Must-Access Prescription Drug Monitoring Programs and the Opioid Overdose Epidemic: The Unintended Consequences*

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Abstract

Although supply-side drug policies that limit access to legal opioids have reduced prescription opioid abuse, growing evidence shows that these policies have had the unintended consequence of increasing use of illegal opioids, including heroin. I add to this literature by studying the consequences of must-access prescription drug monitoring programs (PDMPs), which legally require providers to access a state-level database with a patient's prescription history before prescribing controlled substances under certain circumstances. Using a difference-in-differences specification, I find strong evidence that must-access PDMPs have increased heroin death rates. My estimates indicate that two years after implementation, must-access PDMPs were associated with 0.9 more heroin deaths per 100,000 in a half-year period, relative to control states. My results suggest that even if must-access PDMPs reduce prescription opioid deaths, the decrease is offset by a large increase in illegal opioid deaths.

Keywords: Prescription Drug Monitoring Program, Must-Access PDMP, Opioid, Heroin, Opioid Overdose, Opioid Epidemic

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1 Introduction

Opioid overdoses have reached epidemic levels in the United States. The Centers for Disease Control and Prevention (CDC) calls the opioid crisis "the worst drug epidemic in US history" (Kolodny et al. 2015). Between 1999 and 2017, overdose deaths from opioids, including prescription opioids and illegal opioids, increased six-fold. To contain the epidemic, federal and state governments implemented various policies that limit access to prescription opioids to reduce addiction, and several studies show that these drug control policies did reduce prescription opioid abuse (e.g., Buchmueller and Carey (2018); Cicero and Ellis (2015); Grecu et al. (2019)). Additionally, deaths from prescription opioids decreased by more than 6% between 2011 and 2013 and have remained relatively steady since then. These decreases, however, were limited to prescription opioids. During the same period, deaths from illegal opioids, such as heroin and illicitly made fentanyl, began to increase sharply, and between 2011 and 2016 they more than tripled.[1] As a result, overdose deaths from opioids, including both legal and illegal opioids, steadily increased between 1999 and 2017 and grew faster in the last few years, as shown in Figure 1. The worsening epidemic sparked a debate about the effectiveness of supply-side interventions: do policies that limit access to legal opioids cause users to transition from prescription opioids to illegal opioids?

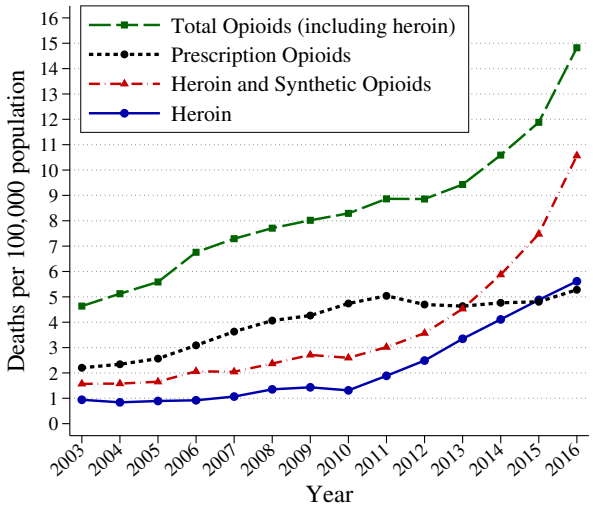
I study whether and to what extent a supply-side intervention can have a spillover effect on illegal opioid use, focusing on prescription drug monitoring programs (PDMPs), one of the most widely adopted statewide drug policies. A PDMP is a state-operated database of patient prescriptions for controlled substances. Authorized providers can access the PDMP database to identify the inappropriate use of pain medications. From the pre-1990s to 2016, all but one state implemented PDMPs (which I refer to as voluntary-access PDMPs). However, because provider access was voluntary, only a small percentage of providers actually enrolled in the program or requested patient histories (PDMP Center of Excellence 2014). From 2010 to 2012, the median PDMP registration rate of licensed prescribers who prescribed at least one controlled substance prescription was only 35%. (Kreiner et al. 2014).

In response to the low participation rates, 16 states implemented a must-access provision between 2007 and 2016, in addition to the existing voluntary-access PDMP. Must-access PDMPs legally require providers to use the PDMP before prescribing or dispensing under certain

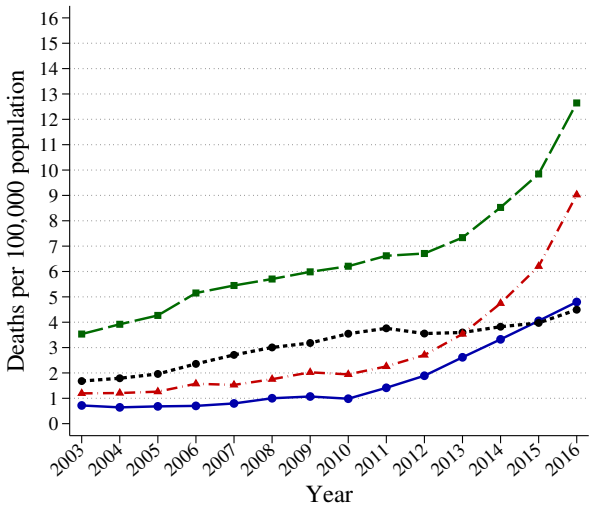
[1] Throughout this paper, when I use the term *opioid* to refer to all opioids, heroin is included.

Figure 1: National Trends in Ruhm-Corrected and Reported Death Rates

Panel A. Ruhm-corrected opioid death rates



Panel B. Raw reported opioid death rates



Notes: The figure plots the national trends in Ruhm-corrected and reported numbers of deaths per 100,000 population calculated using mortality data from the National Vital Statistics System (NVSS). Drug overdose deaths are coded using ICD-10 underlying cause of death codes: X40–X44, X60–X64, X85, and Y10–Y14. To identify drug involvement, the following four drug identification codes are used: heroin (T40.1), natural and semisynthetic opioids such as oxycodone and hydrocodone (T40.2), methadone (T40.3), and synthetic opioids excluding methadone, such as fentanyl (T40.4). I calculate total deaths from any opioid, including heroin, by combining T40.1–T40.4. Prescription opioid deaths are identified using T40.2. The deaths from heroin and synthetic opioids combine T40.1 and T40.4. Heroin deaths are identified using T40.1. Reported mortality rates are based on mentions of the specified drugs on the death certificates. Corrected mortality rates are estimated by using the method suggested by Ruhm (2018), which uses information from death certificates that specified at least one drug category to impute drug involvement for cases in which none was specified.

conditions. Kentucky's mandate on enrollment and PDMP use was associated with about an 8.5% lower overall dispensing of controlled substances in the first year following implementation, showing that mandates can effectively improve PDMP use (Substance Abuse 2017).

However, by making prescription opioids less accessible, must-access PDMPs may lead individuals to switch from prescription opioids to illegal opioids such as heroin or illegal fentanyl. For example, 94% of opioid-addicted individuals who switched from prescription opioids to heroin reported doing so because prescription opioids "were far more expensive and harder to obtain" (Cicero et al. 2014). Given the increased accessibility and reduced prices of heroin, the policy may have caused a significant transition from nonmedical use of prescription opioids to heroin use.

Using the National Vital Statistics System (NVSS) Multiple Cause of Death mortality files from 2003 to 2016, I exploit the variation in overdose deaths involving heroin or other types of opioids resulting from PDMP laws. The heroin mortality rate covers the entire population and

has been widely used by researchers studying heroin use (e.g., Alpert et al. (2018); Evans et al. (2019); Kilby (2015); Meinhofer (2018)). I also use administrative data from the Drug Enforcement Administration (DEA) to quantify how must-access PDMPs affected the legal supply of opioids and to account for the confounding effect of the OxyContin reformulation on heroin death rates. The OxyContin reformulation of 2010 was a nationwide drug policy aimed at reducing opioid abuse and is potentially a strong confounder.[2] Following the approach suggested by Alpert et al. (2018), I control for the differential effects of the reformulation across states by adding to my econometric model a measure of pre-reformulation OxyContin use interacted with the half-year fixed effects. I include this interaction in my preferred specification to address potential omitted variable bias that can arise from the pre-existing correlation between implementation of must-access PDMPs and exposure to the OxyContin reformulation.

I find strong evidence that must-access PDMPs increased heroin death rates and that voluntary-access PDMPs had no substantial effect. Using a difference-in-differences specification that allows the treatment effect to vary over time, I show that the heroin death rate began to increase in the year of policy implementation, and the size of effects steadily grew over time.[3] My estimates indicate that two years after implementation, must-access PDMPs were associated with 0.9 more heroin deaths per 100,000 in a half-year period, relative to control states. The largest detrimental effect of the policy occurred three years after implementation. I graphically present my difference-in-differences estimates and show that the trends in heroin mortality were not different across treatment and control states prior to implementation, providing evidence in support of the parallel trends identifying assumption.

Moreover, I find that the increase in heroin mortality coincided with a sudden decrease in prescription opioid mortality and a decrease in the legal supply of opioids following policy implementation, suggesting that the policies caused users to transition from prescription opioids to heroin. My estimates suggest that even if must-access PDMPs reduced prescription opioid deaths, the decrease was offset by an increase in deaths from illegal opioids, including heroin. Overall, I show that must-access PDMPs had no substantial effect on total opioid-related deaths

[2]Several studies have shown that the OxyContin reformulation caused a transition from nonmedical use of prescription opioids to heroin use, and the heroin death rate began to increase sharply following the reformulation (e.g., Alpert et al. (2018); Evans et al. (2019)).

[3]It may take time for both opioid abusers and physicians to adjust their behavior. Consumers may gradually switch to illegal drugs and providers may also take time to become familiar with the PDMP system and to adjust their prescribing behavior. Some studies emphasize that it is costly for providers to adjust their practice style (e.g., Clemens and Gottlieb (2014); Frank and Zeckhauser (2007)).

in the short term because of these offsetting effects.[4] In the longer term, however, the policies were associated with increased total opioid deaths because the large increase in illegal opioid deaths surpassed the decrease in prescription opioid deaths.

The findings of this paper add to the literature on the spillover effect of PDMPs on heroin use. Most of the previous papers in this literature have focused on the period before 2014 and have found weak or no effects of PDMPs on heroin-related outcomes. Earlier studies could not distinguish between voluntary- and must-access PDMPs (e.g., Radakrishnan (2015); Nam et al. (2017)), and some prior work has used survey or treatment admissions data, which are likely to underreport heroin use, and find no effects (e.g., Radakrishnan (2015); Ali et al. (2017); Grecu et al. (2019)).[5] Kilby (2015) finds only a temporary effect of voluntary-access PDMPs on heroin mortality. Using data through 2013, Meinhofer (2018) finds suggestive evidence that must-access PDMPs increased heroin-related overdose deaths, although these findings are sensitive to the model specification.[6] My paper contributes to this literature by providing robust, causal estimates of the medium-term effect of must-access PDMPs on heroin-related mortality. Two key innovations allow for this contribution. First, I utilize data through 2016, allowing for the inclusion of several additional must-access PDMP implementations and a longer post-treatment period. My findings suggest that estimating the longer-run impact is crucial to identifying the effect on heroin-related deaths.[7] Second, I demonstrate that the results are robust to controlling for other co-occurring state and national opioid-related policies. By employing more recent data than prior work, I am able to include several more reforms in the analysis that allow me to flexibly account for potential confounding effects of state and national opioid-related policies.

This paper also contributes to the literature on the unintended consequences of supply-side drug policies. Evans et al. (2019) and Alpert et al. (2018) investigated the consequences of the 2010 OxyContin reformulation and found strong evidence of the movement from legal opioids to heroin. My findings add to this literature and suggest that a supply-side intervention that

[4]Total opioid-related deaths indicate the deaths that involved any opioid, including both prescription opioids and illegal opioids, at the time of death.

[5]Ali et al. (2017) show that heroin use, dependence, and initiation have no statistically significant association with either voluntary-access or must-access PDMPs but find a statistically significant association between voluntary-access PDMPs and the increased number of days of heroin use.

[6]My paper is different from Meinhofer (2018) on two dimensions—data period and research design. Both studies use the same mortality data, but I use data through 2016 and Meinhofer (2018) uses data through 2013. Also, the outcome variables and model specifications are different in the two papers: I use death rates as the outcome and control for other co-occurring opioid-related policies, while Meinhofer (2018) uses the log of deaths as the outcome and includes the log of population and state-specific time trends in her econometric model. In Appendix Section A, I discuss in detail the differences between the two studies.

[7]In Appendix Section A, I address in detail the consequences of using additional data.

controls access to legal opioids can have the unintended consequence of increased illegal drug use.

The results of this study have clear policy implications: the existence of accessible and affordable close substitutes may reduce the effectiveness of supply-side drug policies. A supply-side intervention can control only the legal supply of opioids but not the demand for opioids. Demand-side interventions, such as improving access to treatment or prevention may be more effective in preventing and mitigating opioid abuse and should be aligned with the existing supply-side policies.

2 Background

2.1 Opioid Abuse

Opioids are a class of drugs that relieve severe pain. Opioids include the illegal drug heroin, synthetic opioids such as fentanyl, and prescription medications, such as oxycodone, hydrocodone, and morphine. If used medically, prescription opioids help relieve pain. However, continued use or abuse of opioids can lead to addiction, tolerance, and physiological dependence.

2.2 Prescription Drug Monitoring Programs

Prescription drug monitoring programs, or PDMPs, are state-level databases that collect information on patients' opioid prescriptions at the point of dispensing (Davis et al. 2014). In most states, authorized providers may access the state's database to identify inappropriate use of pain medications. The earliest programs were based on carbon copies and, thus, did not have the capabilities of the modern electronic system, and only program staff, authorized law enforcement, and regulatory agencies could access the data (OIG 1992). Many early electronic PDMPs required that data be sent only infrequently using methods that are now outdated (Horwitz et al. 2018). PDMPs now involve automated reporting, transitioned to a modern electronic system with increased reporting frequency (Buchmueller and Carey 2018). The types of users permitted to access PDMP data have also been significantly increased: the proportion of state PDMPs that allow physicians to directly access patient-identifiable data increased from 23.1% in 1998 to 93.5% in 2011 (Davis et al. 2014).

However, even if more timely and complete patient prescription history data are available and accessible, provider participation rates are low when PDMP is not mandated (Haffajee et al.

2015).[8] In response to the low participation rates, 16 states implemented a must-access provision on top of the existing voluntary-access PDMPs between 2007 and 2016. Must-access PDMPs legally require providers to use the PDMP before actual prescribing or dispensing under certain conditions. Must-access provisions have successfully increased provider utilization. In Kentucky, Tennessee, New York, and Ohio, must-access provisions increased providers' registration and utilization of PDMPs and decreased the prescription of certain drugs (PDMP Center of Excellence 2016).

Table 1 shows the start dates of the laws investigated in this paper. Horwitz et al. (2018) serves as the source of information for the month and year that states first enacted any type of PDMP.[9] By the end of 2016, all states except Missouri had passed some type of PDMP laws. Effective dates of must-access PDMPs, obtained from Mallatt (2019), are listed in the second column of Table 1.[10,11] I use these dates for my main analysis and test the robustness of the results using alternative dates of must-access PDMPs taken from Sacks et al. (2019), which are listed in the third column of Table 1. In the last column of Table 1, I report dates for pill mill laws suggested by Mallatt (2018).[12]

In my main analysis, I do not take into account differences among must-access laws, although the strength of these laws varies greatly across states. Delaware's PDMP requires provider access only with reasonable suspicion of abuse, as did the initial PDMP of Ohio (until 2015). Prior to 2015, Oklahoma's initial law applied only to methadone, and Vermont's initial PDMP required access only the provider wrote a replacement prescription for one that had been lost or stolen. In contrast, Kentucky, Massachusetts, New Mexico, New York, Tennessee, West Virginia, and the recent laws in Ohio, Oklahoma, and Vermont applied must-access laws to all care settings and

[8] As noted above, the median PDMP registration rate, defined as the proportion of prescribers registered to use the PDMP among licensed prescribers who issued one or more controlled substance prescription between 2010 and 2012, was 35% (Kreiner et al. 2014).

[9] In Appendix Table A2, I report heroin mortality results based on alternative enactment dates from the Prescription Drug Abuse Policy System (PDAPS) and the National Alliance for Model State Drug Laws (NAMSDL), which were the most commonly used in previous papers. In the last column of Appendix Table A2, I also report the results using the dates PDMP data became accessible to any authorized user, suggested by Horwitz et al. (2018), instead of enactment dates.

[10] See Mallatt (2019) and https://sites.google.com/site/justinemallatt/home/pdmp-dates for more detailed information.

[11] South Carolina enacted a must-access law in April 2016, but the law applied only to Medicaid and state health plans. Following Mallatt (2019), I code South Carolina as a voluntary-access PDMP, but I obtain similar results when I code it as having a must-access PDMP in 2016.

[12] Although not reported in the paper, my results are robust to using several alternative start dates of pill mill laws (e.g., PDAPS, Mallatt (2019)). Given the robustness of my results, I follow Mallatt (2018) because it provides dates of pill mill laws that are more comparable to those from Buchmueller and Carey (2018). In this paper, I do not use the policy dates from Buchmueller and Carey (2018) because their sample period is shorter than mine.

Table 1: State Laws

State	Any PDMP	Must-Access PDMP		Pill Mill Laws
		Start Date	Alternative	
Alabama	2005m11		2013m5	
Alaska	2008m9			
Arizona	2007m9			
Arkansas	2013m3			
California	Pre-1990			
Colorado	2005m6			
Connecticut	2006m10	2015m10	2015m10	
Delaware	2011m9	2012m3	2012m3	
District of Columbia	2014m2			
Florida	2010m12			2011m7
Georgia	2011m7		2014m7	
Hawaii	Pre-1990			
Idaho	Pre-1990			
Illinois	Pre-1990			
Indiana	Pre-1990		2014m7	
Iowa	2006m5			
Kansas	2008m7			
Kentucky	1998m7	2012m7	2012m7	2011m7
Louisiana	2006m7	2014m8	2008m1	2005m7
Maine	2004m1			
Maryland	2011m10			
Massachusetts	1992m12	2013m6	2014m7	
Michigan	Pre-1990			
Minnesota	2009m1			
Mississippi	2006m6			2011m9
Missouri				
Montana	2011m7			
Nebraska	2011m8			
Nevada	1996m1	2007m10	2007m10	
New Hampshire	2012m6		2016m1	
New Jersey	2009m8	2015m7	2015m11	
New Mexico	2004m7	2012m10	2012m9	
New York	Pre-1990	2013m9	2013m8	
North Carolina	2006m1			
North Dakota	2006m12			
Ohio	2005m5	2011m11	2012m3	2011m5
Oklahoma	1991m1	2010m11	2011m3	
Oregon	2009m7			
Pennsylvania	Pre-1990			
Rhode Island	Pre-1990	2014m7	2016m6	
South Carolina	2006m6			
South Dakota	2010m3			
Tennessee	2003m1	2013m1	2013m7	2012m1
Texas	Pre-1990			2009m6
Utah	1995m7			
Vermont	2008m6	2013m11	2015m5	
Virginia	2003m9	2015m7	2015m7	
Washington	2011m8			
West Virginia	1995m6	2012m6	2012m6	2014m9
Wisconsin	2010m6			
Wyoming	2003m7			

Notes: The table reports the start dates of state laws enacted until December 31, 2016. Each column reports the dates obtained from a separate source.

ingredients, and required providers to access the PDMP even without suspicion of abuse. In Section 5.6, I identify heterogeneity in the effects of a must-access PDMP by the strength of the law.

2.3 Substitution of Heroin

An important feature of the opioid addiction epidemic is the relationship between prescription opioids use and heroin use (Kolodny et al. 2015). Heroin is a highly addictive illegal drug made from morphine and is pharmacologically similar to prescription opioids. Prior nonmedical use of prescription opioids may lead to heroin use (Becker et al. 2008; Muhuri et al. 2013). According to the federal government's National Survey on Drug Use and Health (NSDUH), 79.5% of individuals who use heroin for the first time report previous nonmedical use of prescription opioids (Muhuri et al. 2013).[13] Using national-level data, Becker et al. (2008) showed that heroin users are 3.9 times as likely to report nonmedical use of opioids in the previous year. These studies provide a clear link between prescription opioid abuse and heroin use and suggest that heroin is a close substitute for prescription opioids.

How do must-access laws cause a transition from nonmedical use of prescription opioids to heroin use? Must-access PDMPs directly affect the legal supply of opioids by limiting access to controlled substances. By making prescription opioids less accessible, the policy may also reduce prescription opioid abuse. Several studies have found that must-access PDMPs did reduce prescription opioid abuse (e.g., Birk and Waddell (2017); Buchmueller and Carey (2018); Grecu et al. (2019)). Appendix Figure A1 suggests that the national trend in the legal supply of opioids is highly correlated with that in prescription opioid deaths.

However, as prescription opioids become less accessible because of the must-access law, individuals may substitute heroin for prescription opioids.[14] The magnitude of the actual substitution is determined by individual characteristics as well as accessibility to substitutes (Alpert et al. 2018). Given the increased accessibility, reduced prices, and the higher purity of heroin, must-access PDMPs may cause a transition from nonmedical use of prescription opioids to heroin use. Moreover, most heroin is now laced with illegal fentanyl, a synthetic opioid that is 50–100 times stronger than morphine, to improve its potency. Therefore, must-access policies

[13] Muhuri et al. (2013) report that the incidence of heroin use among people who reported previous nonmedical use of prescription opioids was 19 times as high as the incidence among individuals who reported no previous abuse.

[14] There is a large black market for opioids, an illegal trading system that avoids government regulation, on which individuals can buy close substitutes. This secondary market provides not only illegal opioids but also legal opioids. Highly regulated opioids, such as oxycodone, have fueled the black market for prescription opioids.

may cause even worse outcomes, by pushing people to more dangerous illegal opioids.

3 Data

3.1 Mortality

I use the National Vital Statistics System (NVSS) Multiple Cause of Death mortality files to study annual overdose deaths from 2003 to 2016. Following the coding suggested by the CDC, I categorize deaths related to opioids. First, I code drug overdose deaths using the ICD-10 underlying cause-of-death codes of unintentional (X40–X44), suicide (X60–X64), homicide (X85), and undetermined intent (Y10–Y14). Second, I use ICD-10 drug identification codes, which contain information about the drugs found in the body at death. The following four drug identification codes are used: T40.1 for heroin, T40.2 for natural and semisynthetic opioids such as oxycodone and hydrocodone, T40.3 for methadone, and T40.4 for synthetic opioids excluding methadone, such as fentanyl.

In this paper, I investigate four categories of drug overdose deaths: (i) deaths from heroin (T40.1), (ii) deaths from heroin or synthetic opioids other than methadone (T40.1, T40.4), (iii) deaths from natural and semisynthetic opioids (T40.2), and (iv) total deaths from any opioid, including heroin (T40.1–T40.4). Since heroin is an opioid, when I use the term *opioid* to refer to all opioids, heroin is included. A single overdose death often involves the presence of multiple drugs at the time of death.[15] The mortality outcomes I investigate in this paper are total mortality unless otherwise noted; total mortality may involve other drugs present at the time of death.[16] Although this approach does not allow for the death to be attributed to a single drug when multiple drugs are related to the death, increases or decreases in the involvement of a specific drug reflect substitution patterns (Alpert et al. 2018). To identify clear substitution patterns, I also investigate mortality outcomes based on the exclusive involvement of a specific opioid. For example, the exclusive measure of heroin mortality, which I refer to as heroin-only mortality, indicates the deaths that involved heroin (T40.1) but not the other types of opioids (T40.2–T40.4) at the time of death.

To measure illegal opioid deaths, I use the second category of overdose death, which

[15] Therefore, a single death might be included in more than one category when calculating the number of overdose deaths involving specific drugs.

[16] For example, total heroin deaths include the deaths that involved not only heroin but also other types of opioids at the time of death.

combines heroin and synthetic opioids other than methadone (hereinafter referred to as synthetic opioids).[17] While legal uses of fentanyl do exist, increases in synthetic opioid deaths since 2013 have been driven primarily by illicit fentanyl use (Rudd et al. 2016). I combine T40.1–T40.4 to measure total deaths from any opioid, including heroin, which I refer to as total opioid-related deaths. To measure deaths from prescription opioids, I use the third category of overdose deaths, which involves natural and semisynthetic opioids (hereinafter referred to as natural opioids). Earlier studies followed the CDC's traditional method of calculating prescription opioid deaths, which combined T40.2–T40.4. However, due to the recent surge in deaths that may involve illicit fentanyl, the CDC began analyzing synthetic opioids other than methadone (T40.4) separately from T40.2–T40.4.[18] I follow this more conservative method in this paper and also exclude methadone (T40.3), which had abnormal overdose trends, from my measure of prescription opioid deaths.[19] However, including methadone does not change my results pattern.

A major limitation of most prior estimates of opioid mortality rates is that they underreport actual rates because the specific drugs that caused the death are frequently not identified on the death certificates (Ruhm 2018). To obtain more accurate estimates of opioid mortality, I use corrected estimates of mortality rates following the method suggested by Ruhm (2018), which uses information from death certificates that specify at least one drug category to impute drug involvement for cases in which only unspecified drugs were mentioned on the death certificates.[20] Figure 1 shows that corrected rates are 20–35% higher every year than reported rates for any type of opioid. Throughout the paper, I use the corrected mortality rates as the outcome variables.

3.2 Legal Supply of Opioids

I use administrative data on shipments of prescription opioids from the Drug Enforcement Administration (DEA)'s Automation of Reports and Consolidated Orders System (ARCOS) to examine whether must-access PDMPs reduce the legal supply of opioids. ARCOS is a federal data system initiated in response to the 1970 Controlled Substances Act that tracks the

[17] Throughout this paper, I use the phrase *illegal opioid deaths* synonymously with *heroin and synthetic opioid deaths*.

[18] Data for synthetic opioid deaths involve both legal and illegal synthetic opioids because toxicology testing cannot distinguish between legal and illegal synthetic opioids.

[19] During 2002–2006, the methadone overdose death rate increased, on average, 22.1% per year; however, beginning with 2006 warnings from the Food and Drug Administration (FDA), efforts to reduce the use of methadone for pain have been made; as a result, after 2006, methadone overdose deaths declined 6.5% per year; see Jones et al. (2016) for a detailed description of trends in methadone overdose deaths.

[20] See Ruhm (2018) for more details on the method of computing the corrected mortality rates.

transactions and deliveries of controlled substances from manufacturers to retail distributors at the state level. ARCOS contains data on all Schedule I and II substances, as well as on narcotic substances in Schedule III that are sold or distributed. I use ARCOS data to examine whether must-access PDMPs reduce the total distribution of opioids by state, focusing on oxycodone and hydrocodone.[21] Oxycodone and hydrocodone, which are both semisynthetic opioids widely prescribed to treat pain, are some of the most commonly abused prescription opioids. The national trends in the legal supply of oxycodone and hydrocodone are presented in Panel A of Appendix Figure A1.

3.3 Exposure to the OxyContin reformulation

To address potential omitted variable bias that can arise from the pre-existing correlation between implementation of must-access PDMPs and exposure to the 2010 OxyContin reformulation, I account for the reformulation in my econometric model. I proxy differential exposure to the reformulation across states with a measure of pre-reformulation OxyContin use. Following Alpert et al. (2018), I consider two alternative measures of OxyContin use. First, using ARCOS 2004–2009 data on the legal supply of opioids, I define OxyContin use as the relative importance of oxycodone compared to that of hydrocodone (oxycodone / [oxycodone + hydrocodone]) in per capita (morphine equivalent) doses.[22] Hydrocodone is considered a substitute for oxycodone, and states that disproportionately consume oxycodone relative to hydrocodone are expected to be more affected by the reformulation (Alpert et al. 2018). For each state, I calculate the population-weighted average of the relative importance of oxycodone compared to that of hydrocodone, combining the 2004h1 through 2009h2 periods.

Second, I use data from the National Survey on Drug Use and Health (NSDUH), which is a nationally representative household survey. The advantage of this survey is that it includes information on nonmedical use of OxyContin, although this data was self-reported. In the NSDUH data, OxyContin misuse rate is defined as the percentage of the population over the age of 12 indicating nonmedical use of OxyContin. Using the NSDUH data, Alpert et al. (2018) define each state's pre-reformulation OxyContin misuse as the population-weighted rate of

[21] I convert oxycodone and hydrocodone in grams, reported in the ARCOS data, into those in morphine milligram equivalents (MMEs) using the standard MME conversion factors (https://www.cms.gov/Medicare/Prescription-Drug-Coverage/PrescriptionDrugCovContra/Downloads/Oral-MME-CFs-vFeb-2018.pdf (last accessed May 2020)) and then dividing by 60 (to convert MMEs to doses).

[22] The ARCOS data provide information on the total supply of oxycodone but not the OxyContin supply specifically. However, OxyContin accounted for a large fraction of oxycodone supply in the pre-reformulation period: for example, in 2002, OxyContin accounted for 68% of oxycodone distribution (Paulozzi 2006).

OxyContin misuse that combines the 2004–2005, 2006–2007, and 2008–2009 waves. I use the same measure, which I refer to as the NSDUH measure hereinafter.

Two alternative measures of OxyContin use, the ARCOS measure and the NSDUH measure, are strongly correlated. Alpert et al. (2018) show that states with high shares of oxycodone relative to hydrocodone distribution had higher rates of OxyContin misuse and that their estimation results are similar regardless of whether they use the ARCOS measure or the NSDUH measure. In this study, I control for exposure to the reformulation using the ARCOS measure in my main specifications, although I also present the estimates when I instead use the NSDUH measure to account for exposure to the reformulation.[23]

3.4 Descriptive Statistics

Table 2 provides basic statistics on outcome variables and the state-half-year level covariates for each group before policy implementation. The analysis of this study focuses on the ten states that passed must-access laws between 2010h1 and 2013h2. I present summary statistics for these ten states and for 34 states that had no must-access PDMPs until 2016h2.[24] I treat states with no PDMP and states with a voluntary-access PDMP, but not those with a must-access PDMP, as a single control group because I find no substantial effects of voluntary-access PDMPs on any of the outcome variables I investigate in this paper. Between 2003h1–2009h2, ten treatment states have more oxycodone doses per capita and a higher rate of prescription opioid death. However, the heroin death rate is lower in the treated states, and there are no substantial differences in other outcomes across the two groups. The age composition and average unemployment rates are similar between the groups. However, the ten treatment states reflect a higher share of whites and a smaller share of Hispanics.

4 Empirical Strategy

I examine the causal effects of must-access PDMPs on heroin and opioid-related mortality rates by exploiting variation in the start date of the policy. As my main econometric model, I use a

[23] The major limitation of using ARCOS data to measure the differential effects of the reformulation is that this data source provides information on total oxycodone use through legal channels but does not capture nonmedical use specifically. However, the advantage of using this administrative data is that the ARCOS system is not associated with issues of underreporting, which is one of the main limitations of NSDUH survey data. Given the robustness of my results across the ARCOS and NSDUH measures, I consider the former as my preferred measure.

[24] Although 35 states did not implement a must-access PDMP until 2016h2, I exclude Florida from my control group in all analyses. Florida is an outlier that experienced both a sharp increase and a decrease in oxycodone supply within a decade. See Appendix Section B for more details.

Table 2: Summary Statistics, 2003h1–2009h2

Outcome (mean, 2003h1–2009h2)	All 44 states	States with must-access PDMPs (10 states)	States having no must-access PDMPs	Source
Per capita legal supply of opioids (morphine-equivalent doses)				
Oxycodone	1.474	1.862	1.362	ARCOS
	(0.767)	(0.758)	(0.733)	
Hydrocodone	0.849	0.892	0.837	ARCOS
	(0.427)	(0.662)	(0.331)	
Ruhm-corrected overdose deaths per 100,000				
Heroin	0.509	0.466	0.521	Vital Statistics
	(0.324)	(0.36)	(0.312)	
Heroin and synthetic opioids	0.979	1.012	0.97	Vital Statistics
	(0.471)	(0.583)	(0.434)	
Prescription opioids	1.545	1.708	1.498	Vital Statistics
	(0.806)	(1.215)	(0.636)	
Total opioids (including heroin)	3.131	3.411	3.05	Vital Statistics
	(1.353)	(1.925)	(1.126)	
Measures of pre-reformulation OxyContin use				
Oxycodone / (oxycodone + hydrocodone)	0.61	0.69	0.59	ARCOS, 2004h1–2009h2
	(0.16)	(0.12)	(0.16)	
OxyContin misuse rate (%)	0.55	0.71	0.5	NSDUH, 2004–2009
	(0.23)	(0.19)	(0.22)	
Population (%)				
0–14	0.21	0.19	0.21	Census
15–24	0.14	0.14	0.15	Census
25–44	0.28	0.28	0.28	Census
45–64	0.25	0.26	0.25	Census
65–84	0.11	0.11	0.1	Census
85+	0.02	0.02	0.02	Census
Race/ethnicity (%)				
Non-Hispanic white	0.67	0.73	0.65	Census
Non-Hispanic black	0.12	0.11	0.12	Census
Hispanic	0.15	0.1	0.16	Census
Other race	0.07	0.06	0.07	Census
Unemployment rate (%)	5.99	5.88	6.02	BLS
Observations	616	140	476	
Number of states	44	10	34	

Notes: Each column describes the balanced panel of state-half-year from 2003h1 to 2009h2. Observations are weighted by state population, and standard deviations are in parentheses. The first column includes all the 44 states included in the analysis sample. The second column includes the ten treated states that implemented must-access PDMPs from 2010h2 to 2013h2. The last column includes the 34 control states that did not implement must-access policies until 2016h2. Florida is excluded from the control sample (see Appendix Section B). I combine states having no PDMP and states having a voluntary-access PDMP but not a must-access PDMP into a single control group.

difference-in-difference specification that allows the treatment effect to vary over time, often called the event study specification. This model sets each state's first post-period to period zero and compares the outcomes between the treatment and control states in every pre- and post-period, relative to the last pre-period. The main specification is as follows:

$$Y_{st} = \alpha_s + \alpha_t + \sum_{k \neq -1} \beta_k \mathbf{1}(Policy_{sk}) + X_{st}\delta + oxy_s \cdot \omega_t + \varepsilon_{st} \tag{1}$$

where Y_{st} is the number of opioid deaths per 100,000 in a given state s over half-year t. $\mathbf{1}(Policy_{sk})$ is 1 if a given state enacted a must-access PDMP k periods ago, and $k \geq 0$ denotes a post-period. A negative k denotes a pre-period, indicating -k periods prior to implementation. I control for state fixed effects α_s to account for time-invariant state-specific characteristics and time (half-year) fixed effects α_s to account for time-varying national shocks and trends in opioid availability, heroin prices, and other common factors across states. X_{st} is a vector of state- and time-varying control variables, which includes the log unemployment rate, share of the population in six age groups (0–14, 15–24, 25–44, 45–64, 65–84, 85+), share non-Hispanic black, share Hispanic, and share other race.[25] I also include a time-varying indicator for whether the state had a pill mill law.

oxy_s denotes the measure of OxyContin use in state s in the pre-reformulation period (2004h1–2009h2). I use the ARCOS measure to account for pre-reformulation OxyContin use, although I also present the results obtained using the NSDUH measure (see Section 3 for a description of these measures). I interact this time-invariant and state-specific OxyContin use variable with the half-year fixed effects, ω_t, to account for the differential effects of the 2010 OxyContin reformulation across states. ϵ_{st} is the error term. Standard errors are clustered at the state level.

β_k are the parameters of interest, which summarize the treatment effect of must-access PDMPs on the outcomes, k periods after implementation. The last pre-period ($k = -1$) is used as the reference period. To estimate the medium-run effects of the policies, I consider an event time window that runs from 15 half-years prior to implementation to six half-years post-implementation. My analyses focus on the ten treated states that were consistently observed during this time window.[26] Because states implemented must-access PDMPs with different timing, some of my treated states are not observed in distant relative periods. To make my treated sample balanced in relative periods, I trim all periods outside the -15/+6 window.[27]

The key identifying assumption is that absent must-access PDMPs opioid death rates would have continued to trend in parallel across treatment and control states. To visually assess whether

[25] I obtain total population, population by age group, and population by race from the census. I assume that population estimates are recorded in the second half of the year and use linear interpolation to estimate population levels for the first half of the year.

[26] In Appendix Section E, I address the consequences of this choice in detail by illustrating that the estimated short-run effect is similar if I consider a shorter time window to add more treated states in the analysis. Given this similarity, I prefer the time window that allows me to look at the longer-term effects.

[27] Sun and Abraham (2020) explain that researchers bin or trim distant relative periods to accommodate unbalanced relative periods, but they provide no theoretical advantage to either approach. If I bin distant periods, I find that the policy has a larger effect on heroin mortality in the last post-period, which is primarily driven by one treated state.

there are systematic differences in trends in the outcome between groups prior to policy implementation, I plot the β_k coefficients from the baseline specification (equation 1). For interpretation, I report β_2, β_4, and β_6, which indicate the one-year effect, two-year effect, and three-year effect, respectively.

5 Results

This section comprises three parts. First, I provide evidence that the per capita legal supply of opioids declined following the states' implementation of must-access PDMPs. Second, I show that must-access PDMPs were associated with increased heroin mortality and that the size of effects grew over time. I also present the negative effects of must-access laws on deaths from prescription opioids and discuss the net effects on total opioid-related mortality. Third, I identify heterogeneity in the effects of must-access PDMPs on my outcomes generated by different strengths of must-access laws.

5.1 Effects of Must-Access PDMPs on the Legal Supply of Opioids

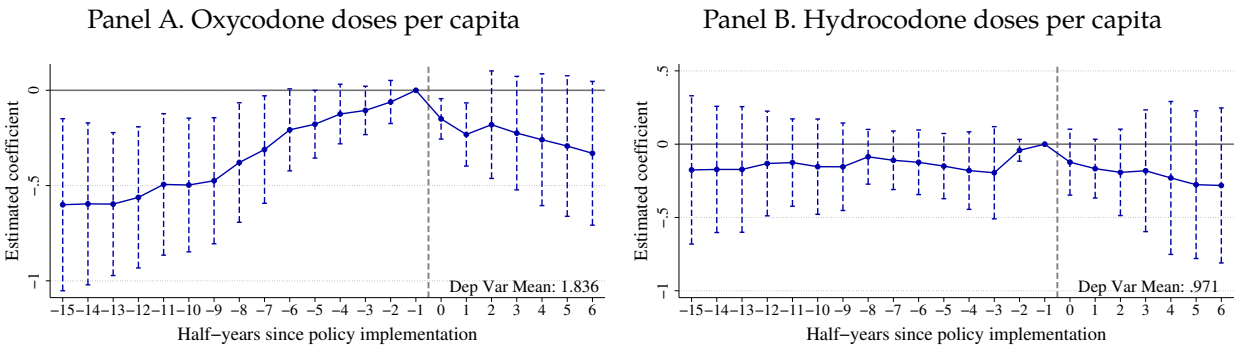
First, I investigate the effect of must-access PDMPs on the legal supply of opioids. Figure 2 plots the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1). The dependent variable is the per capita legal supply of oxycodone (in morphine equivalent doses) in Panel A and the per capita legal supply of hydrocodone (in morphine equivalent doses) in Panel B.

Figure 2 suggests a negative association between must-access PDMPs and the per capita legal supply of opioids. Panel A of Figure 2 shows that there was a negative trend break in the per capita legal supply of oxycodone in the first post-period, although there was an upward trend in the entire pre-period. As shown in Panel B, I also find suggestive evidence of a negative association between must-access PDMPs and hydrocodone supply, although the coefficients are statistically insignificant.

5.2 Effects of Must-Access PDMPs on Heroin Mortality

Trend Break Estimate In this study, I use a difference-in-difference specification that allows the treatment effect to vary over time (see equation 1). However, one concern with this specification is how I test and summarize the estimated effects. Unlike a typical

Figure 2: Effects of Must-Access PDMPs on the Legal Supply of Opioids



Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1). The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Observations are weighted by state population. Outcome variables are the per capita legal supply of oxycodone (Panel A) and hydrocodone (Panel B) in morphine equivalent doses. In all panels, the treated states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the treated sample is balanced in relative periods from -15 to +6. The distant relative periods outside the -15/+6 event time window are trimmed. The control states are the 34 that did not implement must-access policies until 2016h2, and the control sample is balanced from 2003h1 to 2016h2. Florida is excluded from the control sample in all panels (see Appendix Section B). In all panels, I control for fixed effects for state and half-year, an indicator for whether a state had a pill mill law, the ARCOS measure of pre-reformulation OxyContin use interacted with the half-year fixed effects, and the following time-varying covariates: the log unemployment rate, share of the population in six age groups (0–14, 15–24, 25–44, 45–64, 65–84, 85+), share non-Hispanic black, share Hispanic, and share other race.

difference-in-differences specification that treats the entire pre-period as the reference period, my baseline specification compares outcomes between the treated and control groups in each post-period, relative to the last pre-period. Therefore, the estimated effects can be sensitive to what happened in the last pre-period. To address this, I report trend break estimates, following Finkelstein (2007), in addition to the original coefficients. My trend break estimates provide the results for testing the n-period change in β_k after the reference period relative to the n-period change in β_k prior to the reference period. For example, the two-year effect is calculated using the following equation:

$$\Delta 5 = (\beta_4 - \beta_{-1}) - (\beta_{-1} - \beta_{-6}) = \beta_4 + \beta_{-6} \tag{2}$$

where β_{-1} equals to zero because the last pre-period is used as the reference. $\Delta 5$ summarizes the five-half-year change in the outcome after the reference period relative to the five-half-year change prior to the reference period for the treated states, relative to the control states. In Tables 3–5, I report the one-year, two-year, and three-year outcome changes, respectively ($\Delta 3, \Delta 5$, and $\Delta 7$). My interpretation of the results relies more heavily on trend break estimates (Δn) than on the original regression coefficients (β_k). However, when two statistics are qualitatively similar, I

consider the original regression coefficients as my preferred statistics because they allow for a direct comparison of estimates across figures and tables.

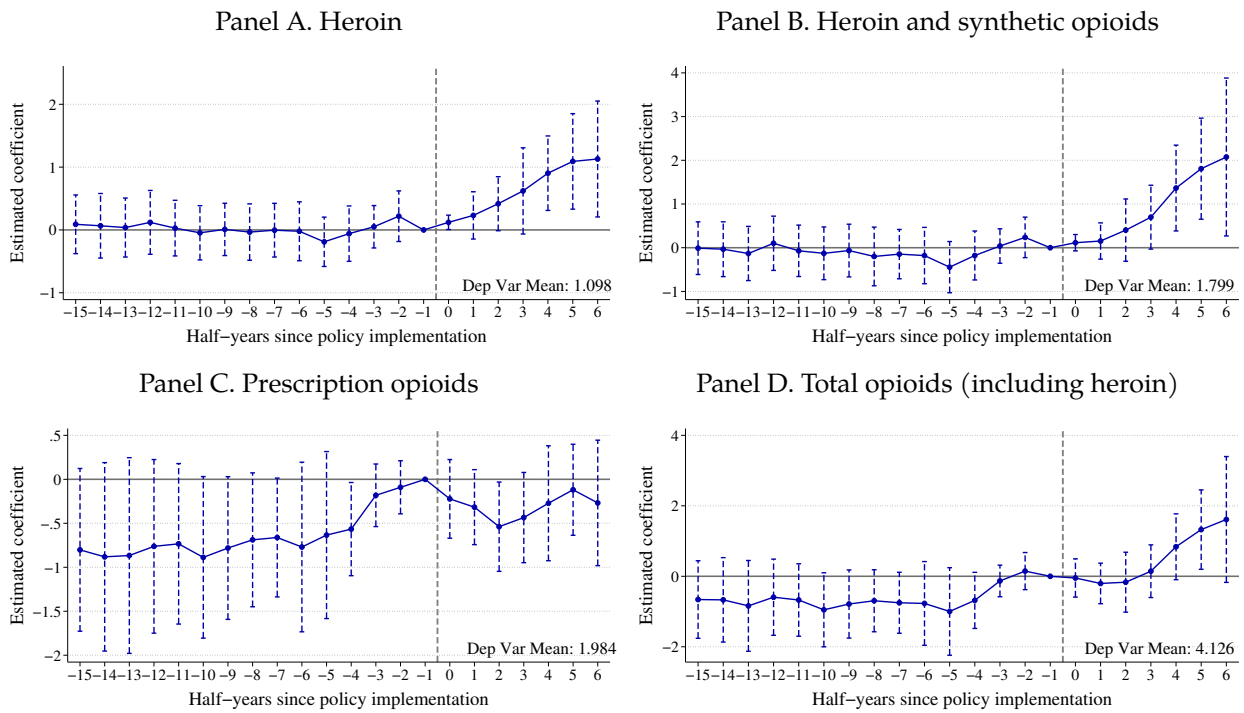
Baseline Heroin Estimates In Figure 3, Panel A presents the baseline heroin estimates. Panel A shows the effects of must-access PDMPs on heroin mortality by plotting the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) with total heroin deaths per 100,000 as the dependent variable. The panel shows the point estimates and 95 percent confidence intervals. The corresponding regression coefficients and trend break estimates are presented in columns 4 and 5 of Table 3 Panel A, respectively. In Table 3, I report the one-year, two-year, and three-year effects of the policy. Standard errors in all specifications are clustered at the state level.

Figure 3 Panel A provides strong evidence that must-access PDMPs were associated with an increased heroin death rate. Panel A shows that the trends in heroin mortality in the pre-period did not differ across the treatment and control states, providing evidence in support of the parallel trends assumption. In the first post-period, however, there was a sudden increase in heroin mortality in states with must-access PDMPs, and the size of effects steadily grew over time. The coefficients are positive and statistically significant in every post-period, although the coefficient for event time +1 is statistically insignificant. Moreover, the sudden increase in heroin mortality coincided with a sudden decline in prescription opioid mortality following implementation (see Figure 3 Panel C), providing evidence of the substitution of heroin for prescription opioids.[28]

In Table 3 Panel A, the baseline estimates for heroin mortality are reported in column 4, and the corresponding trend break estimates are reported in column 5. Because my baseline estimates reveal no pre-trend in heroin mortality (see Figure 3 Panel A), the original regression coefficients (β_k) are qualitatively similar to the corresponding trend break estimates (Δn). Given this similarity, I consider the original baseline estimates as my preferred heroin estimates. Column 4 indicates that a year after implementation, heroin mortality in a half-year period increased by 0.42, and the size of effects grew larger over time. Two years after implementation, having a must-access PDMP was associated with 0.9 more heroin deaths per 100,000 in a half-year period compared with states without the policy. The largest effect occurred in the last post-period, for

[28]The results for prescription opioid mortality are provided in section 5.4.

Figure 3: Effects of Must-Access PDMPs on Opioid Mortality



Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1). The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Observations are weighted by state population. In Panel A, the dependent variable is heroin deaths per 100,000 (drug code T40.1). In Panel B, the dependent variable is the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4). In Panel C, the dependent variable is prescription opioid deaths per 100,000 (drug code T40.2). In Panel D, the dependent variable is total deaths from any opioid, including heroin (drug codes T40.1–T40.4). In all panels, the Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treated states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the treated sample is balanced in relative periods from -15 to +6. The distant relative periods outside the -15/+6 event time window are trimmed. The control states are the 34 control states that did not implement must-access policies until 2016h2, and the control sample is balanced from 2003h1 to 2016h2. Florida is excluded from the control sample in all panels (see Appendix Section B). In all panels, I control for fixed effects for state and half-year, an indicator for whether a state had a pill mill law, the ARCOS measure of pre-reformulation OxyContin use interacted with time dummies, and the following time-varying covariates: the log unemployment rate, share of the population in six age groups (0–14, 15–24, 25–44, 45–64, 65–84, 85+), share non-Hispanic black, share Hispanic, and share other race.

which I estimate an effect of 1.13.[29]

Consequences of Adding Controls I examine the consequences of controlling for state- and time-varying covariates and co-occurring opioid-related policies. Panels A–D of Figure 4 plot estimates from the baseline difference-in-differences specification (equation 1) with different sets of controls and with heroin mortality as the dependent variable. In Figure 4 Panel A, I include

[29]Given that the population-weighted mean of the ten treated states' heroin-related death rates (Ruhm-corrected number of deaths per 100,000 in a half-year period) in 2015h2 was 4.1, must-access PDMPs had a substantial impact on heroin deaths in these states. The weighted mean of heroin death rates among the ten treated states increased from 2 in 2012h2 to 4.1 in 2015h2, while that among the 34 control states increased from 1.14 to 2 during the same period.

Table 3: Effects of Must-Access PDMPs on Heroin Death Rates and Illegal Opioid Death Rates

	Original Estimates (β_k)				Trend Break Estimates (Δn)
	(1)	(2)	(3)	(4)	(5)
Panel A. Heroin deaths per 100,000 population (T40.1)					
1-year effect (β_2 or $\Delta 3$)	0.52**	0.51**	0.41	0.42*	0.36
	(0.25)	(0.24)	(0.26)	(0.21)	(0.34)
2-year effect (β_4 or $\Delta 5$)	1.08***	1.04***	0.87**	0.90***	0.88**
	(0.35)	(0.33)	(0.38)	(0.29)	(0.43)
3-year effect (β_6 or $\Delta 7$)	1.42***	1.39***	1.10*	1.13**	1.10**
	(0.50)	(0.49)	(0.56)	(0.46)	(0.52)
Mean of dependent variable	1.098	1.098	1.098	1.098	1.098
R^2	0.768	0.809	0.834	0.845	0.845
Panel B. Heroin and synthetic opioid deaths per 100,000 population (T40.1, T40.4)					
1-year effect (β_2 or $\Delta 3$)	0.56	0.54	0.36	0.40	0.22
	(0.41)	(0.43)	(0.38)	(0.35)	(0.34)
2-year effect (β_4 or $\Delta 5$)	1.67***	1.63**	1.24**	1.36***	1.19**
	(0.57)	(0.63)	(0.54)	(0.49)	(0.45)
3-year effect (β_6 or $\Delta 7$)	2.72***	2.68**	1.98**	2.08**	1.88**
	(0.99)	(1.07)	(0.95)	(0.90)	(0.83)
Mean of dependent variable	1.799	1.799	1.799	1.799	1.799
R^2	0.712	0.746	0.786	0.807	0.807
Ruhm (2018) correction	X	X	X	X	X
State fixed effects	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X
Time-varying covariates		X	X	X	X
Pill mill laws		X	X	X	X
OxyContin reformulation			X	X	X
Measure of OxyContin use			NSDUH	ARCOS	ARCOS
Observations	1,172	1,172	1,172	1,172	1,172

Notes: The table shows the 1-year effect (the original coefficient β_2 or the trend break estimate $\Delta 3$), 2-year effect ($\beta 4$ or $\Delta 5$), and 3-year effect (β_6 or $\Delta 7$) from the baseline specification (equation 1) with different sets of controls. Columns 1–4 report the original estimates (β_k), and column 5 presents the trend break estimates (see equation 2). Although each regression includes a full set of indicators for pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. Observations are weighted by state population. In Panel A, the dependent variable is heroin deaths per 100,000 (drug code T40.1). In Panel B, the dependent variable is the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4). In all columns, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In column 1, I include only the fixed effects for state and half-year, and the indicators for pre- and post-periods. In column 2, I add an indicator for whether a state had a pill mill law and the following state- and time-varying controls: the log unemployment rate, share of the population in six age groups (0–14, 15–24, 25–44, 45–64, 65–84, 85+), share non-Hispanic black, share Hispanic, and share other race. Columns 3–6 additionally control for the 2010 OxyContin reformulation by including a measure of pre-reformulation OxyContin use interacted with the half-year fixed effects: column 3 uses the NSDUH measure, and columns 4 and 5 use the ARCOS measure of OxyContin use. In all columns, the treatment states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the treated sample is balanced in relative periods from -15 to +6. Distant event periods outside the -15/+6 window are trimmed. In all columns, the control states are the 34 that did not implement must-access policies until 2016h2, and the control sample is balanced from 2003h1 to 2016h2. Florida is excluded from the control sample in all columns (see Appendix Section B). Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

only the fixed effects for state and time and the indicators for pre- and post-periods (the corresponding regression coefficients are presented in column 1 of Table 3 Panel A). I gradually

add more controls in subsequent panels to arrive at my baseline estimates presented in Panel D.[30] Using the simple specification in Figure 4 Panel A, I find that the coefficients are positive and statistically significant in all the post-periods, though the coefficient for event time +1 is statistically insignificant. However, there is evidence of an upward trend in heroin mortality in the last few pre-periods when both the covariates and the OxyContin reformulation are not controlled for. This pre-trend may reflect the confounding effects of the reformulation, which caused a transition from nonmedical use of prescription opioids to heroin use prior to most of the treatment states' implementation of must-access PDMPs. The shaded area in Panel A indicates the time of the reformulation, which was introduced in 2010h2. The number of treated states at the time of the reformulation in each event time period is presented in the parentheses below that period.

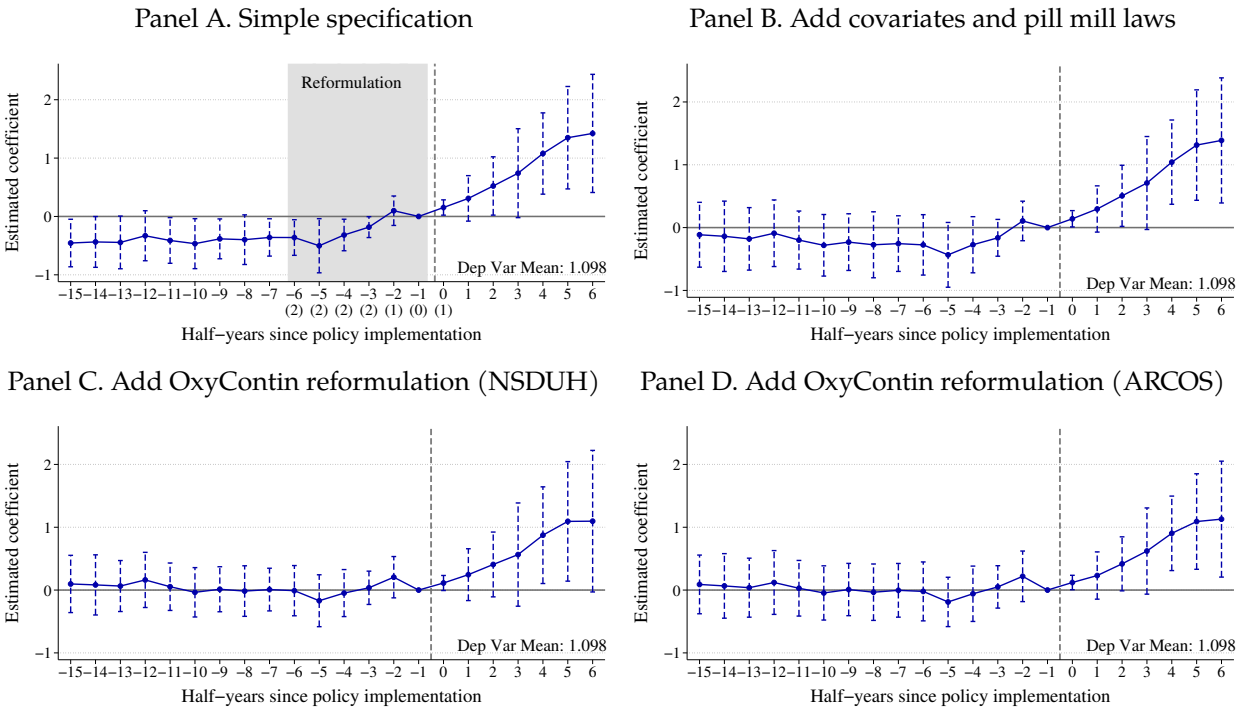
Panel B of Figure 4 presents the estimates when the state- and time-varying covariates and an indicator for whether a state had a pill mill law are included in the regression (the corresponding estimates are presented in column 2 of Table 3 Panel A).[31] The estimates for the post-periods are largely unaffected by adding these controls, but almost all the coefficients for the pre-periods become statistically insignificant. However, I still observe suggestive evidence of a pre-trend in heroin mortality when the reformulation is not controlled for.

In Panels C and D, I additionally control for the confounding effect of the 2010 reformulation by adding a measure of pre-reformulation OxyContin use interacted with the half-year fixed effects to the regression. As described in Section 3, I use the two alternative measures of OxyContin use: in Panel C, I interact the NSDUH measure with the half-year fixed effects (the corresponding regression coefficients are presented in column 3 of Table 3 Panel A), and in Panel D, I interact the ARCOS measure with the half-year fixed effects (the corresponding regression coefficients are presented in column 4 of Table 3 Panel A). I obtain similar results across these two measures; compared with the estimates in Panel B, I see in Panels C and D that the size of the estimated effect on heroin mortality slightly decreases in every post-period. Importantly, as noted above, I observe no evidence of a pre-trend when accounting for the reformulation. These findings suggest that controlling for the reformulation allows for a transparent identification of

[30] The baseline estimates presented in Figure 4 Panel D are identical to those in Figure 3 Panel A.

[31] Pill mill laws, which impose regulations on pain clinics to prevent them from issuing opioid prescriptions without medical indication, were enacted in three treated states around the time of policy implementation. I view pill mill laws as complements to must-access laws, as Buchmueller and Carey (2018) do. Appendix Figure A20 suggests that, in the absence of a must-access law, a pill mill law had no independent effect on my outcomes. See Appendix Section C for more details.

Figure 4: Sensitivity of Heroin Estimates to Adding Controls



Notes: The figure shows how adding controls affects the heroin estimates by plotting the coefficients on indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) with different sets of controls. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. In all panels, the dependent variable is the Ruhm-corrected numbers of heroin deaths per 100,000 (drug code T40.1), which are calculated using data from the National Vital Statistics System (NVSS). In Panel A, I only include the fixed effects for state and half-year and the indicators for pre- and post periods. The gray shaded area in Panel A indicates the time of the reformulation, which was introduced in 2010h2. The number of treated states at the time of the reformulation in each event time period is presented in the parentheses below that period. In Panel B, I add an indicator for whether a state had a pill mill law and the following time-varying covariates: the log unemployment rate, share of the population in six age groups (0–14, 15–24, 25–44, 45–64, 65–84, 85+), share non-Hispanic black, share Hispanic, and share other race. In Panels C and D, I add a measure of pre-reformulation OxyContin use interacted with time fixed effects. I use the two alternative measures of pre-reformulation OxyContin use: the NSDUH measure (Panel C, see Section 3) and the ARCOS measure (Panel D, see Section 3). The estimates presented in Panel D are identical to those in Figure 3 Panel A. Observations are weighted by state population. The treated states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the treated sample is balanced in relative periods from -15 to +6. The distant relative periods outside the -15/+6 event time window are trimmed. The control states are the 34 control states that did not implement must-access policies until 2016h2, and the control sample is balanced from 2003h1 to 2016h2. Florida is excluded from the control sample in all panels (see Appendix Section B).

the impact of must-access PDMPs by accounting for the pre-existing correlation between the implementation of must-access PDMPs and exposure to the reformulation.[32]

To summarize, I find a strong and positive association between must-access PDMPs and

[32]For all the outcome variables, I find similar results regardless of whether I use the ARCOS measure or the NSDUH measure. I present the mortality results that I obtain using the NSDUH measure in columns 3 of Tables 3 and 5 (corresponding regression coefficients are displayed in Appendix Figure A3).

heroin death rates.[33] Heroin mortality began to increase in the first post-period, and the size of the effects grew over time. The results patterns are similar across different specifications, but I observe an upward pre-trend in heroin mortality when I do not control for the reformulation. I find that two years after implementation, the treated states had 0.9 additional heroin deaths per 100,000 in a half-year period relative to the control states.

Heroin-Only Mortality I examine the more exclusive measure of heroin deaths, which I refer to as heroin-only mortality. The heroin-only mortality indicates the deaths that involved heroin (T40.1) and no other types of opioids (T40.2–4) at the time of death. Column 1 of Table 4 reports the trend break estimates (equation 2) from the baseline specification (equation 1) with heroin-only mortality as the dependent variable. As seen in Column 1, all the trend break estimates are statistically insignificant and much smaller than those obtained using total heroin mortality as the dependent variable (see column 5 of Table 3 Panel A). Most heroin is now laced with illicit fentanyl, which may explain why the policy had little effect on heroin-only mortality while having a strong effect on total heroin mortality. This motivates me to investigate the combined deaths from heroin and synthetic opioids, which I use as my measure of illegal opioid mortality.

5.3 Effects of Must-Access PDMPs on Illegal Opioid Mortality

I examine how must-access PDMPs affected deaths from illegal opioids, such as heroin and illicit fentanyl. Figure 3 Panel B plots the coefficients on the indicators for pre- and post-periods from the main difference-in-differences specification (equation 1) with the combined deaths from heroin and synthetic opioids per 100,000 as the dependent variable. The plot shows the point estimates and 95 percent confidence intervals. The corresponding regression coefficients and trend break estimates are presented in columns 4 and 5 of Table 3 Panel B, respectively. Columns 1–4 of Table 3 Panel B report the original regression coefficients from the baseline specification (equation 1) with different sets of controls. In Table 3, controls are identical to those in Table 3 Panel A. Standard errors in all specifications are clustered at the state level.

As seen in Figure 3 Panel B and in columns 4 and 5 of Table 3 Panel B, illegal opioid mortality began to increase in the year of implementation, and the size of effects grew over time, although the one-year effect is statistically indistinguishable from zero. Compared to the estimates for

[33] The findings in this paper complement a contemporaneous working paper by Mallatt (2019), which finds that must-access PDMPs increase heroin crime rates within opioid-dense counties during my sample period.

Table 4: Effects of Must-Access PDMPs on the Exclusive Measures of Opioid Death Rates

	Heroin Only	Illegal Opioid-Only	Prescription Opioid-Only
	(1)	(2)	(3)
Dependant variable: Drug overdose deaths per 100,000 population			
1-year effect ($\Delta 3$)	0.16	0.30	-0.93**
	(0.31)	(0.32)	(0.43)
2-year effect ($\Delta 5$)	0.38	1.09**	-1.03
	(0.41)	(0.44)	(0.64)
3-year effect ($\Delta 7$)	0.25	1.74**	-0.95*
	(0.43)	(0.76)	(0.53)
Mean of dependent variable	0.836	1.436	1.532
R^2	0.842	0.801	0.863
Ruhm (2018) correction	X	X	X
State fixed effects	X	X	X
Half-year fixed effects	X	X	X
Time-varying covariates	X	X	X
Pill mill laws	X	X	X
OxyContin reformulation	X	X	X
Measure of OxyContin use	ARCOS	ARCOS	ARCOS
Observations	1,172	1,172	1,172

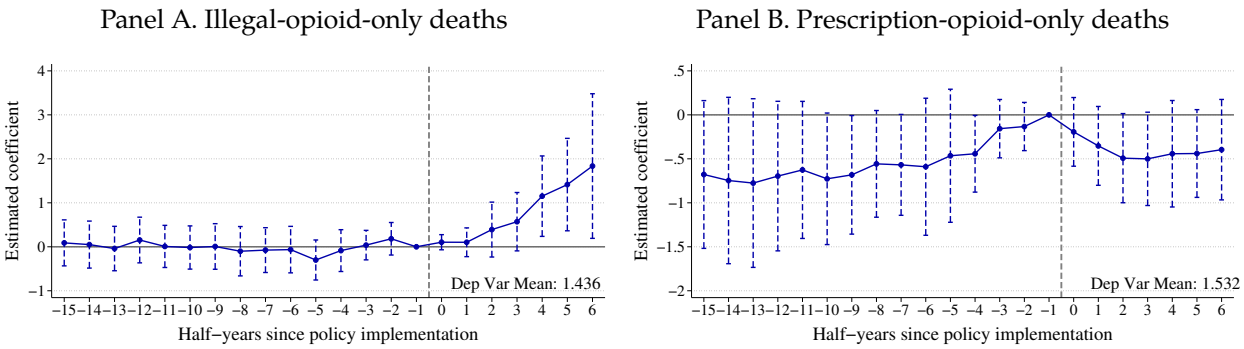
Notes: The table shows the 1-year effect (the trend break estimate $\Delta 3$), 2-year effect ($\Delta 5$), and 3-year effect ($\Delta 7$) from the baseline specification (equation 1). In all columns, I report the trend break estimates (see equation 2). Although each regression includes a full set of indicators for pre- and post-periods, I report the three statistics above for brevity. The last pre-period is omitted. Observations are weighted by state population. In column 1, the dependent variable is heroin-only deaths per 100,000, which involved drug code T40.1 but not T40.2, T40.3, or T40.4 at the time of death. In column 2, the dependent variable is illegal-opioid-only deaths per 100,000, which involved drug code T40.1 or T40.4 but not T40.2 or T40.3. In column 3, the dependent variable is prescription-opioid-only deaths, which involved drug code T40.2, but not T40.1, T40.3 or T40.4. In all columns, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. The analysis sample and controls are identical to those in Table 3. Fixed effects for state and half-year are always included. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

heroin mortality (see Figure 3 Panel A and columns 4 and 5 of Table 3 Panel A), the magnitude of the policy impact on illegal opioid mortality is larger in every post-period, and the effects grew much faster over time.[34]

Illegal-Opioid-Only Mortality Now, I examine the more exclusive measure of illegal opioid mortality. In Figure 5, Panel A displays the coefficients from the baseline specification (equation 1) with illegal-opioid-only deaths per 100,000 as the dependent variable. The corresponding trend break estimates are reported in column 2 of Table 4. The results pattern in Figure 5 Panel A is

[34]Some caution in the interpretation of my illegal opioid results is needed. There is a limitation in assessing whether there was a pre-trend in illegal opioid mortality because the surge in synthetic opioid deaths began in the states' post-period. Although I find no evidence of a pre-trend in illegal opioid mortality, it may not fully support the parallel trend assumption.

Figure 5: Effects of Must-Access PDMPs on the Exclusive Measures of Opioid Deaths



Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1). The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Observations are weighted by state population. In Panel A, the dependent variable is illegal-opioid-only deaths per 100,000, which involved drug code T40.1 or T40.4 but not T40.2 or T40.3 at the time of death. In Panel B, the dependent variable is prescription-opioid-only deaths, which involved drug code T40.2, but not T40.1, T40.3 or T40.4. In all panels, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the analysis sample and controls are identical to those in Figure 3. Fixed effects for state and half-year are always included.

similar to that in Figure 3 Panel B, in which total illegal opioid mortality is the outcome variable. These findings suggest that must-access PDMPs increased illegal opioid mortality not attributable to prescription opioids.

5.4 Effects of Must-Access PDMPs on Prescription Opioid Mortality

Figure 3 Panel C investigates the impact on prescription opioid mortality by plotting the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) with total prescription opioid deaths per 100,000 as the dependent variable. The plot displays the point estimates and 95 percent confidence intervals. The corresponding regression coefficients and trend break estimates are presented in columns 4 and 5 of Table 5 Panel A, respectively. Columns 1–4 of Table 5 Panel A report the original regression coefficients from the baseline specification (equation 1) with different sets of controls. In Table 5, controls are identical to those in Table 3. Standard errors in all specifications are clustered at the state level.

In Figure 3 Panel C, I see that there was an upward trend in prescription opioid mortality in the last few pre-periods, but following implementation, prescription opioid mortality began to decrease. Although this pre-trend suggests that the trends in prescription opioid mortality were different across the treated and control groups prior to implementation, the negative trend break

Table 5: Effects of Must-Access PDMPs on Prescription Opioid Death Rates and Net Effects

	Original Estimates (β_k)				Trend Break Estimates (Δn)
	(1)	(2)	(3)	(4)	(5)
Panel A. Prescription opioid deaths per 100,000 population (T40.2)					
1-year effect (β_2 or $\Delta 3$)	-0.46**	-0.52**	-0.51**	-0.54**	-1.10**
	(0.22)	(0.24)	(0.22)	(0.25)	(0.46)
2-year effect (β_4 or $\Delta 5$)	-0.14	-0.23	-0.23	-0.27	-1.04
	(0.28)	(0.31)	(0.29)	(0.32)	(0.74)
3-year effect (β_6 or $\Delta 7$)	-0.03	-0.14	-0.20	-0.27	-0.96
	(0.29)	(0.33)	(0.31)	(0.35)	(0.62)
Mean of dependent variable	1.984	1.984	1.984	1.984	1.984
R^2	0.818	0.854	0.864	0.861	0.861
Panel B. Total opioid-related deaths per 100,000 population (T40.1–T40.4)					
1-year effect (β_2 or $\Delta 3$)	0.01	-0.04	-0.16	-0.17	-0.85
	(0.47)	(0.50)	(0.42)	(0.42)	(0.54)
2-year effect (β_4 or $\Delta 5$)	1.19**	1.10*	0.80	0.84*	0.07
	(0.55)	(0.63)	(0.52)	(0.46)	(0.76)
3-year effect (β_6 or $\Delta 7$)	2.30**	2.23**	1.60*	1.61*	0.92
	(0.99)	(1.09)	(0.94)	(0.89)	(0.91)
Mean of dependent variable	4.126	4.126	4.126	4.126	4.126
R^2	0.762	0.802	0.824	0.842	0.842
Ruhm (2018) correction	X	X	X	X	X
State fixed effects	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X
Time-varying covariates		X	X	X	X
Pill mill laws		X	X	X	X
OxyContin reformulation			X	X	X
Measure of OxyContin use			NSDUH	ARCOS	ARCOS
Observations	1,172	1,172	1,172	1,172	1,172

Notes: The table shows the 1-year effect (the original coefficient β_2 or the trend break estimate $\Delta 3$), 2-year effect ($\beta 4$ or $\Delta 5$), and 3-year effect (β_6 or $\Delta 7$) from the baseline specification (equation 1) with different sets of controls. Columns 1–4 report the original estimates (β_k), and column 5 presents the trend break estimates (see equation 2). Although each regression includes a full set of indicators for pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. Observations are weighted by state population. In Panel A, the dependent variable is prescription opioid deaths per 100,000 (drug code T40.2). In Panel B, the dependent variable is total deaths from any opioid, including heroin, per 100,000 (drug codes T40.1–T40.4). In all columns, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. The analysis sample and controls are identical to those in Table 3. Fixed effects for state and half-year are always included. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

in the first post-period provides suggestive evidence of a negative association between must-access PDMPs and prescription opioid mortality.

Figure 3 Panel C also suggests that the negative effect of the policy on prescription opioid mortality was temporary: the magnitude of effects faded out over time. In Table 5 Panel A, the estimated one-year effects are statistically different from zero in all columns. The trend break estimates in column 5 indicate that a year after policy implementation (or three half-years after the reference period), must-access PDMPs were associated with 1.1 less prescription opioid

mortality relative to the three-half-year change prior to the reference period.[35] In contrast, two-year and three-year effects are statistically insignificant in all columns. One possible explanation for the temporary negative effects of must-access PDMPs on total prescription opioid mortality is that people may gradually substitute illegal opioids for prescription opioids, and deaths involving both prescription and illegal opioids increase over time. I investigate the more exclusive measure of prescription opioid mortality and find evidence supporting this possibility, as presented below.

Prescription-Opioid-Only Mortality In Figure 5 Panel B, the dependent variable is deaths caused by prescription opioids but not also heroin, methadone, or synthetic opioids, which I refer to as prescription-opioid-only mortality. The corresponding trend break estimates are reported in column 3 of Table 4. Figure 5 Panel B shows that the negative effects of must-access PDMPs on prescription-only mortality are more persistent compared with the effects on total prescription opioid mortality (see Figure 3 Panel C), although coefficients for the later periods are still statistically insignificant. The more persistent effects on prescription-opioid-only mortality, combined with the temporary effects on total prescription opioid mortality, support the possibility that existing users gradually switched to illegal opioids. In sum, the results for prescription opioid deaths provide another important piece of evidence of a transition from prescription opioids to illegal opioids.

5.5 Net Effects of Must-Access PDMPs on Total Opioid-Related Mortality

Finally, I examine the net effects of must-access PDMPs on total deaths from any opioid, including prescription opioids, heroin, and synthetic opioids. In this paper, I provide evidence that must-access PDMPs increased illegal opioid mortality, but I also find the negative effects on prescription opioid mortality. These offsetting effects are more clearly observed in Figure 5. Estimating the net effects of must-access PDMPs on the total opioid-related mortality is particularly important because the total opioid death rate is the target at which drug policies are ultimately aimed.

Figure 3 Panel D plots the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) with total opioid-related mortality as the dependent variable. The plot displays the point estimates and 95 percent confidence intervals. The corresponding regression coefficients and trend break estimates are presented in columns 4

[35] The magnitude of the trend break estimates reported in column 5 is larger than that of the original regression coefficients in column 4, because of the upward pre-trend in prescription opioid mortality (see Figure 3 Panel C).

and 5 of Table 5 Panel B, respectively. Columns 1–4 of Table 5 Panel B report the original regression coefficients from the baseline specification (equation 1) with different sets of controls. Controls are identical to those in Table 3. Standard errors in all specifications are clustered at the state level.

Figure 3 Panel B shows that the size of the policy impacts on total opioid mortality was close to zero in the first four post-periods and then increased over time. In Table 5 Panel B, I observe that the estimated three-year effects are positive and statistically significant in most columns, although the size of the three-year effects are not stable across specifications. In summary, I find that must-access PDMPs had no substantial effect on total opioid-related mortality in the short term because of the offsetting effects; however, in the longer term, the policies were positively associated with total opioid mortality because the increase in illegal opioid mortality outweighed the decrease in prescription opioid mortality.

5.6 Heterogeneous Treatment Effects

In my main results discussed above, I do not consider different strengths of must-access laws among the ten treatment states, although they vary greatly among states. To investigate heterogeneity in the effects of must-access laws across different strengths, I divide the ten states' must-access laws into three types: limited laws, discretionary laws, and broad laws.[36] Limited laws are defined as those that apply only to limited ingredients (the initial PDMP in Oklahoma) or laws that require access under limited circumstances (the initial PDMP in Vermont); discretionary laws are those that rely on provider suspicion of abuse (Delaware and the initial PDMP in Ohio); and broad laws are those that apply to all clinic settings and ingredients and require provider access, even without suspicion of abuse.[37] One challenge in my heterogeneity analysis is that the three treatment states strengthened their laws from limited to broad in 2015. Because it is difficult to distinguish between the long-run effect of the initial laws and the immediate effect of the strengthened laws, my heterogeneity analysis focuses on the states' initial must-access laws. The fact that the three treated states strengthened their laws at least two years after the implementation of the initial must-access laws allows me to look at the two-year effect of the initial laws.

Figure 6 presents the trend break estimates summarizing the two-year effect that I obtain when I interact the indicators for all the seven post-periods from the baseline specification

[36] This categorization is proposed by Buchmueller and Carey (2018).

[37] Kentucky, Massachusetts, New Mexico, New York, Tennessee, West Virginia, and the recent laws in Ohio, Oklahoma, and Vermont are coded as having a broad law.

(equation 1) with indicators for three law types in a single regression.[38,39] The red (horizontal) dashed line shows the overall effect among the ten treated states, indicating the two-year trend break estimate ($\Delta 5$) obtained from the baseline specification (see equations 1 and 2). Figure 6 suggests that broad laws and discretionary laws had stronger effects than limited policies. For most of my outcomes, limited laws had little or no effect, while the other two types of laws were positively associated with heroin mortality (Panel A) and illegal opioid mortality (Panel B), and negatively associated with prescription opioid mortality (Panel C) and oxycodone doses per capita (Panel E). The estimates indicate that the net effect of must-access laws is close to zero, regardless of the strength of the must-access law (Panel D). Interestingly, discretionary laws had even stronger effects than broader laws for some of the outcomes, which is largely driven by Ohio's policy. In Appendix Section C, I discuss in detail why Ohio's initial must-access policy, which relied on provider suspicion, had strong impacts.[40] In sum, my heterogeneity analysis suggests that broad and discretionary laws had stronger effects than limited laws, which had little effects on my outcomes, and that discretionary laws had even stronger impacts on some outcomes than broad laws.[41] In addition, in Appendix Figures A7 and A8, I present event studies separated by subgroups that I obtain when I limit the treated states to each subgroup; the results shown in these figures are consistent with those presented in Figure 6.[42]

6 Robustness Tests

In Table 6, I test the robustness of heroin results to a number of alternative explanations for the association between must-access PDMPs and increased heroin mortality (the corresponding

[38] More specifically, the trend break estimates (summarizing the two-year effects) for each law type presented in Figure 6 are calculated as follows: $\Delta 5 = (\beta_4 * \mathbf{1}(\text{Law Type}) - \beta_{-1}) - (\beta_{-1} - \beta_{-5})$.

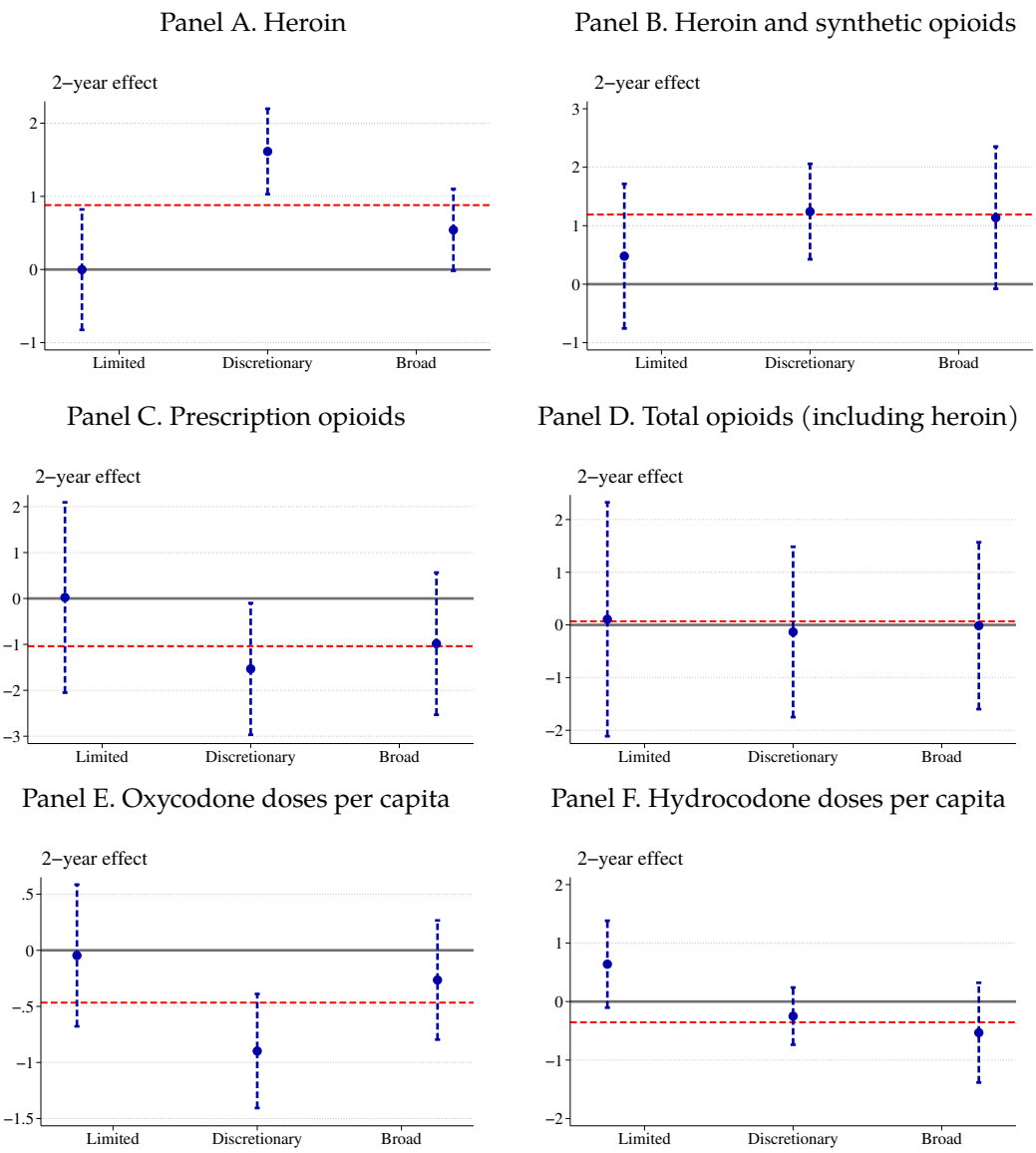
[39] I interact law types with the indicators for the post-periods instead of the full set of indicators for pre- and post-period, because each of the limited and discretionary subgroups consists of only two states, and thus the pre-trends in outcomes are noisy for these subgroups (see Panels A.2 and B.2 of Appendix Figure A7).

[40] I propose three possible explanations for the strong impact of Ohio's initial must-access law—a sharp increase in PDMP utilization, the existence of a complementary law, and high accessibility of heroin. See Appendix Section C for more detail.

[41] My findings are consistent with several of Buchmueller and Carey's (2018) findings, which provide mixed results on heterogeneous effects by subgroups: for their quantity-based outcomes, they find that broad laws had similar but a slightly larger size of effects than discretionary laws and that limited policies had little effect; however, for the other outcomes, their results suggest no clear pattern; for example, they show that discretionary laws had stronger effects than broader policies for some of their shopping outcomes, and in most other cases, the estimate for each law type cannot be distinguished from the overall estimate.

[42] Note that in Appendix Figures A7 and A8, I use a shorter event time window (-15/+4) than that used in the baseline analysis (-15/+6), to focus on the states' initial must-access laws. The distant periods outside the -15/+4 window are trimmed. Although the mortality results for the discretionary and limited subgroups, each of which includes only two states, are noisy, Appendix Figures A7 and A8 provide further evidence of heterogeneity across different types of must-access laws.

Figure 6: Heterogeneous Treatment Effects—Three Types of Must-Access Laws



Notes: The figure shows the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). Each panel presents the estimates obtained when I interact the indicators for all the post-periods (from event time 0 to +6) from the baseline specification (equation 1) with three indicators for limited, discretionary and broad laws. The (horizontal) dashed red line presents the overall estimate, for reference. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. For all the outcomes, I report the trend break estimates summarizing the two-year effect ($\Delta 5 = (\beta_4 * \mathbf{1}(\text{Law Type}) - \beta_{-1}) - (\beta_{-1} - \beta_{-6})$). I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. In all panels, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. Observations are weighted by state population. In all panels, the analysis sample and controls are identical to those in Figure 3. Fixed effects for state and half-year are always included.

Table 6: Robustness of Heroin Estimates

	Heroin Deaths per 100,000 (T40.1)							
	(1) Baseline	(2) Reported mortality	(3) Voluntary PDMPs	(4) Include FL	(5) Add MMLs	(6) Add NALs	(7) Good Sam laws	(8) Alternative dates
1-year effect (β_2)	0.42*	0.47**	0.40*	0.42*	0.35	0.46**	0.44**	0.53
	(0.21)	(0.22)	(0.21)	(0.23)	(0.26)	(0.20)	(0.21)	(0.34)
2-year effect (β_4)	0.90***	1.02***	0.89***	0.88***	0.87**	1.00***	0.94***	0.98***
	(0.29)	(0.29)	(0.29)	(0.31)	(0.32)	(0.29)	(0.29)	(0.35)
3-year effect (β_6)	1.13**	1.24***	1.11**	1.10**	1.08**	1.13**	1.16**	1.02*
	(0.46)	(0.43)	(0.45)	(0.46)	(0.47)	(0.42)	(0.46)	(0.53)
Ruhm (2018) correction	X		X	X	X	X	X	X
Number of treatment states	10	10	10	10	10	10	10	8
Number of control states	34	34	34	35	34	34	34	31
Observations	1,172	1,172	1,172	1,200	1,172	1,172	1,172	1,044
Mean of dependent variable	1.098	0.872	1.098	1.082	1.098	1.098	1.098	1.114
R^2	0.844	0.831	0.845	0.844	0.850	0.849	0.844	0.845

Notes: The table tests the robustness of my baseline estimates for heroin mortality to alternative explanations. The table shows the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6), obtained from the baseline specification (equation 1). Although each regression includes a full set of indicators for pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. Observations are weighted by state population. In all columns, the dependent variable is the heroin deaths per 100,000 (drug code T40.1). In column 1, I repeat my baseline heroin estimates from column 4 of Table 3. In the subsequent columns, I change or add some factors one by one. In column 2, I use the raw reported numbers of deaths, and the other columns use the Ruhm-corrected numbers of deaths. Both the corrected and reported numbers of deaths are calculated using data from the National Vital Statistics System (NVSS). In column 3, I control for an indicator for whether a state had a voluntary-access PDMP. In column 4, I include Florida in the analysis sample. Florida is excluded from the control sample in the other columns (see Appendix Section B). In columns 5–7, I include several other co-occurring opioid-related policies one by one: in column 5, I include a time-varying indicator for whether the state had medical marijuana laws (MMLs) as well as an indicator for whether the state had legal and operational dispensaries; in column 6, I add an indicator for whether the state had naloxone access laws (NALs), and in column 7, I add an indicator for whether the state had Good Samaritan overdose prevention laws. In column 8, I use alternative start dates of must-access PDMPs listed in the third column of Table 1, and in this estimation, the treated states are the 8 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 31 that did not implement must-access policies until 2016h2, excluding Florida. In all columns, the distant event periods outside the -15/+6 window are trimmed. In all columns, I control for fixed effects for state and half-year, an indicator for whether a state had a pill mill law, the ARCOS measure of pre-reformulation OxyContin use interacted with the half-year fixed effects, and the time-varying covariates that are identical to those in column 4 of Table 3. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

regression coefficients are displayed in Panel A of Appendix Figure A10). In column 1 of Table 6, I repeat my baseline estimates for heroin mortality (from column 4 of Table 3) to test the sensitivity to specification. Standard errors in all columns are clustered at the state level. In column 2, I use the raw reported mortality rate as the dependent variable instead of the Ruhm-corrected mortality rate. I estimate a slightly larger effect when I use raw reported mortality, but the results are qualitatively similar.[43,44] In column 3, I control for whether a state

[43] Appendix Figure A9 displays the estimates from the baseline specification (equation 1) with the reported mortality rates as the dependent variable.

[44] However, note that the mean values are different across the two measures. The Ruhm (2018) correction suggests how to correct the problem of underreporting of overdose deaths. By construction, raw reported mortality rates have a smaller sample mean and standard deviation than Ruhm-corrected mortality rates. Given the smaller standard deviation of the reported mortality rates, the fact that I find the larger effect on reported heroin mortality than on Ruhm-corrected heroin mortality (see columns 1 and 2 of Table 6) supports the possibility that using reported mortality may overstate the policy effect on heroin death rates.

had a voluntary-access PDMP and find similar results.[45,46] In column 4, I include Florida in the analysis sample, and the estimates are similar.[47] In columns 5–7, I include several other co-occurring opioid-related policies one by one to test for the sensitivity of my baseline estimates to adding each variable.[48] In column 5, I include a time-varying indicator for whether the state had medical marijuana laws (MMLs) as well as an indicator for whether the state had legal and operational dispensaries and find similar results, although the one-year effect becomes insignificant. In column 6, I add an indicator for whether the state had naloxone access laws (NALs), and in column 7, I add an indicator for whether the state had Good Samaritan overdose prevention laws. Columns 5–7 suggest that my estimates are robust to including several other co-occurring state opioid-related policies. Although not reported, I also obtain similar and statistically significant coefficients when I include these co-occurring policies in a single regression. In column 8, I use alternative start dates of must-access laws taken from Sacks et al. (2019) and find similar effects of must-access PDMPs on heroin mortality, though the one-year effect becomes statistically insignificant with these dates.[49] Appendix Table A4 conducts the same robustness tests as those in Table 6 for the other mortality outcomes, and I find similar results. Finally, Appendix Table A3 shows the sensitivity of my heroin estimates to dropping one treated state (the corresponding regression coefficients are displayed in Panel A of Appendix Figure A11); Panels B–D of Appendix Figure A11 show the results for the same tests for the other mortality outcomes. I find that my baseline estimates for the mortality outcomes are robust to removing one treated state. My heroin mortality estimates are statistically significant regardless of which treated state is dropped, but when I drop Ohio, the magnitudes of the estimates are slightly attenuated.[50]

[45] In Appendix Table A2, I conduct the same test using the following alternative start dates of voluntary-access PDMPs: enactment dates from the PDAPS and NAMSDL, and the modern system user access dates suggested by Horwitz et al. (2018). My estimates are stable across different dates.

[46] I find no substantial effect of voluntary-access PDMPs on the outcome variables investigated in this paper using any source of start dates. First, the size of the coefficients on the indicator for voluntary-access PDMPs is small compared with that for must-access PDMPs. Second, although the coefficients on voluntary-access PDMPs reported in Appendix Table A2 are statistically significant, they become insignificant when I bin distant relative periods rather than trim them.

[47] As mentioned in Section 4, my oxycodone estimates are sensitive to whether I include Florida in the sample. In Appendix Section B, I discuss this sensitivity in detail. Although not reported, my oxycodone results are robust to removing one state when Florida is excluded from the control group.

[48] Data on these policies are derived from PDAPS.

[49] In column 8, the treated states are the eight that implemented must-access PDMPs between 2010h2 and 2013h2, which are consistently observed from event time -15 to +6; the control states are the 31 that did not implement must-access policies until 2016h2, excluding Florida (see the third column of Table 1 for the start dates). Therefore, the difference between the estimates is driven by not only the inconsistency in start dates but also the differential policy effects across states.

[50] In Appendix Section C, I show that Ohio's must-access PDMP had stronger effects on my outcomes than the other states' policies and I propose three possible explanations for the strong effects of Ohio's must-access policy.

7 Conclusion

I examine the spillover effects of must-access PDMPs on illegal opioid use. Using a difference-in-differences approach, I find strong evidence that must-access PDMPs have had the unintended consequences of increased heroin use, which has not been reported in most prior studies. This increase began in the year of policy implementation, and the effects grew over time. Two years following implementation, having a must-access PDMP was associated with 0.9 additional heroin deaths per 100,000 in a half-year period compared with control states. I also find that the increase in deaths from heroin coincided with a sudden decrease in prescription opioid mortality following implementation. The estimates from this paper suggest that the negative effects of must-access laws on prescription opioid mortality were offset by the positive effects on illegal opioid mortality. Overall, I show that must-access PDMPs had no effect on total opioid-related mortality in the short term because of the offsetting effects, but in the longer term, the unintended effect on illegal opioid mortality surpassed the intended effect on prescription opioid mortality.

The findings of this study suggest that focusing on supply-side policies that limit access to legal drugs may simply cause users to shift to using close substitutes. Given the existence of accessible and affordable substitutes for prescription opioids, more robust policies are needed to address the opioid overdose epidemic. Demand-side interventions, such as medication treatment of opioid dependence (e.g., opioid substitution therapies), may be more effective at reducing overall opioid abuse. However, in the long run, must-access PDMPs may have different net effects because they can also reduce initiation into prescription opioids. Although must-access PDMPs target existing users, Sacks et al. (2019) suggests that they are in fact effective at reducing opioid initiation. I investigate the medium-run effects of must-access laws in the first three to six years and find that must-access PDMPs have led to worse outcomes. The long-run impacts of these policies will be determined by many factors, such as the composition of new and existing users, the magnitudes of policy effects on those users, accessibility to substitutes, and accessibility to medication treatment and prevention.

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APPENDIX - FOR ONLINE PUBLICATION

A Comparisons with the Prior Literature

In this section, I compare my study with the prior literature on two dimensions—data period and model specification. Because Meinhofer (2018) is most closely related to my paper, I focus on comparing my study with hers. For simplicity, Meinhofer (2018) is often referred to as Meinhofer in this section. In Appendix Figure A14, I show how my heroin mortality results change if I drop recent data or use the approach suggested by Meinhofer with my analysis sample. Similarly, in Appendix Figure A15, I show how my replication of Meinhofer's event study results for heroin mortality change if I use my model specification instead of hers, while keeping everything else unchanged. Overall, my exercises suggest two things. First, using a longer period, which allows for including additional post-periods and several more implementations of must-access PDMPs, is key to identifying the overall spillover effects of the policy. Second, the heroin estimates from my model specification provide stronger evidence of the spillover effect of must-access PDMPs compared with those from Meinhofer's event study specifications, regardless of whether I include a longer data period or not. The estimates from my model specification are statistically more significant in the post-period and better address concerns about pre-trends in heroin mortality. These differences generated by the specification choices are more pronounced when I use a longer period.

A.1 Data Period

In Appendix Figure A14, I show the consequences of employing more recent data. Each column of Appendix Figure A14 corresponds to different sample periods. In the left column (Panels A, C, and E), I use my full sample (2003h1–2016h2), and in the right column (Panels B, D, and F), I drop the 2014h1–2016h2 period and reconstruct the sample so that the treated states are balanced in relative periods. The sample from the right column includes the five treated states that are consistently observed from nine half-years prior to implementation and three half-years after implementation. The number of treated states observed in each event time period is presented in the parentheses below that period.[51]

[51] While my full sample (used in the left column) excludes one treated state, Nevada, to include more pre-periods, the sample from the right column does not exclude Nevada because it has five treated states only and the estimates are not robust to excluding one of them. The difference in the number of pre-periods across the two samples is attributable to the fact that Nevada, which implemented the earliest must-access PDMP in the nation, has nine pre-periods in my sample.

The first row (Panels A and B) shows how my baseline estimates for heroin mortality (Panel A) change if I drop the recent data period. The two panels display the coefficients on the indicator for pre- and post-periods from the baseline specification (equation 1) on the full sample (Panel A) and on the sample without the recent data (Panel B). As seen in Panel B, even if I drop the period after 2013, I still find suggestive evidence of the spillover effects on heroin mortality. However, compared with the estimates in Panel A, the estimates in Panel B have much larger effect sizes, and all the coefficients in Panel B become close to zero and insignificant if I drop Ohio, one of the five treated states. Overall, Panels A and B suggest that using a longer data period, which allows for including several additional implementations of must-access PDMPs and additional post-periods, is crucial to obtaining robust estimates that reflect the overall spillover effects on heroin mortality.

A.2 Model Specification

In this paper, I provide causal evidence that must-access PDMPs have increased heroin mortality, and my estimates are robust to controlling for several other co-occurring state and national opioid-related policies, including the 2010 OxyContin reformulation. Above, I show that using more recent data is crucial to identifying these effects of must-access policies. However, not only a longer data period but also my model specification contributes to my findings. Because the importance of controlling for the reformulation is discussed in Section 5, I focus on describing the consequences of other specification choices in this section. Because Meinhofer (2018) is most closely related to my paper, I focus on comparing my econometric model with hers.

Below, I present my baseline specification (equation 1, provided below for convenience as equation A1) and the event study specifications used in Meinhofer (equations M1 and M2). Equation M1 is Meinhofer's (2018) preferred event study specification. Note that the notations in equations M1 and M2 slightly differ from those in the original equations presented in Meinhofer (2018) (I use my preferred notations for an easier comparison of specifications), although they are fundamentally the same. To distinguish between year in equations M1 and M2 and half-year in equation 1, I change the subscript for my half-year variable from t to h to indicate half-year, only in this section. The model specifications used in the two studies are as below.

Equation (1) from my paper:

$$y_{sh} = \alpha_s + \alpha_h + \sum_{k \neq -1} \beta_k \mathbf{1}(Policy_{sk}) + X_{sh}\delta + oxy_s \cdot \omega_h + \varepsilon_{sh} \tag{A1}$$

Equation (4) from Meinhofer (2018):

$$ln(Y_{stq}+1) = \alpha_s + \alpha_t + \alpha_q + \sum_{k\neq -1} \beta_k \mathbf{1}(Policy_{sk}) + \gamma ln(P_{stq}) + \theta_s \cdot t + \varepsilon_{stq} \quad \text{(M1)}$$

Equation (3) from Meinhofer (2018):

$$ln(Y_{stq}+1) = \alpha_s + \alpha_t + \alpha_q + \sum_{k\neq -1} \beta_k \mathbf{1}(Policy_{sk}) + \gamma ln(P_{stq}) + \varepsilon_{stq} \quad \text{(M2)}$$

where $ln(Y_{stq}+1)$ is the log of quarter-level overdose deaths, and Meinhofer (2018) adds 1 to all outcomes to avoid losing observations with count zero. P_{stq} is state population, and $y_{sh} = Y_{sh}/P_{sh} * 100,000$ is overdose deaths per 100,000. α_s are state fixed effects, and α_t, α_h, and α_q are fixed effects for year, half-year, and quarter (seasonality), respectively. $oxy_s \cdot \omega_t$ in equation 1 indicates the measure of pre-reformulation OxyContin use interacted with the half-year fixed effects. $\theta_s \cdot t$ in equation M2 is state-specific (year-level) trends. ε_{stq} is the error term. Note that the regressions in my study are weighted by state population, while those in Meinhofer are not; to be consistent, throughout this section, the regressions estimating my specification (equation 1) are weighted by population, while the regressions estimating Meinhofer's (equations M1 and M2) are unweighted. Although not reported in the paper, my baseline heroin estimates are stable across the weighted and unweighted regressions.

In Appendix Figure A14, each row estimates one of the three specifications: the top row (Panels A and B) estimates my baseline specification (equation 1), the middle (Panels C and D) estimates Meinhofer's preferred event study specification (equation M1), and the bottom (Panels E and F) estimates Meinhofer's alternative event study specification (equation M2). Panel C shows that my results from the full sample (Panel A) are substantially affected if I use Meinhofer's preferred specification, while everything else that includes the full sample remains unchanged. If I use the same specification as in Panel C and drop the period after 2013, I obtain the results shown in Panel D, which are expected to be similar to Meinhofer's heroin results (Meinhofer uses data through 2013). In fact, Panel D has a results pattern similar to my replication of Meinhofer (see Panel A of Figure A15), although balancedness, data frequency, and the time window length are different across the two figures (the replication of Meinhofer is explained in detail below). All the panels in Figure A14 use half-year frequency data, and thus Panels C–F employ equations M1 and M2, which are based on quarter frequency, by including half-year fixed effects instead of fixed effects

for year and quarter (seasonality).

Compared with Panels A and B, which are based on my specification, Panels C and D show that the estimated policy effects are statistically less significant, and there is evidence of a pre-trend prior to policy implementation. In particular, with the more recent data in Panel C, using equation M1 leads to a clear upward pre-trend in the entire pre-period, providing evidence of a violation of the parallel trends identification assumption. Finally, Panels E and F display the results obtained using equation M2, which drops state-specific time trends from equation M1. Panels E and F suggest that the estimates presented in Panels C and D, which are based on equation M1, are sensitive to dropping state-specific time trends. The sizes of the coefficients are much smaller in Panels E and F than in Panels C and D, and Panel F suggests no effect of the policy on heroin-related deaths.

A.3 Replication of Meinhofer (2018)

As discussed above, Figure A14 suggests that the estimates from my specification, as compared with those from Meinhofer's preferred specification, provide more compelling heroin results. However, one may have a concern about whether other factors drive these findings, such as legal coding, Ruhm (2018) correction, or data frequency. To address this concern, I perform an exercise similar to that in Appendix Figure A14 but using the replication of Meinhofer (2018). I first replicate Meinhofer's event study results for heroin-related deaths and then test how these estimates change if I use my specification (equation 1) instead, while keeping everything else unchanged.

Panel A of Appendix Figure A15 shows my replication of Meinhofer's event study results for heroin-related deaths, and Panel B presents the estimates that obtained when I use my baseline specification (equation 1) instead, while everything else, including the analysis sample from Panel A, remains unchanged. Although I cannot directly compare the estimates from my baseline model with those from Meinhofer's log-transformed model, the estimates from Panels A and B have similar trends in both the pre- and post-periods. However, the estimates in Panel B better address the pre-treatment differences between the treated and control groups, and the coefficients in the post-period are statistically more significant compared with those presented in

Panel A. These findings are consistent with those from Figure A14.[52] In summary, Appendix Figures A14 and A15 suggest that regardless of whether more recent data are included or not, my specification (equation 1) allows for clearer evidence of the spillover effect on heroin deaths than equation M1 and that the differences generated by the specification choices are more pronounced when I include additional years of data.

B Dropping Florida

In the 2000s, increasing numbers of pill mills caused a dramatic rise in the opioid supply in Florida. As a result, Florida was at the center of the nation's opioid epidemic in the late 2000s and was an extreme outlier both in levels of and trends in opioid supply (Meinhofer 2016). In response, Florida passed several laws in 2010 and 2011 that strictly regulated pain clinics. These aggressive regulations led to a huge drop in the opioid supply in Florida. Appendix Figure A17 displays the trends in each state's per capita legal supply of oxycodone. This figure shows that Florida is an outlier that experienced both a sharp increase and decrease in oxycodone supply within a decade. There was a large spike in Florida's per capita legal supply of oxycodone throughout the 2000s until the pill mill peak in 2010, and then the oxycodone supply began to decrease sharply as a result of aggressive regulations in 2010 and 2011.

I exclude Florida from my control group for all analyses in this study. The key assumption of my difference-in-differences model is that, in the absence of must-access PDMPs, the trends in the outcomes would have been the same across the treated and control groups. However, given that Florida experienced dramatic policy changes around the time of my treatment states' implementation of the must-access PDMPs, including Florida may potentially violate the parallel assumption. In fact, I find that the results for oxycodone doses per capita are sensitive to whether I include Florida in my control group or not, as shown in Appendix Figure A16. The first panel of the top row of Appendix Figure A16 presents the oxycodone estimates from my baseline specification (equation 1) obtained using a sample that includes my ten treatment states and all of the 35 states that did not implemented a must-access PDMP until 2016h2, including Florida. As illustrated in Appendix Figure A16, the estimates from this sample are sensitive to removing Florida. In any panels without Florida, I observe a sudden decrease in oxycodone supply

[52] Note that the sample used in Panels B and D of Figure A14 is balanced in relative (half-year) periods from -9 to +3, while the sample used in Figure A15 is unbalanced in relative (quarter) periods from -9 to +7 (the corresponding half-year event time window is -4.5/+3.5). Although the event time windows and the number of treated states included are different in the two figures, the results patterns in the overlapped relative periods are similar across the figures.

following policy implementation, and a negative trend in oxycodone supply is found in the entire post-period. However, once I include Florida in my analysis, all coefficients for the post-periods become close to zero. Although not reported, after dropping Florida, my oxycodone results are robust to removing one of the treated or control states. In contrast to the sensitivity of my oxycodone results to including Florida, my mortality results are robust to whether Florida is included, as shown in Section 6 (see columns 1 and 4 of Table 6 and those of Appendix Table A4).

C Must-Access PDMP in Ohio

In this section, I explore the policy effect in Ohio in particular and propose three possible explanations for the strong effect of Ohio's must-access PDMP, which relied on provider suspicion. In Section 6, I test the robustness of the baseline heroin mortality estimates to removing one treated state (see Appendix Table A3, the corresponding regression coefficients are presented in Panel A of Appendix Figure A11). As shown in Appendix Table A3, regardless of which treated state is dropped, the estimates are statistically significant and qualitatively similar to the baseline estimates. However, when I drop Ohio, the magnitudes of the heroin mortality estimates are slightly attenuated, although the coefficients for the two- and three-year effects remain statistically significant (see column 7 of Appendix Table A3).

In this section, I first explore the effect of must-access policy within Ohio and show that Ohio's policy had stronger effects on my outcomes than the policies in the other treated states. I then propose three possible explanations for the strong impact of Ohio's initial must-access PDMP on the heroin death rate—a sharp increase in PDMP utilization, the existence of a complementary law, and high accessibility of heroin. As mentioned in Section 5.6, Ohio implemented its initial must-access PDMP in 2011h2 and then strengthened its must-access law in 2015h2 (at event time +8). However, the strengthened law cannot explain why my heroin estimates are affected when I remove Ohio because the most distant post-period in my analysis is event time +6 (3 years after implementation) and Ohio strengthened its law at event time +8 (4 years after the initial implementation). Therefore, in this section, I focus on discussing why Ohio's initial must-access PDMP, which relied on provider suspicion, had stronger effects on the outcomes than the must-access policies in the other treated states. Throughout this section, I use the phrase *Ohio's initial must-access PDMP* synonymously with *Ohio's must-access PDMP*. Also, note that Ohio has the second-largest population among my ten treated states, and my regressions are weighted by

state population. The strong impact of Ohio's (initial) must-access policy and Ohio's large population explain why the coefficients become smaller when I remove Ohio.

C.1 Effects of Must-Access PDMP within Ohio

I first investigate how the must-access PDMP impacted the mortality outcomes within Ohio. In Appendix Figure A18, I present the estimates from the baseline specification (equation 1) that I obtain by dropping all the treated states except Ohio.[53] The estimates in blue show the impact of Ohio's must-access PDMP, and the point estimates in red are my baseline estimates, indicating the overall effects of the policy among the ten treated states, including Ohio. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed gray line indicates 2010h2, when the OxyContin reformulation was introduced (between event times -3 and -2).

Appendix Figure A18 suggests that Ohio's must-access PDMP had stronger impacts on most outcomes compared with the overall effects among the ten treated states. Following implementation of the must-access PDMP policy, heroin and illegal opioid mortality sharply increased (Panels A and B), and these increases coincided with sudden decreases in prescription opioid mortality (Panel C) and the legal supply of opioids (Panels E and F). Note that a sharp decrease in prescription opioid mortality at event time -5 is due to a temporary drop in Ohio's prescription opioid deaths in 2009h1. Overall, in Appendix Figure A18, I observe a clear and strong substitution pattern between legal and illegal opioid mortality following Ohio's policy implementation and I find no apparent pre-trends in the outcomes, evidence of that supports the parallel trends assumption. As mentioned above, Ohio has the second-largest population among my ten treated states, and my regressions are weighted by state population.[54] The strong impact of Ohio's must-access policy and Ohio's large population explain why my heroin mortality estimates become smaller when I remove Ohio. Below, I propose three possible explanations that may account for the strong effects of the must-access PDMP in Ohio.

[53] An alternative way to estimate the impact of Ohio's PDMP is to use the full sample and interact an indicator for Ohio with the full set of indicators for pre- and post-periods. However, the results I observe when limiting my treatment group to Ohio are similar to those I obtain when including the interactions.

[54] This is another reason why removing Ohio has a relatively larger effect on the estimates than dropping one of the other treated states. Although not reported in the revised paper, I find similar policy effects regardless of whether I weight observations by state population, but removing Ohio has a smaller effect on the estimates with the unweighted regressions.

C.2 PDMP Utilization

The first plausible explanation for the strong impact of Ohio's must-access PDMP is that Ohio's policy was associated with a dramatic increase in PDMP utilization. In 2011h2, Ohio enacted its initial must-access law, which required prescribers to review a patient's prescription history at the beginning of treatment and annually after that, if they had reason to believe that treatment with controlled substances in Schedules II–V would exceed 12 continuous weeks (Urahn 2016). Even though this initial must-access law primarily relied on provider suspicion, the utilization of the PDMP increased dramatically, from 911,000 reports requested in 2010, to 1.8 million in 2011, 5.4 million in 2012, 7.3 million in 2013, 10.8 million in 2014, and 16.5 million in 2015 (the 2016 Ohio Automated Rx Reporting System (OARRS) Annual Report[55]).

Other actions may also have increased PDMP participation in Ohio. In 2012h2, the state published guidelines for prescribing opioids in emergency departments, and in 2013h2 guidelines were established for the long-term prescription of opioids, both initiatives may have encouraged the utilization of the PDMP and reduced opioid prescriptions. For example, the Governor's Cabinet Opiate Action Team (2014) encouraged providers to "consider checking Ohio Automated Rx Reporting System (OARRS) for all patients who will receive an opiate," demonstrating that Ohio encouraged providers to use the PDMP in situations beyond those prompted by their suspicion.

In 2015h2, Ohio strengthened the must-access law by adding further requirements for the utilization of the PDMP. Based on the updated laws, prescribers must request a PDMP report on a patient under certain circumstances, even without provider suspicion. Following implementation of the updated mandate in 2015, PDMP utilization increased again, from 1.2 million queries in April to 1.4 million queries in September, reflecting a 17% increase (PDMP Center of Excellence 2016).

Transition from Ohio's initial must-access PDMP in 2011h2 to the updated program in 2015h2 occurred gradually, rather than a single implementation date marking a sudden increase in PDMP utilization (Urahn 2016). As a result of Ohio's consistent efforts, utilization of Ohio's PDMP system increased dramatically, and opioid prescriptions decreased sharply. Following the implementation of Ohio's initial must-access PDMP in 2011, the rate of individuals who see five or more prescribers and five or more pharmacies in a three month period to obtain controlled substances (commonly referred to as doctor shopping) decreased by over half, by the last quarter

[55] https://www.ohiopmp.gov/documents/Annual%20Report%20(2016).pdf (last accessed May 2020)

of 2013 (PDMP Center of Excellence 2014). According to the 2016 OARRS Annual Report (see footnote 61), for the period 2012 to 2016, the total doses of opioids dispensed to Ohio patients decreased by 162 million doses (or 20.4%), while the number of opioid prescriptions issued to Ohio patients decreased by 2.5 million (or 20%); during that same period, the state experienced a 78.2% decrease in the number of individuals who see multiple prescribers to obtain controlled substances illicitly. Ohio is one of the potential models for states looking to mandate PDMP use (PDMP Center of Excellence 2016). The dramatic increase in PDMP utilization, which resulted from implementing the initial must-access PDMP, publishing guidelines on opioid prescriptions, and a sharp decrease in opioid prescriptions as a result of increasing PDMP utilization, can explain why Ohio's mandate had dramatic effects on heroin and other opioid mortality even if it relied on provider suspicion.

C.3 Complementary Law—Pill Mill Law

Another initiative that may account for the strong effects of the must-access PDMP in Ohio is the enactment of a complementary law. Around the time of policy implementation of the must-access PDMP, three states (Kentucky, Ohio, and Tennessee) also enacted pill mill laws, which impose strict regulations on pain clinics to prevent them from issuing opioid prescriptions without medical indication. Buchmueller and Carey (2018) view pain clinic laws as complements to must-access laws, as they target a slightly different channel of misuse than PDMP policies. Must-access laws target a large fraction of providers, while pain clinic laws directly regulate the behavior of the small share of providers, who prescribe high volumes of opioids without medical indication. Ohio and Kentucky, which implemented the must-access PDMP a year after implementing the pill mill law, experienced large decreases in opioid prescriptions following the mandate (PDMP Center of Excellence 2016). Appendix Figure A19 presents the effect of the must-access PDMP within each state by plotting the estimates from the baseline specification (equation 1) that I obtain by dropping all the treated states except one. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed red line indicates 2010h2, when the OxyContin reformulation was introduced. Appendix Figure A19 suggests that for most outcomes, must-access PDMPs had larger impacts in Ohio and Kentucky, compared with the policies in other treated states. The pill mill law, considered a complementary law to must-access policies, may have contributed to the strong impact of Ohio's must-access PDMP.

However, this also raises a concern that pill mill laws may not be complements to must-access laws but a confounder that drives variation in my mortality outcomes. To address this concern, I test whether pain clinic laws have an independent effect when passed in the absence of a must-access law, following the approach used by Buchmueller and Carey (2018). If pill mill laws alone have little or no impact on my outcomes, they are not likely to drive my results. In Appendix Figure A20, I test for an independent effect of pain clinic laws using data on the 35 states, that did not implement a must-access PDMP until 2016h2. Among these 35 states, Florida, Mississippi, and Texas implemented pain clinic regulations between 2009h1 and 2011h2. I consider the event time window -12/+6, during which these three states are consistently observed. The distant relative periods outside this event time window are trimmed. For this test, I control for a full set of indicators for pre- and post-periods around the enactment of pill mill laws, the full set of state- and time-varying covariates that I use in the baseline analysis, the ARCOS measure of OxyContin use interacted with the time fixed effects, an indicator for whether the state had a voluntary-access PDMP, and the fixed effects for state and half-year. Appendix Figure A20 suggests that, in the absence of a must-access law, pill mill laws in these three states had no effect on all the outcomes except oxycodone supply. The negative effects on oxycodone supply observed in Panel E are primarily driven by Florida, which experienced a dramatic change in its oxycodone supply (see Appendix Section B); I find no effect of pill mill laws on oxycodone supply if I drop Florida. I conclude that this policy is not likely to affect mortality outcomes when providers are not also required to access the PDMP database. My findings are consistent with Brighthaupt et al. (2019), who find pill mill laws had no effect on prescription opioid, heroin, or synthetic opioid overdose deaths in Ohio and Tennessee. It is possible that a pill mill law alone has no substantial effects, but it may work to strengthen the effects of the must-access PDMP if implemented together.

C.4 Accessibility of Heroin

Finally, high accessibility of heroin in Ohio is also likely to contribute to the strong association between must-access PDMP and heroin mortality. The nation's major heroin routes, I-70 and I-75, pass through Ohio, allowing users easy access to heroin and illegal fentanyl. It is not surprising that a supply-side drug policy has a stronger spillover effects in the area where people can easily find affordable substitutes.

D Additional Heterogeneity Analysis

In Appendix Figure A19, I investigate the effect of the must-access PDMP within each treated state by plotting the estimates from the baseline specification (equation 1) that I obtain by dropping all the treated states except one. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed red line indicates 2010h2, when the OxyContin reformulation was introduced. Although the estimates are noisy because the treated sample includes one state only, the results presented in Appendix Figure A19 allows for a better understanding of the differential effects of must-access policies across states.

E Additional Robustness Analysis

E.1 Consequences of Excluding Six Treated States

To estimate the policies' medium-run effects, I focus on the treated states that were consistently observed during an event time window that runs from -15 to +6. As a result, among the 16 states that implemented a must-access PDMP during my sample period, the six states that were not observed at some point during the -15/+6 window are excluded.[56] To address the consequences of excluding these six states, I test how my estimates change when I include more treated states in the analysis sample. Note that I construct the sample so that the treated states are balanced in relative periods, so the event time window decreases as the number of treated states included increases. Appendix Figure A4 presents the coefficients obtained when I estimate the baseline specification (equation 1) on three different samples. The dashed red line presents my baseline estimates, obtained using the sample that includes the ten treated states that are consistently observed during the -15/+6 event time window. The short-dashed blue line corresponds to the 12 treated states that are consistently observed during the -15/+4 event time window, and the black solid line corresponds to the 15 treated states that are consistently observed during the -15/+2 window.[57]

[56] My sample period is from 2003h1 to 2016h2, and in this period, the 16 states implemented must-access PDMPs from 2007h2 to 2016h2.

[57] Although there are the 16 states that implemented must-access PDMPs until 2016h2, the sample with the -15/+2 window has 15 states. This is because Nevada, which implemented the must-access PDMP for the first time in the nation, only has the nine pre-periods that are observed during my sample period. Although not reported, including Nevada in my analysis sample by limiting the number of pre-periods to nine does not change my main results. I prefer to include more pre-periods by dropping Nevada because I can observe the trends in heroin mortality for a longer period while not affecting the estimates significantly.

In Appendix Figure A4, the lines connecting the estimates from each of these three samples closely track one another for all outcomes. Given that the estimated short-term effects are similar across the three samples, I prefer to use the sample that allows me to look at the longer-term effects, which is my analysis sample. By looking at the longer-term effects, I can better understand how heroin mortality and other mortality outcomes change over time and compare how the longer-run impacts differ from the short-run impacts. In Appendix Table A1, I also report the summary effect of must-access laws among all the 16 treatment states, which I obtain by replacing the full set of indicators for pre- and post-periods with a single indicator for the entire post-period.

E.2 Analysis with the Post-Reformulation Data Period

Appendix Figure A12 presents the estimates from my baseline specification (equation 1) obtained when I drop the pre-reformulation time period and reconstruct the sample so that the treated sample is balanced in related periods. I consider an event time window that runs from -2 to +6, during which the nine treated states were consistently observed. The distant periods outside the -2/+6 are trimmed. As presented in Appendix Figure A12, I see that estimates are very similar to my baseline estimates. In addition, the consequences of controlling for the reformulation are similar to those observed in my baseline analysis: although not reported, if I drop the controls for the reformulation (the interaction of the measure of pre-reformulation OxyContin use and the time fixed effects) from the regressions, the estimated effects on heroin mortality and illegal opioid mortality become larger, which is consistent with my findings from the baseline analysis (see Figure 4). In summary, Appendix Figure A12 suggests that my estimates are robust to dropping the pre-reformulation period and that accounting for the reformulation is important, not only for addressing pre-trends but also for obtaining more accurate estimates.

E.3 Synthetic Control Analysis

As described in Section 5, I find that must-access PDMPs have increased the heroin death rate and that this increase coincided with a sudden decrease in prescription opioid mortality. These findings implicitly assume that any pre-treatment differences between the groups can be explained by my econometric model (equation 1). However, a concern that some of the unaccounted for pre-period differences between the two groups may be responsible for my

results motivates me to conduct a synthetic control analysis as a robustness analysis. I construct a comparable synthetic control state for each treated state based on pre-period data in such a way that the synthetic control state outcome trends are similar to those of the treated state prior to policy implementation. If the baseline results are comparable to those from the synthetic control analysis, my results are not likely to be driven by unaccounted for pre-treatment differences between the groups.

For each of my ten treated states, I construct a synthetic control state from the 34 control states that never implemented a must-access policy,[58] matching on the value of the outcome variable in each of the 15 pre-treatment periods.[59,60] Each synthetic control state is composed of a weighted average of observations from the subset of the 34 control states. A set of synthetic controls are constructed for each of the following outcomes: heroin mortality, prescription opioid mortality, total opioid-related mortality, and oxycodone doses per capita. Table A5 shows the makeup of the synthetic states for each outcome.

Using observations from the treated and synthetic control groups, I create a sample so that the treated and synthetic control samples are strongly balanced in relative periods, from -15 to +6. Using this sample, I calculate the outcome gap between each treated state and its synthetic control state for each event time. Appendix Figure A21 plots how the (unweighted) average of these gaps changes over time. In addition, Appendix Figure A22 depicts the outcome trends for the treated and synthetic control groups separately. The solid black line reflects how the (unweighted) average outcomes change over time in the treated states, and the dashed red line reflects the trends for the control group.

As shown in Appendix Figures A21 and A22, my synthetic control analysis suggests a larger policy effect size, but the results pattern is very similar to that observed in my main analysis (see Figure 3). Although I still observe upward pre-trends in prescription opioid mortality and oxycodone consumption,[61] the sudden decreases in these outcomes in the first post-period provide suggestive evidence for the substitution.

[58] Florida is excluded in the analysis (see Appendix Section B).

[59] A synthetic control analysis has been more widely conducted for a single treated unit or multiple units with the same treatment timing, but recent papers extend this method for the case of multiple units with differential timing of treatment (e.g., Kleven (2019); Acemoglu et al. (2016)).

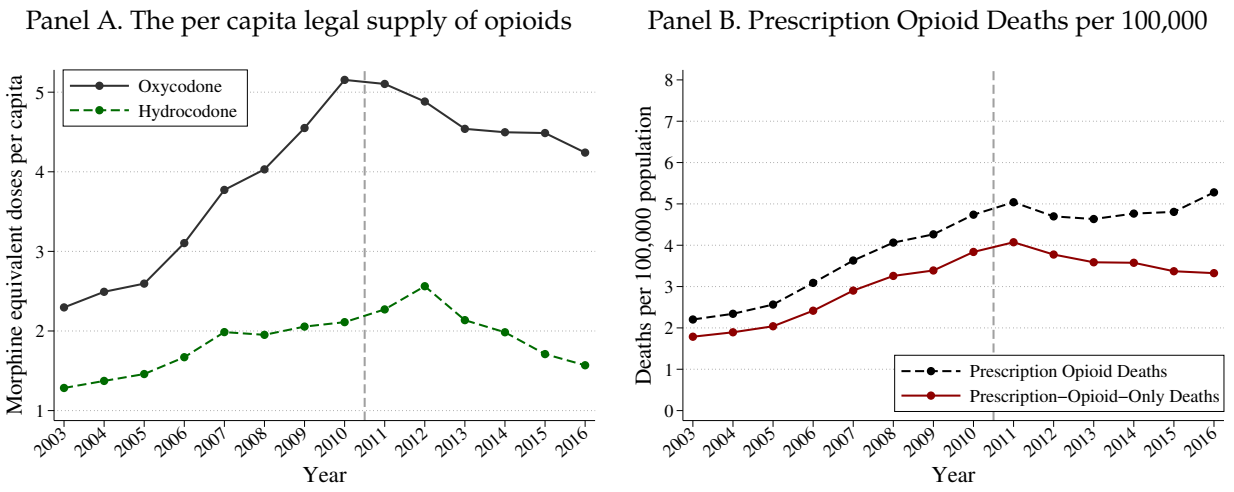
[60] I use the Stata command `synth` to construct synthetic controls. See https://fmwww.bc.edu/RePEc/bocode/s/synth.html for a description of the `synth` command.

[61] The reason for this is that a few treated states experienced a sharp increase in prescription opioid mortality and in the legal supply of oxycodone in the pre-period.

E.4 Alternative Measures of Pre-Reformulation OxyContin Use

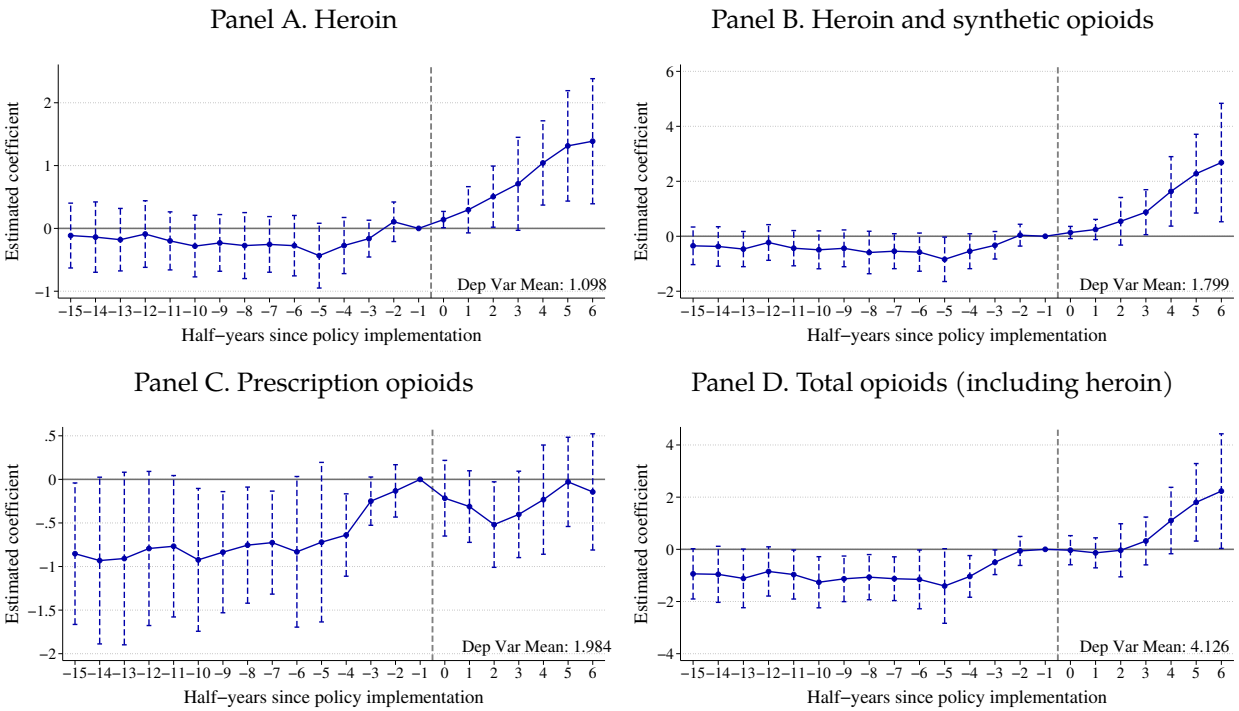
Appendix Figure A13 displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I use two alternative measures of pre-reformulation OxyContin use (separate regressions): Panel A uses oxycodone / hydrocodone in (morphine equivalent) doses per capita, and Panel B uses the Google Trend measure suggested by Beheshti (2019). My heroin mortality results are robust to using each of these alternative measures.

Figure A1: National Trends in the Legal Supply of Opioids and Prescription Opioid Death Rates



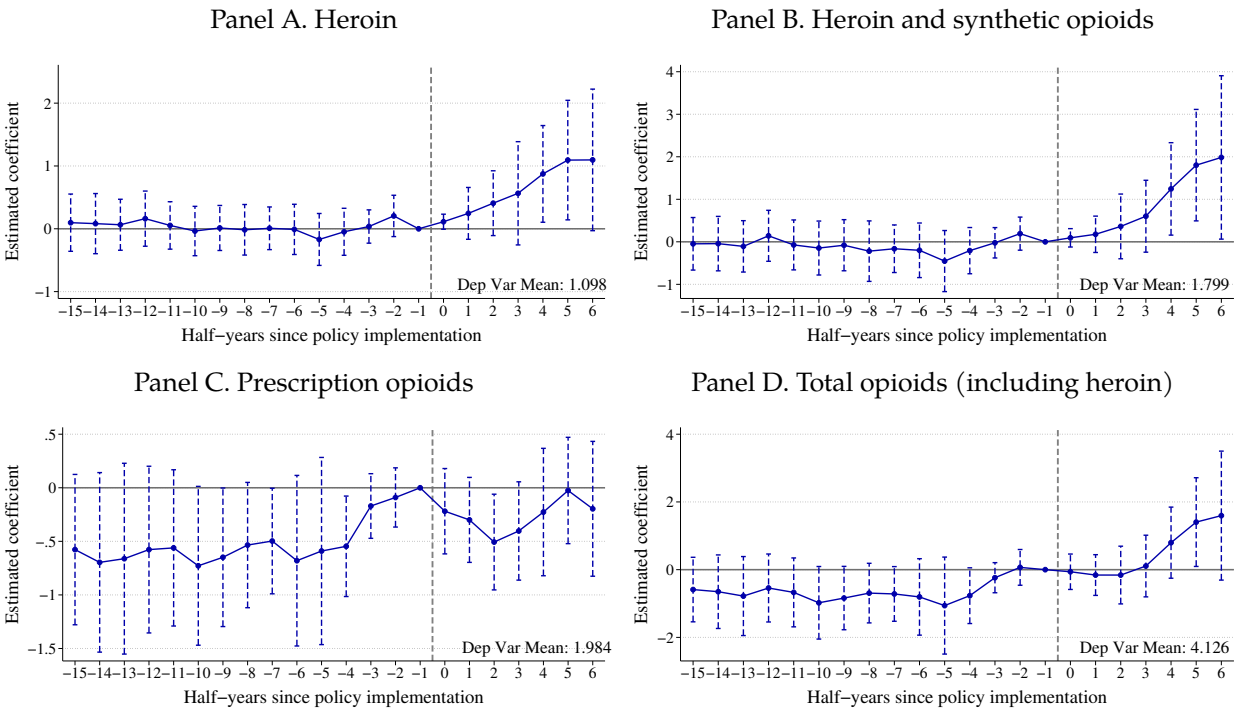
Notes: The figure plots the national trends in the per capita legal supply of opioids (Panel A) and Ruhm-corrected numbers of deaths from prescription opioids per 100,000 population (Panel B). The legal supply of oxycodone and hydrocodone in morphine equivalent doses obtained from the DEA's Automation of Reports and Consolidated Orders System (ARCOS). Ruhm-corrected numbers of deaths per 100,000 population are calculated using data from the National Vital Statistics System (NVSS). Drug overdose deaths are coded using ICD-10 underlying cause of death codes: X40–X44, X60–X64, X85, and Y10–Y14. Prescription opioid mortality rates, which use ICD-10 drug code T40.2, are identical to those in Figure 1. Prescription-opioid-only deaths indicate the deaths involved T40.2 but not T40.1, T40.3 or T40.4 at the time of death.

Figure A2: Baseline Results without the Controls for the Reformulation



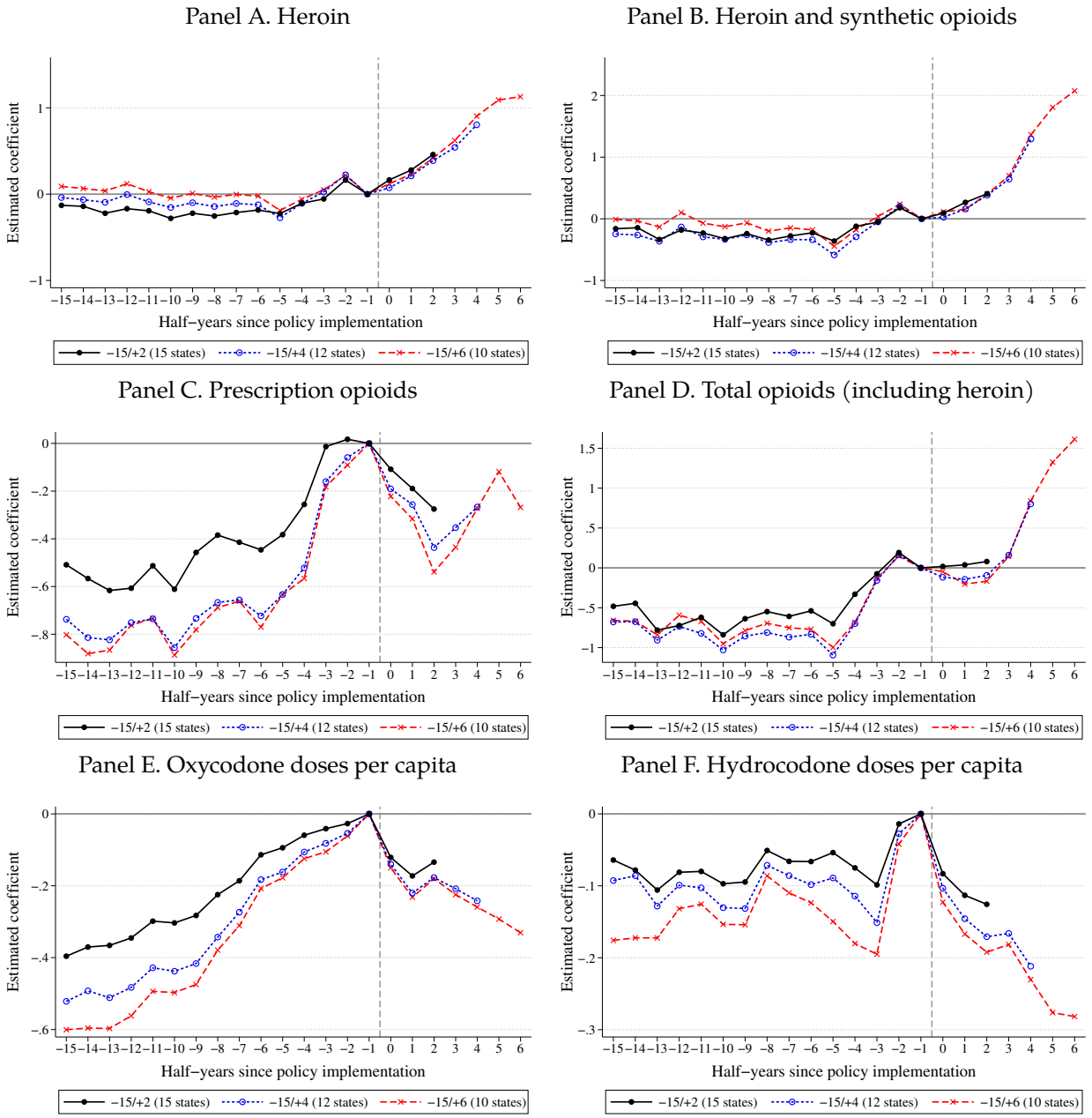
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1) without the controls for the reformulation. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4) in Panel B, prescription opioid deaths per 100,000 (drug code T40.2) in Panel C, and total deaths from any opioid, including heroin (drug codes T40.1–T40.4) in Panel D.

Figure A3: Baseline Results with the NSDUH Measure of OxyContin misuse



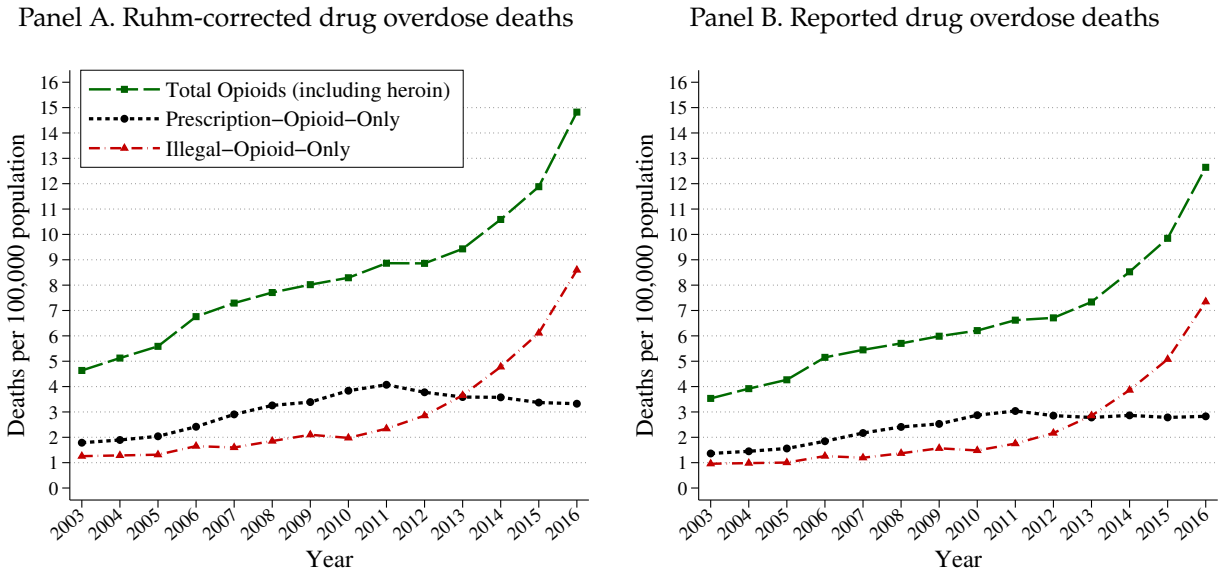
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1). In all panels, the NSDUH measure, instead of the ARCOS measure, is interacted with time fixed effects to account for exposure to the reformulation. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4) in Panel B, prescription opioid deaths per 100,000 (drug code T40.2) in Panel C, and total deaths from any opioid, including heroin (drug codes T40.1–T40.4) in Panel D.

Figure A4: Baseline Results with Different Event Time Windows



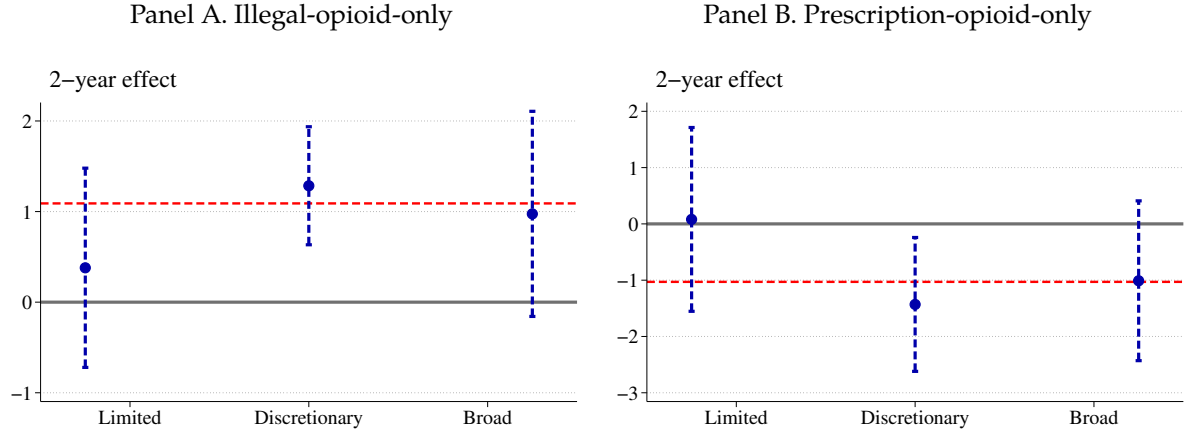
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1) on three samples with different event time windows (separate regressions). Each sample includes my baseline control group and treated states that were consistently observed during one of the following event time windows: -15/+6, -15/+4, or -15/+2. The dashed red line presents the baseline estimates, obtained using the sample that includes the 10 treated states that are consistently observed during the broadest event time window (-15/+6). The short-dashed blue line corresponds to the 12 treated states that are observed during the -15/+4 event time window. The black solid line corresponds to the 15 treated states that are observed during the narrowest event time window (-15/+2). The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Florida is excluded from the control sample in all panels (see Appendix Section B). The controls are identical to those in Figure 3.

Figure A5: National Trends in the Exclusive Measures of Opioid Death Rates



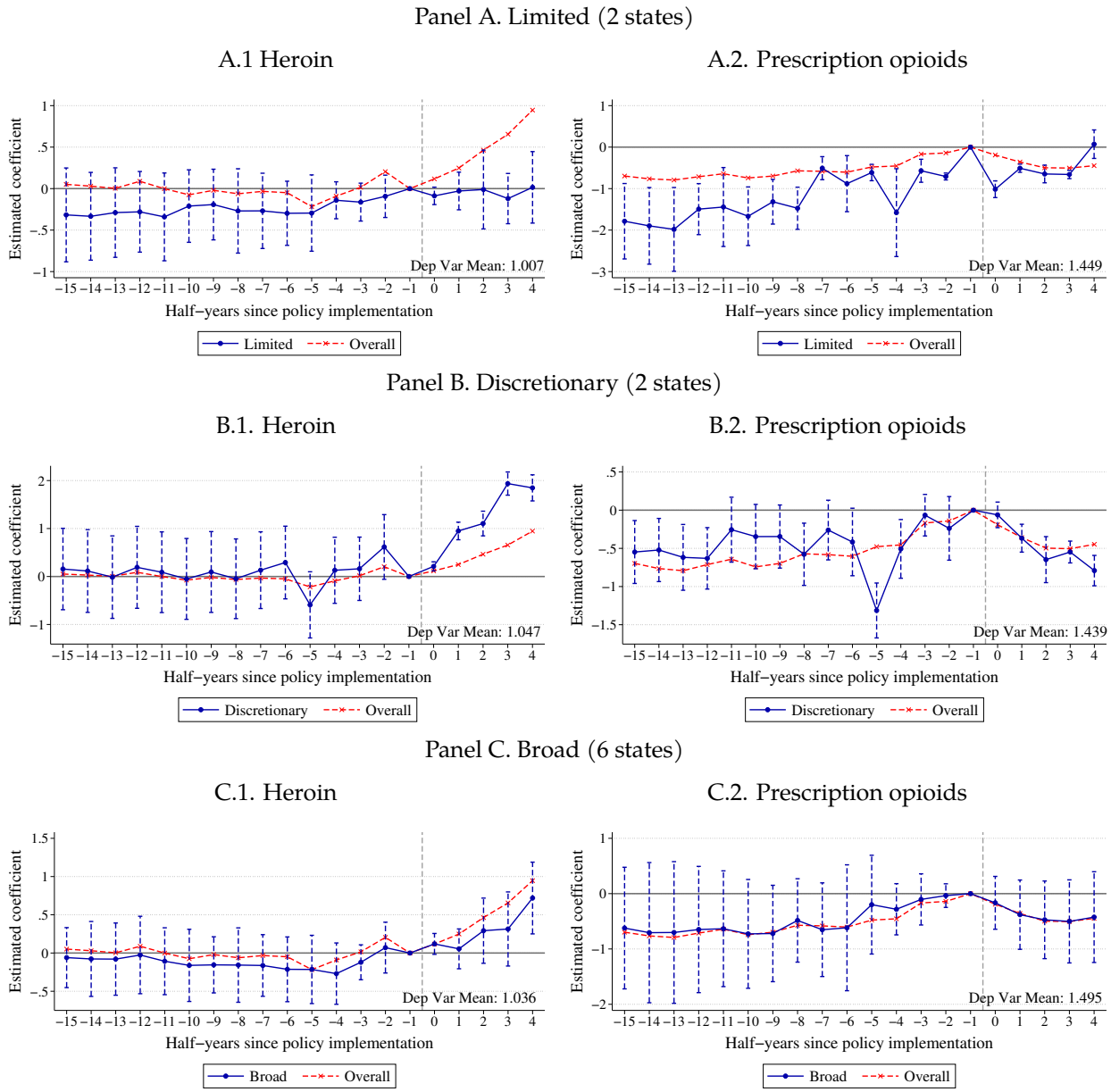
Notes: The figure plots the national trends in corrected and reported numbers of deaths per 100,000 population calculated using mortality data from the National Vital Statistics System (NVSS). Drug overdose deaths are coded using ICD-10 underlying cause of death codes: X40–X44, X60–X64, X85, and Y10–Y14. To identify drug involvement, the following four drug identification codes are used: heroin (T40.1), natural and semisynthetic opioids such as oxycodone and hydrocodone (T40.2), methadone (T40.3), and synthetic opioids excluding methadone, such as fentanyl (T40.4). I calculate total deaths from any opioid, including heroin, by combining T40.1–T40.4. Prescription-opioid-only deaths indicate the deaths involved T40.2 but not T40.1, T40.3, or T40.4 at the time of death. Illegal-opioid-only deaths indicate the deaths involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death. Reported mortality rates are based on mentions of the specified drugs on the death certificates. Corrected mortality rates are estimated by using the method suggested by Ruhm (2018), which uses information from death certificates that specified at least one drug category to impute drug involvement for cases in which none was specified.

Figure A6: Heterogeneous Treatment Effects on Exclusive Mortality Outcomes



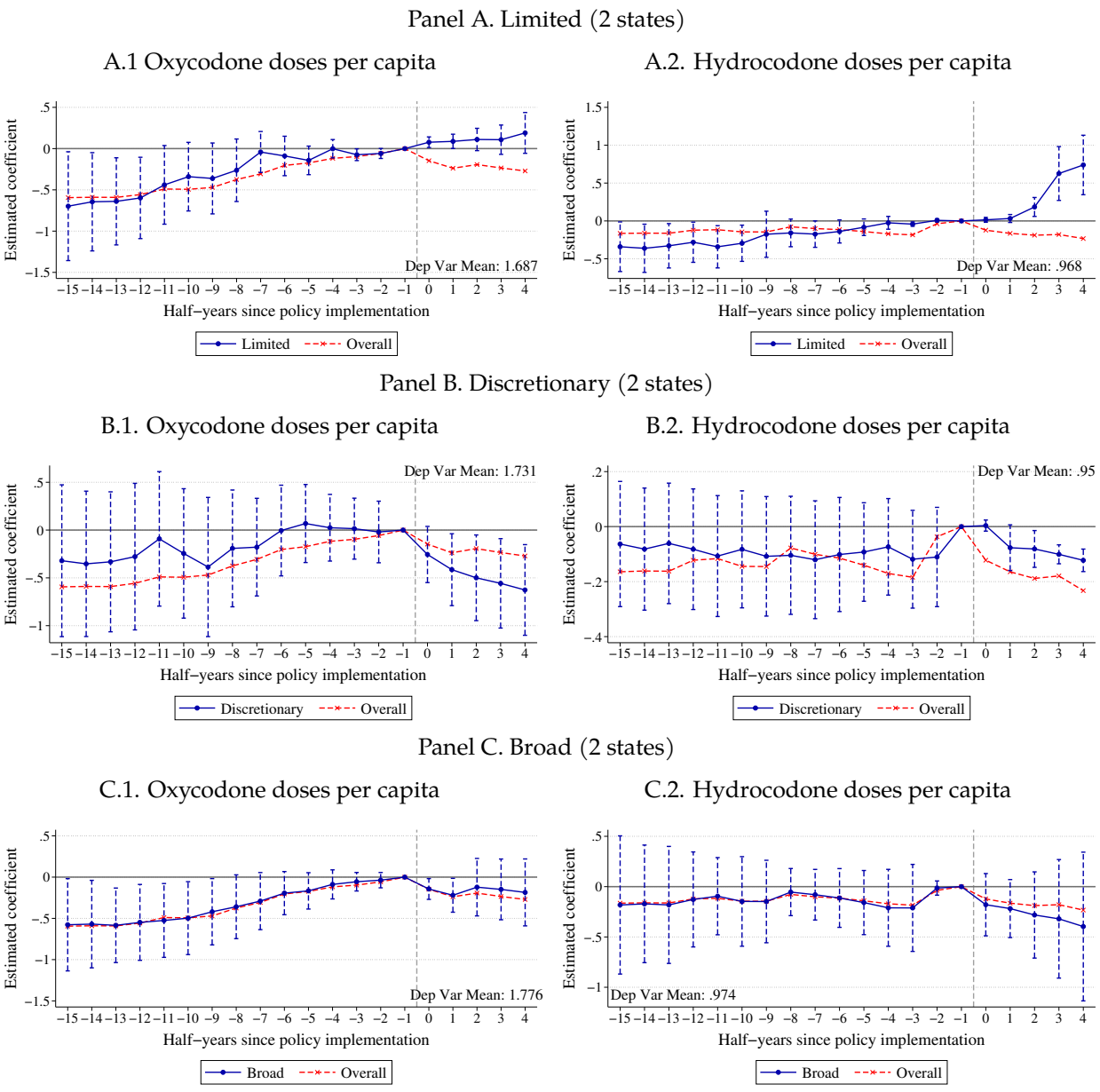
Notes: The figure shows the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). Each panel presents the estimates I obtain when I interact the indicators for all the post-periods (from 0 to +6) from the baseline specification (equation 1) with three indicators for limited, discretionary and broad laws. The (horizontal) dashed red line presents the overall estimate, for reference. In Panel A, the dependent variable is illegal-opioid-only deaths per 100,000, which involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death. In Panel B, the dependent variable is prescription-opioid-only deaths, which involved T40.2 but not T40.1, T40.3, or T40.4. Ruhm-corrected numbers of deaths are used in all panels. In all panels, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. For all the outcomes, I report the trend break estimates summarizing the two-year effect ($\Delta 5 = (\beta_4 * \mathbf{1}(\text{Law Type}) - \beta_{-1}) - (\beta_{-1} - \beta_{-6})$). I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Observations are weighted by state population. In all panels, the analysis sample and controls are identical to those in Figure 3. Fixed effects for state and half-year are always included.

Figure A7: Separate Event Studies by Law Type—Opioid Deaths



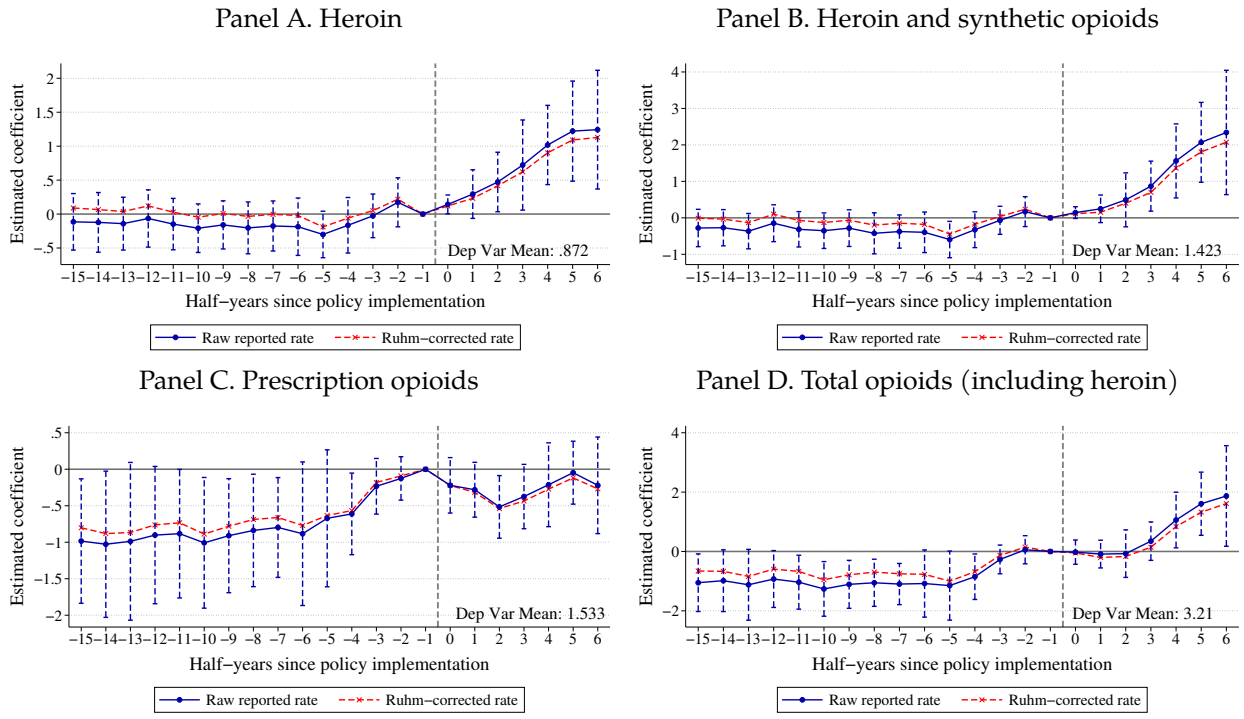
Notes: The figure presents the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I limit the treated group to each of the three law type. The treated sample is balanced in relative periods from -15 to +4, and the distant relative periods outside the -15/+4 event time window are trimmed. The dashed red line indicates the overall effects of must-access PDMPs among the ten treated states. In all panels, the control sample is the baseline control sample. In the left column (Panels A.1, B.1, and C.1), the dependent variable is heroin deaths per 100,000 (drug code T40.1). In the right column (Panels A.2, B.2, and C.2), the dependent variable is prescription opioid deaths per 100,000 (drug code T40.2). In all panels, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The controls are identical to those in Figure 3.

Figure A8: Separate Event Studies by Law Type—Opioid Supply



Notes: The figure presents the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I limit the treated group to each of the three law type. The treated sample is balanced in relative periods from -15 to +4, and the distant relative periods outside the -15/+4 event time window are trimmed. The dashed red line indicates the overall effects of must-access PDMPs among the ten treated states. In all panels, the control sample is the baseline control sample. In the left column (Panels A.1, B.1, and C.1), the dependent variable is oxycodone (morphine equivalent) doses per capita. In the right column (Panels A.2, B.2, and C.2), the dependent variable is and hydrocodone (morphine equivalent) doses per capita. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The controls are identical to those in Figure 3.

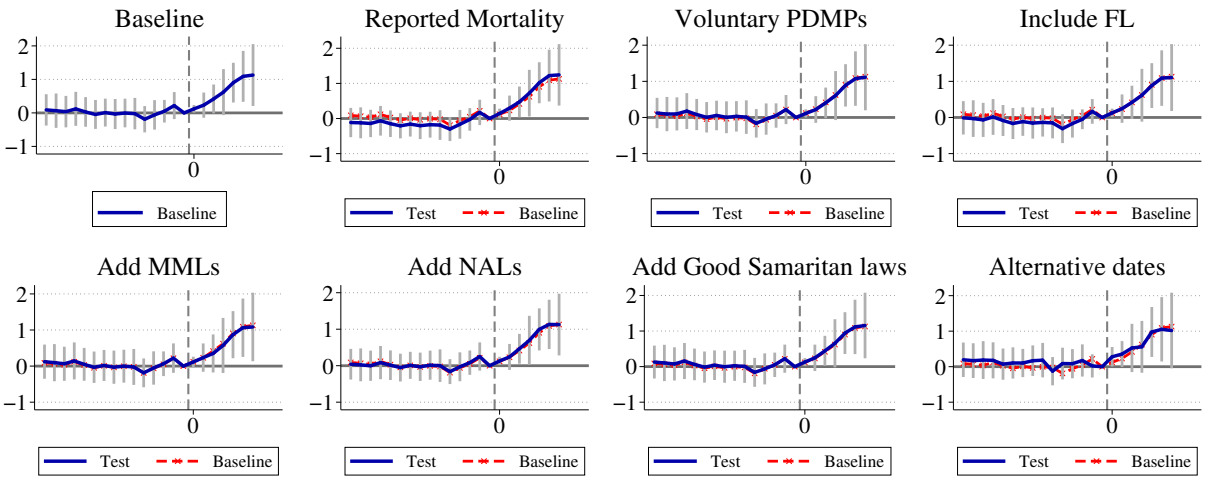
Figure A9: Effects of Must-Access PDMPs on Raw Reported Opioid Death Rates



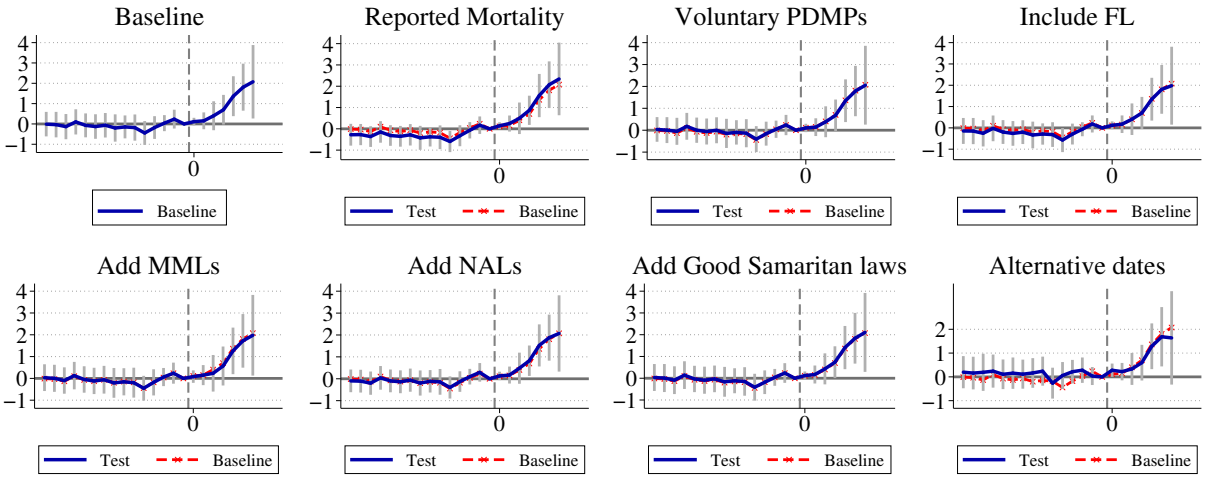
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) that I obtain when I use the raw reported death rates instead of the Ruhm-corrected death rates. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. In Panel A, the dependent variable is heroin deaths per 100,000 (drug code T40.1). In Panel B, the dependent variable is the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4). In Panel C, the dependent variable is prescription opioid deaths per 100,000 (drug code T40.2). In Panel D, the dependent variable is total deaths from any opioid, including heroin (drug codes T40.1–T40.4). The raw reported numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used in all panels. Observations are weighted by state population. The sample and controls are identical to those in Figure 3.

Figure A10: Robustness of the Baseline Mortality Estimates to Alternative Explanations

Panel A. Heroin

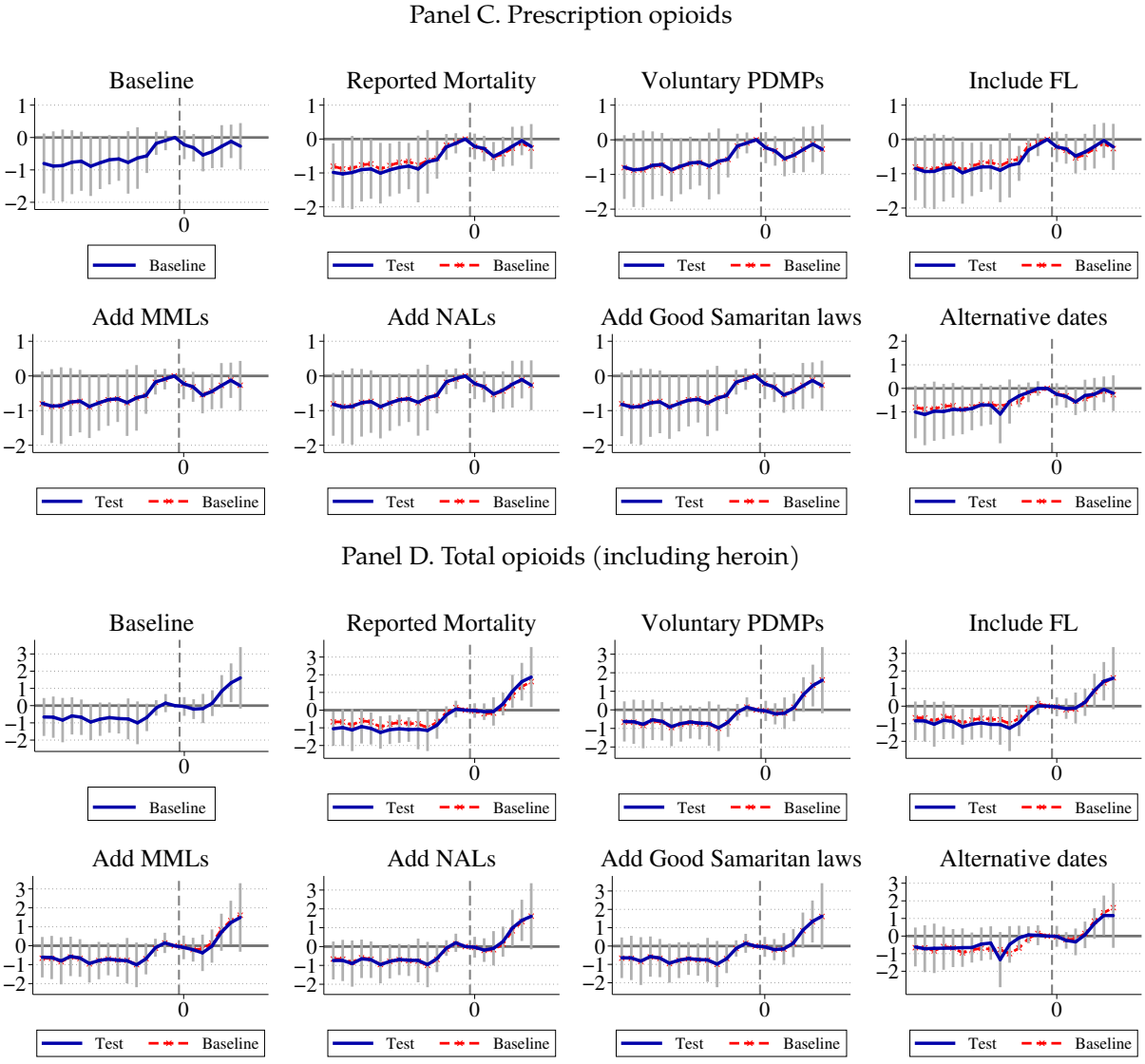


Panel B. Heroin and synthetic opioids



(continued)

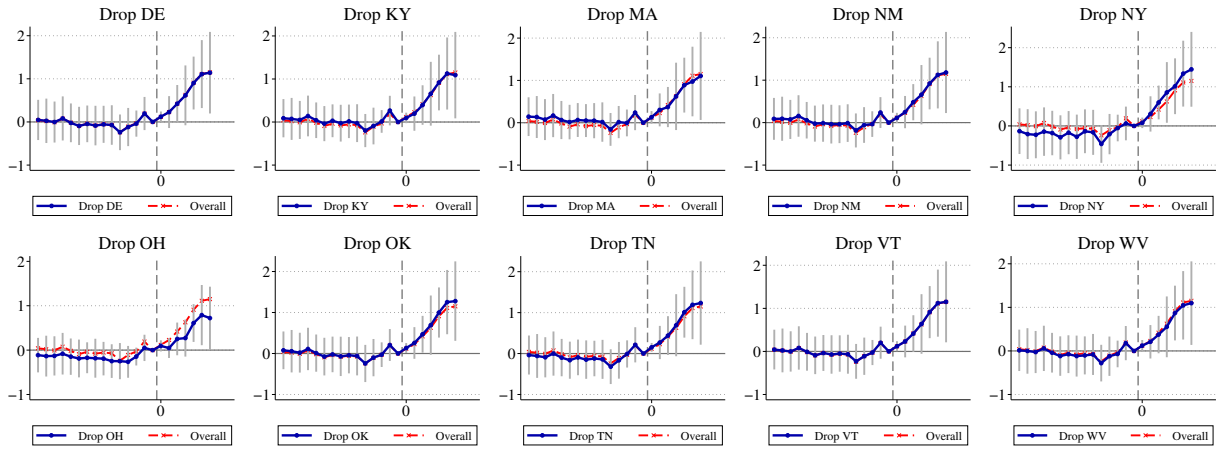
Figure A10: Robustness of the Mortality Estimates to Alternative Explanations (continued)



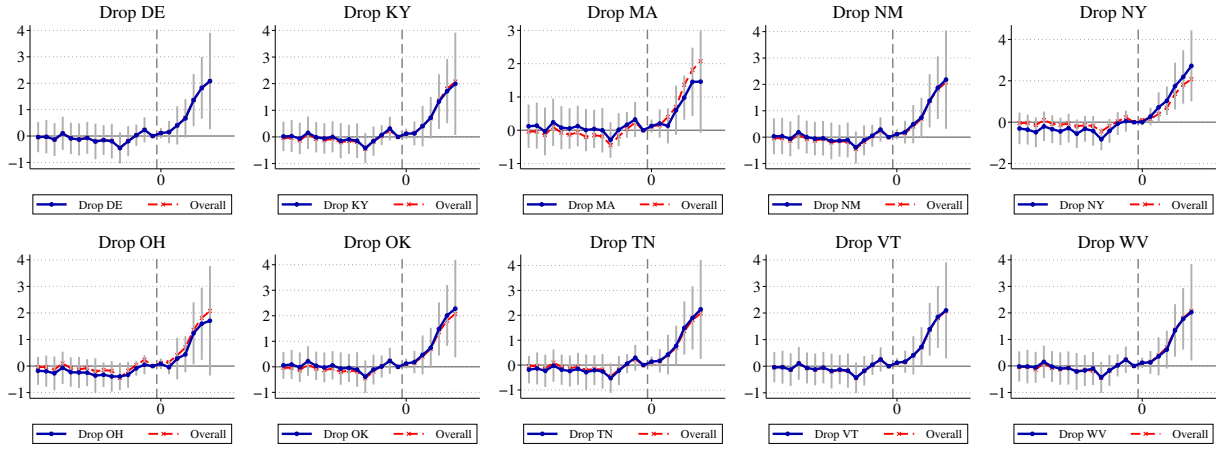
Notes: The figure shows the robustness of the baseline estimates to several sensitivity tests. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4) in Panel B, prescription opioid deaths per 100,000 (drug code T40.2) in Panel C, and total deaths from any opioid, including heroin (drug codes T40.1–T40.4) in Panel D. The estimates in Panel A are identical to those presented in Table 6. The estimates in Panels B, C, and D are identical to those presented in Appendix Table A4.

Figure A11: Robustness of the Mortality Estimates to Dropping One Treated State

Panel A. Heroin



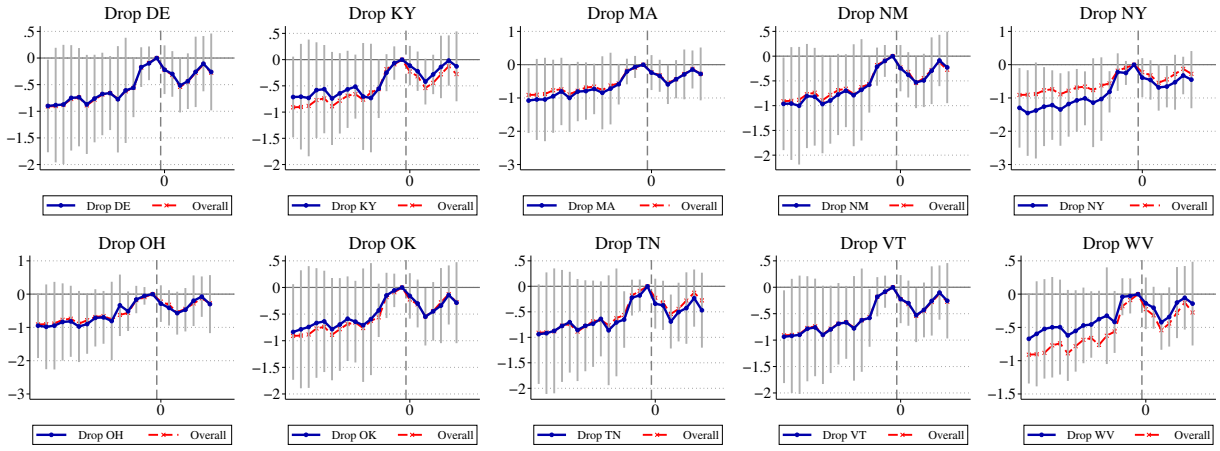
Panel B. Heroin and synthetic opioids



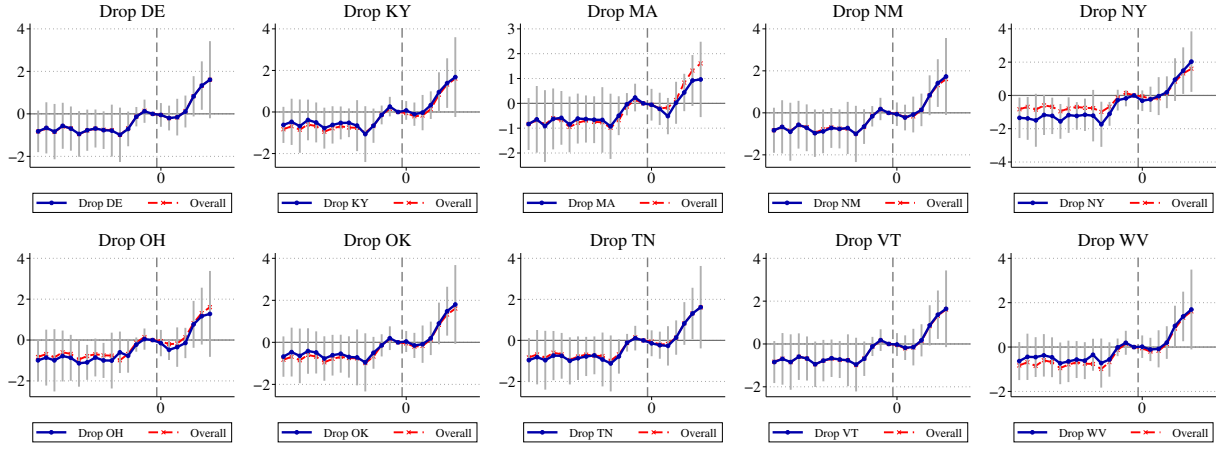
(continued)

Figure A11: Robustness of the Mortality Estimates to Dropping One Treated State (continued)

Panel C. Prescription opioids

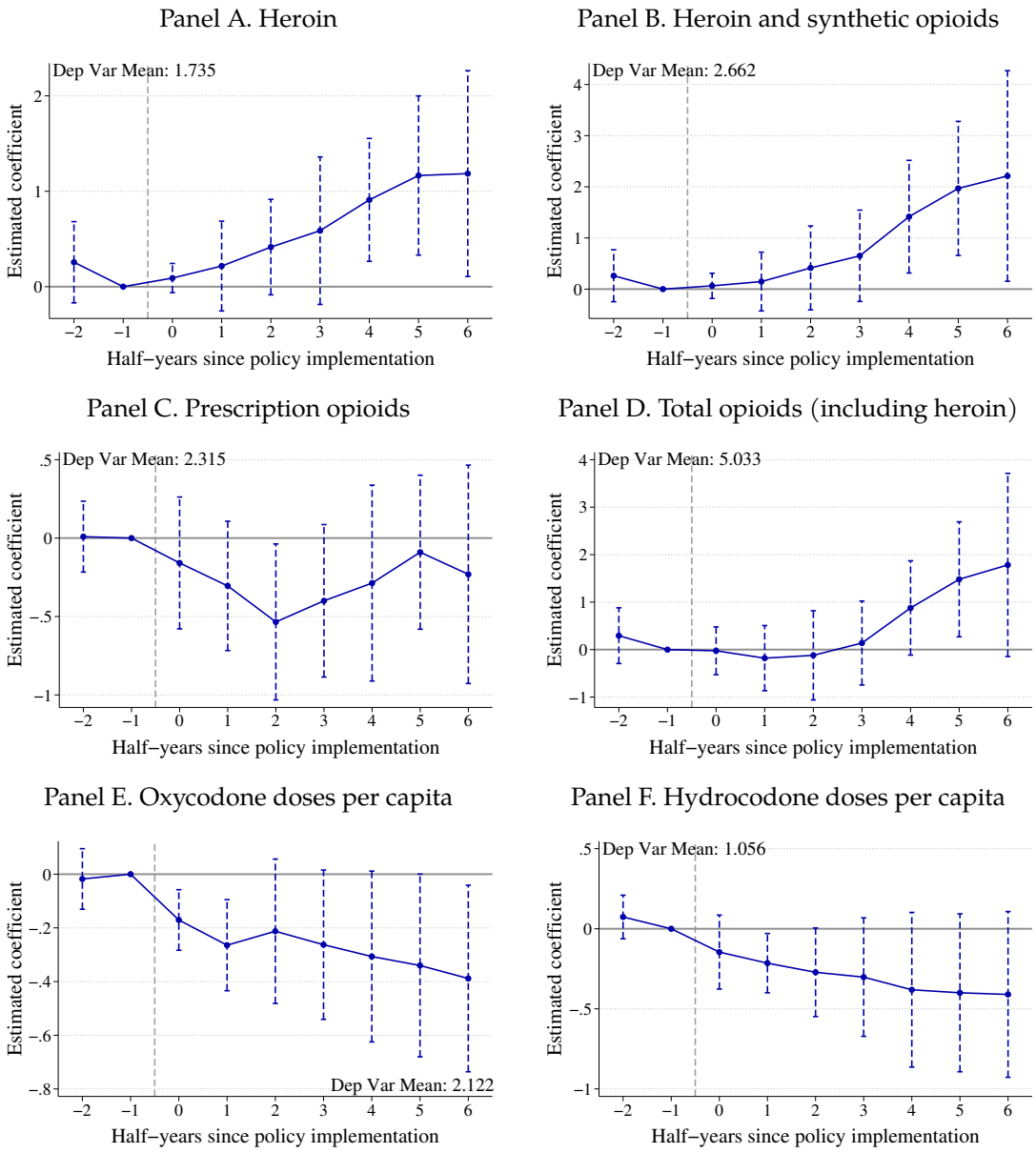


Panel D. Total opioids(including heroin)



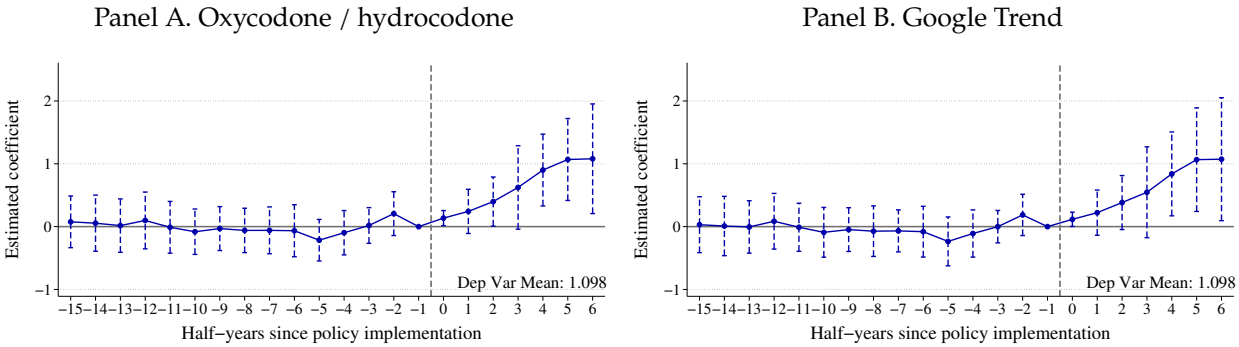
Notes: The figure shows the sensitivity of the mortality estimates to dropping one of the ten treated states. Each panel displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I drop one of the ten treated states. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The vertical dashed gray line indicates the implementation timing of a must-access PDMP. The dashed red line presents the baseline estimates indicating the overall effects among all the ten treated states. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, and total opioid-related death rate (T40.1–T40.4) in Panel D. Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used in all panels. The control states are the 34 that did not implement must-access policies until 2016h. Florida is excluded from the control sample (see Appendix Section B). Observations are weighted by state population. In all panels, the controls are identical to those in Figure 3.

Figure A12: Robustness of the Baseline Estimates to Dropping the Pre-Reformulation Period



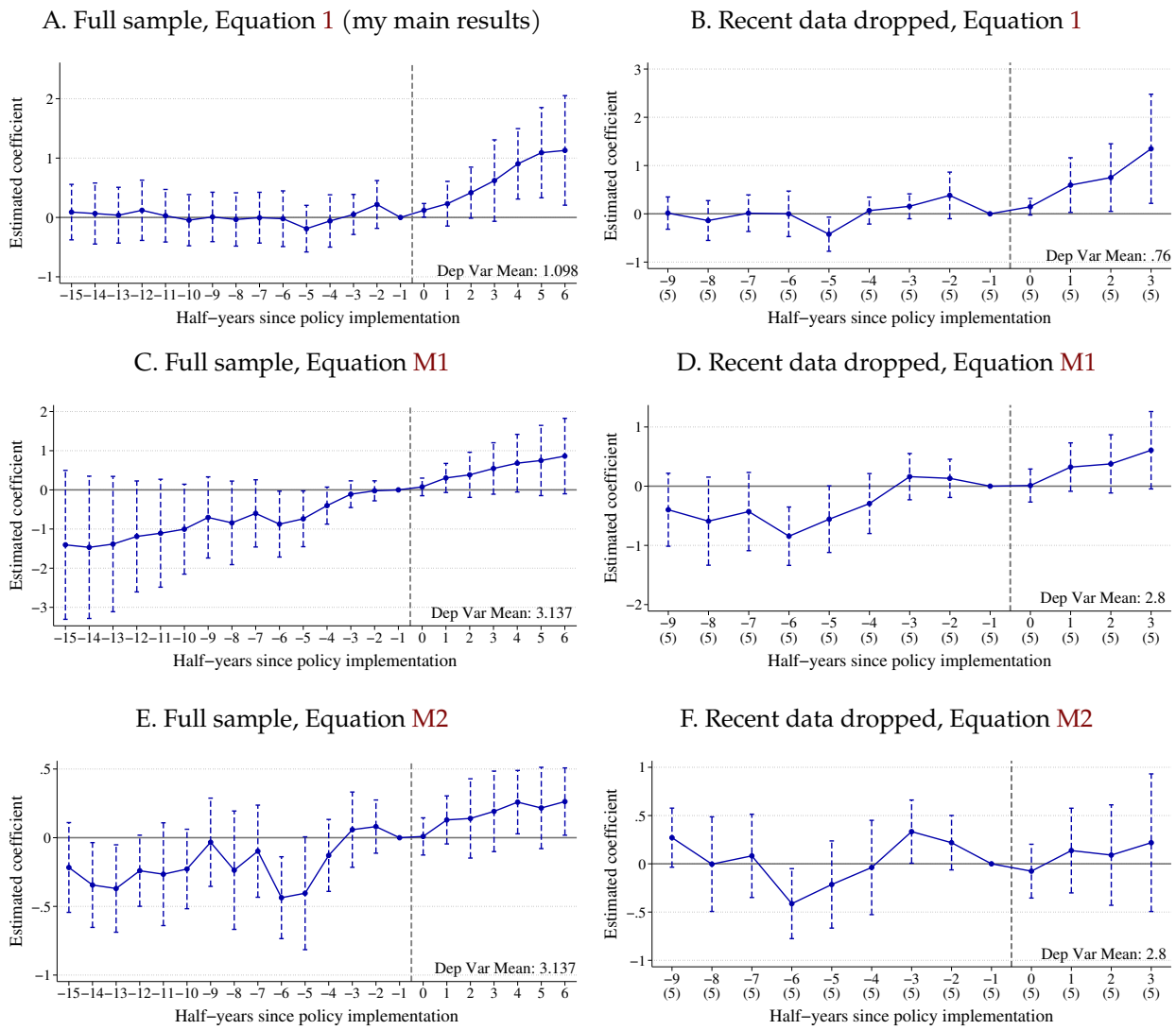
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1) obtained when I drop the pre-reformulation period (the reformulation was introduced in 2010h2). The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. In Panels A–D, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treatment states are the nine that implemented must-access PDMPs from 2011h2 to 2013h2, and the treated sample is balanced in relative periods from -2 to +6. The distant relative periods outside the -2/+6 event time window are trimmed. The control states are the 34 that did not implement must-access policies until 2016h2, and the control sample is balanced from 2010h2 to 2016h2. Florida is dropped (see Appendix Section B). Observations are weighted by state population. The controls are identical to those in Figure 3.

Figure A13: Alternative Measures of Pre-Reformulation OxyContin Use



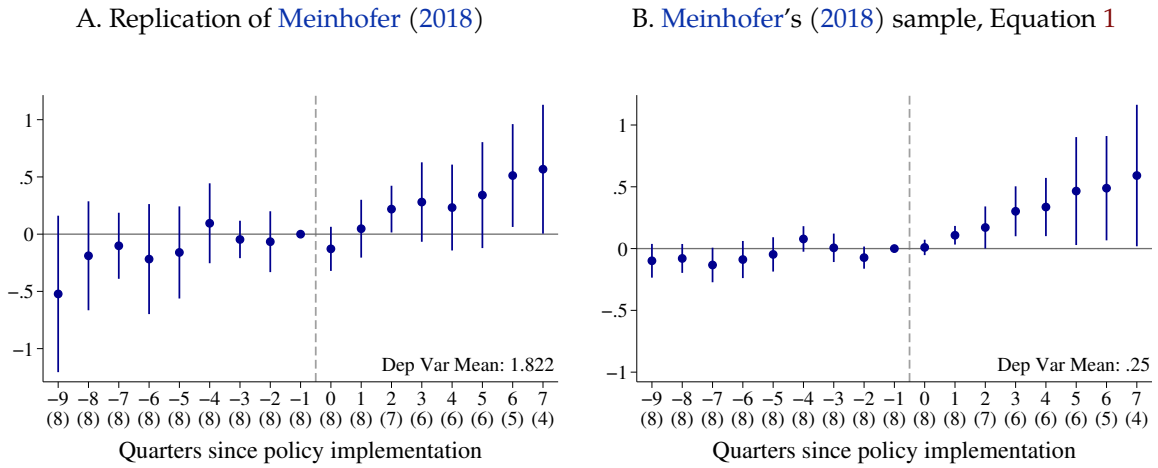
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I use two alternative measures of pre-reformulation OxyContin use (separate regressions): Panel A uses oxycodone/hydrocodone in morphine equivalent doses per capita, and Panel B uses the Google Trend measure obtained from Beheshti (2019). The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The dependent variable is heroin deaths per 100,000 (drug code T40.1). Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The controls are identical to those in Figure 3.

Figure A14: My Analysis Sample with Prior Literature Specification



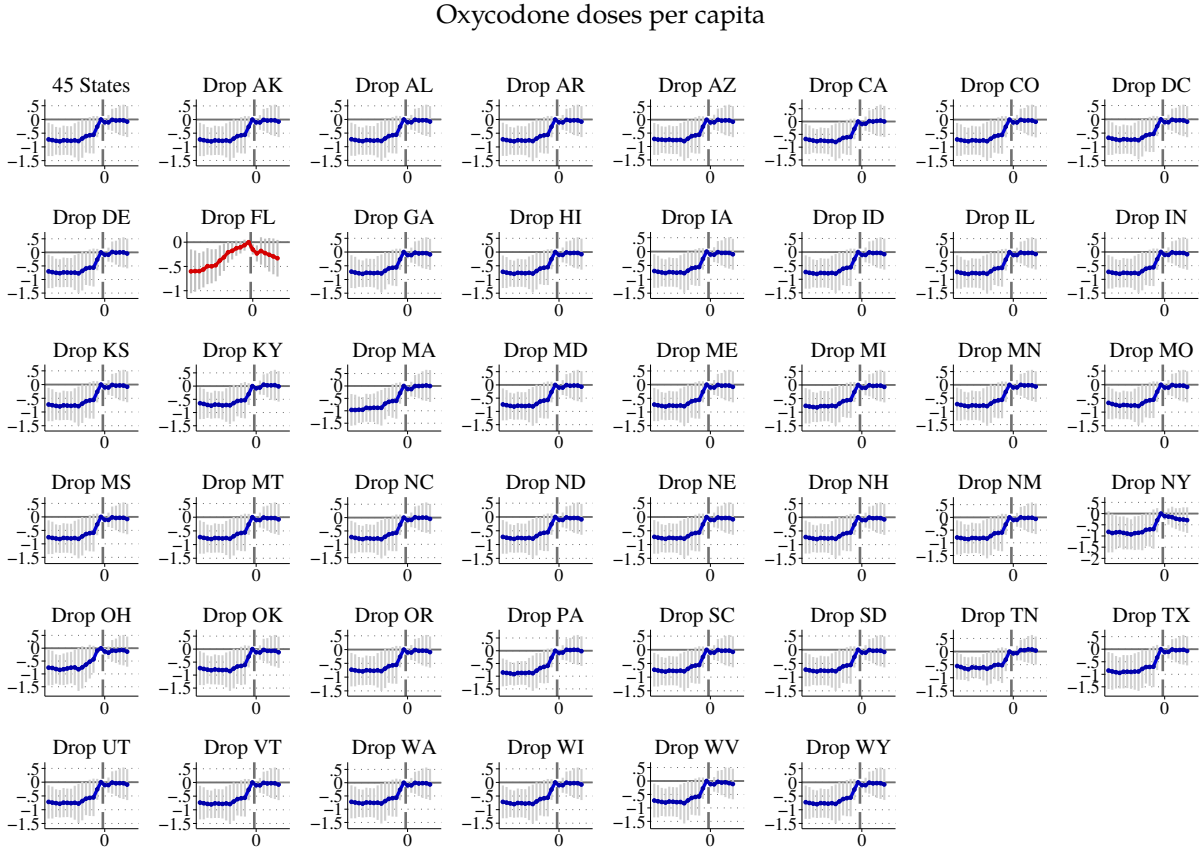
Notes: Each column corresponds to different sample periods. In the left column (Panels A, C, and E), I use my full sample (2003h1–2016h2), and in the right column (Panels B, D, and F), I drop the 2014h1–2016h2 period and reconstruct the sample so that the treated states are balanced in relative periods from -9 to +3. The sample from the right column includes the five treated states. The number of treated states observed in each time period is presented in the parentheses below that period. In all panels, the treated sample is balanced in relative periods, and the distant relative periods outside the given event time window are trimmed. Each row uses one of the three specifications: the top row (Panels A and B) uses my baseline specification (equation 1), the middle (Panels C and D) uses Meinhofer's preferred event study specification (equation M1), and the bottom (panels E and F) uses Meinhofer's alternative specification (equation M2). The regressions estimating my specification (equation 1) are weighted by population, while the regressions estimating Meinhofer's (equations M1 and M2) are unweighted.

Figure A15: Meinhofer's (2018) Analysis Sample with My Specification



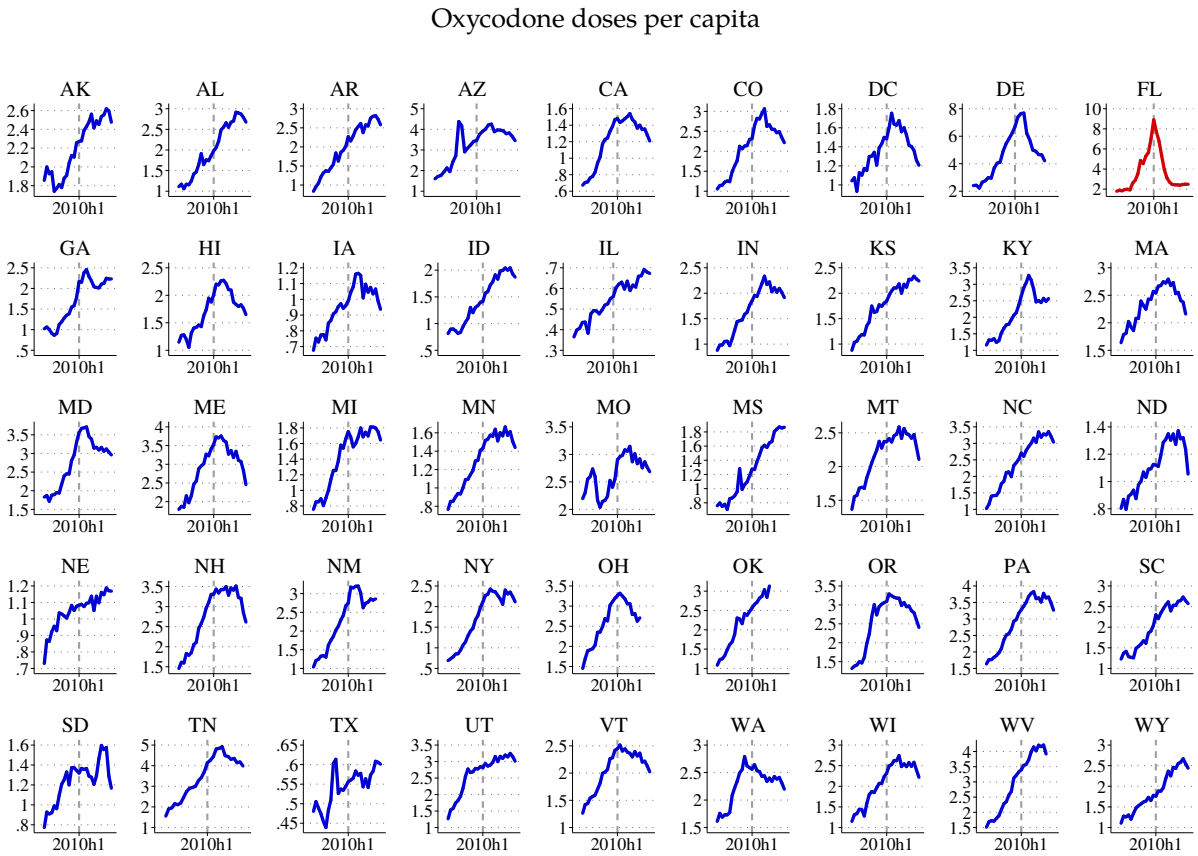
Notes: This is Appendix Figure A15 in the revised paper. The figure shows how Meinhofer's (2018) heroin results are affected if I use my specification instead. Panel A displays the replication of Meinhofer's (2018) event study results for heroin mortality, and Panel B presents the estimates that I obtain when I use my baseline specification (equation 1) instead, while everything else, including the analysis sample from Panel A, remains unchanged. The regression estimating my specification (equation 1) is weighted by population, while the regression estimating Meinhofer's (equations M1) is unweighted. The sample from Panel A is unbalanced in relative (quarter) periods from -9 to +7. The number of treated states observed in each event time period is presented in the parentheses below that period. The distant relative periods that are outside the -9/+7 event time window are dropped. The control sample is balanced from 2000q1 to 2013q4.

Figure A16: Sensitivity of Oxycodone Results to Dropping Florida



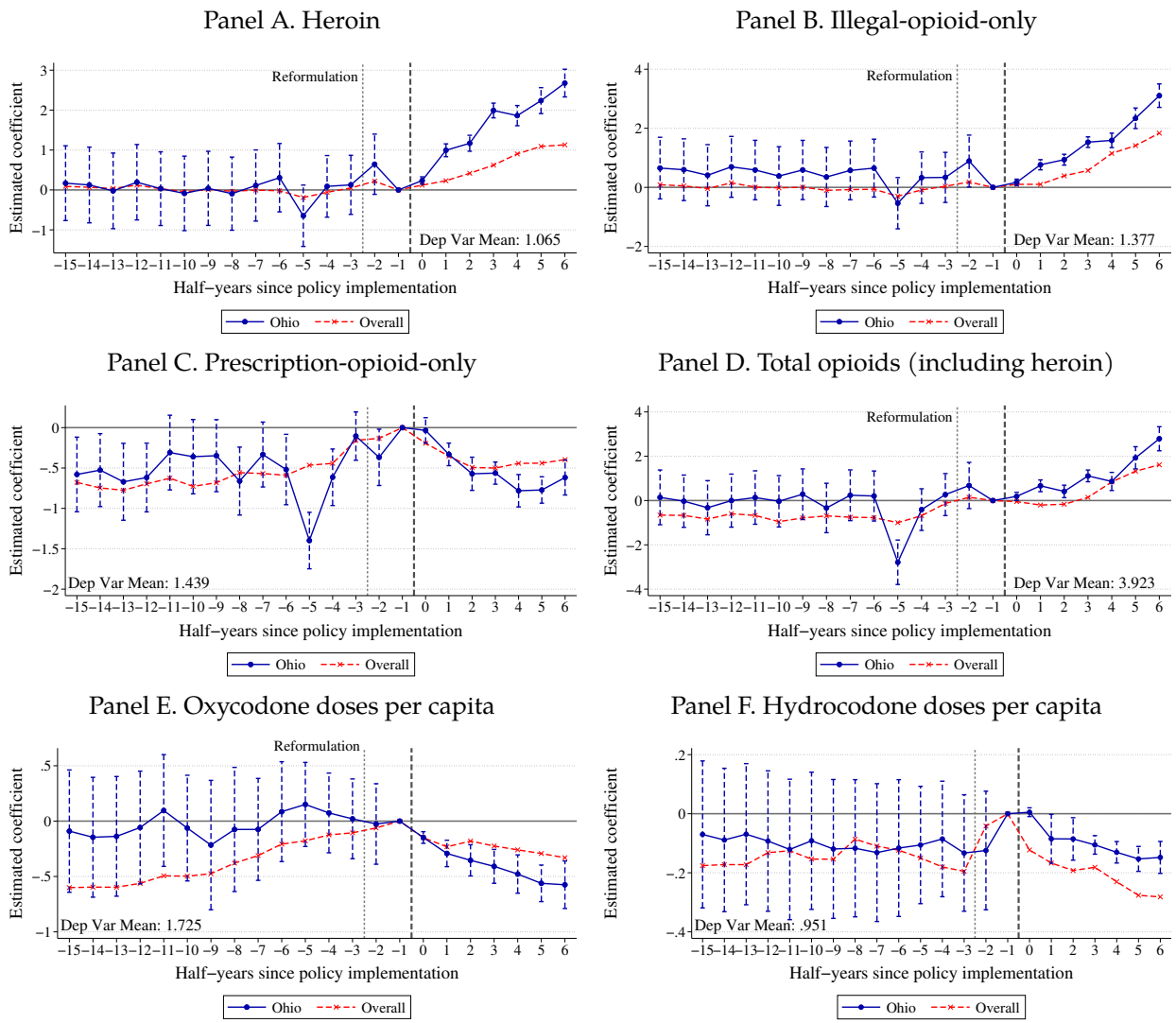
Notes: The top left panel displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I include Florida in my analysis sample. The subsequent panels show the sensitivity of the top left panel's estimates to removing one of the 45 states. In the sample from the top left panel, the treated states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 35 that did not implement must-access policies until 2016h2. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The dependent variable is oxycodone (morphine equivalent) doses per capita.

Figure A17: Trends in Per Capita Legal Supply of Oxycodone by State



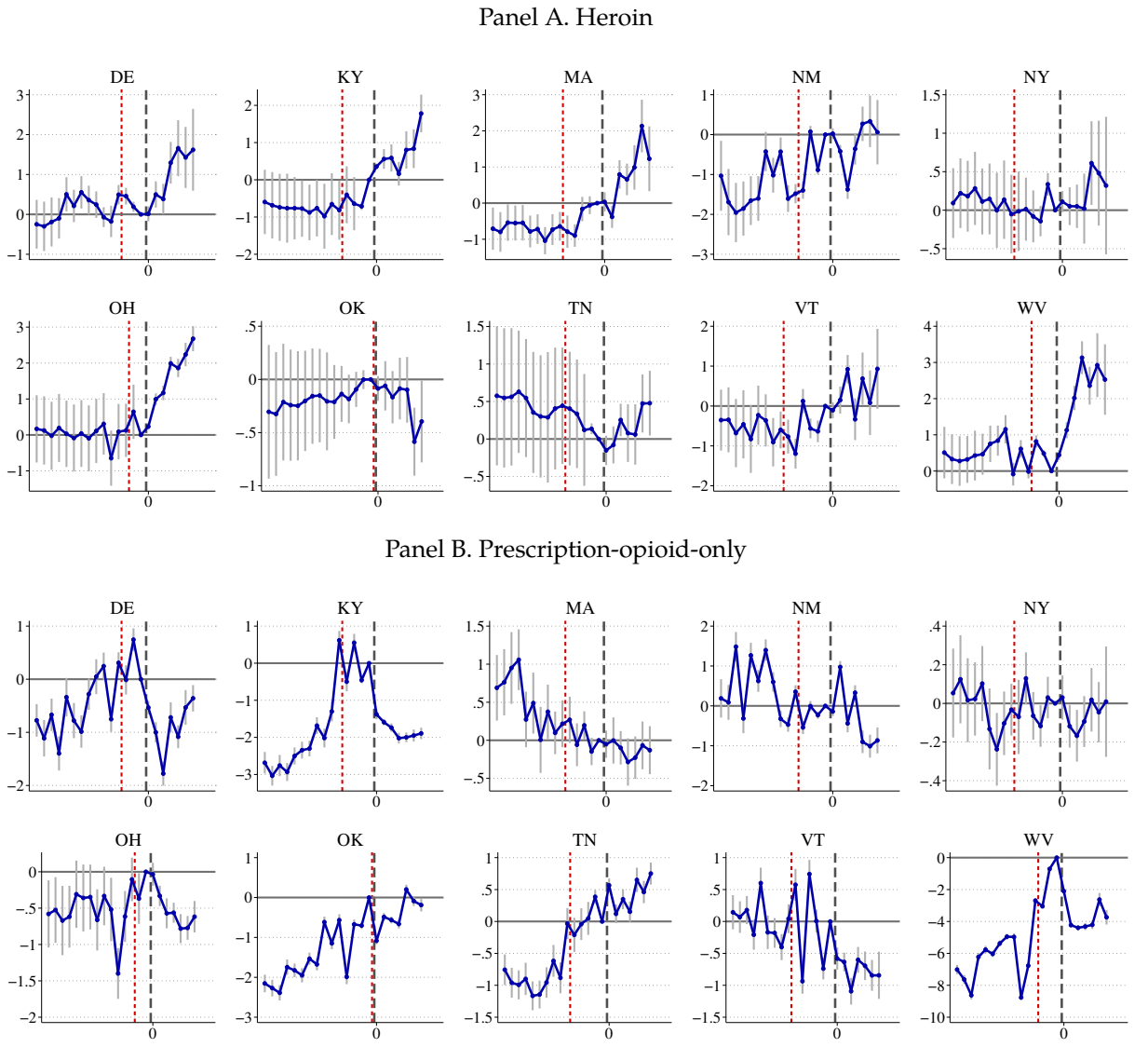
Notes: Each panel displays the trends in a state's legal supply of oxycodone (morphine equivalent) doses per capita in the half-year period. The dashed gray line indicates 2010h1.

Figure A18: Effects of the Must-Access PDMP within Ohio



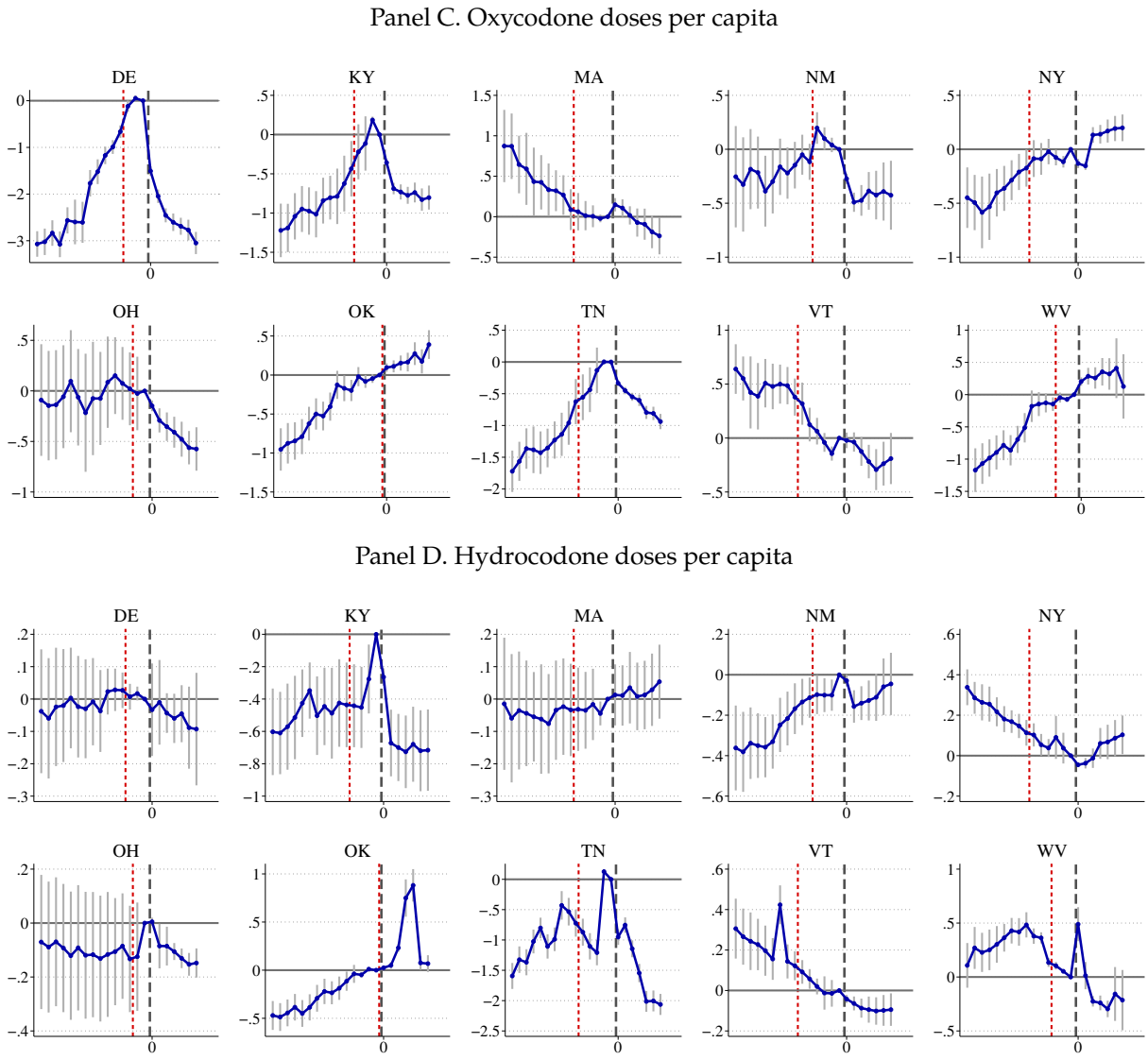
Notes: The figure presents the effect of the must-access PDMP within Ohio. The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I drop all the treated states except for Ohio. The last pre-period is omitted. The dashed red line indicates the overall effects of must-access PDMPs among the ten treated states. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed gray line indicates 2010h2, when the OxyContin reformulation was introduced (between event times -3 and -2). In all panels, the control sample is the baseline control sample. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal-opioid-only deaths per 100,000, which involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death in Panel B, prescription-opioid-only deaths, which involved T40.2 but not T40.1, T40.3, or T40.4 in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. Observations are weighted by state population. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The controls are identical to those in Figure 3. Fixed effects for state and half-year are always included.

Figure A19: Effects of the Must-Access PDMP within a Single State



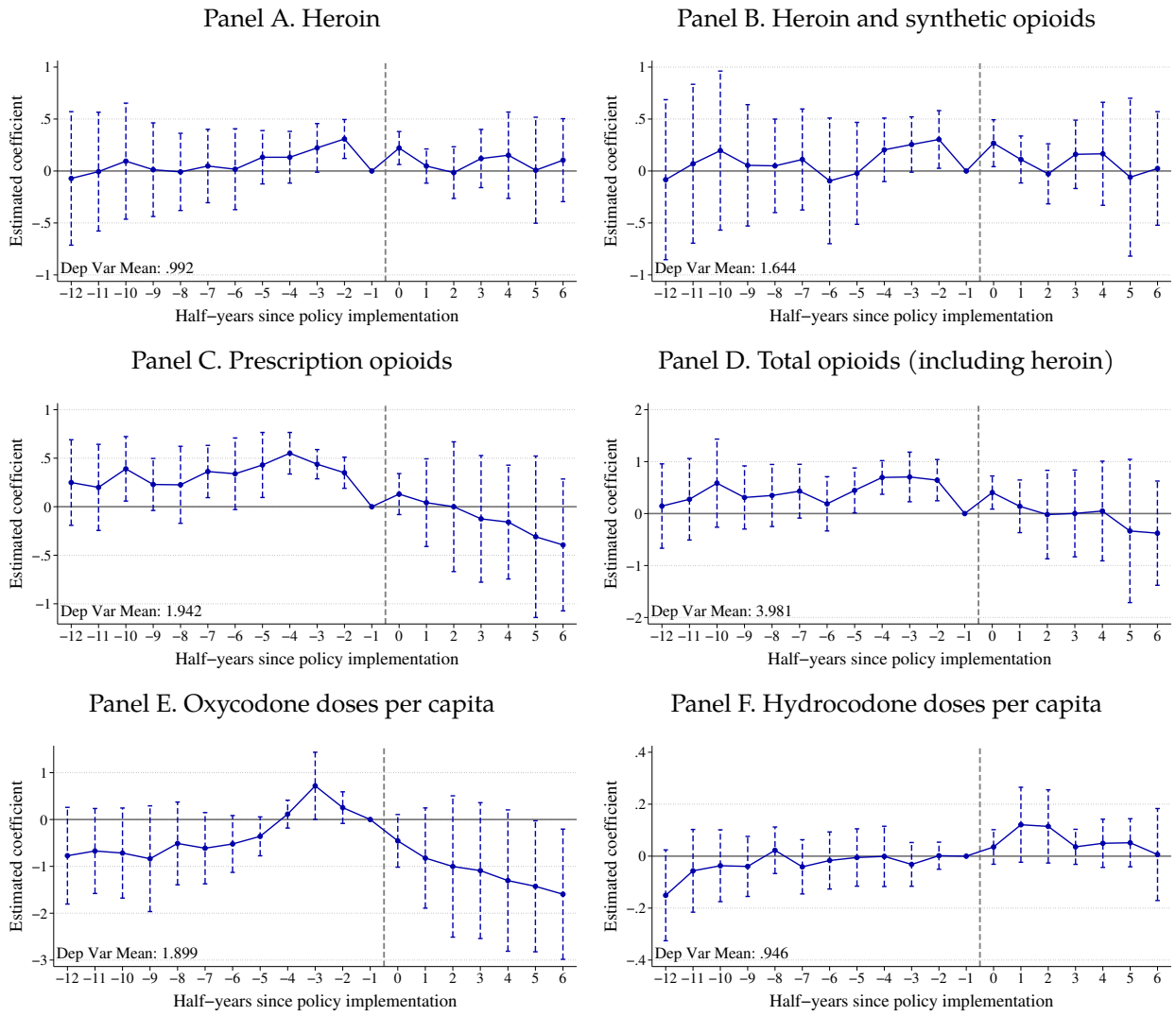
(continued)

Figure A19: Effects of the Must-Access PDMP within a Single State (continued)



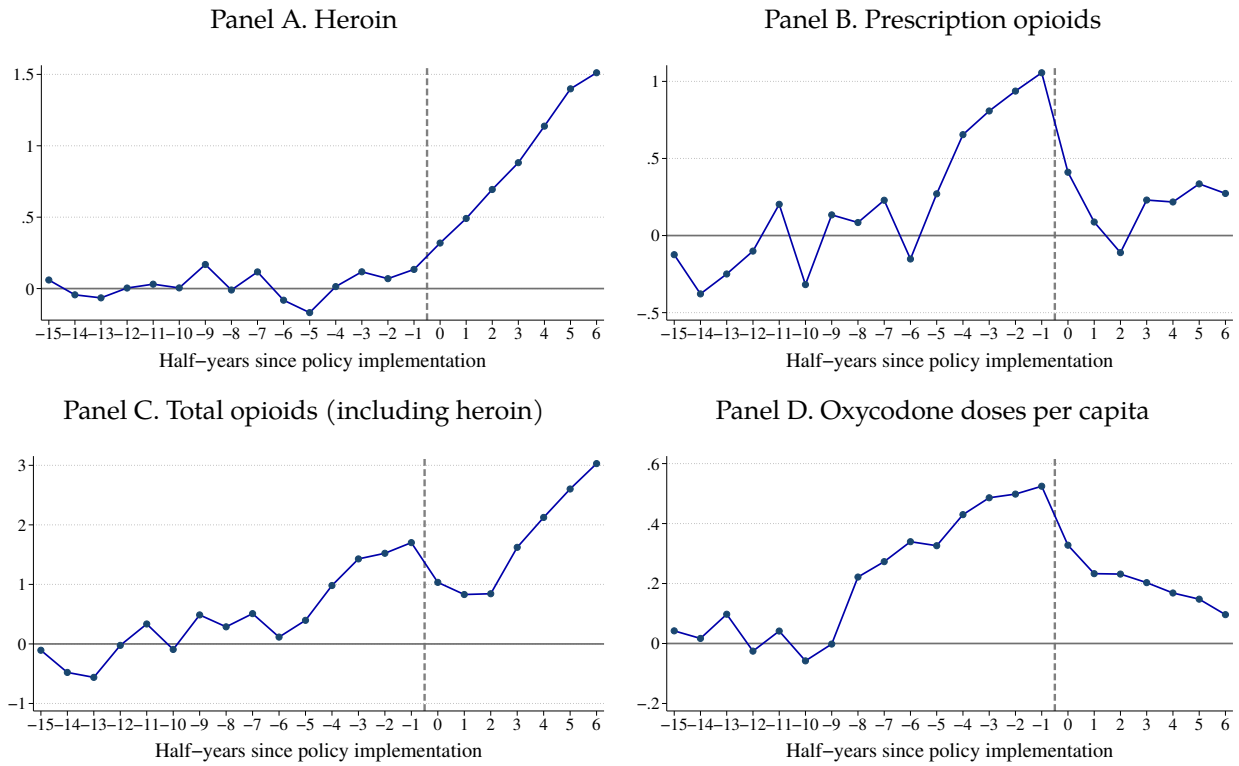
Notes: The figure shows the effect of must-access PDMP within a single treated state. The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences model (equation 1) obtained when I drop all the treated states except for one. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed red line indicates 2010h2, when the OxyContin reformulation was introduced. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription-opioid-only deaths, which involved T40.2 but not T40.1, T40.3, or T40.4 in panel B, oxycodone (morphine equivalent) doses per capita in Panel C, and hydrocodone (morphine equivalent) doses per capita in Panel D. Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used in Panels A–B. Observations are weighted by state population. The control states are the 34 that did not implement must-access policies until 2016h2. Florida is dropped (see Appendix Section B). Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The controls are identical to those in Figure 3.

Figure A20: Effects of Pill Mill Laws Among States without Must-Access PDMPs



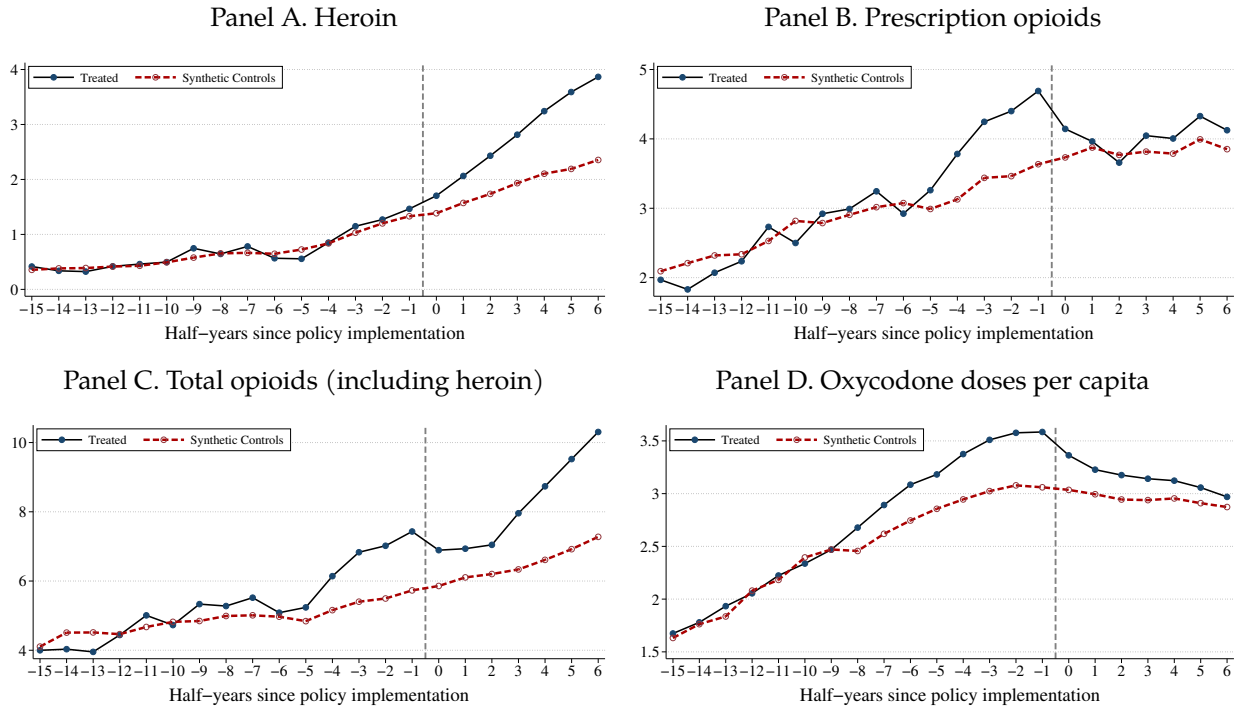
Notes: The figure shows an independent effect of pain clinic laws among the 35 states that did not implement a must-access PDMP until 2016h2. Among these 35 states, Florida, Mississippi, and Texas implemented pain clinic regulations between 2009h1 and 2011h2. In the regressions, I control for a full set of indicators for pre- and post-periods around the enactment of pill mill laws, the full set of state- and time-varying covariates that I use in the baseline analysis, the ARCOS measure of OxyContin misuse interacted with the time fixed effects, an indicator for whether the state had a voluntary-access PDMP, and the fixed effects for state and half-year. The figure displays the coefficients on the indicators for pre- and post-periods. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The treated sample is balanced in relative periods from -12 to +6. The distant relative periods outside the -12/+6 event time window are trimmed. The control states are balanced from 2003h1 to 2016h2. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. In Panels A–D, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. Observations are weighted by state population. The controls are identical to those in Figure 3.

Figure A21: Outcome Gap Between the Treated and Synthetic Control Groups



Notes: The figure displays how the (unweighted) average of the outcome gaps between the treated and synthetic control groups changes over time (see Section E.3). Appendix Figure A21 plots how the (unweighted) average of these gaps changes over time. The outcome is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription opioid death rate (T40.2) in panel B, total opioid-related death rate (T40.1–T40.4) in Panel C, oxycodone (morphine equivalent) doses per capita In Panels A–C, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treated states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 10 synthetic controls (see Appendix Table A5). Both the treated and synthetic control samples are balanced in relative periods from -15 to +6.

Figure A22: Synthetic Control Analysis—Differential Trends



Notes: The figure displays the trends in the outcomes separately for the treated and synthetic control groups. The solid black line presents how the (unweighted) average outcomes change over time in the treated states, and the dashed red blue line displays the trends for the control group. The outcome is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription opioid death rate (T40.2) in panel B, total opioid-related death rate (T40.1–T40.4) in Panel C, oxycodone (morphine equivalent) doses per capita In Panels A–C, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treated states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 10 synthetic controls (see Appendix Table A5). Both the treated and synthetic control samples are balanced in relative periods from -15 to +6.

Table A1: Effects of Must-Access PDMPs on Opioid Overdose Deaths—Summary Effect

	Overdose Deaths per 100,000					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Heroin deaths and illegal opioid deaths per 100,000						
	Heroin (T40.1)			Heroin and Synthetic Opioids (T40.1, T40.4)		
Average effect	1.17**	1.00***	0.60***	1.97***	1.76***	0.99**
	(0.44)	(0.32)	(0.21)	(0.70)	(0.59)	(0.41)
R^2	0.771	0.814	0.862	0.716	0.754	0.827
Mean of dependent variable	1.151			1.884		
Panel B. Prescription opioid deaths and total opioid-related deaths per 100,000						
	Prescription Opioids (T40.2)			Total Opioids (including heroin) (T40.1–T40.4)		
Average effect	0.58***	0.44**	0.28	2.13***	1.84***	1.04**
	(0.21)	(0.17)	(0.20)	(0.69)	(0.59)	(0.42)
R^2	0.811	0.842	0.852	0.756	0.797	0.849
Mean of dependent variable	1.971			4.186		
Panel C. Illegal-opioid-only deaths and prescription-opioid-only deaths per 100,000						
	Illegal-Opioid-Only (T40.1, T40.4 but not T40.2 or T40.3)			Prescription-Opioid-Only (T40.2 but not T40.1, T40.3, or T40.4)		
Average effect	1.61**	1.44***	0.80**	0.27*	0.17	0.13
	(0.61)	(0.52)	(0.36)	(0.15)	(0.12)	(0.14)
R^2	0.710	0.749	0.820	0.815	0.843	0.846
Mean of dependent variable	1.5119			1.5123		
Ruhm (2018) correction	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X	X
Time-varying covariates		X	X		X	X
Pill mill laws		X	X		X	X
OxyContin reformulation			X			X
Number of treatment states	16	16	16	16	16	16
Number of control states	34	34	34	34	34	34
Observations	1,400	1,400	1,400	1,400	1,400	1,400

Notes: The table reports the estimated coefficients obtained when I replace a full set of indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) with a single indicator for the entire post-period. In each column, I include different sets of controls. I use the full sample of the balanced panel of state-half-year from 2003h1 to 2016h2, and Florida is dropped (see Appendix Section B). The treatment states are the 16 that implemented must-access PDMPs until 2016h2. The control states are the 34 that did not implement must-access policies until 2016h2, excluding Florida. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in columns 1–3 of Panel A, combined deaths from heroin and synthetic opioids per 100,000 (T40.1, T40.4) in columns 4–6 of Panel A, prescription opioid deaths per 100,000 (T40.2) in columns 1–3 of Panel B, total deaths from any opioid, including heroin, per 100,000 (T40.1–T40.4) in columns 4–6 of Panel B, illegal-opioid-only deaths per 100,000 (which involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death) in columns 1–3 of Panel C, and prescription-opioid-only deaths per 100,000 (T40.2 but not T40.1, T40.3, or T40.4) in columns 4–6 of Panel C. Ruhm-corrected mortality rates are used in all regressions. Observations are weighted by state population. Controls are identical to those in columns 1, 2, and 4 of Table 3. Fixed effects for states and half-years are always included. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels respectively.

Table A2: Robustness of Heroin Estimates—Voluntary-Access PDMPs

	Heroin Deaths per 100,000 (T40.1)				
	(1)	(2)	(3)	(4)	(5)
	Baseline	Voluntary-access PDMPs			
		Enactment date			User access
		Horwitz	PDAPS	NAMSDL	Horwitz
1-year effect (β_2)	0.42*	0.40*	0.41*	0.42*	0.40*
	(0.21)	(0.21)	(0.21)	(0.21)	(0.22)
2-year effect (β_4)	0.90***	0.89***	0.90***	0.90***	0.88***
	(0.29)	(0.29)	(0.29)	(0.29)	(0.30)
3-year effect (β_6)	1.13**	1.11**	1.12**	1.13**	1.10**
	(0.46)	(0.45)	(0.46)	(0.46)	(0.46)
Voluntary-access PDMPs		-0.19*	-0.18*	-0.07	-0.17*
		(0.11)	(0.10)	(0.09)	(0.09)
Ruhm (2018) Correction	X	X	X	X	X
State fixed effects	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X
Time-varying covariates	X	X	X	X	X
OxyContin reformulation	X	X	X	X	X
Voluntary-access PDMPs		X	X	X	X
Number of treatment states	10	10	10	10	10
Number of control states	34	34	34	34	34
Observations	1,172	1,172	1,172	1,172	1,172
Mean of dependent variable	1.098	1.098	1.098	1.098	1.098
R^2	0.845	0.846	0.846	0.845	0.847

Notes: The table shows the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6) from the baseline specification (equation 1). Although each regression includes a full set of indicators for the pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. In all columns, the dependent variable is Ruhm-corrected numbers of heroin deaths per 100,000 (drug code T40.1), which are calculated using data from the National Vital Statistics System (NVSS). In column 1, I repeat my baseline estimates from column 4 of Table 3 Panel A. In all columns, I control for fixed effects for states and half-years, the ARCOS measure of pre-reformulation OxyContin misuse interacted with the time fixed effects, and the time-varying covariates, which are identical to those in column 4 of Table 3. In columns 2–5, I additionally control for voluntary-access PDMPs. Each column uses start dates of voluntary-PDMPs from a separate source: columns 2–4 use the enactment dates suggested by Horwitz et al. (2018), the PDAPS, and the NAMSDL, respectively; column 5 uses the dates PDMP data became accessible to any authorized user, suggested by Horwitz et al. (2018). Observations are weighted by state population. The treatment states are the 10 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 34 that did not implement must-access policies until 2016h2. Florida is (see Appendix Section B). In all columns, the sample and controls are identical to those in column 4 of Table 3. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels respectively.

Table A3: Robustness of Heroin Estimates to Removing a Single State

	Heroin Deaths per 100,000 (T40.1)										
	(1) Baseline	(2) Drop DE	(3) Drop KY	(4) Drop MA	(5) Drop NM	(6) Drop NY	(7) Drop OH	(8) Drop OK	(9) Drop TN	(10) Drop VT	(11) Drop WV
1-year effect (β_2)	0.42*	0.42*	0.40*	0.37	0.48**	0.60***	0.25	0.47**	0.44*	0.41*	0.36*
	(0.21)	(0.22)	(0.23)	(0.24)	(0.22)	(0.22)	(0.18)	(0.23)	(0.24)	(0.21)	(0.21)
2-year effect (β_4)	0.90***	0.90***	0.91***	0.88***	0.91***	1.01***	0.61***	0.99***	1.00***	0.91***	0.85***
	(0.29)	(0.30)	(0.32)	(0.32)	(0.30)	(0.35)	(0.21)	(0.30)	(0.30)	(0.30)	(0.30)
3-year effect (β_6)	1.13**	1.12**	1.07**	1.08**	1.16**	1.43***	0.71**	1.26**	1.21**	1.13**	1.08**
	(0.46)	(0.46)	(0.49)	(0.51)	(0.47)	(0.47)	(0.35)	(0.48)	(0.50)	(0.46)	(0.47)
Ruhm (2018) correction	X	X	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X	X	X	X	X	X	X
Time-varying covariates	X	X	X	X	X	X	X	X	X	X	X
OxyContin reformulation	X	X	X	X	X	X	X	X	X	X	X
Number of treatment states	10	9	9	9	9	9	9	9	9	9	9
Number of control states	34	34	34	34	34	34	34	34	34	34	34
Observations	1,172	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150
Dep var mean	1.098	1.097	1.091	1.085	1.092	1.080	1.060	1.109	1.106	1.098	1.096
R^2	0.845	0.845	0.843	0.840	0.845	0.841	0.831	0.845	0.850	0.845	0.845

Notes: The table shows the sensitivity of the baseline estimates for heroin mortality to removing a single treatment state. The table reports the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6) from the baseline specification (equation 1). Although each regression includes a full set of indicators for the pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. In all columns, the dependent variable is Ruhm-corrected numbers of heroin deaths per 100,000 (drug code T40.1), which are calculated using data from the National Vital Statistics System (NVSS). Observations are weighted by state population. In column 1, the treatment states are the ten that implemented must-access PDMPs from 2010h2 to 2013h2. In each of columns 2–11, I remove one of these ten states from the analysis sample. In all columns, the treated sample is balanced in relative periods from -15 to +6, and the distant relative periods outside the -15/+6 window are trimmed. In all columns, the control states are the 34 that did not implement must-access policies until 2016h2, and the baseline control sample is balanced from 2003h1 to 2016h2. Florida is dropped (see Appendix Section B). In all columns, the controls are identical to those in column 4 of Table 3. Fixed effects for state and half-year are always included. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels respectively.

Table A4: Robustness of Other Estimates

	Overdose Deaths per 100,000							
	(1) Baseline	(2) Reported mortality	(3) Voluntary PDMPs	(4) Include FL	(5) Add MMLs	(6) Add NALs	(7) Good Sam laws	(8) Alternative dates
Panel A. Heroin and synthetic opioid deaths per 100,000 (T40.1, T40.4)								
1-year effect (β_2)	0.40	0.49	0.39	0.41	0.23	0.48	0.43	0.33
	(0.35)	(0.37)	(0.35)	(0.37)	(0.42)	(0.35)	(0.35)	(0.33)
2-year effect (β_4)	1.36***	1.56***	1.35***	1.35***	1.26**	1.52***	1.41***	1.29**
	(0.49)	(0.50)	(0.48)	(0.50)	(0.53)	(0.48)	(0.49)	(0.47)
3-year effect (β_6)	2.08**	2.34***	2.06**	1.98**	1.98**	2.07**	2.11**	1.64*
	(0.90)	(0.85)	(0.89)	(0.91)	(0.92)	(0.86)	(0.90)	(0.97)
Mean of dependent variable	1.799	1.423	1.799	1.7994	1.799	1.799	1.799	1.792
R^2	0.807	0.797	0.808	0.807	0.814	0.815	0.808	0.813
Panel B. Prescription opioid deaths per 100,000 (T40.2)								
1-year effect (β_2)	-0.54**	-0.52**	-0.54**	-0.48*	-0.56**	-0.52*	-0.55**	-0.57**
	(0.25)	(0.21)	(0.25)	(0.25)	(0.26)	(0.26)	(0.26)	(0.28)
2-year effect (β_4)	-0.27	-0.21	-0.28	-0.21	-0.28	-0.24	-0.29	-0.25
	(0.32)	(0.28)	(0.33)	(0.32)	(0.32)	(0.34)	(0.33)	(0.34)
3-year effect (β_6)	-0.27	-0.22	-0.27	-0.22	-0.28	-0.27	-0.28	-0.21
	(0.35)	(0.33)	(0.35)	(0.33)	(0.35)	(0.36)	(0.36)	(0.37)
Mean of dependent variable	1.984	1.533	1.984	2.039	1.984	1.984	1.984	2.016
R^2	0.861	0.843	0.862	0.860	0.862	0.862	0.862	0.867
Panel C. Total opioid-related deaths per 100,000 (T40.1–T40.4)								
1-year effect (β_2)	-0.17	-0.07	-0.18	-0.11	-0.37	-0.09	-0.15	-0.33
	(0.42)	(0.40)	(0.42)	(0.44)	(0.48)	(0.45)	(0.43)	(0.38)
2-year effect (β_4)	0.84*	1.06**	0.82*	0.90*	0.71	0.99**	0.86*	0.71
	(0.46)	(0.46)	(0.46)	(0.47)	(0.49)	(0.48)	(0.48)	(0.43)
3-year effect (β_6)	1.61*	1.87**	1.60*	1.59*	1.49	1.61*	1.63*	1.16
	(0.89)	(0.84)	(0.89)	(0.88)	(0.90)	(0.87)	(0.89)	(0.90)
Mean of dependent variable	4.126	3.210	4.126	4.205	4.126	4.126	4.126	4.140
R^2	0.842	0.837	0.842	0.841	0.848	0.845	0.842	0.851
Ruhm (2018) correction	X		X	X	X	X	X	X
Number of treatment states	10	10	10	10	10	10	10	8
Number of control states	34	34	34	35	34	34	34	31
Observations	1,172	1,172	1,172	1,200	1,172	1,172	1,172	1,044

Notes: Notes: The table tests the robustness of my baseline heroin mortality estimates to alternative explanations. The table shows the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6), obtained from the baseline specification (equation 1). Although each regression includes a full set of indicators for pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. Observations are weighted by state population. In all columns in panel A, the dependent variable is combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4). In all columns in panel B, the dependent variable is prescription opioid deaths per 100,000 (drug codes T40.2–T40.3). In all columns in panel C, the dependent variable is total deaths from any opioid, including heroin (drug codes T40.1–T40.4). In column 1 of Panel A, I repeat my baseline estimates from column 4 of Table 3 Panel B. In column 1 of Panels B and C, I repeat my preferred estimates from column 4 of Table 5. In column 2, I use the raw reported numbers of deaths, and the other columns use the Ruhm-corrected numbers of deaths. Both the corrected and reported numbers of deaths are calculated using data from the National Vital Statistics System (NVSS). In column 3, I control for an indicator for whether a state had a voluntary-access PDMP. In column 4, I include Florida in the analysis sample. Florida is dropped from the control group in the other columns (see Appendix Section B). In columns 5–7, I include several other co-occurring opioid-related policies one by one: in column 5, I include a time-varying indicator for whether the state had medical marijuana laws (MMLs) as well as an indicator for whether the state had legal and operational dispensaries; in column 6, I add an indicator for whether the state had naloxone access laws (NALs), and in column 7, I add an indicator for whether the state had Good Samaritan overdose prevention laws. Columns 5–7 suggest that my estimates are robust to including several other co-occurring state opioid-related policies. In column 8, I use alternative start dates of must-access PDMPs listed in the third column of Table 1, and in this estimation, the treated states are the 8 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 31 that did not implement must-access policies until 2016h2, excluding Florida. In all columns, the distant event periods outside the -15/+6 window are trimmed. In all columns, I control for fixed effects for state and half-year, an indicator for whether a state had a pill mill law, the ARCOS measure of OxyContin misuse interacted with the half-year fixed effects, and the time-varying covariates that are identical to those in column 4 of Table 3. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

Table A5: Synthetic Control States

Treated State	Synthetic Control State					
Panel A. Heroin deaths per 100,000 (T40.1)						
Delaware	30.1% UT,	20.5% AZ,	17.7% ID,	10.9% DC,	7% KS,	13.9% Other
Kentucky	54% WI,	23.5% AK,	16.6% AZ,	4.9% DC,	1% MN,	
Massachusetts	48.6% AK,	19.6% DC,	13.6% NH,	8.9% MD,	4% ME,	5.2% Other
New Mexico	47.4% MO,	28.3% PA,	24.3% UT			
New York	17.7% WA,	16.3% NH,	14.1% NC,	13.7% WY,	13.5% MD,	24.7% Other
Ohio	65.9% MO,	23.8% UT,	6.1% MD,	4.1% DC		
Oklahoma	68.7% ND,	13.3% NH,	7.1% NE,	4.7% NC,	3.7% AZ,	2.5% Other
Tennessee	34.4% MS,	34.2% ND,	12.1% AZ,	7.5% AL,	5.5% KS,	6.3% Other
Vermont	54.1% MN,	31% AK,	8% IL,	6.9% WY		
West Virginia	24.5% KS,	20.6% MO,	19.8% MT,	18.6% HI,	11.3% OR,	5.2% UT
Panel B. Prescription opioid deaths per 100,000 (T40.2)						
Delaware	48.5% WY,	38.1% IN,	11.7% AK,	1.6% UT		
Kentucky	74.8% UT,	25.2% WY				
Massachusetts	79.2% TX,	14.7% ND,	4.6% DC,	1.5% CO		
New Mexico	97.6% UT,	2.4% AK				
New York	25.5% IA,	22.6% IL,	21.1% MD,	17.5% HI,	8.7% DC,	4.5% Other
Ohio	34.3% AZ,	29.1% NH,	16.5% WY,	15.1% HI,	4.9% AR,	0.1% SD
Oklahoma	55.7% UT,	28.6% AK,	15% WY,	0.7% NH		
Tennessee	35.9% WY,	31.9% UT,	27.1% AZ,	5.1% PA		
Vermont	35% ID,	23.2% UT,	19.1% ND,	12.3% DC,	6.1% TX,	4.4% WY
West Virginia	100% UT					
Panel C. Total opioid-related deaths per 100,000 (T40.1–T40.4)						
Delaware	48.8% MO,	22.1% WY,	12.2% CO,	10.1% AK,	5.3% AL,	1.4% NH
Kentucky	75.9% UT,	24.1% MO				
Massachusetts	61.1% IL,	19.1% CO,	8.1% HI,	6.4% UT,	2.3% KS,	3% Other
New Mexico	100% UT					
New York	29.7% IA,	22.6% KS,	16.9% DC,	13.6% MD,	12.9% HI,	4.5% Other
Ohio	89.6% AZ,	4.9% MO,	2.5% UT,	1.8% WY,	1.1% MD	
Oklahoma	57.8% UT,	26.4% AK,	15.8% NH,			
Tennessee	47.3% MO,	23.2% UT,	10.7% PA,	7.2% WI,	6.5% ME,	5% Other
Vermont	54.8% IL,	37.5% UT,	5.2% WY,	2.5% DC		
West Virginia	100% UT					
Panel D. Oxycodone doses per capita						
Delaware	100% AZ					
Kentucky	54.1% GA,	27.9% PA,	18% MD			
Massachusetts	23.8% AK,	19.6% UT,	13.3% MO,	12.9% NE,	11.2% WA,	19.2% Other
New Mexico	42.3% MD,	40.8% CO,	17% GA			
New York	66.7% GA,	26% IN,	7.3% NH			
Ohio	38.2% ME,	26.8% NH,	18.6% WA,	8.6% MO,	7.7% PA	
Oklahoma	27.2% UT,	25.6% AR,	21.3% MI,	12.1% OR,	9% AZ,	4.8% NH
Tennessee	50.2% ME,	43.4% AZ,	6.4% PA			
Vermont	29.2% WA,	20.3% MD,	16% NE,	12.4% MI,	10.5% NH,	11.6% Other
West Virginia	37.9% NH,	28.2% MD,	20.9% PA,	13% ME		

Notes: This table shows how synthetic control states included in the sample from Appendix Figures A21 and A22 are constructed. Each synthetic control state is calculated as a linear combination of the subset of my 34 control states (Florida is excluded from the control sample; see Appendix Section B). Values are independently rounded, and for synthetic states with more than six control states, remaining states are grouped into an "other" category. The outcome is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription opioid death rate (T40.2) in panel B, total opioid-related death rate (T40.1–T40.4) in Panel C, oxycodone (morphine equivalent) doses per capita