Regulation attenuation: Neighbor spillovers and policies in the opioid epidemic

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Abstract

Prescription drug monitoring programs (PDMPs)—online systems that health care providers and pharmacists can use to query patient prescription records—are one of the most widely-used state tools in regulating the prescribing and dispensing of opioids. However, the staggered adoption of PDMPs over time has created opportunities for patients to evade monitoring by going to a state that does not have a PDMP. This paper evaluates how spillovers attributable to policy non-coordination between neighboring states impact the effectiveness of PDMPs. I find that after prescribers gain access to PDMPs, opioid volume and prescription opioid deaths decrease in counties with a PDMP that are insulated from opportunities for evasion. I find a similar effect in counties with a PDMP that are exposed to evasion. This suggests that exposure to evasion through proximity to non-PDMP areas does not significantly attenuate the policy effect. I also find evidence that opioid volume and prescription opioid deaths decrease in counties without a PDMP that are exposed to spillovers from counties with the policy. Illicit opioid deaths are not affected in any counties with a PDMP but decrease in counties without a PDMP that are exposed to spillovers. I discuss the potential mechanisms through which spillovers may operate.

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1 Introduction

The Center for Disease Control and Prevention (CDC) has called the opioid epidemic one of the worst drug overdose epidemics in the history of the United States (Kolodny et al., 2015). Contributing factors included excessive prescribing by providers as well as patients obtaining prescriptions from multiple prescribers and dispensers—practices known as "doctor-shopping" and "pharmacy-shopping", respectively. Public officials have turned primarily to supply-side policies to deal with the epidemic, among them prescription drug monitoring programs (PDMPs). In their most recent form, PDMPs are digital databases that track the movement of controlled substance prescriptions in a given state. Prescribing providers (e.g., primary care physicians, dentists, surgeons), dispensers (e.g., pharmacists, clinicians), law enforcement, and/or other authorized users can query the system to monitor the prescribing and dispensing of controlled substances—including opioids—to patients. The goal of PDMPs is to reduce unnecessary opioid prescribing, thereby reducing rates of opioid misuse, substance use disorder, diversion, and overdoses.

Despite the policy's potential to reduce dangerous over-prescribing and adverse patient outcomes, evidence on PDMP effectiveness is mixed. Some papers demonstrate that initial PDMP implementation leads to a reduction in opioid volume (Kilby, 2015; Curtis et al., 2006; Reisman et al., 2009). Other work has shown that PDMPs do not have an effect on opioid prescriptions (Meara et al., 2016) or that only PDMPs in states with "mandatory access" (MA) laws—whereby providers (dispensers) are legally required to query the PDMP prior to writing (filling) a prescription—are effective (Buchmueller and Carey, 2018; Meinhofer, 2018).

One potential explanation for the mixed evidence is the spillovers that arise due to policy non-coordination between neighboring jurisdictions. The staggered adoption of PDMPs over time has created opportunities for some patients to evade detection by their state's PDMP by going to a neighboring state without one.[1] This means that even if PDMPs are effective

[1] Anecdotal reports of opioid users traveling across the state border to access prescription opioids from out-of-state providers and pharmacies once their state of residence implements a PDMP are documented in Quinones (2015).

at reducing the supply of prescription opioids to patients in that state, their impact may be attenuated in areas that are exposed to evasion. Moreover, outcomes in states without a PDMP could be affected by treatment spillovers from states with the policy.

This paper investigates how non-coordination between neighboring jurisdictions impacts the effectiveness of PDMPs. To do this, I separately estimate the effect of PDMPs in counties insulated from opportunities for evasion and in counties exposed to evasion. Among counties without a PDMP, I also differentiate between those that are exposed to treatment spillovers and those that are insulated from spillovers. A county is defined as "exposed", either to evasion or spillovers, if at least one of its neighbors has a different policy. County neighborhoods are defined based on geographic proximity.

I use a difference-in-differences model and exploit the cross-state variation in the timing of PDMP implementation to estimate the effect of PDMPs and spillovers on opioid volume and opioid-related mortality. I assign counties to one of three distinct treatments based on PDMP implementation in the county and among its neighbors. Non-PDMP counties that are insulated from treatment spillovers are the control group. Balance tests show that each of the treatment groups is similar along a number of demographic and economic factors to the comparison control group in the period before the treatment. I also provide some supportive evidence for the exogeneity assumption of the timing of policy implementation across states.

Estimates for counties with a PDMP that are insulated from evasion capture the main effect of a PDMP. In these counties, I find that online access to a PDMP for prescribers leads to a decrease in opioid volume and prescription opioid deaths. These effects are consistent with the intended goal of the policy, which is to increase the patients' cost of obtaining prescription opioids and to nudge prescribers and dispensers toward better practices through increased oversight of prescriber and patient behavior. I show that these findings contrast with estimates from the literature and estimates from a model that does not account for neighbor spillovers and instead treats all counties with a PDMP as the treated group and all counties without a PDMP as the control group. The no-spillovers model shows no statistically significant effect of a PDMP on either opioid volume or prescription opioid mortality.

Specifying the treatment and control groups as counties that are insulated from evasion and spillovers allows me to estimate the main effect of a PDMP and provides suggestive evidence for why previous work has not found conclusive results of the policy's impact.

The implementation of PDMPs could also have spillovers to markets for other narcotic substances. Opioid users who experience an increase in the cost of accessing opioids often substitute to other cheaper substances like heroin.[2] At the same time, changes in prescriber and dispenser practices will decrease the number of new opioid users, which will also affect mortality rates. I find that in counties with a PDMP that are insulated from evasion, PDMPs have no significant effect on mortality from illicit opioids. These results suggest that the increase in substitution to illicit opioids and the decrease in the total number of opioid users offset each other in the aggregate.

I next investigate the role that evasion and spillovers may play in attenuating the effect of the PDMP and masking it in the analysis that does not account for these factors. As I discuss in more detail in the conceptual framework section, there may be ambiguous cumulative effects in both counties with and without a PDMP that are in proximity to neighbors with different policies. I predict that in counties with a PDMP that are exposed to evasion, opioid volume and prescription opioid deaths would decrease but the effect on illicit opioids deaths would be ambiguous. In counties without a PDMP exposed to policy spillovers, I predict that prescription opioid deaths would decrease but the effect on opioid volume and illicit opioid deaths will depend on the channels through which the spillovers operate.

[2]This was the case when OxyContin, a popular opioid produced by Purdue Pharmaceuticals, was reformulated to deter abuse (Alpert et al., 2018). The initial formulation of OxyContin was a dry pill that would slowly release the 30-120 mg of its main ingredient oxycodone (depending on the size of pill) over a period of 8-12 hours. The gradual release was a therapeutic breakthrough that meant that patients would not have to remember to take pills as often to manage their pain. However, OxyContin turned out to be highly abusable. First, the opioid-only formulation made it more appealing than other opioids like Vicodin and Lorcet that also contain acetaminophen (the main ingredient in non-opioid painkillers like Tylenol), which has to be separated from the opioid prior to use. Patients furthermore found that they could achieve a stronger high by crushing and snorting OxyContin pills, bypassing the slow-release formula and releasing the large dose of oxycodone all at the same time. In 2010, the Food and Drug Administration (FDA) approved a reformulated gel capsule version of OxyContin that was more difficult to abuse by crushing. Substitution from prescription opioids to heroin was remarkably prevalent. Early on in the epidemic, people misusing prescription opioids were 40 times more likely to use heroin than people who did not misuse them, and 80% of heroin users had reported previously using prescription opioids (Center for Disease Control & Prevention, 2018b).

Assessing these predictions in the data, I find that exposure to evasion may not have a significant effect on county outcomes, unlike exposure to spillovers. First, prescriber online access to a PDMP reduces opioid volume and prescription opioid deaths in PDMP counties exposed to evasion. There is also no effect on illicit opioid deaths. The magnitudes of these policy effects are similar to those in PDMP counties that are insulated from evasion. This suggests that proximity to untreated areas and opportunities for evasion may not play a significant role in actually attenuating the effect of the policy. At the same time, in non-PDMP counties exposed to spillovers, I estimate a decrease in opioid volume and both mortality outcomes. This suggests that the mixed evidence on the effectiveness of PDMPs may be primarily due to spillovers affecting outcomes in counties without a PDMP rather than due to evasion attenuating outcomes in counties with the policy. Additionally, the negative effect on illicit opioid deaths in the non-PDMP counties suggests that increased monitoring of opioid prescriptions may be one of the channels through which spillovers operate. In these counties, monitoring behavior could be affected by elements of PDMP policies that target non-resident pharmacies.

I conduct an additional robustness check for the mortality outcomes. The qualitative results for both prescription opioid and illicit opioid mortality are robust to alternative categorizations of deaths used in the literature (Kilby, 2015; Patrick et al., 2016; Rudd et al., 2016; Ruhm, 2018). The robustness check also suggests that the decrease in prescription opioid deaths is primarily driven by a reduction in deaths due to natural opioids (e.g, oxycodone, hydrocodone).

This paper fits into several literatures. First and foremost, it fills a gap in the existing literature that evaluates the effect of PDMPs. Several papers have examined the impact of PDMPs on opioid volume (Kilby, 2015; Meinhofer, 2018), prescription opioid deaths (Kilby, 2015; Meinhofer, 2018; Garin et al., 2018; Grecu et al., 2019; Patrick et al., 2016), and spillovers to markets for other substances (Alpert et al., 2018; Evans et al., 2018; Garin et al., 2018).[3] However, despite the potential significance of spillovers, most of the exist-

[3]Research on PDMPs has also looked at the impact of the policy on many other outcomes such as pain and missed work days (Kilby, 2015), suicides (Borgschulte et al., 2018), employment (Currie et al.,

ing literature on PDMPs assumes that there are no spillovers between states and that a state's PDMP—or lack thereof—only affects outcomes within its own borders. Previous work that considers spillovers either examines policy spillovers from treated to untreated areas (Buchmueller and Carey, 2018; Meinhofer, 2018) or estimates how exposure to evasion attenuates the effect of PDMPs (Grecu et al., 2019). To my knowledge, my study is the first to incorporate both exposure to spillovers and exposure to evasion into the analysis.

This paper also fits into the literature on the evaluation of local policies in the presence of neighbor spillover effects. Many policies—such as taxes, gun laws, substance regulation, and public health mandates—are implemented locally at the state, county, or municipal level. Work in this area has demonstrated that both treated and untreated jurisdictions can be affected by exposure to neighbors with different policy implementation statuses.[4] This paper contributes evidence on the impact of PDMPs in the presence of neighbor spillovers. The findings may be informative for policy makers who are considering implementing other measures that restrict substance access.

This paper is organized as follows. Section 2 provides background information on the institutional setting, including the history of the opioid epidemic and the operation of PDMPs. Section 3 discusses neighbor spillovers and the potential mechanisms that could affect outcomes in counties exposed to them. Section 4 describes the data. Section 5 describes the empirical strategy. Section 6 presents and discusses the results. Section 7 presents and discusses the robustness analysis. Section 8 concludes.

2018), crime (Dave et al., 2020), foster care admissions (Gihleb et al., 2018), and infant health (Ziedan and Kaestner, 2020), among others.

[4]Bollinger and Sexton (2018) find that after Berkeley, CA, implemented a tax on sugary beverages, the decrease in soda sales in Berkeley was partially offset by an increase in soda sales in the neighboring areas without the tax. Hao and Cowan (2020), examining the legalization of recreational marijuana in Colorado and Washington, find that police arrests for illegal marijuana possession increased in the border counties of neighboring states that did not pass the same legislation. They attribute this increase to changes in police enforcement practices. In a study of state business closures during the COVID-19 pandemic and their impact on mobility, Zhao et al. (2021) show that focal states with closure policies experienced greater reductions in mobility if they were surrounded by other states that implemented similar policies.

2 Background

2.1 The United States opioid epidemic

The US opioid epidemic has progressed in three waves, with each wave characterized by the leading cause of the increasing mortality rate. The first wave of mortality was driven by prescription opioids (Center for Disease Control & Prevention, 2019). From the 1908's, a growing movement among physicians who advocated urgently for drug-based treatment of pain and its adoption as the "fifth vital sign" was spurred on by advertising from pharmaceutical companies of their opioid products (Holmgren et al., 2020). Pain clinics that colloquially became known as "pill mills" for their excessive and clinically questionable prescribing of opioids, patients, and insurance companies also contributed to the start and spread of the epidemic (Quinones, 2015).[5]

To address the wave of mortality driven by prescription opioids, in the early 2000's states responded by passing various legislation to limit opioid prescribing to patients. The myriad responses were successful at reducing the supply of prescription opioids, but the resulting supply shock ushered in the second wave of the opioid epidemic as users switched to a cheaper, more accessible alternative: heroin (Alpert et al., 2018). More recently, there has been an increase in deaths due to synthetic opioids like tramadol, fentanyl, and carfentanil.

Despite the growing role of heroin and fentanyl in the opioid epidemic, prescription opioids continue to play a significant part in the crisis. In 2017, more than 191 million prescription opioids were dispensed to individuals in the United States. Although the mortality rate due to prescription opioids has plateaued since 2010, these deaths still account for 30% of all opioid-related deaths. (Center for Disease Control & Prevention, 2018c)

[5]Initially, opioids were only used to treat patients with cancer and those in hospice and palliative care. In the 1980-90's, several factors coalesced to start the crisis. In their advertising, pharmaceutical companies heavily relied on two publications to promote the belief that addiction among patients using opioids was rare: a 100-word letter to the editor published in *New England Journal of Medicine* in 1980 and a thirty-eight patient observational study published in *Pain* in 1986 (Porter and Jick, 1980; Portenoy and Foley, 1986). Other important factors included changes in which health care services were reimbursed by insurance companies and patients advocating for better treatment of their chronic pain conditions (Quinones, 2015).

2.2 The history of PDMPs

The surveillance of controlled substances has a long history in the United States. From the 1930's to 1980's, eight states implemented a "triplicate" prescription monitoring program (Holmgren et al., 2020).[6] The "triplicate" program required providers to write controlled substance prescriptions on carbon paper to create copies, one of which remained with the pharmacist and one of which was mailed to the centralized state database. In the 1990's, five states implemented an electronic PDMP,[7] although these were primarily used by law enforcement in the state (Holmgren et al., 2020). In 1997, Nevada became the first state to grant online access to the PDMP to prescribers.

As the severity of the opioid epidemic became clear in the early 2000's, the federal government undertook efforts to help states address the crisis. In 2002, Congress established the Harold Rogers Prescription Drug Monitoring Program to support state PDMP initiatives. In 2003, the National Alliance for Model State Drug Laws (NAMSDL) published the first Model Prescription Monitoring Program (PMP) Act, a legislative blueprint that states could use to enact their own PDMPs. Between 2002 and 2015, 49 states and 1 U.S. territory had received Bureau of Justice Assistance (BJA) grants to "plan, implement, or enhance [their] PDMP" (U.S. Department of Justice, 2016), and by 2017, 49 states, the District of Columbia, and St. Louis County (Missouri)[8] have implemented a PDMP.[9] The map of state-wide PDMPs by year of implementation is shown in Panel A of Figure 1.

[6]The eight states were California (which implemented the program in 1939), Hawaii (1943), Illinois (1961), Idaho (1967), New York (1973), Rhode Island (1978), Texas (1981), and Michigan (1988) (Holmgren et al., 2020).

[7]The five states to implement an electronic PDMP were Oklahoma, Nevada, Massachusetts, Utah, Indiana, and Kentucky. (Holmgren et al., 2020)

[8]In Missouri, the only state that has not yet passed a state-wide program, counties can opt in to join the St. Louis County PDMP. The St. Louis PDMP currently covers 85% of the population and 94% of providers (St. Louis County Department of Public Health, 2021).

[9]Three U.S. territories–Guam, Northern Mariana Islands, and Puerto Rico–and the Defense Health Agency have also implemented a PDMP.

2.3 PDMP use and related policies

PDMPs provide access for authorized users to patient prescriptions of controlled substances.[10] Records of patient controlled substance prescriptions in the PDMP come from reports submitted by dispensers like pharmacists, authorized clinicians, and veterinarians. The reports are primarily submitted by in-state dispensers, but states can also mandate reporting by non-resident pharmacies. For a given state, non-resident pharmacies are out-of-state pharmacies that deliver prescriptions to residents of that state. In addition to requiring reporting by in-state dispensers, a state with a PDMP can also require non-resident pharmacies to submit to the PDMP reports on the controlled substance prescriptions that are mailed to their state's residents. As of 2016, 47 pharmacies required reporting by non-resident pharmacies (National Alliance for Model State Drug Laws, 2016). Unfilled prescriptions are not reported to the PDMP.

Prescribers and dispensers who register with the PDMP can query patient records prior to writing or filling a prescription. When querying patient records, they can look for factors like high dosage, multiple providers, and potentially dangerous drug interactions (Center for Disease Control & Prevention, 2021). If prescribers suspect opioid misuse, they can refuse to write the script and refer the patient to treatment. Dispensers can refuse to fill the prescription and potentially contact the prescriber with their concerns.

Providers have not always endorsed PDMP use as a solution for the opioid epidemic. Prescribers frequently complain about the time and effort necessary to use the system (Gourlay, 2013; Islam and McRae, 2014), feeling "nickel-and-dimed" with each query that takes "about three to five minutes" (Radomski et al., 2018). As a result, multiple states have documented that when querying the PDMP is voluntary, PDMP utilization rates by prescribers can be as low as 10-16% (Arditi, 2014; Carey et al., 2021; Electronic-Florida Online Reporting of Controlled Substances Evaluation (E-FORCSE), 2012).

[10]Controlled substances are designated as such under the Controlled Substances Act (CSA) and are divided into five schedules. "Substances are placed in their respective schedules based on whether they have a currently accepted medical use in treatment in the United States, their relative abuse potential, and likelihood of causing dependence when abused." (Drug Enforcement Administration, 2021)

Because of this resistance, states have sought to promote higher PDMP utilization by prescribers. One widely used option is mandatory access (MA) laws that require prescribers to query the system prior to writing a controlled substance prescription.[11] The map of statewide MA PDMPs by year of implementation is in Panel B of Figure 1. As of March 2021, 10 states had not yet implemented any MA PDMP law for prescribers. Less frequently, states may also require that dispensers query the PDMP prior to filling a prescription. These mandates are less common than ones targeting prescribers: to date, only 15 states require pharmacists to query a PDMP prior to filling a prescription.[12] Prescribers and dispensers may face professional, civil, and even criminal penalties if they do not comply with MA PDMP regulations. Studying Kentucky's transition from a voluntary PDMP to an MA PDMP, Carey et al. (2021) find that MA legislation is successful at substantially increasing prescriber utilization rates (although compliance is still not universal).

In addition to mandating utilization by prescribers and/or dispensers, policy makers can use complementary policies to augment PDMP effectiveness. For example, states can mandate more frequent reporting by dispensers to update prescription records more frequently and thereby reduce opportunities for "shopping" by patients. However, some "shopping" may still go undetected even if all of the states have a PDMP if those PDMPs do not share records with each other. To prevent "shopping" across the border, states may find it advantageous to join one of the two services that facilitates data sharing between PDMPs and set up the requisite pairwise agreement with another member of the service. Since prescribers from a given state cannot query the PDMP of another state if the two states are not in the same service and have not signed an agreement, data sharing between states remains incomplete.

[11]The cases when prescribers are required to query the PDMP vary by state and can depend on the length of the prescription, the schedule of the controlled substance, the practice setting, the patient's age and medical condition being treated, and whether the prescription is a first time or a refill, among other characteristics.

[12]Prescription Drug Monitoring Program Training and Technical Assistance Center (PDMP TTAC), which collects information on PDMPs in the US, lists 19 states that have implemented mandatory access regulations for dispensers. I verified that 15 of these programs specifically included pharmacists among the dispensers required to query the PDMP and have become operational.

3 PDMPs and neighbor spillovers

For patients with opioid use disorder, pain clinics would be a low cost option to obtain scripts for opioids in large quantities. Patients who cannot access a pain clinic might instead have to resort to "doctor-shopping", visiting multiple providers. With script(s) in hand, users would have to go to a pharmacy to fill them, but local pharmacists may recognize questionable prescriptions and refuse to fill them. Filling multiple prescriptions in one location may also be difficult, so users would be forced to travel out of town to find pharmacies.

Searching for either prescribers or dispensers may not be restricted to within-state travel. When states do not coordinate the timing of their PDMP implementation with each other, this creates opportunities for users to evade monitoring through cross-state travel.[13] Evasion would mitigate the impact of a PDMP in counties that implemented the policy but have a neighbor without it. In turn, untreated states may be affected by policy spillovers if there is a neighboring state with a PDMP. These policy spillovers may potentially act through multiple channels, such as an increase in the number of opioid users or required reporting by non-resident pharmacies. These channels are described in more detail in the framework below.

To understand how neighbor spillovers would affect outcomes, it is helpful to conceptualize opioid users' choices and the costs associated with those choices. In each period, there are some continuing patients and newly initiated patients who make up the total pool of opioid users in each of two states A and B. Opioid users can pay the cost to obtain prescription opioids, which can include time and effort to find prescribers and pharmacists to write and

[13] Quinones (2015) documents testimonies from opioid users who frequently traveled to other states to obtain prescription opioids. The decision to travel across state borders was often influenced by increased oversight of prescribing in their home state and lack thereof in another. The quote below is one of several examples in the book that illustrates this:

> *"Kentucky, to its credit, was one of the first to put in a system tracking what drugs each patient had been prescribed and by whom. But seven states border Kentucky. Many eastern Kentuckians have relatives who left to find work in other states, and, after decades of out-migration, state lines tend not to matter much to folks from the region. The Kentucky prescription monitoring system, therefore, very quickly had the unintended consequence of pushing the state's new opiate addicts out across state lines in search of pills, tapping first their networks of friends and relatives."* (*Dreamland*, p.242)

fill the prescription; the monetary cost of travel, provider visit, and pharmacy fill; or the cost of obtaining prescription opioids on the illicit market. Traveling out of state may be a cost-effective option because of easier access to providers (potentially through pain clinics) or greater anonymity (prescribers don't know the user or pharmacists don't recognize the name of the prescriber). In addition to accessing prescription opioids, opioid users could substitute to heroin or other illicit drugs if access to substitutes is cheaper. Finally, users could also choose to exit narcotics markets altogether (this would include both stopping to take opioids for patients who have not developed substance dependence and entering substance abuse treatment for those who have).

The three outcomes that I will consider in the empirical analysis are opioid volume (opioid quantity supplied), deaths due to prescription opioids, and deaths due to illicit opioids. In this framework, opioid volume is determined by the number of opioid users who obtain prescription opioids *in* a given state, regardless of their state of residence. On the other hand, the number of deaths due to prescription opioids would depend on the number of opioid users actually living in the state, as well as the substitution rate to illicit opioids and the exiting rate from the market. Finally, deaths due to illicit opioids would depend on the number of opioid users living in the state and the substitution rate from prescription opioids to illicit drugs.

I now describe a simplified framework with two states and staggered PDMP implementation, first with no neighbor spillovers and then allowing for neighbor spillovers. In the initial setting, I assume that travel is costly between the two neighboring states, so users from each state can only obtain prescription opioids in their state of residence, and that states can only enforce the policy within their borders. As a result, after state A implements a PDMP (and state B does not), there would be no attenuation of the policy effect through exposure to evasion as opioid users could not evade the PDMP through cross-border travel. Likewise, there would be no policy spillovers, since PDMP implementation would only affect prescribers and dispensers in the state with the policy.

The increased monitoring of prescriber and patient behavior through the PDMP would

lead to two changes in state A. First, the cost of accessing prescription opioids would increase, and second, the behavior of prescribers and/or dispensers would change such that they become more careful about prescribing opioids to new patients. The higher cost of access would increase substitution from prescription opioids to other substances and would also increase the rate of exiting from opioid use. This substitution would take effect immediately, so in the short-run, I would predict a decline in opioid volume and prescription opioid deaths and an increase in deaths from illicit opioids. Over time, the change in prescriber/dispenser behavior would shrink the number of patients using opioids. In the long-run, I would predict that this would reinforce the decline in opioid volume and prescription opioid deaths, but it may dampen the increase in illicit opioid deaths, potentially reversing the sign of the effect.

In state B, where there is no direct policy implementation or policy spillovers from state A, neither opioid volume nor any mortality outcomes would change.

In a setting with neighbor spillovers, cross-border travel is less costly for some residents, so opioid users will have an additional option to access prescription opioids in the neighboring state. As traveling out-of-state will always be cost effective for some opioid users, some users from state A will access opioids in state B, and some users from state B will access opioids in state A. When state A implements a PDMP, all users obtaining opioids in state A (users from state A staying in-state and users from state B traveling out-of-state) would be impacted by the higher cost of accessing prescription opioids. This will increase the rate of substitution and the rate of exiting for users in both states. Furthermore, the changes in prescriber/dispenser behavior from access to the PDMP would affect the residents in state A. Overall, the qualitative effect on opioid volume and prescription opioid deaths in state A when exposed to evasion would be the same as in state A when it is insulated from evasion: both would decrease. However, as access is available to prescription opioids through state B, the effect on prescription opioid deaths would be attenuated in the exposed state. The effect on illicit opioid deaths will also be ambiguous. In the short-run, illicit opioid deaths will increase (due to higher substitution), while in the long-run, a decline in the number of opioid users (due to the change in prescriber/dispenser behavior) may offset the increase.

In state B, policy spillovers would operate through several channels. One channel is the influx of users from state A who are displaced by the higher cost of accessing prescription opioids. If policy spillovers only operated through this channel, I would predict that in state B opioid volume would increase, prescription opioid deaths would decrease (due to substitution/exit by some displaced users), and illicit opioid deaths would increase, both in the short-term (due to greater substitution) and in the long-term (because there are no changes in prescriber/dispenser behavior).

Another channel through which policy spillovers could operate simultaneously is required reporting by non-resident pharmacies. From the point of view of state A, a non-resident pharmacy is a pharmacy located in state B that would mail prescriptions to residents in A. State A could require non-resident pharmacies in B to report mailed controlled substance prescriptions to the PDMP. While a reporting requirement is not the same as a mandatory access (use) requirement, pharmacists at non-resident pharmacies can potentially become more attentive not only to opioid prescriptions that are being mailed to the residents of state A, but also to all opioid prescriptions filled for residents of state A and even those filled for residents of state B. In this case, changes in dispenser behavior would affect the total pool of opioid users in state B. Therefore, I would predict that policy spillovers would increase opioid volume in the short-run (from the influx of opioid users) but would have an ambiguous effect in the long-run (from the competing effect of a smaller pool of users). In terms of mortality, prescription opioid deaths would still decrease. The effect on illicit opioid deaths would be ambiguous: they would increase in the short-run (due to greater substitution) but may reserve sign in the long-run (due to changes in dispenser behavior).

The predicted effects of PDMP implementation on different areas based on policy status and exposure to evasion and spillovers through neighbors with different policies are summarized in Table 2. In Section 6, I present empirical results for outcomes in the highlighted rows.

4 Data

4.1 PDMP policy dates

For dates when PDMP policies were effective in each state, I use the dates of *direct PDMP online access for prescribers*, or when prescribers were first able to directly access patient records through an online interface rather than having to request the records over phone or fax.[14] Horwitz et al. (2020) and Meinhofer (2018) conduct independent research of legal sources, state websites, and other administrative documentation to verify PDMP policy dates.[15] To address discrepancies in dates between the two studies, I independently verify the access dates, using their dates as a starting point and consulting legal documentation,

[14]When states adopt a PDMP for the first time, researchers draw a distinction between "enactment dates", "operational/implementation dates", and "access dates". Enactment dates are "the date at which a bill, regulation, or administrative action requiring dispensers or prescribers to send to an authority responsible for compiling prescription information [...] regarding written or dispensed prescriptions became law" (Horwitz et al., 2020). The definition for operational/implementation dates may vary and in some cases might overlap with the definition for access dates. Meinhofer (2018) defines implementation dates as "the date when dispensers started reporting [controlled substance] transactions to the database". Meanwhile, Horwitz et al. (2020) define operational dates as "the month and year that PDMP data first became accessible to any party authorized to access it [...] electronically (eg, not via phone or fax)". Access dates are the date when authorized users are granted access to patient reports. This can either be direct access (providers are able to query the PDMP directly to obtain a patient record) or indirect access (providers must submit a request to the PDMP) (Meinhofer, 2018).

[15]Several publicly available databases provide summary information on PDMPs, including relevant policy dates. Previous literature has primarily relied on either the National Alliance for Model State Drug Laws (NAMSDL) or the Prescription Drug Abuse Policy System (PDAPS) to identify program dates (NAMSDL: Kilby (2015), Patrick et al. (2016), Dave et al. (2020), and Grecu et al. (2019); PDAPS: Buchmueller and Carey (2018) and Borgschulte et al. (2018); Gihleb et al. (2018) do not cite their source for dates but reference Buchmueller and Carey (2018) and so most likely also use the PDAPS data). However, despite the widespread use of the dates in these sources, Horwitz et al. (2020) point out several problems with both NAMSDL and PDAPS dates:

> "Different sources often report strikingly divergent dates for the same or similar measure, including measures as foundational as whether a state had a PDMP in a given year. In addition, the methods used to construct the datasets are often unavailable, hindering the comparison of studies and the determination of which dataset is best suited to answer a particular policy question."

Horwitz et al. (2020) further demonstrate using data on prescriptions for Medicare beneficiaries that the choice of dates has a large effect on both the significance and the magnitude of the estimated coefficients. Similar problems and discrepancies in third-party information are highlighted by Meinhofer (2018).

PDMP FAQs, and other sources when necessary. The detailed process for date verification is described in Appendix B. The final list of access dates is in Column 2 of Table 8.[16]

Recent work on PDMPs has also focused on analyzing the impact of MA PDMPs. The mixed evidence on the effectiveness of voluntary PDMPs and the higher PDMP utilization rates when mandatory access laws are in place suggested that MA PDMPs would be the policies that were able to impact prescribing and other opioid-related outcomes. I focus on PDMP access to determine whether spillovers play a role in masking the main effect of the policy.

4.2 Opioid volume

Data on opioid volume from 2006 to 2014 comes from the Automation of Reports and Consolidated Orders System (ARCOS) collected by the Drug Enforcement Administration (DEA). ARCOS collects data on all legal transactions involving controlled substances, such as movement of inventory by manufacturers and sales of opioids to pharmacies, hospitals, clinics, practitioners, and substance abuse treatment centers.[17]

A limitation of the data is that it does not contain information on point-of-sale transactions to users such as prescriptions dispensed to patients. As a proxy measure for prescriptions, I use transactions involving non-military retail pharmacies and non-military practitioners. Because opioid prescriptions differ in their morphine content and strength, I calculate a standardized measure of opioid potency. I use information on the base ingredients for each opioid product from the National Drug Code Dictionary provided by the DEA to calculate the weight (in grams) for each component ingredient (e.g., codeine, dihydrocodeine, morphine sulfate, etc.). I then use conversion factors for oral opioids issued by the Centers for Medicare and Medicaid Services to convert the component weight to milligram morphine equivalent

[16]Missouri is the only state to still not have a state-wide PDMP. St. Louis County passed its own PDMP that became operational with prescriber access in January 2017. Since then, other Missouri counties have signed on to join the St. Louis PDMP. I account for this county-level PDMP using access dates from the St. Louis PDMP office.

[17]This data was released to the public as part of a multi-district civil action lawsuit against the largest manufacturers and distributors of opioid pills.

(MME) units. Finally, I aggregate the opioid quantity in MMEs at the quarter-year-county level and use annual county population to calculate MME per capita.

4.3 Opioid deaths

Data on opioid deaths from 2003 to 2016 are available through the CDC National Vital Statistics System (NVSS) Multiple Causes of Death (MCOD) files, which provide the census of deaths in the United States. I classify all deaths due to drug poisonings using ICD-10 underlying cause of injury codes X40-X44, X60-X64, X85, and Y10-Y14. I further classify opioid-related deaths using the following ICD-10 codes for contributing cause of death: T40.1 (heroin), T40.2 (natural opioids, such as oxycodone, hydrocodone, etc.), T40.3 (methadone), and T40.4 (synthetic opioids other than methadone, such as fentanyl). (Center for Disease Control and Prevention, 2013)

For my primary outcomes, I consider deaths due to the following two types of opioids: prescription opioids and illicit opioids. One challenge is how to attribute deaths due to synthetic opioids (T40.4). While synthetic opioids can be prescribed to patients as part of their treatment, in recent years a growing share of opioid-related mortality has been tied back to illicitly manufactured synthetic opioids (Center for Disease Control & Prevention, 2018a). Therefore, the T40.4 flag by itself is not informative about the origin of the drugs. However, illicit synthetic opioids are typically mixed with other illicit drugs, like heroin and cocaine, to increase the potency of the base drug, rather than being sold on their own (Center for Disease Control & Prevention, 2018a). Based on this, I define prescription opioid and illicit opioid deaths as follows using the contributing cause of death codes:

- **Prescription opioids:** all deaths that have been flagged for natural opioids (T40.2) and methadone (T40.3), including deaths with multiple contributing causes, plus all deaths that have been flagged for synthetic opioids (T40.4) but not heroin (T40.1), cocaine (T40.5), or psychostimulants (e.g., methamphetamine) (T43.6).[18]

[18]To be categorized as deaths due to Rx opioids, deaths flagged for T40.4 can include other contributing cause flags, so long as none of those flags are for heroin, cocaine, or psychostimulants.

- **Illicit opioids:** all deaths that have been flagged for heroin (T40.1), including deaths with multiple contributing causes, plus all deaths that have been flagged for synthetic opioids (T40.4) and either cocaine (T40.5) or psychostimulants (T43.6) (or both).

As a robustness check, I use alternative classifications for prescription opioid and illicit opioid deaths that have been used in previous literature. The definitions are presented in Panel B of Table 3. Results of the robustness check are discussed in Section 7.1.

Deaths are aggregated to the quarter-year-county level. County of death is attributed based on the country of residence of the deceased. Using population, I calculate mortality rates per 100,000 population as the outcome measure.

4.4 County-level controls

In the analysis I also use various political and demographic control variables. Information on political parties in charge of the executive and legislative branches in each state comes from the National Governors' Association and the National Conference on State Legislatures. Data on annual county-level median income, poverty rate, population, share of population over 60, and share of population by race (Hispanic, non-Hispanic Asian, non-Hispanic Black, and non-Hispanic White) comes from the US Census. Data on the annual county-level unemployment rate comes from the Bureau of Labor Statistics.

5 Empirical strategy

Recent work on PDMPs estimates the policy effect using a quasi-experimental approach that exploits the variation in the timing of PDMP implementation across states (Kilby, 2015; Patrick et al., 2016; Buchmueller and Carey, 2018; Meinhofer, 2018):

$$Y_{it} = \alpha_0 + \alpha_1 \times PDMP_{it} + \boldsymbol{\alpha}_W' \boldsymbol{W}_{it} + \lambda_i + \delta_t + \epsilon_{it} \tag{1}$$

where Y_{it} is the outcome of interest for jurisdiction (state or county) i in time period t.

$PDMP_{it}$ is a treatment indicator variable equal to unity for the first time period t in which the relevant PDMP policy (e.g, enactment, implementation, online access, mandatory access, etc.) has been operational in the corresponding jurisdiction. Coefficient $\hat{\alpha}_1$ estimates the static effects of the PDMP. $\boldsymbol{W}_{it}$ is the vector of control variables. Unit and time fixed effects are given by λ_i and δ_t, respectively, and ϵ_{it} is the unobserved error term.

The estimation of $\hat{\alpha}_1$ relies on the stable unit treatment value assumption (SUTVA), which states that a unit's outcome only depends on their own treatment assignment and not on the treatment assignment of any other units (Rubin, 1980).[19] If there are no spillovers, then Eq. 1 satisfies SUTVA. However, if the treatment status of a neighboring state can affect outcomes in a focal state, then SUTVA is violated and coefficient $\hat{\alpha}_1$ will not capture the treatment effect.[20]

I address this by accounting for evasion and spillovers directly, yielding a model that identifies the main effect of the policy separately from the effect in areas exposed to evasion and the effect in areas exposed to spillovers. I define a *neighborhood* as the set of counties adjacent to county c plus non-adjacent counties that have a geographic centroid within 100 miles of county c's geographic centroid. The definition is based on the assumption of the maximum distance that opioid users would be willing to travel to a neighboring market for prescription opioids. County c is insulated (either from evasion or spillovers) if all counties in its neighborhood have the same PDMP policy and is exposed if at least one neighbor has a different PDMP policy.

In each period t, I classify each county as one of four types—*fully treated*, *partially treated*, *contaminated control*, and *insulated control*—based on the PDMP implementation status in county c and among its neighbors. *Fully treated* counties are counties that have a PDMP and whose neighbors all have the same type of PDMP, meaning they are insulated from evasion. *Partially treated* counties are counties with a PDMP that have at least one neighbor with a

[19]The assumption of no spillovers between units is the first part of SUTVA. The second part of SUTVA is that there are no multiple versions of treatments that are hidden from the researcher (Rubin, 1980).

[20]In his evaluation of the "Moving to Opportunity" program, Sobel (2006) showed that in the presence of spillovers (also referred to as "interference") in a randomized experiment, the difference between the means of the treated and control groups does not capture the average treatment effect (ATE).

different policy, meaning they are exposed to evasion. *Contaminated control* (*contaminated*) counties are counties without a PDMP that have at least one neighbor with a different policy, meaning they are exposed to treatment spillovers. Finally, *insulated control* counties are counties without a PDMP that have no neighbors with a PDMP and are thus insulated from spillovers. The treatment assignment categories are summarized in the table below.

Table 1: County treatment types

	All neighbors have same policy **(insulated)**	∃ at least one neighbor with a different policy **(exposed)**
County c has PDMP	**Fully treated**	Partially treated
County c has no PDMP	Insulated control	Contaminated control

I then use the following difference-in-differences model to estimate the static effect of PDMP implementation:

$$Y_{ct} = \beta_0 + \beta_1 FullyTreated_{ct} + \beta_2 PartiallyTreated_{ct} + \beta_3 Contaminated_{ct} + \\ + \boldsymbol{\beta}'_X \boldsymbol{X}_{ct} + \lambda_c + \delta_t + \omega_{ct} \quad (2)$$

where Y_{ct} is the outcome in county c in quarter-year t; $\boldsymbol{X}_{ct}$ is the vector of controls; λ_c and δ_t are the unit and quarter-year fixed effects respectively, and ω_{ct} is the error term. $FullyTreated_{ct}$, $PartiallyTreated_{ct}$, and $Contaminated_{ct}$ are indicator variables equal to one based on the definitions above, with *insulated control* counties as the omitted category. As the treatments take exposure to evasion and spillovers into account, this specification satisfies SUTVA, so long as the definition of exposure follows the parametric form defined earlier.

County treatments from 2003 to 2016 are presented in Figure 2. Analysis of opioid quantity supplied covers years 2006-2014. After Nebraska and St. Louis County implement their PDMPs in 2017, there are no remaining *insulated control* counties in the sample. For this reason, the analysis sample for mortality is limited to 2003-2016.

For this estimation strategy to be valid, I make two identifying assumptions. The first is

that units do not anticipate treatment by adjusting their behavior (i.e., changing outcomes) before the start of treatment (no anticipation). The second is that treated units would have followed the same trend in outcomes as the control units if they had not received the treatment (parallel trends). While not able to test the assumptions directly, I can examine an event study, specifically the coefficients for the pre-treatment period (pre-trends). Flat pre-trends that are statistically indistinguishable from zero would provide suggestive evidence for that the requisite assumptions are reasonable to make.

In this setting, the parametric assumption for spillovers means that there are three treatments, therefore the requisite assumptions need to be assessed for each of them. As the maps in Figure 2 illustrate, *fully treated* is an absorbing treatment state – since no state dissolves its PDMP, once a county becomes *fully treated*, it cannot receive any other treatment. On the other hand, *partially treated* and *contaminated* are potentially transient treatment states. Since a county may experience one or more transient treatments, the pre-trends estimates may provide a noisy comparison of the control group and treatment group of interest. To address the issue of transient treatments, I focus on the counties' initial transition to evaluate pre-trends. I describe the methodology in more detail in Appendix D.

To provide supportive evidence for using the staggered timing of PDMP adoption for identification, I test the exogeneity of PDMP implementation to my variables of interest by examining whether state observable characteristics predict either the *likelihood* of policy implementation or the *timing* of policy implementation. I follow the approach in Deshpande and Li (2019).[21] I find that political factors predict the likelihood of policy implementation. Reassuringly, neither lagged mortality rate (one of the outcomes and a proxy for the severity of the crisis) nor the number of substance abuse treatment (SAT) facilities (a proxy for state allocation of resources to deal with the epidemic) are significant predictors of the likelihood of policy implementation. Moreover, none of the observable characteristics consistently predict the *timing* of policy implementation. This suggests that the *timing* of online access to

[21] Details of the analysis and the estimates tables are presented in Appendix C.

PDMPs is not related to the observed variables, even if which states implement the policy is, and lends validity to some of the assumptions required for identification.

Finally, I consider the appropriate estimation strategy. The outcomes of interest are opioid volume (measured in MME per capita) and deaths (measures as mortality rates per 100,000 population). Both are non-negative with many zeros and a long right tail in the distributions. One previously favored approach was to take a $\ln(Y + 1)$ transformation of outcome Y. However, this analysis may not yield robust estimates, so to address this, I also estimate the difference-in-differences and event study models using a Poisson pseudo-maximum likelihood (PPML) estimator (Santos Silva and Tenreyro, 2006, 2011). In the result tables, I provide estimates from all specifications for comparison.

All of the estimation samples are limited to counties that have not yet been treated by the start of sample. For the analysis of opioid volume, this excludes counties with any treatment (*fully treated*, *partially treated*, or *contaminated*) as of 2006 Q1. For the analysis of mortality, this excludes counties with any treatment as of 2003 Q1. Including already treated counties may bias the estimates if treatment effects are dynamic. All regressions include quarter-year and county fixed effects. Weighted regressions are weighted by county-year population. Standard errors are clustered at the state level.

6 Results and Discussion

Table 4 presents balance tests for each event study sample, comparing the characteristics of each of the treated groups to those of the *insulated control* group in the period before the start of treatment. While there are some differences between *fully treated* and *insulated control* counties, there are no observed differences between the *partially treated* and *insulated control* groups and between the *contaminated* and *insulated control* groups. In the regressions, I control for the three characteristics for which I observe differences between the treated and control groups (the share of population over 60, median income, and the unemployment

rate). Overall, I conclude that the *insulated control* counties make a suitable control group for each of the treated groups based on observable baseline characteristics.

Tables 5 through 7 present estimates from Eq. 2 that accounts for evasion and spillovers by comparing the change in outcomes in *fully treated*, *partially treated*, and *contaminated* counties to that in *insulated control* counties. As discussed above, I anticipate that the effects of a PDMP will be larger in *fully treated* counties compared to *partially treated* counties. The outcomes in the three tables are opioid volume (MME per capita), prescription opioid deaths (deaths per 100,000 population), and illicit opioid deaths (deaths per 100,000 population), respectively. Columns 1-4 show estimates from the OLS model and columns 5-8 show estimates from the PPML. Regressions in columns 1 and 5 do not include either control variables or populations weights, regressions in columns 2 and 6 only include weights, regressions in columns 3 and 7 only include control variables, and regressions in columns 4 and 8 include both control variables and weights. Column 8 shows estimates from the preferred specification.

The coefficients for *fully treated* counties estimate the main effect of PDMPs, in counties insulated from evasion. I find evidence that opioid volume and prescription opioid deaths decrease by 20.3% and 17.9%, respectively, in response to prescribers gaining direct online access to PDMPs.[22] This is consistent with the predictions that a PDMP would increase the cost of obtaining prescription opioids for patients and/or change prescriber/dispenser behavior. When I compare my results to those in the literature, I find a larger effect of PDMPs on these outcomes than previously estimated. Kilby (2015) finds that after PDMP implementation, there is an 11.1% decline in opioid volume and a 12.5% decline in prescription opioid deaths. Meinhofer (2018) finds no evidence of a statistically significant effect of PDMP direct access on either opioid volume or prescription opioid deaths.[23] Because of differences in the samples and policy dates between the previous studies and this paper, using

[22]The interpretation of a coefficient in a Poisson regression is $(\exp(\beta) - 1) = \%\Delta$ in outcome

[23]Kilby (2015) measures opioid volume in MME per capita per quarter for all Schedule II drugs. The sample is 2000-2013 covering the 38 states that never had an early PDMP (either "triplicate" or early electronic version). Meinhofer (2018) measures opioid volume in opioid grams converted to oxycodone potency units. The sample covers 2000-2013 and includes all states except for Florida.

my study sample I also estimate Eq. 1, in which the treatment group includes all counties with a PDMP and the control group includes all counties without a PDMP, regardless of exposure to either evasion or spillovers. The estimates from the no-spillovers regression for opioid volume and prescription opioid deaths are presented in Tables 11 and 12, respectively, in Appendix E. The no-spillover estimates show that PDMPs do not have a statistically significant effect on either opioid volume or prescription opioid deaths, similar to the estimates in Meinhofer (2018). My results suggest that including areas that are exposed to evasion in the treatment groups and/or areas that are exposed to spillovers in the control group may mask the main effect of the policy.

Table 7 shows that there is no statistically significant effect of PDMP access on deaths from illicit opioids in *fully treated* counties. Unlike in the case of opioid volume and prescription opioid deaths, these results are more in line with the previous literature. Meinhofer (2018) estimates no statistically significant effect of PDMP access on heroin mortality. Kilby (2015) estimates a 7% increase in heroin deaths in the first year after PDMP implementation, but this effect reverses and becomes statistically insignificant over time, suggesting that this is only a short-term effect.[24] My findings appear to be consistent with the explanation that increased substitution to illicit opioids is offset in the long-run by a decrease in the overall pool of opioid users. This suggests that any effects on illicit opioid deaths in exposed counties either with or without a PDMP may not be sufficient to significantly change my results compared to previous work.

I next explore whether the observed discrepancy of PDMP's main effect between my estimates and those in previous literature is due to evasion attenuating the PDMP effect in *partially treated* counties or spillovers affecting outcomes in *contaminated* counties. I find that in *partially treated* counties, opioid volume and prescription opioid deaths decrease by 16.0% and 12.5%, respectively, after prescribers gain PDMP access. While the magnitude of the point estimates is slightly lower than that of the estimates for *fully treated* counties, there is overlap in the confidence intervals. The coefficient for *partially treated* counties in

[24]The no-spillovers regression in Table 13 in Appendix E likewise shows no effect.

the regression on illicit opioid deaths is exactly the same as the coefficient for *fully treated* counties, and it is only statistically significant at the 10% level. This suggests there is little evidence that evasion significantly reduces the PDMPs' effectiveness or leads to differential substitution in counties with a PDMP that are exposed to neighbors without a PDMP.

In contrast, exposure to policy spillovers does seem to have a significant effect on outcomes in counties without a PDMP. I find evidence that opioid volume, prescription opioid deaths, and illicit opioid deaths decrease by 14.0%, 16.2%, and 24.2%, respectively, in *contaminated* counties. As the sign on the point estimates in *contaminated* counties is the same as the sign on the estimates in *fully treated* and *partially treated* counties, this may be one potential explanation for why previous studies have either underestimated or not shown a significant effect of PDMPs in counties with the policy.

I next discuss the mechanisms through which policy spillovers might be affecting exposed counties without a PDMP. The declines in opioid volume and illicit opioid deaths are not consistent with the predictions for *contaminated* counties if policy spillovers only operate through an influx of out-of-state opioid users. This means that policy spillovers are operating through an additional channel, specifically changes in dispenser behavior. Dispenser behavior in exposed counties without a PDMP could change through required reporting by non-resident pharmacies. Details on non-resident pharmacies are described in more detail in Section 2.3. As pharmacists at non-resident pharmacies have to report controlled substance prescriptions to PDMP states, this may increase their awareness of the opioid epidemic and of controlled substance prescriptions that they fill for both residents of the PDMP state and residents of their own state. As a result, the total number of opioid users in non-PDMP counties can also decrease over time. This mechanism could potentially explain the estimated decrease in opioid volume and illicit opioid deaths in *contaminated* counties.

Figures 3 through 5 show the event study plots for each of the three main outcomes. Panel A in each figure shows estimates from Eq. 5 where the treated counties' first transition is to being *fully treated*, Panel B shows estimates when the first transition is to being *partially treated*, and Panel C shows estimates when the first transition is to being *contam-*

inated. Panel A of Figure 3 (opioid volume) shows that the post-treatment event study coefficients are consistent with the pooled estimates from the difference-in-difference regression in Table 5. However, the same is not the case for the other event study plots, where the post-treatment coefficients do not reflect either the magnitude or the statistical significance of the point estimates. One potential explanation is that counties that transition to a given treatment directly after being *insulated control* and those that transition after another treatment experience different effects. The average of those effects would be captured by the point estimates but not by the event study coefficients. Despite the discrepancy between the event study and the difference-in-differences model, the balance tests in Table 4 and the results of the exogeneity of policy implementation test in Appendix C lend validity to the requisite assumptions for the identification of the PDMP effect.

7 Robustness checks

7.1 Alternative definitions of mortality

One concern is that I am not using the appropriate definition for prescription opioid deaths or that I am mis-attributing deaths from synthetic opioids to the wrong category of deaths. To test this, I estimate Eq. 2 with mortality outcomes following the definitions for prescription opioid deaths and illicit opioid deaths from the previous literature. These definitions are listed in Panel B of Table 3. Deaths due to "prescription opioids (K)" are defined as any deaths flagged for natural opioids (T40.2) (Kilby, 2015; Rudd et al., 2016; Ruhm, 2018). Deaths due to "prescription opioids (P)" are defined as any deaths flagged for either natural opioids (T40.2), methadone (T40.3), or synthetic opioids (T40.4) (Patrick et al., 2016). Finally, deaths due to "illicit opioids (R)" are defined as any deaths flagged for either heroin (T40.1) or synthetic opioids (T40.4) (Rudd et al., 2016; Ruhm, 2018). I run the same eight specifications (OLS and PPML, with different combinations of control variables and weights) as in the main regression tables. Regression tables for the robustness check are presented in Appendix F.

Table 14 presents results for "prescription opioids (K)" as the outcome. These are qualitatively similar to the results in Table 6. The point estimate is largest for *fully treated* counties, followed by *contaminated* counties and *partially treated* counties, although the confidence intervals for all estimates overlap. The magnitude of the estimates in Table 14 is larger than that of the estimates in Table 6, suggesting that the decrease in prescription opioid deaths is driven by a decrease in deaths flagged for natural opioids (T40.2). The estimates in Table 15, which presents results when I use "prescription opioids (P)" as the outcome, are also larger than the main set of estimates but smaller than the estimates in Table 14. This suggests that deaths related to methadone also decreased in *fully treated*, *partially treated*, and *contaminated* counties at this time.

Table 16 presents results for "illicit opioids (R)" as the outcome. These are likewise similar to the estimates in Table 7. PDMP access has a negative but statistically insignificant effect in *fully treated* and *partially treated* counties. The effect of the policy is negative and significant in *contaminated* counties. The results of the robustness check suggest that the choice of categorization for mortality does not affect the results. Additionally, the decrease in mortality due to natural opioids is even greater than suggested by the main results for prescription opioid deaths.

8 Conclusion

This study estimates the effect of PDMPs on opioid volume and opioid-related deaths while taking into account opportunities for evasion and neighbor spillovers that arise from policy non-coordination between states. I provide the first analysis that incorporates both exposure to evasion and exposure to spillovers in order to estimate the main effect of the PDMP.

I estimate the main effect of PDMP prescriber access by looking at counties that are insulated from evasion. I find that opioid volume and prescription opioid deaths decrease and there is no effect on deaths from illicit opioids. My findings suggest that PDMPs are

effective at moving the needle on prescription opioid-related outcomes and may do so without exacerbating other related outcomes in the long-run.

I also estimate the impact of evasion and policy spillovers in exposed counties. In exposed counties with a PDMP, I find a decrease in opioid volume and prescription opioid deaths and no significant effect on illicit opioid deaths. The estimates for insulated counties with a PDMP and exposed counties with a PDMP are similar in magnitude, suggesting that evasion does not significantly impact the effectiveness of PDMPs. I do find that policy spillovers affect outcomes in exposed counties without a PDMP. Opioid volume, prescription opioid deaths, and illicit opioid deaths all decrease in response to neighbors' PDMP policies. These findings suggest that policy spillovers are potentially operating through the channel of increased monitoring of controlled substance prescriptions.

The results in this paper suggest that spillovers may be a potential explanation for the mixed findings in previous literature about the effectiveness of PDMPs. Including counties without a PDMP that are being affected by their neighbor's policies in the control group may bias the estimates. Overall, non-coordination may not play a large role in dampening the effect of PDMPs by creating opportunities for evasion and may have positive spillovers to areas without the policy.

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Table 2: Predicted responses to staggered PDMP implementation

Outcome	**With PDMP, insulated from evasion**	**With PDMP, exposed to evasion**	**Without PDMP, exposed to spillovers** (substitution)	**Without PDMP, exposed to spillovers** (substitution and behavior change)	**Without PDMP, insulated from spillovers**
	(1)	(2)	(3)	(4)	(5)
Opioid volume*	↓	↓	↑	↑ or ↓	No change
Opioid users from state $I \in A, B$	↓	↓	↓	↓	No change
Users exiting	↑ (substitution) or ↓ (fewer users)	↑ or ↓	↑	↑ or ↓	No change
Users substituting to heroin	↑ (substitution) or ↓ (fewer users)	↑ or ↓	↑	↑ or ↓	No change
Prescription opioid mortality	↓	↓	↓	↓	No change
Illicit opioid mortality	↑ (substitution) or ↓ (fewer users)	↑ or ↓	↑	↑ or ↓	No change

* Opioid volume in state $I \in A, B$ depends on the number of users in state I prescription opioid market. In a setting without spillovers, this group is equivalent to the users from state I. In a setting with spillovers, this group depends on the number of state I residents staying in-state and the number of out-of-state opioid users coming to state I to access prescription opioids.

Table 3: List of mortality outcomes

	T40.1 (heroin)	**T40.2 (nat opioids)**	**T40.3 (methadone)**	**T40.4 (synth opioids)**
Panel A				
Prescription opioids	If flagged w/ T40.2 or T40.3 only	Any	Any	Only flag/flagged w/o T40.1, T40.5, or T43.6
Illicit opioids	Any	If flagged with T40.1 only	If flagged with T40.1 only	If flagged with T40.1, T40.5, or T43.6 only
Panel B				
Prescription opioids (K)	If flagged w/ T40.2 only	Any	If flagged with T40.2 only	If flagged with T40.2 only
Prescription opioids (P)	If flagged w/ T40.2, T40.3, or T40.4	Any	Any	Any
Illicit opioids (R)	Any	If flagged w/ T40.1 or T40.4 only	If flagged w/ T40.1 or T40.4 only	Any

Definitions in Panel B are attributed as follows:

- **Prescription opioids (K):** Kilby (2015), Rudd et al. (2016), and Ruhm (2018)
- **Prescription opioids (P):** Patrick et al. (2016)
- **Illicit opioids (R):** Rudd et al. (2016) and Ruhm (2018)

Table 4: Descriptive statistics: County characteristics

	Insulated control		Fully treated		
	mean	sd	mean	sd	Diff
Share population over 60	0.22	0.07	0.23	0.07	0.011**
Median income	43.24	10.33	45.46	10.82	2.228*
Poverty rate	15.92	6.15	16.28	6.19	0.364
Unemployment rate	6.34	2.81	7.43	3.34	1.097*
Share pop. Hispanic	0.21	0.23	0.19	0.22	-0.012
Share pop. non-Hispanic Asian	0.02	0.05	0.02	0.05	0.001
Share pop. non-Hispanic Black	0.04	0.07	0.04	0.07	-0.003
Share pop. non-Hispanic White	0.67	0.24	0.68	0.24	0.013
Legislature party, t-1	1.94	0.62	1.90	0.66	-0.038
Governor party, t-1	1.74	0.44	1.70	0.46	-0.039
Same party, t-1	0.66	0.47	0.59	0.49	-0.075
	Insulated control		Partially treated		
	mean	sd	mean	sd	Diff
Share population over 60	0.21	0.05	0.21	0.05	-0.001
Median income	41.52	9.76	41.16	10.23	-0.361
Poverty rate	14.67	6.61	15.58	6.31	0.910
Unemployment rate	5.54	2.31	5.67	2.32	0.132
Share pop. Hispanic	0.06	0.10	0.05	0.08	-0.009
Share pop. non-Hispanic Asian	0.01	0.01	0.01	0.01	-0.001
Share pop. non-Hispanic Black	0.07	0.15	0.09	0.17	0.020
Share pop. non-Hispanic White	0.82	0.19	0.81	0.19	-0.007
Legislature party, t-1	1.70	0.74	1.79	0.81	0.083
Governor party, t-1	1.43	0.49	1.38	0.49	-0.047
Same party, t-1	0.53	0.50	0.46	0.50	-0.070
	Insulated control		Contaminated		
	mean	sd	mean	sd	Diff
Share population over 60	0.21	0.05	0.20	0.05	-0.005
Median income	40.96	10.39	41.74	11.33	0.786
Poverty rate	14.74	5.75	14.90	5.97	0.157
Unemployment rate	5.69	2.21	5.76	2.10	0.073
Share pop. Hispanic	0.07	0.13	0.06	0.10	-0.012
Share pop. non-Hispanic Asian	0.01	0.02	0.01	0.02	0.001
Share pop. non-Hispanic Black	0.08	0.14	0.10	0.15	0.020
Share pop. non-Hispanic White	0.81	0.19	0.81	0.18	-0.008
Legislature party, t-1	1.96	0.72	1.87	0.74	-0.096
Governor party, t-1	1.57	0.50	1.48	0.50	-0.085
Same party, t-1	0.50	0.50	0.50	0.50	-0.005

Summary statistics are based on the event study samples used for the mortality analysis (2003-2016) and are calculated for the period preceding the start of treatment (PDMP online access implementation). "Diff" columns shows difference in the group means. Significance levels for t-statistics: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 5: Regression Estimates of Effect of PDMP on Opioid Volume

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
Fully treated	0.004	-0.165	-0.005	-0.211+	-0.079	-0.213**	-0.083	-0.227**
	(0.053)	(0.105)	(0.052)	(0.111)	(0.114)	(0.077)	(0.113)	(0.074)
Partially treated	0.022	-0.061	0.022	-0.081	0.099	-0.172*	0.086	-0.174*
	(0.061)	(0.110)	(0.058)	(0.100)	(0.133)	(0.078)	(0.123)	(0.073)
Contaminated	-0.059	-0.119	-0.055	-0.114	-0.081	-0.163*	-0.093	-0.151*
	(0.053)	(0.124)	(0.051)	(0.120)	(0.084)	(0.081)	(0.087)	(0.075)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	2.913	2.913	2.915	2.915	130.5	130.5	130.5	130.5
N	81504	81504	81296	81296	67500	67500	67400	67400
Clusters	44	44	44	44	43	43	43	43

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Outcome for OLS regressions: ln(MME per capita + 1); outcome for PPML regressions: MME per capita. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2006-2014. Sample excludes all counties that received any treatment before 2006 Q1 (counties in AL, KY, ME, NV, OK, OH, VA, and WV and all counties in their neighborhoods).

Table 6: Regression Estimates of Effect of PDMP on Prescription Opioid Deaths

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
Fully treated	-0.036	-0.108***	-0.036+	-0.113***	-0.047	-0.198**	-0.036	-0.197**
	(0.022)	(0.027)	(0.022)	(0.027)	(0.054)	(0.061)	(0.054)	(0.061)
Partially treated	-0.026	-0.064*	-0.025	-0.065*	-0.033	-0.135*	-0.030	-0.133*
	(0.022)	(0.028)	(0.021)	(0.027)	(0.051)	(0.057)	(0.052)	(0.055)
Contaminated	-0.037**	-0.075***	-0.036**	-0.074***	-0.038	-0.180***	-0.038	-0.177***
	(0.013)	(0.017)	(0.013)	(0.017)	(0.042)	(0.052)	(0.042)	(0.051)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.437	0.437	0.437	0.437	1.330	1.330	1.330	1.330
N	173292	173292	172932	172932	160020	160020	159772	159772
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).

Table 7: Regression Estimates of Effect of PDMP on Illicit Opioid Deaths

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
Fully treated	-0.069*	-0.091**	-0.068**	-0.089*	-0.317+	-0.228	-0.294+	-0.233
	(0.026)	(0.032)	(0.024)	(0.034)	(0.176)	(0.140)	(0.168)	(0.144)
Partially treated	-0.036	-0.026	-0.034	-0.023	-0.291*	-0.242+	-0.263+	-0.233+
	(0.022)	(0.034)	(0.021)	(0.034)	(0.148)	(0.135)	(0.141)	(0.139)
Contaminated	-0.041*	-0.064*	-0.039*	-0.062+	-0.229*	-0.279*	-0.211*	-0.278*
	(0.018)	(0.032)	(0.017)	(0.031)	(0.101)	(0.128)	(0.096)	(0.130)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.117	0.117	0.117	0.117	0.380	0.380	0.380	0.380
N	173292	173292	172932	172932	118580	118580	118300	118300
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).

Figure 1: PDMP Policy Implementation in the United States

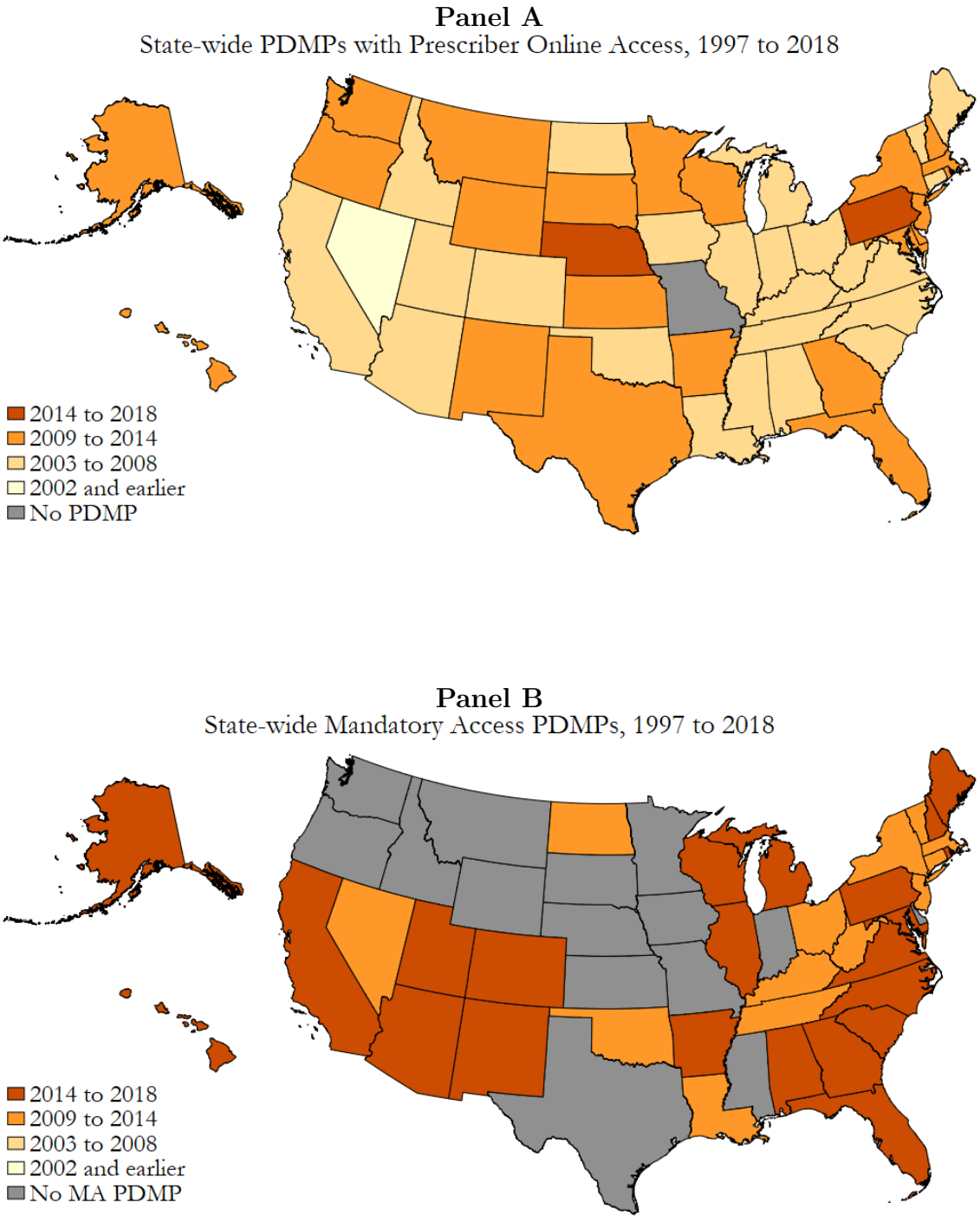
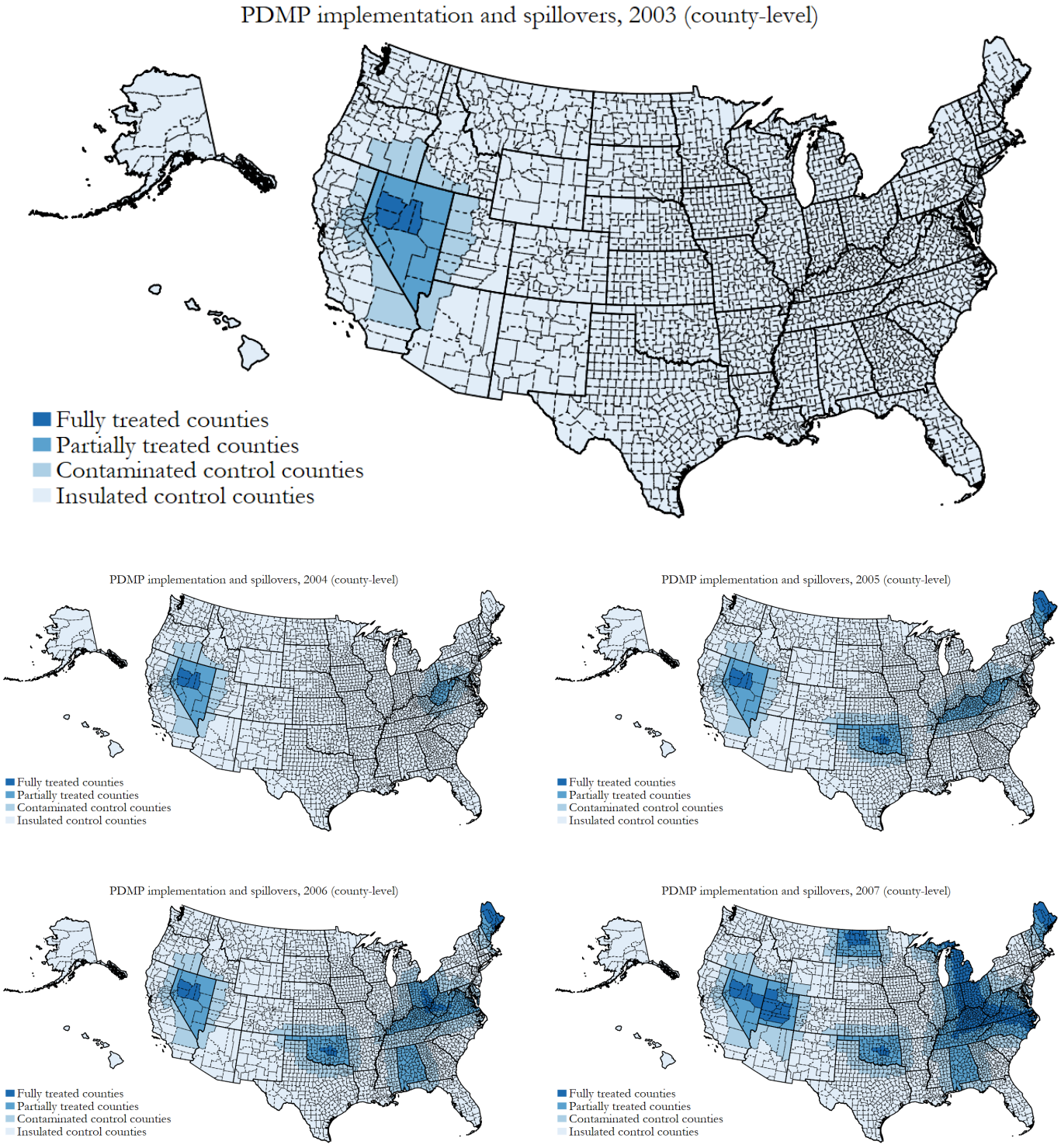


Figure 2: County treatments: PDMP with prescriber online access, 2003-2016



Notes: County treatments in 2014 and 2015 look the same. After Nebraska and St. Louis county implement their PDMPs in 2017, the overlap in county neighborhoods does not leave any insulated control counties in the sample. The opioid volume sample covers years 2006-2014, the mortality sample covers years 2003-2016.

Figure 2: County treatments: PDMP with online access, 2003-2018 (cont.)

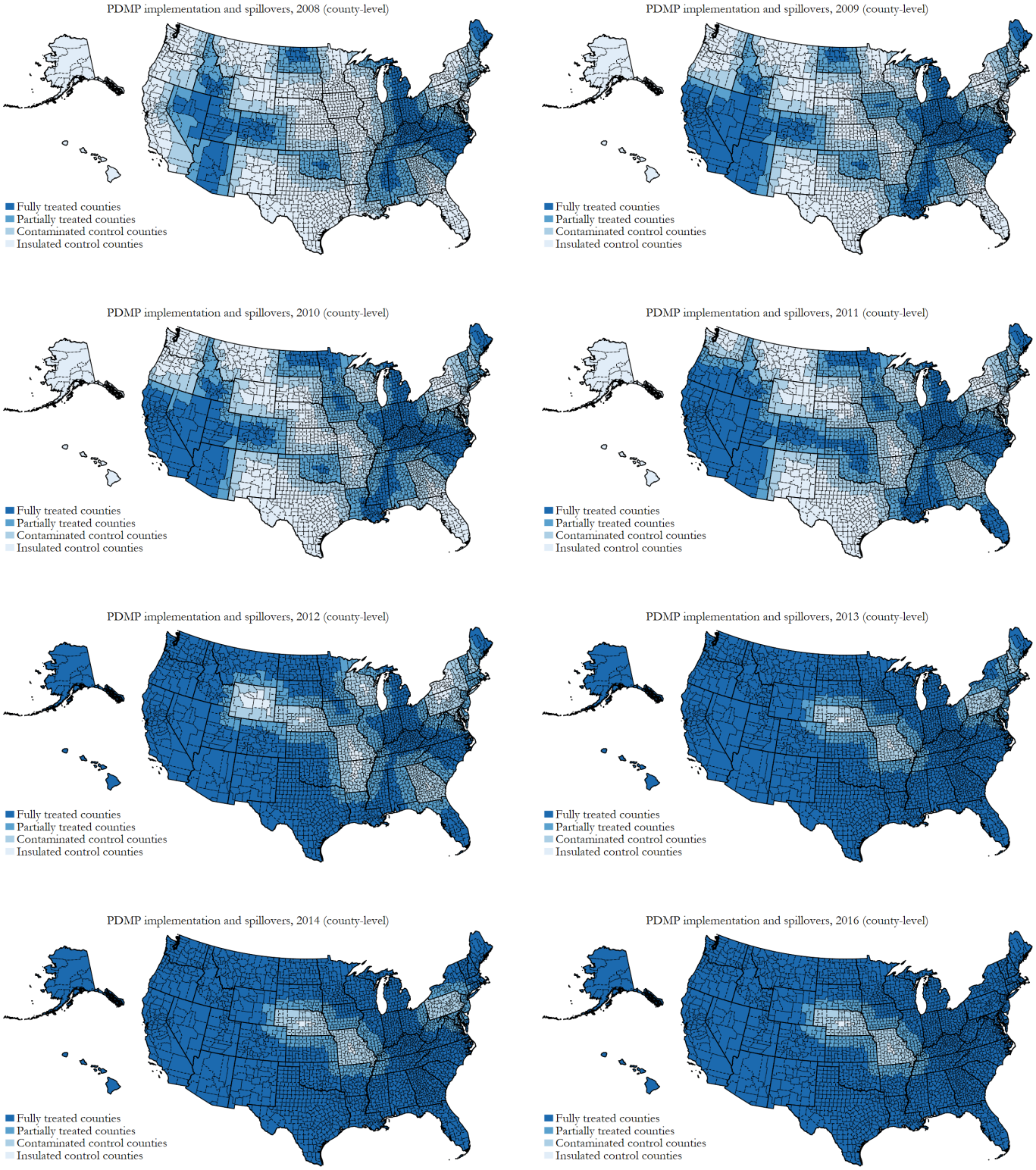
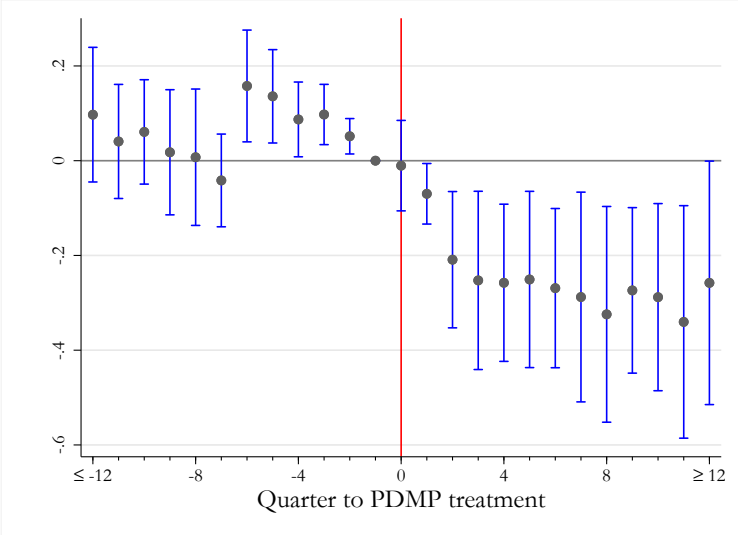
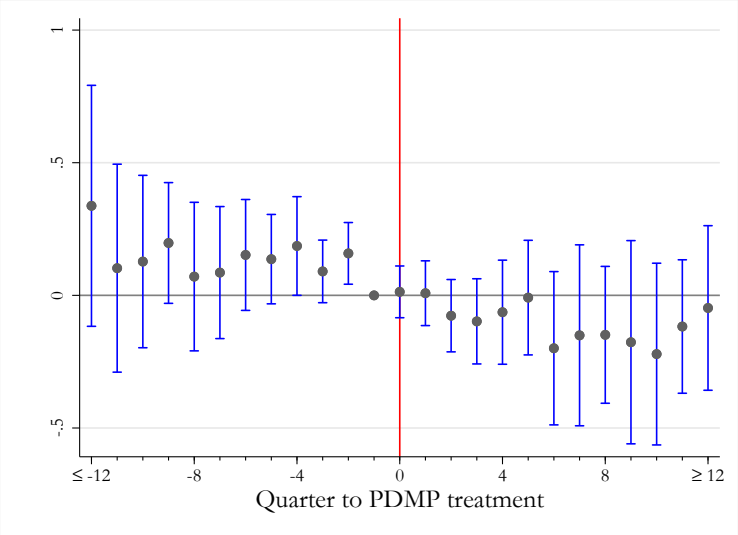


Figure 3: Event study estimates for opioid volume

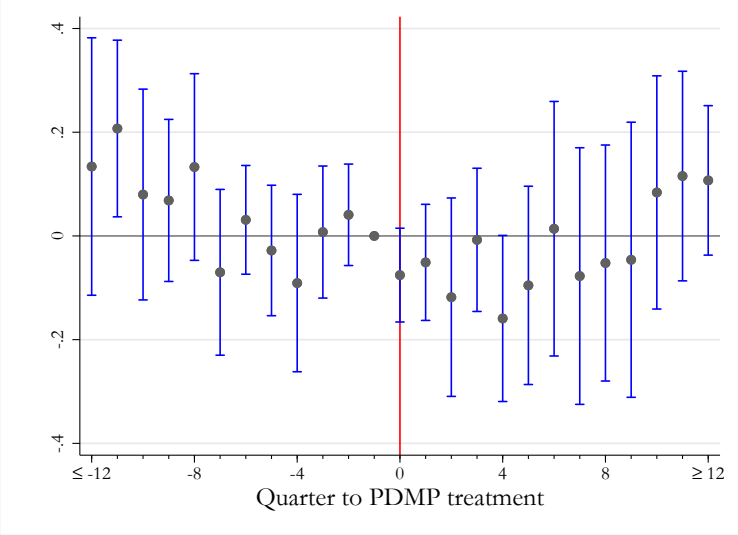
Panel A: fully treated vs. insulated control



Panel B: partially treated vs. insulated control



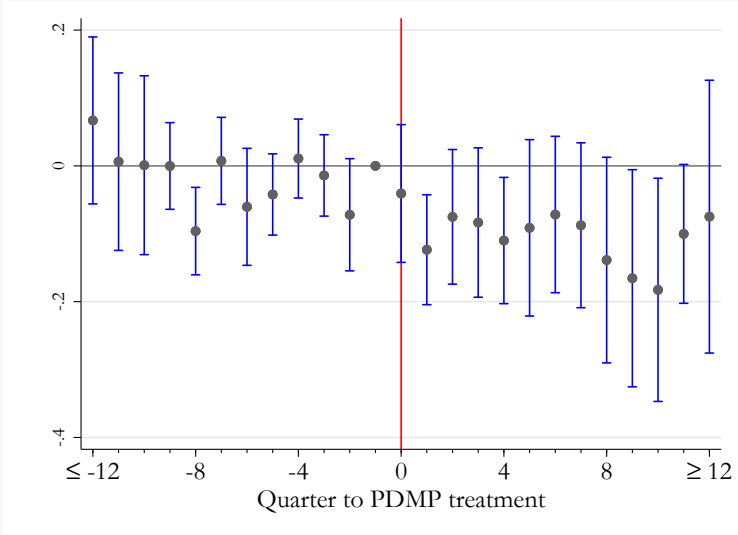
Panel C: contaminated control vs. insulated control



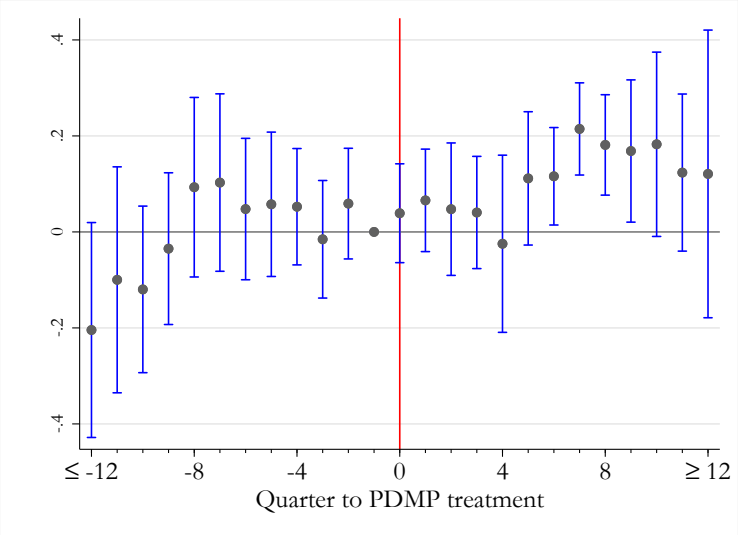
Notes: Data source: DEA ARCOS. Event study is estimated using three separate samples. Treated counties are determined based on the country's first treatment transition to either closed, exposed, or contaminated treatment. Sample covers 2006-2014. Sample excludes all counties that received any treatment before 2006 Q1 (counties in AL, KY, ME, NV, OK, OH, VA, and WV and all counties in their neighborhoods). For more details on sample construction, see Section 5. Event study estimates are calculated using Poisson pseudo-maximum likelihood estimator with county and quarter-year fixed effects, county-level controls (share of population over 60 years of age, median income, and the unemployment rate), and population weights.

Figure 4: Event study estimates for prescription opioid deaths

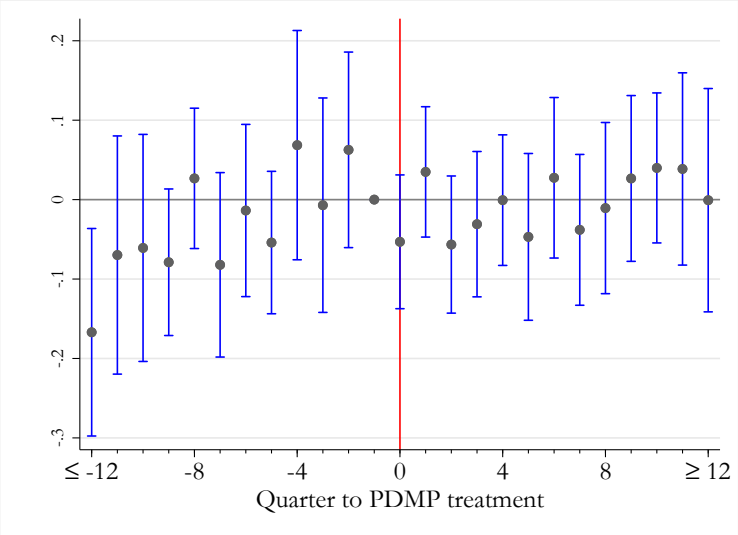
Panel A: fully treated vs. insulated control



Panel B: partially treated vs. insulated control



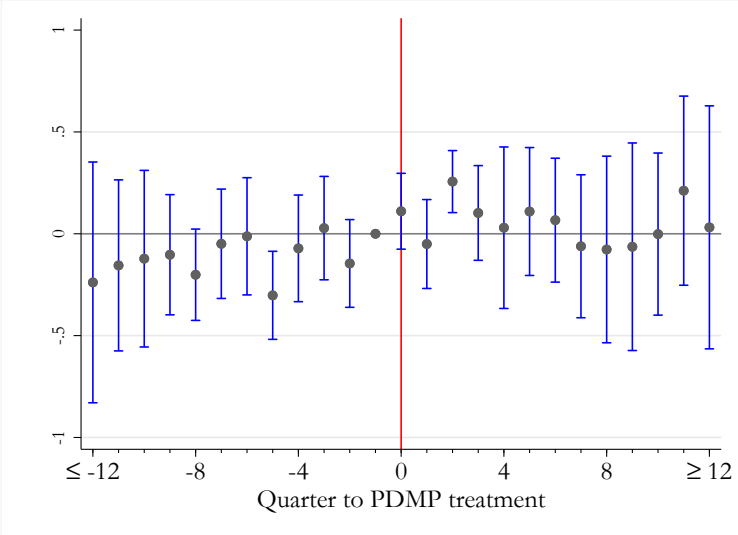
Panel C: contaminated control vs. insulated control



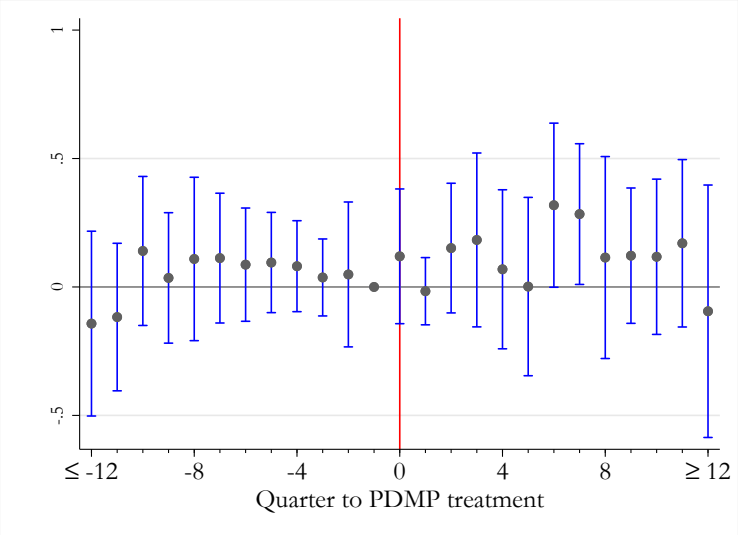
Notes: Data source: CDC NVSS MCOD. Event study is estimated using three separate samples. Treated counties are determined based on the country's first treatment transition to either closed, exposed, or contaminated treatment. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods). For more details on sample construction, see Section 5. Event study estimates are calculated using Poisson pseudo-maximum likelihood estimator with county and quarter-year fixed effects, county-level controls (share of population over 60 years of age, median income, and the unemployment rate), and population weights.

Figure 5: Event study estimates for illicit opioid deaths

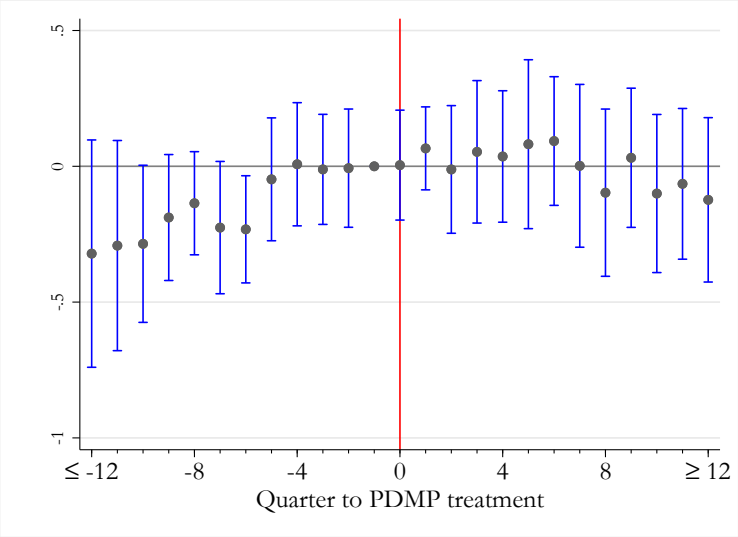
Panel A: fully treated vs. insulated control



Panel B: partially treated vs. insulated control



Panel C: contaminated control vs. insulated control



Notes: Data source: CDC NVSS MCOD. Event study is estimated using three separate samples. Treated counties are determined based on the country's first treatment transition to either closed, exposed, or contaminated treatment. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods). For more details on sample construction, see Section 5. Event study estimates are calculated using Poisson pseudo-maximum likelihood estimator with county and quarter-year fixed effects, county-level controls (share of population over 60 years of age, median income, and the unemployment rate), and population weights.

Appendix A PDMP policy dates

Table 8: Prescription drug monitoring program policy dates

State	**Online access**[a]	**Mandatory access**[b]	**Daily reporting**[c]
(1)	(2)	(3)	(4)
Alabama	Apr 2006	Mar 2017	Jan 2015
Alaska	Jan 2012	Aug 2017	Jul 2018
Arizona	Dec 2008	Oct 2017	Jul 2014
Arkansas	May 2013	Aug 2017[d]	Dec 2017
California	Sep 2009	Oct 2018	Jan 2021
Colorado	Feb 2008	May 2018	Jul 2014
Connecticut	Jul 2008	Oct 2015	Oct 2015
Delaware	Aug 2012	–	Mar 2012
District of Columbia	Oct 2016	Mar 2021	Aug 2016
Florida	Oct 2011	Jul 2018	Jan 2018
Georgia	May 2013	Jul 2018	Jul 2017
Hawaii	Feb 2012	Jul 2018	–
Idaho	Apr 2008	Oct 2020	Apr 2017[d]
Illinois	Dec 2009	Jan 2018	Jan 2012
Indiana	Jul 2007	Jan 2019	Jan 2016
Iowa	Mar 2009	Jun 2019[d]	May 2018
Kansas	Apr 2011	–	Jan 2013
Kentucky	Mar 2005	Jul 2012	Jul 2017[d]
Louisiana	Jan 2009	Aug 2014	Aug 2014
Maine	Jan 2005	Jan 2017	Jan 2016
Maryland	Dec 2013	Jul 2018	Jul 2019
Massachusetts	Dec 2010	Jan 2015	Nov 2015
Michigan	Apr 2007	Jun 2018	Jul 2014
Minnesota	Apr 2010	Jan 2021	Jan 2010
Mississippi	Jul 2008	–	–
Missouri (St. Louis)[e]	–	–	–
Montana	Nov 2012	–	Sep 2018
Nebraska	Jan 2017	–	Jan 2017
Nevada	Jun 1997	Oct 2015	Oct 2015
New Hampshire	Oct 2014	Jan 2017	Jan 2015[d]

Table continued on the next page.

Table 8: Prescription drug monitoring program policy dates (continued)

State	**Online access**[a]	**Mandatory access**[b]	**Daily reporting**[c]
(1)	(2)	(3)	(4)
New Jersey	Jan 2012	Nov 2015	Mar 2015
New Mexico	Jan 2012	Jan 2017	Mar 2015
New York	Sep 2013[d]	Sep 2013[d]	Sep 2013[d]
North Carolina	Jul 2007	Jul 2017[d]	Sep 2017
North Dakota	Sep 2007	Oct 2014	Sep 2007
Ohio	Oct 2006	Apr 2015	Jan 2016
Oklahoma	Nov 2005	Nov 2015	Nov 2010
Oregon	Sep 2011	–	–
Pennsylvania	Aug 2016	Jul 2015[d]	Jan 2017
Rhode Island	Sep 2012	Jul 2016[d]	Jul 2016
South Carolina	Feb 2008	Apr 2016	Jan 2014
South Dakota	Mar 2012	–	Jul 2017
Tennessee	Jul 2007	Apr 2013	Jan 2016
Texas	Aug 2012	Mar 2020	Sep 2017
Utah	Aug 2007	May 2018	Jan 2016
Vermont	Apr 2009	Nov 2013	Jan 2017
Virginia	Jun 2006	Jul 2016	Jan 2017
Washington	Jan 2012	–	Oct 2016
West Virginia	Dec 2004	Jun 2012	Jun 2012
Wisconsin	Jun 2013	Apr 2017	Apr 2017
Wyoming	Jul 2013	–	May 2017

Table 8 notes:

[a] "Access date" refers to the date when prescribers were first able to query the PDMP online to look up patient reports with the patient's prescribing history.

[b] "Mandatory access" refers to the date when it became legally required for a specific authorized party to query patient records in the PDMP prior to writing or dispensing a prescription. Column 2 corresponds to mandatory access dates aimed at prescribing physicians.

[c] "Daily reporting" refers to the date when it became legally required for dispensers of controlled substances to report dispensed substances to the PDMP on a daily basis. The definition of "daily reporting" varies slightly between states and could be explicitly defined in the regulation as either "by the next business day" (e.g., AR), "no later than the close of the next business day" (e.g., FL, IA, MI), or "no later than 24 hours after" the substance was delivered (e.g., NY, OH).

[d] Some policies went into effect in the last 5 days of the month. In these instances, the implementation date was listed as the following calendar month.

[e] Missouri does not have a state-wide PDMP implemented. St. Louis County implemented a PDMP in April 2017, which other counties in the state can join. The table lists policy dates for St. Louis County only. The regression analysis takes into account county-specific implementation dates for the rest of counties in Missouri.

Appendix B PDMP policy date verification

For PDMP implementation dates, previous literature primarily relies on dates compiled by publicly available databases such as National Alliance for Model State Drug Laws (NAMSDL) and the Prescription Drug Abuse Policy System (PDAPS). Horwitz et al. (2020) and Meinhofer (2018) point out various issues with these sources, such as lack of transparency about the details of the regulations to which the dates correspond and the discrepancies between sources about dates that allegedly correspond to the same type of PDMP policy. The authors conduct their own independent research of legal sources, state websites, and other administrative documentation, to verify PDMP implementation dates.

However, even between these two papers, there are still discrepancies between the implementation dates for the same regulation. Therefore, I independently verify the dates for PDMP and MA PDMP implementation using the dates in Horwitz et al. (2020) and Meinhofer (2018) as a starting point. I define the "PDMP implementation date" as the date when the PDMP became available online for prescribers to query patient reports, and the "MA PDMP implementation date" as the earliest date at which it became mandatory for prescribers to query the PDMP prior to prescribing opioids for any group of patients.

The verification process for PDMP dates is as follows:

1. Compare the list of PDMP implementation dates from Horwitz et al. (2020) and Meinhofer (2018).

    - Horwitz et al. (2020) define the PDMP implementation date (what they refer to as "operational date") as the month and year at which PDMP data became accessible to any party authorized to access it (e.g., physician or pharmacist)". This may be only physicians in some states or only pharmacists in others. The authors do not provide additional information on which programs were accessible by physicians. The operational date is restricted to dates when the full program rather than a pilot program became operational. ("We define the operational date as the month and year that PDMP data first became accessible to any party authorized to access it (e.g. physician or pharmacist) electronically e.g. not via phone or fax). Although some states operated pilot programs, allowing access for limited users, we report the date at which the full program became operational." (4))
    - Meinhofer (2018) uses the following definitions: "PDMP implementation is defined as the time when PDMP operations began, direct PDMP access is defined as the time when health-care providers were granted firsthand access to query the database, [...]"(506). I use the "direct PDMP access" dates for comparison purposes.

2. If the sources agree, I use the implementation date from the sources.

3. If the sources disagree, I independently verify the date by referencing official state sources, such as PDMP FAQ and "About" pages, legislative documents, annual reports, conference presentations, and others.

4. If unable to verify information independently through state source, I email the PDMP administrators directly.

5. If PDMP administrators respond to the inquiry, I use the dates provided by the administrators.

6. If PDMP administrators do not respond to the inquiry, I conduct another round of research.

7. If still unable to verify information independently, I refer back to the sources and determined which date to use on a case-by-case basis.

    - If only one source provides a date, I use the available date.
    - If one source provides the year and another month and year, and both agree on the year, I use the source that provides the month and year.
    - Based on the definition used by Meinhofer (2018) and Horwitz et al. (2020) for "implementation", Meinhofer (2018) dates should come chronologically after Horwitz et al. (2020) dates. If the Meinhofer (2018) date comes after Horwitz et al. (2020), I use the former date. If not, I use the Horwitz et al. (2020) date.
    - Where the month in the date given by Meinhofer (2018) is missing, I use January of the provided year.

Out of 51 dates for the 50 states and District of Columbia, Horwitz et al. (2020) and Meinhofer (2018) agreed on 20 dates. Through state documentation and communication with PDMP administrators, I was able to verify 26 additional dates. For the 5 remaining states, I used the procedure outlined in step seven above.

The verification process for prescriber mandatory access (MA) dates followed the same steps as for PDMP access dates. Out of 51 dates, the sources agreed on 5 dates and 14 absences of MA provisions targeting physicians. I was able to verify 44 additional dates, which included confirming that 9 states out of 14 "absences" still had not implemented MA provisions and that 5 had implemented them in 2018 or later. For the remaining two states, I use dates (or lack thereof) provided by Horwitz et al. (2020).

Appendix C Exogeneity of policy implementation

I test the exogeneity of PDMP policy implementation by using the approach in Deshpande and Li (2019). I test whether state observable characteristics predict either the *likelihood* of policy implementation or the *timing* of policy implementation during the 2005-2018 period. PDMPs and related policies are implemented after relevant state laws are enacted, which the political party of the state legislature and/or the governor can facilitate or stall depending on the party's current platform. States where the epidemic is more severe or that are overall allocating more resources to treat substance abuse disorders can also act earlier to adopt these policies. For policy dates, I focus on when prescribers gained direct online access to the PDMP.

To test the likelihood of policy implementation, for each year and for each policy, I test the following specification:

$$\begin{aligned} Implementation_s = \alpha_0 + \big(\alpha_1^G Governor1Yr_s + \alpha_5^G Governor5Yr_s\big) + \\ + \big(\alpha_1^L Legislature1Yr_s + \alpha_5^L Legislature5Yr_s\big) + \big(\alpha_1^P SameParty1Yr_s + \alpha_5^P SameParty5Yr_s\big) + \\ + \sum_{\kappa} \alpha_{\kappa}^M (Mortality_s^{\kappa}) + \sum_{\tau} \alpha_{\tau}^F (SATfacilities_s^{\tau}) + \epsilon_{st} \quad (3) \end{aligned}$$

where $Implementation_s$ is an indicator whether state s implements a policy in the future; $Governor1Yr_s$ ($Governor5Yr_s$) is the political party—Democrat, Republican, or Other—of the governor one (five) year(s) ago;[25] $Legislature1Yr_s$ ($Legislature5Yr_s$) is the dominant political party—Democrat, or Republican, or Other—of the state legislature one (five) year(s) ago;[26] $SameParty1Yr$ ($SameParty5Yr$) is an indicator equal to one if the parties of the governor and the legislature were the same one (five) year(s) ago; $Mortality_s$ is the state drug-related mortality rate per 100,000 population;[27] and $SATfacilities_s$ is the number of substance abuse treatment (SAT) facilities per 100,000 population. Lags for $Mortality_s$ are taken over the preceding $\kappa = 5$ years and lags for $SATfacilities_s$ are taken over the preceding $\tau = 3$ years. $Mortality_s$ is a measure of the severity of the epidemic in the state. $SATfacilities_s$ is a proxy for the level of resources that the government is currently allocating toward the opioid epidemic. The sample for Eq. 3 is all states that have not yet implemented the policy (whether or not they will implement one in the future).

[25]Data on gubernatorial parties is from the National Governors Association. With the exception of Vermont and New Hampshire, US governors serve four-year terms. Using lag terms for one and five years ago captures the party of the governor over two election cycles.

[26]Data on majority legislative parties is from the National Conference of State Legislatures. Terms for state representatives and senators vary across states. I use the majority legislative party one and five years ago as they would coincide with the governor.

[27]Data on state-level drug-related mortality comes from CDC Wide-ranging ONline Data for Epidemiologic Research (WONDER). Data is only available 1999-2018.

To test the timing of policy implementation, for each year and for each policy, I test the following specification:

$$ImplemYear_s = \beta_0 + \left(\beta_1^G Governor1Yr_s + \beta_5^G Governor5Yr_s\right) +$$
$$+\left(\beta_1^L Legislature1Yr_s + \beta_5^L Legislature5Yr_s\right) + \left(\beta_1^P SameParty1Yr_s + \beta_5^P SameParty5Yr_s\right) +$$
$$+ \sum_{\kappa} \beta_{\kappa}^M (Mortality_s^{\kappa}) + \sum_{\tau} \beta_{\tau}^F (SATfacilities_s^{\tau}) + \omega_{st} \quad (4)$$

where $ImplemYear_s$ is the year in which state s implements the policy. The sample in specifications 4 is all states that will implement the policy at some point during the 2005-2018 period but have not yet done so.

Results from select years are shown in the following tables in this section. The estimates from Eq. 3 suggest that some factors, such as the majority legislative party, do predict the likelihood of policy implementation. However, the estimates from Eq. 4 suggest that none of the selected factors consistently predict the timing of policy implementation. Most importantly, this suggests that treatment states do not differ from control states in terms of the severity of the opioid epidemic or state resources allocated to mitigate it, as proxied for by mortality and SAT facilities count. These findings suggest that the *timing* of policy implementation is effectively random, lending credibility to the quasi-experimental approach that exploits the timing of policy implementation.

Table 9: Likelihood PDMP with online access ever implemented

	(1)	(2)	(3)
	2005	2008	2011
Governor Republican, t-1	0.031	0.040	0.038
	(0.017)	(0.026)	(0.030)
Governor Other, t-1	0.063*	0.071*	0.064
	(0.027)	(0.034)	(0.050)
Governor Republican, t-5	0.033*	0.045	0.052
	(0.017)	(0.024)	(0.031)
Governor Other, t-5	0.003	0.015	0.047
	(0.014)	(0.021)	(0.041)
Legislature Republican, t-1	-0.040**	-0.060**	-0.077**
	(0.014)	(0.022)	(0.028)
Legislature Other, t-1	0.037*	0.038	0.044
	(0.015)	(0.020)	(0.033)
Legislature Republican, t-5	-0.052**	-0.065**	-0.086**
	(0.017)	(0.023)	(0.032)
Legislature Other, t-5	0.018	0.010	0.021
	(0.013)	(0.018)	(0.032)
Same party, t-1	0.048*	0.065*	0.082*
	(0.021)	(0.029)	(0.039)
Same party, t-5	0.040*	0.057*	0.084*
	(0.019)	(0.026)	(0.037)
Drug-related mortality rate, t-1	0.000	0.001	0.001
	(0.002)	(0.002)	(0.003)
Drug-related mortality rate, t-2	-0.000	0.000	0.001
	(0.003)	(0.003)	(0.004)
Drug-related mortality rate, t-3	0.001	0.001	0.001
	(0.003)	(0.003)	(0.005)
Drug-related mortality rate, t-4	-0.002	-0.004	-0.006
	(0.002)	(0.003)	(0.004)
Drug-related mortality rate, t-5	0.000	0.000	-0.001
	(0.002)	(0.003)	(0.004)
Number of SAT facilities, t-1	-0.001	-0.002	-0.001
	(0.007)	(0.010)	(0.013)
Number of SAT facilities, t-2	-0.003	-0.004	-0.007
	(0.009)	(0.012)	(0.017)
Number of SAT facilities, t-3	0.006	0.015	0.023
	(0.007)	(0.010)	(0.015)
Observations	686	518	336
Adjusted R^2	0.074	0.110	0.138

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Timing of PDMP with online access implementation

	(1) 2005	(2) 2008	(3) 2011
Governor Republican, t-1	0.454	0.572	-0.042
	(1.378)	(2.179)	(3.059)
Governor Other, t-1	5.302	9.344	0.455
	(5.677)	(5.617)	(3.184)
Governor Republican, t-5	0.294	-0.271	0.147
	(1.044)	(1.615)	(2.958)
Governor Other, t-5	-2.023	0.000	0.000
	(2.346)	(.)	(.)
Legislature Republican, t-1	0.126	-0.363	-0.012
	(1.789)	(1.760)	(1.998)
Legislature Other, t-1	1.511	4.243	7.096
	(2.167)	(3.090)	(4.071)
Legislature Republican, t-5	0.067	1.774	-0.607
	(2.354)	(1.825)	(1.443)
Legislature Other, t-5	0.295	-0.126	-2.114
	(1.817)	(1.310)	(3.153)
Same party, t-1	0.307	1.894	5.205
	(1.351)	(1.948)	(5.679)
Same party, t-5	0.065	-1.162	-1.658
	(1.758)	(1.315)	(4.746)
Drug-related mortality rate, t-1	-0.317	0.255	0.821
	(0.451)	(0.567)	(0.476)
Drug-related mortality rate, t-2	0.505	-0.279	-0.410
	(0.531)	(0.605)	(0.574)
Drug-related mortality rate, t-3	-0.351	0.239	-0.644
	(0.691)	(0.494)	(0.319)
Drug-related mortality rate, t-4	-0.272	-0.377	-0.300
	(0.571)	(0.594)	(0.693)
Drug-related mortality rate, t-5	0.355	0.064	0.513
	(0.586)	(0.467)	(0.347)
Number of SAT facilities, t-1	-1.550	-0.214	-0.117
	(1.465)	(1.216)	(1.528)
Number of SAT facilities, t-2	1.532	1.118	2.308
	(1.916)	(1.778)	(4.839)
Number of SAT facilities, t-3	0.314	-0.719	-1.863
	(1.100)	(1.154)	(3.314)
Observations	45	30	20
Adjusted R^2	-0.274	-0.173	0.575

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix D Event study: triple sample approach

In this section, I propose an approach to estimate an event study in the presence of transient treatments. The three treatment groups are defined based on PDMP implementation in the focal county c and PDMP implementation among its neighbors. The full set of treatment transition paths for counties in this setting can be summarized as follows:

Treatment transition

County	t	$t+1$	$t+2$	$t+3$
1	Insulated control	→ Contaminated	→ Partially treated	→ Fully treated
2	Insulated control	→ Contaminated		
3	Insulated control	→ Partially treated		
4	Insulated control	→ Fully treated		
5	Insulated control	→ Contaminated	→ Partially treated	
6	Insulated control	→ Contaminated	→ Fully treated	
7	Insulated control	→ Partially treated	→ Fully treated	
8	Insulated control			

Transient treatments could lead to noisy event study estimates. Consider an event study that wants to examine pre-trends for *partially treated* counties. If the researcher is using the full sample, in the pre-treatment period the event study would pool observations for counties of type 1, 3, 5, and 7 in the table above, i.e., *insulated control* counties and *contaminated control* counties. (A similar issue would arise when estimating pre-trends for *fully treated* counties.) Then, in the post-treatment period, as some counties switch to become *fully treated* counties earlier than others, post-treatment coefficients in the later periods would be estimated based on a treatment group that included both *partially treated* and *fully treated* counties.

To evaluate pre-trends for each treatment, I focus on the first treatment transition for each county, from *insulated control* to either *contaminated*, *partially treated*, or *fully treated* only.[28] I then compare the counties that receive each treatment separately to the *insulated*

[28]The recent econometrics literature that has been looking more carefully under the hood of difference-in-differences estimators does not provide a framework that can be readily applied to this setting. Imai et al. (2020) propose an estimator to capture the effect of a transient treatment by matching units based on their history of treatment, but their approach only extends to a single binary treatment. In a very recent working paper, de Chaisemartin and D'Haultfoeuille (2020) propose a robust and efficient way to estimate the effect of multiple binary treatments in a single regression, but one of the main underlying assumptions is that all treatments are absorbing. The authors suggest that their approach could work in a setting with multiple

control counties in the sample. Counties that first transition to a given treatment remain in the sample for the duration of that treatment. Once the county has transitioned to another treatment, those observations are dropped from the sample. The counties selected for the treatment group in each of the samples are highlighted in the table below (light blue – contaminated; medium blue – open; dark blue – closed). Only the baseline counties that eventually transition into that treatment and baseline counties that never receive any treatment are included in each sample's control group.

Treatment transition

County	t	$t+1$	$t+2$	$t+3$
1	Insulated control	→ Contaminated	→ Partially treated	→ Fully treated
2	Insulated control	→ Contaminated		
3	Insulated control	→ Partially treated		
4	Insulated control	→ Fully treated		
5	Insulated control	→ Contaminated	→ Partially treated	
6	Insulated control	→ Contaminated	→ Fully treated	
7	Insulated control	→ Partially treated	→ Fully treated	
8	Insulated control			

The event study specification can be generally written out as:

$$Y_{ct} = \tilde{\beta}_0 + \sum_{\tau \neq 0} \tilde{\sigma}_\tau \times SpilloverTreatment_{\tau,ct} + \tilde{\boldsymbol{\beta}}_X' \boldsymbol{X}_{ct} + \lambda_c + \delta_t + \varepsilon_{ct} \tag{5}$$

where $SpilloverTreatment \in \{FullyTreated, PartiallyTreated, Contaminated\}$.

Estimates for the outcomes using the event study specification in Eq. 5 are presented in Figures 3 through 5.

transient sequential treatment stages (as is the case for counties in this paper's setting), but this work is still exploratory.

Appendix E No-spillovers estimation of PDMP impact

Table 11: No-Spillovers Regression Estimates of Effect of PDMP on Opioid Volume

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
PDMP access	0.045	0.018	0.037	-0.017	0.033	-0.081	0.032	-0.098
	(0.045)	(0.092)	(0.045)	(0.096)	(0.080)	(0.063)	(0.083)	(0.063)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	2.915	2.915	2.917	2.917	136.3	136.3	136.4	136.4
N	102868	102868	102644	102644	85392	85392	85292	85292
Clusters	46	46	46	46	45	45	45	45

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Outcome for OLS regressions: ln(MME per capita + 1); outcome for PPML regressions: MME per capita. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2006-2014. Sample excludes all counties that received any treatment before 2006 Q1 (counties in AL, KY, ME, NV, OK, OH, VA, and WV and all counties in their neighborhoods).

Table 12: No-Spillovers Regression Estimates of Effect of PDMP on Prescription Opioid Deaths

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
PDMP online access	-0.009	-0.047*	-0.009	-0.049*	-0.017	-0.076	-0.010	-0.075
	(0.017)	(0.021)	(0.017)	(0.021)	(0.041)	(0.048)	(0.041)	(0.048)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.439	0.439	0.439	0.439	1.336	1.336	1.336	1.336
N	175028	175028	174668	174668	161700	161700	161452	161452
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).

Table 13: No-Spillovers Regression Estimates of Effect of PDMP on Illicit Opioid Deaths

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
PDMP online access	-0.027	-0.026	-0.027	-0.023	-0.165	-0.052	-0.150	-0.049
	(0.020)	(0.026)	(0.020)	(0.027)	(0.133)	(0.101)	(0.129)	(0.102)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.117	0.117	0.117	0.117	0.378	0.378	0.378	0.378
N	175028	175028	174668	174668	120092	120092	119812	119812
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).

Appendix F Robustness check: Alternative definitions of mortality

Regression results using alternative definitions of prescription opioid and illicit opioid mortality are presented in this section. Table 14 presents estimates for prescription opioid mortality as defined in Kilby (2015); Rudd et al. (2016); Ruhm (2018) as all deaths flagged for natural opioids (T40.2). Table 15 presents estimates for prescription opioid mortality as defined in Patrick et al. (2016) as all deaths flagged for at least one of natural opioids (T40.2), methadone (T40.3), or synthetic opioids (T40.4). Rudd et al. (2016) and Ruhm (2018) use an alternative approach to categorize deaths due to synthetic opioids. Table 16 presents estimates for illicit opioids defined as all deaths flagged for at least one of heroin (T40.1) or synthetic opioids (T40.4).

Table 14: Diff-in-diff: Prescription drug (K) mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	OLS	OLS	OLS	OLS	Poisson	Poisson	Poisson	Poisson
Fully treated	-0.029	-0.106***	-0.031	-0.111***	0.005	-0.266***	0.012	-0.268***
	(0.022)	(0.024)	(0.021)	(0.025)	(0.077)	(0.067)	(0.077)	(0.069)
Partially treated	-0.041+	-0.070**	-0.040+	-0.072**	-0.029	-0.184**	-0.026	-0.183**
	(0.023)	(0.025)	(0.023)	(0.024)	(0.067)	(0.064)	(0.068)	(0.064)
Contaminated	-0.050**	-0.073**	-0.048**	-0.073**	-0.064	-0.215**	-0.064	-0.213**
	(0.015)	(0.021)	(0.014)	(0.021)	(0.056)	(0.068)	(0.057)	(0.069)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.292	0.292	0.292	0.292	0.863	0.863	0.864	0.864
N	173292	173292	172932	172932	154336	154336	154116	154116
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).

Table 15: Diff-in-diff: Prescription drug (P) mortality

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
Fully treated	-0.047+	-0.123***	-0.047+	-0.128***	-0.068	-0.221**	-0.056	-0.217**
	(0.024)	(0.029)	(0.024)	(0.029)	(0.059)	(0.073)	(0.058)	(0.075)
Partially treated	-0.033	-0.076*	-0.032	-0.077*	-0.045	-0.169*	-0.043	-0.166*
	(0.024)	(0.033)	(0.024)	(0.032)	(0.055)	(0.076)	(0.056)	(0.075)
Contaminated	-0.044**	-0.092***	-0.043**	-0.092***	-0.053	-0.231***	-0.054	-0.229***
	(0.015)	(0.020)	(0.015)	(0.020)	(0.047)	(0.064)	(0.047)	(0.064)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.449	0.449	0.449	0.449	1.378	1.378	1.378	1.378
N	173292	173292	172932	172932	160356	160356	160108	160108
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).

Table 16: Diff-in-diff: Illicit drug (R) mortality

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Poisson	(6) Poisson	(7) Poisson	(8) Poisson
Fully treated	-0.080*	-0.104**	-0.080**	-0.103*	-0.151	-0.178	-0.134	-0.169
	(0.030)	(0.037)	(0.028)	(0.040)	(0.116)	(0.122)	(0.108)	(0.125)
Partially treated	-0.046	-0.035	-0.043	-0.032	-0.075	-0.162	-0.063	-0.148
	(0.028)	(0.042)	(0.026)	(0.041)	(0.115)	(0.131)	(0.111)	(0.131)
Contaminated	-0.055*	-0.086*	-0.053*	-0.084*	-0.133	-0.289*	-0.129	-0.284*
	(0.021)	(0.036)	(0.020)	(0.035)	(0.082)	(0.113)	(0.079)	(0.114)
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Quarter-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	NO	YES	YES	NO	NO	YES	YES
Weighted	NO	YES	NO	YES	NO	YES	NO	YES
Mean	0.212	0.212	0.213	0.213	0.622	0.622	0.622	0.622
N	173292	173292	172932	172932	146860	146860	146580	146580
Clusters	50	50	50	50	50	50	50	50

Robust standard errors in parentheses. SE clustered at the state level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Notes: Outcome for OLS regressions: ln(deaths per 100,000 population+1); outcome for PPML regressions: deaths per 100,000 population. Controls are annual county-level share of population over 60, median income, and unemployment rate. Weights are by population. Sample covers 2003-2016. Sample excludes all counties that received any treatment before 2003 Q1 (counties in NV and all counties in their neighborhoods).