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ABSTRACT

The Mitigating Role of Prescription Drug Monitoring Programs in the Abuse of Prescription Drugs*

In response to the epidemic of prescription-drug abuse, now 49 US states have passed legislation to establish Prescription Drug Monitoring Programs (PDMPs). These programs track controlled-substance prescribing and usage behavior in an effort to improve patient outcomes and identify and preempt access by drug abusers. We exploit variation in the timing of implementation across states to identify the effectiveness of PDMPs on reducing opioid abuse. In particular, by considering the role of specific program attributes we offer the strongest evidence to date of the potential for PDMP-type policy to decrease opioid-related treatment admissions. We also consider heterogeneity across intensity and tenure of use, which reveals that the largest gains are coming from reductions in the number of less-attached users. Overall, these results have important implications for the effective re-design of PDMP policy.

JEL Classification: I12, I18, K42
Keywords: prescription drug, drug treatment, opioid, abuse

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1 Introduction

Drug overdose is the leading cause of accidental death in the United States. Since 1999, rates of overdose death, drug-treatment admissions, and prescription-drug sales have increased by nearly four times, with prescription drugs now accounting for roughly 40 percent of overdose deaths.[1] Ruhm (2017) has recently demonstrated that opioid deaths are likely much higher than these measures suggest. Prescription-drug abuse began to escalate in the late 1990s—a time when state medical boards were moving toward relaxing restrictions on prescribing opioids for the treatment of chronic pain. Over the same time period, new pain-management standards began to focus on the patient's right to pain reduction, adding pain to a physician's standard checklist along with blood pressure, heart rate, temperature, and respiratory rate.[2] This, along with aggressive marketing and promotion of opioid pain relievers by pharmaceutical companies, physicians significantly increased the number of prescription pain relievers prescribed to patients (Manchikanti et al., 2012). In 2010, the National Survey on Drug Use and Health reported that the second-most-commonly abused illicit drug—second to marijuana—was opioid pain reliever, with one-in-six users indicating that they received the drugs though a physician.[3]

In an attempt to curb growing opioid pain reliever misuse, federal and state governments have implemented legislation and allocated funding to various programs targeting the supply

[1] Center for Disease Control and Prevention, National Center for Health Statistics, National Vital Statistics System, Mortality File. (2015). Number and Age-Adjusted Rates of Drug-poisoning Deaths Involving Opioid Analgesics and Heroin: United States, 2000–2014. Atlanta, GA: Center for Disease Control and Prevention. Available at `http://www.cdc.gov/nchs/data/health_policy/AADR_drug_poisoning_involving_OA_Heroin_US_2000-2014.pdf`

[2] In 2016, the American Medical Association passed a resolution recommending that pain be removed as a vital sign.

[3] For additional consideration of the increase in abuse and in state responses, consider Jones et al. (2015); Warner et al. (2011); Compton et al. (2015); Delcher et al. (2016).

and demand side of the prescription opioid market. While Prescription Drug Monitoring Programs (PDMPs) had been established in many states prior to the onset of the opioid epidemic, they have since been promoted by the CDC as some of the best defenses against the impending crisis.[4] Currently, 49 states—all but Missouri—now have PDMPs (Islam and McRae, 2014). Although the specific elements of PDMPs vary widely by state—considering this heterogeneity will be my focus—these programs provide, at a minimum, an electronic database through which information is collected about patients, drugs being prescribed, and prescribing physicians. Access to these systems allows for the observation of patient-specific prescription histories, with the potential (in some states) to preempt or otherwise disrupt legal misuse, illicit acquisitions, and the reselling of prescription drugs.

Existing literature on the effectiveness of PDMPs have found little-to-no benefit associated with their broad introductions (Haegerich et al., 2014). Yet, PDMPs have varied quite widely in their implementations, with "best practices" around formal requirements and practices being slow to develop. PDMP policies are often passive and far removed from patient interactions, so much so that passing a broad efficacy test would be surprising. For example, while all states require pharmacists to report prescription information to a database, some states have limited access to the database to law-enforcement agencies. In this context, then, we address the potential that there are yet gains associated with well-designed monitoring programs, exploring the efficacy of several component practices. This ultimately reveals that those states that require consultation with the database somewhere within the process of prescription and dispersement are more effectively curbing problematic-prescribing behavior

[4]Roughly 60 percent of implementing states do not mention "overdose" or related terms in their stated objectives or missions statements (Green et al., 2015).

than are states that do not make such a requirement, where they will typically require operating agencies only to *ex post* identify suspicious prescribing and use. In the end, we offer the strongest evidence to date of the potential for the most-aggressive PDMP-type policies to decrease opioid-related treatment admissions, so much so as to prescribe a potential "best practice."

In Section 2, we consider the related literature, where the story has thus far been somewhat discouraging in so far as opioid-related treatments and overdose deaths have not declined with PDMP implementation in general. In Section 3, we describe the broad patterns in the implementation of PDMSs, and consider the efficacy of specific program attributes. Recent evidence suggests the existence of gains when considering specific PDMP designs. We develop my empirical specifications and report results in Section 4. It is in this section that we establish the efficacy of monitoring programs—in a way that will be consistent with priors—and consider where in distributions of intensity and tenure of use the declines are arising. In short, we will look for missing mass in the distribution of treatment admissions across intensity and tenure of use, and argue that gains are coming from among less-frequent and newer users, in states with the most-aggressive PDMP policies. Before concluding, we will also consider the fallout from PDMP implementation on overdose deaths. In 5, we summarize with a discussion of policy prescriptions.

2 Literature and policy background

While the literature related to prescription drug monitoring programs is growing, previous evaluations have focused largely on the impact of the existence of these databases on various

outcomes such as prescribing behavior and patient health. In evaluating physician response to these programs, indications of database use predominantly come from voluntary surveys used to gauge, additional information they may provide, and barriers to use. Using a nationally representative survey of providers, Rutkow et al. (2015) suggest several barriers to physician use of these databases including difficulty navigating the format of programs and the time consuming nature of accessing the database. These barriers may explain low use by prescribing physicians. In an anonymous survey given to prescribers in Connecticut and Rhode Island, Green et al. (2012) finds PDMPs slightly influence physician behavior, specifically when the programs were electronically available though increase drug abuse screening, and substance abuse treatment referrals.

In evaluations using state aggregates, Simeone and Holland (2006) finds a slight decrease in prescriptions among physicians, consistent with physicians reacting to the regime change in the desirable way. However, evidence on the number of prescription opioids dispensed are less conclusive with Brady et al. (2014) finding a reduction in per capita morphine milligram equivalents associated with PDMPs only after 2008.

The effectiveness of these programs on health outcomes is mixed, with estimates suggesting that these programs in general do not significantly affect drug-overdose rates and may have only small negative effects on drug-treatment admissions. Pacula et al. (2015) evaluates the effect of Medicare Part D introduction on substance abuse treatment admissions and overdose deaths and, in doing so, includes a control for state level PDMPs. The authors find insignificant point estimates of these program indicators suggesting these programs have no effect on the outcomes of interest.

In a difference-in-differences framework, both Radakrishnan (2013) and Paulozzi et al.

(2011) directly explore the effect of PDMPs on health outcomes. Paulozzi et al. (2011) finds no significant effect on drug-related mortality or overdose rates, while Radakrishnan (2013) finds small negative effects on opioid-related treatment admissions and reported drug use associated with states having any form of these programs. After controlling for potentially confounding state laws addressing drug abuse, Radakrishnan (2013) finds no significant effects of the existence of these programs on drug-related mortality. Li et al. (2014) and Reifler et al. (2012) find opposite effects, with PDMPs being associated with increases in treatment admissions, poison center exposures, and mortality, while Maughan et al. (2015) finds no association between PDMP exposure and opioid-related emergency room visits. In a more recent analysis, Kilby (2016) explores a wider variety of health outcomes finding that the implementation of a PDMP post 2003 reduces overdose deaths through a reduction in prescribing of opioids however, this supply side restriction also leads to more invasive and expensive pain management techniques as well as more work days missed among injured workers.

While the majority of existing evidence suggests that the effects of these informational databases on the epidemic of opioid abuse are small, previous literature has by and large not accounted for the substantial variation in the attributes of PDMP across states or for the potential that there are offsetting effects on the intensive margin of use. While many public health researchers have indicated the need for detailed evaluation of PDMPs, few empirical studies have addressed individual characteristics of these programs (Griggs et al., 2015; El Burai Félix and Mack, 2014). Of course, evaluating PDMPs without regard for the cross-state variation can hide the promising effects of specific practices. Amid somewhat discouraging patterns in the aggregate, we contribute to supporting a "best practice," of a

sort.[5]

Specifically, we will report on two areas of entity access—access requirements around the database by the prescribing entity, and access requirements around the database by the operating agency. To begin, my priors suggest that "must-access" provisions of a PDMP may be the most effective in curbing abuse. A "must-access" provision requires prescribers to check the database before prescribing opioids to patients.[6] If the largest impacts of supply side restrictions come from greater information to the physician at the point of prescribing, i.e., at the point of physician-patient contact, we expect states with the "must-access" provision to show the largest declines in opioid related abuse. A less stringent but more-common attribute of state PDMPs is the ability for physicians to access the program's data if they wish to, but with no requirement to do so before prescribing (as would be required under the "must-access" provision above). For example, PDMPs that allow physician access may more-directly affect prescribing behavior than those programs that restrict access of the database to non-prescribing entities such as law enforcement. Although this is a less binding requirement, if access to the database changes prescribing behavior at the point of physician-patient contact, states with this provision may find PDMPs more effective in curbing abuse.

While the attributes of PDMPs described above have the potential to directly affect the decision to prescribe opioids at the time of contact with a patient, states also vary in their requirements that the agencies operating the PDMPs actively check the databases for suspicious prescribing and usage behavior. This requirement is post prescribing, and thus

[5]With 49 states now players in this policy environment, we will collapse my reported analysis to where there is systematic variation in outcomes, which ends up being around the most-aggressive PDMP attributes, arguably. In unreported analysis, we have considered a much broader array of attributes, finding no systematic movement in outcomes through my identifying variation.

[6]Although we suggest this attribute is the most restrictive in terms of prescribing behavior, subjectivity in implementation remains for this category and thus may attenuate results

should not directly interfere at the point of prescribing between a physician and patient. Although my priors suggest this requirement may have less of an impact that those that bind at the point of physician-patient contact, required checking of the database may identify problem prescribers and users leading to reductions in overall opioid sales. In a similar vein to above, proactive checking of the database can instead be at the operating agency's discretion (i.e., proactive checking is not required but is allowed). This is a less binding requirement on the operating agency however, if agencies sufficiently check the database, it may simply be the ability to check which becomes the most effective attribute of a state PDMP.

Patrick et al. (2016) also considers PDMPs at the attribute level, and finds larger reductions in opioid-related overdose deaths in states that monitored at least four drug schedules and updated reported drug information at least weekly. In an evaluation of opioid abuse revealed through Medicare claim patterns, Carey and Buchmueller (2016) find reductions in misuse associated with states that require prescribing entities to consult the database when issuing prescriptions under certain conditions. Both studies suggest there are gains to be found in specific attributes of PDMPs. We evaluate those programs with the most-binding requirements for physicians and then evaluate those programs that are less stringent in prescriber expectations to determine which program designs are most effective in reducing opioid misuse and overdose death.

3 Data

There are four sources of data brought together in the consideration of PDMP implementation and any resultant effect on opioid-related treatment and death.

3.1 PDMP implementation

Our independent variables of interest—we will be considering the "effective date" of each state's PDMP between 1998 and 2012 as well as the attributes of these programs—are obtained from the Prescription Monitoring Program database curated by Corey Davis at The Network for Public Health Law and the PDMP Center of Excellence at The Heller School for Social Policy and Management at Brandeis University.[7] The effective dates used in this analysis are the date the statue establishing a prescription drug monitoring program was put into effect.[8] Given the potential lag between effective date and the associated policies actually being administered and/or fully implemented, measurable efficacy may not be immediate. Moreover, estimates may be attenuated to the extent resources are slow to respond to the policy change. Although a small number of states had passed legislation establishing PDMPs prior to 1998 and thus will not add to identification of the effect of the existence of a PDMP, they will provide identifying variation in considering the efficacy of post-1998 amendments.

3.2 Treatment admissions

The Treatment Episodes Data Set (TEDS) is publicly available through the Substance Abuse and Mental Health Services Administration. Collected annually, the TEDS provides information on the number of drug-treatment admissions for all treatment facilities that receive public funding, whether though federal block grants, Medicare/Medicaid payments, or state funds. Privately operated treatment facilities that do not receive public funding do not con-

[7] LawAtlas. The policy surveillance portal [Internet]. Philadelphia (PA): LawAtlas; [cited 2016 Mar 1]. Available from: http://lawatlas.org/ query?dataset=corey-matt-pmp

[8] Although operational status of the PDMP may not occur immediately after the legislation goes into effect, conducting analysis of specific program attributes will address some of the concerns that the PDMP is not immediately effective.

tribute to the dataset, and will therefore not identify the patterns of behavior we report. Each observation in the dataset is an admission to a drug-treatment facility, and the same individual may therefore contribute multiple observations to the dataset. Recorded with each admission are personal characteristics of the individuals seeking treatment including the primary, secondary, and tertiary substances abused, frequency and tenure of each user's engagement with the substance, age categories, method of payment, demographic information, and treatment setting (i.e., ambulatory, detoxification, rehabilitation). Given this information, we are able to directly analyze the effects of the PDMP on opioid related drug-treatment admissions and to identify the potential differences in selection into prescription-drug abuse based on addiction use and tenure. In addition, the TEDS allows for separate identification of treatment admissions based on the referring party. A full 60 percent of treatment admissions are from individuals seeking treatment independently or though a criminal referral, which will enable identification by referral type.

In Table 1, we present summary statistics for drug-treatment admissions in the years 1998-2012. The average number of opioid-related treatment admissions per 100,000 state residents during this time period is 71, with substantial variation given a standard deviation of approximately 80 admissions per 100,000 state residents.[9] Admissions reporting alcohol abuse are most common with an average of 505 alcohol related treatment admissions per 100,000 state residents.

Opioid-related categories reported in the TEDS include, "Non-prescription methadone,"

[9]Because the TEDS treatment admission data restricts ages to those older than 11 years of age, we use the population over the age of 10 in a given state year to calculate the rate per 100,000 residents.

"Heroin," and "Other opioids and synthetics."[10] We adopt "other opioids and synthetics" as our dependent variable (referred to simply as opioids in the rest of the paper), which includes all commonly prescribed opioid pain killers recorded in state PDMP databases. Although substances not commonly prescribed by physicians are included in this category, the TEDS does not allow one to separate these substances from drugs PDMPs commonly target thus we are unable to avoid potential attenuation introduced by this grouping. We do not include non-prescription methadone in this analysis, as methadone is often dispensed from opioid treatment programs (OTPs) which fall under federally assisted drug abuse programs and are thus not allowed to report to PDMPs.[11] We also do not include heroin treatments, as restricting access to prescription drugs may shift users into heroin, potentially hiding any gains being made by PDMPs in curbing prescription-drug abuse.[12]

3.3 Drug-related deaths

As a measure of drug-relate mortality we use data obtained through the restricted-use National Vital Statistics System (NVSS), which records the census of deaths in the United States from the Centers for Disease Control and Prevention. We evaluate the effect of PDMPs on opioid-related mortality, including accidental death, suicide, and undetermined intent by state of residence and year, using the International Classification of Diseases codes (ICD-10)

[10] "Other opioids and synthetics category includes" includes buprenorphine, codeine, Hydrocodone, hydromorphone, meperidine, morphine, opium, oxycodone, pentazocine, propoxyphene, tramadol, and any other drug with morphine-like effects.

[11] Certification of Opioid Treatment Programs (OTPs), SUBSTANCE ABUSE & MENTAL HEALTH SERVS. ADMIN., `http://www.samhsa.gov/medication-assisted-treatment/opioid-treatment-programs` (last visited Oct. 20, 2016).

[12] Likewise, such substitution may have one overestimate the true benefit to human health associated with PDMP attributes and reductions in opioid-treatment admission. Formal analysis of this is ongoing, and will appear in "The Heroin Epidemic: Is There a Role for Supply-Side Restrictions on Prescription Drugs?"

external cause of injury codes.[13]

Unlike the TEDS, NVSS reports opioid-related deaths cause by natural and semi-synthetic opioids (e.g., oxycodone and hydrocodone), as well as fully synthetic opioids excluding methadone (e.g., fentanyl and tramadol).[14] Using this distinction, we can separately identify the effect of PDMPs on natural and semisynthetic opioids (referred to as opioids in Table 10), and on fully synthetic opioids.

3.4 Controls

We obtain state-year population data from the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program as well as median household income and unemployment measures from the Bureau of Labor Statistics. In addition to these, we control for state level Medicaid and Medicare enrollment from the Centers for Medicare & Medicaid Services and supply of treatment centers and pharmacies by state year from the U.S. Census Bureau's County Business Patterns (CBP).

Although PDMPs are the focus of this analysis, we follow Radakrishnan (2013) in controlling for other potentially confounding state legislation affecting access to and use of prescription opioids. These include doctor shopping laws, regulation of pain clinics, medical marijuana legalization, patient identification laws and authorization for the use of Naloxone

[13] X40-X44, X60-64, X85, or Y10-Y14

[14] Drug overdose deaths involving opioids are identified using International Classification of Diseases, Tenth Revision underlying cause-of-death codes X40-X44, X60-X64, X85, and Y10-Y14 with a multiple cause code of T40.0, T40.1, T40.2, T40.3, T40.4, or T40.6.
Opioids include drugs such as morphine, oxycodone, hydrocodone, heroin, methadone, fentanyl, and tramadol.
For each type of opioid, the multiple cause-of-death code was T40.1 for heroin, T40.2 for natural and semisynthetic opioids (e.g., oxycodone and hydrocodone), T40.3 for methadone, and T40.4 for synthetic opioids excluding methadone (e.g., fentanyl and tramadol). Deaths might involve more than one drug thus categories are not exclusive.

in preventing overdose. Effective dates for these alternative laws come from the CDC's Public Health Law Program.[15]

4 Empirics

In the sections below, we consider the efficacy of PDMP broadly. The empirical identification strategy will then be used throughout the analysis to follow, as we consider specific program attributes and the underlying pattern of efficacy across measures of use.

4.1 Do PDMPs matter to treatment admissions?

Given the variation in the timing of adoption of PDMPs by states, we adopt a difference-in-differences approach to separately identify the causal impact of program implementation on substance-abuse treatment admissions, and on overdose deaths. Specifically, we will estimate as a baseline specification,

$$Y_{st} = \alpha + \beta_1 X_{st} + \beta_2 PDMP_{st} + \gamma_s + \delta_t + \epsilon_{st}, \tag{1}$$

where Y_{st} is the log of state aggregate treatment admissions citing opioid abuse in state s in year t. In subsequent specifications, we will also consider two contributing paths—criminal referrals and self-referred admissions. State (γ_s) and year (δ_t) fixed effects can be included in all specifications to account for unobserved time invariant heterogeneity across states and for nationwide drug abuse campaigns. However, in my preferred specifications we will include

[15]Effective dates of medical marijuana legalization are collected from the National Conference of State Legislatures

state-specific trends. Importantly, if states with high drug-abuse rates adopting PDMPs, β_1 will be biased. Bias would also result from states tending to implement these programs differentially in response to increases in drug-abuse rates. Because of these potential biases, my preferred specification will incorporate state-specific time trends in identifying the effect of PDMPs on outcomes. We control for observable state-level heterogeneity with X_{st}, including controls for population, age and racial compositions, yearly unemployment rate, and Medicare/Medicaid enrollments. Given the potential to misidentify the effect of PDMP as related policies vary across states and time, we also control for state-level medical marijuana laws, and various other laws defined by the CDC as intended to reduce prescription-drug abuse (e.g., photo-identification requirements, Naloxone availability, and pain clinic regulation). The parameter of interest, $\hat{\beta}_2$, can be interpreted as the effect of state-level adoption of a PDMP on treatment admissions. ϵ_{st} is a random-error term robust to heteroskedasticity, which we estimate while allowing for state-specific clustering.

As a first pass at the evaluation of PDMPs, we consider the average effect of establishing a PDMP on opioid-related treatment admissions. Although previous literature has found only small effects in similar specifications, we bring three additional years of data to the analysis, which is of particular interest as the recent uptick in heroin overdose has been associated by some with an increase in the cost of acquiring prescription drugs (Volkow, 2014).

In Panel A of Table 2, we report the coefficient estimates that capture the average effect of establishing a PDMP on the log of state-aggregate treatment admissions citing opioid abuse. In Column (1), we report the estimates of an OLS model of the form of Equation (1), controlling for year-fixed effects and time-invariant state heterogeneity. This model suggests that opioid-related treatment admissions increase approximately 14.5 percent with PDMP,

though not significantly different from zero, statistically. Controlling for differences in state-specific trending of treatment admissions (in Column 2) the point-estimate falls in magnitude, remaining insignificant.

In Column (3), we add demographic controls and other potentially confounding prescription-drug legislation.[16] With their inclusion, the magnitude of the effect of PDMPs on opioid-related admissions remains small and we are unable to reject that the effect of a PDMP on opioid-related treatment admissions generally is zero.

We follow the same pattern of reporting when separately considering criminal and self referrals, in panels B and C of Table 2. Although opioid-related criminal referrals are approximately 23-percent higher in opioid-related criminal referrals in Column (1) of Panel B, the effect is not robust to controlling for state-specific time trends. A similar pattern is evident in self referrals (in Panel C). Although Radakrishnan (2013) finds small negative (though insignificant) effects of PDMPs on opioid admissions, my findings are largely consistent with the existing literature suggesting that PDMP implementation has little if any effect on drug-related health outcomes.

4.2 Program attributes

While PDMPs at a minimum require pharmacists or prescribers to report to the database, the effect these programs have on reducing opioid abuse vary substantially across the characteristics and requirements of the state-specific mandates. We evaluate these programs

[16]These controls include: total state population, percent of the population who is black, percent of the population who is white, median income, Medicaid/Medicare enrollment levels, unemployment rate, number of drug treatment centers, number of pharmacies, and indicators for whether or not the state has one the the following drug related laws in effect: doctor shopping laws, Naloxone availability law, pain clinic regulation laws, required patient identification laws, and if medical marijuana has been legalized.

separately as described in Section 2; first addressing programs with the most-aggressive requirements on prescribers and operating agencies. We then subsequently add variation from less-aggressive programs to identify those attributes which affect opioid abuse as reflected in drug-treatment admissions.

In all cases, we present varieties of specification, arriving at my preferred specification while showing the roles being played by state-specific trends, demographic controls, and other drug-related legislation. Likewise, we will estimate the fact of program attributes while separately identifying the potential movement in admissions attributable to PDMP alone. This—as opposed to the joint consideration of adding "PDMP plus a given set of attributes"—is the policy relevant parameter, as now-49 states have active PDMPs and the only initiatives are among the specific attributes a given state might consider implementing.

4.2.1 Does a mandate to consider the database matter to treatment admissions?

In evaluating the effect of PDMPs, it is natural to assume that these programs would most directly affect opioid abuse in those states which require prescribers to check the database before prescribing opioids to patients (a "must-access" provision). In Table 3, we evaluate the effect of having a "must-access" provision on opioid treatment admissions generally, through criminal referrals, and though self referring individuals (Panels A, B, and C respectively). The coefficient estimate on the "must-access" indicator represents the causal impact of these provisions on opioid-related treatment admissions. The coefficient estimate on established PDMP represents the casual impact of a PDMP without this binding provision. In Panel A of Table 3, we first report the result of an OLS model that separately identifies the influence of "must-access" provisions from broader PDMPs, controlling for state and year fixed effects.

With those states-years without established PDMP legislation as the comparison group, the coefficient estimate on the "must-access" provision suggests these provisions decrease overall opioid-related treatment admissions by approximately 4.2 percent where the estimated impact of an established PDMP suggests an increase in treatment admissions of approximately 0.9 percent.

Recalling the potential biases discussed above, in Column (2) of Panel A we control for state-specific time trends as well as state fixed effects and state-year observations of a set of demographic characteristics. With the addition of these controls, the estimated coefficient of the "must-access" provision is -0.354, implying a statistically significant 25.8-percent reduction in opioid-related treatment admissions. Adding controls for other prescription drug-related legislation in Column (3), the magnitude of the coefficient of interest remains relatively stable. Estimates in Column (3) imply that treatment admissions would fall (relative to the mean number of treatments in state-years without "must access") by approximately 561 per year in the average state were they to implement "must-access" protocols.

In Panels B and C of Table 3, we evaluate the effectiveness of the "must-access" provision on criminal and self referrals respectively, together accounting for roughly 60 percent of total referrals. Following the same specification described above, we find a statistically significant decrease in only opioid-related self referrals using those states without established PDMP legislation as the comparison group (columns 1, 2, and 3). However, though the statistical significance of "must-access" provisions on criminal referrals is weak, the point estimate is arguably still economically meaningful.

That the result appears strongest among self-referred treatments is not surprising, as "must access" provisions are operational in the supply chain directly, and would only be

implicated in criminal referrals indirectly. That they are still informative to explaining reductions in criminal referrals is nonetheless encouraging, however, it is widely anticipated that the prescription market is an input into the criminal access to opioid.

As the most-restrictive policy attribute among PDMP practices, "must-access" provisions are seemingly associated with decreases in opioid-related treatment admissions, across both criminal and self-referring users, with the larger responses coming from self-referred admissions. It is in this dimension that future policy should find encouragement, given a literature finding little efficacy in PDMP broadly. While weakly defined and passive PDMP fail to deliver, aggressive requirements matter to outcomes. In short, if these gains reflect decreases in the number of individuals abusing opioids in response to prescribers interacting with the database in this way—specifically, in the supply chain prior to the user's acquisition of the substance—the policy recommendations that follow are obvious.

Before concluding, however, we consider the sensitivity of outcomes to a slight relaxation of this constraint. In Table 4, we differentiate control-states further, by allowing opioid-related treatment admissions to vary by whether physicians and prescriber can even access this data while dealing with patients. Following the same structure as above, we reveal a very knife-edge to the "must-access" result we've established. First, we note that opioid-related treatment admissions do not systematically move with the establishment of "can-access" provisions in PDMP. Second, we note the robustness of the "must-access" states to the inclusion of the slightly less-aggressive but similar provisions captured in the "can-access" distinction.

4.2.2 Does the passivity of oversight matter?

In Table 5, we evaluate the effect of provisions for the proactive checking of opioid-related treatment admissions. In particular, we exploit variation in whether and when states require that the operating agency check the database for suspicious patterns of prescribing (among physicians) and receipt of opioids (among users). The patterns in columns 1 through 3 suggest that this provision does not significantly affect treatment admissions in the either aggregate, or criminal or self referrals. The results suggest that when intervention is limited to the passive provision of information, with no mandate, the information provided by the database does itself significantly alter prescription-drug abuse.

4.3 The distribution of gains

Before considering the underlying heterogeneity—where in the intensity and tenure of use aggregate reductions are arising—we relax the constraint that is implicit in earlier results, that "must-access" provisions act similarly on opioid-treatment admission across all years of implementation. As would be consistent with changing praxis, relaxing this constraint reveals a phase-in period associated with "must-access" provisions, over which treatment admissions increasingly fall. Point estimates on years since the implementation of "must access" are shown in Figure 1, suggesting a growing distinction between treatment states systematic with the adoption of must-access provisions, in the full sample and in a sample restricted to only treatment states.[17]

In evaluating both across state and time series variation in the specific provisions of

[17]Allowing pre-treatment years to vary by treatment and control also supports the common-trends assumptions.

PDMPs, no programs outside of the most-aggressive PDMPs (those with "must-access" attributes) have a significant impact on drug-treatment admissions, my measure of opioid abuse. However, underlying the Treatment Episodes Dataset is a collection of information on usage intensity, by drug reported. As such, we can unpack treatment admission in a way that informs our understanding of where gains are coming from in a more nuanced way. For example, this information allows one to identify which types of users—are light users declining, or is it heavy users who are in decline—are being affected by these supply side interventions. Recall that a single "treatment admission" potentially includes primary, secondary, and tertiary substances abused, as well as the frequency with which those substances were used (e.g., "no use in the last month" through "daily"). Using these measures, we separately identify the parts of the distribution of treated users from which the overall reductions are seemingly arising.

4.3.1 Intensity of opioid use

In considering the above treatment effect, we make inference regarding the effect of PDMP (or specific attributes) on the log-number of treatment admissions. Making no distinction between light and heavy users—across five categories of use intensity, actually—the above analysis implicitly assumes that treatment is common across use intensity.

In Table 6, we relax this assumption and consider the potential reductions in the treatment of opioid users across reported levels of use intensity. The TEDS data provides counts of treatment for opioid related admissions by categories of frequency of use or intensity—categories include no use in the past month, monthly use, 1-2 times weekly, 3-6 times weekly, and daily use. In Panel A of Table 6 we note that the most significant reductions are com-

ing from the lightest users in the distribution of intensity—there are evident 36.2- and 35.6-percent reductions in users reporting only monthly or 1-2 times weekly use report, respectively—and from those admissions reporting daily use, where reductions are 32.8 percent. Considering the average number of admissions in each of the intensity categories, the largest absolute movement of patients is clearly coming from the the two extremes of the distribution of use—were other states to likewise implement "must-access" protocols, estimates in Column (3) imply that annual treatments among monthly users would fall by 87 in the average state, and daily users by 466 (relative to the mean number of treatment admissions in state-years without "must access"). Although substitution across intensities in response to "must-access" provisions is possible, that point estimates are negative across all intensity levels again suggest that the net affect of such mandates is toward beneficial declines in treatment.

As an attempt to account for the potential substitutions across category, in Panel B of Table 6, we include lagged counts of contiguous densities. That is, when predicting counts of admissions reporting "3-6 times weekly" use in year t, for example, we include $t-1$ counts of admissions reporting "1-2 times weekly" and counts of admissions reporting "daily" use, as these are the most-likely category from which substitution may originate. These controls prove informative in predicting treatment counts and, while the magnitudes fall across all intensity levels, we again find significant declines in admissions of those users reporting monthly, 1-2 times weekly, and daily use (25.6, 24.0, and 25.3-percent declines, respectively).

Across Table 6, impact estimates at the (untreated) mean suggest reductions from 25-to-36 percent among the lightest users, and 25-to-32 percent among daily users, dipping slightly in the middle of the distribution. The economic significance of this is further exaggerated by

the smaller densities in the middle of the distribution, making it quite reasonable to consider efficacy following a roughly "U-shaped" pattern in use intensity. Similarly, the available policy variation is explaining more of the variation in treatment admissions in the tails of the distribution of use-intensity, where effect sizes are upwards of 0.16 to 0.30.

As variation across intensity levels could suggest differential selection into categories, in Table 7 we consider known personal characteristics across similar categories.[18] Comparing those categories displaying the largest impacts of PDMPs (i.e., the tails of the distribution) to those where PDMPs have insignificant effects, we do not find striking differences in demographic characteristics. Across all intensity levels, approximately 50 percent of admissions are male and 78 percent are white. Approximately 30 percent of admissions are unemployed and 13 percent report having public insurance while close to 8 percent report being privately insured. Individuals seeking treatment of opioid abuse fall into the 35-44 years age group at approximately 30 percent which seems to be the commonly reported age bin across all intensity levels. Given this information, one cannot attribute the differential impact of "must access" to sorting based on selection on such characteristics.

As one last consideration of the potential error structure across categorical intensities of use, in Table 8 we fully model the simultaneity by three-stage least squares. Doing so accounts directly for the potential that errors across intensities correlate, and movement with "must access" in one category might well drive movement in other categories. Doing so, we find that opioid-treatment admissions are similarly responsive to the policy variation, which suggests that the independence assumption (of Table 6) is not overly restrictive. In

[18]Personal characteristics reported at the time of admission include gender, entity of reference (i.e., criminal, self, school, employer, health care or alcohol counseling referrals), employment and insurance status, age, race, prior treatment admissions, and type of treatment facility. Characteristics are not exhaustive, and need not therefore sum to one.

particular, across all four approaches in Panel A, point estimates among light users (e.g., monthly and 1-2 weekly) and heavy users (i.e., daily use) associated with "must-access" provisions range from -0.392 to -0.449, suggesting decreases of approximately 48-to-57 percent. Including the lagged-neighboring categories in Panel B as described above, the magnitude of the "must-access" provisions again attenuates slightly, though there is significant movement again among monthly, 1-2 times weekly, and daily users.

4.3.2 Tenure of opioid use

In addition to providing frequency of use information, the TEDS includes information on the self-reported "age at first use" for each of the three substances reported by an individual seeking treatment. As this age report is categorical in nature, we consider all possible-but-latent truths (i.e., the four combinations of youngest and oldest starting age and youngest and oldest treatment age). As results are not sensitive to this categorization, in Table 9 we report on the responsiveness of opioid-related treatment admissions by tenure of use using the mid points of all age bins.

As is evident in Table 9, around PDMP-induced supply side restrictions there are differential effects on short-term and long-term users. While point estimates suggest reductions coming from across the distribution of tenure, we see the largest reductions in admissions which report having used for less than six years, and, in particular, 0-3 years of use, where impact at the (untreated) mean is 39 percent. Overall, the range of impacts is monotonically declining in tenure of use, bottoming out at roughly 10-percent reductions in opioid-related treatment admissions among those reporting 16-or-more years of opioid use. "must-access" provisions are also explaining more of the variation in treatment admissions at the lower-

tenure end of the distribution, where effect sizes are upwards of 0.22 and 0.29.

4.4 Must-access PDMP provisions and opioid-related overdose deaths

Thus far, we have established that amid the general lack of sensitivity in opioid-related treatments with implementations of prescription drug monitoring programs, there are areas of encouragement, albeit very specific and narrow avenues of encouragement. Namely, we find a knife-edge result where efficacy is seemingly strong and economically significant. Where states implement PDMPs requiring physicians to access the database before prescribing opioids, we see reductions in opioid-related treatment admissions—a pattern that is not even evident among those merely allowing similar access.

In Table 10, we follow up on the same variation in PDMP to consider the implications on opioid-related deaths. Previous research has found little systematic variation in death around the introductions of PDMP provisions, yet, without distinguishing these most-aggressive practices.[19] In Column (1) of Table 10, we find no statistically significant explanatory power coming from the general establishment of these programs. However, the effectiveness of the mandated interaction with Prescription Drug Monitoring Programs is again demonstrated in Column (2) and Column (3) of Table 10. While PDMPs generally do not affect overall opioid-related overdose deaths, states adopting a "must-access" provision in their program

[19]In recent work, Ruhm (2017) considers the assignment of death to specific drugs involved in drug-poisoning fatalities, recognizing the potential implication of multiple substances. While clearly germane to any consideration of the potential substitution from prescription-opioid to heroin, measurement error in the Multiple Cause of Death files is not likely to be systematic with state-time-varying "must-access" provisions, and level differences in the measure of opioid-related death are thus absorbed into the error structure of our estimator.

is associated with an approximately 33 percent reduction in opioid-related overdose deaths. When we evaluate those state with the more relaxed provision, "can-access," the results closely follow the pattern found in opioid-related treatment admissions. That is, while the effect of "must-access" remains strongly negative and statistically significant, the effect of the "can-access" provision is positive and statistically insignificant suggesting that, when the information provided in these databases is costly to access, allowing access to the information is not sufficient to reduce prescription drug abuse.

5 Discussion

I offer strong evidence of efficacy in prescription drug monitoring programs, in a large literature of weak associations between PDMPs and outcomes. We find that PDMPs with "must-access" attributes—getting between prescribers and patients—lead to a significant reductions in opioid-related drug-treatment admissions. Merely allowing this access cannot be associated with similar decreases, which points further to the need for strict mandates as the knife-edge nature of this result suggests that effective PDMPs are those that actively interfere with the supply chain, often at the point of consultation.

In addition to documenting the extensive margin of episodes of drug treatment, we demonstrate that reductions in treatment admissions are originating from less-attached users—less attached in both intensity of use and tenure of use—in states with the most-aggressive PDMP policies. We also find evidence of these specific monitoring practices driving overdose deaths down, significantly so among states with at least one year of experience with "must access" PDMP provisions. Estimates imply that treatments would fall by 561

per year in the average state were they to implement "must-access" protocols, with the bulk of these coming from reductions in those individuals reporting to have sought treatment following a period of daily opioid use.

Identifying which aspects of the PDMPs are most effective in curbing prescription-drug abuse is crucial to informing policy. In light of existing evidence that suggests only small benefits associated with broader PDMP implementation, the data are a clear encouragement toward requiring prescribers to consult these databases at the point of contact with the patient.

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Table 1
Summary Statistics

	Mean	Std. Dev.	Min.	Max.	N
Panel A: Drug-treatment admissions					
Total ($\times 10^3$)	34.755	49.823	.177	314.56	734
Opioid Related	3.047	4.415	1	43.952	734
Heroin Related	6.160	13.375	0	80.382	734
Marijuana Related	13.524	17.493	.112	126.226	734
Alcohol Related	22.299	32.746	135	236.191	734
Opioid Rate*	71.80	80.3	0.02	571.97	734
Heroin Rate	100.55	152.42	0	781.69	734
Marijuana Rate	298.72	164.62	2.63	972.56	734
Alcohol Rate	505.69	355.74	3.16	2073.78	734
Panel B: State demographics					
Total Pop ($\times 10^7$)	5.962	6.527	.491	37.99	734
% Pop Black	0.11	0.09	0.003	0.37	734
% Pop White	0.83	0.13	0.24	0.98	734
Median Income	46,194	8.098	27.67	71.84	734
Medicaid Enrollment	881.82	1,220.66	34.58	8513.32	734
Medicare Enrollment	852.86	870.12	38.23	5126.61	734
Unemployment Rate	5.61	2.08	2.3	13.7	734
Treatment Centers	290.68	295.51	9	1822	734
Pharmacies	837.63	886.51	30	4591	734
Panel C: Drug-related legislation					
Established PDMP	0.530	0.490	0	1	734
Doctor Shopping Law	0.478	0.500	0	1	734
Naloxone Availability	0.037	0.188	0	1	734
Pain Clinic Law	0.026	0.159	0	1	734
Medical Marijuana	0.192	0.394	0	1	734
Patient ID Law	0.282	0.450	0	1	734

Notes: * = per 100,000 residents over 10 yrs old.

Table 2
Existence of PDMP and opioid-related treatment admissions

	(1)	(2)	(3)
Panel A: Aggregate Treatment Admissions			
PDMP	0.127	-0.020	-0.014
	(0.09)	(0.08)	(0.08)
Observations	734	734	734
Mean (PDMP=0)	1062	1062	1062
Effect Size	0.08	0.01	0.01
R^2	0.93	0.96	0.96
Panel B: Criminally Referred Treatment Admissions			
PDMP	0.191*	0.066	0.060
	(0.11)	(0.09)	(0.09)
Observations	734	734	734
Mean (PDMP=0)	210	210	210
Effect Size	0.12	0.04	0.04
R^2	0.90	0.93	0.93
Panel C: Self Referred Treatment Admissions			
PDMP	0.179*	-0.001	0.016
	(0.10)	(0.10)	(0.10)
Observations	734	734	734
Mean (PDMP=0)	487	487	487
Effect Size	0.11	0.00	0.01
R^2	0.90	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	No	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Included in "Demographic controls" are state-year observations of total population, percent of the population that is black, percent of the population that is white, median income, Medicaid/Medicare enrollment levels, and unemployment rate. Included in "Other drug controls" are the number of drug treatment centers, number of pharmacies, and indicators for whether or not the state has one of the following drug-related laws in effect: doctor-shopping laws, Naloxone availability law, pain-clinic regulation laws, required-patient-identification laws, and if medical marijuana has been legalized. Robust standard errors are reported in parentheses and in all specifications allow for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 3
"Must-access" provisions and opioid-related treatment admissions

	(1)	(2)	(3)
Panel A: Aggregate treatment admissions			
PDMP	0.127	0.001	-0.005
	(0.09)	(0.08)	(0.08)
+ Must access	-0.042	-0.354**	-0.362**
	(0.17)	(0.15)	(0.14)
Observations	734	734	734
Mean (Must access=0)	2247	2247	2247
Effect size (Must access)	0.03	0.24	0.24
R^2	0.93	0.96	0.96
Panel B: Criminally referred treatment admissions			
PDMP	0.189*	0.088	0.069
	(0.11)	(0.09)	(0.09)
+ Must access	-0.070	-0.358*	-0.376**
	(0.20)	(0.18)	(0.18)
Observations	734	734	734
Mean (Must access=0)	441	441	441
Effect size (Must access)	0.04	0.23	0.24
R^2	0.90	0.93	0.93
Panel C: Self referred treatment admissions			
PDMP	0.177*	0.029	0.026
	(0.10)	(0.10)	(0.09)
+ Must access	-0.096	-0.416**	-0.419**
	(0.17)	(0.17)	(0.17)
Observations	734	734	734
Mean (Must access=0)	1049	1049	1049
Effect size (Must access)	0.06	0.25	0.25
R^2	0.90	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	Yes	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 4
"Can-access" provisions and opioid-related treatment admissions

	(1)	(2)	(3)
Panel A: Aggregate treatment admissions			
PDMP	0.224*	-0.016	-0.032
	(0.12)	(0.11)	(0.11)
+ Must access	0.014	-0.357**	-0.368***
	(0.19)	(0.14)	(0.13)
+ Can access	-0.168	0.022	0.036
	(0.12)	(0.10)	(0.11)
Observations	734	734	734
Mean (Must access=0)	1306	1306	1306
Effect size (Must access)	0.01	0.24	0.24
R^2	0.93	0.96	0.96
Panel B: Criminally referred treatment admissions			
PDMP	0.256*	0.017	0.009
	(0.15)	(0.13)	(0.13)
+ Must access	-0.038	-0.372**	-0.388**
	(0.22)	(0.17)	(0.17)
+ Can access	-0.095	0.095	0.080
	(0.13)	(0.13)	(0.13)
Observations	734	734	734
Mean (Must access=0)	243	243	243
Effect size (Must access)	0.02	0.24	0.25
R^2	0.90	0.93	0.93
Panel C: Self referred treatment admissions			
PDMP	0.328**	-0.027	-0.041
	(0.13)	(0.13)	(0.13)
+ Must access	-0.025	-0.427***	-0.433***
	(0.20)	(0.16)	(0.15)
+ Can access	-0.216	0.074	0.090
	(0.13)	(0.12)	(0.12)
Observations	734	734	734
Mean (Must access=0)	617	617	617
Effect size (Must access)	0.01	0.25	0.26
R^2	0.91	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	Yes	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 5
"Proactive checking" and opioid-related admissions

	(1)	(2)	(3)
Panel A: Aggregate treatment admissions			
PDMP	0.123	0.038	0.040
	(0.08)	(0.09)	(0.08)
+ Proactive required	0.254	-0.158	-0.142
	(0.28)	(0.17)	(0.17)
+ Proactive permitted	-0.117	-0.041	-0.067
	(0.12)	(0.12)	(0.11)
Observations	734	734	734
Mean	2035	2035	2035
Effect Size (Required)	0.17	0.11	0.09
Effect Size (Permitted)	0.08	0.03	0.04
R^2	0.93	0.96	0.96
Panel B: Criminally referred treatment admissions			
PDMP	0.207**	0.164	0.171
	(0.10)	(0.10)	(0.11)
+ Proactive required	-0.147	-0.112	-0.052
	(0.33)	(0.19)	(0.20)
+ Proactive permitted	0.030	-0.164	-0.263
	(0.25)	(0.17)	(0.16)
Observations	734	734	734
Mean	414	414	414
Effect Size (Required)	0.09	0.07	0.03
Effect Size (Permitted)	0.02	0.10	0.17
R^2	0.90	0.93	0.93
Panel C: Self-referred treatment admissions			
PDMP	0.177	0.076	0.068
	(0.11)	(0.11)	(0.11)
+ Proactive required	0.096	-0.063	-0.096
	(0.32)	(0.19)	(0.19)
+ Proactive permitted	-0.041	-0.120	-0.085
	(0.22)	(0.13)	(0.12)
Observations	734	734	734
Mean	890	890	890
Effect Size (Required)	0.06	0.04	0.06
Effect Size (Permitted)	0.02	0.07	0.05
R^2	0.90	0.94	0.94
Year FE	Yes	No	No
State FE	Yes	Yes	Yes
State Specific Trends	No	Yes	Yes
Demographic Controls	No	Yes	Yes
Other Drug Controls	No	No	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012). Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 6
"Must-access" provisions and opioid-related treatment admissions, by intensity of use

	No use in last month	Monthly use	Weekly (1-2 times)	Weekly (3-6 times)	Daily use
	(1)	(2)	(3)	(4)	(5)
Panel A: Contiguous categories (lagged) not included					
PDMP	0.034	0.123	0.136	0.089	0.064
	(0.10)	(0.10)	(0.11)	(0.10)	(0.11)
+ Must access	-0.283	-0.532***	-0.483**	-0.279	-0.428**
	(0.17)	(0.11)	(0.22)	(0.24)	(0.17)
Observations	734	734	734	734	734
Mean (Must access=0)	739	239	170	277	1420
% Impact (Must access)	24.6	41.2	38.3	24.4	34.8
Effect Size (Must access)	0.20	0.38	0.34	0.19	0.26
R^2	0.93	0.92	0.92	0.92	0.93
Panel B: Contiguous categories (lagged) included					
PDMP	-0.023	0.051	0.042	0.020	-0.015
	(0.06)	(0.08)	(0.08)	(0.06)	(0.09)
+ Must access	-0.115	-0.364***	-0.311**	-0.083	-0.328**
	(0.14)	(0.10)	(0.15)	(0.18)	(0.12)
Observations	726	726	726	726	726
Mean (Must access=0)	746	242	172	279	1433
% Impact (Must access)	10.9	30.5	26.7	7.9	27.9
Effect Size (Must access)	0.08	0.26	0.22	0.06	0.20
R^2	0.94	0.94	0.94	0.95	0.95

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012), by use intensity. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 7
Demographics across intensity of use, pre-treatment

	No use in last month	Monthly use	Weekly (1-2 times)	Weekly (3-6 times)	Daily use
	(1)	(2)	(3)	(4)	(5)
Male	0.540	0.519	0.525	0.513	0.508
Crime referral	0.302	0.195	0.169	0.151	0.127
Self referral	0.297	0.376	0.385	0.409	0.470
Alcohol referral	0.122	0.098	0.096	0.103	0.105
Health referral	0.091	0.106	0.100	0.114	0.13
School referral	0.007	0.013	0.007	0.006	0.004
Employer referral	0.008	0.014	0.012	0.014	0.010
Unemployed	0.289	0.29	0.284	0.297	0.296
Private insurance	0.069	0.075	0.075	0.083	0.084
Public insurance	0.138	0.133	0.128	0.134	0.145
Age 18-24	0.125	0.144	0.148	0.138	0.108
Age 25-34	0.290	0.294	0.28	0.291	0.299
Age 35-44	0.357	0.308	0.297	0.326	0.363
Age 45-54	0.121	0.088	0.093	0.104	0.134
White	0.829	0.782	0.745	0.788	0.818
Black	0.059	0.062	0.068	0.056	0.054
No prior	0.265	0.295	0.293	0.301	0.310
Ambulance	0.694	0.556	0.501	0.505	0.474
Rehab	0.193	0.196	0.199	0.206	0.198
Detox	0.069	0.157	0.180	0.195	0.266

Source: Treatment Episodes Data Set (TEDS), Substance Abuse and Mental Health Services Administration, 1998–2012.

Table 8
"Must-access" provisions and opioid-related treatment admissions by intensity of use: Simultaneous equations (3sls)

	No use in last month	Monthly use	Weekly (1-2 times)	Weekly (3-6 times)	Daily use
	(1)	(2)	(3)	(4)	(5)
Panel A: Contiguous categories (lagged) not included					
PDMP	0.027	0.123**	0.126**	0.072	0.057
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
+ Must access	-0.279*	-0.532***	-0.478***	-0.271	-0.425**
	(0.17)	(0.16)	(0.17)	(0.17)	(0.18)
Observations	734	734	734	734	734
Mean (Must access=0)	741	240	170	277	1421
% Impact (Must access)	16.5	36.2	35.6	22.9	32.8
Effect Size (Must access)	.12	.32	.30	.17	.24
R^2	0.93	0.92	0.92	0.92	0.93
Panel B: Contiguous categories (lagged) included					
PDMP	0.013	0.098*	0.093*	0.051	0.036
	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)
+ Must access	-0.204	-0.449***	-0.375***	-0.173	-0.381**
	(0.15)	(0.14)	(0.14)	(0.14)	(0.15)
Observations	726	726	726	726	726
Mean (Must access=0)	749	242	172	280	1435
% Impact (Must access)	2.9	25.6	24.0	6.6	25.3
Effect Size (Must access)	.08	.27	.24	.11	.21
R^2	0.94	0.94	0.94	0.94	0.95

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012), by use intensity. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 9
"Must-access" provisions and opioid-related treatment admissions, by tenure of use

	0-3 years	4-6 years	7-10 years	11-15 years	ge16 years
	(1)	(2)	(3)	(4)	(5)
PDMP	-0.108	-0.045	0.097	0.069	0.038
	(0.09)	(0.10)	(0.09)	(0.09)	(0.08)
+ Must access	-0.551***	-0.460***	-0.313**	-0.232*	-0.167
	(0.12)	(0.16)	(0.15)	(0.13)	(0.18)
Observations	734	734	734	734	734
Mean (Must access=0)	728	577	412	299	32865
% Impact (Must access)	42.3	36.9	26.8	20.7	15.4
Effect Size (Must access)	0.36	0.30	0.21	0.16	0.15
R^2	0.94	0.94	0.95	0.95	0.95

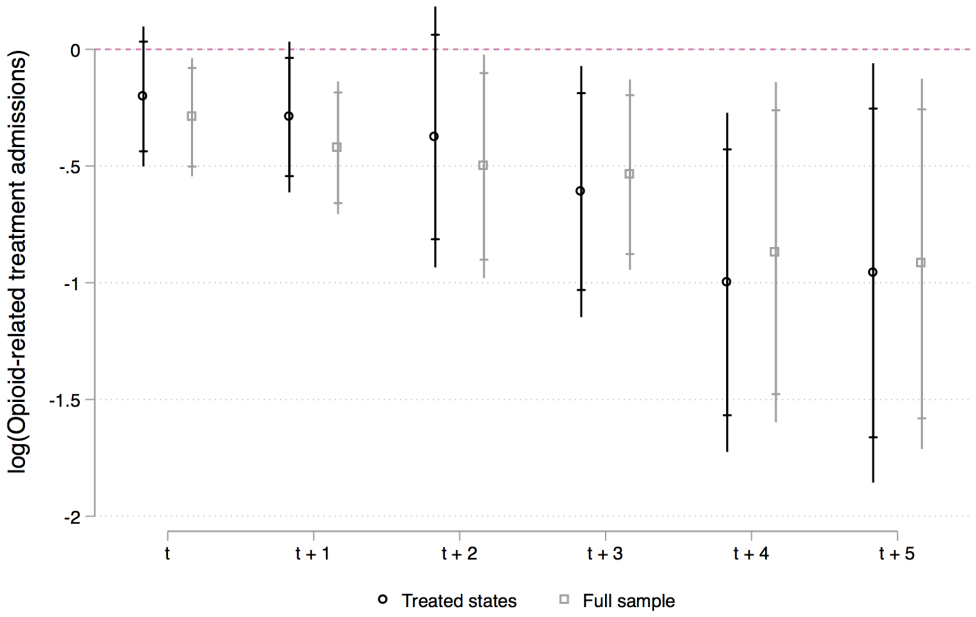
Notes: In each specification, the dependent variable is equal to the (state-year) log-count of opioid-related treatment admissions (TEDS, 1998–2012), by tenure of use. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 10
Opioid-related deaths, 1999-2012

	PDMP	Must Access	Can Access
	(1)	(2)	(3)
PDMP	-0.056	-0.045	-0.141
	(0.10)	(0.10)	(0.18)
+ Must access		-0.399*	-0.416*
		(0.22)	(0.22)
+ Can access			0.124
			(0.12)
Observations	700	700	700
Mean (Must access=0)		257	257
% Impact (Must access)		32.9	34
Effect Size (Must access)		.32	.33
State FE	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Other drug policy	Yes	Yes	Yes

Notes: In each specification, the dependent variable is equal to the (state-year) log-count of deaths involving natural and semi-synthetic opioids (opioid) and fully synthetic opioids (synthetic), from the Vital Statistics of the United States (MCOD, 1999-2012). The mean number of deaths (PDMP=1) is equal to that in state-years with active PDMPs but no "must-access" provision. All specifications include state FE, state-specific trends, demographic controls, and controls or other drug legislation. Robust standard errors are reported in parentheses and allow for clustering at the state level in all specifications. *** significant at 1%; ** significant at 5%; * significant at 10%.

Figure 1
"Must-access" provisions and opioid-related admissions, by year of implementation



Notes: Point estimates are from two separate specifications, following Column (3) of Table 3, relaxing the restriction that treatment be constant across years of implementation. In this table, we plot point estimates and confidence intervals with and without restricting the sample to the five states who experience the arrival of "must access" in the time series available. Confidence intervals are 95%, with 90% indicated by hash marks.