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PRESCRIPTION DRUG MONITORING PROGRAMS, OPIOID ABUSE, AND CRIME

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ABSTRACT

The past two decades have witnessed a substantial increase in opioid use and abuse in the United States. In response to this opioid epidemic, prescription drug monitoring programs (PDMPs) have been implemented in virtually all states. These programs collect, monitor, and analyze prescription opioid data with the goal of preventing the abuse and diversion of controlled substances. A growing literature has found that voluntary PDMPs, which do not require doctors to access PDMPs before prescribing controlled substances, have had little effect on opioid use and misuse. However, PDMPs that do mandate access have been found to be effective in reducing opioid misuse and other related health outcomes. In this paper we study the broader impact of voluntary and mandatory-access PDMPs on crime, and in the process inform the causal link between prescription opioid abuse and crime. Using information on offenses known to law enforcement and arrests from the Uniform Crime Reports (UCR), combined with a difference-in-differences empirical strategy, we find that voluntary PDMPs did not significantly affect crime whereas mandatory-access PDMPs have reduced crime by approximately 3.5%. Reductions in crime are largely associated with violent crimes, particularly homicide and assault. Also, we find evidence that young adults experienced the largest decrease in crime, which is consistent with prior work that also finds relatively larger declines in prescription opioid abuse for this group. Overall, these results provide additional evidence that prescription drug monitoring programs are an effective social policy tool to mitigate the negative consequences of opioid misuse, and more broadly indicate that opioid policies can have important spillover effects into other non-health related domains such as crime.

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1. Introduction

The prescribing behavior of physicians has fueled the opioid crisis (Kolodny et al. 2015). In addition to the availability of new drugs (for instance, market entry of OxyContin in 1996) and aggressive pharmaceutical marketing efforts over the 1990s, the concern that pain was being under-treated led to more aggressive pain management standards as state medical boards liberalized rules governing the prescription of opioid analgesics for chronic non-cancer pain. As a result, total opioid prescriptions filled increased from 107 million in 1992 to 274 million in 2012 (Pezalla et al. 2017).

Chronic pain afflicts over 100 million Americans and is one of the most common reasons that patients consult a physician (Nahin 2015). The proper use of opioids can mitigate the burden of acute pain, such as post-surgical pain (Manchikanti et al. 2010), and indeed a substantial portion of outpatient opioid prescribing can be traced to a hospital procedure (Brummett et al. 2017). However, while expanded availability and access to prescription (Rx) opioids has benefitted many, it has also led to unintended consequences in the form of diversion of these drugs for non-medical purposes.

Almost 19 million individuals (ages 12+) misused Rx psychotherapeutic drugs in the past year, with most of the misuse centered on Rx pain relievers (62%) (2016 National Survey of Drug Use and Health).[1] Overdose deaths from opioid analgesics have increased seven-fold since 1999,[2] with economic costs of the opioid epidemic exceeding $500 billion annually (Council of Economic Advisors 2017).[3] Some of the recent upsurge in deaths related to heroin (an illicit

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[1] See: https://www.samhsa.gov/data/sites/default/files/NSDUH-DetTabs-2016/NSDUH-DetTabs-2016.pdf. (Accessed 3/7/2018).

[2] Authors' calculations based on age-adjusted death rates from CDC Wonder.

[3] The CEA found that previous estimates of the economic cost of opioid abuse (for instance, Florence et al. 2016) were considerably understated due to the underestimation of the value of the lives lost due to opioid-related overdoses.

opioid) can also be indirectly linked to prescription opioids, with four out of five new heroin users having started out by misusing prescription opioids (Jones et al. 2013).

In order to restrain the diversion of Rx opioids for non-medical use and address the role played by physician prescribing, a popular state-level intervention has been to implement Prescription Drug Monitoring Programs (PDMPs). PDMPs are statewide databases that track the prescribing and dispensing of controlled substances, and thus provide key information to physicians and pharmacists on the patient's prescription history. While individuals can obtain Rx drugs for non-medical use through several sources including theft, street purchases, and from a friend or relative, physicians remain the leading source for those who are at highest risk of overdose (Jones, Paulozzi, and Mack 2014). These individuals obtain Rx opioids through their own prescriptions, and often times from multiple providers without the prescribers being aware of the other prescriptions, a practice known as "doctor shopping". Doctor shopping can also be an important indirect source for the user by making up an essential part of supply for street dealers (Inciardi et al. 2009).[4] PDMPs can help identify patients who may be doctor shopping, misusing Rx drugs, are at risk of overdose, and thus would benefit from timely treatment interventions.

Currently all states and D.C. have an operational PDMP, though utilization of these programs by providers in many states remains voluntary. In these states where providers have discretion in whether or not to refer to the PDMP prior to prescribing an opioid (or another controlled substance), utilization rates tend to be quite low, hovering between 14-25% (Alexander et al. 2015), and unsurprisingly such voluntary PDMPs are found to have limited to no effect on opioid abuse. A growing number of states have enhanced their programs and

[4] Numerous additional problems have also been identified with how opioids are prescribed, including overlapping or early refill of prescriptions, dose escalation, and high daily dose rates (Mack et al. 2015).

instituted universal registration and mandatory-access provisions, requiring providers to register on and query the PDMP prior to prescribing any controlled substance. Several individual state audit studies have shown that mandatory access PDMPs have effectively increased utilization and query rates.[5] There is an emerging consensus that these stricter programs have also therefore led to robust reductions in opioid misuse and related negative consequences. Mandatory-access PDMPs have reduced opioid misuse among Medicare Part D participants (Buchmueller and Carey 2018), and also reduced opioid abuse and opioid-related mortality among adults in the general population (Ali et al. 2017; Grecu, Dave, and Saffer 2018).[6] The CDC, U.S. Government Accountability Office, and the President's Commission on opioid abuse have all stressed the importance of states mandating PDMP use among licensed prescribers, as an integral part of a comprehensive strategy to combat opioid misuse (U.S. GAO 2009; Christie et al. 2017).[7]

What remains unclear are the potential spillovers from these interventions, and any resulting success in reducing Rx drug abuse, on other outcomes. Opioid abuse has been linked with many adverse consequences including: higher health care costs (White et al. 2005), lower worker productivity (Hansen et al. 2011), more suicides (Borgschulte et al, 2018), and a complementary increase in cocaine and marijuana abuse (Dave et al. 2017). Given the links between drug abuse, mental health, and crime, policies that lead to changes in Rx opioid abuse may also generate spillover effects on criminal behaviors, which could have substantial

[5] For instance, the number of prescriber and pharmacist PDMP registrations increased by 77% and 680% respectively, existing but inactive accounts decreased by 50%, and queries increased from an average of 11,000 per month to 1.2 million per month, following New York's enactment of mandated use in August 2013. Enrollment in the PDMP database in Kentucky increased by 264% (and multiple provider episodes – "doctor shopping" – decreased by 52%) and queries in Ohio increased by 505% (and multiple provider episodes decreased over 40%) following the enactment of mandatory access PDMP provisions). See Grecu et al. (2018) and http://www.namsdl.org/library/27CD066B-AF5B-BF3E-9B06857DF279C60A/.

[6] Note that these studies also found that merely having an operational PDMP without mandated access is largely ineffective.

[7] See: https://www.cdc.gov/drugoverdose/policy/index.html.

economic effects. Florence et al. (2016) estimate criminal justice costs of about $8 billion annually related to Rx opioid abuse.

We provide one of the first studies of the impact of PDMPs, and in particular the impact of recently popular mandatory access provisions, on an important societal outcome, crime. Our study also speaks to the larger question of how policies that restrict access to Rx opioids per se within the healthcare system would impact broader outcomes such as crime. While such restrictions can reduce overall abuse, leading to a potential decrease in crime, if abusers are resorting to other illicit drugs or more dangerous supply channels to continue their habit then such policies may generate unintended costs through greater engagement in crime and violence. Given the growing literature on the impact of PDMPs on the misuse of opioids, and the well-documented link between substance abuse and crime (Carpenter 2007; Pedersen and Skardhamar 2010), enhanced PDMPs could have a considerable external impact on crime. Moreover, given the substantial costs associated with crime in general, and the fact that crime associated with opioid use is particularly costly (Hansen et al. 2011), if there are spillovers on criminal engagement, then they are likely to be of an order of magnitude that is economically significant.

Many states have yet to enact these stringent provisions to their PDMPs, and some providers resist using the PDMP due to time constraints, learning costs, and because often times these databases are not well-integrated into the electronic medical records of the medical practice (Grecu, Dave and Saffer 2018).[8] There have also been some drawbacks to mandatory access PDMPs, which include additional costs to the healthcare system and compliance difficulties (Islam and McRae 2014). Hence, the overall value of these programs is still actively debated,

[8] For instance, challenges by some MA physician and dentist groups to the breadth of circumstances proposed for PDMP queries have contributed to a 2-year delay in the final implementation of a legally-required mandate (Haffajee et al. 2015).

despite recommendations from policymakers and public health organizations urging states to adopt these provisions. Failure to account for potential crime costs associated with these programs – either positive or negative – can substantially skew the cost-benefit calculus.

While a few studies have imputed the criminal justice cost burden associated with Rx opioid abuse (Hansen et al. 2011; Florence et al. 2016), these have been based on a descriptive apportionment approach and not meant to be interpreted as causal estimates. To the best of our knowledge, this is the first study to specifically inform the causal link between Rx opioid abuse and crime. In particular, we exploit variation in the timing of the implementation of PDMPs and enhanced mandatory access provisions across states, within a difference-in-differences research design. We find consistent evidence that the mandatory provisions are associated with a significant reduction in overall crime, on the order of about 3-4%. This decrease is mainly driven by violent crimes, with the largest effects expectedly realized among young adults. Consistent with the prior studies that do not find voluntary PDMPs to be effective in reducing opioid misuse, we also do find that voluntary PDMPs do not affect crime and the only effects are in states that implemented mandatory access PDMPs.

The remainder of the paper proceeds as follows. The next section briefly provides some background on the previous literature and the pathways through which Rx opioid abuse, and PDMPs, could impact crime. Section 3 describes the data sources, followed by a discussion of the empirical methods in Section 4. We present the results and robustness checks in Section 5, and the concluding section summarizes our findings and places them in context along with some policy implications.

2. Background

2.1 PDMPs and Opioid Abuse

A large literature has studied the effects of prescription drug monitoring programs, which can be separated into earlier studies that used data predating most of the mandatory access provisions and more recent work that has specifically assessed the effectiveness of voluntary vs. mandatory access PDMPs. Many of the studies based on older data, or data which do not differentiate voluntary vs. mandatory access programs, find very limited or nil effects of the programs on measures of opioid use and misuse (McDonald, Carlson, and Izrael, 2012; Reifler et al., 2012; Jena et al. 2014). Haegerich et al. (2014), in their review of the evaluation literature on state policy programs targeted at addressing Rx drug overdose, conclude that these previous studies "have not clearly established significant effects on total opioid prescribing or health outcomes with PDMPs" (p. 37). These inconsistent and limited effects are likely driven by the low provider query rates in states that do not mandate PDMP use. As stressed in the GAO report (U.S. GAO 2009), in order for PDMPs to work to their fullest potential, prescribers and dispensers must refer to the data prior to prescribing and filling a prescription.[9]

[9] PDMPs are enacted and operationalized at the state-level; thus, each state follows its own mode of monitoring and enforcing that healthcare providers are utilizing the PDMP where mandated. Different state agencies may be responsible for administering the PDMP, including substance abuse or consumer protection or licensing agencies. In the majority of states (36 states), however, PDMPs are administered either by the state's board of pharmacy or the department of health (Grecu et al. 2018). The state's appropriate licensing board – typically the medical board and/or the board of pharmacy – has the authority to impose (or refer to the appropriate licensing agency to impose) disciplinary actions that can include revocation, suspension, or non-renewal of the provider's license for inappropriate prescribing of opioids and failure to register on and refer to the PDMP. Any licensed prescriber who fails to register on the PDMP and query the system, or fails to submit the accurate prescribing information or inappropriately prescribes controlled substances, is also subject to other civil or criminal penalties as defined in each state's legislation, which can vary across states. Referral to law enforcement agencies, however, is generally confined to cases wherein physicians are prescribing for diversion purposes; a warning or license suspension (following multiple warnings) is relatively more typical for non-compliance with PDMP mandates. States can also conduct frequent and automated analyses of their PDMP – generating reports on providers who exhibit problematic prescribing and dispensing – and use this information to investigate further and impose warnings and disciplinary actions as necessary. Disciplinary actions can result from such regular audits as well as from complaints originating from dispensers, law enforcement, or consumers regarding any inappropriate prescribing. States also can establish a Medicaid Fraud Control Unit to investigate suspicious behavior based on PDMP information.

The recent wave of studies has moved this literature forward by specifically disentangling the effects of voluntary vs. the more recent mandatory access PDMP provisions. They find robust evidence of significant declines in opioid misuse and related adverse health consequences from mandatory access PDMPs but generally not from programs with no utilization mandates. For instance, Buchmueller and Carey (2018) find that mandatory access PDMPs significantly reduced measures of misuse, including excessive quantity and doctor shopping behaviors, among the Medicare Part D population. Their results reflect a 5-6% decline in the share of opioid takers with overlapping claims (multiple scripts for the same drug at a point in time) and with more than seven months supply, and an 8-16% drop in doctor shopping behavior (share of individuals obtaining opioids from five or more prescribers and pharmacies).

Ali et al. (2017), based on self-reported information from the National Surveys of Drug Use and Health, also find a significant drop in doctor shopping (defined in their data as obtaining Rx drugs from two or more doctors) and a reduction in the number of days of misuse at the intensive margin (by about 42% relative to the mean).[10] Grecu, Dave, and Saffer (2018) assess the effects of these policies on substance abuse treatment admission flows stemming from various Rx drugs and on mortality from drug poisonings. They also confirm the broader findings and find statistically and economically significant reductions in these measures of abuse, with the largest effects concentrated on Rx opioid abuse and among young adults ages 18-24 (32% decline in treatment admissions and 26% decline in opioid-related mortality).[11] Their study does not find any substantial effects associated with voluntary PDMPs.

[10] They do not report marginal effects, but find approximately a 24% decline in the odds of doctor shopping associated with the must-access PDMP policies relative to voluntary PDMPs.

[11] The effectiveness of mandatory access PDMPs is driven by the sharp increase in utilization and query rates. For instance, the number of registered prescribers and pharmacists increased by 77% and 680% respectively, existing but inactive accounts decreased by 50%, and queries increased from an average of 11,000 per month to 1.2 million

2.2 Substance Abuse, PDMPs and Crime

All of these studies that have evaluated the impact of PDMPs have assessed measures of Rx drug abuse or associated health indicators, and at best assessed spillovers into the use of other drugs. We are not aware of any work that has evaluated broader spillovers of these interventions on total crime. Given the robust and consistent findings from this literature that utilization mandates have been highly effective, it is plausible that the reduction in Rx opioid abuse may also impact criminal behaviors. Broadly, substance use can affect crime through three pathways, including a pharmacological effect by affecting aggression or violent tendencies, an economic effect whereby drug users may resort to income-generating crime in order to finance their drug use habit, and/or a "systemic" effect as participants interact in illicit markets that inherently tend to resort to a high degree of violence and criminal activity in their sales and distribution networks (Corman and Mocan 2000).[12] These channels also point to important effects on both violent and property (income-generating) crime.

Various prescription drugs, including certain opioids and others that are likely to be abused, have been linked to reports of violence towards others. Based on data on adverse drug events reported to the FDA, Moore et al. (2010) find that many anti-depressants, sedatives, and drugs for attention deficit hyperactivity disorder are associated with serious acts of violence; oxycodone, an opioid, was among the top 20 Rx drugs associated with violence adverse drug events.[13] Opioid-dependent fathers tend to be more violent towards their intimate partners (Moore et al. 2011), and behavioral symptoms of Rx drug abuse can include excessive mood

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per month, following New York's enactment of mandated use in August 2013 (see: http://www.pewtrusts.org/~/media/assets/2016/12/prescription_drug_monitoring_programs.pdf).

[12] Violence occurs in drug markets partly because consumers and suppliers are not able to rely on contracts and the court system to resolve disputes.

[13] Number of violence cases for oxycodone was over 4 times greater than for all other evaluated drugs, adjusting for the volume of reports.

swings and hostility.[14] To the extent that policies such as mandatory access PDMPs are effective in reducing Rx drug and opioid abuse, and reducing the use of potentially complementary substances such as cocaine and alcohol with similar links to psychopharmacologic violence (Davis 1996; Corman and Mocan 2000), they may reduce aggression and lead to a reduction in violent acts. On the other hand, Rx drug withdrawal is also associated with agitation, anxiety, depression, and irritability (WHO 2009), and restricting access to Rx drugs could increase violent acts by exposing drug users to protracted periods of withdrawal. Decreased use and abuse of addictive substances, and better mental health, have generally been linked to lower rates of both property and violent crime (Grogger and Willis, 2000; DeSimone, 2007; Cuellar et al., 2004; Markowitz, 2005; Marcotte and Markowitz, 2011; Fryer et al. 2013).[15] Though these studies focused on illicit drugs such as cocaine and heroin, the broader causal link underscored here may also carry over to Rx opioids.

More specific to Rx opioid abuse, doctor shopping has been found to be a significant source of diversion (Simeone 2017), including sourcing street dealers. Underground drug markets are particularly associated with violent crime as well as property crime. In this context, mandatory PDMPs represent an adverse supply shock not just for those who may be accessing opioids for non-medical use through the healthcare system but also for those who may be obtaining Rx opioids on the street. To the extent that this may lead to further declines in Rx drug abuse, criminal activity – both income-generating and violent crime – may decline.

On the other hand, disruptions to access of Rx drugs may also generate perverse or even no effects on crime through potential substitution and compensatory behaviors. For instance, in

[14] See https://www.mayoclinic.org/diseases-conditions/prescription-drug-abuse/symptoms-causes/syc-20376813.
[15] In order to bypass the endogeneity between substance abuse and crime and between mental health and crime, these studies rely on natural experiment and exogenous shocks, for instance exploiting changes in illicit drug prices, emergence of crack cocaine, and mental health treatment.

the context of methamphetamines, large supply-side disruptions have not been found to have any major effects on violent or property crime, and any transient changes in prices and indicators of abuse returned to pre-interventions levels within 4-18 months (Dobkin and Nicosia 2009). In the context of Rx opioids, both substitution to other supply sources for the same Rx drugs as well as substitution to other illicit drugs are possible. Given that doctor shopping and physicians are an important supply source for patients who misuse opioids, constraining this access may lead them to seek out underground channels outside the healthcare system.

There is some emerging evidence that supply-side interventions that limit access to opioids may increase the use of some other illicit substances. Notably, the reformulation of OxyContin into an abuse-deterrent formulation, and its market entry in 2010, has been found to be associated with a sharp increase in mortality from heroin overdose (Alpert, Powell & Pacula 2017; Evans & Lieber 2017). Interactions with supply and distribution networks in illicit drug markets have been especially prone to violence, gang activity, and crimes involving guns.[16] Furthermore, the street price of Rx drugs tends to be considerably higher than the pharmacy price (Sajan et al. 1998; Surrat et al. 2012; Dasgupta et al. 2013), raising the total cost of access for a user substituting from the formal healthcare system to underground sources. Thus, if some users are now substituting to these underground supply sources as a result of the PDMPs, then this may lead to an increase in violent crime and possibly property crime.

In the only other study on PDMPs and spillovers into non-health outcomes that we are aware of, Mallatt (2017) finds some increase (about 11% on average) in heroin crime incidents,

[16] Drug use has been found to be correlated with aggressive and violent behavior (Murray et al. 2008), and in terms of drug epidemics, the rise of heroin in the 1970's and the crack cocaine epidemic of the 1980's were both associated with substantial increases in violent crimes, including gun crimes and homicides (Szalavitz and Rigg 2017).

with stronger effects in counties which had higher rates of opioid prescribing at baseline.[17] Based on descriptive trends, some studies have linked the recent increase in homicide rates to the re-emergence of heroin and transition from Rx opioids to other illicit opioids (Rosenfeld 2016). Rosenfeld (2016) notes that the greater demand and entry of more users into the illicit drug market leads to greater opportunities and incentives for the sellers, and more disputes among sellers over territories and customer access and more disagreements between sellers and buyers can lead to greater violence.

At the same time, studies directly linking interventions targeted at Rx opioid abuse and spillovers on other illegal drugs are limited and have not reached a consensus. Meinhofer (2017) considers how mandated PDMP use has impacted spillovers into the illegal drug market, and finds only weak evidence of an increase in illegal drug deaths. Grecu, Dave, and Saffer (2018) find that mandated PDMP use did not significantly affect indicators of heroin abuse, but did reduce measures of cocaine and marijuana abuse.[18] In contrast to the supply disruption caused by the entry of abuse-deterrent OxyContin, these studies find very weak adverse or even beneficial spillovers on other illicit drugs.[19]

The upshot of this discussion is that the overall effects of disruption to Rx opioid access on criminal behaviors are a prior indeterminate. The net effects depend on the extent of potential substitution into other illicit drugs vs. the overall reduction in the pool of addicts. The various

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[17] Mallatt (2017) focuses solely on heroin crime, in order to gauge spillovers from restricted access to Rx opioids on the illicit opioid – heroin – market.

[18] Degenhardt et al. (2005) exploit a supply shock in Australia in 2001, which sharply reduced heroin supply, and find a transient increase in cocaine use among injecting drug users, which was associated with an increase in violent crime.

[19] Doleac and Mukherjee (2018) study the effects of Naloxone (an opioid antagonist, effective at reversing overdose from Rx opioids) access laws, and find an increase in opioid-related theft associated with greater access to Naloxone. They attribute this to an ex ante moral hazard effect and to change in the composition of the population towards surviving active drug users, who are more likely to commit such crimes.

reinforcing and/or counteracting channels also suggest that there may heterogeneous responses across crime types, and in particular point to potentially important (negative or positive) effects on violent crime, which generate much of the societal costs associated with crime (McCollister et al. 2010). To the best of our knowledge, we provide the first study on the broader spillover effects of PDMPs on total crime and across specific crime categories. As policies and interventions proliferate at the federal, state, and local levels targeted at curbing the opioid epidemic, it is important to account for spillovers on other outcomes and markets. Hence, our study contributes more broadly towards understanding how supply-side interventions which disrupt access to Rx opioids impact crime. Finally, this study contributes to the larger literature on the effects of substance use on crime, providing evidence on the causal link between Rx opioid abuse and crime by exploiting the adoption of the mandatory PDMP provisions as a source of exogenous variation in access to and diversion of Rx drugs.

3. Data

3.1 Crime

We assemble measures of crime using data spanning 2003-2015 from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR) monthly files, and use three separate datasets within the UCR, each providing complementary strengths.[20] All law enforcement agencies that operate under a U.S. jurisdiction, state, county, city, university/college, tribal and federal law enforcement agencies, submit crime data to the UCR, either through a state UCR program or directly to the FBI's UCR program. These files include the most commonly reported violent and property crimes (Part I crimes) including murder, manslaughter, rape, robbery,

[20] This is important and provides a validation check, given the inherent difficulties in measuring crime, a limitation not unique to our study.

assault, burglary, larceny, and motor vehicle theft. Between 88 to 96 percent of the U.S. population is covered by agencies that report to the FBI's UCR Program (Maltz 1999). In our primary analysis, we use the Offenses Known and Clearances by Arrests segments of the UCR. Our main outcome is the number of offenses for all known crimes divided by the population covered by an agency (per 100,000 residents). In addition to total crime, we estimate effects on Part I violent crime (homicide-murder and manslaughter, rape, robbery, assault and simple assault) and Part I property crime (burglary, larceny, and motor vehicle theft), as well as separately for each of the disaggregated crime types.

Known crimes are considered the most accurate crime outcome as they are not an endogenous function of police enforcement; however, a drawback is that data on known crimes do not include information about the offender. Thus, we also supplement our main analyses with information from the UCR Arrest Data, which are valuable for two reasons. First, arrest data include information on drug-related crimes. In particular, UCR Arrests report information for sale, manufacture or possession of: (1) opium/cocaine/derivatives, (2) marijuana, (3) synthetic narcotics, and (4) other dangerous non-narcotics. Second, arrest data include information on the demographics of the offender, which allow us to determine whether the propensity to commit crime changed in response to PDMP implementation differentially by age. Since young adults ages 18-24 are more likely to engage in criminal activity in general and also the most likely to adjust their opioid abuse patterns in response to the implementation of PDMPs (Grecu et al. 2018), we expect the effect on crime among individuals of this age group to be disproportionately impacted by the policy. We further supplement our analyses with data from the UCR Supplementary Homicide Reports (SHR), with the added advantage that they contain information regarding the age of both the offender and the victim, albeit only for homicides.

Finally, it is important to note that the UCR data are reported at the agency level. In order bypass heterogeneity in the reliability of reporting across agencies, we follow the crime literature and present separate analyses for large agencies (covered population of over 10,000 residents) and agencies that reported crimes consistently over all 12 months of the year.

3.2 Prescription Drug Monitoring Programs

To model the impact of PDMP legislation on crime, we use dates on which a given state's PDMP became operational, derived from the National Alliance for Model State Drug Laws (NAMSDL). While PDMP programs have been in existence for quite some time, in 2003 the Department of Justice began supporting initiatives to implement PDMPs, and the NAMSDL published the Model Prescription Monitoring Program Act and appropriated funds for its deployment (Dekker 2007). Thus, we chose to begin our analysis period in 2003, which provides a sample of PDMPs that are more homogeneous and potentially more effective across states. The NAMSDL also contains information about mandatory-access provisions. These stronger statutes were implemented, starting in 2007 and required all licensed prescribers and dispensers to register on the PDMP and to query the PDMP prior to prescribing or dispensing controlled substances.

Mandatory access broadly requires that providers must access to the PDMP in at least some circumstances, and there is some heterogeneity across states. For instance, Delaware mandates access in the strictest sense in that it requires that both prescribers and dispensers must access the PDMP before writing and dispensing any script for Schedule II-IV controlled substances. In contrast, some states "mandate" access, though only in limited circumstances or not for all providers. For instance, Georgia only requires that physicians practicing at a pain clinic must regularly check the PDMP on all new and existing patients. North Carolina requires that a medical director of an opioid treatment program must use the PDMP upon admission of a new patient and continue thereafter. We construct an indicator for mandatory access provisions based on the strictest definition, capturing states that require both physicians and pharmacists to register on and query the PDMP prior to prescribing and/or dispensing any controlled drug.

3.3. Other Drug and Alcohol Policies

In order to account for other confounding shifts, we control for several additional policies and laws that were enacted over the sample period and which may also potentially have impacted drug use and crime. Specifically, we control for ID Laws, which require pharmacists to request and check identification prior to dispensing controlled substances, and physical exam requirement (PER) laws, which require a physical examination or a bona fide physician-patient relationship prior to prescribing controlled substances. Dates of implementation of ID and PER laws are obtained from the National Conference of State Legislatures and the Centers for Disease Control and Prevention, and are cross-validated with the review of individual state legislatures and the Federation of State Medical Boards.

We further control for Naloxone access laws, which expand access to Naloxone to people other than the person at risk of overdose in order to facilitate friends and family of the abuser to administer the opioid antagonist in case of an overdose (Rees et al. 2017). We also control for Good Samaritan Laws, which exempt those who seek medical assistance for someone experiencing overdose from arrest and prosecution for minor drug and alcohol law violations (Rees et al. 2017). Information on these laws is obtained from the Policy Surveillance Program, which is funded by the Robert Wood Johnson Foundation and the Network for Public Health Law.[21]

[21] https://www.networkforphl.org/_asset/qz5pvn/network-naloxone-10-4.pdf

We also control for “pill mill laws”, which typically require all pain management clinics to be certified by the state’s medical board on a recurring basis, and to be owned and operated by a licensed physician. Under these laws, state medical board employees also inspect pain management clinics in order to verify compliance and revoke physicians’ licenses for failure to comply with its provisions (Lyapustina et al. 2016). Finally, we control for policies pertaining to marijuana legalization, marijuana decriminalization, and medical marijuana, and control for beer taxes, cigarette taxes and whether the state has 0.08 blood alcohol content (BAC) per se limit laws.

3.4. Demographic and Police Composition Data

Police department employment data were obtained from the UCR Program Data: Police Employee (Law Enforcement Officers Killed and Assaulted Program - LEOKA) from 2003 to 2015. Specifically, three control variables are constructed from this dataset: the natural logarithm of the number of officers and the number of employees in the police force. We control for the state-level demographic composition using data from the bridged-race population estimates, which are produced by the U.S. Census Bureau in collaboration with the National Center for health Statistics (NCHS).[22] In particular, we construct the share of the population composed by minors, individuals ages 18-25, males under 18 years of age, and males ages 18-25 years, as well as the overall share of males. Income per capita and seasonally adjusted unemployment rates are obtained from the U.S. Bureau of Labor Statistics and the County Business Patterns (CBP), and account for shifts in the state’s economy.[23] Finally, we control for the poverty rate, marriage rate,

[22] https://wonder.cdc.gov/controller/datarequest/D9

[23] Note that this dataset provides annual statistics for businesses with paid employees and excludes mostly establishments with government employees.

and the share of residents with a college degree, some college, high school, less than high school.[24]

4. Methods

Our empirical analysis is motivated by the mechanisms described above through which mandatory PDMPs, which have been shown to significantly reduce Rx opioid abuse, can have spillover effects on crime. To assess these relationships, we exploit variation in the timing of PDMP implementation across states, and estimate the following difference-in-differences (DD) specification:

$$Y_{jst} = \alpha_0 + \beta_0 Post_{st} + \beta_1 Post_{st} * MA_{st} + \delta X_{st} + \gamma_t + \gamma_j + \gamma_s * t + \varepsilon_{jst} \quad (1)$$

Equation (1) can be interpreted as a reduced-form crime supply function. The analysis is at the agency-year level, and the outcome (denoted by Y_{jst}) represents the rate of offenses known to police per 100,000 residents in a given agency *j*, in state *s* and year *t*. Models are estimated for all Part 1 crimes, and separately for violent and property crimes. The variable $Post_{st}$ is a dummy variable that indicates if a state has an operational PDMP in place, and MA_{st} is a dummy variable indicating that the state has enhanced its prescription drug monitoring program and implemented mandated-access provisions. The coefficient of interest is β_1, which represents the net reduced-form effect of mandated PDMP use, relative to states that have an operational but voluntary PDMPs, operating through all reinforcing and/or competing channels as discussed earlier. Given that all states currently have an operational PDMP in place, β_1 provides the

[24] These measures are obtained from the University of Kentucky Center for Poverty Research Welfare Data (http://www.ukcpr.org/data), and alternately computed from the Annual Social and Economic Supplement of the March Current Population Surveys.

relevant average policy response of a state strengthening their PDMP with strict mandatory access provisions.[25]

All specifications control for an extended vector of socioeconomic and policy factors (X_{st}) including demographic information (share of population composed by minors, individuals age 18-25, males under 18 years of age, males 18-25 years of age, males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, pill mill laws, BAC laws, beer taxes, cigarette taxes), police composition (number of officers and employees in the police force) and other socioeconomic variables (income per capita, unemployment rate, poverty rate, marriage rates and share of residents that have a college degree, some college, high school, and less than high school).

All specifications further include agency fixed effects (γ_j), and year fixed effects (γ_t). The agency fixed effects account for time-invariant differences across agencies (and hence time-invariant differences across states, since agencies are nested within states). Time fixed effects account for national trends in crime rates over the sample period. We further include state-specific linear trends ($\gamma_s * t$) to control for unmeasured systematically time-varying confounding factors across states, which can account to some degree for systematic differential trends across implementation vs. non-implementation states prior to the policy. Standard errors are clustered at the state level, and all models are weighted by the population covered by the agency (Angrist and Pischke 2007).

We extend the baseline model in several ways. In addition to aggregated Part 1 crimes, we perform secondary analysis where the effect of PDMPs is evaluated by offense since

[25] The average policy effect of a state shifting from no PDMP to a mandatory PDMP would be $ß_0 + ß_1$.

aggregated crime may mask nuanced changes in relatively infrequent crimes. Specifically, the following crimes are evaluated: homicide (which combines murder and manslaughter), rape, robbery, assault, burglary, larceny, motor vehicle theft and simple assault. Also, as a robustness check, we estimate Equation (1) using agency-level data on three different subsamples: large agencies or those agencies with at least 10,000 residents, agencies that report offenses consistently over all 12 months of the year, and large agencies that consistently report offenses over all 12 months.

Furthermore, drawing on the previous literature that has documented significant heterogeneity across age groups with respect to the non-medical use of opioids, we estimate the impact of PDMPs on crime conditioned on age group. While information on the offender's age is not contained in the UCR Offenses Known Segment, we can observe the offender's age in the arrest data, which have also been commonly used in the crime literature (Corman et al. 2014). Specifically, we re-estimate Equation (1) for total arrests across four different age groups (18-24, 25-44, combined 18-44 and 45+). Another advantage of these data is that they allow separate analyses for drug-related arrests, which we capitalize on to assess effects on arrests related to specific categories of drugs.

Finally, we also estimate the effect of PDMPs on the demographics of the offender and victim with respect to homicide incidents. In particular, we examine the following dependent variables: the rate of homicides where the offender (victim) was a male (female) between the ages of 18-24, 25-44, 18-44, and 45 and over, as well as the rate of homicides where the weapon used was a firearm or involved a knife/beating. The rates are computed as the counts of homicides in the agency per 100,000 state residents.[26] If PDMPs impact violent crime, and

[26] Homicides are expected to be linked to opioid use due to the strong evidence that violence is a consequence of prohibiting addictive substances, specifically homicide and gun violence (Werb et al. 2011). In particular, studies of

homicides in particular, we expect the strongest impact among offenders who are young adult males - the group whose opioid abuse and adverse health events are most impacted by mandatory PDMPs (Grecu et al. 2018).

A critical assumption necessary for the DD research design to credibly identify the causal effect is that trends in non-implementation states are a valid counterfactual for trends in implementation states in the absence of mandatory access provisions (Angrist and Pischke 2007; Colman and Dave 2018). While we partly account for any deviations from this "parallel trends" assumption by including state-specific linear trends in all models, we also conduct a fully-specified conditional event study based on the following specification.

$$Y_{jst} = \alpha + \sum_j \beta^k I[D_{st}^k = 1] + \delta X_{st} + \gamma_t + \gamma_j + \varepsilon_{jst} \qquad (2)$$

As before, the outcome is the number of total Part I offenses per 100,000 residents. In this specification D_{st}^k is an indicator that has the value of one when state *s* has enacted a PDMP *k* years away from the contemporaneous period. Note that when k<0 it indicates lead pre-policy effects, that the PDMP will be enacted *k* years in the future, and when k>0 it indicates post-policy effects, that the PDMP program was enacted *k* years in the past. Also note that because different states implemented PDMPs at different times, the sample is unbalanced in event time. To overcome this issue, we follow Kline (2012) and focus on event-time coefficients within a five-year window. Outside of the five-year window we impose endpoint restrictions or "bin up" this analysis by using one pooled, period-invariant coefficient for periods more than five years before the PDMP was enacted and another period-invariant coefficient for time periods more than five years after the PDMP was enacted. Analysis is performed for all states and separately

drug gangs show that a significant amount of gang activity involves homicide and assault (Levitt and Venkatesh 2000; Rainbow 2010; Klein, Maxson and Cunningham, 1991) and particularly gun-related homicide (Miron 1999; Levitt and Rubio 2005).

for mandated access states. We normalize β^{-1} to zero and therefore all parameters β^{k} should be interpreted as the policy effect on crime relative to the year prior to implementation. Finding that the lead effects (β^{k}=0 for k<0) are insignificant and close to zero in magnitude would indicate that treated and control states did not differ with respect to pre-policy trends in crime, and thus would provide some validation of the parallel trends assumption.

The event study framework performs two functions. First, it allows us to directly test for differential pre-policy trends by evaluating the magnitude and significance of the lead coefficients (k<0). Second, it allows us to decompose the dynamics of the main DD effect from Equation (1). That is, the main DD effect represents the average effect on crime over the post-policy window. For instance, Grecu, Dave and Saffer (2018) show that, while mandatory access provisions are highly effective, the effects become stronger over time. This compounding is partly due to the diffusion of physician knowledge and training as they become more versed with using the PDMPs and partly because there may be lags in the disruption to supply due to stockpiling. Furthermore, even if access to Rx opioids is disrupted, alternate sources may substitute over time. The event study allows us to capture any such dynamic effects that may either accumulate or dissipate over time.

Finally, to evaluate the validity of our empirical estimates we perform a placebo check similar to the randomization inference outlined in Abadie and Gardeazabal (2003). In this falsification exercise, agency-year indicators for whether mandated access is active are re-shuffled and randomly assigned. Equation (1) is then re-estimated with this placebo or "shuffled" pseudo-PDMP indicator, and this process is repeated 100 times, each time using a different set of placebo indicators. Once the estimation is complete, all 100 placebo coefficients are plotted and compared with the results of our primary DD analysis.

5. Results

Using crime data from the UCR, we estimate a DD model to evaluate the impact of PDMP laws on crime. Section 4.1 presents summary statistics. Section 4.2 presents the results of the primary DD analysis for the impact of PDMPs on Part I Crimes. Sections 4.3 and 4.4 present effects on arrests and homicides across separate age groups, and on the demographics of offenders and victim. Section 4.5 presents the results of the event study and section 4.6 presents results of the falsification diagnostic and other specification checks. We conclude the section by placing our crime effects in context and framing them within prior estimates on opioid abuse.

4.1. Summary Statistics

Table 1 presents the summary statistics for our analysis samples. The first column presents the entire sample (208,069 agency-year cells). The second column is restricted to agencies that have at least 10,000 residents (71,299 agency-year cells) and the third column is restricted to agencies that report offenses over all 12 months (157,551 agency-year cells) of the year. Panel A presents summary statistics from the UCR Offenses Known Segment aggregated at the agency-year level, and Panel B presents summary statistics for our key PDMP measures, and other policy and demographic controls, which are at the state level.

In terms of crime rates, overall there were approximately 3,531 crimes per 100,000 residents with 1,473 (42%) of these constituting violent crimes and the other 2,072 (58%) being property crimes. When restricting the dataset to agencies with at least 10,000 residents, these rates slightly increase (4,214 overall, 1,809 violent and 2,405 property crimes). Also, agencies reporting over all 12 months of offenses expectedly have a slightly higher number of crimes (4,571 overall, 1,897 violent and 2,677 property) than all agencies.

Over our sample period, the number of states with an operational PDMP (either voluntary or mandatory) increased from 1 state in 2003 to 48 states in 2015. [27] Starting in 2007, states started to require that prescribers and dispensers register on the PDMP and query the database prior to prescribing and dispensing a controlled drug. The number of states with these mandatory PDMP provisions increased from 0 in 2003 to 11 in 2015, and in 2015, 19% of the population resided in states with mandatory-access PDMPs.

4.2 Effect of PDMPs on Part I Crimes

Table 2 presents the coefficients β_0 and β_1 from Equation (1) using agency-year level data, where the dependent variable is the number of offenses per 100,000 residents. We present estimates for total Part I offenses, and then separately for violent and property offenses. Panel A presents the coefficients for the universe of agencies with at least 10,000 residents that report offenses all 12 months, Panel B presents the results for large agencies, and Panel C reports the results for agencies that report across all 12 months.

The estimates in Table 2 suggest three main findings. First, there is no indication that voluntary PDMPs have had any economically or statistically significant effect on crime. This is consistent with much of the prior work that concludes that just having an operational PDMP without any mandate on providers to query the databases has not been effective in reducing Rx drug or opioid abuse (Haegerich et al. 2014; Buchmueller and Carey 2018; Grecu, Dave and Saffer 2018). Second, we do find evidence that mandatory-access PDMPs significantly decrease crime. Specifically, mandatory PDMPs are found to significantly reduce overall crime by between 3.3 – 3.6% (Panels A – C), relative to voluntary PDMPs. Third, the estimates also suggest that much of the realized decrease in crime is driven by a reduction in violent crime.

[27] Hawaii enacted a voluntary operational PDMP legislation in 1996.

Enacting mandatory provisions to the PDMP significantly reduces violent crime by 5.5 – 6.9%. The effect on property crime is insignificant at conventional levels, with estimates ranging from a 1-2% reduction in property crime.

We decompose these aggregate crime effects across specific offenses in Table 3. Again, voluntary PDMPs are consistently found to have no significant effect on any of the offense types; the coefficients are statistically insignificant, generally close to zero, and much smaller in magnitude than the effects of mandatory access provisions. In contrast, mandatory-access PDMPs have a significant negative effect on homicide, assault and simple assault. Estimates suggest that mandatory-access PDMPs reduced homicides by about 0.5 per 100,000 residents (16% decline compared to the baseline mean), relative to voluntary PDMPs. Note, homicides tend to be a rare event and have several agency-year cells with zero counts. To better capture the skewness and zero counts, in Table A3 we present estimates from fixed-effects Poisson regression and results indicate a 5.7%-7% reduction in homicides.[28]

We also find a statistically significant decrease in assault by 52.3 assaults per 100,000 population (5%) and a decrease of 55.4 simple assault cases per 100,000 population (6.6%).[29] We do find a relatively sizeable decline in one type of property crime, notably motor vehicle theft (about 10.5%), though the effect is highly imprecise and not significant.[30]

4.3 Effect of PDMPs on Arrests

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[28]The Poisson model could not be estimated with agency fixed effects and hence were estimated with agency-level data but with state fixed effects. Results are robust to estimation via negative binomial regression.

[29] In addition, we computed a pairs cluster percentile-t bootstrap (Cameron and Miller, 2015; Hansen, 2018; Cameron, Gelbach and Miller, 2008), where we resample states and for every resample compute a bootstrapped t-statistic for the true null hypothesis that the bootstrap coefficient is equal to the full sample coefficient. The percentiles of the bootstrapped t-statistics can be used to adjust up or down the full sample standard error. This more refined approach to inference leaves our main results unchanged in terms of their qualitative conclusions. See Cameron and Miller (2015) for a description of paired-clustered percentile t-bootstrap.

[30] Property crimes increased somewhat in the short term as a result of PDMP, which was accompanied by a longer-term decline (See section 4.5 for more discussion). Therefore, an average post-policy effect for property crimes may mask important dynamics.

Next, we turn to the arrest data, which allow us to observe the age of the offender and also assess effects specifically on drug-related offenses. Table 4 presents these estimates, where the outcome represents the number of arrestees within a particular age group per 100,000 agency residents. We expect to find relatively stronger effects among younger adults for several reasons. First, reflecting an age-crime gradient (Ulmer and Steffensmeier 2014), engagement in criminal activity tends to peak into late adolescence and early adulthood. Second, young adults ages 18-24, followed by adults ages 25-44, tend to have the highest prevalence of non-medical use of Rx drugs and abuse/dependence on pain relievers.[31] Third, prior work has shown that young adults, and in particular young-adult males, have experienced the largest decrease in opioid abuse and related mortality as a result of mandatory access PDMPs (Grecu et al. 2018).

In Table 4 we find validating evidence that mandatory-access PDMPs significantly reduced crime for younger adults (18 through 44), and not older adults (45+). Specifically, the stricter regulations are found to reduce Part I arrests by 4% and 4.8% among adults ages 18-24 and 25-44 respectively, with much smaller and statistically insignificant effects (1.8% decline) among older individuals (Panel A).[32]

Table 4 also presents estimates of how the PDMPs impact offenses specifically involving the sale, manufacture or possession of drugs. Panels B-E report estimates across separate drug categories involving: opium/cocaine/derivatives (e.g. morphine, heroin, cocaine and codeine); marijuana; synthetic narcotics (e.g. manufactured addictive narcotics such as Demerol and

[31] Data from 2014 National Survey of Drug Use and Health indicate that the prevalence of Rx drug abuse and dependence (pain reliever abuse and dependence) is 3 times (2.8 times) higher and 2.6 times (2.5 times) higher among adults ages 18-25 and ages 26-44 relative to adults 45+, respectively.

[32] Estimates are highly robust to the other analysis samples presented in Table 2. In addition, we computed a pairs cluster percentile t-bootstrap (Cameron and Miller, 2015; Hansen, 2018; Cameron, Gelbach and Miller, 2008) as described in footnote 29. After we adjusted the standard error using the percentiles of the bootstrapped t-statistics, our mail results remain unchanged in terms of their qualitative conclusions.

methadone), and other dangerous non-narcotics (e.g. Barbiturates and Benzedrine).[33] These results suggest that mandatory-access PDMPs significantly increased arrests related to opium, cocaine and derivatives for older adults (ages 45+) with no effect on other age groups. This pattern is suggestive of these individuals potentially substituting from Rx opioids to heroin following restrictions on access due to mandatory PDMP provisions, consistent with similar substitution found in previous work (Alpert et al. 2017; Evans et al. 2018). We do not find any significant effects on arrests related to these drugs for younger adults, which may reflect that substitution effects into other opioids and heroin may be weaker for this group. Note also that previous work has reported strong net decreases in opioid abuse among younger adults as a result of mandatory PDMPs (Grecu et al. 2018). Hence, if there is a decrease in Rx opioid abuse among younger adults, and some of these individuals are diverted into treatment and do not substitute into other illicit drugs, then we would not expect strong effects on drug arrests.

In addition, some recent work has found that opioids and marijuana may be substitutes, and that medical marijuana may be associated with a decrease in opioid use and abuse (Liang et al. 2018; Bachhuber et al. 2014).[34] Consistent with this literature, we find a positive effect of PDMPs on marijuana-related arrests (on the order of 3-9%); however, this effect is statistically insignificant at conventional levels. In Panel D we find large and negative, but imprecisely estimated effects, associated with the impact of PDMPs on arrests related to synthetic narcotics,

[33] See https://ucr.fbi.gov/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/offense-definitions

[34] Grecu et al. (2018) on the other hand find that restricting Rx opioids lead to complementary declines in marijuana abuse. Hence, the relationship between marijuana and opioids is far from clear in the literature, and the cross-effects may not be symmetric with respect to the effects of opioid policy on marijuana use vs. the effects of marijuana policy on opioid use and abuse.

and in Panel E we find a negative and significant association between PDMPs and arrests related to other "dangerous non-narcotics," such as barbiturates and Benzedrine.[35]

4.4 Homicide

Given that we find significant and relatively sizeable effects of mandatory-access PDMPs on homicide, we further evaluate the extent to which this is driven by a particular demographic group among offenders and victims. For this exercise, we use the UCR Homicide Supplements which report information about the demographics of the offender and victim, and details about the weapon used, among other details on the circumstances of the homicide. For consistency, we aggregate the data at the agency-year level.

Table 5 presents the results of the DD model estimated with the UCR Homicide Supplements where the dependent variable is the number of homicides per 100,000 residents that had a particular characteristic. For instance, Panel A presents evidence that the number of homicides committed by individuals between the ages of 18-44 per 100,000 residents decreased by 0.31, which represents a 7.7% decrease. When disaggregating this effect further, we find that the effect is mostly driven by a decrease in the rate of homicides committed by young offenders (age 18-24). On the other hand, mandatory-access PDMPs did not affect the rate of homicides committed by older individuals (ages 45 and older). In line with Panel A, Panel B also shows that the stricter PDMP provisions decreased the number of 18-44 year-old homicide victims per 100,000 residents, without affecting victimization among older individuals.

Panels C and D focus on the extent to which homicide patterns among males changed in response to the stricter PDMPs. First, we find that mandatory-access PDMPs have decreased the

[35] Prior studies estimate a large effect of drug restrictions on drug-related arrests. For instance, Lynch et al. (2003) document that a government disruption that tripled methamphetamine prices resulted in in a 50% decrease in felony methamphetamine arrests. Also, Doleac and Mukherjee (2018) estimate that access to naloxone results in a 17%-27% increase in arrests for possession and sales of opioids.

rate of 18-44 year-old male homicide offenders per 100,000 residents by 7.4% and the rate of 18-44 year-old male homicide victims by 14.3%. Second, the effect among male offenders is particularly large among younger males (18-24 years-old) while the effect among male victims is equally large among 18-24 or 25-44 year olds. Third, there is no effect of the PDMPs on homicides among older male perpetrators or victims. Panels E and F report parallel results on the homicide patterns involving females. We find a marginally significant decrease in the rate of young female offenders and no other significant effects.

Overall, these estimates from the demographic composition of homicides suggest that the decline that we find in homicides is primarily driven by a decline among young adult male offenders and victims (ages 18-44). There is no change in the rate of committing a homicide or being victim to one among older adults. These results are validating in that young men are the demographic group that has adjusted their abuse patterns the most in response to stricter PDMP implementation; our study finds that arrest rates among this group have also been the most responsive which is consistent with our findings with the homicide data.

Regarding weapon use, the results show that mandated PDMPs significantly decreased the rate of homicides that occurred with a firearm by almost 9%. To the extent that restricting Rx opioids (and other Rx drugs) through strict PDMP regulations affects the circumstances under which individuals access the illicit drug market, wherein interactions are particularly more likely to involve guns, one would expect more pronounced effects on gun-related homicides. Previous literature has found that the drug market contributes to violent disputes, murders, and non-fatal shootings with handguns (Maher and Dixon 2001; Blumstein 1995; Maher and Dixon 1999; Ramussen et al. 1993; Miron 1999; Levitt and Rubio 2005; Sullivan and Elkus 2008).[36] The

[36] On the other hand, prohibiting drugs or disrupting drug markets also lead to the inevitable consequences of gun violence and homicides (Werb et al, 2011).

reduction in homicides, and in particular homicides involving young adult males and involving handguns, suggest that mandatory access PDMPs, on the net, may not have increased interactions with the illicit drug market, which is prima facie consistent with the prior literature that found that these interventions resulted in a net decrease in opioid abuse and related health consequences.

As a consistency check on the data, we estimate whether the effect on the number of homicides per 100,000 residents using the UCR Homicide Supplement is in line with the results previously reported in Table 3 based on the UCR Known Offenses Segment. Table 6 reports that the mandatory-access PDMP regulations led to a reduction in the homicide rate by 4.3% overall and by 7.7% when the analysis is restricted to large agencies. These estimates are similar to the Poisson estimates presented in Table A3.

4.5 Event Study and Timing of the Effects

We visually present the event study in Figure 1. Specifically, the figure presents the coefficients β^j from Equation (2), for states with voluntary PDMP access only and then separately presents the coefficients for the states that implemented mandatory access PDMPs. Given the inherent noisiness of the crime data, a limitation not unique to our study, disentangling the timing of the effects is an imprecise exercise.

Our event-study results underscore three points, all of which instill a degree of confidence to our estimates. First, there is consistent, dynamic evidence that voluntary PDMPs did not impact crime in any significant manner. Given that most of the literature has found little to no first-order effects of these discretionary programs on opioid abuse, this result adds confidence to the validity of our model. Second, we find that the lead pre-policy effects are close to zero (e.g. the coefficients β^j are statistically indistinguishable from zero for t<0), which indicate that the reduction in crime only materializes after the implementation of the mandatory access PDMP. This suggests that mandated access PDMPs (or PDMPs in general) were not endogenously implemented in response to changes in crime trends. Finally, a reduction in crime materializes after the implementation of the mandatory access PDMPs, and this effect persists and slightly increases over time. Lagged effects are indicated in prior work, and plausible, given the time it takes for physicians to learn and become well-versed in accessing the databases. Also, this makes sense given the potential lags between restricted access to Rx opioids and substitution into alternate sources or diversion into treatment (Grecu et al. 2018).[37]

4.6 Additional Specification Checks

Figure 2 presents the results of our falsification test. In this figure, all 100 placebo parameters are plotted (small open circles), alongside our actual main policy effect. Visual inspection of Figure 2 suggests that our estimated effect of mandated-access PDMPs is considerably higher than the placebo estimates.

We implemented several additional checks to verify that our main results are robust to alternate specifications and adjustments for sampling issues, and to assess plausibility (results not shown unless indicated otherwise). First, we confirmed that our results are generally not sensitive to functional form, alternately specifying the outcome as the natural log of the crime

[37] Table A4 further explores the dynamic effects of PDMPs by presenting lagged policy responses for both property and violent crime. As before, estimates are imprecise and should be interpreted with caution. When we decompose the DD effects into shorter-term (0-1 years post-implementation) and longer-term effects (2-3 years and 4-7 years post-implementation), we find more consistent evidence that the stricter PDMP provisions are associated with a decrease in property crime over the longer term, by about 2-5% over 2-3 years following the enactment and by about 8-12% over 4-7 post. Prior work in the context of heroin (Moore and Schnepel 2018) also finds that a supply shock that increased the price of heroin by 400% resulted in a short term smaller increase in property crimes accompanied by a longer term decline; hence, an average post-policy effect for property crime may mask important dynamics as in our case. We find a similar pattern for violent Part I offenses; generally larger longer-term effects, on the order of 3.6% – 5% over 2-3 years and 13% – 19% over 4-7 years, however the sign of the effect of PDMP on violent crime is negative regardless of the time horizon.

rate (Tables A1 and A2) or the natural log of the count of offenses (or arrests).[38] Also, we separately control for the agency population, and estimate models via fixed effects Poisson or negative binomial (for homicides, as discussed above).

Additionally, we evaluate the sensitivity of our results to weighting. In our main analyses, we weight all models by the agency population. This produces a policy effect that represents an average over individuals (as opposed to an average over agencies, if the models are unweighted) and can also improve precision of the estimates since crime rates in a small agency may be more variable over time. However, our coefficient estimates, patterns of results, and general conclusions are not materially affected if the models are unweighted. Finally, given that the policy is measured at the state level, we also aggregated up all crime data to the state-year level, and re-estimated all specifications. Aggregation to the state-year level also does not materially impact our results.

4.7 Effects in Context

Our estimates thus far suggest that mandatory access PDMPs have led to a significant reduction in overall crime, in both violent and to some extent property crime, although effects are imprecise for property crimes. The effect magnitudes indicate about a 3.5% reduction in the total number of offenses overall, and specifically a 4% reduction in the number arrests among young adults age 18-24. These are reduced-form estimates directly linking the policy lever to a key societal outcome. We can combine the reduced-form effect on crime with the reduced-form effect on Rx drug abuse from the literature to impute an "implied instrumental variables" (IV)

[38] One potential issue with taking the natural logarithm of a crime rate is that some agencies may report a zero in certain years, even though this is unlikely given that crimes are aggregated between violent and property crimes. In order to take the zeros into account, we replace the number of offenses with 0.25, 0.5, 0.75 and 1 when the offense count is zero, hence not changing the number of offenses for those state-year cells that have an actual count above zero, and present those estimates in Table A1 and Table A2.

estimate of the structural effect of shifts in Rx drug abuse on crime.[39] Specifically, Grecu, Dave and Saffer (2018), based on similar DD and event-study specifications, find robust evidence that mandatory PDMPs reduced Rx opioid abuse among young adults ages 18-24 by between 26-32% (26% for opioid-related mortality, and 32% for treatment admissions). Combining these sets of estimates, the implied IV-based elasticity of total crime with respect to Rx opioid abuse is about 0.14 for young adults ages 18-24.[40]

We can further use these sets of estimates to project the numbers of arrests that could be prevented at the margin from reducing Rx drug abuse. In 2016, about 2.45 million adults (ages 18-25) misused opioid pain relievers (based on the 2016 NSDUH), and law enforcement made about 2.44 million arrests among this age group (based on the UCR Arrest files). The reduced-form estimates indicate that mandatory PDMP provisions may have decreased the number of young adult Rx opioid abusers by about 711,000 and decreased total arrests among young adults by about 97,600. This indicates that for every 7 to 8 or so fewer Rx drug abusers, about one arrest appears to have been averted. Thus, the marginal effect of Rx opioid abuse on arrests is also about 0.14 (97,600 / 711,000). This compares to an average probability of an arrest relative to having ever misused opioids, of about 0.31.[41] Hence, the marginal probability implied by our estimates is reasonable and "in the ball park"; that it is somewhat smaller than the average probability implies a concave crime production function with respect to Rx opioid abuse.

[39] Note that the causal effect of Rx drug abuse on crime (∂Crime / ∂Rx Abuse) can be decomposed as the ratio of two reduced-form effects: (∂Crime / ∂Rx Abuse) = (∂Crime / ∂PDMP) / (∂Rx Abuse / ∂PDMP)

[40] The reduced-form effect of the policy on crime is about 4% and the reduced-form of the policy on Rx opioid abuse is about 29% (26-32%), among young adults. Thus, the implied IV elasticity, akin to a Wald estimate, is: (-0.04 / -0.29) or 0.137.

[41] Data from the 2014 NSDUH indicate that among young adults ages 18-24, who had ever misused opioid pain relievers, 31.1% had reported being arrested.

While these imputed estimates help to frame the potential importance of PDMPs in affecting crime, help derive a structural causal effect of Rx drug abuse on crime, and also help assess the plausibility of the effect magnitudes, they are meant to be suggestive and should be interpreted with caution. The implied structural causal effect of Rx abuse on crime assumes that shifts in Rx opioid abuse are the only proximate channel through which mandatory access PDMPs affect crime, which appears plausible. Furthermore, small changes in the underlying reduced-form effects (numerator and denominator of the Wald estimate) can lead to large changes in the implied structural effect. Finally, the structural effect represents a local average treatment effect, capturing how Rx drug abuse impacts crime for the marginal abuser who is deterred from misusing Rx drugs due to the access restrictions (though they may substitute into other drugs, or transition into treatment and complete abstinence). Nevertheless, this exercise provides some validation that the effect sizes are of a plausible order of magnitude, being consistent with prior "first-order" effects of the policy on Rx opioid abuse and also consistent with descriptive data on the percent of opioid abusers who are arrested.

6. Conclusion

The abuse of opioids in the United States has quadrupled in the last 15 years and has reached epidemic proportions. In an attempt to mitigate opioid abuse almost every state has implemented a PDMP, and while the early literature on the effects of PDMPs did not find these programs to be effective, numerous recent studies have found a significant effect of mandatory-access PDMPs on both the misuse of opioids and opioid related deaths (Grecu, Dave, and Saffer 2018; Buchmueller and Carey 2018; Alpert, Powell, and Pacula 2017). However, there are costs associated with PDMPs, particularly mandatory-access PDMPs, and there is still some debate regarding the appropriateness of PDMP legislation.

Furthermore, while the recent opioid epidemic has its roots within the formal healthcare system and originated with the rapid growth in the prescribing of opioid analgesics, it is unclear how restricting Rx opioids per se within the healthcare system would impact societal outcomes and population well-being. On the one hand, such restrictions may reduce overall abuse and adverse health consequences, though there may also be unintended costs due to the possibility that abusers may substitute to other illicit drugs or more dangerous supply channels to continue their habit. This study contributes to this debate and presents some of the first empirical analyses on the broader spillover impact of PDMP mandates on non-health related domains. Our study also more generally informs the question of how policies that specifically restrict the prescribing of opioids (and other controlled drugs) impact an important societal outcome, overall crime.

We find consistent evidence that mandatory-access PDMPs, but not voluntary PDMPs, have led to a reduction in criminal activity. Our main estimates suggest that the stricter PDMP regulations reduced overall crime by 3.5%, which is primarily driven by reductions in violent crimes (5.5%), specifically assaults and homicides, and driven by young adult offenders (mostly 18-44 years of age). The age-specific result is especially validating given that it coincides with the previous literature that also indicates the strongest first-order effects of the PDMP mandates on opioid abuse among this subgroup of younger adults. There are also some suggestive decreases in property crime, particularly over the longer term with sizeable effect magnitudes (2-8%), but the estimates are highly imprecise. Though PDMPs were not implemented as a tool to fight crime, its implementation has affected crime to an extent comparable to more controversial and costly policies, such as increasing the size of the police force by approximately 10% (Evans and Owens 2007; Chalfin and McCrary 2007).

The suggestive decline in property crime is consistent with a large literature that links substance use to crime and finds evidence of an economic effect whereby addicts may resort to property crime as a way to fund drug habits (Carpenter 2007; Silverman and Spruill 1977; Manzoni et al 2006). The reduction in violent crime, and in particular homicides, reflects the pharmacologic and systemic effects linking substance abuse to crime. Prior work has found that mandatory PDMPs have led to robust reductions in opioid abuse and overdose-related mortality. Even if some of these individuals are substituting into heroin or alternate underground supply sources for Rx drugs, the reduction in crime we find implies that on the net the marginal Rx drug abuser, who is impacted by the PDMP restrictions, has less exposure to the illicit drug market (which is strongly associated with violence, homicides, and gun-related deaths; Werb et al. 2011), has less exposure to the pharmacologic effects of the drugs (which may further help to reduce violence and aggression), and has less of an incentive to resort to crime to fund their drug addiction.[42]

The Centers for Disease Control and Prevention (CDC), Government Accountability Office (GAO), and the President's Opioid Commission (U.S. GAO 2009; Christie et al. 2017) have all stressed that in order for PDMPs to be effective, providers must access the data. They underscore the importance of moving towards universal registration and utilization, and recommend that all states institute mandatory access provisions. PDMP mandates are proliferating, though these mandates continue to face some opposition by physician and dentist groups on the grounds that they are intrusive, burdensome and difficult to implement in practice,

[42] While individuals who are already addicted to Rx opioids may enter the underground market to substitute towards illicit drugs, there will be newer cohorts that will not become addicted to Rx opioids as a result of the restrictions and hence that substitution would be less likely to occur over time. This propagation of the effects across subsequent cohorts may also explain the lag in the policy response that we found and why the effects become larger over time.

take up time that could be otherwise spent treating patients, and can result in substantial punitive consequences for prescribers (Haffajee et al. 2015). At the end of our sample period, in 2015, 11 states, representing 20% of the population had required that providers must use the PDMP prior to prescribing and dispensing a controlled drug; the rest continued to leave PDMP registration and use to the discretion of the providers or mandated use in limited circumstances. Our estimates for violent Part I offenses suggest that expanding strict PDMP mandates from the 20% coverage rate to universal coverage across the U.S. could reduce violent crime by about 4.9% or by about 58,766 offenses.[43] This would result in economic cost savings of up to $12.9 billion annually.[44] Monetizing the suggestive decrease in property crimes that we find would add to the cost savings. Overall, these findings specifically provide additional evidence that prescription drug monitoring programs are an effective social policy tool to mitigate the negative consequences of opioid misuse, and more broadly indicate that opioid policies can have important spillover effects into other non-health related domains such as crime that should be considered in any cost-benefit calculus.

[43] Table 2 reported that mandatory PDMP provisions reduced violent offenses by between 5.5 to 6.9%, or on average 6.1%. Expanding coverage by 80% (from 20% to universal coverage) would therefore result in approximately (0.8*6.1) 4.9% reduction in violent crime. In 2015, the FBI reported 1,199,310 violent Part I crimes (see https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/tables/table-1). Thus, a 4.9% reduction translates into 58,766 fewer offenses.

[44] McCollister et al. (2010) present crime-specific estimates, combining the tangible and intangible costs, for Part I and some Part 2 crimes. Aggregating their violent crime estimates, based on the specific shares of each offense in total violent crimes for 2008, and converting to 2015 dollars yields the total cost of a violent offense as $220,094.

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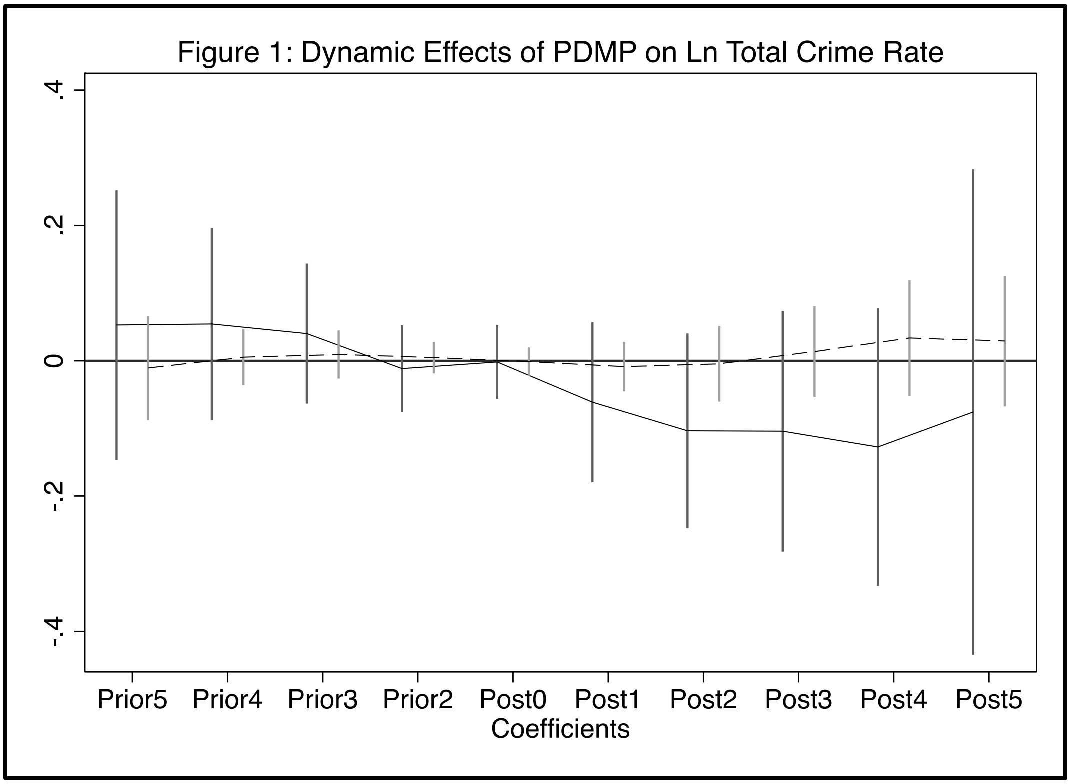
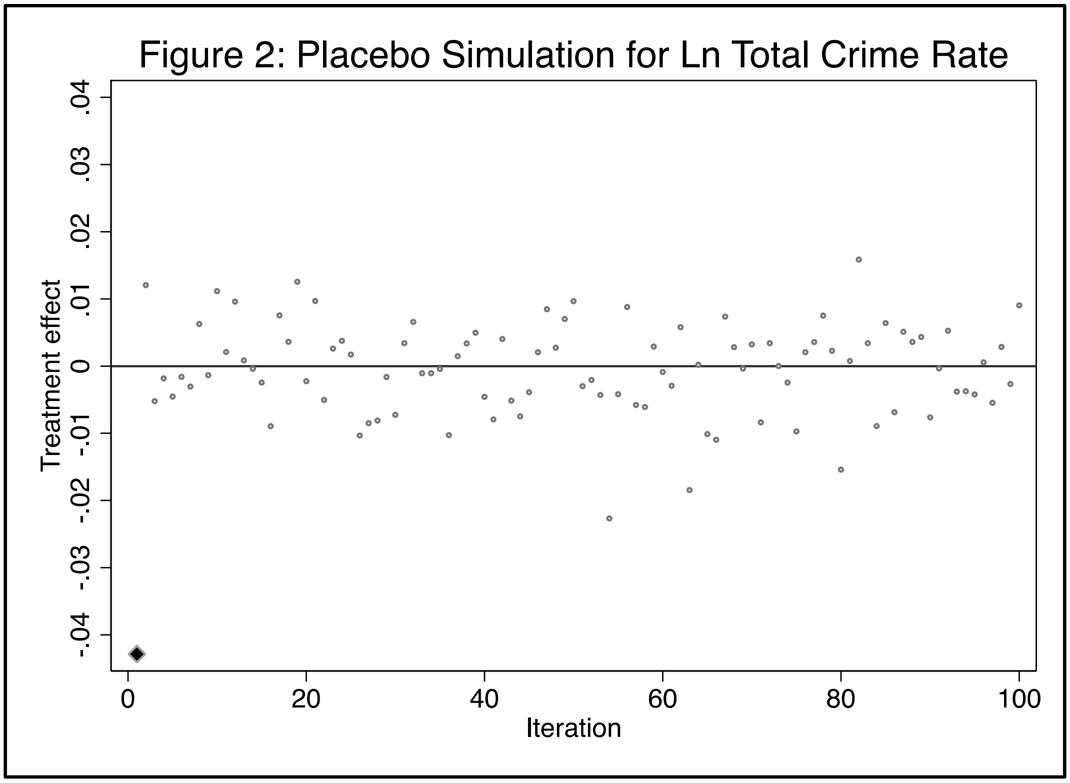


Note: Coefficients from an event study
Source: Authors' calculations
Solid line represents mandatory-access PDMP states while dashed line represents voluntary PDMP states.



Note: Coefficients from an event study
Source: Authors' calculations

Table 1: Summary Statistics

Panel A: Offenses per 100k Residents, Agency-Level			
	All Agencies	**Agencies 10k**	**Agencies 12m**
Total	3531.192	4214.529	4571.278
Violent	1473.956	1809.672	1897.924
Property	2072.834	2405.065	2677.506
Homicide	28.487	4.754	20.262
Rape	42.521	25.261	38.563
Robbery	62.040	62.352	64.090
Assault	791.516	950.456	1011.359
Simple Assault	638.994	769.928	813.807
Burglary	439.972	538.879	553.980
Larceny	1518.173	1690.739	1957.845
MV Theft	150.163	175.912	178.984
Panel B: State-Level Demographics			
% Ages 0-17	0.238	0.240	0.239
% Ages 18-25	0.112	0.113	0.113
% Male	0.491	0.491	0.491
% Male Ages 0-17	0.122	0.123	0.122
% Male Ages 18-25	0.057	0.058	0.058
Unemployment Rate	6.499	6.611	6.492
PDMP	0.437	0.462	0.440
PDMP Mandatory Access (MA)	0.060	0.061	0.057
ID Laws	0.460	0.465	0.446
Physical Examination (PER) Law	0.743	0.792	0.757
Marijuana Legalization	0.009	0.009	0.009
Marijuana Decriminalization	0.262	0.311	0.253
Medical Marijuana Law	0.183	0.227	0.198
Naloxone Access Law	0.217	0.246	0.228
Good Samaritan Law	0.130	0.147	0.136
Observations	208069	71299	157551

Note: Panel A represents offenses known at the agency level per 100,000 agency residents. The first column has information for all agencies, the second column is restricted to agencies with at least 10,000 residents, and the third column is restricted to agencies that report offenses all 12 months.

Table 2: Effect of PDMP on Crime Rates (per 100k)
UCR Offenses Known

	Total	Violent	Property
Panel A: Large Agencies (10k) that Reported 12 Months			
PDMP	-63.762	-3.820	-59.942
	(47.851)	(19.253)	(38.719)
PDMP MA	-150.330*	-107.913**	-42.417
	(86.802)	(52.882)	(74.049)
Observations	65,160	65,160	65,160
Pre-Mean	4614	1973	2642
% Change	-3.258%	-5.469%	-1.605%
Panel B: Large Agencies (10k)			
PDMP	-7.084	6.252	-13.336
	(69.592)	(23.773)	(57.101)
PDMP MA	-148.066	-125.259**	-22.807
	(101.161)	(56.092)	(82.661)
Observations	71,299	71,299	71,299
Pre-Mean	4251	1817	2434
% Change	-3.483%	-6.894%	-0.937%
Panel C: Agencies that Report 12 Months			
PDMP	-54.006	-1.061	-52.945
	(46.739)	(17.771)	(37.410)
PDMP MA	-165.869*	-112.764**	-53.105
	(86.383)	(50.323)	(73.371)
Observations	157,551	157,551	157,551
Pre-Mean	4620	1903	2717
% Change	-3.590%	-5.926%	-1.955%
Year fixed effects	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes
State trends	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. All models control All specifications control for the share of population composed by minors, individuals ages 18-25, men, men under 18 years of age, and men ages 18-25 years); ID laws, PER laws, Naloxone access laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, and medical marijuana laws, pill mill laws, BAC laws, beer taxes, and cigarette taxes; number of police officers and employees in the police force; and income per capita, unemployment rate, poverty rate, marriage rates and share of residents that have a college degree, some college, high school, and less than high school education.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3: The Effect of PDMP on Crime Rates (per 100k), Large Agencies that Reported 12 Months
UCR Offenses Known

	Homicide	**Rape**	**Robbery**	**Assault**	**Simple Assault**	**Burglary**	**Larceny**	**MV Theft**
PDMP	-0.042	0.243	0.915	-4.490	-0.446	-14.843	-40.860	-4.239
	(0.093)	(0.608)	(1.918)	(9.550)	(9.263)	(11.103)	(25.354)	(7.773)
PDMP MA	-0.556***	-1.989	2.382	-52.306*	-55.444**	-0.248	-21.443	-20.726
	(0.182)	(1.264)	(3.476)	(29.439)	(24.645)	(26.582)	(45.293)	(17.925)
Observations	65,160	65,160	65,160	65,160	65,160	65,160	65,160	65,160
Pre-Mean	3.482	26.81	68.17	1036	837.9	590	1855	196.6
% Change	-15.968%	-7.419%	3.494%	-5.049%	-6.617%	-0.042%	-1.156%	-10.542%
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. All models control All specifications control for the share of population composed by minors, individuals ages 18-25, men, men under 18 years of age, and men ages 18-25 years); ID laws, PER laws, Naloxone access laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, and medical marijuana laws, pill mill laws, BAC laws, beer taxes, and cigarette taxes; number of police officers and employees in the police force; and income per capita, unemployment rate, poverty rate, marriage rates and share of residents that have a college degree, some college, high school, and less than high school education.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Effect of PDMP on Arrest rates per 100k, Large Agencies
UCR Arrests

	Age 18-24	Age 25-44	Age 45 Plus	Age 18-44
Panel A: Part I Crimes				
PDMP	-1.142	-3.892	-2.557	-5.033
	(3.994)	(6.028)	(2.014)	(9.596)
PDMP MA	-11.135*	-19.911*	-2.464	-31.046**
	(5.895)	(9.932)	(3.107)	(14.926)
Observations	59,865	59,865	59,865	59,865
Pre-Mean	281.7	416.2	132.6	697.9
% Change	-3.953%	-4.784%	-1.858%	-4.448%
Panel B: Sale/Manufacture/Possession of Opium/Cocaine/Derivatives (Morphine, Cocaine, Heroin, Codeine)				
	Age 18-24	**Age 25-44**	**Age 45 Plus**	**Age 18-44**
PDMP	0.673	-0.245	-0.194	0.428
	(1.054)	(1.530)	(0.862)	(2.520)
PDMP MA	0.146	0.986	2.165*	1.133
	(2.472)	(4.490)	(1.254)	(6.800)
Observations	50,904	50,904	50,904	50,904
Pre-Mean	32.41	59.73	16.76	92.14
% Change	0.450%	1.651%	12.918%	1.230%
Panel C: Sale / Manufacture / Possession of Marijuana				
	Age 18-24	**Age 25-44**	**Age 45 Plus**	**Age 18-44**
PDMP	1.947	-0.514	0.197	1.432
	(2.713)	(1.935)	(0.442)	(4.527)
PDMP MA	3.779	2.351	0.958	6.130
	(4.338)	(4.488)	(0.896)	(8.583)
Observations	57,822	57,822	57,822	57,822
Pre-Mean	104.9	71.25	13.89	176.1
% Change	3.602%	3.300%	6.897%	3.481%
Panel D: Sale / Manufacture / Possession of Synthetic Narcotics (Demerol, Methadone)				
	Age 18-24	**Age 25-44**	**Age 45 Plus**	**Age 18-44**
PDMP	0.627	1.605	0.133	2.232
	(0.382)	(1.099)	(0.238)	(1.420)
PDMP MA	-2.629	-1.921	-0.266	-4.550
	(1.751)	(1.710)	(0.489)	(3.277)
Observations	37,091	37,091	37,091	37,091
Pre-Mean	12.74	24.24	6.199	36.98
% Change	-20.636%	-7.925%	-4.291%	-12.304%
Panel E: Sale / Manufacture / Possession of Other Dangerous Non-Narcotics (Barbiturates, Benzedrine)				
	Age 18-24	**Age 25-44**	**Age 45 Plus**	**Age 18-44**
PDMP	-0.125	-3.174	-1.409*	-3.299
	(1.166)	(2.475)	(0.813)	(3.570)
PDMP MA	-7.106**	-10.284**	-2.844**	-17.390**
	(2.728)	(5.102)	(1.258)	(7.701)
Observations	50,677	50,677	50,677	50,677
Pre-Mean	27.99	54.05	14.98	82.04
% Change	-25.388%	-19.027%	-18.985%	-21.197%
Year fixed effects	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. See notes for Table 3.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of PDMP on Homicide Circumstances, Large Agencies (per 100k)
UCR Supplementary Homicide Reports

Panel A: Offender's Age Demographics	**Age 18-44**	**Age 18-24**	**Age 25-44**	**Age 45 Plus**
PDMP	-0.011	-0.028	-0.013	0.004
	(0.072)	(0.049)	(0.047)	(0.023)
PDMP MA	-0.310*	-0.266***	-0.076	0.002
	(0.166)	(0.097)	(0.123)	(0.050)
Observations	26,998	26,998	26,998	26,998
Pre-Mean	4.005	1.697	2.585	1.097
% Change	-7.740%	-15.675%	-2.940%	0.182%
Panel B: Victim's Age Demographics	**Age 18-44**	**Age 18-24**	**Age 25-44**	**Age 45 Plus**
PDMP	0.103	0.017	0.104**	0.027
	(0.077)	(0.051)	(0.049)	(0.037)
PDMP MA	-0.481***	-0.185*	-0.323***	-0.043
	(0.156)	(0.109)	(0.079)	(0.105)
Observations	26,998	26,998	26,998	33,473
Pre-Mean	4.385	1.526	2.916	3.443
% Change	-10.969%	-12.123%	-11.077%	-1.249%
Panel C: Male Offender's Age Demographics	**Age 18-44**	**Age 18-24**	**Age 25-44**	**Age 45 Plus**
PDMP	0.010	-0.015	0.003	0.004
	(0.073)	(0.051)	(0.047)	(0.022)
PDMP MA	-0.270	-0.270***	-0.038	-0.014
	(0.175)	(0.100)	(0.134)	(0.043)
Observations	26,998	26,998	26,998	26,998
Pre-Mean	3.644	1.559	2.296	0.954
% Change	-7.409%	-17.319%	-1.655%	-1.468%
Panel D: Male Victim's Age Demographics	**Age 18-44**	**Age 18-24**	**Age 25-44**	**Age 45 Plus**
PDMP	0.149*	0.036	0.124***	0.021
	(0.075)	(0.051)	(0.046)	(0.029)
PDMP MA	-0.492***	-0.215*	-0.307***	0.016
	(0.158)	(0.117)	(0.069)	(0.083)
Observations	26,998	26,998	26,998	26,998
Pre-Mean	3.446	1.276	2.201	1.244
% Change	-14.277%	-16.850%	-13.948%	1.286%
Year fixed effects	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes

Table 5 (continued): Effect of PDMP on Homicide Circumstances, Large Agencies (per 100k)

Panel E: Female Offender's Age Demographics	Age 18-44	Age 18-24	Age 25-44	Age 45 Plus
PDMP	-0.040*	-0.025*	-0.020	-0.010
	(0.023)	(0.012)	(0.018)	(0.008)
PDMP MA	-0.084*	-0.024	-0.055	-0.009
	(0.048)	(0.023)	(0.034)	(0.019)
Observations	26,998	26,998	26,998	26,998
Pre-Mean	0.567	0.208	0.373	0.149
% Change	-14.815%	-11.538%	-14.745%	-6.040%
Panel F: Female Victim's Age Demographics	**Age 18-44**	**Age 18-24**	**Age 25-44**	**Age 45 Plus**
PDMP	-0.035	-0.015	-0.018	0.009
	(0.029)	(0.016)	(0.024)	(0.021)
PDMP MA	0.014	0.032	-0.019	-0.062
	(0.039)	(0.025)	(0.034)	(0.042)
Observations	26,998	26,998	26,998	26,998
Pre-Mean	1.006	0.262	0.750	0.672
% Change	1.392%	12.214%	-2.533%	-9.226%
Panel G: Weapons	**Firearm**	**Knife/Beat**		
PDMP	0.145	-0.001		
	(0.106)	(0.036)		
PDMP MA	-0.376**	-0.077		
	(0.150)	(0.096)		
Observations	26,998	26,998		
Pre-Mean	4.305	1.360		
% Change	-8.734%	-5.662%		
Year fixed effects	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. See notes for Table 3.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effect of PDMP on Homicides
UCR Supplementary Homicide Reports

	All Agencies	**Large Agencies**
PDMP	0.111	0.113
	(0.113)	(0.109)
PDMP MA	-0.522***	-0.537***
	(0.184)	(0.186)
Observations	33,473	26,998
Pre-Mean	11.87	6.888
% Change	-4.398%	-7.796%
Year fixed effects	Yes	Yes
Agency fixed effects	Yes	Yes
State trends	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. See notes for Table 3.
*** p<0.01, ** p<0.05, * p<0.1.

Table A1: Effect of PDMP on Ln Crime Rates (per 100k)
UCR Offenses Known

	Ln Crime Rates			Ln Crime Rates Modified for Zero		
	Total	**Violent**	**Property**	**Total**	**Violent**	**Property**
Panel A: Large Agencies (10k) that Reported 12 Months						
PDMP	-0.010	-0.002	-0.017	-0.011	-0.005	-0.018
	(0.009)	(0.009)	(0.012)	(0.009)	(0.009)	(0.012)
PDMP MA	-0.044**	-0.074***	-0.030	-0.043**	-0.077***	-0.027
	(0.019)	(0.021)	(0.028)	(0.018)	(0.021)	(0.028)
Observations	65,099	64,964	65,077	65,160	65,160	65,160
Panel D: Agencies that Report 12 Months						
PDMP	-0.011	-0.002	-0.016	-0.010	-0.005	-0.017
	(0.009)	(0.008)	(0.012)	(0.009)	(0.008)	(0.012)
PDMP MA	-0.048**	-0.078***	-0.034	-0.047**	-0.081***	-0.031
	(0.019)	(0.020)	(0.028)	(0.019)	(0.020)	(0.028)
Observations	155,127	150,944	154,370	157,551	157,551	157,551
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. See notes for Table 3.
*** p<0.01, ** p<0.05, * p<0.1.

Table A2: The Effect of PDMP on Crime, Large Agencies that Reported 12 Months
UCR Offenses Known

	Homicide	Rape	Robbery	Assault	Simple Assault	Burglary	Larceny	MV Theft
Panel B: Ln Crime Rate								
PDMP	-0.019	0.025	-0.014	0.001	-0.010	-0.007	-0.016	-0.003
	(0.018)	(0.015)	(0.013)	(0.009)	(0.010)	(0.020)	(0.011)	(0.017)
PDMP MA	-0.102***	-0.032	0.016	-0.064***	-0.118**	-0.025	-0.021	-0.029
	(0.033)	(0.047)	(0.032)	(0.020)	(0.048)	(0.037)	(0.025)	(0.040)
Observations	30,048	56,113	56,818	64,901	62,776	64,937	65,021	64,123
Panel C: Ln corrected 0.5								
PDMP	-0.013	0.014	-0.014	-0.004	-0.005	-0.007	-0.025	-0.007
	(0.015)	(0.042)	(0.013)	(0.010)	(0.016)	(0.020)	(0.016)	(0.018)
PDMP MA	-0.089***	-0.071	0.005	-0.072***	-0.147***	-0.029	-0.052	-0.037
	(0.025)	(0.060)	(0.030)	(0.022)	(0.037)	(0.035)	(0.038)	(0.046)
Observations	65,160	65,160	65,160	65,160	65,160	65,160	65,160	65,160
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. See notes for Table 3.
*** p<0.01, ** p<0.05, * p<0.1.

Table A3: Poisson Estimates for Homicide
UCR Offenses Known

	Large Agencies		Large Agencies, Reported 12 Months	
PDMP	0.014	0.014	-0.010	-0.010
	(0.009)	(0.019)	(0.009)	(0.019)
PDMP MA	-0.076***	-0.076**	-0.057***	-0.057**
	(0.015)	(0.035)	(0.015)	(0.028)
Observations	71299	71299	65160	65160
Exposure	Yes	Yes	Yes	Yes
State clustered SE	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes

Note: Coefficient estimates from a Poisson regression are presented, with the agency population as the "exposure" variable. The first two columns include agencies that have at least 10,000 residents. The last two columns include agencies that have at least 10,000 residents and that report al 12 months. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level, where noted. See notes for Table 3.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: Short- and Long-term Effects of PDMP on Property Crime
UCR Offenses Known

	Large Agencies	Large Agencies Reporting 12 Months	All Agencies Reporting 12months
Panel A: Violent Crimes			
PDMP MA (0-1 Year)	-90.809	-71.052	-76.917
	(58.960)	(56.767)	(54.295)
PDMP MA (2-3 Years)	-274.740***	-224.611**	-213.123**
	(97.013)	(94.754)	(89.348)
PDMP MA (4-7 Years)	-356.614***	-288.419**	-268.231**
	(124.551)	(122.973)	(114.524)
Observations	60,625	55,345	129,203
Pre-Mean	1809	1967	1932
% Change (0-1 Year)	-5.020%	-3.612%	-3.981%
% Change (2-3 Years)	-15.187%	-11.419%	-11.031%
% Change (4-7 Years)	-19.713%	-14.663%	-13.884%
Panel B: Property Crimes			
PDMP MA (0-1 Year)	22.703	63.735	41.548
	(86.405)	(75.319)	(73.469)
PDMP MA (2-3 Years)	-130.948	-60.412	-72.352
	(134.340)	(115.631)	(112.291)
PDMP MA (4-7 Years)	-287.388	-207.128	-209.460
	(182.085)	(164.673)	(156.862)
Observations	60,625	55,345	129,203
Pre-Mean	2423	2635	2786
% Change (0-1 Year)	0.937%	2.419%	1.491%
% Change (2-3 Years)	-5.404%	-2.293%	-2.597%
% Change (4-7 Years)	-11.861%	-7.861%	-7.518%
Year fixed effects	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes
State trends	Yes	Yes	Yes

Note: OLS coefficients are presented. All models are weighted by agency population. Standard errors are adjusted for arbitrary correlation within states and clustered at the state level. See notes for Table 3.
*** p<0.01, ** p<0.05, * p<0.1.