Three Essays in Applied Economics

Thesis by

Shiyu Zhang

In Partial Fulfillment of the Requirements for the
Degree of
Doctor of Philosophy

Caltech

CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

2022
Defended August 5, 2021

Shiyu Zhang
ORCID: 0000-0002-7517-9717

ACKNOWLEDGEMENTS

First, I want to express my deepest gratitude toward my advisor, Jean-Laurent Rosenthal. I have grown as a researcher and as a person through our weekly and sometimes bi-weekly meetings. This thesis would not have been possible without his support.

Second, I want to thank my committee members and many other Caltech faculties that helped me along the way. Thank you, Mike Ewens, Bob Sherman, Alex Hirsch, Matt Shum, Mike Gibilisco, and Yi Xin for all the insightful discussions and draft editing. I am especially indebted to Mike Alvarez for introducing me to other interesting projects.

Third, I am thankful for the unconditional support my friends and family have bestowed upon me. The Ph.D. journey was not an easy one. I wouldn't be where I am today without their generous support.

ABSTRACT

This thesis consists of three papers, two studying the effectiveness of policy interventions curbing the opioid crisis, and one studying the value of network ties in the Chinese bureaucracy. The two chapters on the opioid crisis are coauthored with Daniel Guth, a fellow Caltech graduate student.

The first chapter studies the effectiveness of the OxyContin reformulation in reducing opioid misuse and overdose. Purdue Pharma reformulated OxyContin in 2010 to make it more difficult to abuse. Previous research argued that OxyContin misuse fell dramatically and OxyContin users switched directly to heroin. Using a novel and fine-grained source of all oxycodone sales from 2006-2014, we show that the reformulation led users to substitute from OxyContin to generic oxycodone and the reformulation had no overall impact on opioid or heroin mortality. In addition, the chapter finds that generic oxycodone, instead of OxyContin, was the driving factor in the transition to heroin in recent years. These findings highlight the important role generic oxycodone played in the opioid epidemic and the limited effectiveness of a partial supply-side intervention.

The second chapter studies the spatial spillover effect of Prescription Drug Monitoring Programs (PDMPs). PDMPs seek to potentially reduce opioid misuse by restricting the sale of opioids in a state. This chapter examines discontinuities along state borders, where one side may have a PDMP and the other side may not. We find that electronic PDMP implementation, whereby doctors and pharmacists can observe a patient's opioid purchase history, reduces a state's opioid sales but increases opioid sales in neighboring counties on the other side of the state border. We also find systematic differences in opioid sales and mortality between border counties and interior counties. These differences decrease when neighboring states both have PDMPs, which is consistent with the hypothesis that the differences were caused by cross-border opioid shopping. Our work highlights the importance of understanding the opioid market as connected across counties or states, as we show that states are affected by the opioid policies of their neighbors.

The third chapter examines the value of patronage ties at lower levels of Chinese bureaucracy. A growing literature shows that connection with the right higher-level politicians is beneficial for advancements in the Communist Party of China. In this chapter, I use a self-collected data set to examine the value of patronage ties in the city committees, a previously overlooked but important level of the Chinese government. I present empirical evidence that the party secretaries are involved in the appointment of committee members. But upon departure, the party secretaries' career success does not improve the committee members' future promotion likelihood. This work highlights that the value of interpersonal connection in China is highly dependent on which level of the government is under inspection.

PUBLISHED CONTENT AND CONTRIBUTIONS

Guth, Daniel and Shiyu Zhang. 2021. "Geographic Spillover Effects of Prescription Drug Monitoring Programs (PDMPs)." *arXiv preprint arXiv:2107.04925* . Shiyu Zhang participated in the conception of the project, analyzed the empirical data, and participated in the writing of the manuscript.
URL: *https://arxiv.org/abs/2107.04925*

Zhang, Shiyu and Daniel Guth. 2021. "The OxyContin Reformulation Revisited: New Evidence From Improved Definitions of Markets and Substitutes." *arXiv preprint arXiv:2101.01128* . Shiyu Zhang participated in the conception of the project, was solely responsible for the empirical analyses, and participated in the writing of the manuscript.
URL: *https://arxiv.org/abs/2101.01128*

TABLE OF CONTENTS

LIST OF ILLUSTRATIONS

LIST OF TABLES

Chapter 1

THE OXYCONTIN REFORMULATION REVISITED: NEW EVIDENCE FROM IMPROVED DEFINITIONS OF MARKETS AND SUBSTITUTES

1.1 Introduction

Since 1999, the opioid epidemic has claimed more than 415,000 American lives (CDC Wonder). What started with fewer than 6,000 opioid-related deaths in 1999 grew steadily every year until fatalities reached 47,573 deaths in 2017. Following a small decline in fatal drug overdoses in 2018, death rates continue to rise. Over the past two decades, millions of Americans have misused prescription opioids or progressed to more potent opioids, first heroin and later fentanyl. Many social scientists have tried to understand how this crisis has grown over two decades despite significant public health efforts to the contrary.

Doctors and health economists have long argued that the drug most responsible for prescription opioid overdose deaths, and the key to understanding the transition from prescription opioids to heroin starting in 2010, was OxyContin. Previous research (Van Zee, 2009), court proceedings (Meier, 2007), and books (Macy, 2018; Meier, 2003) has documented how Purdue Pharma's marketing campaign for OxyContin downplayed the risk of addiction starting in 1996. Since then, according to the National Survey on Drug Use and Health (NSDUH), millions of Americans have misused it. A key question in this area is whether or not making prescription opioids, especially OxyContin, more difficult to abuse will reduce overdose deaths.

In this paper, we show that restricting access to OxyContin led many users to switch to generic oxycodone but had no impact on opioid or heroin mortality. Earlier analyses attributing opioid overdose deaths in the late 2000s and the subsequent rise in heroin deaths to OxyContin are incomplete because they omit generic oxycodone. Our analysis shows that the misuse of generic oxycodone was prevalent before the reformulation that restricted OxyContin access, and was even more so afterward. We also show that heroin overdose deaths increased in areas with high generic oxycodone exposure, not high OxyContin exposure, two years after the OxyContin reformulation. In addition, we show that omitting generic oxycodone in our regressions recovers the results of the literature.

This analysis was not possible until one year ago when The Washington Post won a court order and published the complete Automation of Reports and Consolidated Orders System (ARCOS). The ARCOS tracks the manufacturer, the distributor, and the pharmacy of every pain pill sold in the United States. The newly released data allow us to analyze what happened to sales of generic oxycodone and OxyContin when OxyContin suddenly became more difficult to abuse. The previous literature focused on analyzing OxyContin because of Purdue's notorious role in the opioid crisis. However, the new data shows that the sales of OxyContin was only a small part of the sales of all prescription opioids: in terms of the number of pills, OxyContin was 3% of all oxycodone pills sold from 2006 to 2012; in terms of morphine milligram equivalents (MME), OxyContin has closer to 20% market share over this period. The new transaction-level ARCOS data allows us to track the sales of generic oxycodone and fill in the narrative gaps of how the opioid crisis progressed in the United States.

Following Alpert, Powell and Pacula (2018), Evans, Lieber and Power (2019), and Cicero and Ellis (2015), we treat the introduction of an abuse-deterrent formulation (ADF) of OxyContin as an exogenous shock that should only affect people who seek to bypass the extended-release mechanism for a more immediate high. We construct measures of exposure by combining ARCOS sales and the NSDUH data on drug misuse. The NSDUH is the best survey of people who use drugs at the state level, and by combining it with local sales, we can capture any variation in drug use within the state. We leverage this variation in OxyContin and generic oxycodone exposure to examine how the reformulation affected OxyContin sales, generic oxycodone sales, opioid mortality, and heroin mortality. Our first contribution is that we fix the omitted-variable problem by differentiating between OxyContin and generic oxycodone, and we show that this leads to different conclusions than what previous literature suggests. Our second contribution is disaggregating the data to metropolitan statistical area (MSA), which allows us to address endogeneity at the state level.

To preview our results, we find strong evidence of substitution from OxyContin to generic oxycodone immediately after the reformulation. This substitution was larger in places that had more OxyContin misuse pre-reform, which is consistent with our hypothesis that users would switch between oxycodones rather than move on to heroin. Because this substitution should be concentrated among people misusing OxyContin, the results imply large changes in consumption at the individual level.

Back-of-the-envelope calculation suggests that 68% of the decline in OxyContin sales was substituted to oxycodone in MSAs with high OxyContin misuse. The findings are consonant with surveys like Havens et al. (2014), Coplan et al. (2013), and Cassidy et al. (2014) who all document substitution to generic oxycodone after the reformulation by people seeking to bypass the ADF. We also find suggestive evidence of substitution from generic oxycodone to OxyContin after the reformulation in places where generic oxycodone misuse was high, a channel that has been unexplored in previous research.

Our event study approach also shows that generic oxycodone exposure is predictive of future heroin overdose deaths whereas OxyContin exposure is not. The results are not contingent on methodology or our construction of exposure measures. Crucially, if we run the same exact regressions at the state or MSA level and omit generic oxycodone, we recover the results of the literature where OxyContin misuse appears to be significantly predictive of future heroin overdose deaths. We find that every standard deviation increase in generic oxycodone exposure pre-reformulation is associated with a 40.8% increase in heroin mortality in 2012 from the 2009 baseline level. As further evidence against the argument that there was immediate substitution from OxyContin to heroin after the reformulation, we note that in all of our regressions, the increase in heroin deaths was not statistically significant until 2012. As suggested in O'Donnell, Gladden and Seth (2017), the rise in heroin deaths can be attributed in part to an increase in the supply of heroin as well as the introduction of fentanyl into heroin doses.

Our findings highlight the pitfalls of omitting important substitutes to OxyContin in analyzing the prescription opioid crisis. Purdue Pharma has received well-deserved attention over the years for its role in igniting the crisis. The company has been involved in many lawsuits over the years, but perhaps the most damaging were lesser-known cases that involved losing its patent in 2004[1] which cleared the way for a rapid increase in generic oxycodone sales in the early 2000s. While Purdue Pharma was being sued and scrutinized, several manufacturers took the opportunity to fill in the gaps of OxyContin. By 2006, generic oxycodone outsold OxyContin by more than 3-to-1 after accounting for pill dosage differences. This paper sheds lights on the role generic oxycodone played and continues to play in the opioid crisis and helps policy makers update their picture of the opioid use disorder (OUD) landscape.

[1] Federal ruling, risk management plan proposals for generic oxycodone

The paper also calls attention to the limited effectiveness of a partial supply-side intervention to curb OUD. Purdue Pharma was once a dominant player in the opioid market, but by the time of the reformulation, that dominance had vanished and it was only one of the many manufacturers whose drugs were actively misused by Americans. Purdue was the first company to include abuse-deterrent formulation (ADF) in their opioids, and it was not until recent years that other brands started adding anti-deterrent compounds to their products (Pergolizzi et al., 2018). When substitutions to other abusable opioids are easy, cutting supplies of one kind is less effective.

The rest of the paper runs as follows. Section 1.2 gives more background on the opioid crisis and explains how previous research has characterized the OxyContin reformulation. In Section 1.3, we describe the new ARCOS sales database, the NSDUH misuse data, the NVSS mortality data, as well as our constructed misuse measure and descriptive statistics. Section 1.4 describes our empirical strategy for testing our hypotheses. Section 1.5 discusses our results and what it means for our understanding of the transition between illicit drugs, and Section 1.6 concludes.

1.2 Background and Literature Review

This section proceeds in chronological order. First, we provide a history of oxycodone and its most important formulation, OxyContin. We then describe the OxyContin reformulation in 2010 and what it meant for prescription opioid misuse, as well as how the previous literature analyzed the reformulation. Next, we present the nascent research on substitution between different opioids and how our contribution fits in this strain of work. We conclude with a summary of the literature on heroin mortality in the early 2010s and its link with the prescription opioid crisis.

Oxycodone was first marketed in the United States as Percodan by DuPont Pharmaceuticals in 1950. It quickly found to be as addictive as morphine (Bloomquist, 1963), and in 1965, California placed it on the triplicate prescription form (Quinn, 1965).[2] Before the 1990s, doctors were hesitant to prescribe oxycodone to non-terminally ill patients due to its high abuse potential (DeWeerdt, 2019). The sales of oxycodone-based pain relievers did not take off until the mass marketing of OxyContin, Purdue's patented oxycodone-based painkiller. OxyContin was first approved by the FDA in 1995. The drug's innovation was an 'extended-release' formula,

[2]Triplicate programs required pharmacists to send a copy to the government, and *Origins of the Opioid Crisis and Its Enduring Impacts* (N.d.) show that these had a persisting effect on reducing the number of opioid prescriptions.

which allowed the company to pack a higher concentration of oxycodone into each OxyContin pill and the patients to take the pills every 12 hours instead of 8 hours. OxyContin's original label, approved by the FDA, stated that the "delayed absorption, as provided by OxyContin tablets, is believed to reduce the abuse liability of a drug." In 2001, the FDA changed OxyContin's label to include stronger warnings about the potential for abuse and Purdue agreed to implement a Risk Management Program to try and reduce OxyContin misuse.[3]

OxyContin was one of the first opioids marketed specifically for non-cancer pain. In the early 1990s, pain started to enter the medical discussion as the 'fifth vital sign' and something to be managed. As described in Meier (2003), Van Zee (2009), and elsewhere, Purdue's sales representatives pushed OxyContin and were told to downplay the risk of addiction. Quinones (2015) describes how Purdue cited a 1980 short letter published in the New England Journal of Medicine describing extremely low rates of opioid addiction among hospital patients undergoing hospital stays, but the company repeatedly implied that this result extended to the general population or to individuals who left the hospital with take-home prescriptions of OxyContin. The short letter was uncritically or incorrectly cited 409 times as evidence that addiction was rare with long-term opioid therapy (Leung et al., 2017). As a result of Purdue's aggressive marketing and downplaying of the drug's abuse potential, OxyContin was a huge financial success and effectively catalyzed the prescription opioid crisis.

In May 2007, Purdue signed a guilty plea for misleading the public about the risk of OxyContin and paid more than $600 million in fines. Less than six months later, the company applied to the FDA for approval of a new reformulated version of OxyContin that included a chemical to make it more difficult to crush and misuse (Rappaport, 2009). Although not completely effective in reducing misuse, it was approved by the FDA and after August 2010, it accounted for all OxyContin sales in the United States. Until 2016, with Mallinrockdt's Xtampza ER, Purdue was the only prescription opioid manufacturer to make abuse-deterrent oxycodone pills. The majority of all oxycodone sold over this time was generic oxycodone that remained abusable.[4]

Most research shows that OxyContin misuse fell following the reformulation. As described in Cicero and Ellis (2015), although some users were able to circumvent

[3] From the FDA Opioid Timeline.

[4] Many other companies attempted to make abuse-deterrent opioid pills at the same time, as shown in Webster (2009), but Purdue was the first to market. Adler and Mallick-Searle (2018) and Pergolizzi et al. (2018) list other opioids with an ADF.

the abuse-deterrent formulation (ADF) to inject or ingest, the reformulation did reduce misuse. Evans, Lieber and Power (2019) find that the reformulation coincided almost exactly with a structural break in aggregate oxycodone sales, which had previously been increasing. Shortly after the OxyContin reformulation was implemented, researchers began to notice illicit drug use moving towards other drugs such as heroin or generic oxycodone (Alpert, Powell and Pacula, 2018; Cassidy et al., 2014; Cicero, Ellis and Surratt, 2012; Coplan et al., 2013; Evans, Lieber and Power, 2019; Havens et al., 2014). Our paper extends the analysis of the impact of reformulation on opioid use by separately identifying the shifts in OxyContin and generic oxycodone misuse.

We build upon a rich literature that studies opioid misuse through surveys or analysis of the aggregated ARCOS reports. Surveys mostly polled either informants or users themselves (for details see Inciardi et al. (2009)). The best surveys have been conducted with users in smaller samples at individual treatment facilities, like in Hays (2004) and Sproule et al. (2009). However, selection bias is a problem for surveying treatment facilities, as that is a specific subset of patients whose habits may be different from the overall drug-using population (particularly because they are seeking treatment). Some researchers have also used the quarterly ARCOS reports to study national trends in consumption, like in Alpert, Powell and Pacula (2018), Mallatt (2018), and Atluri, Sundarshan and Manchikanti (2014). The quaterly ARCOS reports have no information on the market share of each brand of prescription opioids, thereby restricting any analysis to the aggregate level only. Our work is closely connected to the second set of papers, but we are able to leverage ARCOS's transaction level data to distinguish sales of OxyContin from generic oxycodone.

This newly released ARCOS data allows us to make two methodological improvements. First, the literature treats the OxyContin reformulation as an exogenous shock at the state level. This assumption is problematic because each state's dependency on OxyContin as well as exposure to the reformulation is the result of the state's regulatory environment (*Origins of the Opioid Crisis and Its Enduring Impacts*, N.d.). These regulatory factors could have an impact on how people react to the reformulation, and thus create a hidden link between OxyContin exposure and the reformulation outcomes. Using the new ARCOS data, we can disaggregate to Metropolitan Statistical Areas (MSAs), which allows our model to identify drug substitutions using within-state variations in opioid sales and mortality while controlling for across-state variations in policies and drug enforcement.

The second benefit of the new ARCOS data set is that it allows us to disaggregate different kinds of prescription opioid sales on a national scale. Previous national studies were unable to distinguish between these drugs due to limitations in existing data. The NSDUH survey, the primary data source for drug misuse at the national level, only documented past year's use of OxyContin. Death certificates do not distinguish between OxyContin and generic oxycodone. The aggregate ARCOS sales group all oxycodone sales into one bin. Because of OxyContin's unique role in fomenting the opioid epidemic, it has received most of the attention of researchers. The literature assumes that the study of OxyContin was equivalent to the study of all oxycodone. As a result, although non-OxyContin oxycodone misuse is significant in size, it has been understudied. One notable exception is Paulozzi and Ryan (2006), which notes that non-OxyContin oxycodone was a better predictor of state opioid deaths than OxyContin.

The previous literature also attempts to link the misuse of prescription opioids to the rise in heroin misuse. Siegal et al. (2003) are the first to suggest the pathway from prescription opioids to heroin, and they further note a reverse in trend where heroin users switched to prescription opioids when heroin was unavailable. Compton, Jones and Baldwin (2016) describes how, by the 21st century, people who initiated heroin use were very likely to have started by using prescription opioids non-medically. The most recent works on OxyContin reformulation suggest that the reformulation played an important part in reigniting the heroin epidemic since 2010. Cicero and Ellis (2015) and Mars et al. (2014), who rely on smaller surveys, find that the predominant drug people switched to after reformulation was heroin. Evans, Lieber and Power (2019) identifies a structural break in heroin deaths in August 2010 that was accompanied by higher growth in heroin deaths in areas with greater pre-reformulation access to heroin and opioids. Similarly, Alpert, Powell and Pacula (2018) show that the rise in heroin deaths was larger in places with higher OxyContin misuse pre-reformulation. However, the evidence linking the reformulation to the rise in heroin death is not conclusive: other researchers suggest that the sharp rise in heroin use may have predated the OxyContin reformulation by a few years (Cassidy et al., 2014; Dasgupta et al., 2014). With the new ARCOS data, we are able to examine the claim that the OxyContin reformulation caused the subsequent heroin epidemic in more detail. In particular, we separate the impact of the reformulation on heroin use from the gradual shifts in oxycodone misuse that are independent of the reformulation.

1.3 Data and Descriptive Statistics

To estimate the impact of the OxyContin reformulation on opioid use and mortality, we combine several data sources including sales of OxyContin and non-OxyContin alternatives from ARCOS, opioid and heroin mortality from the NVSS, and self-reported OxyContin and Percocet misuse from the NSDUH. Our main regression leverages variations in pre-reform exposure to OxyContin and generic oxycodone to identify the impact of the reformulation on opioid sales and mortality. We define a new measure of exposure by interacting the state-level self-reported opioid misuse and MSA-level opioid sales. In this section, we describe the three sources of data, the market definition, and the construction of the OxyContin and generic oxycodone exposure measure, and we present a summary statistics of our data.

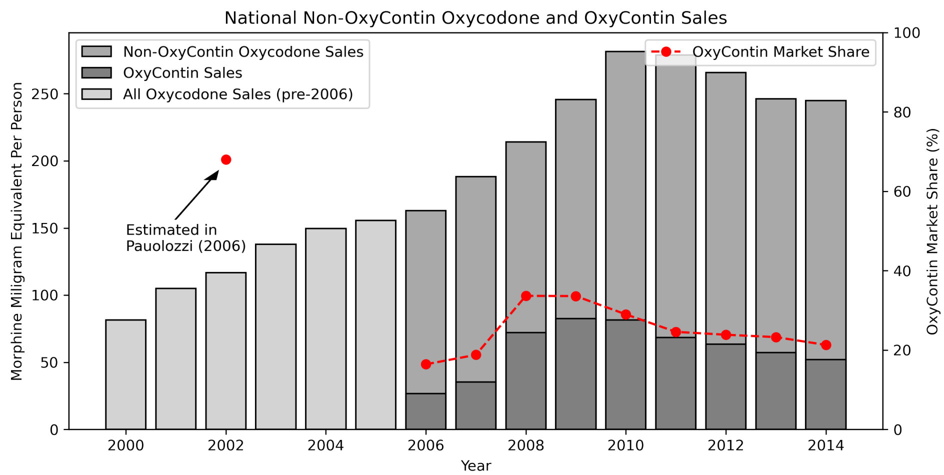
Data

ARCOS and the Sales of Prescription Opioid

As part of the Controlled Substances act, 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 pain pill sold in the United States. The complete database from 2006 to 2014 was recently released by a federal judge as a result of an ongoing trial in Ohio against opioid manufacturers.[5]

The ARCOS database has been used previously to study opioids, but only using the publicly available quarterly aggregated weight of drugs sold (Atluri, Sundarshan and Manchikanti, 2014) or via special request to the DEA (Modarai et al., 2013). The newly released full database reports the manufacturer and the distributor for every pharmacy order. These data allow us to track different brands of prescription opioids separately, and calculate what fraction of oxycodone sold is OxyContin at any level of geographic aggregation. We can thus construct what we believe is the first public time-series of OxyContin and generic oxycodone sales from 2006-2014.

[5] Link to the ARCOS Data published by the Washington Post.



Note: We supplemented the 2006 to 2014 data with publicly available aggregate data from 2000 to 2005. The publicly available aggregate data does not break down the oxycodone sales by manufacturer.

Figure 1.1: Growth of oxycodone and OxyContin sales

As we can see from Figure 1.1, total oxycodone sales increased substantially from 2000 to 2010, with per-person sales nearly quadrupling in the ten-year period. From 2010 to 2015, sales of oxycodone declined slightly as a result of aggressive measures taken by the states and the federal government to counter opioid addiction (Kennedy-Hendricks et al., 2016).

The newly available ARCOS data suggests that the commonly held belief about OxyContin's dominance in the prescription opioid market at the time of reformulation is incorrect. The last time OxyContin's market was estimated was in 2002 by Paulozzi and Ryan (2006), who acquired from the DEA a year's worth of ARCOS data aggregated at the state level. In that year, OxyContin was 68% of all oxycodone sales by active ingredient weight, and scholars have assumed that Purdue's market share stayed high until the OxyContin reformulation. However, as Figure 1.1 shows, by 2006 when our data starts, OxyContin sales only accounted for 18% of all oxycodone sold by weight and never got above 35% during this period. The share is even smaller if we count the number of pills sold, since the average OxyContin active ingredient weight is 5 to 10 times higher than that of oxycodone from other brands. The share of OxyContin decreased dramatically from 2002 to 2006 because Purdue lost the patent rights in 2004. As a result, non-OxyContin oxycodone sales grew much faster in the early 2000s than OxyContin sales. Figure 1.7 in Appendix presents the market share for all oxycodone manufacturers by dosage strength, and Purdue Pharma is only dominant at higher dosages ($\geq$ 40mg). The overestimation of OxyContin's importance in the pre-reform period explains why the previous literature overlooked the role generic oxycodone played in the opioid epidemic.

The ARCOS sales data are the primary variables in our main regressions. We aggregate sales by MSA, year, and brand. To focus on the impact of the reformulation on OxyContin and non-OxyContin alternatives, we group all alternative oxycodone products into one measure, and we will refer to it as generic oxycodone for the rest of the analysis.[6]

NVSS Mortality Data

The second outcome of interest in our main regressions is opioid mortality. We use the restricted-use multiple-cause mortality data from the National Vital Statistics System (NVSS) to track opioid and heroin overdose. The dataset covers all deaths in the United States from 2006-2014. We follow the literature's two-step procedure to identify opioid-related deaths. First, we code deaths with ICD-10 external cause of injury codes: X40–X44 (accidental poisoning), X60–64 (intentional self-poisoning), X85 (assault by drugs), and Y10–Y14 (poisoning) as overdose deaths. Second, we use the drug identification codes, which provide information about the substances found in the body at death, to restrict non-synthetic opioid fatalities to those with ICD-10 code T40.2, and heroin deaths to those with code T40.1. Figure 2 shows the trend over our period of study for the two series.

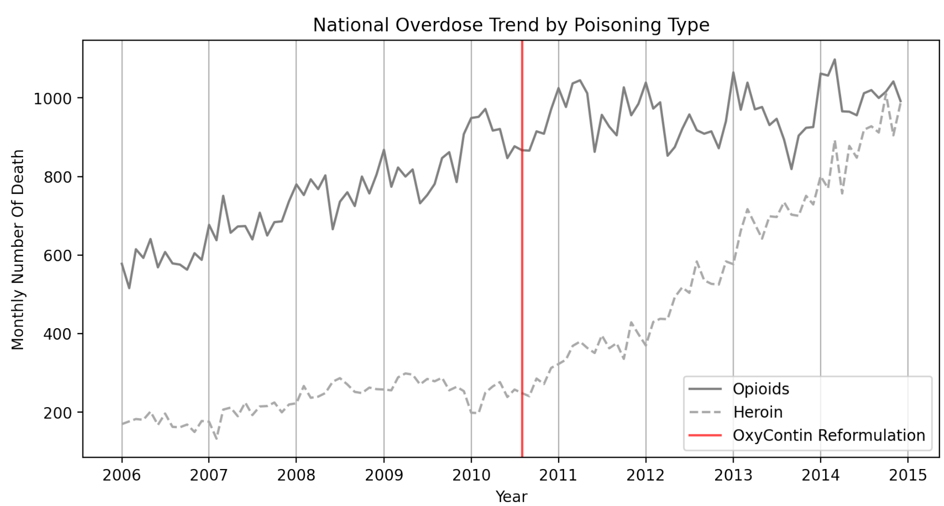


Figure 1.2: Mortality trend

[6] We acknowledge that some non-OxyContin alternatives are branded and non-generic (i.e. Percocet and Percodan or later Roxicodone), but the majority of them are generic products. Generic oxycodone in this paper should be interpreted as all non-OxyContin oxycodone products.

The number of opioid fatalities grew in our sample period, from on average 600 deaths per month to 1000 per month. The number of heroin deaths was stable from 2006 to 2009 at about 200 deaths per month, and then it rose sharply from 2011 to 2015. As we have stated in the literature review section, the cause of the increase in heroin mortality is unclear. While some papers blame the OxyContin reformulation, there is evidence indicating that the availability of heroin increased substantially after 2010 (O'Donnell, Gladden and Seth, 2017).

Since the number of drug overdose deaths with no drug specified accounts for between one-fifth and one-quarter of the overdose cases (Ruhm, 2017), our measures of opioid and heroin deaths likely underestimate the true number of deaths.[7] However, the underestimation would not pose a problem for our regressions. There are variations in how coroners attribute the cause of death across states, but such variation would be captured by the state fixed effects. In addition, we do not anticipate systematic changes to each state's practices due to the reformulation.

NSDUH and Measuring Misuse

We use state-level data from the National Survey on Drug Use and Health (NSDUH) to measure nonmedical use of opioids. The NSDUH publishes an annual measure of OxyContin misuse, asking the respondents whether they have ever used OxyContin "only for the experience or feeling they caused" (NSDUH Codebook). As first described in Alpert, Powell and Pacula (2018), the advantage of the NSDUH misuse measure is that it separates out misuse from medical use. However, only OxyContin is reported in the NSDUH, and there is no equivalent measure for generic oxycodone.

Fortunately, the NSDUH reports PERCTYL2, which asks whether individuals ever misused Percocet, Percodan, or Tylox.[8] These drugs are oxycodone hydrochloride with acetaminophen and have a maximum dosage of 10mg of oxycodone per pill. The three drugs were popular among users in the pre-OxyContin era (Meier, 2003). In the present day, the PERCTYL2 variable captures misuse of not only the three branded drugs but also other generic oxycodone products that are popular on the street.

The most direct evidence supporting this claim is the fact that generic oxycodone pills have often been referred to as 'Percs' colloquially in the last decade. Many

[7] Specifically, we omit ICD-10 code T50.9 (unspecified poisoning) from our analysis, and some fraction of these deaths are due to opioids or heroin, but were not diagnosed or recorded as such.

[8] Percocet Drug Information. Tylox was discontinued in 2012 following the FDA regulations limiting acetaminophen.

news report indicated that generic oxycodone has the street name ‘Perc 30’ but is in fact not Percocet. The Patriot Ledger reported in a 2011 article[9] that ‘Perc 30s’ were the newest drug of choice in South Shore of Massachusetts, saying:

> *‘Perc 30s are not Percocet—the brand name for oxycodone mixed with acetaminophen, the main ingredient in Tylenol—but a generic variety of quick-release oxycodone made by a variety of manufacturers. They are sometimes referred to as “roxys” after Roxane Laboratories, the first company to make the drug, or “blueberries,” because of their color.’*

Since many generic oxycodone users would not know the name of the drug they use other than by its street name, but could distinguish between immediate release oxycodone and extended release OxyContin, it is likely that they answer affirmatively to misusing Percocet when they are, in fact, using generic oxycodone.[10]

There are also several empirical observations that support this claim. The first is that we continue to see increases in the lifetime misuse of Percocet, Percodan, and Tylox even after they were replaced by OxyContin as the preferred prescription opioid to misuse. The misuse rate of Percocet, Percodan, and Tylox increased 30% from 4.1% to 5.6% from 2002 to 2009 (see Figure 1.8 in Appendix), which would not have been possible if these drugs, or what people believed were ‘Percs,’ were not actively misused by new users post-introduction of OxyContin.

The second observation is that, based on the average sales data from 2006 to 2014, a disproportionate number of people has reported misusing Percocet, Percodan, or Tylox as compared to the actual sales of the three drugs. The sales of Endo Pharma, the manufacturer of Percocet and Percodan[11], are orders of magnitude less than the sales of Purdue while more than twice as many people reported misusing the three drugs as compared to OxyContin (see Figure 1.9 in Appendix). A back-of-the-envelope calculation shows that if PERCTYL2 misuse captures only the misuse of Percocet and Percodan, then the proportion of pills misused out of all pills sold is 29 times higher for Percocet and Percodan than the same proportion for OxyContin[12], a very unlikely situation given the popularity of OxyContin on the street.

[9] Patriot Ledger Link. Other references to generic non-OxyContin oxycodone as Perc 30s: Phoenix House, Washington State Patrol, The Boston Globe, The Salem News, Massachusetts Court Filing, Cape Cod Times, Pocono Record, Bangor Daily News, Patch, CNN Op-Ed.

[10] In the ARCOS dataset these pills are simply listed as ‘Oxycodone Hydrochloride 30mg.’

[11] Tylox not included since it was discontinued.

[12] In terms of number of pills circulated, OxyContin is 12.1 times Percocet and Percodan from 2006 to 2014. In terms of misuse, OxyContin is 41% of Percocet and Percodan in the same period.

This deduction is further supported by misuse data reported in the NSDUH. We know that generic oxycodone is commonly misused.[13] If oxycodone has any other drug names, the popularity of that drug name in the NSDUH surveys should increase to reflect the increase in misuse in recent years. In addition to inquiring about popular brands, the NSDUH survey asks respondents to list any other prescription oxycodone that they have misused before. Dozens of pain relievers are reported, but in 2010 "oxycodone or unspecified oxycodone products" was only named by 0.10%[14] of the respondents. No other brand of oxycodone pills are reported as commonly misused. We know from the reports in press and documents in court that generic oxycodone is a popular opioid on the street, and we know that Percocet is the only other commonly misused opioid documented in the NSDUH survey. Thus, the only way to reconcile the discrepancy between these two sources is that people mistakenly perceive generic oxycodone as Percocet or respond to the NSDUH as if they do. Thus, we use lifetime OxyContin and lifetime Percocet misuse for the construction of OxyContin and generic oxycodone exposure measures in Section 1.3.3.

Market Definition and Endogeneity Problems

Previous studies of the OxyContin reformulation depend on state-level variations to causally identify the impact of the reformulation. Treating OxyContin reformulation as an exogenous shock at the state level is potentially problematic. Although the timing of the reformulation is exogenous, each state's exposure to it is a result of a combination of the state's regulatory environment and Purdue's initial marketing strategy (*Origins of the Opioid Crisis and Its Enduring Impacts*, N.d.). These factors have substantial impact on how people in a state respond to the reformulation, creating a hidden link between exposure to the reformulation, the identifying variation, and subsequent drug use, the outcome variable.

One can limit the impact of endogenous regulation by disaggregation, but only if there is substantial intra-state variation in exposure to the reformulation. Both the ARCOS database and the NVSS mortality data have great geographic detail. Conducting our analysis on metropolitan statistical areas (MSAs), we find large

[13] Law enforcement and journalists have previously identified the 30mg oxycodone pill as the most commonly trafficked opioid, see DEA Link, ICE Link, and Palm Beach Post Link.

[14] NSDUH Codebook variables (ANALEWA through ANALEWE) list the other pain relievers reported. Even if we assumed that all 2.49% of respondents saying they took a prescription pain reliever not listed had taken generic oxycodone, it is still less than half of the reported Percocet misuse.

variation in both OxyContin use and opioid mortality across MSAs in the same state. At the aggregate level in 2009, the average OxyContin market share in a state is 35.6%. 65 of the 379 MSAs (17.1%) in our sample have an OxyContin market share that is 10% greater or smaller than their state average. The average opioid mortality is 0.343 deaths per 100,000 population in 2008. The variation in death is even more significant. More than 310 (83%) MSAs have a mortality rate 20% higher or lower than their state average, and more than 192 (51%) have a mortality rate 50% higher or lower than their state average. We present the full distribution of deviations of the OxyContin market share and opioid mortality from state average in Figure 1.10 and Figure 1.11 in the Appendix.

Disaggregating to the MSA-level allows us to control for the state's regulatory environment and hence eliminate the most problematic source of endogeneity. We use intra-state variation in exposure to the reformulation for identification. Intra-state heterogeneity in opioid use is associated with past economic conditions (Carpenter, McClellan and Rees, 2017), location of hospitals and treatment centers (Swensen, 2015), preferences of local physicians (Schnell, 2017), and local policy, some of which could still be correlated with the locality's response to the reformulation. Analysis at the MSA level clearly allows us to make a much stronger claim than analysis at the state level.

In addition, as we will show in the next sections, the disaggregation increases the statistical power of our regressions beyond the impact of the tripled sample size. Our results indicate that defining the market at the MSA level better captures the interaction between drug use and mortality than the state level. The important variations in drug use, for example between Los Angeles-Long Beach-Santa Ana at 4.4% of nonmedical use of pain relievers and San Francisco-Oakland-Fremont at 5.6%, disappears when they are aggregated to the state level (*2005-2010 NSDUH MSA Detailed Tables*, 2012).

OxyContin and Non-OxyContin Oxycodone Exposure

Since the OxyContin reformulation was a national event independent of local conditions, we can estimate its impact by comparing the outcomes in areas of high prior exposure to opioids with outcomes in areas of low exposure. Ideally, we want to quantify exposure using the volume of OxyContin misused in each region pre-reform while controlling for the volume of generic oxycodone misused. In practice, we do not observe these quantities. The best proxy in the literature is the self-reported misuse rate from the NSDUH.

Based on the NSDUH misuse, we create a new measure of OxyContin and non-OxyContin oxycodone exposure by combining the NSDUH state-level misuse rate with ARCOS MSA-level sales. Specifically, for each drug, we calculate:

$$\text{Exposure}_m^{\text{pre-reform}} = \text{Lifetime Misuse}_s^{2004-2009} \times \text{Sales}_m^{2009} \tag{1.1}$$

Our measure is the interaction term of sales of OxyContin/generic oxycodone in an MSA and the lifetime misuse rate of that drug in the corresponding state. This new measure has two advantages over the conventional misuse rate from NSDUH: it captures intra-state variation in misuse and it more accurately reflects the current misuse of both OxyContin and generic oxycodone.

The NSDUH surveys approximately 70,000 respondents every year and uses sophisticated reweighting techniques to get accurate state level estimates. Once we get to the MSA level, the number of people surveyed as well as the number of positive responses to questions on opioid misuse are extremely small. As a result, most of the outcomes at the MSA level are censored by the NSDUH to protect individual privacy. Using only the survey data means that we would use same state misuse value for all MSAs and therefore forgo any intra-state variation in drug use. In comparison, our proposed measure relies on deviations from normal sales patterns to generate variations in exposure rates for the MSAs. Our definition assumes that the percentage of people that reported misusing a particular drug in a state is equivalent to the proportion of sales that are being misused. In a state where all the MSAs have identical sales, all the MSAs will have identical exposure rates by definition. However, if one MSA has higher sales of OxyContin compared with the rest of the state, our OxyContin exposure measure in that MSA will be higher than the rest of the state. This construction of exposure mirrors our intuitive understanding that the misuse of a drug in a locality is a function of the overall misuse and the availability of that particular drug in the area.

The NSDUH survey[15] reports past-year misuse of OxyContin but only lifetime misuse of generic oxycodone. Previous studies did not focus on generic oxycodone misuse, so these studies rely on past-year OxyContin misuse rates. In our case, to disentangle substitution among prescription opioids, we have to make the comparison between OxyContin and generic oxycodone equal. Resorting to lifetime misuse rates for both series sacrifices the timely nature of the NSDUH misuse rates. By

[15] In all surveys prior to 2014.

combining the lifetime misuse rates with sales in the year before reformulation, we capture recent changes in the use of both drugs. To make our results comparable with previous studies, in the Appendix section, we repeat our entire analysis with OxyContin last-year misuse and generic oxycodone lifetime misuse. Most of our conclusions stand despite giving OxyContin a more favorable treatment.

To construct our measure, we follow the precedent set in the literature by using a six-year average state level lifetime misuse rate pre-reform (2004–2009) and sales in 2009. The goal of the time average is to reduce the variance of the state-level misuse rates. We check the validity of our measure by regressing opioid death on it and compare the results with the same regressions on either only ARCOS sales or only NSDUH misuse. Results are summarized in Table 1.2 in Appendix. The fit of the generic oxycodone regression is much improved with the interacted variable ($R^2 = 0.187$) relative to using only one with NSDUH misuse ($R^2 = 0.062$) or sales ($R^2 = 0.176$). The improvement is even larger for the OxyContin regression ($R^2 = 0.128$) relative to using only one with NSDUH ($R^2 = 0.084$) or with sales ($R^2 = 0.086$).

Descriptive Statistics

Table 1.1 reports summary statistics for five groups of MSAs: All MSAs, MSAs with high OxyContin exposure, MSAs with low OxyContin exposure, MSAs with high generic oxycodone exposure, and MSAs with low generic oxycodone exposure. MSAs with high OxyContin exposure and MSAs with high generic oxycodone exposure have similar demographic summary statistics. These two groups of MSAs also are not different statistically in their heroin mortality. Disentangling the impact of various opioids on the rise in heroin mortality is impossible with nationally aggregated or state level data due to the high correlation in misuse. The high correlation also implies that regressing heroin death on OxyContin without controlling for generic oxycodone use will likely lead to an overestimation of OxyContin's impact.

MSAs with high misuse differ from MSAs with low misuse. High misuse states have higher sales of both types of prescription opioids (twice as much for both types of opioids), higher mortality rate (twice as much for both opioid and heroin overdose), smaller population, higher average age, higher median income, higher percentage of white population, and lower percentage of black population. The differences in racial composition repeat well established findings in the literature: prescription opioid misuse was originally concentrated among white users, and by 2010, new

Table 1.1: Summary statistics

	All MSAs	MSAs with low OxyContin exposure	MSAs with high OxyContin exposure	MSAs with low oxycodone exposure	MSAs with high oxycodone exposure
NSDUH lifetime misuse rates (2004-2009)					
OxyContin misuse rate (%)	2.22	1.88	2.56	1.87	2.56
Oxycodone misuse rate (%)	5.19	4.22	6.17	3.75	6.64
Annual ARCOS sales (all sample period)					
Oxycontin sales per person	65.71	43.47	88.06	50.70	80.79
Oxycodone sales per person	181.84	112.50	251.55	99.24	264.88
Annual death per 100,000 (all sample period)					
Opioid	0.32	0.23	0.41	0.23	0.42
Heroin	0.13	0.09	0.16	0.10	0.16
Census Demographics (2009)					
Number of MSAs	379	190	189	190	189
Population	679878	745327	614082	663740	696101
Age	36.13	34.68	37.59	34.84	37.43
Male (%)	49.24	49.35	49.13	49.40	49.08
Separated (%)	18.83	18.24	19.42	18.32	19.34
High school and above (%)	84.20	82.79	85.61	83.68	84.72
Bachelor and above (%)	25.36	24.77	25.96	24.85	25.87
Mean income	64213	63414	65016	63058	65374
Low income (%)	35.38	35.79	34.98	35.90	34.86
White (%)	82.17	79.99	84.36	81.22	83.12
Black (%)	11.20	13.09	9.30	11.80	10.60
Asian (%)	3.03	3.47	2.60	3.52	2.54
Native American (%)	0.18	0.20	0.17	0.20	0.17

Note: Simple average, not weighted by population.

heroin users were almost entirely white (Cicero et al., 2014). These differences in demographic variables motivate the inclusion of control variables in our main regressions.

1.4 Empirical Strategies

Our goal is to investigate two questions. First, what was the reformulation's immediate impact on OxyContin and generic oxycodone use? Second, what was the reformulation's long-run effect on opioid mortality, heroin mortality, and on the progression of opioid addiction?

We follow the event study framework from Alpert, Powell and Pacula (2018) to estimate the causal impact of the OxyContin reformulation on OxyContin and generic oxycodone sales and opioid and heroin mortality. We exploit the variations in MSAs' exposure to the reformulation due to the differences in their pre-reform OxyContin use while controlling for pre-reform generic oxycodone use. Our approach is similar to Finkelstein (2007), where the OxyContin reformulation has more "bite," or more of an effect, in areas where OxyContin misuse was higher than in places where generic oxycodone was the preferred drug. The approach allows us to measure whether MSAs with higher exposure to OxyContin experienced larger declines in OxyContin sales, larger increases in alternative oxycodone, or larger increases in opioid and heroin mortality. The empirical framework is:

$$\begin{aligned} Y_{mt} = \alpha_s + \delta_t + \sum_{i=2006}^{2014} \mathbb{1}\{i = t\}\beta_i^1 \times \text{OxyContin Exp}_m^{Pre} \\ + \sum_{i=2006}^{2014} \mathbb{1}\{i = t\}\beta_i^2 \times \text{Oxycodone Exp}_m^{Pre} + X'_{mt}\gamma + \epsilon_{mt} \end{aligned} \tag{1.2}$$

where Y_{mt} are the outcome variables of interest in MSA m at year t; OxyContin Exp_m^{Pre} and Oxycodone Exp_m^{Pre} are time-invariant measures of OxyContin and oxycodone exposure before the reformulation (see Section 1.3.5 for construction), and are interacted with a set of β_t^1 and β_t^2 for each year. We include state fixed effects to control for regulatory differences among states and year fixed effects to control for national changes in drug use. We also include a full set of MSA-level demographic variables. We weight the regression by population and exclude Florida.[16] We show

[16]The literature excludes Florida because it underwent massive increases in oxycodone sales over this period, some of which was trafficked to other states.

the full set of β_t estimates graphically, normalizing by the 2009 coefficient. The β_t identifies the differences in sales and death across MSAs due to their higher or lower pre-reform OxyContin or oxycodone exposure. Standard errors are clustered at the MSA level to account for serial correlation. In the Appendix section, we present beta estimations from variations of our base model, which include (1) using a MSA fixed effect instead of state fixed effect, (2) replacing OxyContin lifetime misuse rate with OxyContin last-year misuse rate, (3) regressing at the state level, and show that our conclusion are insensitive to most of these variations.

To complement our results, we also use a strict difference-in-difference framework to estimate the effect of the reformulation conditioning on OxyContin and non-OxyContin oxycodone exposure levels. Our specification is:

$$\begin{aligned} Y_{mt} = {} & \alpha_s + \gamma_t + \delta_1 \mathbb{1}\{t > 2010\} \\ & + \delta_2 \mathbb{1}\{m \in HighOxyContin\} + \delta_3 \mathbb{1}\{m \in HighOxycodone\} \\ & + \delta_4 \mathbb{1}\{t > 2010\} \times \mathbb{1}\{m \in HighOxyContin\} \\ & + \delta_5 \mathbb{1}\{t > 2010\} \times \mathbb{1}\{m \in HighOxycodone\} + X'_{mt}\beta + \epsilon_{mt} \end{aligned} \tag{1.3}$$

where $HighOxyContin$ and $HighOxycodone$ are the set of MSAs with higher than median pre-reform exposure to OxyContin and oxycodone, respectively. We restrict the regression to include only the three years prior (2008 to 2010) and the three years after (2011 to 2013) the reformulation. The advantage of this specification is that it does not assume that OxyContin or oxycodone exposure affects the outcome variable linearly. Instead of having a flexible δ for each year, we have only one δ for each of the pre- or post-reform periods. In this specification, we simply test whether higher exposure MSAs reacted differently to the reformulation as compared to lower exposure MSAs (if δ_4 and δ_5 are significant). We include state fixed effects to control for state-level heterogeneity, year fixed effects for national trend, and a set of time-varying MSAs level covariates. Again, standard errors are clustered at the MSA level.

1.5 Results

We proceed in two steps. First, we provide direct evidence that the OxyContin reformulation caused OxyContin sales to decrease and generic oxycodone sales to increase, and that the changes in sales are proportional to the pre-reformulation level of OxyContin exposure. Second, we estimate the impact of the reformulation on opioid and heroin mortality. We find that high pre-reformulation levels of OxyContin exposure were not associated with high opioid deaths, but there was a strong positive effect from generic oxycodone exposure in both the pre- and post-reform period. We find that higher pre-reform OxyContin and pre-reform oxycodone exposure were both positively but not significantly associated with later heroin deaths, but the oxycodone coefficient is larger. If we run the heroin regression separately with only OxyContin exposure, we recover the results of the literature, but running the heroin regression with only oxycodone exposure better fits the data.

Reformulation's Impact on Opioid Sales

We begin by showing graphically that OxyContin sales decreased and generic oxycodone sales increased in high OxyContin misuse MSAs immediately after reformulation. Figure 1.3 and Figure 1.4 present the full set of coefficients from estimating the event study framework on OxyContin and generic oxycodone sales. Each data point in the figure is the coefficient of the interactive term of misuse and sale, which we call exposure, for OxyContin or generic oxycodone in a specific year, and it captures any additional change in sales in that year driven by high OxyContin or oxycodone exposure. In Figure 1.3, we observe a larger decrease in OxyContin sales post-reform in MSAs with higher pre-reform OxyContin exposure. As Figure 1.4 shows, higher OxyContin exposure MSAs saw greater increases in generic oxycodone sales post-reform. The effects are well identified at 95% confidence level. An one standard deviation increase in OxyContin exposure translates into an additional 21.2 MME decrease in per person OxyContin sales and 11.8 MME increase in per person oxycodone sales in 2011. These changes represent a 24% decrease in OxyContin sales and a 8.8% increase in oxycodone sales from the 2009 level. The effects are economically significant especially given that the reformulation should only affect the population abusing OxyContin, so this drop in sales is driven by a fraction of all users. The two observations combined support the hypothesis that reformulation caused substantial substitution from OxyContin to generic oxycodone.

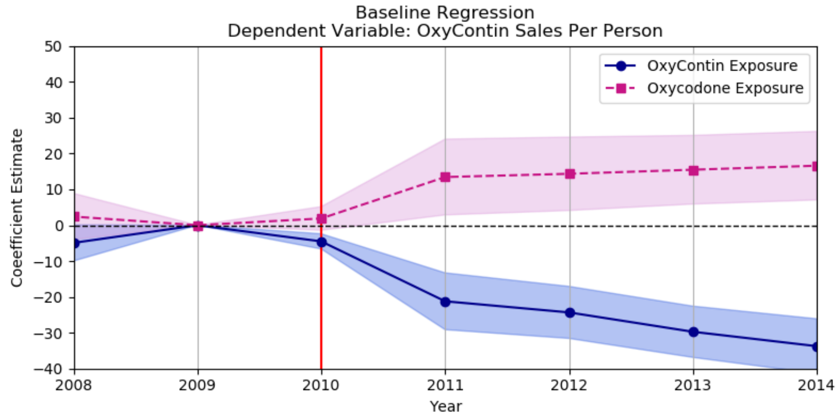


Figure 1.3: Impact of reformulation on OxyContin sales

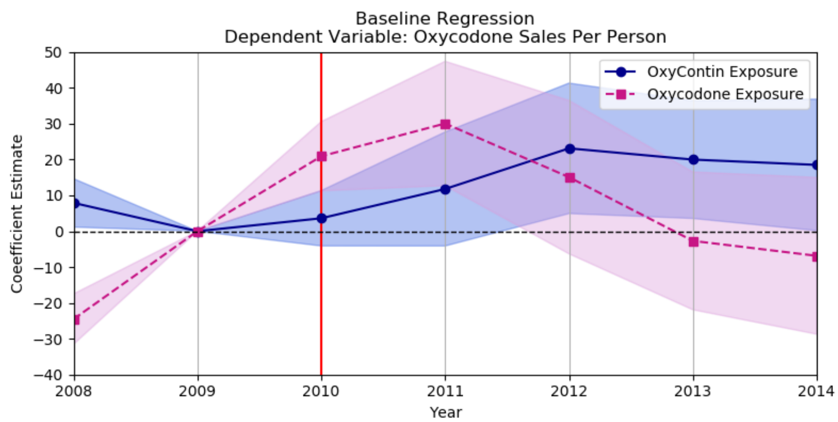


Figure 1.4: Impact of reformulation on generic oxycodone sales

Figure 1.3 also documents that high pre-reform oxycodone misuse MSAs saw large increases in OxyContin sales right after the reformulation. This phenomenon has been unreported previously, but would be consistent with Schnell (2017)'s physician benevolence hypothesis where good physicians switch patients from oxycodone to reformulated OxyContin to lower the future risk of abuse. Although the switch toward OxyContin is smaller in magnitude than the switch from OxyContin, this increase is the first documented positive impact of the OxyContin reformulation in the literature. It seems that both physicians and users saw the two types of drugs as substitutes. Unfortunately, there are not enough MSAs where the switch toward OxyContin is significant enough that it cancels out the switch away from OxyContin to examine the possible substitution channel in the other direction.

Because we include both OxyContin and generic oxycodone misuse in the same regression, we can separate out the increases in oxycodone sales due to its own popularity from the increases due to spillover effects from the OxyContin reformulation. Figure 1.4 shows increasing growth in oxycodone sales in MSAs with higher oxycodone misuse until 2011, and the growth rate declined after. The smoothness of the oxycodone curve indicates that the OxyContin reformulation had no impact on how oxycodone misuse affected oxycodone sales. This trend corresponds well with many states tightening control over opioid prescription policies in 2011 and 2012 in response to rising sales and increased awareness of opioid misuse.

Another way of estimating the impact of the reformulation is through difference-in-difference regressions. Column (1) of Table 1.3 in Appendix shows the regression on OxyContin sales. OxyContin sales in all MSAs decreased by 8.05 MME post-reform, a 9.4% decrease with respect to the average per person sales of 85.6 MME in 2009. High OxyContin misuse MSAs had a higher level of OxyContin sales to start with, but experienced an additional 15.1 MME drop (an additional 17% decrease) post-reform. Given that only 2.46% of the population ever misused OxyContin[17] and the reformulation only affected the people misusing it, a 17% additional decrease in all OxyContin sales would translate into a very significant decrease in sales to the population that misuses it. The negative and significant *Post* $\times$ *High OxyContin* coefficient confirms previous findings that high OxyContin exposure MSAs saw larger decreases in OxyContin sales post-reform.

Column (2) of the same table reports the regression on generic oxycodone sales. Generic oxycodone sales per person increased 41.7 MME in the post period, a 31.2% increase with respect to the average per person alternative oxycodone sales of 133.5 MME in 2009. High OxyContin misuse MSAs experienced an additional 10.3 MME increase, which translates to a 68% conversion from OxyContin to generic oxycodone in those areas. Combining the findings from columns (1) and (2), we see direct substitution from OxyContin to generic oxycodone in local sales immediately after reformulation, and the substitution pattern is more pronounced in MSAs with high OxyContin exposure as expected.

To help our readers visualize the trend of OxyContin and alternative oxycodone sales, in Figure 1.12 in the Appendix, we break all MSAs into three bins by the magnitude of the observed drop in OxyContin sales due to the reform. Then, we plot the per person OxyContin and generic oxycodone sales for the three groups,

[17] NSDUH, 2010.

respectively. By definition, the high empirical drop group experienced the largest decreases in OxyContin sales from 2009 to 2011 (-29%) and the low drop group experienced an increase in OxyContin sales (+15%). Sales of generic oxycodone started at different levels, but shared the same growth rate until the reformulation in 2010. Since 2010, the higher the empirical drop in OxyContin, the faster the growth in generic oxycodone. The high group saw a 72 MME increase (46% from 2009) in generic oxycodone sales, while the low group only saw a 29 MME increase (29% from 2009). The high growth rate of generic oxycodone in high drop MSAs supports the substitution story. The post-reform level of OxyContin sales converges to the same level for all three groups, suggesting that the remaining sales most likely represent non-replaceable demand for medical OxyContin use.

Reformulation and Opioid and Heroin Mortality

Next, we estimate the impact of the reformulation on overdose mortality. In Figure 1.5, we report the full set of coefficients from estimating the event study framework on opioid mortality. Each data point in the figure is the coefficient of the interactive term of misuse and sale for OxyContin or generic oxycodone in a specific year, and it captures any additional change in opioid mortality in that year driven by high OxyContin or oxycodone exposure. The OxyContin coefficients are never significant, suggesting that higher pre-reform OxyContin misuse is not predictive of either higher or lower numbers of opioid deaths post-reform. The lack of any trend indicates that any benefit of the OxyContin reformulation on reducing OxyContin consumption is offset by the substitution to generic oxycodone. In aggregate, the reformulation had no impact on non-heroin opioid deaths.

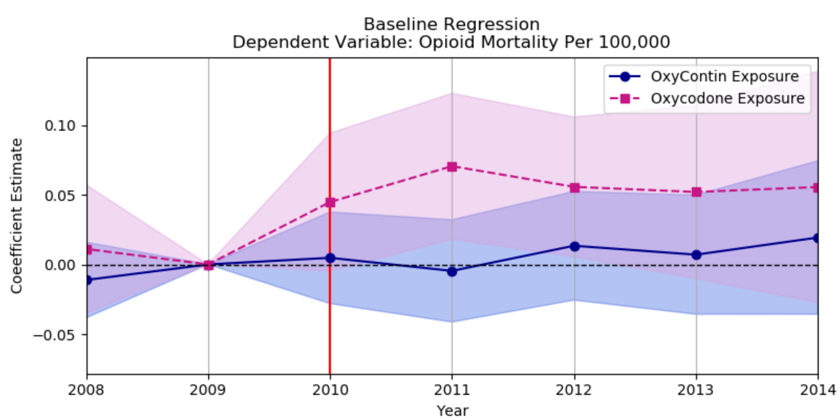


Figure 1.5: Impact of reformulation on opioid mortality

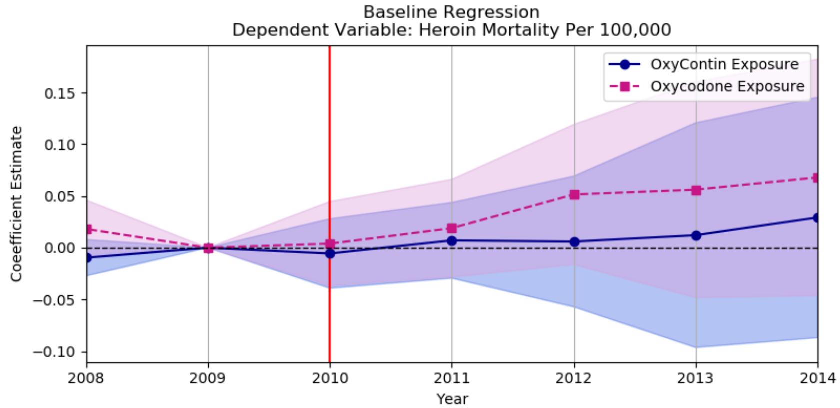


Figure 1.6: Impact of reformulation on heroin mortality

In Figure 1.6, we report the event study coefficients on heroin mortality. Again the OxyContin coefficients are tiny and insignificant, while the oxycodone coefficients grow over time but never reach statistical significance at conventional levels. The lack of statistical significance is due to the small number of heroin moralities in the whole sample and high correlations between OxyContin and oxycodone exposure. If we were to run the OxyContin and oxycodone regression separately (See Figure 1.34 and Figure 1.38 in Appendix), oxycodone exposure had a much larger and more significant impact on heroin mortality. The results provide tentative evidence that the higher the generic oxycodone exposure in an MSA, the higher the increases in heroin mortality. However, the results do not support the alternative hypothesis that the OxyContin reformulation was solely responsible for the increase in heroin mortality.

The difference-in-difference results mirror our finding from the event study framework. Column (3) of Table 1.3 in Appendix suggests that opioid deaths are 0.08 higher in high oxycodone exposure MSAs, which is equivalent to 27% of the average opioid overdose of 0.29 per 10,000 people in 2009. Opioid mortality is 0.05 lower (17% of the 2009 average) in higher OxyContin exposure MSAs after controlling for oxycodone use. Higher OxyContin exposure does not lead to higher or lower opioid overdose post-reform, while higher generic oxycodone exposure is associated with 0.06 (20.6% of 2009 average) more opioid death in the post period.

Column (4) of the same table reports the difference-in-difference regression on heroin death. Heroin mortality has increased by 0.14 in the post period in all MSAs, which is equivalent to a 111% increase from the average 2009 level of 0.126 heroin death per 10,000 population. High OxyContin exposure MSAs did not experience additional jumps in heroin mortality, while high oxycodone exposure MSAs did experience an additional 0.07 (56% with respect to 2009 average) increase in deaths. Again, the evidence from the difference-in-difference regressions indicate that OxyContin was not responsible for the rise in heroin mortality.

In Figure 1.13 in the Appendix, we show the average trend of the opioid and heroin mortality for groups with high, medium, and low observed drop in Oxycontin sales. If the reformulation was responsible for the subsequent heroin epidemic, then the MSAs most likely to have additional jumps in heroin mortality would be the MSAs with the largest OxyContin drop. As shown in the figure, the three groups went through the same explosive growth in heroin mortality (around 38% from 2009 to 2011, and similar rate afterward), indicating that the rise in heroin was independent of the decrease in OxyContin sales. This evidence conclusively rejects the hypothesis that the OxyContin reformulation is solely responsible for the subsequent heroin epidemic.

Discussion

(A) The Reformulation's Impact on Opioid Mortality

Until now, the literature has found mixed results for the effects of the OxyContin reformulation on opioid mortality. In contrast to previous work, we find no statistically significant impact of the reformulation on opioid mortality as a result of substantial substitutions from OxyContin to generic oxycodone post-reform. Increases in generic oxycodone sales compensated for 55% of the drop in OxyContin sales in high OxyContin misuse MSAs by our event study framework, and 68% by our difference-in-difference estimation. Opioid mortality continued to increase in the post-reform period, but was not driven by high OxyContin exposure.

(B) The Reformulation's Impact on Heroin Mortality

Our results stand in direct contrast to the findings of the literature. Instead of being the event that precipitated the heroin epidemic, the OxyContin reformulation shifted misuse to other opioids, of which heroin was only one. We cannot refute the hypothesis that some OxyContin users switched to heroin due to the reformulation. Our analysis refutes the hypothesis that the reformulation was the sole cause of the heroin epidemic. Instead of OxyContin misuse, we identified generic oxycodone misuse as a much more powerful driver of increases in heroin mortality post-2011. What prompted the increases in heroin use is still an unresolved question. Previous research has suggested an increase in the supply of heroin (O'Donnell, Gladden and

Seth, 2017) around this time, as well as crackdowns in Florida on pill-mills reducing the supply of oxycodone (Kennedy-Hendricks et al., 2016).

(C) Bridging the Differences between our Findings and the Literature

One of the innovations we have made in this paper is to shed light on a hidden source of opioid misuse: the misuse of generic oxycodone. This segment of prescription opioids was overlooked by other scholars because of OxyContin's dominance in opioid misuse in the early years as well as, we argue, the lack of identifiable brand names for the generic products. Empirical studies based on market data or interviews of opioid users noted that many people misused generic oxycodone products (Inciardi et al., 2009; Paulozzi and Ryan, 2006). Leaving out oxycodone misuse, an important driver of opioid and heroin mortality that is positively correlated with OxyContin misuse, would produce spurious regression results.

To show that the difference in findings is not driven by our constructed misuse measure, or our choice of framework, we test whether we can reproduce findings in the literature by running all of our regressions using only OxyContin (see Section 1.7 in the Appendix). Our OxyContin misuse exposure individually predicts an increase in opioid and heroin mortality post-reform as the literature claims. This finding is the basis of previous studies supporting the claim that the OxyContin reformulation is the main cause of the subsequent heroin epidemic. However, if we run the same set of regressions using only generic oxycodone (see Section 1.7), we are able to produce the same findings. The only way to differentiate the impact of OxyContin from that of generic oxycodone is to include both in the same regressions. Variations in local OxyContin and oxycodone exposure allow us to identify the impact of both series, if any exist. As we have shown in our main regressions, the impact of OxyContin on heroin disappears after controlling for the effect of generic oxycodone.

(D) Market Definition

Another innovation we have made in this paper is a finer definition of the opioid market. It is important to consider what we gain from disaggregating to the MSA level. The specific OxyContin market share in a state is endogenous to a great many things, including advertising (Van Zee, 2009) and triplicate status (*Origins of the Opioid Crisis and Its Enduring Impacts*, N.d.). Although the OxyContin reformulation was an exogenous shock, its interpretation is made very complicated because its impact depended on each state's regulatory history and prescribing environment. We do our regressions at the MSA level, where there are unobserved local conditions that affected sales of OxyContin and generic oxycodone, while controlling for state-level laws and restrictions. By comparing two different MSAs with the same regulatory environment but different exposures to the reformulation, we can get at the marginal effects of OxyContin and generic oxycodone exposure. Contrasting the state-level regression estimates (see Section 1.7) with our main results, our main results are larger in magnitude and more statistically significant. The MSA level estimation of the effect of exposure on mortality is more stable.

(E) Definition of OxyContin Misuse

The literature relies on NSDUH's OxyContin past-year misuse. To make our findings comparable with previous studies and robust to the choice of misuse measure, we repeat our entire analysis with OxyContin last-year misuse and generic oxycodone lifetime misuse (see Section 1.7 for results.) As noted in Section 1.3, using last-year OxyContin misuse gives an unfair advantage to OxyContin due to the timeliness of the measure. If our findings on oxycodone persist despite the unequal treatment of the two misuse measures, then it is a stronger indication of the essential role generic oxycodone played in the opioid and heroin epidemic.

Comparing the two sets of results, we observe the same decline in OxyContin sales and increase in generic oxycodone sales, although smaller in magnitude. Both sets of coefficients on opioid mortality become positive but insignificant. Finally, comparing the heroin result, at the state level we do detect a positive effect on heroin mortality from OxyContin. In aggregate, our results lose some significance when we replace lifetime OxyContin misuse with last-year OxyContin misuse. The loss of significance, however, is in the direction predicted by the unfair advantage given to OxyContin. This exercise highlights the importance of treating the two misuse measures equally. When we use measures that more accurately capture recent OxyContin misuse than recent generic oxycodone misuse, we could mistakenly attribute the effects of generic oxycodone to OxyContin.

1.6 Conclusion

Researchers have attributed the prescription opioid crisis and recent increase in heroin use to OxyContin. Previous studies have documented how Purdue Pharma's marketing downplayed the risks of OxyContin's abuse potential, which fomented the prescription opioid crisis; recent studies identified the OxyContin reformulation as the event that pushed users to switch to heroin, which precipitated recent increase in heroin use. This paper revisits the roles OxyContin and the OxyContin reformulation played in the opioid crisis with fine-grained sales data that includes OxyContin's most immediate substitute, generic oxycodone. We have three main findings.

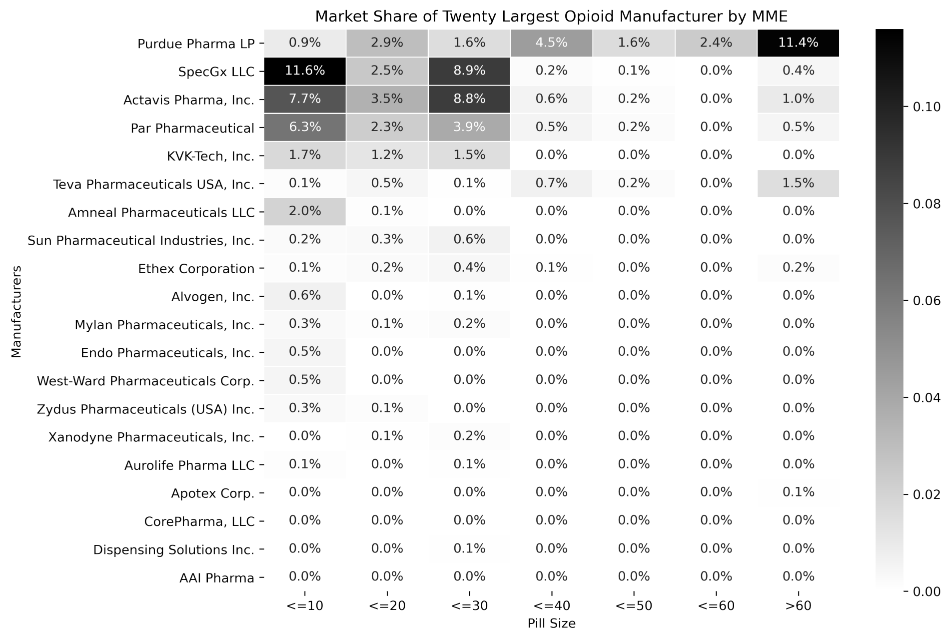
First, we find direct evidence of substitution from OxyContin to generic oxycodone post-reformulation. Our difference-in-difference estimation indicates a 68% substitution from OxyContin to generic oxycodone due to the reform. Looking at the decline in OxyContin sales and rise in generic oxycodone sales from 2002-2006, we believe that this substitution (for different reasons, namely Purdue's loss of its patent) also happened years before the reformulation. The size of this substitution, and indeed the size of the generic oxycodone market pre-reform, may come as a surprise to researchers. Paulozzi and Ryan (2006) estimate that in 2002 OxyContin's market share was 68%. By the time of the reformulation in 2010, it had fallen by more than half. OxyContin played an essential part in igniting the prescription opioid crisis but, after losing its patent in 2004, other companies took up the torch and surpassed Purdue by selling generic oxycodone.

Our second main finding is that the OxyContin reformulation had no overall effect on opioid mortality. In our estimation, the OxyContin coefficients are not significant in the entire sample period, suggesting that higher OxyContin exposure is not predictive of either higher or lower opioid death. The lack of any trend indicates that the benefits of the OxyContin reformulation, if exist, are offset by substitution to oxycodone. In addition, we do find that high oxycodone exposure is predictive of rise in opioid mortality from 2011, confirming the increasingly important role of generic oxycodone in the recent prescription opioid crisis.

Third and most importantly, we show that the heroin overdose deaths after 2010 were predicted by generic oxycodone exposure, not OxyContin exposure. Our main event-study model shows positive and significant effects from oxycodone exposure on heroin deaths after 2012, but OxyContin exposure is not predictive of heroin deaths once we control for oxycodone. The difference-in-difference results are similar, showing that oxycodone exposure was predictive of heroin deaths before or after the reformulation, and OxyContin exposure after the reformulation is weakly positive but not statistically significant. We also do not observe an additional rise in heroin deaths immediately after reformulation in areas where OxyContin sales declined the most post-reformulation. In particular, without including generic oxycodone in the analysis, we recover the same results from the literature that OxyContin was responsible for the rise in heroin deaths. The evidence shows that omitting oxycodone, an important substitute to OxyContin, produces erroneous results. This paper demonstrates the pernicious effects of generic oxycodone, which had thus far escaped scrutiny until the Washington Post acquired data and reported on it.

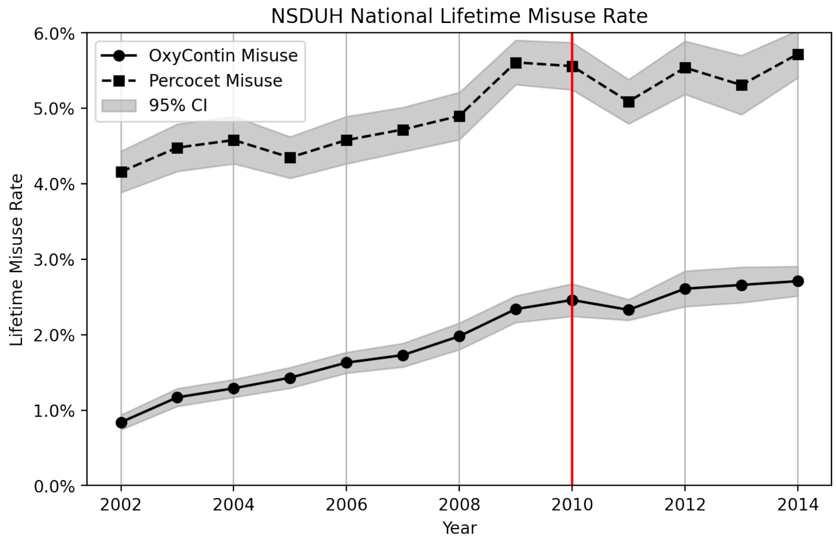
1.7 Appendix

Additional Information



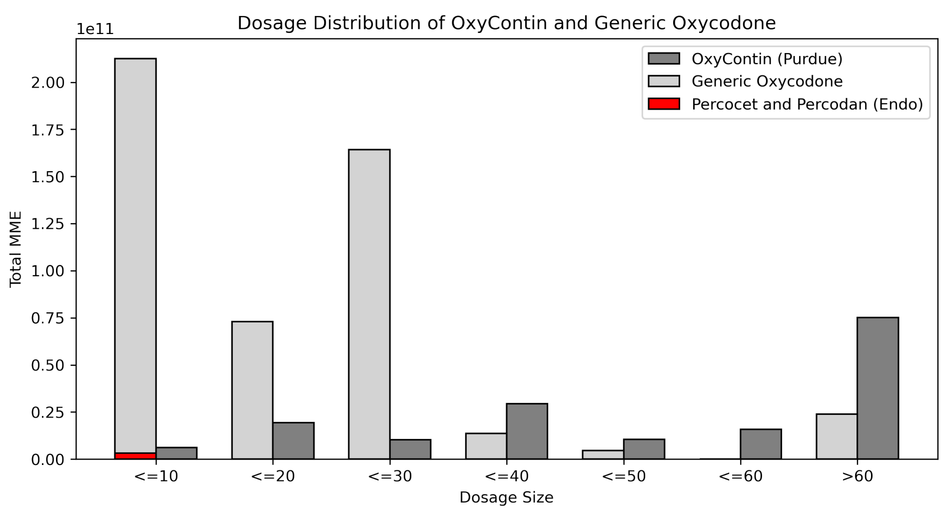
Note: We compute market share based on the average of 2006-2014 sales data. We kept only the top twenty manufacturers for better readability of the table. The rest of the 35 manufacturers combined contribute 0.18% of total sales. During this sample period, Purdue Pharma was the dominant manufacturer of high dosage oxycodone pills ($\geq$ 40mg). In the lower dosage market, three manufacturers (SpecGx, Actavis Pharma, and Par Pharmaceutical) had higher share of the market than Purdue Pharma.

Figure 1.7: Market share of different opioid manufacturers



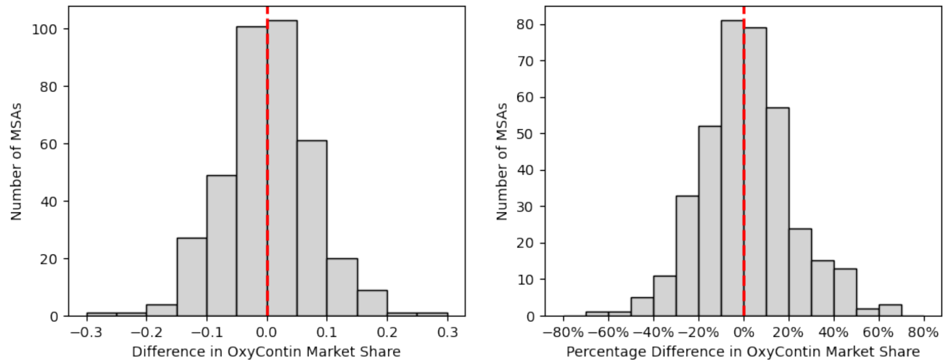
Note: The figure shows the misuse rate of OxyContin (OXYFLAG or OXYCONT2) and the misuse rate of Percocet, Percodan and Tylox (PERCTYL2). Data obtained from annual NSDUH. Percocet was a popular prescription oxycodone to misuse in the pre-OxyContin period. We see in this graph that the PERCTYL2 misuse rate increased 30% from 2002 to 2009, suggesting that the lifetime misuse rate captures more than historical Percocet, Percodan and Tylox misuse.

Figure 1.8: NSDUH national lifetime misuse rate



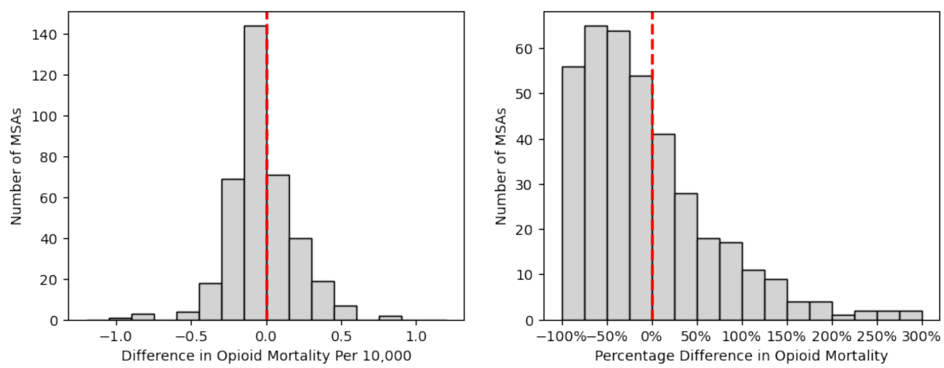
Note: This graph shows the difference in oxycodone sales between Purdue and Endo Pharma. The small market share of Endo Pharma leads us to believe that individuals misreport the drugs they consume on the NSDUH.

Figure 1.9: Comparison of sales of Purdue and Endo Pharma



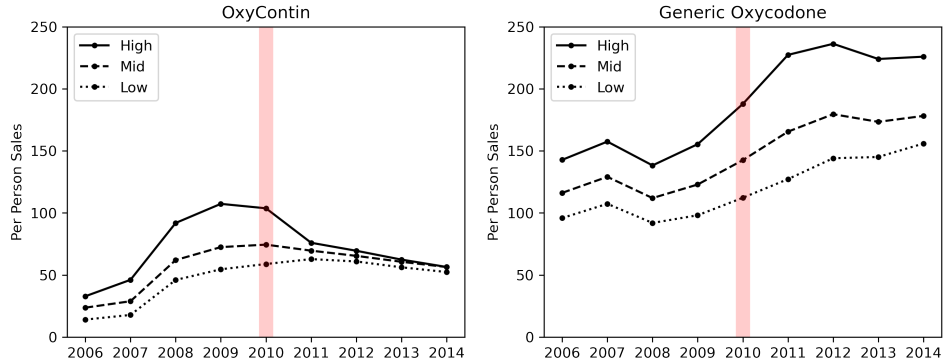
Note: Left is the absolute difference in market share (0.1 means that MSA share is 10% higher than the state average) and right is percentage difference (10% means that MSA share is 1.1 times the state average).

Figure 1.10: Within-state variation in OxyContin market share



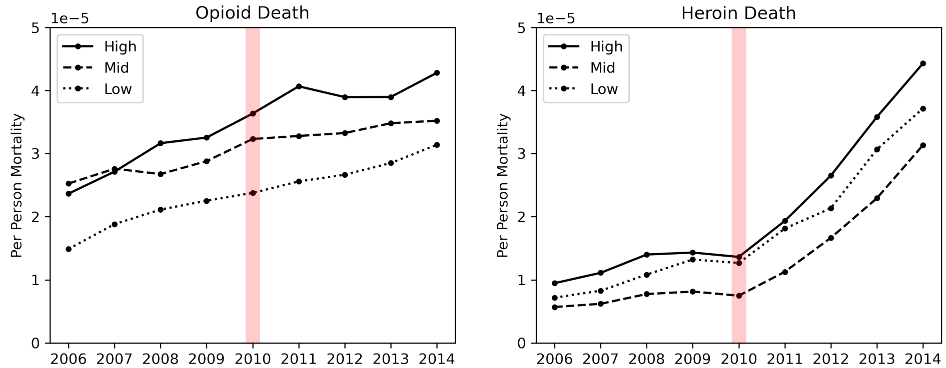
Note: Left is the absolute difference in opioid mortality (0.1 means that MSA mortality per 10,000 people is 0.1 higher than the state average) and right is percentage difference (10% means that MSA mortality per 10,000 people is 1.1 times the state average)

Figure 1.11: Within-state variation in opioid mortality



Note: We categorized all MSAs into high, mid, and low by the drop in the observed per person OxyContin sales from 2009 to 2011. The series are population weighted and Florida is excluded. The high group saw a 30% drop in OxyContin sales, the mid group a 3.9% drop, and the low group a 15% increase. The high group experienced a 46% increase in generic oxycodone sales, the mid group a 34% increase, and the low group a 29% increase. The three groups share similar oxycodone growth trends until the reformulation.

Figure 1.12: Opioid sales by empirical OxyContin drop



Note: Similarly to the previous figure, we categorized all MSAs into high, mid, and low by the drop in the observed per person OxyContin sales from 2009 to 2011. The series are population weighted and Florida is excluded. No trend break in opioid mortality in the high drop group. The high group saw a 35% increase in heroin mortality, the mid group 38%, and the low group 37%. The similar increases in heroin mortality post-reform indicate that drops in OxyContin use post-reform did not lead to additional increase in heroin use.

Figure 1.13: Opioid mortality by empirical OxyContin drop

Table 1.2: Testing constructed exposure measure against opioid mortality

	Opioid overdose deaths per 100,000					
	OxyContin			Generic Oxycodone		
	(1)	(2)	(3)	(4)	(5)	(6)
NSDUH misuse	10.235			2.909		
	(1.719)			(0.570)		
ARCOS sales		0.001			0.001	
		(0.0002)			(0.0001)	
Combined exposure			0.093			0.087
			(0.012)			(0.009)
Number of observations	379	379	379	379	379	379
R-square	0.086	0.089	0.130	0.065	0.178	0.189
Adjusted R-square	0.084	0.086	0.128	0.062	0.176	0.187

Notes: Standard errors are in parentheses. We report coefficients from OLS regressions of opioid mortality on misuse, sales, or exposure. NSDUH misuse rate is the 6-year average OxyContin or Percocet lifetime misuse rate from the pre-reform period (2004-2009). ACROS sales is Oxycontin or generic oxycodone sales per person from 2009. Combined exposure is the product of the previous two measures normalized (see equation 1.1). Overdose from 2009. Regressions are weighted by MSA population.

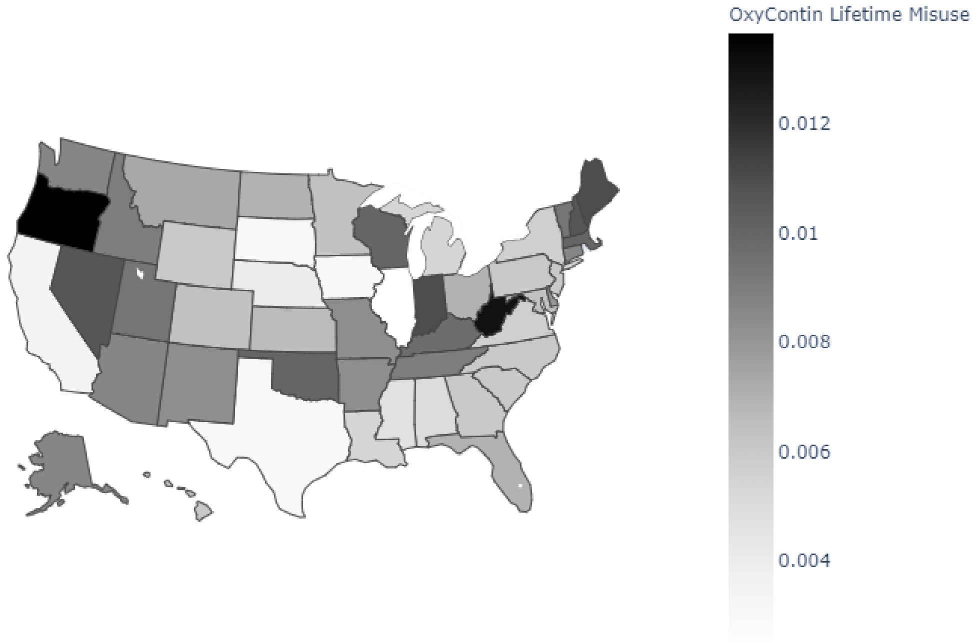
Table 1.3: Difference in difference regression results

	Opioid sales per person		Overdose per 10,000	
	OxyContin	Oxycodone	Opioid	Heroin
	(1)	(2)	(3)	(4)
Post	-8.05	41.74	0.01	0.14
	(2.86)	(4.92)	(0.02)	(0.02)
High OxyContin	47.24	56.46	-0.05	-0.07
	(5.78)	(13.36)	(0.03)	(0.02)
High Oxycodone	26.84	95.90	0.14	0.08
	(6.66)	(15.47)	(0.04)	(0.05)
Post x High OxyContin	-15.14	10.30	0.02	0.03
	(6.39)	(8.90)	(0.02)	(0.02)
Post x High Oxycodone	-2.33	33.99	0.06	0.07
	(6.37)	(8.80)	(0.02)	(0.02)
Number of observations	2148	2148	2148	2148
R-square	0.665	0.737	0.517	0.469
Adjusted R-square	0.654	0.728	0.501	0.452

Notes: We report coefficients from the difference-in-difference estimation (see Equation 1.3). All MSAs in Florida are excluded. In all specifications, we include MSA-level control variables, state fixed effects and year fixed effects. Standard errors are clustered at the MSA level.

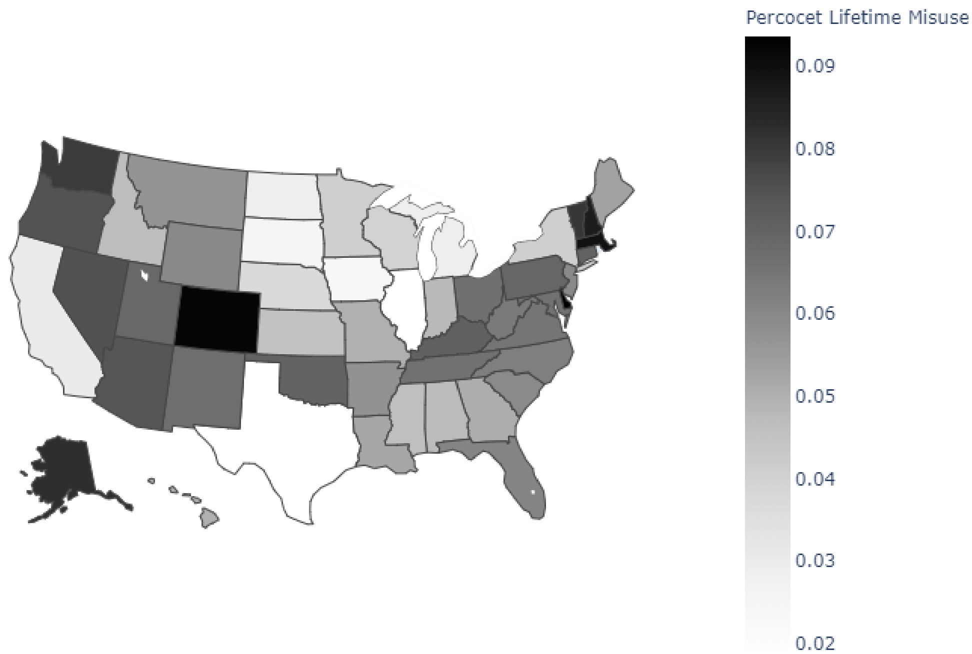
Tables

Map



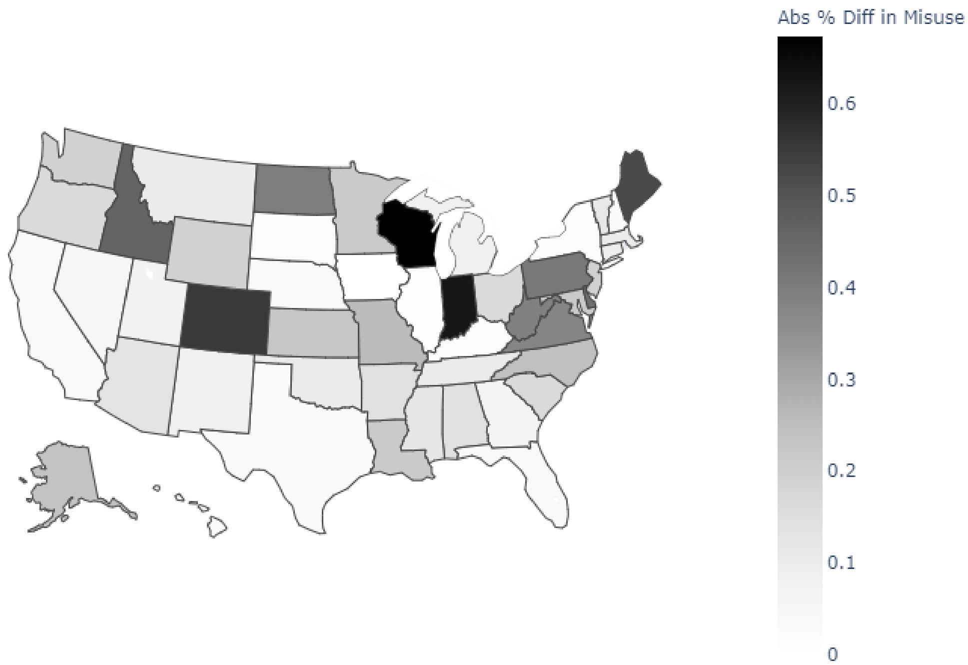
Note: Data from 2004-2009 NSDUH lifetime OxyContin misuse rate (NSDUH ticker OXXYR). 0.01 is interpreted as 1% of the state population has ever misused OxyContin.

Figure 1.14: OxyContin lifetime misuse rate at state level



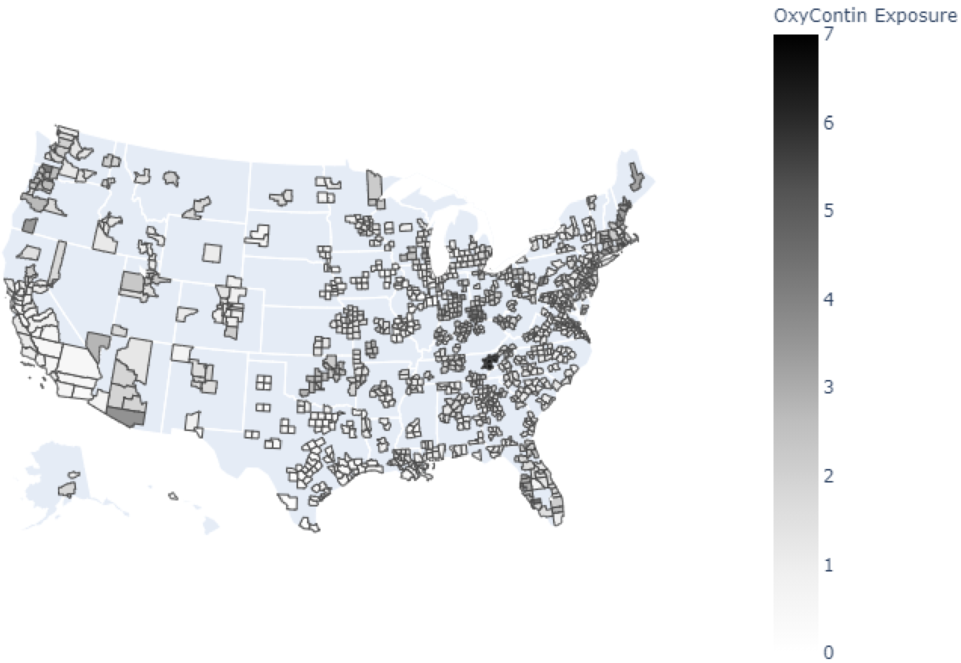
Note: Data from 2004-2009 NSDUH lifetime Percocet, Percodan, Tylox misuse rate (NSDUH ticker PERCTYL2). 0.01 is interpreted as 1% of the state population has ever misused one of the three drugs. Percocet lifetime misuse rate on average is much higher than OxyContin lifetime misuse rate.

Figure 1.15: Percocet lifetime misuse rate at state level



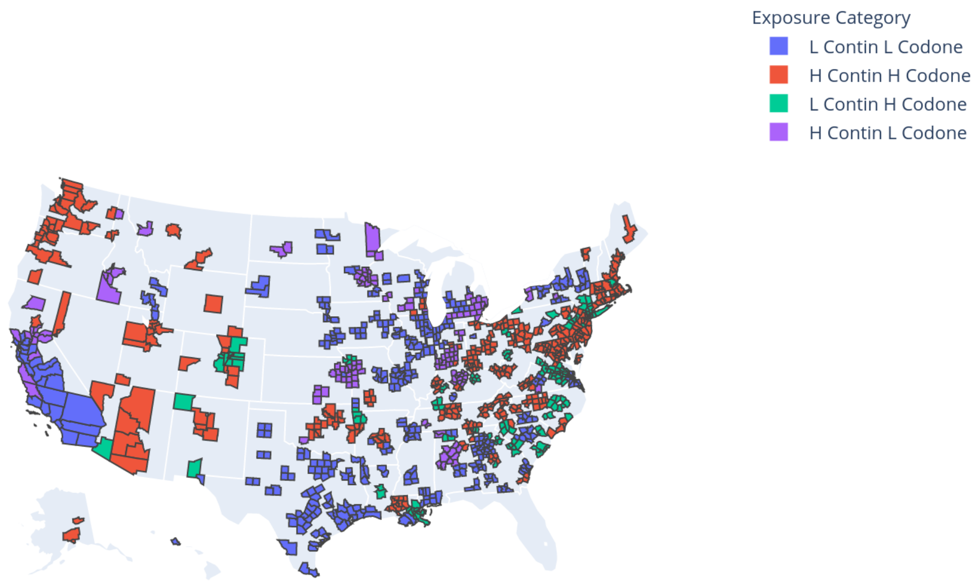
Note: The figure plots the absolute difference in percentile ranking of the two state level lifetime misuse rates. A 0.1 should be interpreted as a 10% difference in percentile ranking between OxyContin lifetime misuse rate and Percocet lifetime misuse rate. For example, Colorado's OxyContin misuse rate is 0.0063 (42 percentile) and its Percocet misuse rate is 0.092 (97 percentile), which is a 55% difference in percentile ranking. We rely on the difference between the two misuse rates to separately identify the impact of OxyContin and oxycodone.

Figure 1.16: Difference in state level misuse rates



Note: This figure shows OxyContin exposure by MSA. We show Florida here, which had very low OxyContin exposure/sales, but omit it from analysis because it had abnormally high generic oxycodone sales with large amounts being trafficked to other states.

Figure 1.17: OxyContin exposure at MSA level



Note: Florida is excluded in this analysis. MSAs grouped by high vs. low OxyContin exposure and high vs. low generic oxycodone exposure.

Figure 1.18: Diff-in-diff regression categories

Alternative Regression Specifications

MSA FE

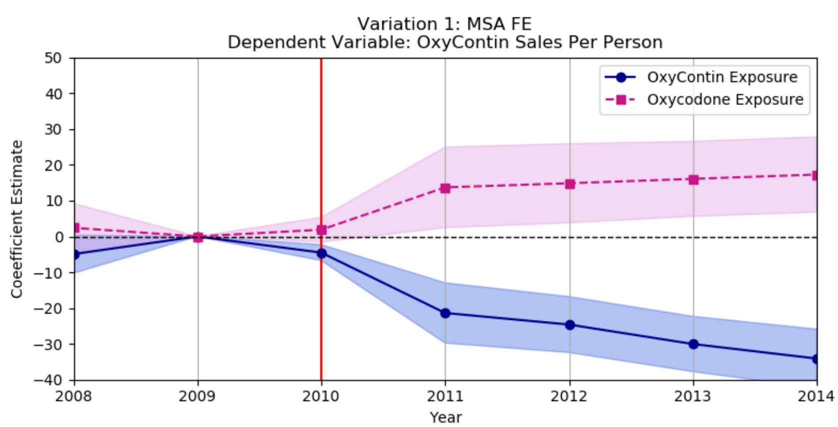


Figure 1.19: Regression on OxyContin sales with MSA FE

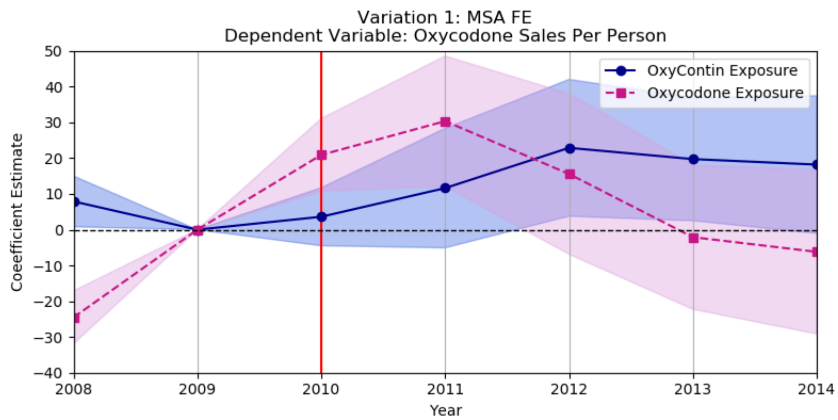


Figure 1.20: Regression on oxycodone sales with MSA FE

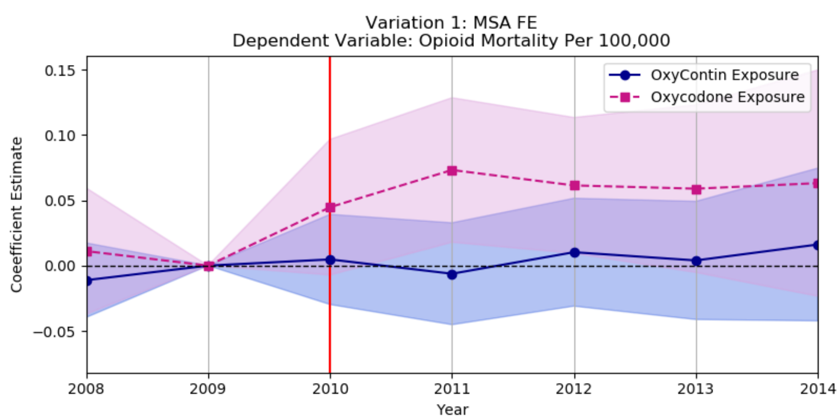


Figure 1.21: Regression on opioid mortality with MSA FE

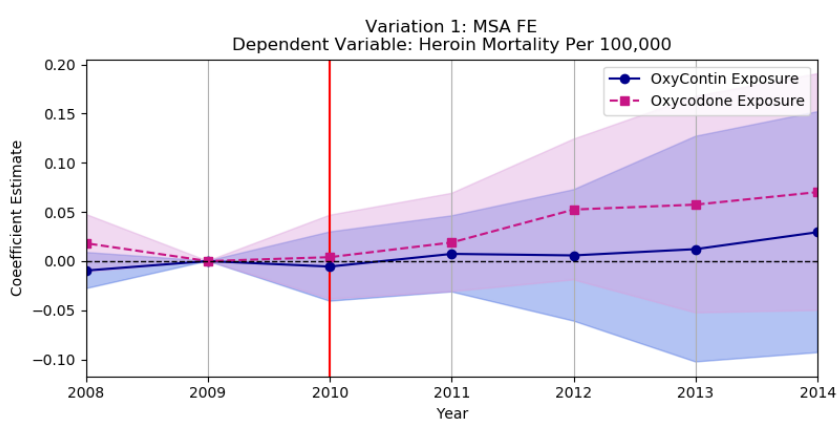


Figure 1.22: Regression on heroin mortality with MSA FE

Last Year OxyContin Misuse

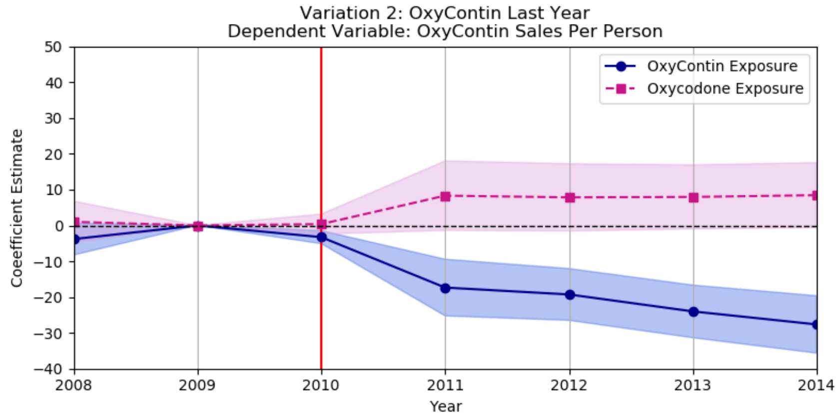


Figure 1.23: Regression on OxyContin sales with last-year OxyContin

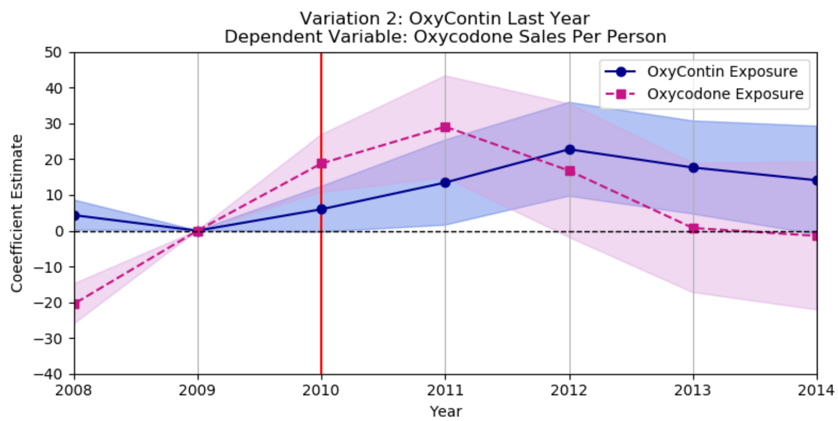


Figure 1.24: Regression on oxycodone sales with last-year OxyContin

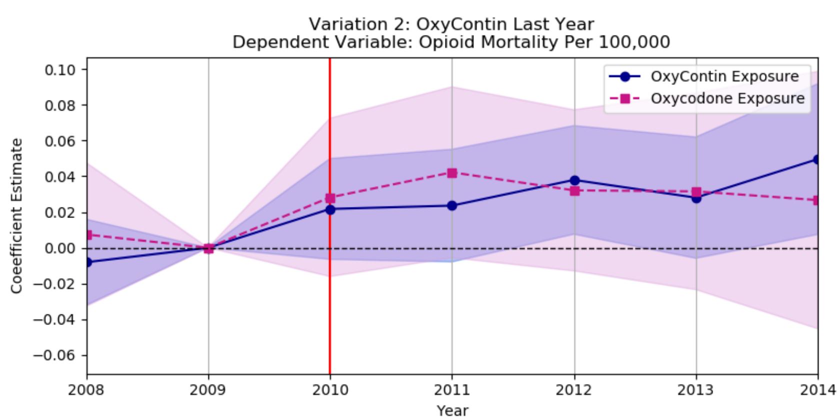


Figure 1.25: Regression on opioid mortality with last-year OxyContin

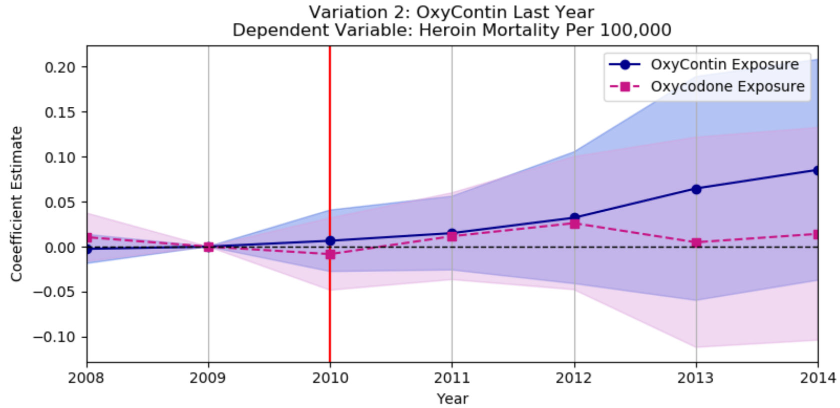


Figure 1.26: Regression on heroin mortality with last-year OxyContin

State Level Regression

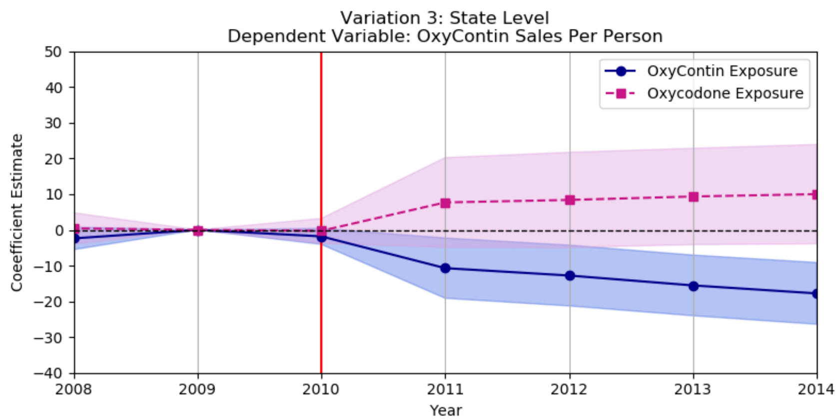


Figure 1.27: Regression on OxyContin sales at state level

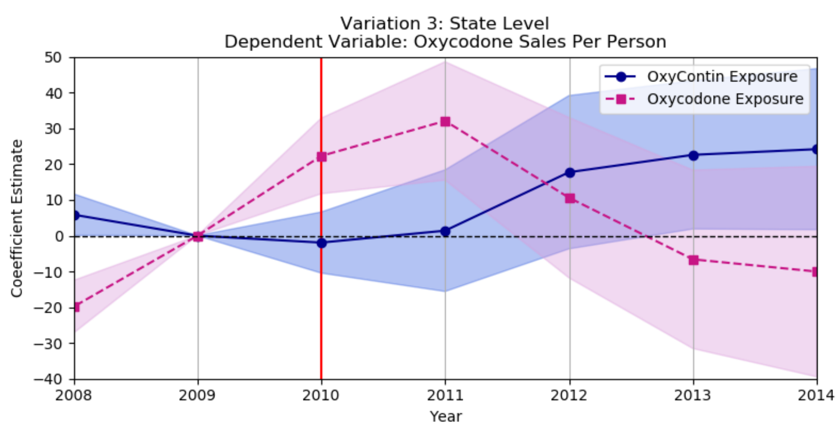


Figure 1.28: Regression on oxycodone sales at state level

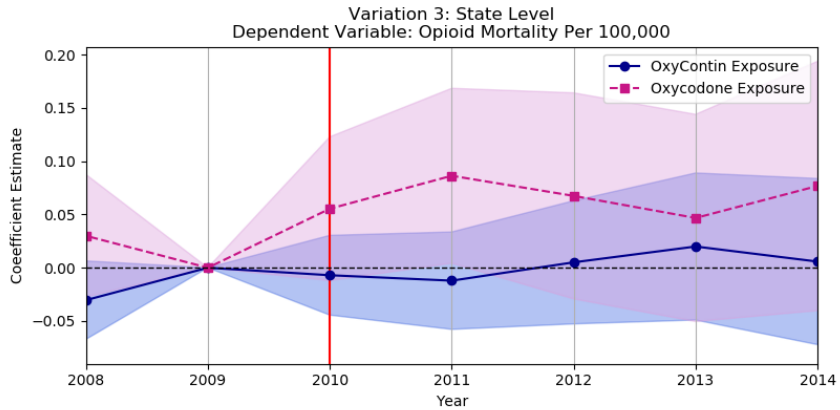


Figure 1.29: Regression on opioid mortality at state level

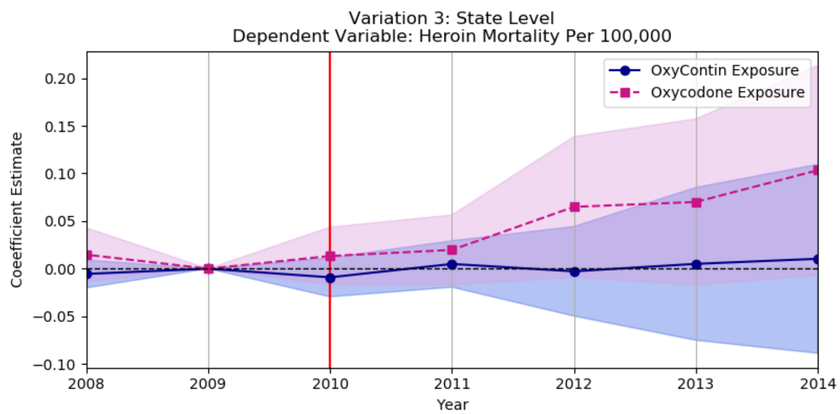


Figure 1.30: Regression on heroin mortality at state level

OxyContin Only

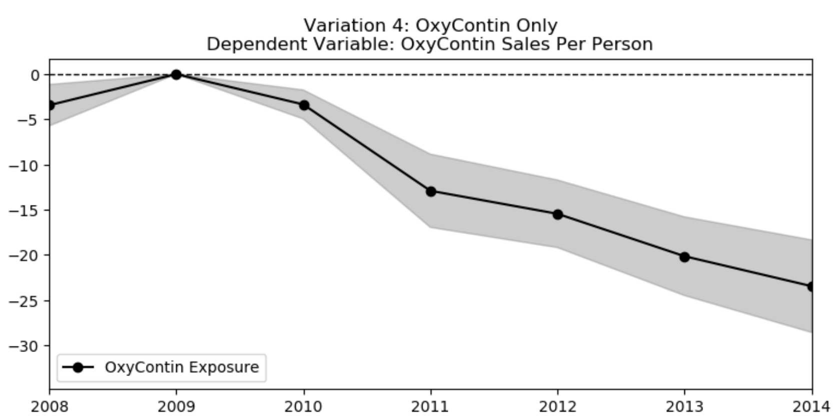


Figure 1.31: Regression on OxyContin sales with OxyContin only

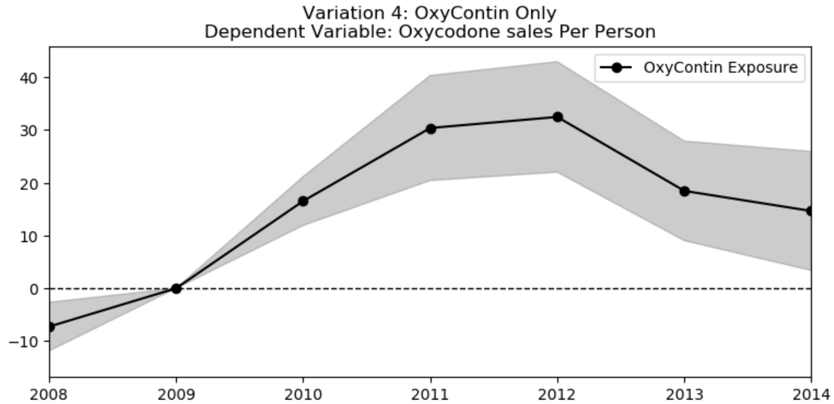


Figure 1.32: Regression on oxycodone sales with OxyContin only

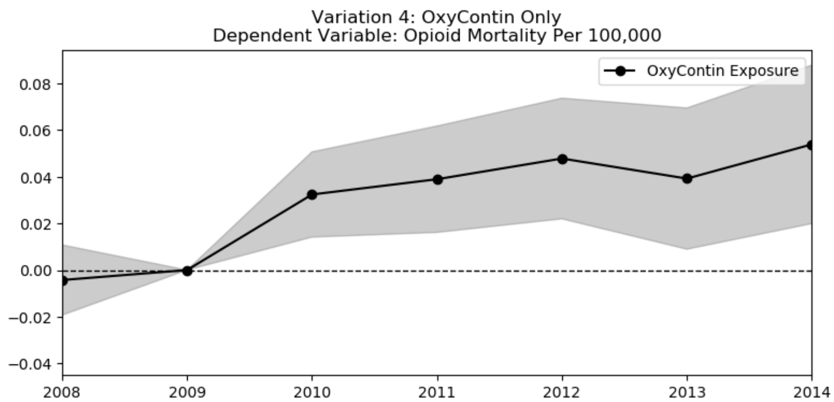


Figure 1.33: Regression on opioid mortality with OxyContin only

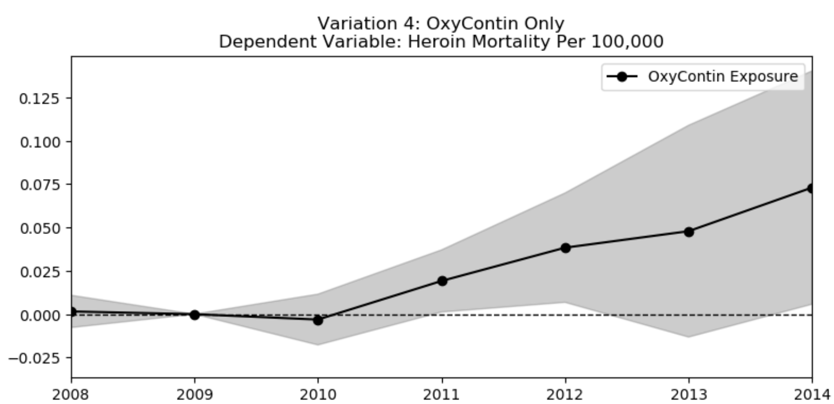


Figure 1.34: Regression on heroin mortality with OxyContin only

Oxycodone Only

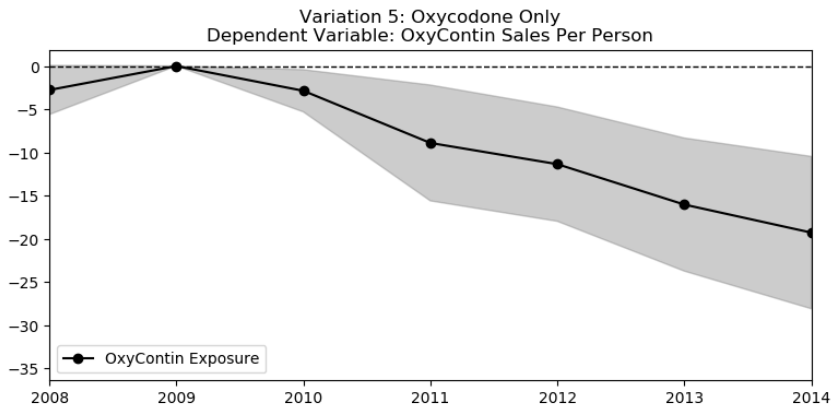


Figure 1.35: Regression on OxyContin sales with oxycodone only

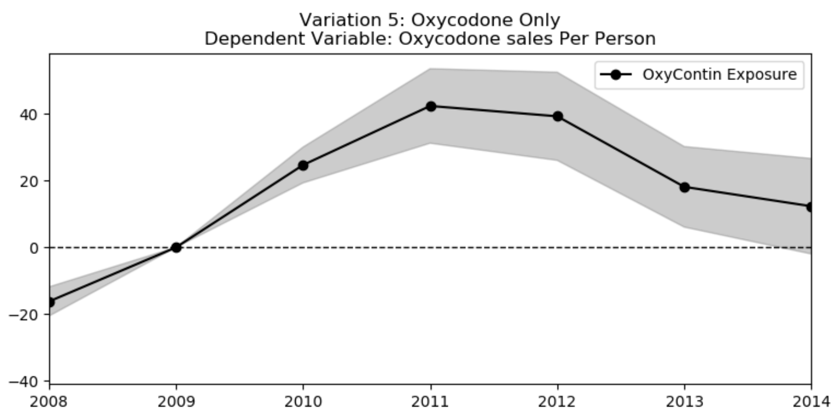


Figure 1.36: Regression on oxycodone sales with oxycodone only

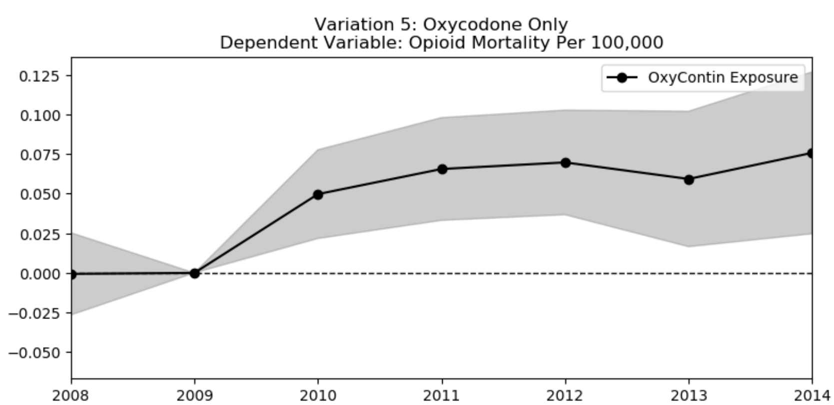


Figure 1.37: Regression on opioid mortality with oxycodone only

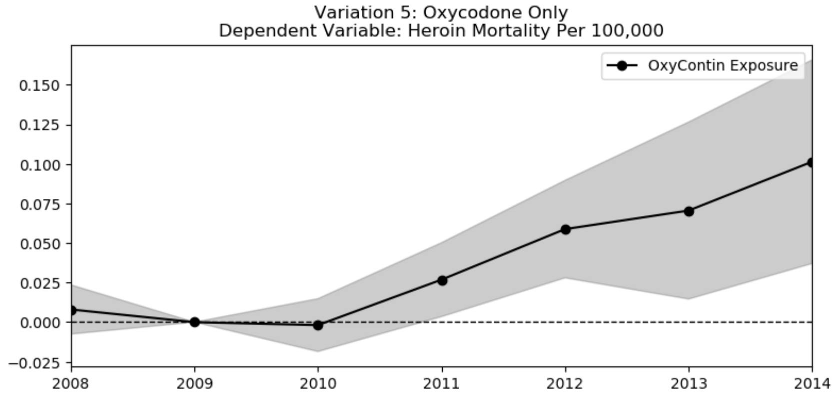


Figure 1.38: Regression on heroin mortality with oxycodone only

Bibliography

2005-2010 NSDUH MSA Detailed Tables. 2012.

Adler, Jeremy A and Theresa Mallick-Searle. 2018. "An overview of abuse-deterrent opioids and recommendations for practical patient care." *Journal of Multidisciplinary Healthcare* 11:323.

Alpert, Abby, David Powell and Rosalie Liccardo Pacula. 2018. "Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids." *American Economic Journal: Economic Policy* 10(4):1–35.

Atluri, Sairam, G Sundarshan and Laxmaiah Manchikanti. 2014. "Assessment of the trends in medical use and misuse of opioid analgesics from 2004 to 2011." *ASIPP* 17:E119–E28.

Bloomquist, Edward R. 1963. "The addiction potential of oxycodone (Percodan®)." *California Medicine* 99(2):127.

Carpenter, Christopher S, Chandler B McClellan and Daniel I Rees. 2017. "Economic conditions, illicit drug use, and substance use disorders in the United States." *Journal of Health Economics* 52:63–73.

Cassidy, Theresa A, Pronabesh DasMahapatra, Ryan A Black, Matthew S Wieman and Stephen F Butler. 2014. "Changes in prevalence of prescription opioid abuse after introduction of an abuse-deterrent opioid formulation." *Pain Medicine* 15(3):440–451.

Cicero, Theodore J, Matthew S Ellis, Hilary L Surratt and Steven P Kurtz. 2014. "The changing face of heroin use in the United States: a retrospective analysis of the past 50 years." *JAMA Psychiatry* 71(7):821–826.

Cicero, Theodore and Matthew Ellis. 2015. "Abuse-deterrent formulations and the prescription opioid abuse epidemic in the United States: lessons learned from OxyContin." *JAMA Psychiatry* 72(5):424–430.

Cicero, Theodore, Matthew Ellis and Hilary Surratt. 2012. "Effect of abuse-deterrent formulation of OxyContin." *New England Journal of Medicine* 367(2):187–189.

Compton, Wilson M, Christopher M Jones and Grant T Baldwin. 2016. "Relationship between nonmedical prescription-opioid use and heroin use." *New England Journal of Medicine* 374(2):154–163.

Coplan, Paul M, Hrishikesh Kale, Lauren Sandstrom, Craig Landau and Howard D Chilcoat. 2013. "Changes in oxycodone and heroin exposures in the National Poison Data System after introduction of extended-release oxycodone with abuse-deterrent characteristics." *Pharmacoepidemiology and Drug Safety* 22(12):1274–1282.

Dasgupta, Nabarun, Kathleen Creppage, Anna Austin, Christopher Ringwalt, Catherine Sanford and Scott K Proescholdbell. 2014. "Observed transition from opioid analgesic deaths toward heroin." *Drug and Alcohol Dependence* 145:238–241.

DeWeerdt, Sarah. 2019. "Tracing the US opioid crisis to its roots." *Nature* 573(7773):S10–S10.

Evans, William, Ethan Lieber and Patrick Power. 2019. "How the reformulation of OxyContin ignited the heroin epidemic." *Review of Economics and Statistics* 101(1):1–15.

Finkelstein, Amy. 2007. "The aggregate effects of health insurance: Evidence from the introduction of Medicare." *The Quarterly Journal of Economics* 122(1):1–37.

Havens, Jennifer R, Carl G Leukefeld, Angela M DeVeaugh-Geiss, Paul Coplan and Howard D Chilcoat. 2014. "The impact of a reformulation of extended-release oxycodone designed to deter abuse in a sample of prescription opioid abusers." *Drug and Alcohol Dependence* 139:9–17.

Hays, Lon R. 2004. "A profile of OxyContin addiction." *Journal of Addictive Diseases* 23(4):1–9.

Inciardi, James, Hilary Surratt, Theodore Cicero, Steven Kurtz, Steven Martin and Mark Parrino. 2009. "The "black box" of prescription drug diversion." *Journal of Addictive Diseases* 28(4):332–347.

Kennedy-Hendricks, Alene, Matthew Richey, Emma McGinty, Elizabeth Stuart, Colleen Barry and Daniel Webster. 2016. "Opioid overdose deaths and Florida's crackdown on pill mills." *American Journal of Public Health* 106(2):291–297.

Leung, Pamela TM, Erin M Macdonald, Matthew B Stanbrook, Irfan A Dhalla and David N Juurlink. 2017. "A 1980 letter on the risk of opioid addiction." *New England Journal of Medicine* 376(22):2194–2195.

Macy, Beth. 2018. *Dopesick: Dealers, doctors, and the drug company that addicted America*. Little, Brown.

Mallatt, Justine. 2018. "The effect of prescription drug monitoring programs on opioid prescriptions and heroin crime rates." *Available at SSRN 3050692* .

Mars, Sarah G, Philippe Bourgois, George Karandinos, Fernando Montero and Daniel Ciccarone. 2014. ""Every 'never' I ever said came true": transitions from opioid pills to heroin injecting." *International Journal of Drug Policy* 25(2):257–266.

Meier, Barry. 2003. *Pain killer: A" wonder" drug's trail of addiction and death*. Rodale.

Meier, Barry. 2007. “In guilty plea, OxyContin maker to pay $600 million.” *New York Times* 10.

Modarai, F, K Mack, P Hicks, S Benoit, S Park, C Jones, S Proescholdbell, A Ising and L Paulozzi. 2013. “Relationship of opioid prescription sales and overdoses, North Carolina.” *Drug and Alcohol Dependence* 132(1-2):81–86.

Origins of the Opioid Crisis and Its Enduring Impacts. N.d. . Forthcoming.

O’Donnell, Julie, Matthew Gladden and Puja Seth. 2017. “Trends in deaths involving heroin and synthetic opioids excluding methadone, and law enforcement drug product reports, by census region—United States, 2006–2015.” *Morbidity and Mortality Weekly Report* 66(34):897.

Paulozzi, Leonard J and George W Ryan. 2006. “Opioid analgesics and rates of fatal drug poisoning in the United States.” *American Journal of Preventive Medicine* 31(6):506–511.

Pergolizzi, Joseph, Robert Raffa, Robert Taylor Jr and Steven Vacalis. 2018. “Abuse-deterrent opioids: an update on current approaches and considerations.” *Current Medical Research and Opinion* 34(4):711–723.

Quinn, Wm F. 1965. “Percodan on Triplicate—The Background of the New Law.” *California Medicine* 103(3):212.

Quinones, Sam. 2015. *Dreamland: The true tale of America’s opiate epidemic*. Bloomsbury Publishing USA.

Rappaport, Bob. 2009. “Application number: 22-272 - medical review(s).”.

Ruhm, Christopher J. 2017. “Geographic variation in opioid and heroin involved drug poisoning mortality rates.” *American Journal of Preventive Medicine* 53(6):745–753.

Schnell, Molly. 2017. “Physician behavior in the presence of a secondary market: The case of prescription opioids.”.

Siegal, Harvey A, Robert G Carlson, Deric R Kenne and Maria G Swora. 2003. “Probable relationship between opioid abuse and heroin use.” *American Family Physician* 67(5):942.

Sproule, Beth, Bruna Brands, Selina Li and Laura Catz-Biro. 2009. “Changing patterns in opioid addiction: characterizing users of oxycodone and other opioids.” *Canadian Family Physician* 55(1):68–69.

Swensen, Isaac D. 2015. “Substance-abuse treatment and mortality.” *Journal of Public Economics* 122:13–30.

Van Zee, Art. 2009. "The promotion and marketing of OxyContin: commercial triumph, public health tragedy." *American Journal of Public Health* 99(2):221–227.

Webster, Lynn. 2009. "Update on abuse-resistant and abuse-deterrent approaches to opioid formulations." *Pain Medicine* 10(suppl_2):S124–S133.

Chapter 2

GEOGRAPHIC SPILLOVER EFFECTS OF PRESCRIPTION DRUG MONITORING PROGRAMS (PDMPS)

2.1 Introduction

Over the past two decades, the opioid epidemic has claimed more than 415,000 American lives (CDC Wonder). To stem the rising tide of opioid misuse, in the early 2000s, states began to regulate prescription opioid sales. Among the different policies that were implemented, we focus on Prescription Drug Monitoring Programs (PDMPs) that require prescribers and dispensers to submit data to a centralized system. In this paper, we study the effects of states' implementation of electronic-access PDMPs, a version of the law that allows doctors and pharmacists to query the patient's prescription history in real-time, on different regions in the same state and on the nearby states. Specifically, we focus on how sales in counties that border other states react differently to new PDMP regulations from sales in 'inland' counties.

Our analysis shows that electronic-access PDMPs reduce prescription opioid sales and opioid mortality. The effect is economically and statistically significant despite the fact that endogenous adoptions of such regulations bias our estimates of their impact downward. We find that border counties (counties that are immediately adjacent to another state) are systematically different from inland counties (counties not immediately adjacent to a county in a different state) and the enactment of ePDMP laws disproportionately affects border counties. These findings are consistent with our hypothesis that the border counties are destinations for consumers who are doctor or pharmacy shopping due to their proximity to another state. We also find a small but significant spillover effect in the form of increased opioid sales and overdose deaths when the neighboring state adopts stricter PDMP regulations.

Using the novel ARCOS data, we confirm the literature's general finding that PDMPs reduce opioid sales and mortality. We also contribute to resolving a debate in the literature about what features of PDMPs are more effective than others. We find that one specific implementation, electronic-access PDMPs (ePDMPs), are most effective at reducing opioid sales and mortality. Compared to a regular PDMP, this version not only requires doctors to submit information, but also allows doctors to see what other opioids a patient has received in real time. To the extent doctors and pharmacists consult the databases, ePDMPs mitigate the problem of individuals going to multiple doctors or pharmacies to secure opiates. We find that ePDMP laws reduce per person sales by 0.006 mg in activate ingredient weight[1], which is equivalent to a 5.6% drop from the 2006 national average, and per 100,000 mortality by 0.279, which is equivalent to a 12.3% from the 2006 national average.

We perform our analyses at the county level, which allows us to measure systemic differences in opioid markets of inland versus border counties due to the presence of state borders. Border counties appear similar to inland counties on observable demographics, but they have significantly higher opioid sales and lower opioid overdose deaths. These differences are consistent with the hypothesis that border counties are more frequently the destinations of doctor or pharmacy shopping, largely because their proximity to other states leads to lower travel costs for out-of-state residents. This difference between border counties and inland counties falls after the state adopts ePDMP, which further confirms our hypothesis that a higher percentage of sales in border counties were trafficked elsewhere for consumption. Our findings challenge the states-as-islands model often assumed in the opioid literature.

We also document negative externalities from these ePDMP laws, in the form of opioid sales and mortality increases in the border counties of neighboring states. We argue that these externalities come from the demand-side response of individuals using opioids, who now acquire these prescription drugs from out-of-state. The substitution to opioids from other states potentially reduces the effectiveness of ePDMPs as a policy intervention. The spatial substitution identified in this paper builds upon our previous work (Zhang and Guth, 2021) showing that partial supply-side interventions, like the OxyContin reformulation, can lead to drug substitution instead of preventing misuse. In the case of ePDMPs, the policy intervention was at the state and not the national level, so sales shifted across state lines instead of across products. This paper adds to the growing literature on the side effects of supply-side intervention curbing the opioid crisis (Alpert, Powell and Pacula, 2018; Kim, 2021).

Our work speaks to the importance of not analyzing individual state policies in a vacuum. Individuals frequently cross these invisible borders in their day-to-day

[1] The active ingredient weight is equivalent to the morphine milligram equivalent (MME) divided by 1500.

lives, and they may thus be subject to different regulatory regimes. The ability for individuals to evade one state's regulations for another extends to all markets regulated at the state level. One of the policy implications of our work is that there are costs to regulating opioids at the state level, and there would be benefits in enacting a national ePDMP. The American College of Physicians has called for a national prescription drug monitoring program and for standardized PDMP laws across states until that point (Kirschner, Ginsburg and Sulmasy, 2014).

The rest of the paper is as follows. Section 2.2 gives a background on PDMP laws as well as an overview of the literature understanding their effects. Section 2.3 describes the county-level sales and mortality data we use, the spread of PDMP laws during this time period, and our categorization of border counties. Section 2.4 describes how we model PDMP-border counties as well as our predictions based on economic theory and known trafficking patterns. Section 2.5 provides our results on sales and mortality, and finally, Section 2.6 concludes.

2.2 Background and Literature Review

Our paper connects three different strands of literature. First, we contribute to the literature on the opioid crisis and policies curbing opioid misuse. Second, we take methods from spatial economics and apply them to cross-border opioid sales and misuse. Third but not least, we build upon modern analyses of the effects of PDMP laws.

The Opioid Crisis and Interventions

Over the past two decades, millions of Americans have misused prescription opioids. In 2019 alone, 1.6 million people had an opioid use disorder and 70,630 people died from an opioid overdose[2]. Opioid use disorder has devastating consequences for the individual, the family, and the community. The CDC estimates the total "economic burden" of prescription opioid misuse to be 78 billion dollars a year.

Many victims of the epidemic got their first access to an opioid from a doctor's prescription. Previous research has documented large variations in opioid prescribing and sales, both within and across states. McDonald, Carlson and Izrael (2012) shows that the ratio of per-capita oxycodone sales in counties in the 75th percentile to counties in the 25th percentile is approximately 7 to 1. Their best model can only predict one-third of the variation in sales by county. Finkelstein, Gentzkow

[2]The US Department of Health and Human Services on the Opioid Epidemic (link)

and Williams (2018) uses Medicare data to track individuals who move between counties, and the paper finds that location has a noticeable effect on an individual's access to opioids. The paper estimates that 30% of the difference in opioid prescribing between counties can be explained by these place-specific factors. Our work is connected to the opioid prescription literature in that we both study location-specific effects, but our data is on opioid shipments to pharmacies which occurs further down the prescription pipeline. We add to this literature by showing that being on the state border is one of these factors that affect local opioid sales and misuse.

Over the last two decades, states have made repeated attempts to regulate the sales of prescription opioids in the hope of preventing further opioid misuse. Litigation against Purdue Pharma, the manufacturer of the drug that ignited the opioid crisis, led the company to reformulate OxyContin in 2010. The reformulation led to reduced sales of OxyContin, but spurred on an increase in alternative oxycodone and heroin misuse (Alpert, Powell and Pacula, 2018; Evans, Lieber and Power, 2019; Zhang and Guth, 2021). Many states enacted new PDMP laws or tightened existing ones. The evidence of the effectiveness of such laws is mixed (for more detail see Section 2.2), and some argue that the new restrictions led to increases in heroin mortality (Dave, Deza and Horn, 2021; Kim, 2021). Some states, Florida included, passed legislation that requires pill mills—rogue pain management clinics that were inappropriately prescribing and dispensing opioids—to register with the state. The pill mill laws have led to a moderate decrease in opioid prescription and use (Kennedy-Hendricks et al., 2016; Rutkow et al., 2015). One common theme in this strand of the literature is a substitution toward alternative drugs when the original supply became restricted. We add to this literature by evaluating the effectiveness of PDMP laws while taking into consideration potential spatial spillovers.

Spatial Spillover and Opioids

Our work ties tightly into the literature studying the distribution of economic activities across space. Many works have noted how geographic characteristics have a direct impact on manufacturing, sales, and trade. Holmes (1998) finds sharp increases in manufacturing activity across the border in so-called 'pro business' states. Similarly, Nachum (2000) finds that location and agglomeration effects can explain which states transnational corporations choose to put their headquarters in. Fox (1986) examines border counties and finds that changes in state taxes can shift purchases across state lines. Garrett and Marsh (2002) examines lottery sales in Kansas and estimates that the state lost $10.5 million dollars in net lottery revenue to cross-border shopping in 1998. We use border counties, a concept from this literature, to show how state policy differentially affects different locations. Our setting provides the perfect environment to test for spillovers because we have detailed sales data on exactly where opioids are sold, which is not common in other settings.

We also contribute to a small but significant literature on cross-border prescription shopping. Crossing state and national borders to take advantage of favorable regulatory environments to obtain drugs is not a new concept in the literature. Casner and Guerra (1992) documents patients crossing the US-Mexico border to purchase prescription medication cheaply and without a prescription. McDonald and Carlson (2014) estimates that approximately 30% of "doctor shoppers" had opioid prescriptions from multiple states. Cepeda et al. (2013) finds that 4% of non-shoppers visited more than one state to purchase opioids, and for individuals who visited multiple pharmacies to purchase opioids, the median distance between pharmacies was about 12.6 miles. We add to this literature by leveraging decentralized policy change to systematically identify the impact of cross-border shopping on opioid sales.

Prescription Drug Monitoring Programs

Before any Prescription Drug Monitoring Programs, individuals could freely doctor or pharmacy shop[3], and there was no tools for doctors or pharmacists to know how many other prescriptions an individual has. PDMPs are state-level databases that track controlled substance prescriptions in a state. The modern precursor to the PDMP was California's 'Triplicate Prescription Program' enacted in 1939[4]. The law required the dispensing pharmacist to fill out standardized forms for controlled substances and mail a copy to a centralized state repository. The California program set the blueprint for PDMPs and many states followed suit in subsequent decades. The legality of PDMPs was tested in *Whalen v. Roe*, where the Supreme Court unanimously ruled that storing this personal medical information did not violate a person's right to privacy.

These original PDMPs collected information from doctors and pharmacists via mail or fax, and doctors and pharmacists could not immediately query a patient's opioid history. Oklahoma implemented the first fully electronic PDMP in 1990 that directly and routinely sent records to a state database (Holmgren, Botelho and Brandt, 2020).

[3]Doctor shopping refers to the behavior of individuals going to multiple doctors to get opioid prescriptions to evade scrutiny, and pharmacy shopping refers to going to multiple pharmacies to get the prescriptions filled.

[4]New York had the first PDMP law in 1918, but rescinded it three years later.

Currently, the electronic-access PDMPs allow registered doctors and pharmacists to query the data set in real-time and see all opioids an individual received in that state. The 21st century saw a wave of expansion to electronic PDMPs and by 2019, all but one US state have implemented e-access PDMPs (Mallatt, 2019). The next wave of PDMP regulation is the must-access or mandatory PDMPs. These laws require doctors and pharmacists to check an individual's opioid history before dispensing opioids. Absent the mandate, only filling the information is mandatory; checking a patient's history is voluntary. The must-access laws are often based on, and enacted after, electronic PDMPs. By 2017, 19 states have enacted some version of must-access PDMPs.

Most states do not share any information collected from PDMPs with other states[5]. The lack of information sharing made it feasible for individuals to partially circumvent the regulation by shopping across state borders. If state A adopted an ePDMP, an individual would face greater difficulty getting a second opioid prescription filled in-state. This difficulty could occur either from doctors, who upon observing that a patient already has an opioid prescription do not write another, or from pharmacists who refuse to fill it for the same reason. However, an individual could attempt to get and fill a second prescription in a neighboring state. We aim to evaluate the propensity for individuals to get opioid prescriptions outside of their state, specifically to avoid PDMP regulations.

There is a wide array of studies on the effects of PDMPs. One typical corroborated result in the literature is that PDMPs decrease prescription opioid sales (Kilby, 2016; Reisman et al., 2009; Simeone and Holland, 2006) and reduce abuse and mortality (Patrick et al., 2016; Simeone and Holland, 2006). Some papers note that certain formulations of PDMPs are more effective than others. Effective features include monitoring more drugs and updating the database weekly (Patrick et al., 2016), and identifying and investigating cases proactively (Simeone and Holland, 2006). Bao et al. (2016) looked at 22 states from 2001 to 2010 that implemented electronic access to PDMPs and showed the implementation reduced oxycodone prescriptions from ambulatory visits to physician offices by 30%. A set of papers claim that only must-access PDMPs (MA-PDMPs) are effective in reducing opioid misuse (Buchmueller and Carey, 2018; Dave, Deza and Horn, 2021; Grecu, Dave and Saffer, 2019; Kim, 2021; Meinhofer, 2018) which conflicts with existing results

[5]Lin et al. (2019) shows that in 2014, 23 states had some sort of data sharing agreement, but many of these agreements were one-way, and only Michigan and Indiana shared this information with all of their neighboring states.

on the effectiveness of non-mandatory PDMPs. We contribute to this debate by showing that ePDMP laws were effective at reducing sales and overdose deaths during our sample period.

The disagreement in the literature on what features of PDMPs are more effective than others is partly the result of each paper employing its own categorization of laws and testing the effectiveness on different outcome variables. Assembling an accurate policy data set across all 50 states is inherently challenging (Schuler et al., 2021). Horwitz et al. (2018) point out that the inconclusive and contradictory results may be due to the large variations in dates used in different studies. Existing sources of enactment dates rarely acknowledge the researchers' decisions in creating such a data set, and the public sources have a large disagreement. In this paper, we use the "modern system operational date" variable from Horwitz et al. (2018) in our main analysis. We will elaborate on the choice of the "modern system operational date" over other implementation dates in Section 2.3.

States adopted PDMP policies at different times, but the literature generally does not address potential endogeneity concerns. For our regression specification, one particularly worries that states might be more likely to adopt PDMPs because they have the infrastructure to make the laws effective. If so, a naive regression's coefficients would be biased in favor of the hypothesis that the laws matter but such upward biases are unlikely. We argue that adoptions of PDMP laws are endogenous to local conditions but in ways that bias coefficients downwards towards zero rather than upwards. Specifically, places that are experiencing more opioid misuse or higher growths in sales or overdoses are the most likely to adopt measures like PDMPs. A simple difference-in-difference estimation of the effect of the law underestimates its impact and biases against the key hypotheses we want to test. Our estimation of the impact of PDMP laws on sales and mortality both suffer from this bias, but we are capturing statistically significant coefficients nonetheless. Moreover, since the enactment of PDMPs in a state is independent of the differences between border and inland counties in that state, and independent of conditions in nearby states, our estimation of the border effect and spillover will not be affected by the endogeneity problem.

2.3 Data

In this section, we introduce the data source of our sales and mortality data, describe our choice of PDMP implementation dates, define how we characterize border counties, and present summary statistics.

ARCOS Sales Data and NVSS Mortality Data

As part of the Controlled Substances Act, 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 pain pill sold in the United States. The complete database from 2006 to 2014 was recently released by a federal judge as a result of an ongoing trial in Ohio against opioid manufacturers.[6] The part of ARCOS that we use in this paper is shipments of oxycodone from manufacturers to pharmacies. In theory, the manufacturer to pharmacy shipments are not equivalent to sales to the consumers. However, since pharmacies do not keep large stocks of opioids, the aggregated annual data of sales from manufacturers to pharmacies is practically equivalent to the annual sales of pharmacies to consumer sales. The benefit of ARCOS data is that it allows disaggregation to arbitrarily fine geographical levels, which is essential for the identification of the border effects, and it contains all opioid sales which allows us to identify spatial substitution. The ARCOS sales data is the primary outcome variable in our regressions.

We care about how PDMP laws affect opioid sales, but ultimately we are interested in preventing their effects on overdoses and deaths. The second outcome of interest in our main regression is opioid mortality. We use the restricted-use multiple-cause mortality data from the National Vital Statistics System (NVSS) to track opioid overdoses. The dataset covers all deaths in the United States from 2006-2014. We follow the literature's two-step procedure to identify opioid-related deaths. First, we code deaths with ICD-10 external cause of injury codes: X40–X44 (accidental poisoning), X60–64 (intentional self-poisoning), X85 (assault by drugs), and Y10–Y14 (poisoning) as overdose deaths. Second, we use the drug identification codes, which provide information about the substances found in the body at death, to restrict non-synthetic opioid fatalities to those with ICD-10 code T40.2.

PDMP Enactment Dates

As discussed in the background section, there are multiple sets of PDMP enactment dates, and the literature disagrees on which is the most effective in reducing opioid misuse. In this paper, we consider three sets of dates: (a) the legislated start date (any PDMP), which is the year that dispensers or prescribers would be required to send

[6] Link to the ARCOS Data published by the Washington Post.

prescriptions to a central database, (b) the electronic access date (ePDMP), which is the year that the PDMP data becomes accessible to the dispensers or prescribers through a centralized electronic system, and (c) the must access date (MA-PDMP), which is the year when certain dispensers or prescribers are required to check an individual's opioid history before dispensing. In Figure 2.1 in Appendix, we graph the three enactment dates for each state. Most states started with the most basic version of the PDMP and gradually adopted e-access in the 2000s. Only a handful of states adopted must-access PDMP during our time period.

We use ePDMP dates in our main regression analysis. The reasons are twofold. First, ePDMPs have a large impact on prescriptions and sales both conceptually and empirically. Conceptually, an ePDMP streamlines the process by which the prescribers and dispensers check a patient's prescription history. Before an ePDMP, prescribers and dispensers were required to report opioid prescriptions but could not easily tell what other prescriptions an individual had. ePDMPs allows them to check a patient's opioid history online in real-time, so they could more easily refuse opioids to questionable patients. Although an ePDMP is less restrictive than a MA-PDMP, it is reasonable to assume that a large number of doctors who are conscious of the severity of the opioid crisis would have taken advantage of the electronic system when it became available. Empirically, Horwitz et al. (2018) finds this set of dates is most correlated with reductions in opioid sales after comparing it with 9 other sets of dates[7].

Second, our sample period has a higher coverage of enactment of ePDMP as compared to the other two dates. There is reasonable consensus in the literature that each wave of new PDMP legislation tightens the legal supply of opioids and reduces misuse (although the literature disagrees on which version is the most effective). Given that each round of legislation may have some impact, we want to work with the one that gives us the most identification power. The switch from no PDMP to any PDMP happened in the 1990s and early 2000s, and by 2006, the start of our sample period, 31 states have already adopted it. The adoption of ePDMPs took place mainly during our sample period: 37 states adopted ePDMP between 2006 and 2014. Only 10 states enacted MA-PDMP during our sample period. Working with ePDMPs allows us to use data from more states to estimate the impact of the law. Our β estimations would be less reliant on trends from a few states.

[7]The paper did not compare ePDMP with MA-PDMP.

Our ePDMP dates are obtained from Horwitz et al. (2018)[8]. We use Horwitz as our main source because this paper is the most systematic methodological paper on PDMP implementation timing that we have reviewed. In robustness, we use ePDMP dates published by the Prescription Drug Abuse Policy System (PDAPS), an organization funded by the National Institute on Drug Abuse to track state laws related to prescription opioid abuse. To check if other PDMP laws have similar spillover effects, we use any PDMP dates from Horwitz et al. (2018) and MA-PDMP dates from Sacks et al. (2021). We list all sets of dates in Table 2.4 in the Appendix.

Defining Border Counties and Assigning ePDMP Status

We define a border county as a county that neighbors at least one county in an adjacent state and an inland county as a county that borders only counties of the same state. After excluding Alaska, Hawaii[9], and Florida[10] from our data, we have 2906 counties, 37.3% of which are border counties (see Figure 2.2 in Appendix for a visual representation of border and inland counties). For each inland county, we document whether an ePDMP law has been implemented. For each border county, we document whether a law has been implemented in that state and the bordering state(s). If a county is bordering multiple states and these states have different ePDMP statuses, the nearby law of the county will be the ePDMP status under which the majority of the nearby population live[11]. We only need to do this calculation on 653 county-year observations, which is 6.6% of all border county-year observations. See Figure 2.3 in Appendix for an example of the calculation.

The transition from states not having an ePDMP to having an ePDMP is key to our identification. During our sample period, over 60% of all counties transitioned from no ePDMP to ePDMP (see adoption rate in Figure 2.4 in Appendix). Identification of border coefficients relies on law change in a county and law changes in nearby border counties. The majority of the transitions in border counties also took place

[8]The authors coined their e-access dates the "modern system operational date." Although the naming is different, the two definitions are conceptually identical.

[9]Alaska and Hawaii neighbor no US states.

[10]Florida experienced a dramatic rise in opioid supply in the 2000s, and then a significant drop due to crackdown on pill mills in 2010–2011. It is common practice in the literature to exclude Florida from the analysis.

[11]The underlying idea is that the ePDMP status of more populous nearby counties would have a bigger impact on my county than the ePDMP status of less populous nearby counties. Specifically, we sum up the population adjacent to a border county by ePDMP status. If more nearby population resides under the states with PDMP law than no law, the county's nearby law variable will be 1; if more nearby population resides under the states with no law, it will be 0.

during our sample period: over 80% of border counties had no ePDMP regulations in 2006 and that number decreases to less than 20% by the end of 2014 (see detailed transitions in Figure 2.5 in Appendix).

Summary Statistics

Since we are comparing border counties to inland counties of the same state, it is important that we acknowledge any potential differences between the two sets of counties, especially those associated with opioid use. In Table 2.1, we document the population-weighted average of opioid sales, mortality, and important demographics, and ePDMP coverage of the two sets of counties. Border counties have a significantly higher level of opioids sales throughout the sample period. They have lower levels of opioid mortality in 2006, but the difference loses significance since 2010. We will discuss these differences in outcome variables in our hypotheses and result section. The two sets of counties are quite similar on all demographic dimensions. Since some of these demographic factors are associated with higher levels of opioid misuse, it is important we control for demographic differences in our regressions.

Table 2.1: Summary statistics

Variables	All counties	Border counties	Inland counties	Test of equality (p-value)
Opioid-related statistics				
Sales per person (2006)	0.101	0.113	0.095	3.28e-11
Sales per person (2010)	0.163	0.185	0.150	0.004
Sales per person (2014)	0.158	0.181	0.145	0
Opioid overdose per 10,000 (2006)	2.22	2.01	2.33	0.003
Opioid overdose per 10,000 (2010)	3.35	3.29	3.38	0.574
Opioid overdose per 10,000 (2014)	3.84	4.02	3.75	0.164
Demographics (2009)				
Population	98,853	92,914	102,392	0.397
Average Age	36.11	36.8	35.7	0.149
Male (%)	49.2	49.0	49.3	1.53e-07
Separated (%)	18.2	18.6	18.1	0.001
High School and above (%)	83.4	83.9	83.1	0.002
Bachelor and above (%)	27.4	27.2	27.6	0.004
Mean income	70,130	71,063	69,625	0.05
Low income (%)	33.2	33.3	33.2	0.703
White (%)	78.6	79.0	78.4	0.279
Black (%)	12.8	13.6	12.4	0.015
Asian (%)	4.94	3.87	5.51	0
Native American (%)	0.178	0.141	0.197	8.41e-05
PDMP-related statistics				
Number of counties	2906	1085	1821	
Have ePDMP by 2006 (%)	18.6	17.8	20.1	
Have ePDMP by 2010 (%)	50.5	52.0	49.5	
Have ePDMP by 2014 (%)	87.2	85.1	88.3	

Notes: Means are weighted by county population. For opioid-related statistics, border counties have significantly higher levels of opioid sales throughout the sample period. Mortality is higher in inland counties, but the difference is not significant in all three years we tested. Many of the differences in demographics between border and inland counties are statistically significant but not economically. The adoption rates of ePDMP laws are similar between the two sets of counties.

2.4 Hypotheses and Empirical Framework

Hypotheses

In this section, we lay out our hypotheses and discuss the underlying assumptions and their implications on the market structure of prescription opioids. We start with a simplified model with no spatial spillover.

The state-as-island model. Consider states as isolated islands in an ocean. Due to the separation, opioids sold in each state can only be consumed in that state. Since county location bears no significance in this model, sales patterns and mortality should be similar in border and inland counties of the same state after controlling for demographic differences. For example, San Bernadino County, on the state border between California and Arizona, should behave similarly to Fresno County, landlocked within California. Although the adoption of a PDMP is endogenous to local conditions, a priori we would not expect the law to have differential effects on border and inland counties. Since all opioids sold locally are consumed locally, changes in sales due to PDMP laws should translate directly to changes in use patterns, and by extension, to changes in local opioid mortality, ignoring any substitution to illegal opioids[12]. The adoption of PDMP in one state should have no impact on opioid sales or mortality in the neighboring state. The testable hypotheses of the state-as-island model are:

> *Hypothesis 1a:* Under the state-as-island model, sales and mortality patterns are similar in border and inland counties.
>
> *Hypothesis 1b:* Under the state-as-island model, changes in sales translates into changes in mortality.
>
> *Hypothesis 1c:* Under the state-as-island model, adoption of PDMP in one state has no impact on sales or mortality in the neighboring states.

However unrealistic the above model is, it is assumed in many important studies on the opioid crisis. States are treated as isolated markets where all pills sold are consumed locally with the exception of Florida, which most papers exclude. The state-as-island model is applicable in situations when the spillover effect is small compared to the main effect, or if the spillover's impact is tangential to the main question. The literature has documented many occasions when the state-as-island model fails. Individuals cross the state border to take advantage of favorable

[12] We focus on opioid mortality, but as described in the literature review, some papers do find substitution to heroin following implementation of MA-PDMP laws.

lottery situations (Garrett and Marsh, 2002); patients cross the US-Mexico border to purchase prescription medication cheaply and without a prescription (Casner and Guerra, 1992). The decentralized enactment of PDMP creates differences in regulatory environments and incentivizes individuals to seek out the less regulated market. Next, we consider a model with spatial spillover.

The spatial spillover framework. Consider two states not separated by an imaginary ocean. Both opioids and people can cross the state line. As a result, opioids purchased in one state may or may not be consumed in that state. When individuals are incentivized to purchase opioids from a neighboring state, their cost of doing so is highly dependent on the distance traveled. Under these assumptions, vicinity to the state border has consequences on opioid sales and diversion. For someone living on the Arizona side of the Arizona-California state border, the cost of travelling to San Bernadino County for additional pills is much lower than that of travelling to some inland county within California.

The question remains as to when are individuals incentivized to cross the state border? Before any PDMP law, patients could obtain multiple prescriptions and get them filled in the same state with minimal constraint. When states adopt some version of the PDMP, doctor and pharmacy shopping within the same state becomes more difficult. However, because most states do not share their PDMP data with the neighboring states, the cost of obtaining additional pills from the neighboring states remains the same despite enactment of PDMP locally. As the cost of within-state pill shopping increases due to progressively stricter PDMP regulations (from PDMP to ePDMP to MA-PDMP), more and more individuals would be incentivized to cross a nearby border. By the start of our time period, 31 states had enacted some version of the PDMP, which means that some individuals would already be going to other states for pills. Hence, we expect a higher share of the border counties' sales to be diverted elsewhere for consumption during our sample period. Because the diverted pills are not consumed locally, we expect the sales to mortality ratio to be higher in a border county.

> *Hypothesis 2a:* Under the spillover framework, border counties will have higher sales but lower moralities as compared to inland counties of the same state.

Variation in diversion rates between inland and border counties implies that the two sets of counties will respond differently to new PDMP regulations. When states enact stricter PDMPs, the local pill shoppers and the out-of-state pill shoppers are similarly affected by the new rule. Since a higher share of the border counties' sales is from pill shoppers, the law change will have a bigger impact on the border counties. The endogeneity of adoption may bias the overall estimation toward zero, but should not affect how the border counties react to the law change relative to the inland counties. In addition, as the cost of local pill shopping increases due to stricter laws, local pill shoppers are more incentivized to cross the state border, and hence sales in border counties of the neighboring states would increase.

> *Hypothesis 2b:* Under the spillover framework, when the local state adopts a stricter PDMP, border counties will experience a larger decrease in sales relative to inland counties of the same state.

> *Hypothesis 2c:* Under the spillover framework, when the nearby state adopts a stricter PDMP, border counties will experience a larger increase in sales relative to inland counties of the same state.

In this stylized model, the mapping from sales to mortality is less direct when spatial spillover was not possible. With the state-as-island model, the enactment of a PDMP law puts a hard constraint on the opioid misuser's ability to acquire prescription opioids. Assuming no other substitution, changes in opioid sales in one location translate directly into changes in opioid mortality in that location. With spatial spillovers, changes in opioid sales in one place may lead to changes in mortality elsewhere. Since a larger share of the border counties' sales was consumed elsewhere, the adoption of stricter PDMP will result in a smaller drop in opioid mortality in the border counties. The enactment of PDMP in a nearby state increases sales in the border counties, but should have no additional impact on mortality, assuming that people traveling to acquire pills go back to their home counties to consume them. In reality, how mortality responds to a PDMP law depends on many factors, including the state of the black market, the availability of alternative drugs, and the ease of getting drugs from the nearby states. Since we cannot control for all of these relevant factors, we expect the mortality results to be less sharp than the sales results.

> *Hypothesis 2d:* Under the spillover framework, when the local state adopts stricter PDMPs, border counties will experience a smaller decrease in mortality relative to inland counties of the same state.

> *Hypothesis 2e:* Under the spillover framework, when the nearby state adopts stricter PDMPs, border counties will experience no additional

change in mortality relative to inland counties of the same state.

See Figure 2.6 in Appendix for a visual representation of the hypotheses of the spillover framework.

Empirical Framework

We want to test (1) how counties react to the enactment of ePDMP laws, (2) if border counties react differently as compared to inland counties, and (3) how the adoption of an ePMDP in one state affects border counties in the adjacent state. We use the following empirical framework to test our hypothesis:

$$
\begin{aligned}
Y_{ct} = &\alpha_s + \delta_t + \beta_1 \text{ Law}_{ct} + \beta_2 \text{ Border}_c + \beta_3 \text{ Law}_{ct} \times \text{Border}_c + \\
&\beta_4 \text{ Nearby Law}_{ct} \times \text{Border}_c + X_{ct}\gamma + \epsilon_{mt}
\end{aligned}
$$

where Y_{ct} are the outcome variables of interest: sales and mortality in county c in year t. Ideally, because each county has different initial conditions, we want to control for these conditions to get at the impact of the law change. However, because the location of a county and its border status does not change over time, any time-invariant differences between the border and inland counties would be absorbed by the county fixed effects if added. Hence, we use a full set of state fixed effects α_s and county characteristics X_{ct} as controls. We also add year fixed effects to control for national changes in drug use over time.

Our coefficients of interest are the full set of β's: β_1 estimates the impact of ePDMP laws on sales and mortality; β_2 estimates the baseline difference in sales and mortality between border and inland counties of the same state; β_3 estimates how the law affects the border counties differently as compared to the inland counties; and β_4 estimates how the enactment of an PDMP in one state impacts sales and mortality in the bordering counties of the neighborhood state, as compared to inland counties in the neighborhood state.

One notable feature of our empirical strategy is that the identification of the border effects (β_3 and β_4) does not require any assumption about the exogeneity of law change. As we have discussed in the literature review, enactments of PDMP laws are endogenous. When each state decides to implement ePDMP is a function of many factors, including its regulatory environment, the severity of its opioid crisis, the current political climate, and many others. These factors are highly correlated with the pre-enactment level of sales and mortality and the post-enactment response. If states are more likely to pursue stringent opioid regulations when conditions are bad, β_1 would underestimate the true impact of the law change. In terms of the estimation of the difference (β_3) and the spillover effect (β_4), law changes can be considered as random events.

2.5 Results

PDMP Law and spatial Spillover in Sales

The full set of β from our main regression is presented in column (5) of Table 2.2. We start with a simple two-way fixed effects model in column (1). We replace county fixed effects with the set of state fixed effects in (2) to (5) to estimate the border coefficients. In (2), we replicate the same regression as in (1) to show that changing from county to state fixed effects has no discernible impact on the ePMDP law coefficient. Starting in column (3), we add border status and interact it with ePDMP law to separately estimate the impact of ePDMP law on border counties. To ensure that differences in population characteristics between border and inland county are not driving the identification, in column (4) to (5), we control for county characteristics (average age, % male, % separated, education level, mean income, % low income, and ethnicity). These are variables that the literature has characterized as being influential in driving opioid use and overdose (Wright et al., 2014). In column (5), we add an indicator for whether the nearby state adopted ePDMP for each border county. We repeat the same analysis using the alternative ePDMP enactment dates from PDAPS. The results are documented in in Table 2.6 in the Appendix.

Table 2.2: Impact of ePDMP laws on opioid sales using Horwitz (2018) modern system operational dates

	Dependent variable:				
	Sales per person				
	(1)	(2)	(3)	(4)	(5)
β_1 - PDMP law	-0.006***	-0.005***	-0.002	-0.003	-0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
β_2 - Border county			0.006***	0.004***	0.004***
			(0.002)	(0.001)	(0.002)
β_3 - Law x border			-0.009***	-0.008***	-0.008***
			(0.002)	(0.002)	(0.002)
β_4 - Nearby law x border					0.0004
					(0.002)
County FE	Yes				
State FE		Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls				Yes	Yes
Observations	26,154	26,154	26,154	26,154	26,154
R^2	0.000	0.458	0.458	0.519	0.519

β_1 estimates the average effect of the enactment of ePMDP laws on opioid sales. Before adjusting for differential response due to the location of the county, we find that ePDMP reduces opioid sales. The coefficient is consistently negative in all specifications, but only significant before the inclusion of border coefficients. The border coefficient β_2 is consistently positive from (3) to (5), indicating that border counties start with higher sales as compared to inland counties of the same state. The estimation of β_2 supports *hypothesis 2a* (spillover framework) over *hypothesis 1a* (state-as-island framework). β_3, the law and border interaction term, is consistently negative. Although border counties start with higher per person sales, they experience a much larger drop in sales post-ePDMP than inland counties in the same state. The results are consistent with *hypothesis 2b* (spillover framework) that a higher percentage of sales in border counties are diverted elsewhere for non-medical use. Comparing the size of β_1 across specifications, we see that the estimated impact of ePMDP law on sales is largest in columns (1) and (2) and decreases and loses significance once we interact law with border status. If we do not separately account for abnormal behaviors in the border counties, the coefficients in (1) and (2) over-estimate the effect of the law change on opioid sales in a “normal” county. We observe the same pattern using our alternative e-access dates in Table 2.6.

In regression (5) of Table 2.2, β_4 is not well identified. Using our alternative e-access date, β_4 is significant and positive. We need to be careful in interpreting β_4 since the coefficient is measured with respect to sales in inland counties of the same state. Suppose A and B are two neighboring states and A experiences a law change. We have tentative evidence that counties in B that border A experience a faster growth (or slower decline) in sales than the inland counties in B. The findings support *hypothesis 2c* (spillover framework) over *hypothesis 1c* (state-as-island framework). Implementation of an ePDMP in one state increases the sales of opioids in border counties of nearby states.

Putting the coefficients together, border counties start with higher sales, experience a larger decrease if the local state enacts the ePDMP, and an additional increase if the nearby state enacts the ePDMP (only if we use alternative ePDMP dates). When states on both sides of the border adopt ePDMPs, most of the border effects cancel out. As the difference in regulation disappears between states, border counties lose their higher-than-average sales and their significance in cross-border opioid trafficking. In addition, the decrease in sales due to ePDMP laws are driven mostly by decreases in the border counties. The inland counties experience no significant drops in sales once we control for the border-law interactions. In the robustness section, we discuss what impact adoption timing has on how border states react to the enactment of electronic PDMP locally and nearby.

Translating our coefficients to real terms using Table 2.2, if we don't differentiate the border counties from inland counties, (1) shows that the law reduces per person sales by 0.006 MME, which is equivalent to a 5.6% drop from the national average in 2006. Since only a portion of sales are diverted for non-medical use, a 5.6% overall decrease is large if we translate it into drops in diversions. when we do account for border status, our estimation shows that the law reduces inland county sales by 0.003 mg in active ingredient on average (2.8%). In addition, the law reduces the border county's sales by 0.011 mg in active ingredient weight (10.2%), which is more than three times as much as the drop in inland counties.

PDMP Law and Spatial Spillover in Mortality

We have shown that the adoption of PDMP laws decreases local sales but has spillover effects on nearby states. Ultimately, however, what we care about is the consequences these laws have on actual opioid misuse and overdose. In this section, we use the same econometric specifications to test what impact an ePDMP law enacted in a state has on mortality in local and nearby counties. We expect the mortality results to be less sharp than sales results since there are many intervening factors between access to prescription opioids and opioid overdoses. Spatial spillovers, as identified in the previous section, are one. Substitution toward other alternative drugs is another. The literature has many examples of how restricting access to one drug resulted in substitution toward another potentially more lethal substance (Alpert, Powell and Pacula, 2018; Kim, 2021; Zhang and Guth, 2021).

Table 2.3: Impact of ePDMP laws on opioid mortality using Horwitz's(2018) modern system operational date

	Dependent variable:				
	Mortality per 100,000 residents				
	(1)	(2)	(3)	(4)	(5)
β_1 - PDMP law	-0.217***	-0.192***	-0.302***	-0.318***	-0.279***
	(0.051)	(0.069)	(0.075)	(0.073)	(0.074)
β_2 - Border county			-0.580***	-0.666***	-0.763***
			(0.063)	(0.062)	(0.067)
β_3 - Law x border			0.320***	0.366***	0.254***
			(0.085)	(0.083)	(0.088)
β_4 - Nearby law x border					0.297***
					(0.076)
County FE	Yes				
State FE		Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls				Yes	Yes
Observations	26,154	26,154	26,154	26,154	26,154
R^2	0.000	0.283	0.285	0.318	0.318

The coefficients on PDMP law are straightforward to interpret. Across the specifications, PDMP laws reduce opioid overdose. The reduction is economically significant. Using estimates from column (5), a -0.279 drop per 100,000 people translates into a 12.3% drop from the national opioid fatality rate in 2006. A negative and significant β_2 indicates that border counties have a lower level of baseline overdoes rate, which is consistent with our hypothesis that border counties do not abuse as many opioids but export a high percentage of their sales for misuse elsewhere (*Hypothesis 2a*). Given that the extra sales originating from border counties are not consumed locally, the adoption of PDMP laws should have no extra impact, if not less, on mortality in these counties. In columns (3) to (5), our estimation of β_3 is positive and significant. The size of β_3 is almost as large as β_1 in all three specifications, suggesting that the adoption of ePDMP has nearly no impact on a border county, which supports *Hypothesis 2d*.

In regression (5), we find β_4 to be positive and significant, which suggests that the mortality rate in border counties neighboring a state with a new ePDMP law increases faster (or decreases slower) than that in the inland counties of the same state. We get similar findings using the alternative ePDMP dates (Table 2.7). A positive β_4 does not support *Hypothesis 2e* that nearby enactment of ePDMP has no addition impact on the border counties. While the sales results suggest that people from recently restrictive states cross the state line to acquire opioids from the neighbor county, the mortality results suggest that these people not only shop across state lines, but also stay in the neighbor county to consume these opioids. Validating this mechanism is beyond the scope of the data we have, and we leave it to future researchers.

The differences in the mortality and the sales results are direct evidence that prescription opioids are trafficked across state lines. If opioids sold in each county are consumed locally, the mortality result should mirror that of the sales result. However, we find that border counties start with higher levels of sales but lower levels of mortality. Enactment of ePDMP leads to additional drops in sales in border counties, but fewer drops in mortality. The overall evidence supports the spillover framework over the state-as-island framework.

Effectiveness of Alternative PDMP Laws

To check if other PDMP laws have similar spillover effects, we run our main regression using two additional dates: any PMDP dates from Horwitz et al. (2018) and MA-PDMP dates from Sacks et al. (2021). The results on sales are documented in Table 2.8 in Appendix. The enactments of PDMP and must-access PDMP are not associated with reductions in opioid sales during our sample period. These findings are not conclusive evidence that PMDPs or MA-PDMPs are ineffective in reducing opioid sales. As we have stated in Section 3.2 and shown in Figure 2.1, our sample period covers very few enactments of PDMPs and MA-PDMPs. Most states had already enacted some version of the PDMP by the start of our sample period, hence we only observe PDMP law change in a few states that had been slow in action. Similarly for MA-PDMP, we only observe law change in the few early-mover states. The limited data combined with the endogeneity of adoption means that we do not have enough power to identify the effects of PMDP and MA-PDMP using 2006 to 2014 data.

We identify no border or spillover effects using the two alternative dates. The results suggest that identification of the border and spillover effects is sensitive to using the "correct" PDMP law. On the border coefficient, we know from previous regressions that the enactment of ePDMPs on both sides of the border makes the border counties lose their significance in cross-border shopping. Not finding a border effect using PDMP or MA-PDMP dates further validates our main hypotheses. On the spillover effect, if the law itself did not lead to a significant reduction in opioid sales in the first place, there is no reason to expect individuals to cross-border shop.

2.6 Conclusion

In this paper, we examined the effects of ePDMP laws on both the states they were enacted in and neighboring states. Following the literature, we find that opioid sales fall in states that adopted electronic access PDMPs. After controlling for border and spillover effects, we estimate that ePDMP laws reduce per-person opioid sales by 5.6% from the median sales in 2006, a considerable drop because the laws should only affect the fraction of users doctor or pharmacy shopping. We find that the decrease was driven by border counties in particular, where sales decreased 10.2% post-ePDMP. We also find that ePDMP laws reduce opioid overdoses in a state, with approximately a 12.3% decrease relative to per-capita mortality in 2006. These findings confirm the understanding in the literature that PDMP laws are effective in curbing the opioid epidemic.

The decentralized adoption of ePDMPs created opportunities for individuals to cross the state border to acquire opioids from a less restrictive state. Counties on the border are more likely to be destinations for doctor or pharmacy shopping due to the lower travel cost from other states. Our paper is the first to document a differential pattern in opioid use and a differential response to law changes in counties due to their proximity to the state border. Before the enactment of an ePDMP, border counties have significantly higher opioid sales and lower rates of overdose as compared to inland counties of the same state. When the state adopts an ePDMP, its border counties experience a larger drop in sales and a smaller decrease in mortality. In addition, when the nearby state adopts an ePDMP, we observe a larger increase (or smaller decrease) in sales in counties neighboring the law change state as compared to inland counties in that same state. The spillover effect indicates that the benefits of ePDMPs are partially mitigated because individuals purchase opioids from neighboring states when their state adopts an ePDMP.

The qualitative differences between border and inland counties in opioid sales and overdose have implications for all studies on the opioid crisis. Previous studies treat each state as an independent market and assume that local opioid sales have a one-to-one mapping to local opioid consumption. This simplifying assumption is the correct one to make in many situations. For example, in the study of the OxyContin reformulation, each state is treated with the same regulatory change. Spillover effects due to preexisting regulatory or cultural differences still exist, but they are irrelevant to measuring the impact of OxyContin reformulation on opioid use. However, in many other situations, where change takes place on a state-by-state basis, treating each state as an independent market may bias the estimation. In the case of PDMP laws, not accounting for cross-border sales overestimates the benefits of the law change.

The spillovers we have identified in this paper have implications beyond the opioid crisis. We have documented a direct negative externality from having state-based opioid policies instead of a national one. In a counterfactual world where all states adopt electronic access of PDMPs at the same time, all states would get the sales reduction without the increased sales from cross-border trafficking. These findings speak to the advantages and disadvantages of a federalist system. On one hand, decentralization allows each state to experiment and adopt politics based on their own conditions. Information from early adopters could flow to late adopters, thereby providing late adopters with real-world data on policy effectiveness. On the other hand, decentralization kills coordination and there is often a cost in failures to coordinate. Individuals, resources, and businesses are often not confined to one location. Regulatory differences among states allow entities unwilling to comply to move to a different state, thereby offsetting the positive benefit of new regulations.

This study is the second in a series of papers using the ARCOS data to better understand the opioid crisis. The first paper discusses substitution toward generic oxycodone when OxyContin was no longer abusable due to Purdue's reformulation. This paper discusses geographic substitution when obtaining opioids from one location becomes more restrictive due to the enactment of an ePDMP.

2.7 Appendix

Additional Tables

Table 2.4: PDMP adoption timing (1)

State	PDMP	ePDMP (Horwitz)	ePDMP (PDAPS)	MA-PDMP
Alabama	2005-11-01	2006-04-01	2007-06-28	
Alaska	2008-09-01	2012-01-01	2012-01-01	
Arizona	2007-09-01	2008-12-01	2008-12-01	
Arkansas	2013-03-01	2013-05-01	2013-05-16	
California	1939-01-01	2009-09-01	2009-09-01	
Colorado	2005-06-01	2008-02-01	2008-02-04	
Connecticut	2006-10-01	2008-07-01		2015-10-01
Delaware	2011-09-01	2012-08-01	2012-08-21	2012-03-01
District of Columbia	2014-02-01	2016-10-01		
Florida	2010-12-01	2011-10-01	2011-10-17	
Georgia	2011-07-01	2013-05-01	2013-07-01	2014-07-01
Hawaii	1943-01-01	2012-02-01	1997-01-01	
Idaho	1967-01-01	2008-04-01	1999-06-01	
Illinois	1961-01-01	2009-12-01		
Indiana	1997-01-01	2007-07-01	2004-12-29	2014-07-01
Iowa	2006-05-01	2009-03-01	2009-03-19	
Kansas	2008-07-01	2011-04-01	2011-04-01	
Kentucky	1998-07-01	1999-07-01	1999-07-01	2012-07-01
Louisiana	2006-07-01	2009-01-01	2009-01-01	2008-01-01
Maine	2004-01-01	2005-01-01	2005-01-01	
Maryland	2011-10-01	2013-12-01	2013-12-20	
Massachusetts	1992-12-01	2011-01-01	2011-01-01	2014-07-01
Michigan	1988-01-01	2003-01-01	2003-01-01	
Minnesota	2009-01-01	2010-04-01	2010-04-15	
Mississippi	2006-06-01	2008-07-01	2005-12-01	
Missouri				
Montana	2011-07-01	2012-10-01	2012-11-01	
Nebraska	2011-08-01	2017-01-01	2011-04-14	
Nevada	1996-01-01	2011-02-01	1997-07-01	2007-10-01
New Hampshire	2012-06-01	2014-10-01	2014-10-16	2016-01-01
New Jersey	2009-08-01	2012-01-01	2012-01-05	2015-11-01
New Mexico	2004-07-01	2005-08-01	2005-08-01	2012-09-01
New York	1972-01-01	2013-06-01		2013-08-01
North Carolina	2006-01-01	2007-07-01	2007-10-01	
North Dakota	2006-12-01	2008-10-01	2007-09-01	

Notes: Date in the second column is the enactment/legislated start date for any PDMP from Horwitz et al. (2018). Date in the third column is the modern system operational date from Horwitz et al. (2018). Date in the forth column is the electronic access dates from PDAPS. Date in the fifth column is the must-access PDMP date from Sacks et al. (2021). Table continues on the next page.

Table 2.5: PDMP adoption timing (2)

State	PDMP	ePDMP (Horwitz)	ePDMP (PDAPS)	MA-PDMP
Ohio	2005-05-01	2006-10-01	2006-10-02	2012-03-01
Oklahoma	1991-01-01	2006-07-01	2006-07-01	2011-03-01
Oregon	2009-07-01	2011-09-01	2011-09-01	
Pennsylvania	1972-01-01	2016-08-01		
Rhode Island	1978-01-01	2012-09-01	2012-07-01	2016-06-01
South Carolina	2006-06-01	2008-02-01	2008-09-01	
South Dakota	2010-03-01	2012-03-01	2012-03-01	
Tennessee	2003-01-01	2010-01-01	2007-01-01	2013-07-01
Texas	1981-09-01	2012-08-01	2012-06-30	
Utah	1995-07-01	2006-01-01	1997-01-01	
Vermont	2008-06-01	2009-01-01	2009-04-01	2015-05-01
Virginia	2003-09-01	2006-01-01	2006-06-01	2015-07-01
Washington	2011-08-01	2012-01-01	2012-01-04	
West Virginia	1995-06-01	2013-05-01	2004-12-01	2012-06-01
Wisconsin	2010-06-01	2013-06-01	2013-06-01	
Wyoming	2003-07-01	2013-07-01	2004-10-01	

Table 2.6: Impact of ePDMP laws on opioid sales using PDAPS dates

	Dependent variable:				
	Sales per person				
	(1)	(2)	(3)	(4)	(5)
β_1 - PDMP law	-0.010***	-0.011***	-0.008**	-0.008**	-0.008**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
β_2 - Border county			0.006***	0.005***	0.003*
			(0.002)	(0.001)	(0.002)
β_3 - Law x border			-0.009***	-0.008***	-0.009***
			(0.002)	(0.002)	(0.003)
β_4 - Nearby law x border					0.003*
					(0.002)
County FE	Yes				
State FE		Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls				Yes	Yes
Observations	26,154	26,154	26,154	26,154	26,154
R^2	0.005	0.459	0.459	0.520	0.520

Notes: We run the same regressions as Table 2.2 using alternative ePMDP dates. The results are very similar to our main findings: ePDMP reduces sales, but the reduction is less once we control for border counties; border counties have higher levels of sales and they experience sharper decline when ePDMP is enacted; enactment of ePDMP in nearby states increases sales in border counties of the local state.

Table 2.7: Impact of ePDMP laws on opioid mortality using PDAPS dates

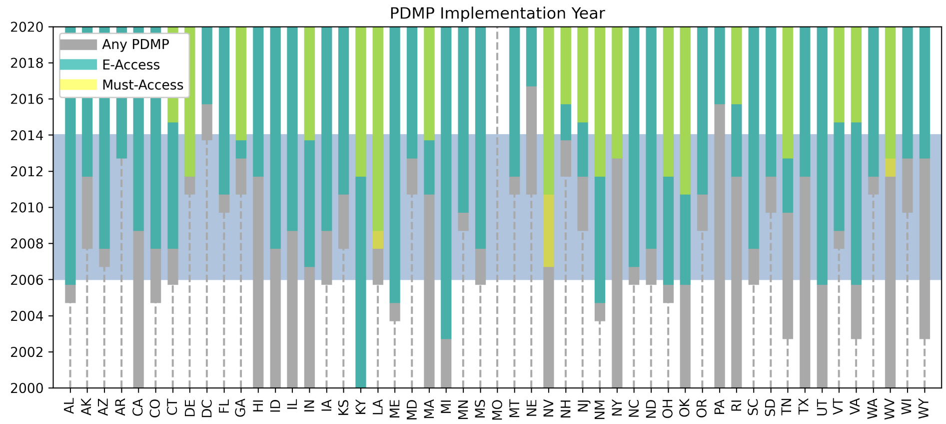
	Dependent variable:				
	Mortality per 100,000 residents				
	(1)	(2)	(3)	(4)	(5)
β_1 - PDMP law	-0.419***	-0.391***	-0.432***	-0.457***	-0.431***
	(0.052)	(0.071)	(0.075)	(0.073)	(0.074)
β_2 - Border county			-0.488***	-0.594***	-0.682***
			(0.062)	(0.062)	(0.070)
β_3 - Law x border			0.146*	0.234***	0.173*
			(0.088)	(0.086)	(0.089)
β_4 - Nearby law x border					0.210***
					(0.075)
County FE	Yes				
State FE		Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls				Yes	Yes
Observations	26,154	26,154	26,154	26,154	26,154
R^2	0.001	0.283	0.286	0.318	0.319

Notes: We run the same regressions as Table 2.3 using alternative ePDMP dates. Again, the results are almost identical to our main findings. Enactment of ePDMP laws reduces overdose. Border counties start with lower opioid mortality, but experience almost no drop when the state enacts ePDMP. Nearby enactment of ePDMP has a spillover effect on the mortality in the border counties of the local state.

Table 2.8: Impact of ePDMP laws on opioid sales using any PDMP dates, e-access dates, and must-access dates

	Dependent variable:				
	Sales per person				
	(1)	(2)	(3)	(4)	(5)
(A) Any PDMP					
β_1 - PDMP law	0.006***	0.005***	0.005**	0.004	0.003
	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)
β_2 - Border county			0.002	0.003	0.002
			(0.003)	(0.003)	(0.003)
β_3 - Law x border			0.0001	-0.004	-0.004
			(0.003)	(0.003)	(0.003)
β_4 - Nearby law x border					-0.001
					(0.002)
(B) Electronic access PDMP (main regression)					
β_1 - PDMP law	-0.006***	-0.005***	-0.002	-0.003	-0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
β_2 - Border county			0.006***	0.004***	0.004***
			(0.002)	(0.001)	(0.002)
β_3 - Law x border			-0.009***	-0.008***	-0.008***
			(0.002)	(0.002)	(0.002)
β_4 - Nearby law x border					0.0004
					(0.002)
(C) Must access PDMP					
β_1 - PDMP law	0.004***	0.003	-0.001	0.001	0.001
	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
β_2 - Border county			0.001	0.0001	-0.001
			(0.001)	(0.001)	(0.001)
β_3 - Law x border			0.012**	0.006	0.006
			(0.005)	(0.004)	(0.004)
β_4 - Nearby law x border					0.010***
					(0.003)
County FE	Yes				
State FE		Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls				Yes	Yes
Observations	26,154	26,154	26,154	26,154	26,154

Additional Figures



Note: The horizontal blue rectangle marks our sample period (2006 to 2014). For ePDMP, 9 states adopted it before the start of our sample period, 16 states adopted it in the first half of our sample, 18 states adopted in in the second half, and 8 states had not adopted it by the end of our sample period.

Figure 2.1: PDMP implementation dates by state

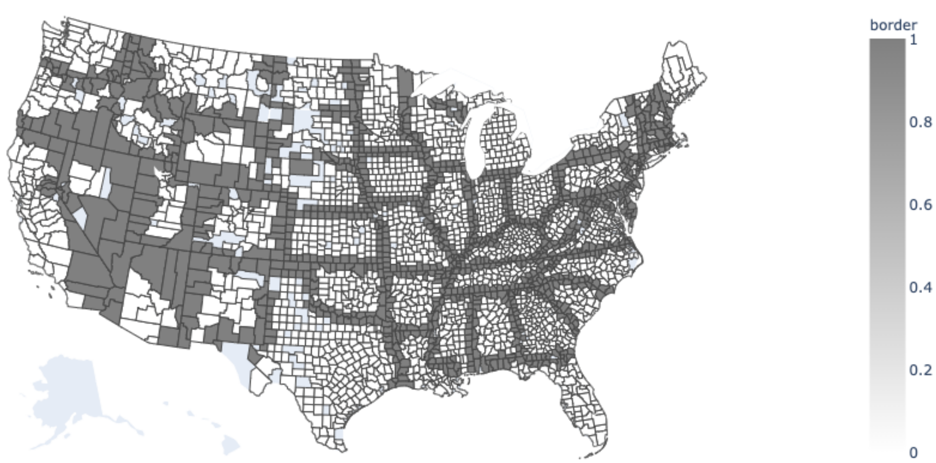
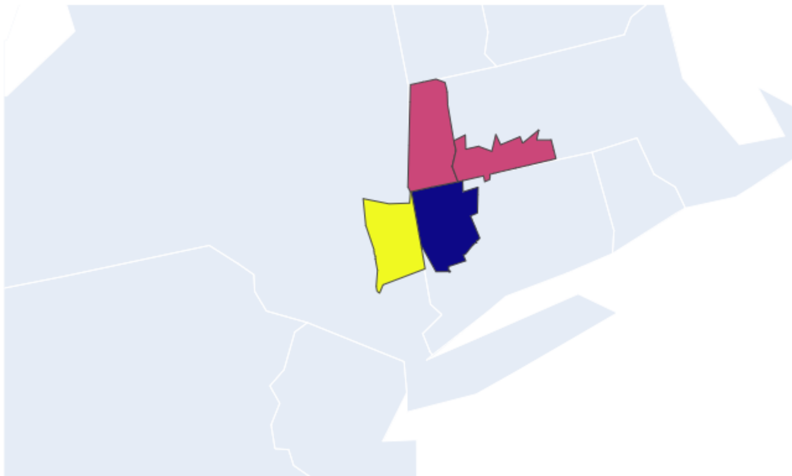


Figure 2.2: Map of border vs. inland counties



Note: This picture illustrates how we calculate the nearby law variable for Litchfield County, Connecticut (blue) in 2012. The Litchfield County borders three counties from nearby states: Dutchess County from New York (yellow), and Birkshire County and Hampden County from Massachusetts (pink). In 2012, the state of New York has not adopted ePDMP and the state of Massachusetts has. To calculate the nearby law variable for Litchfield County, we sum up the population nearby with no ePDMP $(294,000)$ and the population nearby with ePDMPs $(125,000 + 466,000 = 591,000)$. Since more people nearby live in counties with ePDMP, nearby law for Litchfield County in 2012 is 1.

Figure 2.3: Calculating nearby ePMDP status for counties bordering several states

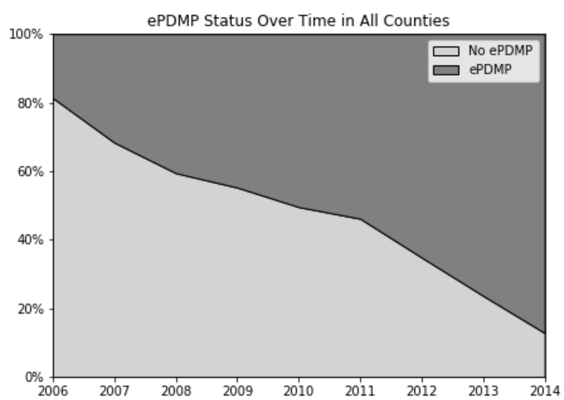
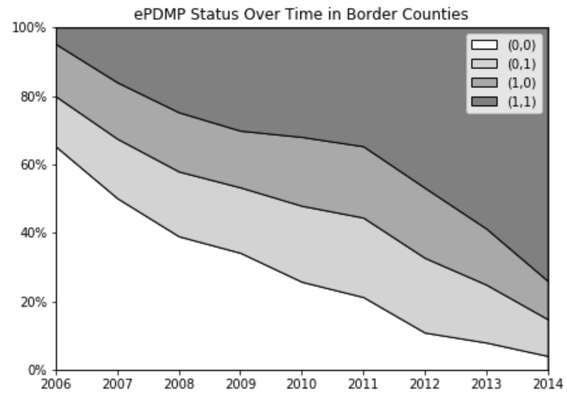


Figure 2.4: ePDMP adoption over time in all counties



Note: For the ease of reference, we use the (my law, nearby law) syntax to denote the ePDMP status of a border county. A border county of $(0,0)$ has no ePDMP law and its cross-state neighbors also do not have one; a border county of $(1,0)$ has an ePDMP law itself but its nearby state does not; a border county of $(0,1)$ does not have a law itself but its nearby state does; and a border county of $(1,1)$ has an ePDMP law itself and so do its out-of-state neighbors.

Figure 2.5: ePDMP adoption over time in border counties

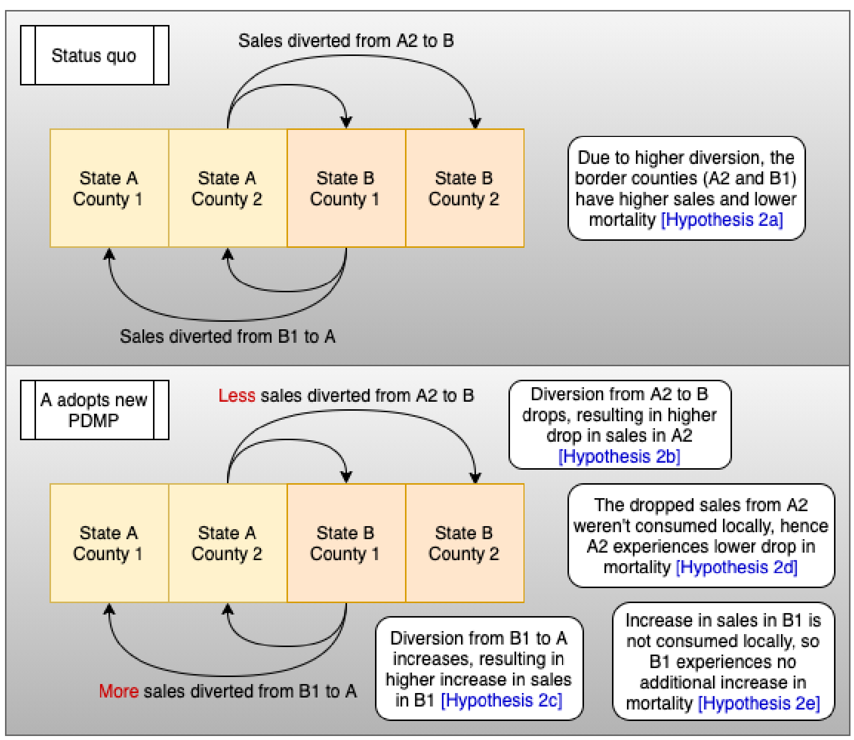


Figure 2.6: Visual presentation of the spillover framework

Bibliography

Alpert, Abby, David Powell and Rosalie Liccardo Pacula. 2018. "Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids." *American Economic Journal: Economic Policy* 10(4):1–35.

Bao, Yuhua, Yijun Pan, Aryn Taylor, Sharmini Radakrishnan, Feijun Luo, Harold Alan Pincus and Bruce R Schackman. 2016. "Prescription drug monitoring programs are associated with sustained reductions in opioid prescribing by physicians." *Health Affairs* 35(6):1045–1051.

Buchmueller, Thomas C and Colleen Carey. 2018. "The effect of prescription drug monitoring programs on opioid utilization in Medicare." *American Economic Journal: Economic Policy* 10(1):77–112.

Casner, Paul R and Luis G Guerra. 1992. "Purchasing prescription medication in Mexico without a prescription. The experience at the border." *Western Journal of Medicine* 156(5):512.

Cepeda, M Soledad, Daniel Fife, Yingli Yuan and Greg Mastrogiovanni. 2013. "Distance traveled and frequency of interstate opioid dispensing in opioid shoppers and nonshoppers." *The Journal of Pain* 14(10):1158–1161.

Dave, Dhaval, Monica Deza and Brady Horn. 2021. "Prescription drug monitoring programs, opioid abuse, and crime." *Southern Economic Journal* 87(3):808–848.

Evans, William N, Ethan MJ Lieber and Patrick Power. 2019. "How the reformulation of OxyContin ignited the heroin epidemic." *Review of Economics and Statistics* 101(1):1–15.

Finkelstein, Amy, Matthew Gentzkow and Heidi Williams. 2018. "What drives prescription opioid abuse? Evidence from migration." *Stanford Institute for Economic Policy Research Working Paper* pp. 18–028.

Fox, William F. 1986. "Tax structure and the location of economic activity along state borders." *National Tax Journal* pp. 387–401.

Garrett, Thomas A and Thomas L Marsh. 2002. "The revenue impacts of cross-border lottery shopping in the presence of spatial autocorrelation." *Regional Science and Urban Economics* 32(4):501–519.

Grecu, Anca M, Dhaval M Dave and Henry Saffer. 2019. "Mandatory access prescription drug monitoring programs and prescription drug abuse." *Journal of Policy Analysis and Management* 38(1):181–209.

Holmes, Thomas J. 1998. "The effect of state policies on the location of manufacturing: Evidence from state borders." *Journal of Political Economy* 106(4):667–705.

Holmgren, A Jay, Alyssa Botelho and Allan M Brandt. 2020. "A history of prescription drug monitoring programs in the United States: Political appeal and public health efficacy." *American Journal of Public Health* 110(8):1191–1197.

Horwitz, Jill, Corey S Davis, Lynn S McClelland, Rebecca S Fordon and Ellen Meara. 2018. The problem of data quality in analyses of opioid regulation: The case of prescription drug monitoring programs. Technical report National Bureau of Economic Research.

Kennedy-Hendricks, Alene, Matthew Richey, Emma E McGinty, Elizabeth A Stuart, Colleen L Barry and Daniel W Webster. 2016. "Opioid overdose deaths and Florida's crackdown on pill mills." *American Journal of Public Health* 106(2):291–297.

Kilby, Angela. 2016. Opioids for the masses: welfare tradeoffs in the regulation of narcotic pain medications. In *2016 Fall Conference: The Role of Research in Making Government More Effective. Appam*.

Kim, Bokyung. 2021. "Must-access prescription drug monitoring programs and the opioid overdose epidemic: The unintended consequences." *Journal of Health Economics* 75:102408.

Kirschner, Neil, Jack Ginsburg and Lois Snyder Sulmasy. 2014. "Prescription drug abuse: executive summary of a policy position paper from the American College of Physicians." *Annals of Internal Medicine* 160(3):198–200.

Lin, Hsien-Chang, Zhi Wang, Linda Simoni-Wastila, Carol Boyd and Anne Buu. 2019. "Interstate data sharing of prescription drug monitoring programs and associated opioid prescriptions among patients with non-cancer chronic pain." *Preventive Medicine* 118:59–65.

Mallatt, Justine. 2019. "Unintended consequences of prescription monitoring: Policy-induced substitution to illicit drugs." *Available at SSRN 3418615* .

McDonald, Douglas C, Kenneth Carlson and David Izrael. 2012. "Geographic variation in opioid prescribing in the US." *The Journal of Pain* 13(10):988–996.

McDonald, Douglas C and Kenneth E Carlson. 2014. "The ecology of prescription opioid abuse in the USA: geographic variation in patients' use of multiple prescribers ("doctor shopping")." *Pharmacoepidemiology and Drug Safety* 23(12):1258–1267.

Meinhofer, Angélica. 2018. "Prescription drug monitoring programs: the role of asymmetric information on drug availability and abuse." *American Journal of Health Economics* 4(4):504–526.

Nachum, Lilach. 2000. "Economic geography and the location of TNCs: Financial and professional service FDI to the USA." *Journal of International Business Studies* 31(3):367–385.

Patrick, Stephen W, Carrie E Fry, Timothy F Jones and Melinda B Buntin. 2016. "Implementation of prescription drug monitoring programs associated with reductions in opioid-related death rates." *Health Affairs* 35(7):1324–1332.

Reisman, Richard M, Pareen J Shenoy, Adam J Atherly and Christopher R Flowers. 2009. "Prescription opioid usage and abuse relationships: an evaluation of state prescription drug monitoring program efficacy." *Substance Abuse: Research and Treatment* 3:SART–S2345.

Rutkow, Lainie, Hsien-Yen Chang, Matthew Daubresse, Daniel W Webster, Elizabeth A Stuart and G Caleb Alexander. 2015. "Effect of Florida's prescription drug monitoring program and pill mill laws on opioid prescribing and use." *JAMA Internal Medicine* 175(10):1642–1649.

Sacks, Daniel W, Alex Hollingsworth, Thuy Nguyen and Kosali Simon. 2021. "Can policy affect initiation of addictive substance use? evidence from opioid prescribing." *Journal of Health Economics* 76:102397.

Schuler, Megan S, Beth Ann Griffin, Magdalena Cerdá, Emma E McGinty and Elizabeth A Stuart. 2021. "Methodological challenges and proposed solutions for evaluating opioid policy effectiveness." *Health Services and Outcomes Research Methodology* 21(1):21–41.

Simeone, Ronald and Lynn Holland. 2006. *An evaluation of prescription drug monitoring programs*. US Department of Justice, Office of Justice Programs, Bureau of Justice.

Wright, Eric R, Harold E Kooreman, Marion S Greene, R Andrew Chambers, Aniruddha Banerjee and Jeffrey Wilson. 2014. "The iatrogenic epidemic of prescription drug abuse: county-level determinants of opioid availability and abuse." *Drug and Alcohol Dependence* 138:209–215.

Zhang, Shiyu and Daniel Guth. 2021. "The OxyContin reformulation revisited: New evidence from improved definitions of markets and substitutes." *arXiv preprint arXiv:2101.01128* .

Chapter 3

THE VALUE OF CONNECTION IN CHINESE BUREAUCRACY: NEW EVIDENCE FROM A SYSTEMATIC EVALUATION AT THE CITY LEVEL

3.1 Introduction

The Chinese bureaucracy is often characterized as “a structure of authority that is governed by human relationships” (Pye, 1995). A growing literature shows that connection with the right higher-level politicians is beneficial for advancements in the Communist Party of China (CCP) (Francois, Trebbi and Xiao, 2016; Jia, Kudamatsu and Seim, 2015; Keller, 2015; Meyer, Shih and Lee, 2016; Opper, Nee and Brehm, 2015; Shih, Adolph and Liu, 2012). The rapid economic growth of China seems to be in direct conflict with the literature’s view that patronage institutions are inefficient as compared to meritocratic ones (Evans and Rauch, 1999; Geddes, 1994; Ilkhamov, 2007; Mueller, 2009). Is China an outlier in this respect, or is there more to the statement that the Chinese bureaucracy is characterized by interpersonal relationships?

In this paper, we use a new data set to measure the value of patronage ties for members of party standing committees of prefecture-level cities in China. These committees are in the middle of China’s administrative hierarchy, between the provincial and the district governments. They are important building blocks of the CCP’s governance structure and essential stepping stones for core leaders of the Party[1]. However, no previous study has looked at the value of patronage ties formed at this level of the government. Scholars examine the role of performance at this level and argue that the CCP uses meritocratic competition for promotion to induce local officials to produce desirable economic outcomes (Li and Zhou, 2005; Xu, 2011). In fact, Landry, Lü and Duan (2018) presented evidence that the lower the level, the more performance matters. In this paper, we examine if the value of patronage ties is different at lower levels of the bureaucracy.

The connection variable we use is based on assignments into the city committee. A committee member is connected to the city party secretary—the head of the

[1] In the 19th Politburo, 3 out of the 7 members (42%) of the Standing Committee and 11 out of the 25 members (44%) of the Politburo served in prefecture-level city committees at some point in their career. Source: link to biographies of members of the Politburo.

city committee—that oversaw his/her promotion into the committee. Similarly to a network in real life, every individual in the system is connected to someone, but the value of that connection can vary based on how influential the individual's patron becomes. If connections formed in the city committees are valuable, we would expect that those connected to party secretaries who later became successful will benefit more from that connection. Compared to the common practice in the literature, we only test whether a specific type of connection is valuable in the next stage of the committee member's career.

We find that relationships formed in the city committee are not carried over when the party secretary gets promoted. We present empirical evidence that the party secretaries are involved in the appointment of committee members. The high level of involvement in appointment, combined with the subsequent years of collaboration in the committee, paves the ground for a close relationship between the committee member and the party secretary. Thus ties form, but do they matter? We find that, upon departure, the party secretaries' career success does not improve the committee members' future promotion likelihood. In other words, the value of interpersonal connection in China is highly dependent on which level of the government is under inspection.

This paper makes two innovations over the literature. Firstly, we assemble a new dataset of thousands of city leaders and examine the value of connection in a previously overlooked but important level of the Chinese government. Secondly, we develop an innovative method to measure connections formed at one level of an organization and test their subsequent value as individuals move out of that organization. The common practice in the literature starts with a group of individuals already successful, usually members of the alternate central committee or the central committee. Scholars then trace backward in time for connections formed in the past and group connections formed in all kinds of organizations into one variable. This methodology has two potential shortcomings: it is susceptible to selection bias and it ignores any differentiation due to experience from different organizations.

On selection bias, imagine a network where everyone is highly likely to get connected. While connections may be necessary for promotion, they have little value because most connected individuals are not promoted. Looking only at connections among successful individuals will lead scholars to deduce that connections are useful even if they are not, because the approach misses the fact that all the individuals not promoted were also connected. One strategy to avoid the pitfalls of selection bias is to select individuals based on one level of the government and estimate the value of connection based on promotion to the next. The dual-level structure allows scholars to compare the connections of individuals that were successful in getting to the second level versus those that were not. Our methodology is similar in this respect: we select individuals based on assignments into the city committee, and trace forward for promotions in the future.

In addition, grouping connections formed in different organizations into one variable ignores any differentiation due to experience. Experience from certain cities or positions could add value to one's career while the same experience in other cases could be useless. Because of the add-on value of certain positions, successful individuals transition to the top through similar career trajectories. These individuals were automatically connected through co-working ties, but it was their job, not their connection, that provided them with positive value. Hence, testing the value of connection while ignoring how it is formed can potentially produce false-positive findings (see the details of this argument in Fisman et al. (2020)). Our methodology limits the scope of analysis to one type of connection: that with the party secretary. By conditioning on one type of connection, we make precise statements about its value without confounding it with other factors. The approach we developed in this paper can be applied recursively to higher levels of the hierarchy to pinpoint where such a connection becomes consequential if ever.

This approach required a new dataset of city committee members that we have collected. The new data set covers 46 prefecture-level cities from five provinces in China and spans a period of 20 years. The data allows us to track the assignments of thousands of individuals into the city committee and their subsequent assignments after the city committee. We are able to link committee members with the party secretaries in charge at the time of their assignments into the city committee, and test how changes in the connected party secretary's power influence the committee member's future. To the best of our knowledge, this data set is the first systematic documentation of turnover at the city level in China.

The study of connection is important beyond its implication on promotion outcomes. The informal role connection plays shapes the motivation of the participants and endows different players with different resources. City leaders with informal ties to the incumbent provincial leaders deliver significantly faster economic growth (Jiang, 2017; Wong and Zeng, 2020). External support networks increase the local politicians' responsiveness to ordinary citizens by reducing the politicians' reliance on local vested interest (Jiang and Zeng, 2020). Lei (2020) argues that patron-client relationships boosted infrastructure investment and stimulated the growth of state-owned enterprises during the financial crisis.

The chapter proceeds as the following. In Section 3.2, we discuss the institutional background of personnel management system in China and the city party committee and introduce the literature on connection in China. Section 3.3 describes the data collection process. In Section 3.4, we develop the hypothesis and our regression framework. The results and discussions are presented in Section 3.5. Section 3.6 concludes.

3.2 Background and Literature Review

In this section, we outline the personnel management practices in CCP, provide background information on the functions of the city standing committee, and review the existing literature on the value of connection in China.

Personnel Management in China

We start by summarizing the formal appointment procedures in the CCP. Regulations on the Work of Selecting and Appointing Leading Party and Government Cadres[2] is the guiding document on cadre management. According to the document, there are five stages in the appointment process. In the **motion** stage (Article 11 to 13), the party committee or the organization department collects information on the opening and drafts documents on how to recruit. In the second, **democratic recommendation**, stage (Article 14 to 22), relevant groups hold meetings to recommend potential candidates for the opening. The Regulations explicitly state that democratic recommendation is a necessary step for the appointment of any leaders. In the third, **appraisal**, stage (Article 23 to 33), the relevant party committee decides on a list of candidates based on performance and nomination from the democratic recommendation stage. Then, the local organization department vets the candidates through investigations and interviews. The next step of the process is **discussion and deliberation** (Article 34 to 39). In this stage, the relevant committee and leaders in the level above the position deliberate and vote on the list of candidates based on the information gathered in stage three. In the final stage, the candidate is appointed to the post.

The procedures outlined in the Regulations imply that the party secretary plays a

[2] We will outline the Regulation published in 2014 since it aligns with our sample period better than the newest 2019 version. See link for the newest version in Chinese.

key role in many, if not all, stages of the assignment process of a new committee member. The party secretary chairs the committee that drafts the opening; he leads the democratic recommendation meetings; he is interviewed in the appraisal stage; and he leads the committee that votes on potential candidates. Nevertheless, the party secretary does not fully control assignments. In fact, even if the party secretary has the intention to do so, he is constrained by formal procedures and provincial oversight. Within the limit of formal procedure, the party secretary can influence the assignments or at least veto individuals of which he does not approve. In addition, the party secretary has power over the next stages of a committee member's career. When an existing committee member is being considered for promotion elsewhere, as the current leader with direct oversight, the party secretary will be interviewed in the appraisal stage to give his opinion on the individual's capabilities.

The party secretary wields considerable influence over the career of the committee member even operating within the limits of formal procedures. It is important to note that beyond the rules just outlined, there are informal ways through which party leaders increase their assignment power in practice. Zeng (2016) argues that the assignment procedures represent attempts to increase bottom-up participation within the party, however the process is hindered by loopholes in regulations and informal practices. For example, party leaders make appointments during recess periods to override the nomination and the election process. Based on his data looking at the provincial standing committee, Zeng finds that such override is a common practice: about 65% of the assignments of provincial standing committee members happened during recess periods. While we do not have data to evaluate what percentage of the city committee member assignments were done without completing the entire assignment process, the informal practices Zeng observed in higher levels of the government are likely to take place at the city level. These informal practices would further increase the party secretary's power in managing his city committee, providing a strong base for relationships to form between the city party secretary and the committee members he promoted.

The City Party Standing Committee of Prefecture-Level City

In this subsection, we will provide a short overview of both the prefecture-level city and the city party standing committee. Prefecture-level city is an administrative division in the CCP's bureaucratic hierarchy, ranking below a province and above a county. A prefecture-level city is not a "city" in the literal sense, but an administrative unit consisting of typically one main urban area and its surrounding rural area.

Currently, there are 293 prefecture-level cities in China.

The party standing committee is de facto the highest local leadership council of the CCP in any area of jurisdiction. The city party standing committee is the highest leadership council in the city. The committee typically consists of 9 to 11 members. Members have primary titles associated with the city committee and secondary titles indicating their areas of jurisdiction. Primary titles are party secretary, vice party secretary, and standing member of the committee. Secondary titles include mayor, deputy vice mayor, head of important departments and committees in the city, and party secretary of important counties in the city. The composition of secondary titles is stable across cities and over time. The official term of service for city committees is five years, although timing of assignment and individual terms of office are quite variable. Committee meetings are usually held twice a month, but can occur more often if necessary. Members collectively decide on important issues of the city, from ideology to economic development, and work closely together under the leadership of the city party secretary.

Value of Connection

Scholars of Chinese politics have long studied the role connections play in the career advancements of elite members of the CCP. The literature focuses on the top of the CCP hierarchy and explores several definitions of connection. One definition creates a connection among the members of privileged groups. Francois, Trebbi and Xiao (2016) identified four prominent faction affiliations: the Princelings, the military, the Youth League Faction, and the Shanghai Gang[3]. The paper finds substantial promotion premium for individuals who are in the same faction with the general party secretary of the CCP.

A second definition creates a connection when two individuals are in the same work unit at the time. A work unit is any organization or department that the CCP controls, from governments to ministries to state-owned enterprises. Compared to the factional ties, this definition broadens the positions where connections can form from a selective few (Shanghai municipality or the Communist Youth League) to in theory any positions in any work unit. Shih, Adolph and Liu (2012) find strong evidence that ties with top leaders played a large role in the political advancement

[3]The Princelings are descendants of prominent senior CCP officials; the Youth League Faction is an informal faction that includes officials who originated from the Communist Youth League (CYL); the Shanghai Gang is a group of officials that rose to prominence in connection to Shanghai municipal administration under the former general secretary Jiang Zemin.

of members of the Central Committee. Jia, Kudamatsu and Seim (2015) argue that connection and performance play complementary roles in promotions. When connected to central leaders, one standard deviation increase in provincial economic growth increases the promotion probability of the provincial leader 5.3 percent more than that of an unconnected provincial leader.

One limitation of both definitions is the inability to separate the quality of the candidate from the value of connections. Faction affiliations and co-working ties are formed endogenously, and the experience itself may change the quality of the candidates. A higher likelihood of promotion could come from either the positive benefits of connection or simply efficient selection of high-ability individuals into particular factions or jobs. Fisman et al. (2020) raised the same concern that regressing connection on promotion without controlling for where the connection was formed may produce false-positive results. Promising individuals, and especially the high quality candidates running with small steps through the system, are often assigned to prestigious postings in certain departments. Because the patrons in higher offices often rotated through the same set of postings, the similarity in posting patterns will lead to a false-positive result on the value of connection when in reality the boost in promotion probability arises from the assignment itself or the differences in the quality of the candidates.

The issue is further exacerbated by selection bias. The literature starts with a group of individuals at the top and looks back in time for connections formed in the past. Prestigious positions that are stepping stones for elites are more likely to show up in the data, and people who have rotated through such stepping stones are more likely to score a positive on connection. The structure of the data means that we cannot know how well connected are the individuals who did not make the cut to the top circles. If the unobserved individuals are similarly connected, then it would undermine findings in the literature. The literature's positive findings could be the result of a self-fulfilling prophecy where individuals that benefited from having connections get selected into the data and individuals who did not benefit were screened out. The positive findings in the literature are a necessary but not sufficient condition for connection to be beneficial across the board.

Fisman et al. (2020) partly addresses this concern by adding a fixed effect for each work unit. The paper finds that the connection coefficients are positive before adding the fixed effects, but inconclusive after. We employ a different strategy to address this quality and selection issue. In our data, we use one job, membership in the city party standing committee, as the selection criteria, and then trace individual's accomplishments after the city committee. We thus do not select individuals based on whether their next job is a promotion or not. By conditioning on the one job, we can resolve the differences in job quality without adding hundreds of fixed effects as controls. We use variations in the city party secretary's outcome to create variations in the value of the connections formed in the city committee (it is intuitively better to be connected to a city party secretary who gets a promotion than to one who does not). The common initial assignment allows us to focus on the value of the connection without raising concerns about the value of the assignment.

Another limitation of the literature is its disproportionate focus on the top of the CCP hierarchy. Most studies find connections with the general secretary, the top leaders, or members of the Politburo Standing Committee (PSC), the top leadership body, to be beneficial. Putting aside the selection issue, the literature is evaluating the value of connection at the level of the government where connection is most likely to matter. At the top, the selection criteria are more ambiguous, the cost of faction building is smaller, and thus leaders have more room to manipulate assignments. At lower levels, tasks are more likely to be single-dimensional, and thus performance is measurable and comparable across individuals. Tournaments based on explicit performance goals can be implemented to incentivize effort (Lazear and Rosen, 1979; Li and Zhou, 2005). The more explicit promotion guideline means that local leaders would incur a higher reputation cost for supplying connected clients with promotions. In addition, personnel management at the provincial level is sufficiently detached from the central government that their practices and the value of interpersonal connections could be completely different. To the best of our knowledge, our paper is the first systematic test of the value of connections to provincial leaders on the city leaders' career advancement.

Another issue with the definition of connection in this literature is that it is a necessary but not sufficient condition for identifying an actual relationship. Co-working experience creates an opportunity for the patron and the client to connect, but how much and how well the two individuals interact is not observed. Meyer, Shih and Lee (2016) tested four ways of measuring factional ties as suggested by the literature. They found that clients connected to patrons under the most restrictive measure, in which the two individuals work together in the same unit and the junior member had a rotation or promotion under the supervision of the senior member, enjoyed the highest boost in promotion likelihood. Keller (2015) similarly found that co-working experience sealed with promotion is one of the most precise measures of patronage relations. In this paper, we infer connections based on active formations of patron-client pairs. Two individuals are connected when we observe the client (committee member) is promoted to a position (committee membership) that is under the direct supervision of the patron (city party secretary). More precisely, a committee member is connected to the party secretary in place when that member is appointed to the city committee. In Section 5.1, we will show that the transition timings of the committee members are highly correlated with the entry and exit of their connected party secretary, indicating that our definition of connection captures a meaningful relationship between the patron and the client.

3.3 Data

A New Database for Chinese City-Level Leaders

We overcome the selection bias in the literature by assembling a new data set consisting of all individuals assigned to the city party standing committee of 46 prefecture-level cities spanning five provinces over a period of 18 years. The data collection process has three steps. We first obtain assignment information from the city governments' official yearbooks. The yearbooks report changes to city leadership down to the month of the appointment or dismissal. We combined the eighteen one-year snapshots to obtain the committee turnover data from 2000 to 2018 for each city. We were able to accurately document leadership turnovers for 46 out of the 62 prefecture-level cities in the five provinces. For the 16 missing cities, either the complete history of city yearbooks is not available online or the turnover data is not consistently documented in the yearbooks. In Section 7.1.1 in the Appendix, we compared the missing cities with those in our data on observable dimensions. The in-sample cities are larger in population and higher in per capital GDP, which motivates our inclusion of city fixed effects in some of our specifications. We do not expect the missing status to be correlated with personnel practices in the city. The inability to locate some cities shrinks our sample, but should not bias our findings.

The second step of the data collection process is to obtain individual-level information. The city yearbooks contain no additional information on the committee members other than their assignments. We thus supplemented the turnover data with career biographies downloaded from various sources[4]. We successfully located biographies for 100% of all the party secretaries and 79.2% of the standing

[4]Sources include individual biographies on Baidu Baike (link), Local Leader Database on People.cn (link), Chinese Political Elite Database maintained by Junyan Jiang (link).

committee members in our data set. We failed to locate the remaining 20.7% percent of the committee members because the biographies of these individuals are not published digitally in any news articles, most likely because they retired directly from the city committee. We present documentation of data attrition and discuss the potential biases introduced by the missing individuals in Section 7.1.2 in the Appendix. Our regression results are robust to dropping the missing individuals or including them after making reasonable assumptions about their career outcome.

In the last step of the data collection process, we process and combine the information from the two data sources. We infer the connection variable from the assignment data; we parsed out relevant information, such as date of birth, education, gender, and ethnicity from career biographies to use as controls; and we hand-coded the post city committee outcome from career biographies. We will discuss the methodology used in measuring connections and categorizing outcomes in the next sections.

Measuring Connections

Connections between the party secretary and the committee member are the key to this paper's agenda. Formally, committee member C is connected to party secretary P if C is assigned to the city committee when P is in charge. The connection definition does not require the party secretary to be acquainted with the committee member prior to the assignment. The process of appointment we discussed at the beginning of the paper makes it likely that the two individuals have some prior connection. Whether a prior connection exists or not is neither observable nor of consequence to our argument. Given the party secretary's level of personal involvement in the appointment process, assignment into the city committee can be interpreted as a "favor" granted by the party secretary to the new committee member. The approval of the city party secretary creates a superior-subordinate tie and forms a solid foundation for future political cooperation between the two, even if a tie does not exist before the assignment.

This definition of connection generates a 1 to N mapping from the party secretary to members of the standing committee. The party secretary works with both committee members that are connected to him and those that are connected to party secretaries before him. The average party secretary is connected with 6.2 committee members (the range is 1 to 16, see Figure 3.4 in the Appendix for distribution). On average, the committee member works with his connected party secretary for two years in the city committee before one of them moves to a different position. A quarter of the committee members never work with any other party secretary than their connected one.

Categorizing Transitions

What the city committee members do after they leave the city committee is the second crucial variable in the analysis. The party secretary and the committee member can experience one of four mutually exclusive outcomes: promotions, transitions without promotions, transitions to honorary positions/retirement, and exits from leadership. In Section 7.2 in Appendix, we list all the possible outcomes for the party secretary and the committee and our categorization of the outcomes.

Before going into each category, we briefly introduce the CCP's civil servants ranking system. There are two kinds of ranks: the rank of the department/organization and the rank of an individual. Each department has a publicized organizational rank. Unless otherwise specified, the rank of any individual could be inferred based on the rank of his department and his position within the department[5]. The rank of an individual is not explicitly noted unless his rank does not match the rank of his position. All city party secretaries of prefecture-level cities share the same bureau-director rank and all standing committee members are at the vice bureau-director rank (See Figure 3.3 in Appendix for all levels). Promoted party secretaries join the vice provincial-ministerial rank, which is the rank of the head of any vice provincial-level organization or the vice head of any provincial-level organization. Standing committee members who are promoted join the bureau-director rank (the rank of the party secretaries).

Promotions. Promotion is the most important outcome. We use a conservative categorization of promotion and place all ambiguous cases into transitions without promotions. For the party secretaries, promotion requires going to the vice provincial-ministerial rank. Of the 261 party secretaries in our data, 31.4% were promoted after the party secretary job in which we found them. The most common jobs after promotion are vice governor in the province, standing member of the provincial city committee, and mayor or party secretary in vice provincial-level cities.

For committee members, an upward movement does not necessarily imply an increase in rank. The committee members are at the lower end of the vice bureau-

[5]The top executive within each organization has the rank of the organization. The rank of any other individual can be inferred by adding the rank difference between the individual and the executive to the rank of the organization.

director rank. On very rare occasions do we observe someone transitioning from the standing committee directly to a position in the bureau-director rank. Most committee members move within the vice bureau-director rank one or two times before they get an actual increase in rank. Without explicit criteria, we rely on definitions of promotion used in the literature at this level and our understanding of China's political hierarchy to hand-code committee members promotions. See Section 7.2.3 in the Appendix for the criteria and the justification. 26.6% of the committee members documented were promoted. The most common titles that signal promotions for committee members are vice party secretary or deputy vice mayor of prefecture-level cities, vice head or head of provincial departments (depending on the importance and the rank of the department), and standing member of vice provincial city committees.

Transitions without promotions. We define transitions without promotion as any change in job title that does not entail promotions as defined above or movements to honorary positions in the People's Congress or the Political Consultative Conference. In our data, 30.1% of the party secretaries and 15.3% of the committee members moved horizontally after their city committee assignments. For the party secretaries, horizontal movement means staying in the bureau-director rank. Common destinations include head of provincial departments or ministries, head of the city party school, and party secretary or chairman of state or province owned enterprises.

Common destinations for committee members include head or vice head of city departments or ministries, and heads or vice heads of provincial departments or ministries. Although not as glorious as promotions, horizontal movements are positive outcomes for the standing committee members. Frequent transitions are signs of Party cultivation. Kou and Tsai (2014) characterizes "spring with small steps" as a mechanism to assist promising cadres who failed to get promotions early on to escape being trapped in lower-level positions. Frequent rotation will help highlight the ability of the individuals and enrich their experience and readies them for promotions. In addition, since there is no rank increase to benchmark promotions, our categorizations of promotions and horizontal movements are subjective in nature. To make sure that the categorization is not driving our results, in alternative specifications, we group horizontal movements and promotions together as positive outcomes and check if the likelihood of positive outcomes changes with connection ties. Our main results are not sensitive to whether we include horizontal transitions in positive outcomes.

Transitions to honorary positions. Transitions to honorary positions include all movements to the Provincial or City People's Congress and the Provincial or City People's Political Consultative Conference. Positions in these two organizations are considered as ceremonial by most scholars (Meyer, Shih and Lee, 2016). The frequency at which individuals transfer out of these ceremonial positions and return to power is low in the data. Honorary positions are in effect terminal postings before retirement. Approximately 28.6% of party secretaries and 12.8% of committee members transfer to honorary positions after the city committee.

Exits. Exits include voluntary withdrawals, direct retirements, and criminal convictions. Individuals whose digitized career biographies that we were not able to locate also fall into this category. For the party secretaries, 2.2% were missing, 1.1% exited due to corruption investigation, and 1.9% exited directly. For the committee members, 15.4% were missing, 2.4% exited due to investigation, and 4.0% exited directly. The four categories do not add up to one because some individuals are still in office at the end of our observation period, which amount to 4.2% of the party secretaries and 10.1% of the committee members.

3.4 Hypotheses and Empirical Strategy

Hypotheses

Promotion is a function of many factors: the availability of openings, individual ability and qualifications, and endorsements from coworkers, superiors, and patrons. The previous literature emphasizes the second and third factors, often debating whether elite mobility is explained by the performance model or the connection model. Our data do not include job openings for most mid-level positions; we partially observe ability, proxied by the combination of age, education, and job performance; we only observe specific types of connections made, such as co-working and hometown ties. In this paper, we test whether specific kinds of connection ties—connection with the current party secretary and connection with a promoted party secretary—are valuable for committee members while partially controlling for individual ability.

Specifically, committee members are treated with two different party secretary outcomes depending on who leaves the committee first. The first treatment occurs when the connected party secretary exits the city committee. The committee member transitions from working under his connected party secretary to working under a new commanding officer. If the party secretary's influence over the committee member's next assignment is constrained by whether he is still in office, the treatment will lower the committee member's promotion likelihood. The second treatment happens when the connected party secretary is promoted. In the case of promotion, the committee member is treated with having a close connection with someone in higher office. If gaining power increases the party secretary's influence over his connections' next assignments, we would expect the promotion likelihood of the connected committee member to increase.

There are four possible ways for the two treatments to impact the committee member's promotion likelihood. In case 1, the party secretary exercises more influence over the connected committee members' next assignments both when he is in office and after he has been promoted. There is no institutional or reputational constraint on the party secretary on recommending his clients, and connections are valuable as long as the party secretary stays in power. In case 2, the party secretary has influence while he is in the committee. Connections with the party secretary are valuable but limited in scope. From the party secretary's point of view, recommending a current connected subordinate for promotion is a common practice, but once out of the committee, going out of one's way to help a past subordinate is risky behavior. From the committee member's perspective, promotions need to happen before the connected party secretary leaves, otherwise, the member is much less likely to get promoted later on. In case 3, the party secretary does not exercise much influence over the connected members' promotion while he is in office, but he helps out old subordinates once he himself gets promoted. Connection is valuable but the party secretary's influence is limited without a promotion. Gaining higher status is essential in helping the connected subordinates. In this case, we expect to see the committee members' promotion destinations to be highly correlated with where the party secretary was promoted. In case 4, the member's promotion does not depend on the secretary's ability or promotion. The value of connection at this level is marginal and dominated by other factors such as opening availabilities and individual ability. Either the party secretaries are not influential, or faction building is of no value to them.

In an ideal situation, we would first compare the promotion likelihood of a committee member when his connected party secretary is on the city committee with when his connected party secretary has left. Given that the connected party secretary has left the committee, we would then check if the committee member's promotion likelihood changes with whether the connected party secretary gets a promotion. Obviously, we only observe one particular outcome for each pair. The best we can do is to hold the individual and assignment level characteristics constant and compare how the party secretary's outcomes impact the committee member's promotion likelihood.

Important individual-level characteristics to take into account include age, education, gender, ethnicity, and shared hometown tie. All these variables except age are straightforward to control for because they are fixed in time. Controlling for age is potentially complicated but essential for our analyses. It is complicated because age interacts with tenure through talent, experience, and mandatory retirement age. If one enters a job at a younger age, one can afford to accumulate more experience while staying eligible for promotion. However, if one enters the job at an older age, one's promotion window closes much sooner. The mechanism implies that the baseline promotion likelihood as a function of tenure for people in different age groups could be non-proportional, which would violate a key assumption of our hazard model. At the same time, properly controlling for age is essential for our identification. All other things equal, being younger than one's coworkers signals ability because the individual rotated through lower-level positions much faster. These individuals "sprint with small steps" through the system and are perceived to be either individuals with high ability or individuals with solid ties to the central government (Kou and Tsai, 2014). Self-fulfilling prophecy or not, the individuals "sprinting with small steps" have a high likelihood of promotion from the current level onward. The marginal impact of connection on promotion likelihood could differ based on whether the individual is "sprinting with small steps" or not. In the baseline regression, we add age as a control variable. To rule out the possibility of misspecification due to age's complicated impact on one's career, we run the model on different age cohorts separately and present the result in the robustness section.

Regression Framework

We use a discrete time competing risk model for the regression analysis. We defined three states of interest: promotion (P), transition (T), and exit (E). For each interval t, we define a categorical response y_{it}:

$$y_{it} = \begin{cases} 0 & \text{if no event in } t; \\ r & \text{if event of type } r \text{ in } t \ (r = \text{P,T,E}). \end{cases}$$

We define the probability of promotion during interval t as $p_{it}^{r} = Pr(y_{it} = r | y_{i,t-1} = 0)$ for $r = P, T, E$. We estimate 3 equations contrasting event type r with no event:

$$\log\left(\frac{p_{it}^{r}}{p_{it}^{0}}\right) = \alpha^{r} D_{ti} + \beta^{r} X_{it} + u$$

As stated in Section 4.1, we use two forms of αD_{ti} to model the impact of tenure: a step function of t and a quadratic function of t. The variables of interest, X_{it}, come in two sets: time-variant connection variables and time-invariant controls. The first connection variable denotes whether the party secretary is still in the city committee or not (Treatment 1). The variable is 0 at the beginning and turns 1 when the party secretary exits. The second connection variable denotes whether the party secretary gets a promotion or not (Treatment 2). This variable also starts at 0 and turns 1 once the party secretary exits the city committee with a promotion. Time invariant controls include age of the committee member at assignment, education level, ethnicity, gender, city fixed effects, and year fixed effects. We report the statistical results that show the chance for promotion compared with the status quo. We present the full set of β^{P} under different definitions of promotion and varying inclusion of controls. In robustness, we (1) estimate β using selective subsets of the data, and (2) estimate β^{P} with a binary response model treating all other types of events as censored.

3.5 Results

Assignment Patterns in City Committees

One prerequisite of our main analysis is that the connection variable captures meaningful relationships between the party secretary and the standing committee members. As we have described in Section 3.2.1, the party secretary has a formal role in the assignment of new committee members and the transition of existing ones. Empirically, we observe that the entry of a party secretary brings a high influx of new members. Symmetrically, a secretary's exit leads to rapid cleansing of the committee so that there is not much variation in committee size over time. As outsiders to the system, we cannot tell who exactly is pulling the promotion levers, but the evidence is consistent with the hypothesis that the party secretary has a high level of control over the appointment to the city committee. In this section, we will argue that the observed patterns are not solely the result of political cycles, the design of the city committees, or provincial interventions. After ruling out the obvious alternative explanations, we argue that the synchronized movements suggest that the party secretary plays an active role in forming and managing ties with standing committee members.

The entry of a new party secretary produces immediate turnover in the city committee. We observe an influx of new committee members and rapid exits of members connected with the previous party secretary. Out of all standing committee assignments, 45.8% take place within the first year of the arrival of a new party secretary, and 65.0% within the first two years. If assignments of committee members were to occur evenly over time, only 28.4% of the assignments should take place within the first year based on an average tenure of 3.7 years. In terms of the number of assignments, we observe 3.7 assignments within the first year of the party secretary's entry, and that number decreases to 2 per year for the rest of the party secretary's term (see Figure 3.6 in Appendix).

To make space for assignments of new committee members, existing members of the city committee quickly exit the committee when a new party secretary comes in. Figure 3.8 in Appendix plots the exit timing of the standing committee members relative to the turnover of the party secretary. We observe higher than usual turnover right after the exit (assignment) of the current (new) party secretary. 30% of all the committee members' exits take place within one year of the appointment of a new party secretary. The spike in the exit rate is driven by the exit of people who are connected with the party secretary leaving office. In Figure 3.9, we plot the transition probability of committee members connected with party secretary whose tenure equals 2, 3, 4, and 5 years. Regardless of the length of the party secretary's tenure, we observe the highest turnover rate after the connected leader leaves office.

The correlation in entry and exit timing alone is not proof that the party secretary is actively recruiting and managing committee members. The correlation could arise mechanically out of a personnel management system with periodic top to bottom turnover. In theory, each city committee has a standard term of five years, and if a higher than usual number of replacements took place at the end of the term, we would expect the assignment timing of the party secretary and the committee members to align. To rule this out as the main contributing factor, we run a regression of the number of appointments on whether a party secretary was recently assigned to the city while controlling for potential political cycles. The baseline regression model is:

$$\text{Number of New Appointments}_{c,t} = \beta_0 + \beta_1 \text{NPS}_{c,t} + \beta_2 \text{NPS}_{c,t-1} + \beta_3 \text{NPS}_{c,t-2} + \delta_c + \epsilon_{c,t} \quad (3.1)$$

where $\text{NPS}_{c,k}$ denotes whether city c got a new party secretary at year k. We include city fixed effects to control for unobserved heterogeneity in the city that resulted in different assignment rates. On top of the baseline model, we include years relative to the national congress to control for the reshuffling of politicians due to national events; in an alternative specification, we add year fixed effects to control for any macro-level events that may impact assignments. If the committee appointment cycles drive everything, β_1 should be zero after the inclusion of different controls. On the other hand, if there is coordination in entries, β_1 should remain positive. Results are reported in column (1) to (3) in Table 3.4 in the Appendix. The influx of committee members post party secretary entry remains high after controlling for national trends.

The correlation we observe could still be a feature of the institutional design of the city committee if city committees start the five-year term in different years. If the turnover year is different for each city, then a global control for political cycles will not capture the turnover cycle in each city. Empirically, however, the turnover of party secretaries rarely aligns with the five-year schedule: only 28.2% of party secretaries were in office for the length of the standard term plus or minus one year (for full distribution see Figure 3.5 in Appendix).

To completely rule out this possibility, we run two alternative specifications to the regression in (1). In the first one, we include a separate set of national congress controls for each city, allowing each city to have a different five-year cycle; in the second specification, we exclude assignments that are most likely to be the result of committee reshuffling. We do so by excluding assignments by party secretaries whose predecessor stayed for the full term[6]. We present the results in columns (4) and (5) in Table 3.4. β_1 remains significant after controlling for individual city cycles and the out-of-sync assignments. Further analysis shows that the arrival rate of new committee members is independent of the normality or abnormality of the previous party secretary's term. In Figure 3.7, we plot the number of first-year assignments

[6]The reasoning here is that if the previous party secretary was in sync with the committee's five-year cycle, then assignments by the current party secretary are more likely due to systematic reshuffling.

of the new party secretary against the tenure of the previous party secretary and we observe no correlation between the two measures.

The turnover patterns we present suggest that the entries and exits of committee members are highly dependent on the entry and exit of the connected party secretary. The correlation is significant even after controlling for alternative reasons such as turnover cycles. The evidence is consistent with the hypothesis that the party secretary is actively involved in the careers of his connected members, providing validation for the measure of connection we use for the next part of the paper.

Non-parametric Analysis of Promotion Likelihood

Before diving into regression analysis, we start with the non-parametric estimation of the committee members' promotion prospects under different treatments. Table 3.1 has three parts: in (A), we document the sample size for each treatment group and year in office; in (B), we calculate promotion likelihoods under each treatment and year; and in (C), we repeat the analysis using a broader definition of committee member promotion. In the notes under Table 3.1, we walk through the numbers for an example case of $t = 3$. The non-parametric analysis is a first pass at evaluating the value of connection. We break down the data by tenure and treatments only and do not control for any other supply or demand-side factors that would impact promotion likelihood.

The first two rows of Table 3.1(A) document the number of committee members in the city committee at the beginning of year t. The values in the two rows decrease over time as more committee members exit the city committee each year. As t increases, more committee members transition from having their connected party secretaries in the city committee to having them out of the city committee. The high correlation between the committee member's tenure and getting treatment 1 is why controlling for tenure is essential for our estimation. In the next few rows of (A), we further break down the committee members whose party sectaries have exited the city committee by the outcome of the party secretaries (Treatment 2). The percentage of committee members whose connected party secretaries are promoted varies year by year with no noticeable pattern.

After counting the number of data points in each treatment, we present the empirical average promotion likelihood for each group in Table 3.1(B). We first break down the likelihood by whether the connected party secretary is still in office or not (Treatment 1). The variance of promotion rate overtime is low, and promotion is possible at

Table 3.1: Nonparametric estimation of promotion likelihood

	Year t							
	1	2	3	4	5	6	7	8+
(A) Number of committee members in the city committee given								
Party secretary in	1002	551	314	138	58	18	5	1
Party secretary out	244	387	409	373	292	215	158	201
-PS promoted	48	94	61	79	53	32	19	57
-PS not promoted	196	293	348	294	239	183	139	144
-% promoted	19.7%	24.3%	14.9%	21.2%	18.2%	14.9%	12.0%	28.4%
(B) Likelihood of promotion for committee members in year t given								
Party secretary in	6.6%	8.3%	9.2%	9.4%	5.2%			
-PS promoted*	2.5%	5.7%	9.1%	10.9%	10.5%			
Party secretary out	7.0%	4.9%	8.6%	7.0%	12.7%	6.5%	10.8%	9.9%
-PS promoted	9.3%	4.4%	10.8%	5.7%	14.4%	5.5%	14.0%	8.4%
(C) Likelihood of non-retirement movement for committee members in year t given								
Party secretary in	11.2%	12.7%	15.0%	13.0%	10.3%			
-PS promoted*	4.5%	9.4%	12.8%	13.0%	15.8%			
Party secretary out	11.1%	8.3%	13.0%	12.3%	17.1%	13.0%	13.3%	15.8%
-PS promoted	14.4%	8.0%	16.2%	10.6%	16.5%	11.0%	14.0%	11.8%

Notes: Let us take $t = 3$ as an example. Documented in (A) is the breakdown of the number of data points we have in each treatment group at year 3. At the beginning of year three, 723 committee members are in the city committee, 315 of whom are working with their connected party secretaries and 409 are working with new party secretaries. Out of the 409 committee members with new party secretaries, 61 (14.9%) saw their previous and connected party secretaries leave with a promotion and 348 (85.1%) saw their previous and connected party secretaries leave without one. Documented in (B) is the promotion likelihood at year 3 given each type of treatment. For the 314 committee members whose connected party secretary is in the city committee, 9.2% are promoted out of the committee member job by the end of year 3; and for the 409 committee members working with new party secretaries, 8.6% are promoted. If we further condition on the party secretary's outcome, for the 61 committee members whose party secretaries have exited with a promotion, 10.8% of them are promoted in year 3. The stared (*) PS promoted category is calculated using the party secretaries' outcomes in the future of the committee member's promotion. For the committee members whose party secretaries are still in office but would be promoted later on, the probability of promotion is 9.1%. In (C), we document the same likelihood as in (B) but with a broader definition of promotion. In (B) and (C), promotion likelihood under party secretary in for $t \geq 6$ is dropped from the table due to small number of data points.

any point of the committee member's tenure for both groups. This suggests that the committee member's promotion is not determined by one particular mechanism. For example, if only the committee members sprinting with small steps get promoted, we would observe a high likelihood in the beginning and a lower likelihood as t increases. If only experience matters, then the probability would rise after year 3 or 4. Alternatively, if connection with the current party secretaries is essential for promotion, then the likelihood under party secretary in-committee should be much larger than that under party secretary out-of-committee. The fact that there is no noticeable pattern over time or between the two groups indicates that a combination of factors contributes to the committee member's promotion likelihood. We observe similar patterns in (C) after broadening the definition of promotion.

To estimate the impact of Treatment 2, we calculate the promotion likelihood of only committee members whose party secretaries were promoted after leaving. If connection with promoted party secretaries is valuable, then promotion likelihood under party secretary promoted should be higher than under party secretary out. We observe no clear difference in promotion likelihood for one group over the other in either (B) or (C). Having a connected patron in power does not consistently lead to better committee member outcomes. Based on the non-parametric analysis, we cannot reject the hypothesis that connections matter, but it seems that connection with the party secretary is not the dominant factor in the committee members' promotion at this stage. Since population characteristics change over time and the non-parametric analysis does not account for these changes, we proceed to regression analysis in the next section.

Regression Analysis

Table 3.2 summarizes the results of the multinomial logit analysis. We present the set of β^P here and estimations of β^T and β^E can be found in Section 3.7 in Appendix. As stated in Section 3.4, we use two functional forms to model tenure: a step function of t in (1) to (3) and a quadratic function of t in (4) to (6). We start with the most basic regression of the two treatments and the age of the committee member in (1) and (4). Since other individual characteristics may lead to different baseline promotion rates, we add additional individuals level controls in (2) and (5). The timing and location of the committee member assignments could also have an impact on the baseline promotion rate, hence, we add assignment level controls in (3) and (6). As we can see in Table 3.2, most coefficients are stable across the six regression setups.

Table 3.2: Multinomial logit analysis on chance for promotion on committee members

	Dependent variable:					
	Committee member promotion					
	(1)	(2)	(3)	(4)	(5)	(6)
PS out	0.060	0.009	-0.155	-0.137	-0.188	- 0.453**
	(0.153)	(0.155)	(0.171)	(0.156)	(0.159)	(0.177)
PS promotion	0.225	0.276*	0.175	0.223	0.276*	0.172
	(0.165)	(0.167)	(0.193)	(0.165)	(0.167)	(0.194)
Start age	-.055***	- .051***	- .058***	- .052***	- .048***	- .054***
	(0.013)	(0.014)	(0.015)	(0.013)	(0.014)	(0.015)
Education - bachelors		0.053	0.090		0.068	0.095
		(0.228)	(0.246)		(0.228)	(0.247)
Education - masters		0.240*	0.302**		0.242*	0.310**
		(0.138)	(0.146)		(0.139)	(0.147)
Female		-0.343*	-0.358*		-0.332*	-0.342
		(0.202)	(0.208)		(0.202)	(0.209)
Han		-0.163	-0.031		-0.172	-0.009
		(0.538)	(0.559)		(0.540)	(0.563)
InSync			-0.137			-0.212*
			(0.127)			(0.129)
Step function	Yes	Yes	Yes			
Quadratic function				Yes	Yes	Yes
City FE			Yes			Yes
Number of observations	4698	4624	4624	4698	4624	4624

Notes: Only β^P is presented here.

Losing the connected party secretary harms committee member's promotion likelihood, although the coefficients are not all negative and are only significant in the last specification. Post exit, promotion of the connected party secretary increases the promotion likelihood of the committee members across specifications but only estimates in columns (2) and (5) are statistically significant. The coefficients support the hypothesis that the party secretary has more influence when he is still in office and when he gets a promotion, although that impact is not well identified based on the current data.

To get a better idea of the quantitative impact of various factors, we calculate the odds ratio using coefficients from (6). The exit of the connected party secretary leads to a change of odds of promotion by a factor of 0.63, which means that individuals without a connected party secretary in office are 37% less likely to be promoted in each period after the connected party secretary's exit. The promotion of the party secretary post-exit increases the odds ratio by a factor of 1.19, which means that connection with a successful party secretary increases the probability of promotion by 19% in each period. The value of connection with a successful party secretary is positive but not enough to offset the negative impact of the party secretary's exit from the city committee. Other statistically significant coefficients include start age, master's degree, and InSync. Each one-year increase in start age decreases the odds by 0.94, having a master's degree increases the odds by 1.27, and arriving in sync with the party secretary decreases the odds by 0.80.

Since our coefficients are not well identified, we stratify the data to check if the value of connection is more significant for specific subsets of individuals. We run the regression in Table 3.2 column (3) and only present the key variables. First, we stratify the data by the start age of the committee members. The underlying idea is that younger individuals sprinting with small steps through the system might benefit differently from the connection as compared to individuals closer to the retirement age. The coefficients are documented in (1) to (3) of Table 3.6(A) in Appendix. The exit of the connected party secretary has a similar negative effect on the promotion likelihood of the younger and the older cohort, but the party secretary's promotion has a positive benefit only on the younger cohort. However, in all specifications, no coefficients are statistically significant at conventional levels.

Second, to ensure no one province is steering the coefficients in a specific direction because of some province-specific promotion practices, we run the same regression on subsets of the data dropping each province at a time. Results are presented in (5) to (8) of Table 3.6(B). The coefficient estimates of PS out are consistently negative, while the estimates of PS promotion are unstable. Provincial norms matters for whether connections to successful party secretaries are valuable or not.

Third, to rule out any bias created by the codification of promotion, we run the same regression using more inclusive definitions of positive outcomes. The results are reported in (9) to (12) of Table 3.6(C). Broadening the definition of promotion for the committee members has no impact on our estimates, confirming our intuition that promotion and horizontal transitions are both positive outcomes for the committee members. Committee members are more likely to move up or horizontally if their connection stayed in the committee or got promoted. Broadening the definition of promotion for the party secretaries flips the coefficients. We expect the coefficients to be different since there is a qualitative difference between promotion and horizontal movement for the party secretaries. A party secretary who received an increase in rank wields considerably more influence than a party secretary who moved horizontally. The comparison suggests that only the party secretaries who moved to consequential positions were able to help their former subordinates. However, the interpretations here are tentative since no coefficients are significant at conventional levels. These findings are consistent with the prior observation that performance play a larger role in the career advancement of lower-level officials.

We repeated the main analysis using a multivariate logit model, treating all other outcomes as censored. The estimates, shown in Table 3.5, are similar to our current results. Putting the different pieces together, we find some evidence that ties with the current party secretary and with promoted party secretaries are valuable, but the estimates are not significant at conventional levels for most configurations or subsets of the data. Young age, education, and being male is beneficial for promotion as we would expect. These factors are well-identified in most configurations.

3.6 Conclusion

To summarize, analysis on assignment timing suggests that our measure of connection captures real relationships between the party secretary and the committee members. The evidence is consistent with the hypothesis that the party secretaries are actively involved in the appointments of members of the committees they run. Despite having a close relationship, in the non-parametric analysis, we find that an active connection with the current party secretary or the increase of power of a connected party secretary does not increase the committee member's promotion likelihood. After controlling for other determinants of promotion in the regression analysis, we find that the outcomes of the committee member is slightly better when his connection is still in office or if he leaves with a promotion. Both effects are small and not well identified.

Based on previous findings in the literature, we expected connection to be consequential in the Chinese bureaucracy. The positive benefit of connection is well identified in many articles, even with connection variables loosely defined as shared work experience, hometown, or education ties. The differences between our findings and results in the literature highlight that the value of connection is not homogeneous across the system. Some connections, which the literature is more likely to select by focusing on top of the hierarchy, prove to be beneficial. However, China is too large to be run solely by individuals connected to the party secretary general or members of the Politburo. The majority of the local bureaucrats never made it to the radar of core party leaders, but the connections they have made with their local leaders could still be beneficial, which is what this paper tests for.

Our results do not negate the existing understanding that connections are consequential at the top. There are many potential reasons why the city party secretary connection is less valuable immediately after the city committee assignment: reputation cost of faction building could be higher, the benefit of having connected clients could be lower, stricter institutional constrain placed on the party secretaries because they are often promoted to vice-leadership positions of an organization, and so on. The natural follow-up question to our research is: do the connections formed in the city committee resurface in subsequent stages of someone's career? If so, at what level? Based on previous literature and news articles on China, we know that individuals benefit from network ties formed early on with someone who later becomes a core party leader[7]. Subsequent scholars could use our methodology to trace forward in time to study if connections formed in the city committee become beneficial in the later stages of one's career. Tracing one type of connection through time would allow scholars to make precise statements on when ties formed in the cities start to matter in the Chinese hierarchy.

Our study adds to the current understanding of Chinese bureaucracy in the literature by showing that the value of connection is not standardized across individuals and different levels of the system. As Doyon and Keller (2020) suggest, the value of connection, even within a large group of similarly ranked people, is sensitive to exactly to whom one is connected. In future works, we want to evaluate other types of connections formed at the city level to completely rule out the possibility that connections we did not account for in this paper matter. We also want to apply our methodology to higher levels of the bureaucracy, such as the provincial committee. If we find that the exact type of connection ties are more valuable in the provincial committee, we would be able to strengthen our statement that the difference in level drives differences in results.

[7]There are numerous articles on how the current party secretary general promotes his former subordinates into key positions. Source: link1 and link2

3.7 Appendix

Missing Data Problem

Missing Cities

We are able to document the turnover data of 46 out of the 62 prefecture-level cities from Guangdong, Zhejiang, Fujian, Jiangsu, and Shandong province. In Figure 3.1, we compare the population and the per person GDP of the in sample cities with that of the missing cities. The in-sample cities are larger in population and higher in per capital GDP. The differences in city size motivates our inclusion of city fixed effects in some of our specifications. While it is more likely that committee members from affluent cities have a high likelihood of promotion, our connection variable would not be biased as long as the value of connection with a successful party secretary is not correlated with the size of the city.

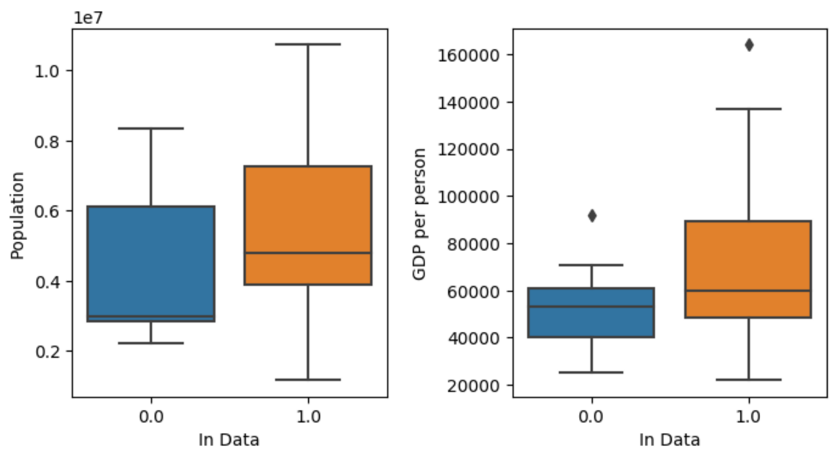


Figure 3.1: Characteristics of in-sample vs. missing cities

Missing Individuals

Data attrition occurs at different stages of the data collection process:

- We start with the full sample of individuals listed in the city yearbooks (S1).
- We remove ineligible individuals, including military personnel (E1) and left and right truncation individuals (E2) to arrive at the sample of eligible individuals (S2). We remove military personnel because their career trajectories are different from civil servants. Right truncation refers to individuals who are still in the city committee by the end of our sample period. We do not know the outcome of right-truncated individuals. Left truncation refers to individuals who were already assigned to the city committee when our sample period begins. We do not know who assigned the left-truncated individuals into the city committee and when that happened.

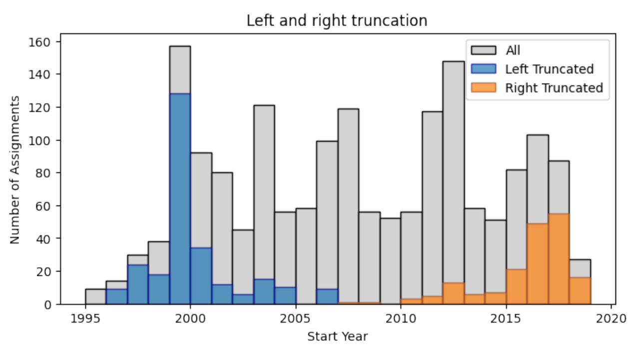


Figure 3.2: Left and right truncation by year

- We remove individuals with key information missing (date of birth (E3) and outcome (E4)) and arrive at our final sample (S3).

Table 3.3: Data attrition by province

	Total CM (S1)	Military (E1)	Truncation (E2)	Eligible (S2)	No DoB (E3)	No outcome (E4)	Valid (S3)	Valid % (S2/S3)
Zhejiang	185	5	155	150	3	21	128	85.3%
Fujian	127	3	95	92	3	16	76	82.6%
Guangdong	366	14	278	264	8	58	198	66.7%
Jiangsu	485	53	347	294	55	60	215	73.1%
Shandong	423	40	337	297	44	73	221	83.7%
Total	1586	115	1212	1097	113	228	838	76.4%

The only problematic attrition of the data is the removal of individuals with key information missing. The percentage of individuals with incomplete information ranges from 15% in Zhejiang to 34% in Guangdong, the average being 24%. To get an idea of why individuals were missing, we randomly chose a city and tracked down all missing individual through news reports and party documents. We were able to find most of the missing individuals and infer their outcomes from secondary sources. The majority of these individuals either directly transferred to the People's Congress or the Political Consultative Conference in the city (retirement positions), or exited right away after the city committee. In robustness regressions, we make the assumption that all missing individuals retire directly after the city committee and infer their date of birth based on the retirement time. The inclusion of the missing individuals does not change our regression results.

Categorizing Outcomes

Categorizing Party Secretary Outcomes

See PDF attached on the next page.

地区级别	单位	具体职务	Location	Organization	Job Title	Outcome	Count
				Center			
中央	海关总署	党组书记	Center	General Administration of Customs	Party Secretary	promotion	1
中央	中央人民政府驻澳门特别行政区联络办公室	副主任	Center	Liaison Office of the Central Government in the Macao Special Administrative Region	Vice Director	horizontal	1
				Province			
省	省委	常委	Province	Provincial Committee	Standing Member	promotion	23
省	省委	副书记	Province	Provincial Committee	Vice Party Secretary	promotion	2
省	省委	秘书长/副秘书长	Province	Provincial Committee	Secretary General/Vice Secretary General	horizontal	3
省	省政府	副省长	Province	Government	Vice Governor	promotion	40
省	省政府	秘书长	Province	Government	Secretary General	horizontal	5
省	省政府	省长助理	Province	Government	Assistant to the Governor	horizontal	4
省	省政府	党组成员	Province	Government	Committee Member	horizontal	1
省	省政府	参事	Province	Government	Counselor	horizontal	1
省	组织部	常务副部长/副部长	Province	Organization Department	Vice Minister	horizontal	2
省	统战部	部长/副部长	Province	United Front Work Department	Minister/Vice Minister	horizontal	3
省	省委党校	校长	Province	Party School	Director	horizontal	1
省	发展和改革委员会	党组书记	Province	Commission of Development and Reform	Party Secretary	horizontal	3
省	经济和信息化委员会	书记/主任	Province	Commission of Economy and Informatization	Party Secretary/Director	horizontal	2
省	城乡建设与环境资源保护委员会	副主任委员	Province	Commission of Urban and Rural Construction and Environmental Resources Protection	Vice Director	horizontal	1
省	审计厅	厅长	Province	Department of Audit	Director	horizontal	1
省	民政厅	厅长	Province	Department of Civil Affairs	Director	horizontal	1
省	农业厅	厅长	Province	Department of agriculture	Director	horizontal	1
省	司法厅	党委书记/厅长	Province	Department of Justice	Party Secretary/Director	horizontal	1
省	公安厅	厅长	Province	Department of Public Security	Director	horizontal	2
省	国土资源厅	党组书记/厅长	Province	Department of Land and Resources	Party Secretary/Director	horizontal	2
省	劳动和社会保障厅	厅长	Province	Department of labor and social security	Director	horizontal	1
省	住房和城乡建设厅	厅长	Province	Department of housing and urban-rural development	Director	horizontal	1
省	食品药品监督管理局	局长	Province	Food and Drug Administration	Director	horizontal	1
省	工商行政管理局	党组书记/局长	Province	Department of Business Administration	Director	horizontal	1
省	供销合作社	主任/副主任	Province	Supply and Marketing Cooperative	Director/Vice Director	horizontal	1
省	社会科学院	党委书记	Province	Academy of Social Sciences	Party Secretary	horizontal	1
省	人大常委会	主任/副主任/党组书记	Province	Standing Committee of People's Congress	Director/Vice Director/Party Secretary	retirement	21
省	政协常委会	副主席/党组成员/常委	Province	Standing Committee of People's Consultative Conference	Vice Chairman/Standing Member	retirement	15
				Vice-provincial Level City			
副省级市	市委	书记	VPL City	City Committee	Party Secretary	promotion	2
副省级市	市委	市委副书记	VPL City	City Committee	Vice Party Secretary	horizontal	8
副省级市	政府	市长	VPL City	Government	Mayor	promotioin	9
副省级市	政协常委会	主席/副主席	VPL City	Standing Committee of People's Consultative Conference	Chairman/Vice Chairman	retirement	3
				Prefectural Level City			
地级市	市委	市委书记	City	City Committee	Party Secretary	horizontal	10
地级市	市委	市委副书记	City	City Committee	Vice Party Secretary	horizontal	2
地级市	军区	第一书记	City	Military	Party Secretary	horizontal	5
地级市	市委党校	校长	City	Party School	Director	horizontal	9
地级市	人大常委会	主任/副主任/党组书记	City	Standing Committee of People's Congress	Director/Vice Director/Party Secretary	retirement	35
地级市	经济技术开发区管委会	主任	City	Committee of Economic and Technological Development Zone	Director	promotion	1
				Other			
	国有企业	董事长/党组书记/总经理		State Owned Enterprise	Chairman/party secretary/GM	horizontal	8
	亚洲开发银行	中西亚局首席城市发展专家		Asian Development Bank	Chief Expert	horizontal	1

Categorizing Committee Member Outcomes

See PDF attached on the next page.

地区级别	单位	具体职务	Location	Organization	Job Title	Outcome	Count
Center							
中央	外交部	国外工作局局长	Center	Ministry of Foreign Affairs	Director of X Bureau	promotion	1
中央	组织部	党建研究所所长	Center	Organization Department	Head of Party Building Research Institute	promotion	1
中央	组织部	x局副局长	Center	Organization Department	Vice Director of X Bureau	promotion	3
中央	财经领导小组办公室	经济x组副组长	Center	Office of Financial Leadership	Vice Leader of Group X	promotion	1
中央	发改委	办公厅副主任	Center	Development and Reform Commission	Vice Director of General Office	promotion	1
中央	国务院法制办公室	秘书行政司司长	Center	Legislative Affairs Office of State Council	Director of the Secretary and Administration Department	promotion	1
Direct-Administered Municipalities							
直辖市	区委	副书记	Municipality	District Committee	Vice Party Secreatry	promotion	1
直辖市	县委书记	书记	Municipality	District Committee	Party Secreatry	horizontal	1
直辖市	区政协	主席	Municipality	District's Consultative Conference	Chairman	retirement	1
Province							
Party Commitee							
省	省委	副秘书长	Province	Provincial Committee	Vice Secretary General	promotion	3
省	省纪委	副书记	Province	Commission for Discipline Inspection	Vice Party Secretary/Standing Member	promotion	21
省	省纪委	派驻省地方纪检组组长	Province	Commission for Discipline Inspection	Leader of Inspection Team Dispatched to Cities	horizontal	6
省	省纪委	委员	Province	Commission for Discipline Inspection	Committee Member	horizontal	1
省	省委组织部	副部长	Province	Organization Department	Vice Minister	promotion	5
省	省委宣传部	副部长	Province	Propaganda Department	Vice Minister	promotion	4
省	省委政法委	副书记	Province	Commission for Political and Legal Affairs	Vice Party Secretary	promotion	2
省	省委巡视组	组长	Province	Provincial Committee Inspection Team	Team Leader	horizontal	12
省	省委巡视组	巡视员	Province	Provincial Committee Inspection Team	Inspector	retirement	1
省	省委改革办(发改委)	常务副主任	Province	Reform Committee	Deputy Vice Director	promotion	1
省	省委党史工办	主任/副主任	Province	Office of Party History	Director	retirement	3
省	省委台湾工办	副主任	Province	Office of Taiwan Affairs	Vice Director	retirement	1
Province							
Government, Organizations and Commissions							
省	政府	办公厅副主任	Province	Government	Vice Office Director	promotion	3
省	政府	副秘书长	Province	Government	Vice Secretary General	promotion	6
省	共青团	书记	Province	Youth League	Party Secretary	promotion	4
省	共青团	副书记	Province	Youth League	Vice Party Secretary	horizontal	1
省	妇联	主席/党组书记	Province	Women's Federation	Director/Party Secretary	promotion	2
省	妇联	副主席	Province	Women's Federation	Vice Director	horizontal	2
省	作协	党组书记	Province	Wriers Association	Party Secretary	horizontal	1
省	侨联	党组书记	Province	Federation of Oversea Chinese	Party Secretary	promotion	1
省	省总工会	副主席	Province	Worker's Union	Vice Chairman	horizontal/retirement	1
省	省经济贸易委员会	党组成员	Province	Economic and Trade Commission	Comittee Member	horizontal	1
省	省发展和改革委员会	副主任	Province	Development and Reform Commission	Vice Director	promotion	3

地区级别	单位	具体职务	Location	Organization	Job Title	Outcome	Count
省	省经济和信息化委员会	副主任/党组成员	Province	Economics and Information Commission	Committee Member	horizontal	3
省	省国有资产监督管理委员会	党委副书记	Province	SOE Supervision and Administration Commission	Vice Party Secretary	promotion	3
省	省国有资产监督管理委员会	副主任	Province	SOE Supervision and Administration Commission	Vice Director/Vice Party Secretary	horizontal	3
省	省海洋港口发展委员会	副主任	Province	Marine Port and Development Commission	Vice Director	horizontal	1
省	省检查委员会	委员	Province	Inspection Commission	Committee Member	horizontal	1
Province							
Departments, Bureaus, and Offices							
省	省公安厅	党委副书记/副厅长/党委常委	Province	Department of Public Security	Vice Party Secretary/Vice Director/Standing Committee Member	promotion	12
省	省公安厅	党委委员/巡视员/纪委书记	Province	Department of Public Security	Committee Member/Inspector/Secretary of Discipline	horizontal/promotion	9
	省民政厅	副厅长	Province	Department of Civil Affairs	Vice Director	horizontal	2
省	省建设厅	党组书记	Province	Department of Construction	Party Secretary	promotion	1
省	省教育厅	副厅长	Province	Department of Education	Vice Director	horizontal	2
省	省监察厅	副厅长	Province	Department of Supervision	Vice Director	horizontal	3
省	省水利厅	厅长	Province	Department of Water Resources	Director	promotion	1
省	省水利厅	党组副书记	Province	Department of Water Resources	Vice Party Secretary	horizontal	1
省	省商务厅	厅长/党组书记	Province	Department of Commerce	Director/Party Secretary	promotion	2
省	省商务厅	副厅长	Province	Department of Commerce	Vice Director	horizontal	2
省	省交通厅	党组副书记	Province	Department of Transportation	Vice Party Secretary	promotion	3
省	省文化厅	党组成员	Province	Department of Culture	Committee Member	horizontal	2
省	省林业厅	副厅长	Province	Department of Forestry	Vice Director	horizontal	2
省	省审计厅	副厅长	Province	Department of Audit	Vice Director	horizontal	5
省	省司法厅	副厅长	Province	Department of Law and Justice	Vice Director	horizontal	3
省	省环境保护厅	厅长	Province	Department of Environmental Protection	Director	promotion	2
省	省环境保护厅	副厅长	Province	Department of Environmental Protection	Vice Director	horizontal	1
省	省科学技术厅	党组副书记	Province	Department of Science and Technology	Vice Party Secretary	horizontal	1
省	省科学技术厅	副厅长	Province	Department of Science and Technology	Vice Director	promotion	1
省	省国家安全厅	厅长	Province	Department of Homeland Security	Director	promotion	1
省	省国土资源厅	副厅长	Province	Department of Land and Resources	Vice Director	horizontal	1
省	省国土资源厅	巡视员	Province	Department of Land and Resources	Inspector	retirement	1
省	省文化和旅游厅	党组成员	Province	Department of Culture and Travel	Committee Member	horizontal	1
省	省人力资源和社会保障厅	副厅长	Province	Department of Human Resources and Social Security	Vice Director	horizontal	5

地区级别	单位	具体职务	Location	Organization	Job Title	Outcome	Count
省	省档案局	局长	Province	Bureau of Archives	Director	horizontal	1
省	省信访局	副局长	Province	Bureau of Letters and Calls	Vice Director	horizontal	2
省	省民防局	副局长	Province	Bureau of Civil Defense	Vice Director	horizontal	1
省	省旅游局	党组书记/局长	Province	Department of Travel	Director/Party Secretary	horizontal	4
省	省统计局	局长	Province	Bureau of Statistics	Director	horizontal	1
省	省地税局	副局长	Province	Bureau of Land Tax	Vice Director	horizontal	1
省	省地质局	副局长	Province	Bureau of Land Geology	Vice Director	horizontal	2
省	老干部局	副局长	Province	Bureau of Retired Cadres	Vice Director	horizontal	1
省	省公务员局	局长	Province	Bureau of Cadre Management	Director	horizontal	1
省	省新闻出版局	副局长	Province	Bureau of News and Publication	Vice Director	horizontal	3
省	省医疗保障局	局长	Province	Bureau of Healthcare and Social Security	Director	horizontal	1
省	省广播电视局	副局长	Province	Bureau of Broadcast and Television	Vice Director	horizontal	2
省	省监狱管理局	党委书记	Province	Bureau of Prison Management	Party Secretary	horizontal	1
省	省海洋与渔业局	副局长	Province	Bureau of Oceans and Fisheries	Vice Director	horizontal	1
省	省工商行政管理局	副局长	Province	Bureau of Industry and Commerce	Vice Director	horizontal	3
省	省农业资源开发局	局长	Province	Bureau of Agricultural Resources Development	Director	horizontal	1
省	省安全生产监督管理局	局长	Province	Bureau of Work Safety	Director	promotion	3
省	省质量技术监督局	副局长	Province	Bureau of Quality and Technical Supervision	Vice Director	horizontal	1
省	国家自然资源督查广州局	局长	Province	Natural Resources Supervision x Division	Director	horizontal	1
省	国家粮食和物资储备局吉林局分	党组书记		National Grain and Material Reserve Bureau x Branch	Party Secretary	horizontal	1
省	省文明办/政务服务管理办公室/法制办公室/铁路办公室/金融办/省扶贫办/防空办	副主任	Province	Office of X	Vice Director	horizontal	8
Province							
Others							
省	省人大常委会	委员	Province	Standing Committee of People's Congress	Committee Member	retirement	2
省	省政协	副秘书长	Province	Political Consultative Conference	Vice Secretary General	retirement	1
省	省政协	港澳台委员会副主任	Province	Political Consultative Conference	Vice Director of Hongkong, Taiwan, and Macau Affairs	retirement	1
省	省高级人民法院	党组成员	Province	The Supreme People's Court	Committee Member	promotion	1
省	省贸促会	副会长	Province	Trade Promotion Commission	Vice Chairman	horizontal	1
省	省直属机关工委	副书记	Province	Work Committee for Offices Directly under the Province	Vice Party Secretary	horizontal	4
省	省哲学社会科学界联合会	党组书记	Province	Federation of Philosophy and Social Sciences	Party Secretary	retirement	1
省	省政府驻北京办事处	主任	Province	Provincial Office in Beijing	Director	promotion	1

地区级别	单位	具体职务	Location	Organization	Job Title	Outcome	Count
省	省供销合作社	党组书记	Province	Supply and Marketing Cooperative	Party Secretary	promotion	1
省	省农村信用社联合社	党委副书记	Province	Rural Credit Cooperative Association	Vice Party Secretary	horizontal	1
省	省政府	侨务办公室主任	Province	Political Consultative Conference	Director of Overseas Chinese Affairs Office	promotion	1
省	省防范和处理邪教问题小组/省依法治省工作领导小组/支持浙商创业创新促进浙江发展工作领导小组	办公室主任/副主任	Province	Leading Group for x	Office Director/Vice Director	horizontal	3
省	省公共资源交易中心	党组书记	Province	Public Resources Trading Center	Party Secretary	horizontal	1
省	广州体育学院	党委书记	Province	Guangzhou Institute of Physical Education	Party Secretary	promotion	1
省	省属国企	董事长/总经理/党委书记	Province	State Owned Enterprises	Chairman of the Board/CEO/Party Secretary	promotion	22
省	省属国企	党委副书记/党委委员	Province	State Owned Enterprises	Vice Party Secretary/Committee Member	horizontal	3
省	省太湖水污染防治办公室	副主任	Province	Taihu Lake Water Pollution Prevention Office	Vice Director	horizontal	1
	xx区	党工委副书记/委员/管理委员会主任	Province	X New District	Work Committee Vice Director/Committee Member/ Director of Managing Comission	horizontal	3
Vice Provincial Level City							
副省级市	市委	常委	VP City	City Committee	Standing Member	promotion	9
副省级市	市委	副秘书长	VP City	City Committee	Vice Secretary General	horizontal	1
副省级市	市政府	副市长/党组副书记	VP City	Government	Vice Party Secretary/Vice Mayor	promotion	2
Prefectural Level City							
地级市	市委	市委副书记	City	Party Committee	Vice Party Secretary	promotion	172
地级市	市委	常委	City	Party Committee	Standing Member	horizontal	46
地级市	市委政法委	副书记	City	Commission for Political and Legal Affairs	Vice Party Secretary	horizontal	3
地级市	市委组织部	部长	City	Organization Department	Minister	horizontal	1
地级市	政府	常务副市长		Government	Deputy Vice Mayor	promotion/horizontal	9
地级市	政府	副市长/党组成员	City	Government	Vice Mayor/Committee Member	horizontal	15
地级市	政府	党组副书记	City	Government	Vice Party Secretary	horizontal	1
地级市	政府办公室	二级巡视员	City	Government	Inspector	retirement	1
地级市	x局/市委x部/x厅/x学院/x会	部长/局长/厅长/院长/会长	City	X Department/X Burea/X Commission	Head	horizontal	7
地级市	人大常委会	党组书记/主任/副主任/党组副书记/党组成员	City	Standing Committee of People's Congress	Director/Party Secretary/Vice Director/Vice Party Secretary/Committee Member	retirement	123
地级市	政协常委会	主席/党组书记/副主席/党组副书记/党组成员	City	Standing Committee of People's Consultative Conference	Chariman/Party Secretary/Vice Chairman/Vice Party Secretary/Committee Member	retirement	91
地级市	总工会	主席	City	Worker's Union	Chariman	retirement	1
地级市	监察委员会	主任	City	Supervisory Committee	Director	horizontal	1
County Level City							
县级市	市委	市委书记	County Level City	City Committee	Party Secretary	horizontal	1

地区级别	单位	具体职务	Location	Organization	Job Title	Outcome	Count
Other							
	X研究院	副院长		Chinese Academy of X	Vice Head	horizontal	2
	x大学	党委书记		X University	Party Secretary	promotion	3
	x学院	党委委员		X School	Committee Member	horizontal	3
	x石油管理局	党委副书记		X Petroleum Administration	Vice Party Secretary	horizontal	1
	正厅级岗位			Duputy Director Level Postings		promotion	3
	副厅级岗位			Vice Duputy Director Level Postings		horizontal	7
	其他岗位			Other Outside Positions		exit	17
	直接退休			Direct Exit		exit	60
	调查			Investigated		exit	42
	未知			Unknown		unknown	271

Guide on Categorizing Committee Member Promotions

1. All increases in rank from vice deputy-executive to deputy-executive level;
2. Vice party secretary or deputy vice mayor of prefecture level cities;
3. Vice party secretary or deputy vice director of important provincial ministries or departments. A ministry or department is considered important if the head of that organization is or has been a standing member of the provincial committee;
4. Vice party secretary or deputy vice director of deputy-executive level ministries or departments in the center.

Tables

Table 3.4: Regression analysis on assignment timing

	Dependent variable:				
	Number of new appointments				
	(1)	(2)	(3)	(4)	(5)
PS entry year	1.513***	1.526***	1.409***	1.684***	1.261***
	(0.224)	(0.222)	(0.218)	(0.245)	(0.311)
PS entry year + 1	0.231	0.395*	0.243	0.471*	0.464
	(0.230)	(0.231)	(0.226)	(0.259)	(0.321)
PS entry year + 2	-0.165	0.106	0.217	-0.117	0.160
	(0.241)	(0.241)	(0.233)	(0.271)	(0.336)
Constant	1.562***	1.922***	2.305***	2.726**	1.702*
	(0.558)	(0.570)	(0.647)	(1.137)	(0.947)
City FE	Yes	Yes	Yes		Yes
National Congress		Yes			Yes
Year FE			Yes		
City x National Congress				Yes	
Exclude prev. tenure = 5					Yes
Exclude prev. convicted					
Number of observations	840	840	840	840	395
R-square	0.093	0.132	0.216	0.391	0.162
Adjusted R-square	0.038	0.075	0.149	0.158	0.049

Notes: In regression (4), we exclude all assignments by party secretaries whose predecessors stayed for 4 to 6 years, and all assignments by party secretaries whose predecessors were not documented in our data set.

Table 3.5: Logit analysis on chance for promotion on committee members

	Dependent variable:					
	Committee member promotion					
	(1)	(2)	(3)	(4)	(5)	(6)
PS out	0.048	0.003	-0.145	-0.124	-0.166	-0.404**
	(0.152)	(0.154)	(0.170)	(0.155)	(0.157)	(0.176)
PS promotion	0.205	0.264	0.154	0.200	0.259	0.146
	(0.163)	(0.165)	(0.191)	(0.163)	(0.165)	(0.192)
Start age	-0.072***	-0.069***	-0.077**	-0.070***	-0.066***	-0.075***
	(0.013)	(0.014)	(0.015)	(0.013)	(0.014)	(0.015)
Education - bachelors		0.037	0.075		0.049	0.081
		(0.226)	(0.244)		(0.226)	(0.245)
Education - masters		0.236*	0.300**		0.233*	0.304**
		(0.137)	(0.145)		(0.138)	(0.146)
Female		-0.352*	-0.366*		-0.341*	-0.351*
		(0.201)	(0.208)		(0.201)	(0.208)
Han		-0.150	-0.032		-0.157	-0.016
		(0.534)	(0.555)		(0.535)	(0.558)
Same province		0.120	0.173		0.108	0.165
		(0.136)	(0.144)		(0.136)	(0.145)
InSync			-0.130			-0.192
			(0.126)			(0.128)
Step function	Yes	Yes	Yes			
Quadratic function				Yes	Yes	Yes
City FE			Yes			Yes
Number of observations	4698	4624	4624	4698	4624	4624

Table 3.6: Alternative specifications of the multinomial logit regression

	Dependent variable: Committee member promotion				
(A) Group by start age:	All	≤ 48	> 48		
	(1)	(2)	(3)		
PS out	-0.155	-0.204	-0.165		
	(0.171)	(0.213)	(0.315)		
PS promotion	0.175	0.294	-0.007		
	(0.193)	(0.242)	(0.377)		
(B) Exclude province:	GD	ZJ	FJ	JS	SD
	(4)	(5)	(6)	(7)	(8)
PS out	-0.122	-0.137	-0.250	-0.143	-0.083
	(0.196)	(0.190)	(0.181)	(0.197)	(0.197)
PS promotion	0.190	0.024	0.196	0.506**	-0.028
	(0.216)	(0.223)	(0.202)	(0.225)	(0.219)
(C) Vary promotion definition:	P1+C1	P1+C2	P2+C1	P2+C2	
	(9)	(10)	(11)	(12)	
PS out	-0.155	-0.184	0.080	-0.049	
	(0.171)	(0.137)	(0.200)	(0.162)	
PS promotion	0.175	0.150	-0.260	-0.124	
	(0.193)	(0.160)	(0.197)	(0.160)	

Notes: Only estimations of β^P for key variables are presented here. In part (A), we use the median start age of 48 as the cutoff for the two age cohorts. In part (B), (4) excludes Guangdong (GD), (5) excludes Zhejiang (ZJ), (6) excludes Fujian (FJ), (7) excludes Jiangsu (JS), and (8) excludes Shandong (SD). In part (C), P1 and C1 are the baseline definitions of promotion for the party secretary and the committee member. C2 and P2 broadens the definition to include horizontal movements of party secretaries and standing committee members.

Table 3.7: Multinomial logit analysis on chance for exit on committee members

	Dependent variable:					
	Committee member promotion					
	(1)	(2)	(3)	(4)	(5)	(6)
PS out	0.303*	0.224	0.199	0.056	-0.018	-0.138
	(0.163)	(0.166)	(0.184)	(0.167)	(0.171)	(0.193)
PS promotion	0.091	0.091	0.114	0.118	0.121	0.153
	(0.159)	(0.163)	(0.193)	(0.160)	(0.163)	(0.195)
Start age	0.283***	0.286***	0.300***	0.295***	0.300***	0.315***
	(0.019)	(0.020)	(0.021)	(0.019)	(0.021)	(0.022)
Education - bachelors		0.082	0.136		0.069	0.121
		(0.181)	(0.200)		(0.182)	(0.202)
Education - masters		0.069	0.074		0.140	0.131
		(0.136)	(0.147)		(0.137)	(0.149)
Female		-0.039	-0.074		-0.050	-0.096
		(0.232)	(0.242)		(0.234)	(0.244)
Han		-0.700	-0.529		-0.757	-0.553
		(0.475)	(0.486)		(0.475)	(0.487)
InSync			-0.040			-0.135
			(0.137)			(0.139)
Step function	Yes	Yes	Yes			
Quadratic function				Yes	Yes	Yes
City FE			Yes			Yes
Number of observations	4698	4624	4624	4698	4624	4624

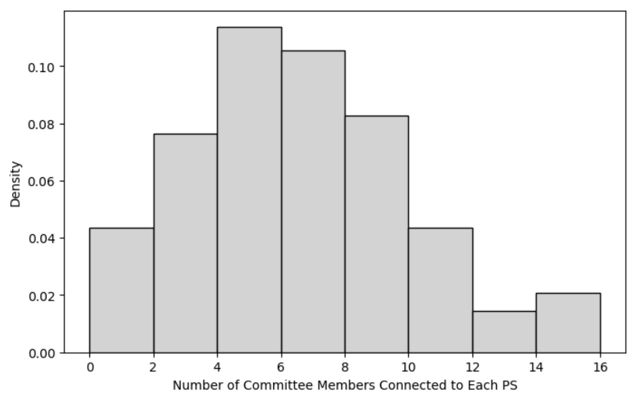
Table 3.8: Multinomial logit analysis on chance for horizontal movement on committee members

	Dependent variable:					
	Committee member promotion					
	(1)	(2)	(3)	(4)	(5)	(6)
PS out	0.035	0.006	-0.217	0.120	-0.158	-0.458**
	(0.186)	(0.188)	(0.207)	(0.194)	(0.196)	(0.218)
PS promotion	-0.084	-0.107	0.064	-0.072	-0.095	0.085
	(0.224)	(0.228)	(0.255)	(0.224)	(0.228)	(0.256)
Start age	-0.019	-0.011	-0.018	-0.015	-0.006	-0.013
	(0.016)	(0.017)	(0.018)	(0.016)	(0.018)	(0.019)
Education - bachelors		0.580*	0.453		0.596**	0.465
		(0.304)	(0.316)		(0.304)	(0.317)
Education - masters		0.047	0.041		0.059	0.054
		(0.160)	(0.168)		(0.160)	(0.168)
Female		-0.041	0.032		-0.040	0.036
		(0.231)	(0.236)		(0.231)	(0.236)
Han		0.781	1.006		0.759	1.012
		(1.018)	(1.029)		(1.018)	(1.030)
InSync			-0.241			-0.309*
			(0.159)			(0.161)
Step function	Yes	Yes	Yes			
Quadratic function				Yes	Yes	Yes
City FE			Yes			Yes
Number of observations	4698	4624	4624	4698	4624	4624

Figures

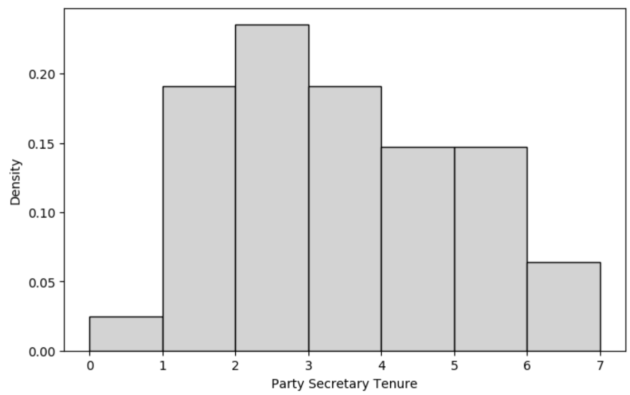
Level	Rank (EN)	Rank (CH)	Prefecture Level City Positions
1	National Leader	Guo (国)	
2	Subnational Leader	Fu Guo (副国)	
3	Provincial-Ministerial	Bu (部)	
4	Vice Provincial-Ministerial	Fu Bu (副部)	
5	Bureau-Director	Ting (厅)	Party secretary, mayor
6	Vice Bureau-Director	Fu Ting (副厅)	Vice party secretary, vice mayor, standing committee member
7	Division Head	Chu (处)	Head of departments and bureaus
8	Vice Division Head	Fu Chu (副处)	Vice Head of departments and bureaus
9	Section Head	Ke (科)	
10	Vice Section Head	Fu Ke (副科)	
11	Section Member	Ke Yuan (科员)	
12	Ordinary Staff		

Figure 3.3: Ranking system of civil servants



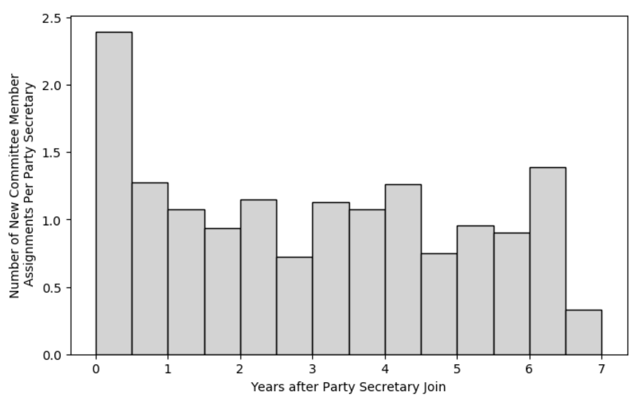
Note: The figure plots the full distribution of the number of committee member assignments each party secretary makes while in office. The average number of assignments is 6.2 and the median is 6.

Figure 3.4: Histogram of the number of connections



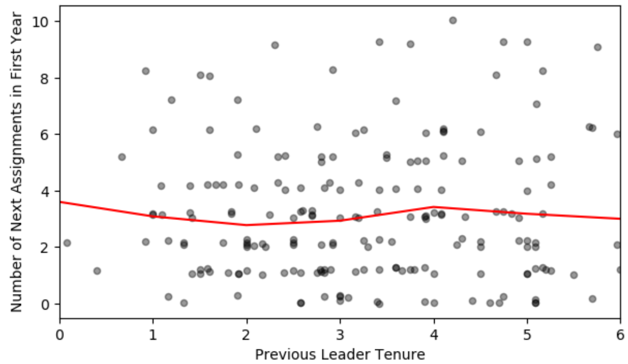
Note: The figure plots the percentage of party secretaries with varying tenure. The majority of the party secretaries (63.6%) has a tenure of less than 4 years, 28.2% has a standard tenure between 4 and 6 years, and 8.1% stays for more than 6 years. Left and right truncated party secretaries are excluded from the figure.

Figure 3.5: Histogram of party secretary's tenure



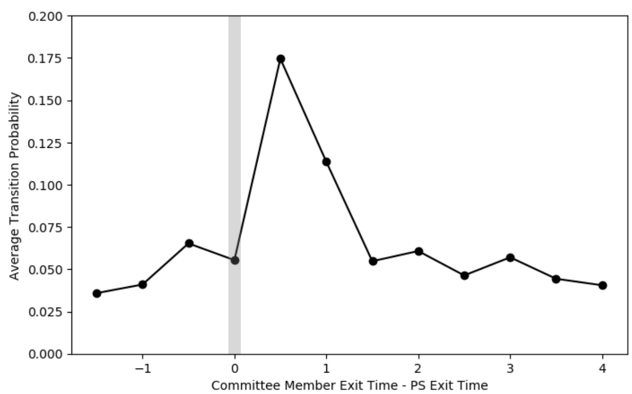
Note: The figure shows the number of new assignments for each half year after the party secretary's assignment. In terms of total count, 45.8% of assignments to the standing committee took place within the first year of the party secretary's assignment, and 65.0% took place within the first two years. Left truncated individuals are excluded from this analysis.

Figure 3.6: Committee member's assignment timing



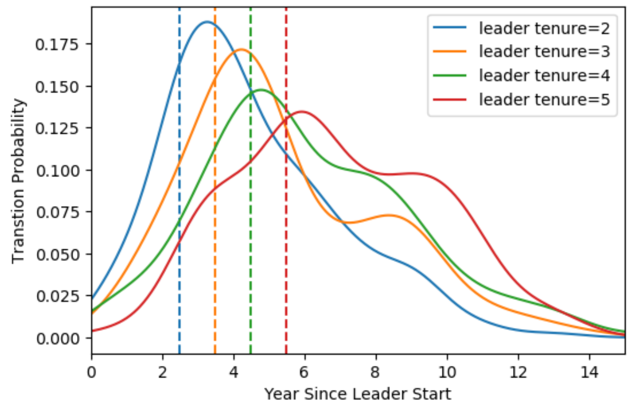
Note: The figure shows the correlation between the previous party secretary's tenure and the number of new committee member assignments within the first year of the current party secretary. There is significant variation in the number of new assignments, but previous leader tenure does not seem to be a determining factor.

Figure 3.7: Impact of previous party secretary's tenure on new assignments



Note: The figure shows the average exit rate of standing committee members relative to the exit timing the of the connected party secretary. The turnover is highest right after the party secretary leaves office: 17.5% of the standing committee exit within half a year and 28.8% exit within a year.

Figure 3.8: Committee member exit timing relative to the exit of the connected party secretary



Note: The figure shows the average exit rate of standing committee members grouped by the length of tenure of their connected leader. Regardless of the length of the party secretary's tenure, we observe the highest turnover rate after the connected leader leaves office.

Figure 3.9: Committee member exit rate by leader tenure

Bibliography

Doyon, Jérôme and Franziska Barbara Keller. 2020. "Knowing the wrong cadre? Networks and promotions in the Chinese party-state." *Political Studies* 68(4):1036–1053.

Evans, Peter and James E Rauch. 1999. "Bureaucracy and growth: A cross-national analysis of the effects of" Weberian" state structures on economic growth." *American Sociological Review* pp. 748–765.

Fisman, Raymond, Jing Shi, Yongxiang Wang and Weixing Wu. 2020. "Social ties and the selection of China's political elite." *American Economic Review* 110(6):1752–81.

Francois, Patrick, Francesco Trebbi and Kairong Xiao. 2016. Factions in nondemocracies: Theory and evidence from the Chinese Communist Party. Working Paper 22775 National Bureau of Economic Research.

Geddes, Barbara. 1994. *Politician's dilemma: building state capacity in Latin America*. Number 25 Univ of California Press.

Ilkhamov, Alisher. 2007. "Neopatrimonialism, interest groups and patronage networks: the impasses of the governance system in Uzbekistan." *Central Asian Survey* 26(1):65–84.

Jia, Ruixue, Masayuki Kudamatsu and David Seim. 2015. "Political selection in China: the complementary roles Of connections and performance." *Journal of the European Economic Association* 13(4):631–668.

Jiang, J. 2017. "Making bureaucracy work: patronage networks, performance incentives, and economic development in China." *American Journal of Political Science* 62:982–999.

Jiang, Junyan and Yu Zeng. 2020. "Countering capture: elite networks and government responsiveness in China's land market reform." *The Journal of Politics* 82(1):13–28.

Keller, Franziska. 2015. "Networks of power: a social network analysis of the Chinese Communist Elite, 1982-2012.".

Kou, Chien-wen and Wen-Hsuan Tsai. 2014. ""Sprinting with small steps" towards promotion: solutions for the age dilemma in the CCP cadre appointment system." *The China Journal* (71):153–171.

Landry, Pierre F., Xiaobo Lü and Haiyan Duan. 2018. "Does performance matter? Evaluating political selection along the Chinese administrative ladder." *Comparative Political Studies* 51(8):1074–1105.

Lazear, Edward P and Sherwin Rosen. 1979. Rank-Order tournaments as optimum labor contracts. Working Paper 401 National Bureau of Economic Research.

Lei, Zhenhuan. 2020. "Political patronage, infrastructure, and economic growth: evidence from China." *Comparative Political Economy: Comparative Capitalism eJournal* .

Li, Hongbin and Li-An Zhou. 2005. "Political turnover and economic performance: the incentive role of personnel control in China." *Journal of Public Economics* 89:1743–1762.

Meyer, David, Victor C. Shih and Jonghyuk Lee. 2016. "Factions of different stripes: gauging the recruitment logics of factions in the reform period." *Journal of East Asian Studies* 16(1):43–60.

Mueller, Hannes. 2009. "Patronage or meritocracy: Political institutions and bureaucratic efficiency." *Unpublished manuscript, Institut d'Analisi Economica, CSIC* .

Opper, Sonja, Victor Nee and Stefan Brehm. 2015. "Homophily in the career mobility of China's political elite." *Social Science Research* 54:332 – 352.

Pye, Lucian W. 1995. "Factions and the politics of Guanxi: paradoxes in Chinese administrative and political behaviour." *The China Journal* (34):35–53.

Shih, Victor, Christopher Adolph and Mingxing Liu. 2012. "Getting ahead in the Communist Party: explaining the advancement of central committee nembers in China." *American Political Science Review* 106(1):166–187.

Wong, Stan Hok-Wui and Yu Zeng. 2020. "Familiarity breeds growth: The impact of factional ties on local cadres' economic performance in China." *Economics & Politics* 32(1):143–171.

Xu, Chenggang. 2011. "The fundamental institutions of China's reforms and development." *Journal of Economic Literature* 49(4):1076–1151.

Zeng, Qingjie. 2016. "Democratic procedures in the CCP's cadre selection process: implementation and consequences." *The China Quarterly* 225:73–99.