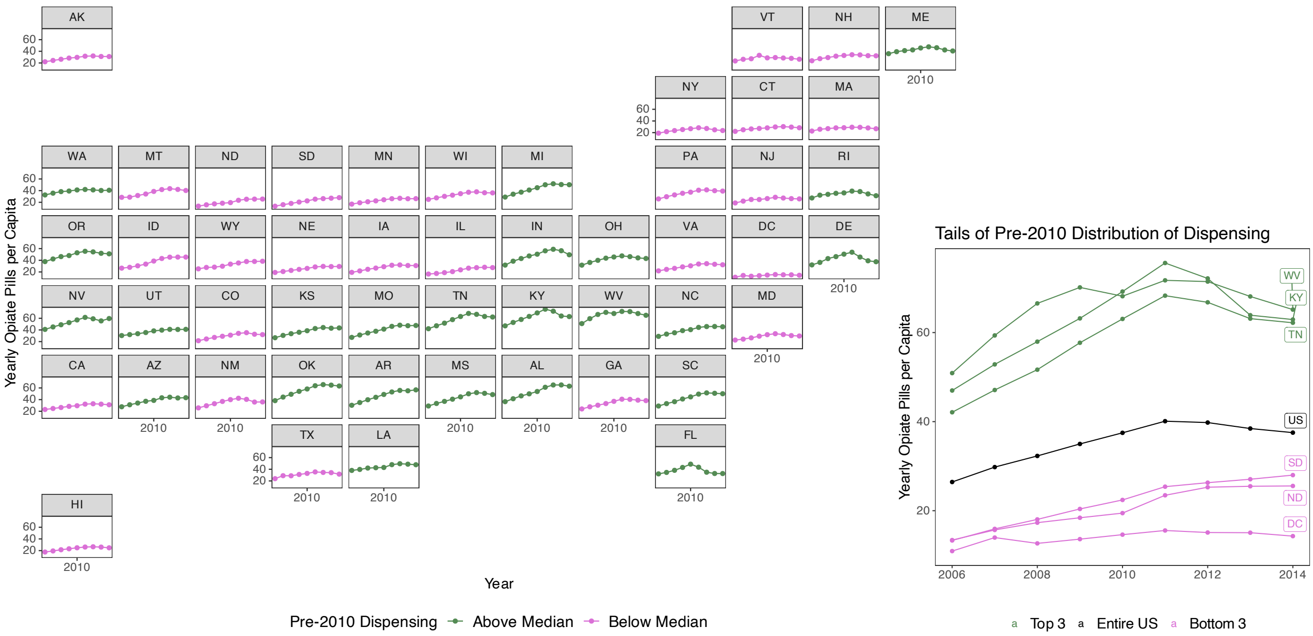


Figure A.4: Yearly per Capita Opioid Dispensing by State (2006 to 2014)

Notes: Per capita opioid dispensing is calculated by dividing the total yearly opioid doses dispensed in a given state by the yearly state population. The states are colored in by whether their pre-2010 average yearly per capita dispensing is above or below the US median. The right panel isolates the top and bottom 3 states. These figures show the significant spatial and temporal variation in a yearly per capita opioid dispensing at the state-level.

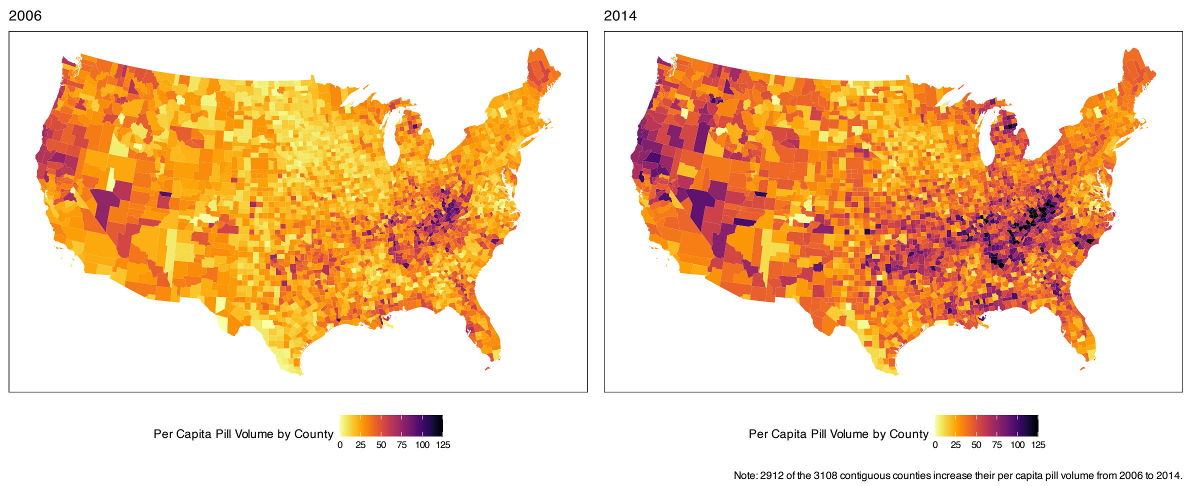


Figure A.5: Variation in per capita pill volume changes by county

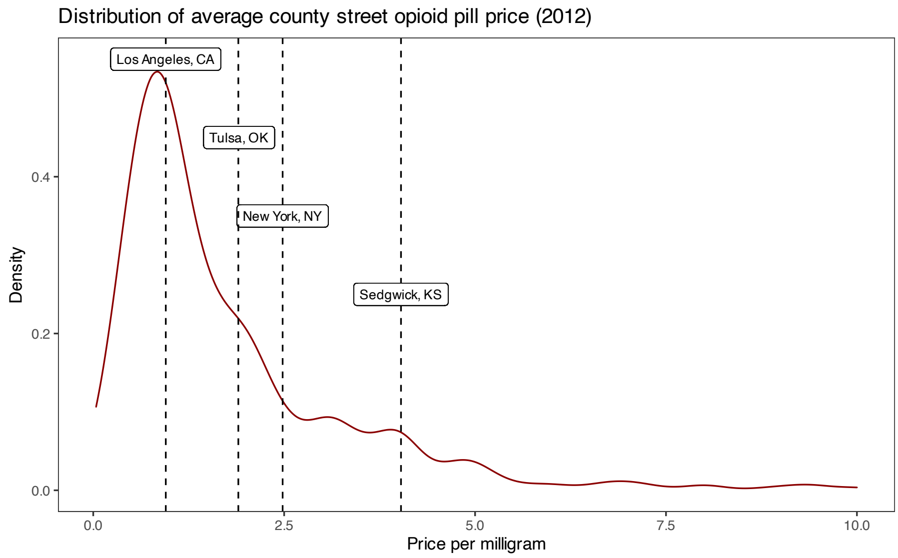


Figure A.6: County-level opioid pill price per milligram

Notes: This figure presents the cross-sectional distribution of street pill price per milligram. Four counties are highlighted to show the significant variation in prices, as generally, the only other available data on illicit drug prices are single yearly nationwide prices for marijuana, cocaine, heroin, and methamphetamine from the Office of National Drug Control Policy.

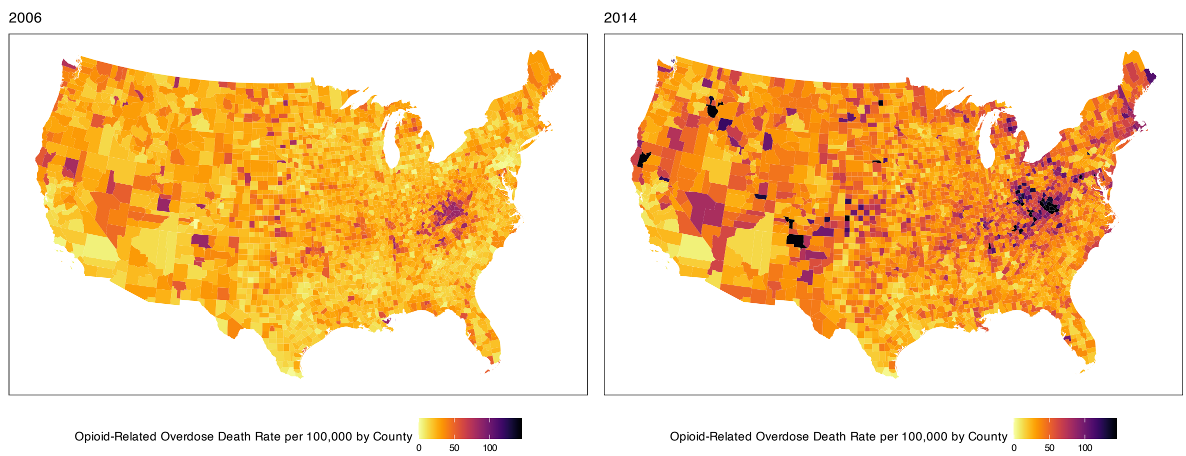


Figure A.7: Variation in the levels and growth of opioid-related death rates by county

Notes: 2628 of the 3108 contiguous counties experience increases in their overdose rate from 2006 to 2014. County rates are calculated by dividing opioid-related overdose deaths from the CDC by population of that year, then scaling by 100,000.

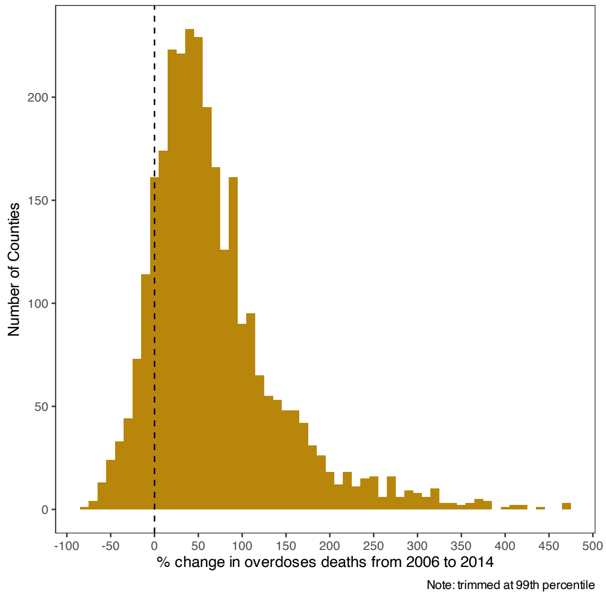


Figure A.8: County-level Variation in Opioid-Related Deaths

Notes: Similar to Griffith et al. (2021), opioid-related deaths were determined using the following ICD-10 codes: T40.0 (Opium), T40.1 (Heroin), T40.2 (Other opioids), T40.3 (Methadone), T40.4 (Other synthetic narcotics), T40.6 (Other and unspecified narcotics), X40-X44 (Accidental poisoning), X60-64 (Intentional self-poisoning), Y10-Y14 (Poisoning) by non-opioid analgesics, antipyretics and antirheumatics; antiepileptic, sedative-hypnotic, antiparkinsonism and psychotropic drugs, not elsewhere classified; narcotics and psychodysleptics [hallucinogens], not elsewhere classified; other drugs acting on the autonomic nervous system; other and unspecified drugs, medicaments and biological substances. I also include ICD-10 code X85 (Assault by drugs, medicaments and biological substances).

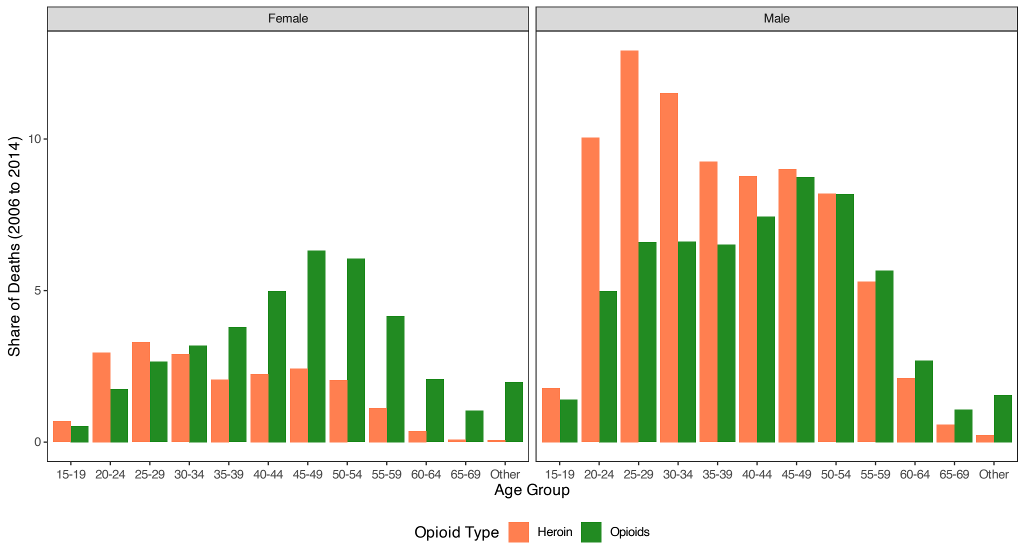


Figure A.9: Differences in age profiles of overdose deaths by opioid type and gender

Table A.1: Counties with a Pharmacy Crackdown versus a Doctor Crackdown (2006 to 2014)

	Pharmacy (N=261)		Practitioner (N=837)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
% Medicare	14.2	4.0	16.3	4.1	2.1	0.3
% Black	16.5	16.6	12.4	13.6	-4.1	1.1
Unemployment rate	6.8	2.2	7.6	3.1	0.9	0.2
Doctors per 100K	394.0	267.2	291.7	191.4	-102.3	17.8
Specialists per 100K	127.3	101.0	89.8	67.5	-37.5	6.7
Opioid overdoses	179.4	176.7	162.5	243.3	-17.0	13.8
% Manufacturing	9.6	4.8	10.3	5.5	0.7	0.4
Population density	17.6	25.1	10.7	19.3	-6.9	1.7
Population in 10,000	66.0	58.9	71.5	128.7	5.4	5.8
Pills per capita	37.5	13.4	43.7	20.8	6.2	1.1
Pharmacies	189.3	170.3	205.6	353.9	16.3	16.1

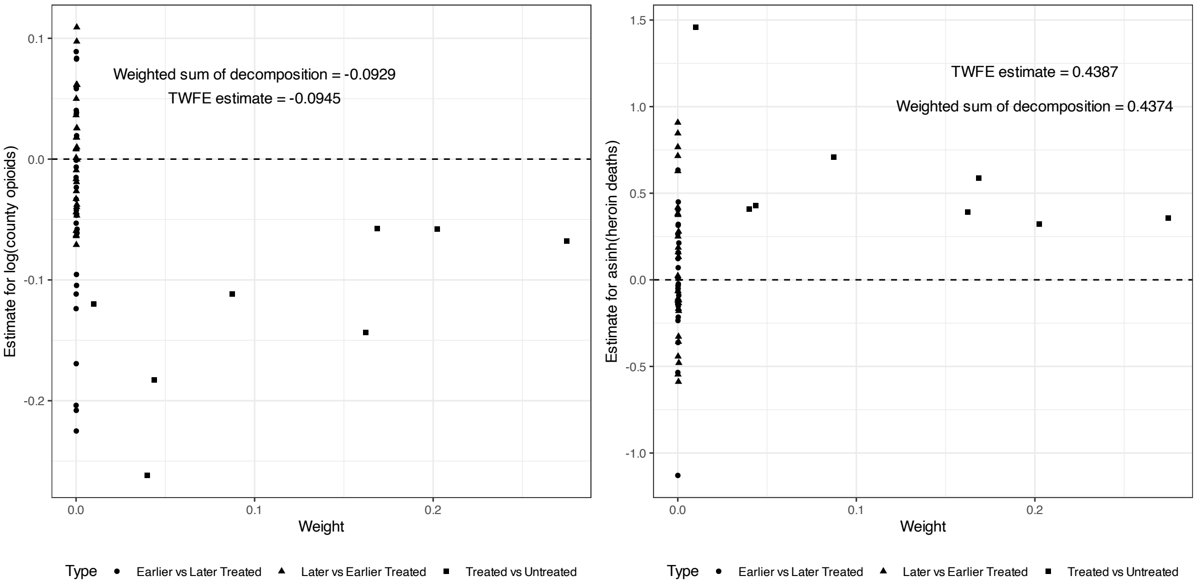
Table A.2: Counties with DEA Crackdown versus those Without (2006 to 2014)

	DEA Action (N=1017)		No Action (N=25766)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
% Medicare	15.9	4.2	19.2	4.6	3.4	0.1
% Black	13.4	14.8	8.1	13.9	-5.3	0.5
Unemployment rate	7.5	3.0	7.1	3.0	-0.4	0.1
Doctors per 100K	316.5	221.4	144.3	167.5	-172.2	7.0
Specialists per 100K	98.8	80.2	34.3	56.4	-64.6	2.5
Opioid overdoses	168.5	236.3	20.8	48.2	-147.7	7.4
% Manufacturing	10.3	5.4	12.7	7.0	2.4	0.2
Population density	12.4	21.8	2.3	17.7	-10.1	0.7
Population in 10,000	69.5	119.2	7.9	19.6	-61.6	3.7
Pills per capita	42.2	20.0	39.5	24.4	-2.6	0.6
Pharmacies	197.0	326.8	24.2	65.7	-172.9	10.3

Table A.3: Counties with a Crackdown versus Above Average Suspicious (2006 to 2014)

	Above Average (N=5310)		DEA Action (N=1017)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
% Medicare	17.9	4.4	15.9	4.2	-2.1	0.1
% Black	10.4	14.8	13.4	14.8	3.0	0.5
Unemployment rate	7.2	2.9	7.5	3.0	0.3	0.1
Doctors per 100K	193.1	194.7	316.5	221.4	123.3	7.4
Specialists per 100K	52.6	66.4	98.8	80.2	46.2	2.7
Opioid overdoses	37.2	73.6	168.5	236.3	131.3	7.5
% Manufacturing	12.6	6.5	10.3	5.4	-2.3	0.2
Population density	6.1	36.0	12.4	21.8	6.3	0.8
Population in 10,000	15.8	31.9	69.5	119.2	53.7	3.8
Pills per capita	40.8	24.1	42.2	20.0	1.3	0.7

Panel A: Doctor Crackdowns



Panel B: Pharmacy Crackdowns

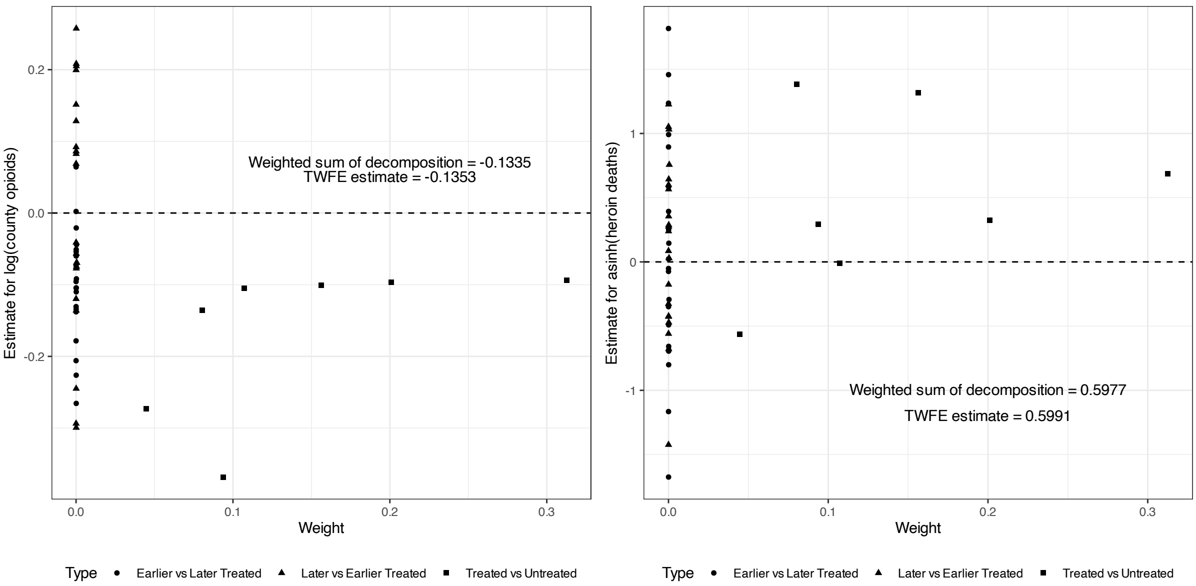


Figure A.10: Bacon Decomposition for the Impacts of Crackdowns

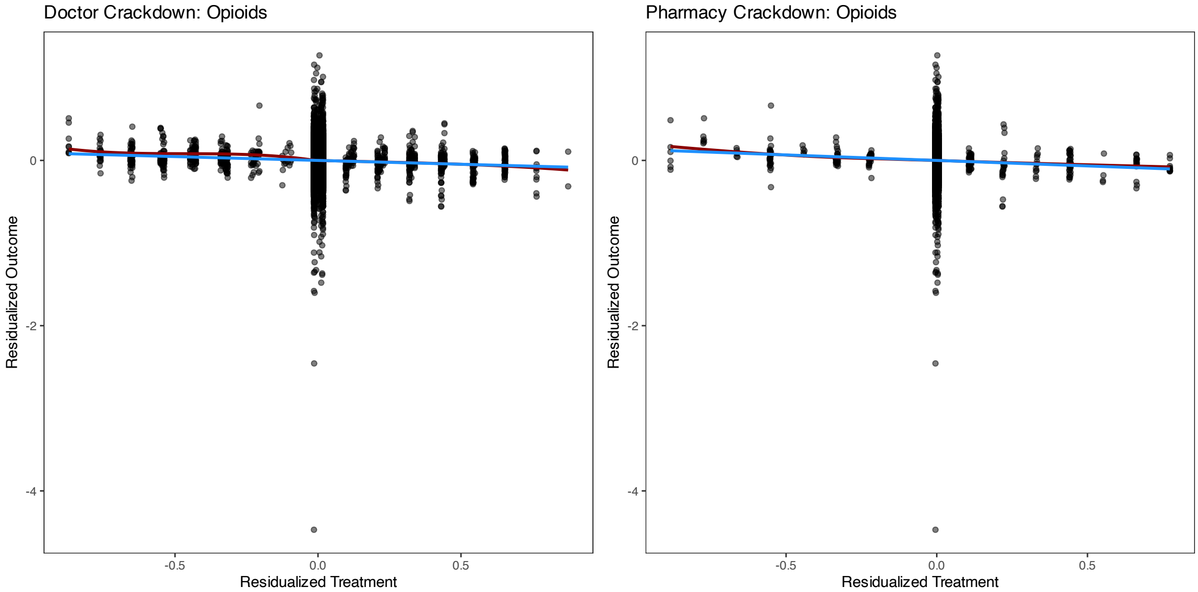


Figure A.11: Test of homogeneity of treatment effects from Jakiela (2021)

Notes: These figures plot the relationship between the residuals from a regression of the log of opioids dispensed on county and year fixed effects and the residuals from a regression of treatment on county and year fixed effects. If treatment effects are homogeneous, the relationship is linear. The line of best fit (loess) is in red and the best linear fit is in blue. The left panel is for doctor crackdowns and the right is for pharmacy ones, where the outcome is the log of county opioids dispensed.

Table A.4: Testing of implication of identifying assumption at the county-level

	Early DEA Action (Dummy Variable)	
	Doctors	Pharmacies
	(1)	(2)
log(opioids in previous year)	−0.105	−0.133
	(0.131)	(0.243)
log(overdoses in previous year)	0.200	−0.103
	(0.134)	(0.209)
Rural	−0.007	0.039
	(0.056)	(0.132)
Doctors per 100,000	0.059	0.102
	(0.070)	(0.206)
Population Density	0.034	−0.016
	(0.060)	(0.198)
Percent Manufacturing	0.068	−0.155
	(0.060)	(0.131)
Mental health clinics per 100,000	0.002	0.075
	(0.058)	(0.127)
log(median income)	0.023	−0.038
	(0.063)	(0.116)
Joint F-stat	1.168	1.266
Observations	93	29
R^2	0.100	0.336
Note:	All covariates z-score transformed	

Table A.5: Testing implication of identifying assumption at the city-level

	Date of DEA action	
	Doctors	Pharmacies
	(1)	(2)
log(city opioids)	0.054	0.121
	(0.089)	(0.124)
Observations	1,120	346
R^2	0.941	0.967
Notes:	*p<0.1; **p<0.05; ***p<0.01	
	Year FEs included in both specifications	
	Sample limited to year before treatment	
	Outcome is continuous variable (0 to 108)	

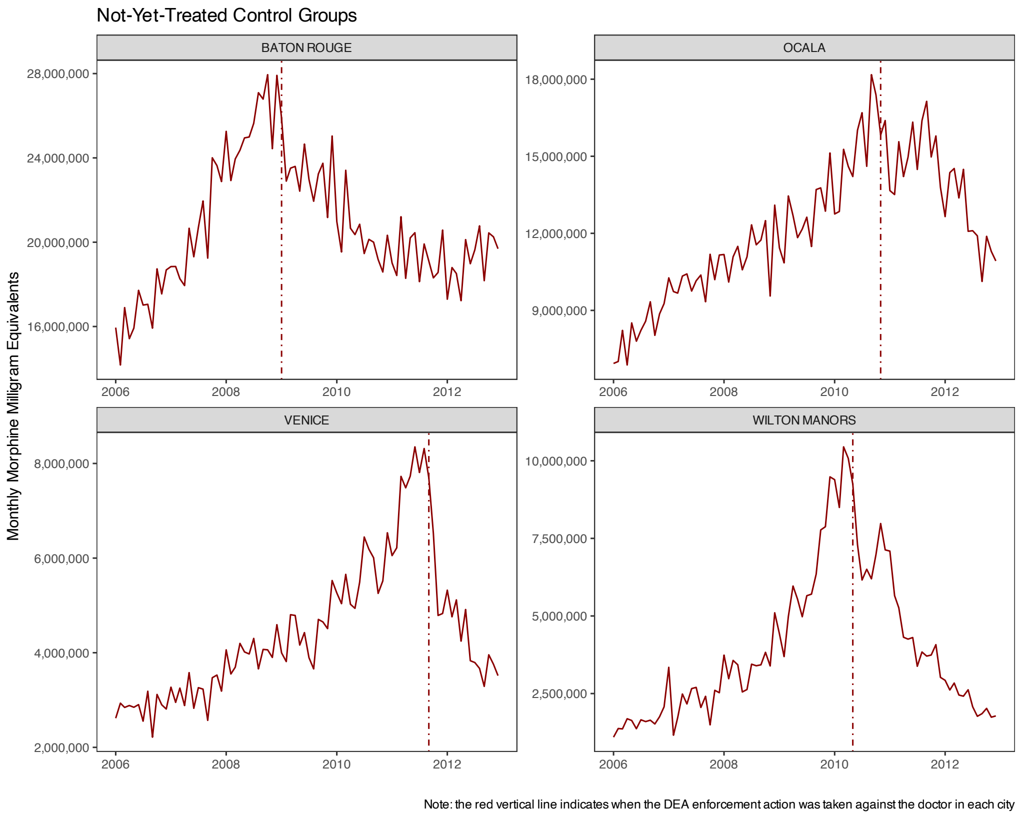


Figure A.12: Examples of City-Level Impacts of Doctor Crackdowns

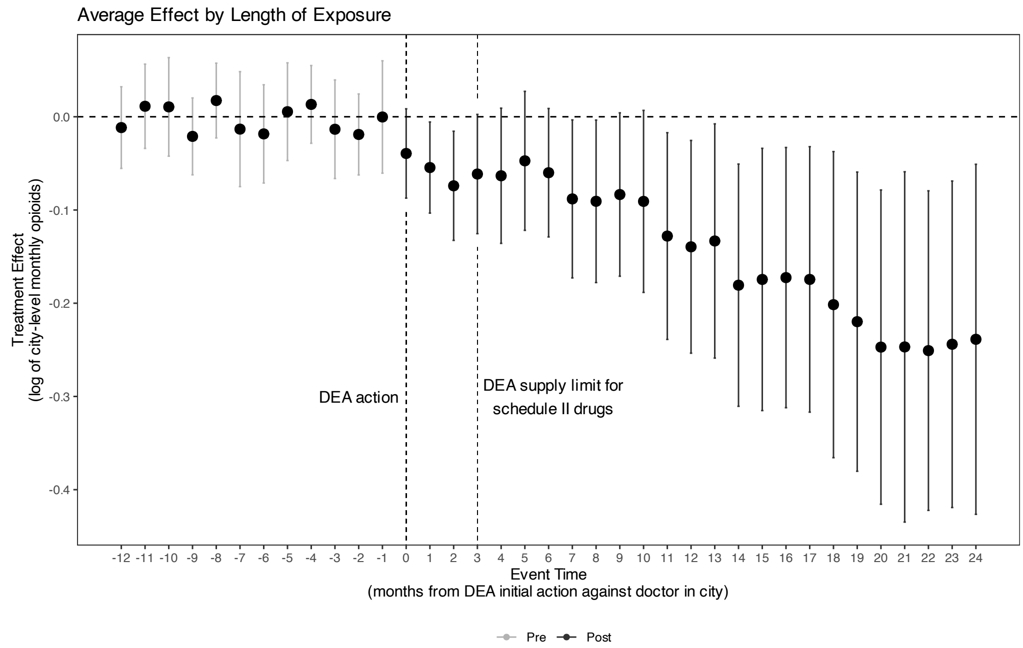


Figure A.13: City-Level Impact of a DEA Action against a Doctor

Notes: The outcome is the logarithm of monthly city-level opioids, where the treatment is the DEA action against taken against a doctor. The sample is limited to cities that had a doctor with a DEA registration revoked, and the control group are the not yet treated cities. Figure 5 instead uses never treated cities. Standard errors are clustered at the city-level and computed using a multiplier bootstrap, and the estimation method is doubly robust from Callaway and Sant'Anna (2021). The first dotted line captures the treatment date, while the second one captures the 90-day supply limit that the DEA places on any controlled substance prescription. Any prescriptions after the 90-day limit should not be attributable to the sanctioned doctor.

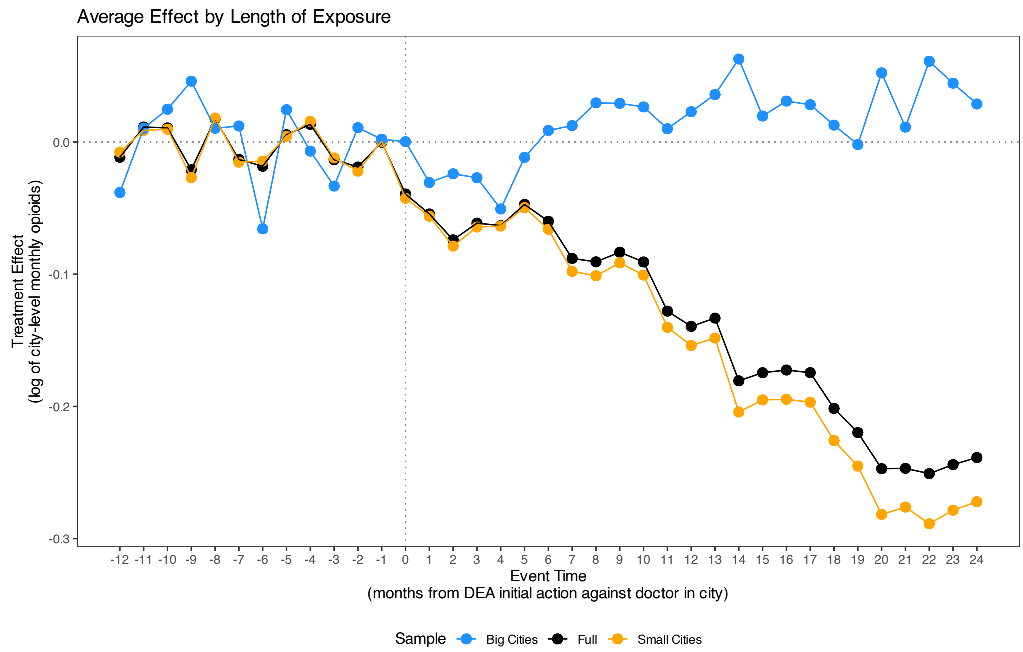


Figure A.14: City-Level Impact of DEA Action against a Doctor

Notes: The outcome is the logarithm of monthly city-level opioids, where the treatment is the DEA action against taken against doctor. The control group consists of the not yet treated cities. Large cities are those in the top 25% of the distribution of city population, but I vary this threshold and the results are not sensitive. I remove standard errors to highlight the trends, given the relatively small sample size in each group, but standard errors are clustered at the city-level and computed using a multiplier bootstrap; the estimation method is doubly robust.

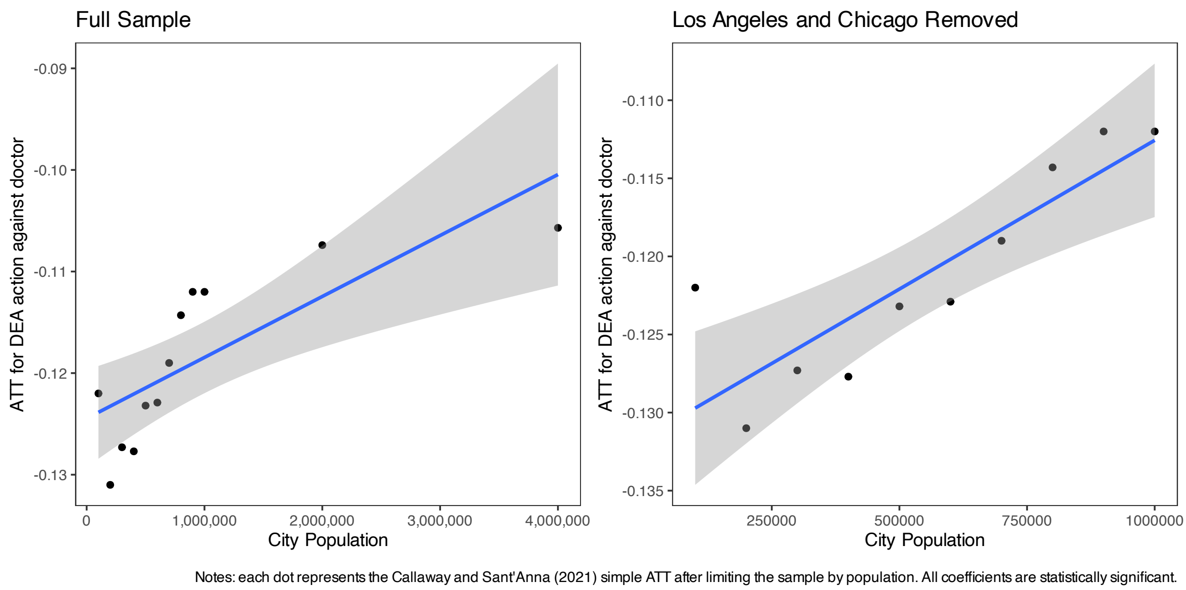


Figure A.15: Decline in Dispensing after Doctor Crackdown is Greater for Smaller Cities

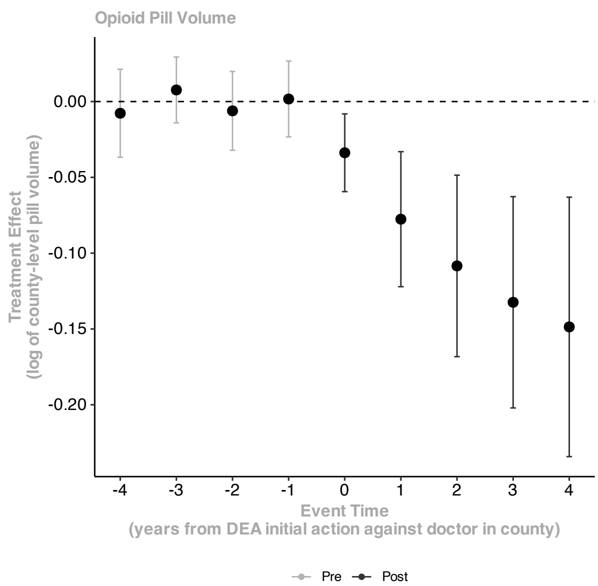


Figure A.16: The Impact of Doctor Crackdowns on County-Year Opioid Dispensing

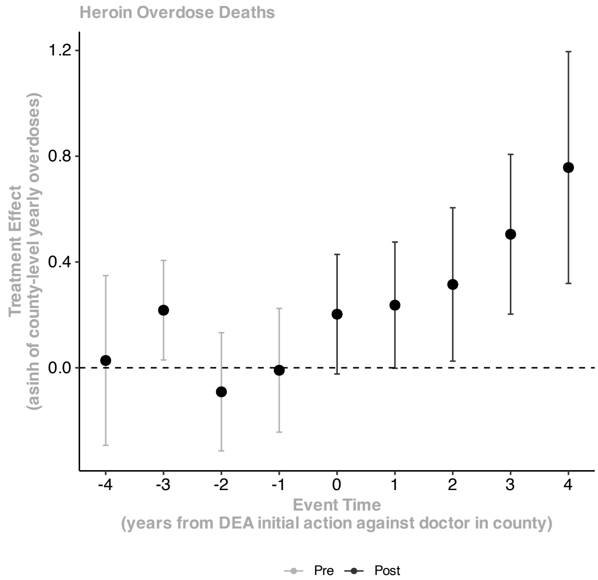


Figure A.17: The Impact of Doctor Crackdowns on Overdose Deaths

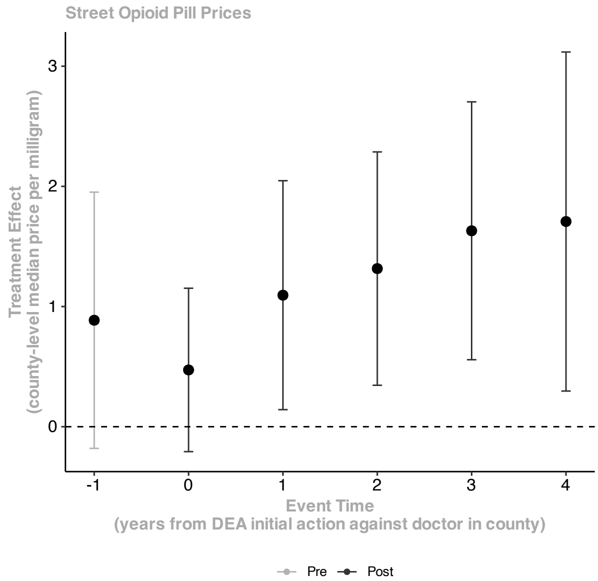


Figure A.18: The Impact of Doctor Crackdowns on Prices

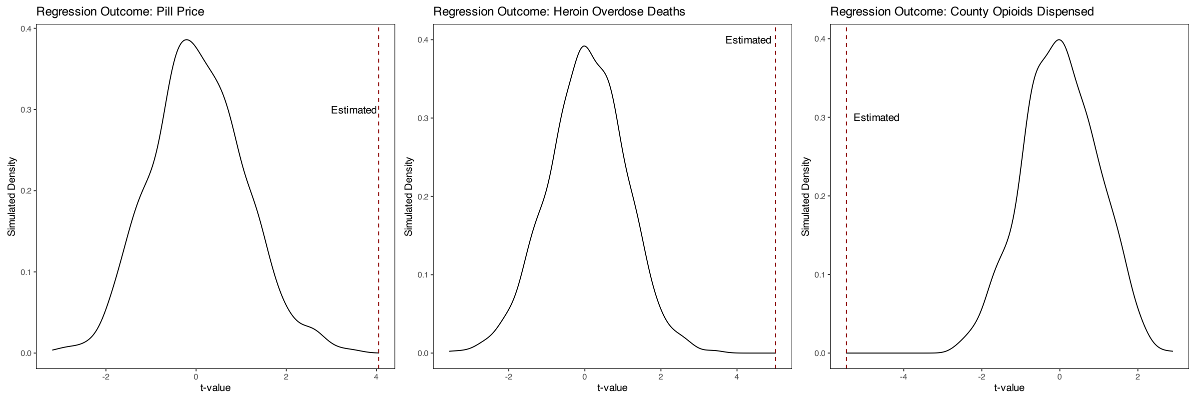


Figure A.19: Permutation Test for Impacts of Doctor Crackdowns (t-statistics)

Notes: I first randomize which unit gets treated, then conditional on receiving treatment, I randomize what year they are treated. I run equation (1) 1000 times using this new sample, where the figures are the distribution of the t-statistics obtained from these regressions. Randomizing treatment dates across only treated units produces similar results.

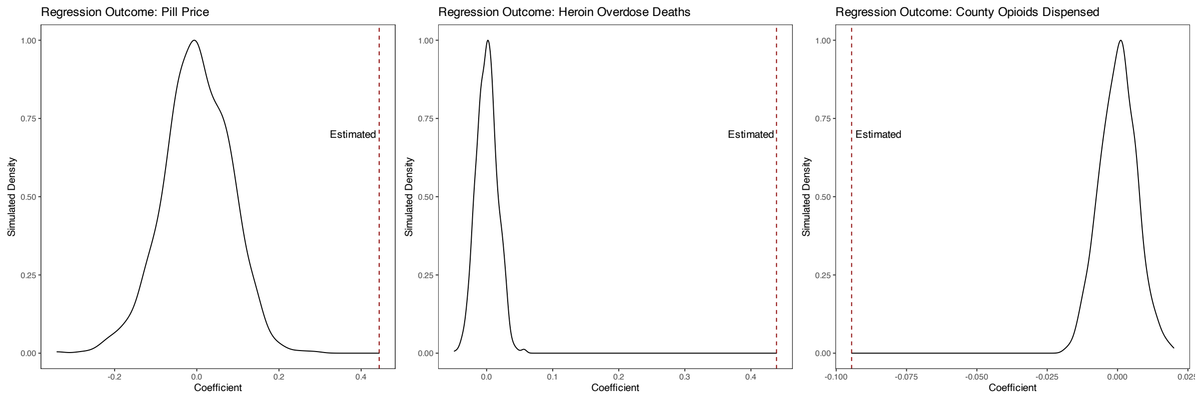


Figure A.20: Permutation Test for Impacts of Doctor Crackdowns (coefficients)

Notes: I first randomize which unit gets treated, then conditional on receiving treatment, I randomize what year they are treated. I run equation (1) 1000 times using this new sample, where the figures are the distribution of the coefficients obtained from these regressions. Randomizing treatment dates across only treated units produces similar results.

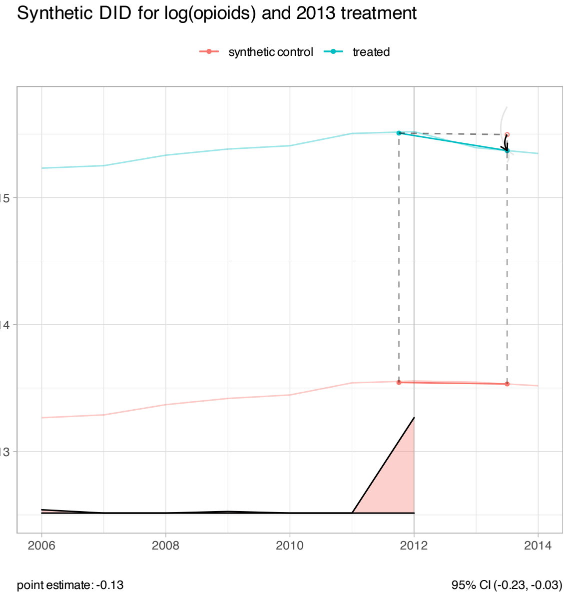
Table A.6: The Impacts of Rogue Doctor Crackdowns on Opioid Overdoses and Dispensing

	Heroin	Opioids	Heroin	Opioids	Heroin	Opioids
	Ever Suspicious		Above Mean Suspicious		Above 90th per Capita	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-DEA Action	0.432***	−0.085***	0.291**	−0.085***	0.290	−0.157***
	(0.093)	(0.018)	(0.127)	(0.023)	(0.234)	(0.047)
Control Mean	11.47	242.073	11.398	255.192	5.36	185.296
Observations	17,994	17,994	5,811	5,811	3,286	3,286
R^2	0.808	0.993	0.874	0.997	0.847	0.993

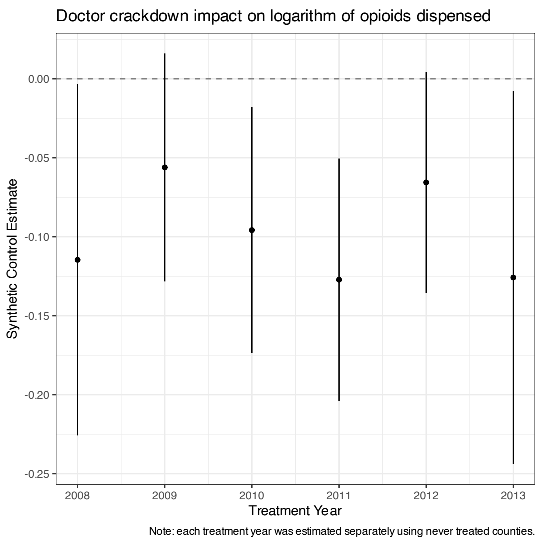
Notes: *p<0.1; **p<0.05; ***p<0.01
Outcomes are county-year and log transformed
County and year fixed effects included in all specifications
Standard errors are clustered at the county level

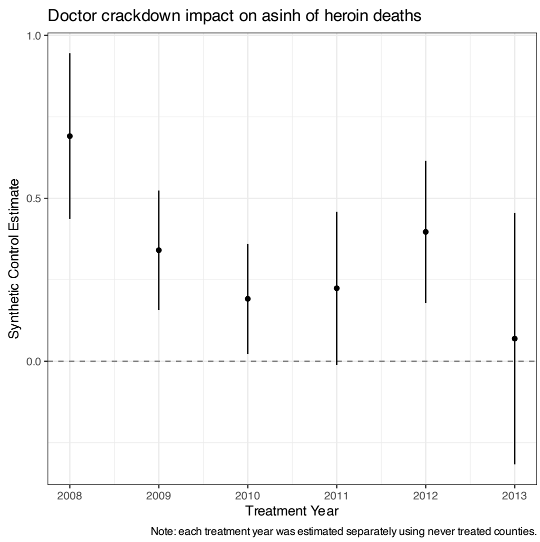
Figure A.21: Synthetic DID Method for Impacts of Doctor Crackdowns

Panel A: Parallel Trends and Estimated Effect for 2013 Treatment (Example)



Panel B: The Impacts of Crackdowns on Opioid Street Prices, Overdoses, and Dispensing





Notes: Panel A shows an example of what parallel trends look like, as I estimate the synthetic control DID method separately for each treatment year. I do this because the method is for a single treatment date and it is unclear how to aggregate/ weight the coefficients from different treatment years. Standard errors were calculated using the placebo method suggested in Arkhangelsky et al. (2021). Panel B shows the impacts of crackdowns for each treatment year starting in 2008.

Table A.7: The Impacts of Rogue Doctor Crackdowns on Opioid Dispensing and Overdoses

Parameter	Event	County Opioids	Heroin Deaths
Synthetic DID Estimate	Post-DEA Action	-0.0975	0.319
		(0.0419)	(0.1317)

Estimation Method: Arkhangelsky et al. (2021)

Table A.8: Impacts of Rogue Doctor Crackdowns on All Cause Mortality by Age and Gender

	Males (15-29)	Males (30-49)	Females (15-29)	Females (30-49)	All (10-65)	Elderly (85+)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-DEA Action	-1.210^{***}	-4.130^{***}	-0.309^{**}	-1.557^{***}	-6.023^{***}	-0.009
	(0.292)	(0.625)	(0.145)	(0.429)	(2.142)	(0.028)
Control Mean	4.906	14.162	1.88	8.715	81.098	86.651
Observations	26,765	26,765	26,765	26,765	26,765	26,765
R^2	0.584	0.753	0.436	0.706	0.875	0.910

Notes: *p<0.1; **p<0.05; ***p<0.01
County and year fixed effects included in all specifications
Standard errors are clustered at the county level
Outcomes are in rates per 100,000

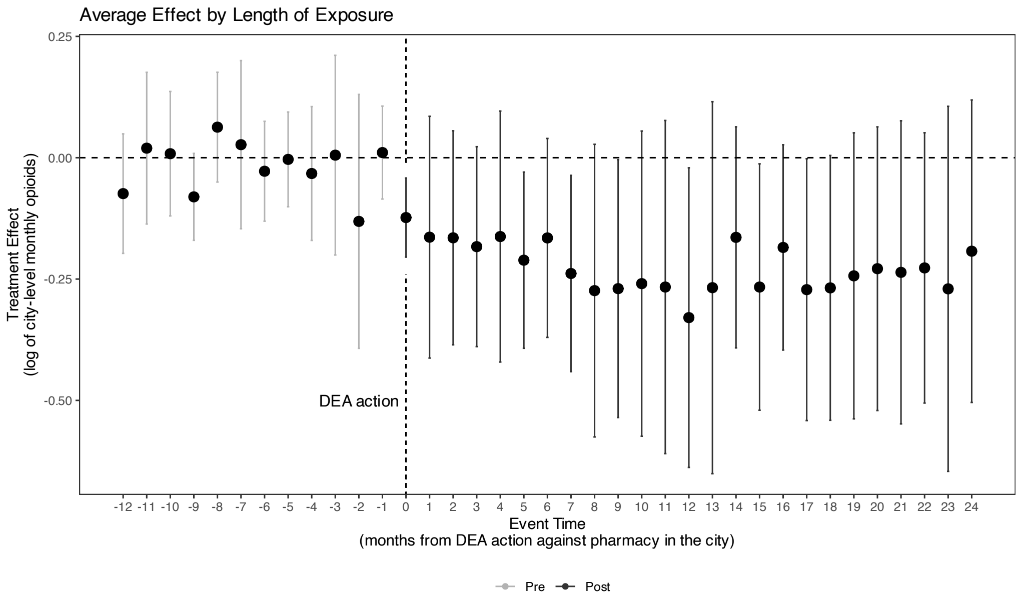


Figure A.22: City-Level Impacts of DEA Action against a Pharmacy

Notes: The outcome is the logarithm of monthly city-level opioids, where the treatment is the DEA action against taken against pharmacy. The sample is limited to cities that had a pharmacy with a DEA registration revoked, and the control group are the not yet treated cities. Standard errors are clustered at the city-level and computed using a multiplier bootstrap, and the estimation method is doubly robust from Callaway and Sant'Anna (2021).

Table A.9: Impacts of Rogue Pharmacy Crackdowns on Dispensing and Mortality

	Opioids Dispensed	Heroin Deaths	Opioid Deaths
	(1)	(2)	(3)
Post-DEA Action	-0.135^{***}	0.599^{***}	-0.039
	(0.034)	(0.168)	(0.048)
Control Mean	3.544	1.519	12.32
Observations	26,765	26,765	26,765
R^2	0.990	0.799	0.888

Notes: *p<0.1; **p<0.05; ***p<0.01
County and year FEs included in all specifications
Standard errors are clustered at the county level
Outcomes are county-year and log transformed

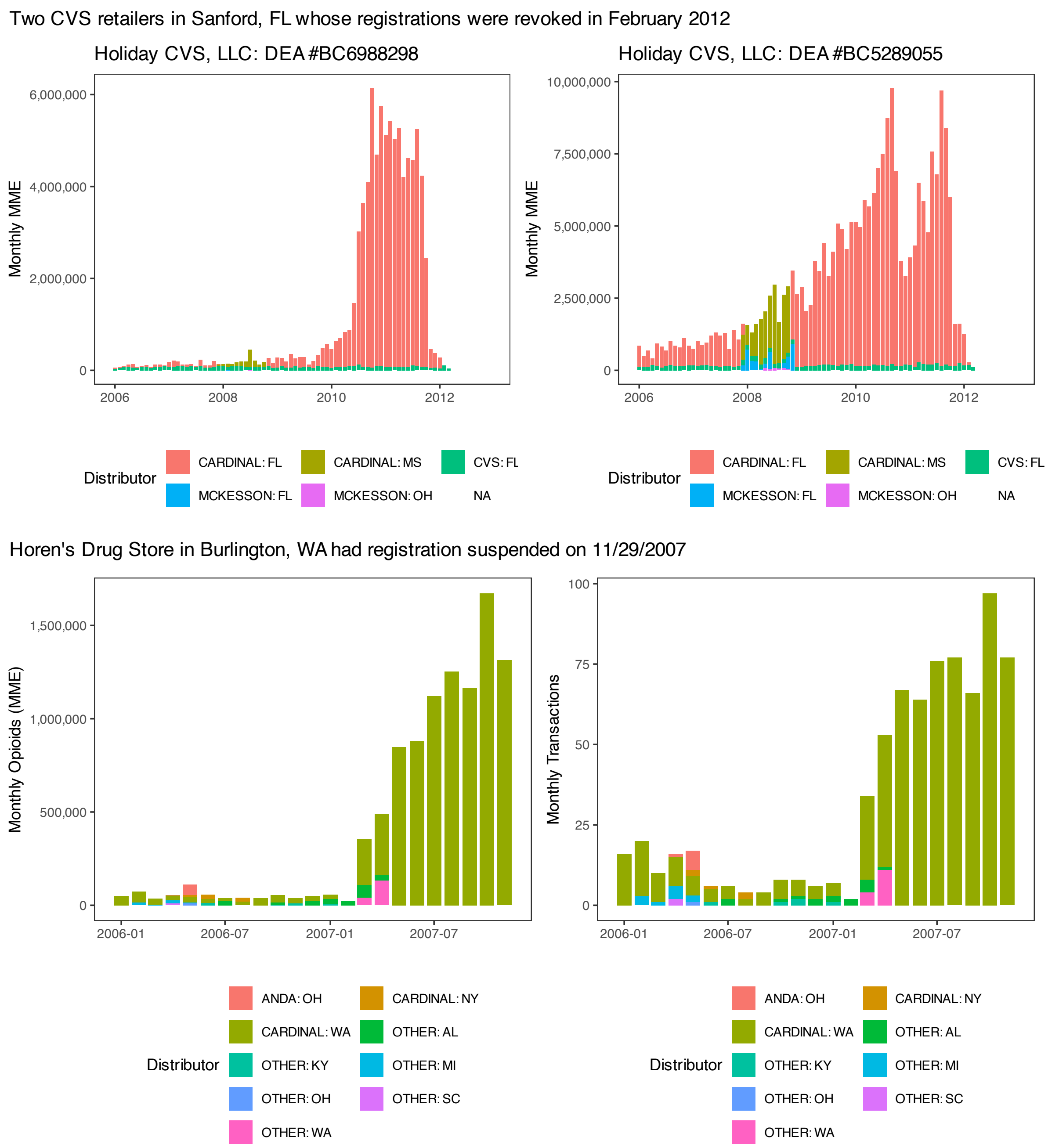


Figure A.23: Examples of Sanctioned Pharmacies

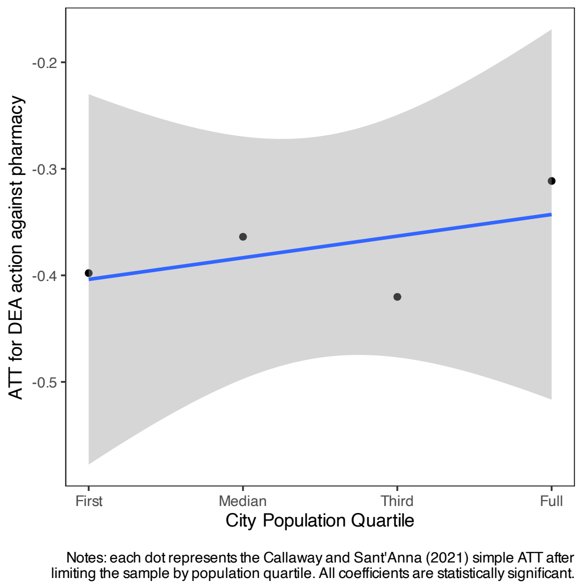


Figure A.24: Decline in Dispensing after Doctor Crackdown is Greater for Smaller Cities

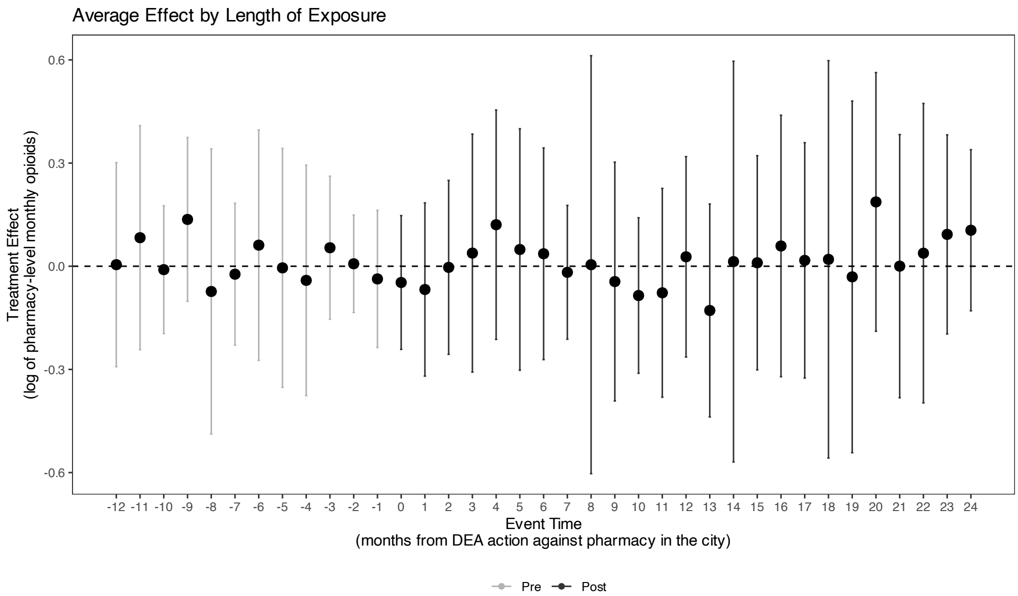


Figure A.25: Pharmacy-Level Impacts of DEA Action against a Pharmacy

Notes: The outcome is the logarithm of monthly pharmacy-level opioids, where the treatment is the DEA action against taken against pharmacy. The sample is limited to pharmacies in cities that had a pharmacy with a DEA registration revoked, and I remove the pharmacies that were sanctioned. The control group consists of the not yet treated pharmacies. Standard errors are clustered at the city-level and computed using a multiplier bootstrap, and the estimation method is doubly robust.

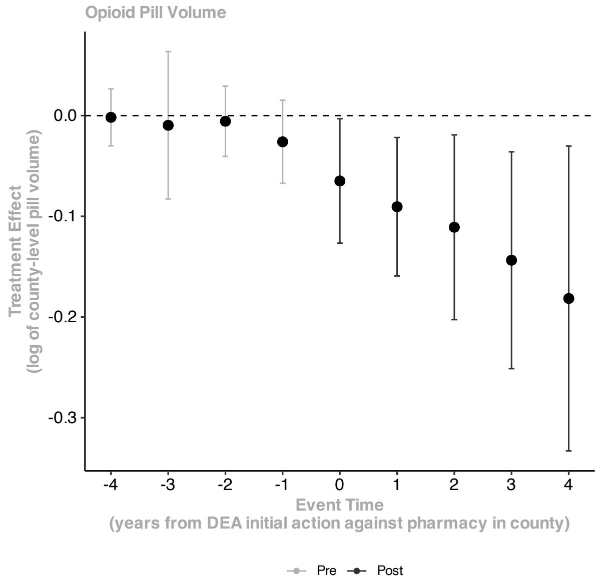


Figure A.26: The Impact of Pharmacy Crackdowns on County-Year Dispensing

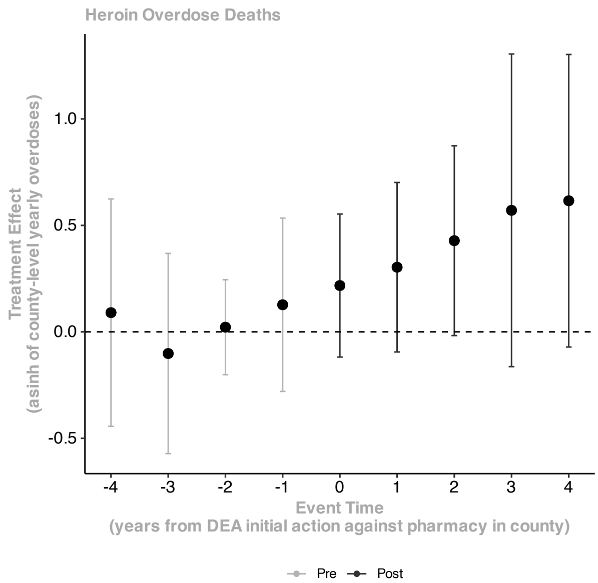


Figure A.27: The Impact of Pharmacy Crackdowns on Overdose Deaths

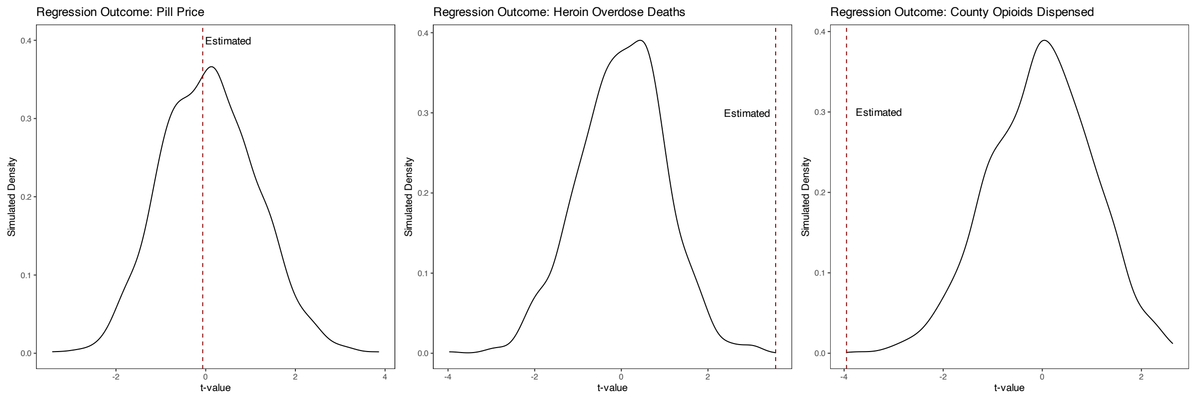


Figure A.28: Permutation Test for Impacts of Pharmacy Crackdowns (t-statistics)

Notes: I first randomize which county gets treated, then I randomize what date they are treated. I then rerun equation (1) 1000 times, where these figures are the distribution of the t-statistics obtained from these regressions. Randomizing treatment dates across only treated units produces similar results.

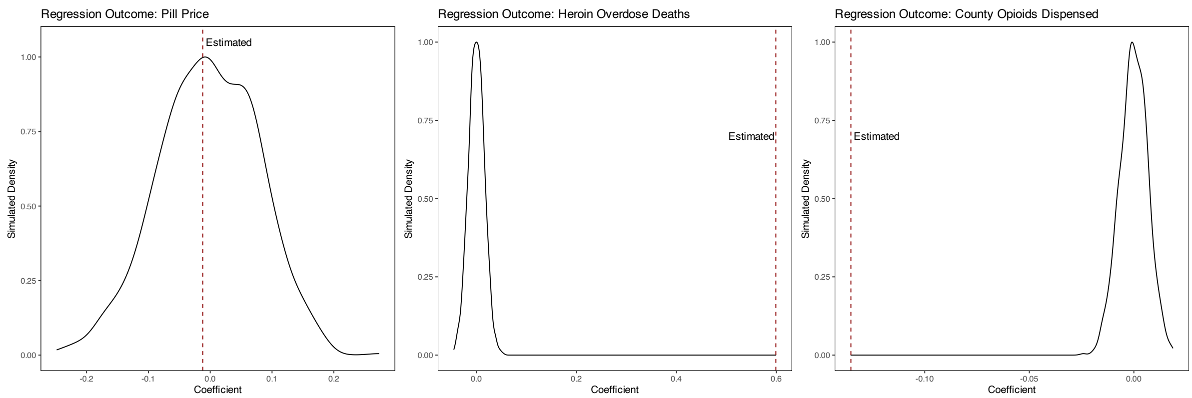


Figure A.29: Permutation Test for Impacts of Pharmacy Crackdowns (coefficients)

Notes: I first randomize which unit gets treated, then I randomize what date they are treated. I then rerun equation (1) 1000 times, where these figures are the distribution of the coefficients obtained from these regressions. Randomizing treatment dates across only treated units produces similar results.

Table A.10: Impacts of Rogue Pharmacy Crackdowns on All Cause Mortality by Age and Gender

	Males (15-29)	Males (30-49)	Females (15-29)	Females (30-49)	All (10-65)
	(1)	(2)	(3)	(4)	(5)
Post-DEA Action	-1.892^{***}	-4.225^{***}	-0.101	-1.854^{***}	-7.936^{*}
	(0.658)	(1.324)	(0.174)	(0.603)	(4.127)
Control Mean	5.019	14.321	1.902	8.818	82.26
Observations	26,765	26,765	26,765	26,765	26,765
R^2	0.584	0.753	0.436	0.706	0.875

Notes: *p<0.1; **p<0.05; ***p<0.01
County and year fixed effects included in all specifications
Standard errors are clustered at the county level
Outcomes are in rates per 100,000

Table A.11: The Impacts of Rogue Pharmacy Crackdowns on Opioid Overdoses and Dispensing

	Heroin	Opioids	Heroin	Opioids	Heroin	Opioids
	Ever Suspicious		Above Mean Suspicious		Above 90th per Capita	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-DEA Action	0.538***	−0.141***	0.369	−0.108***	0.786	−0.408***
	(0.176)	(0.036)	(0.229)	(0.039)	(0.486)	(0.157)
Treated Mean	19.743	231.521	20.863	239.622	21.811	220.172
Observations	18,413	18,413	5,840	5,840	3,008	3,008
R^2	0.810	0.993	0.872	0.997	0.834	0.991

Notes: *p<0.1; **p<0.05; ***p<0.01
Outcomes are county-year and log transformed
County and year fixed effects included in all specifications
Standard errors are clustered at the county level

Table A.12: The Impacts of Rogue Pharmacy Crackdowns on Opioid Dispensing and Overdoses

Parameter	Event	County Opioids	Heroin Deaths
Synthetic DID Estimate	Post-DEA Action	-0.1724	0.2283
		(0.1027)	(0.2446)

Estimation Method: Arkhangelsky et al. (2021)

Table A.13: Impacts of Rogue Pharmacy Crackdowns on Dispensing, Street Prices, and Mortality

	Opioids Dispensed	Pill Price	Heroin Deaths
	(1)	(2)	(3)
Post-DEA Action	−0.053	−0.348	0.337**
	(0.043)	(0.271)	(0.150)
(Post-DEA Action)X(2nd Wave)	−0.101**	0.336*	0.322***
	(0.048)	(0.204)	(0.115)
Control Mean	35.27	1.441	1.499
Observations	26,765	5,057	26,765
R^2	0.990	0.430	0.799

Notes: *p<0.1; **p<0.05; ***p<0.01
County and year FEs included in all specifications
Standard errors are clustered at the county level
Outcomes are county-year and log transformed

Table A.14: The Impacts of Highway Access on Opioid Overdoses (Pharmacy Crackdowns)

	asinh(Heroin Overdose Deaths)	
	(1)	(2)
Post-DEA Action	0.506**	0.825***
	(0.249)	(0.253)
(Post-DEA Action)X(Within 2 miles)	0.215	
	(0.318)	
(Post-DEA Action)X(asinhmiles)		−0.192*
		(0.108)
Observations	26,765	26,765
R^2	0.799	0.800
Notes:	*p<0.1; **p<0.05; ***p<0.01	
	County and year FEs included	
	Standard errors clustered at county level	

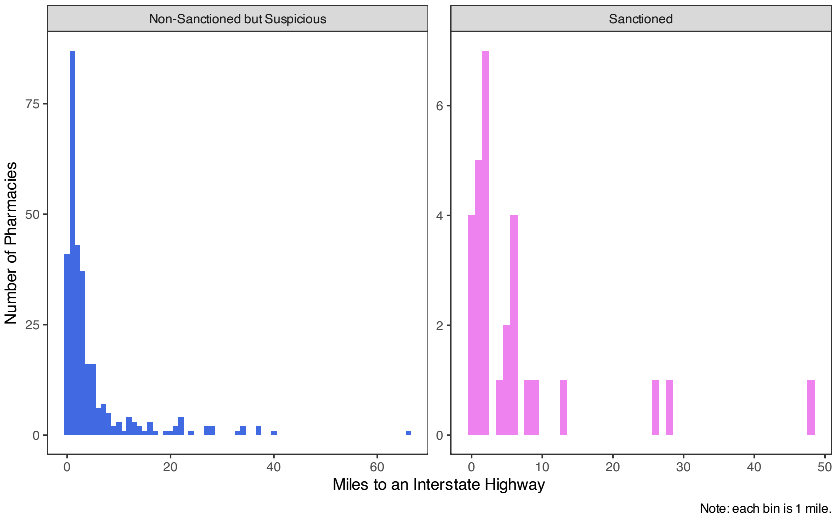


Figure A.30: Distance to the nearest interstate highway

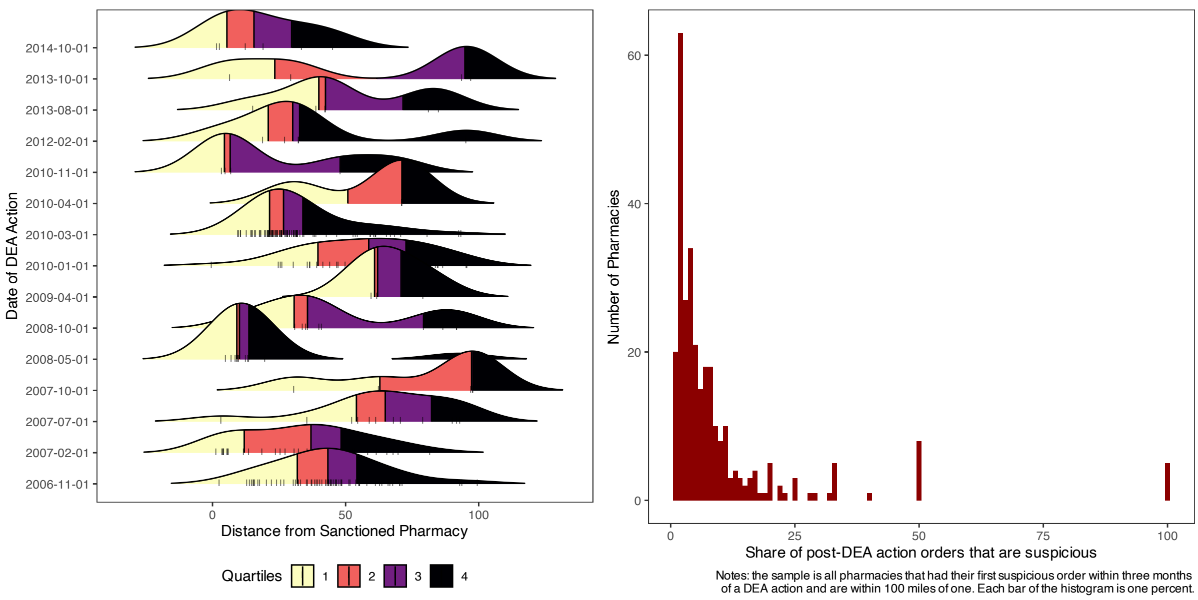


Figure A.31: Distribution of Distances to Sanctioned Pharmacy and Share of Orders that are Suspicious

B Comparison to State- and Federal-Level Interventions

This paper is one of the first to examine the impacts of supply chain actor crackdowns, which generate a targeted or local supply shock. However, there is a large literature examining the impact of more aggregate interventions. These state or federal reforms often affect addicts and rogue supply chain actors, as well as legitimate users and doctors. Researchers primarily focus on prescription drug monitoring programs (PDMPs), pill mill reforms, and the OxyContin reformulation. A PDMP is an electronic database that tracks controlled substance prescriptions in a state and can provide information on prescribing and patient behaviors. All states currently have an operational PDMP, but they were initially optional. Over time, certain states made them must-access; this means that doctors must not only report all controlled substance prescriptions, but also consult the PDMP before writing one. Pill mill laws impose regulations on pain clinics and the OxyContin reformulation introduced an abuse-deterrent form of the blockbuster drug considered to be the main catalyst of the epidemic.

In order to compare the magnitude of my estimates to the literature, I estimate equation (1) but replace the treatment variable of a DEA crackdown with one of the aforementioned interventions. For completeness, I run the regressions separately at the county- and state-level, and the results are in Figure B.1. They show that county- and state-level opioids decrease after all of these interventions, as intended. The supply impacts of doctor and pharmacy crackdowns are generally larger than the those of these policies, but qualitatively similar. Heroin overdose deaths generally increase as well, but at the county-level, they are nowhere near as large as the estimates from the doctor and pharmacy crackdowns. At the state-level, the ecological fallacy is likely an issue.

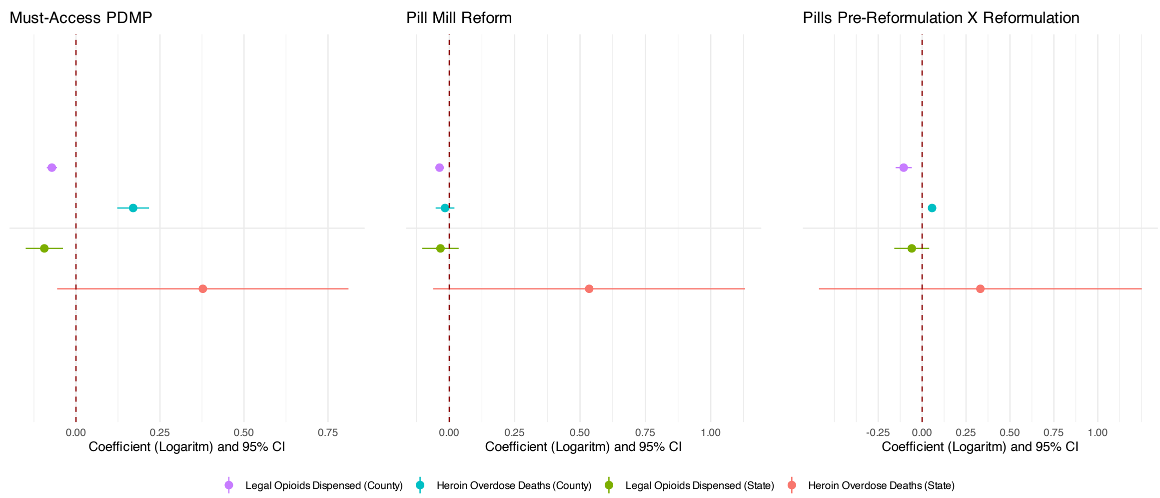


Figure B.1: Impacts of State and Federal Opioid Supply Interventions

Notes: the "County" models included county and year fixed effects, while the "State" models include state and year fixed effects. The former are clustered at the county-level, while the latter are at the state-level.