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PRESCRIPTION DRUG MONITORING PROGRAMS, OPIOID ABUSE, AND CRIME

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ABSTRACT

We study the spillover effects of prescription drug monitoring programs (PDMPs) on crime, and in the process inform how policies that restrict access to Rx opioids per se within the healthcare system would impact broader non-health domains. In response to the substantial increase in opioid use and misuse in the United States, PDMPs have been implemented in virtually all states to collect, monitor, and analyze prescription opioid data with the goal of preventing misuse and the diversion of controlled substances. 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 PDMPs reduced overall crime by 5%. These reductions in crime are associated with both violent and property crimes. This decrease in crime is also reflected by a decrease in crime-related arrests as well as drug-related arrests. Overall, these results provide additional evidence that PDMPs are an effective social policy tool to mitigate some of 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 undertreated led to more aggressive pain management standards, and 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). Though opioid prescribing has since fallen, the volume of prescriptions remains more than two times higher than in 1992. 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 addiction and the diversion of these drugs for non-medical purposes.

Overdose deaths from opioid analgesics have increased seven-fold since 1999,[1] with economic costs of the opioid epidemic exceeding $500 billion annually (Council of Economic Advisors 2017).[2] Though the crisis has shifted in recent years with an upsurge in overdose deaths related to non-prescription opioids such as heroin or illicit fentanyl, prescription opioids continue to play a role as four out of five new heroin users started out by misusing prescription opioids (Jones et al. 2013).

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

[2] 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.

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 relatives, physicians remain the leading source for those who are at highest risk of overdose (Jones, Paulozzi, and Mack 2014). Notably, individuals may obtain excessive Rx opioids through their own prescriptions, 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).[3] PDMPs can help identify patients who may be doctor shopping, misusing Rx drugs, or are at risk of overdose. Also, PDMPs can help identify patients that would benefit from timely treatment interventions.

Currently all states and D.C. have an operational PDMP, though utilization of these programs by providers largely remains voluntary and the systems vary based on their comprehensiveness and degree of integration. In many 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 PDMPs are found to have limited to no effect on opioid misuse. A growing number of states have enhanced and modernized their programs, instituting universal registration and mandatory-access provisions and requiring providers to register on and query the

[3] 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).

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.[4] There is an emerging consensus that these stricter programs have also 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 misuse and opioid-related mortality among adults in the general population (Ali et al. 2017; Grecu, Dave, and Saffer 2019).[5] 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).[6]

What remains unclear are the potential spillovers from these interventions, and any resulting success in reducing Rx drug misuse, on other outcomes. Opioid misuse 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 use (Grecu, Dave and Saffer 2019). Given the links between drug misuse, 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 economic effects.[7]

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[4] 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/.

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

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

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

We provide some of the first evidence on the impact of PDMPs on an important societal outcome, crime. Our study also speaks to the larger and complex question of how policies that restrict access to Rx opioids per se within the healthcare system can have a broader impact on societal outcomes such as crime. While restricting Rx opioids can reduce Rx opioid misuse, leading to a potential decrease in crime, if individuals substitute to other illicit drugs or more dangerous supply channels then such policies could actually 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 misuse and crime (Carpenter 2007; Pedersen and Skardhamar 2010), such policies 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 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 et al. 2019).[8] There have also been some drawbacks associated with PDMPs, which include additional costs to the healthcare system and compliance difficulties (Islam and McRae 2014, Stucke et al. 2018). Hence, the overall value of these programs is still actively debated, despite recommendations from policymakers and public health organizations urging states to

[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).

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 misuse (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. We provide one of the first studies to specifically inform the causal link between Rx opioid misuse 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 on the order of about 5% for overall crime, driven by decreases in both violent and property crimes.

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 misuse, 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 misuse

A large literature has studied the effects of PDMPs, 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 between voluntary and 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). 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]

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

[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. 2019). 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.

than a seven-month 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 (2019) assess the effects of PDMPs on substance use disorder 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 misuse, with the largest effects concentrated on Rx opioid misuse and among young adults ages 18-24 (32% decline in treatment admissions and 26% decline in opioid-related mortality).[11] Kaestner and Ziedan (2019) provide evidence of a significant first-stage with respect to prescribing patterns, and show that the adoption of a modern PDMP system accessible to all users is associated with a 4-8% decrease in retail opioid prescriptions.

2.2 Substance misuse, PDMPs and Crime

Most of the studies that have evaluated the impact of PDMPs have assessed measures of Rx drug misuse or associated health indicators, and at best assessed spillovers into the use of other drugs. Given the robust and consistent findings from this literature that certain forms of PDMPs have been highly effective, it is plausible that the reduction in Rx opioid misuse may also impact criminal behaviors. Broadly, substance use can affect crime through three pathways,

[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 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).

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 misused, 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-related 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 misuse can include excessive mood swings and hostility.[14]

If PDMPs are effective in reducing opioid misuse, and effective in reducing the use of other complementary substances such as cocaine and alcohol (which have also been linked to aggression and violence; Davis 1996; Corman and Mocan 2000), then we may see a reduction in violent crime. Decreased use and misuse 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;

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[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.
[14] See https://www.mayoclinic.org/diseases-conditions/prescription-drug-abuse/symptoms-causes/syc-20376813.

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 misuse, 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 misuse, 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 and generate important dynamics in the market response. For instance, in 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 misuse 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

[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.

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 2018; Evans & Lieber 2019). 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.

Very little work has evaluated broader spillovers of opioid-related interventions. In the only other study on PDMPs and spillovers into crime-related outcomes that we are aware of, Mallatt (2019) finds a strong increase (about 112% on average) in crime incidents related to heroin possession, with stronger effects in counties which had higher rates of oxycodone 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

[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).

[17] Mallatt (2019) focuses solely on heroin and opioid crime, specifically related to possession, in order to gauge spillovers from restricted access to Rx opioids on substitution into illicit opioids. Kaestner and Ziedan (2019) consider broader socioeconomic outcomes including employment, earnings, public assistance, and marital status, and find little evidence that state interventions targeting Rx opioids are significantly associated with these outcomes.

interventions targeted at Rx opioid misuse and spillovers on other illegal drugs are limited and have not reached a consensus and find very weak adverse or even beneficial effects on other illicit drugs (Meinhofer 2018; Grecu et al. 2019).[18]

The upshot of this discussion is that, while spillover effects on crime are plausible, the net effects of disruption to Rx opioid access on criminal behaviors are a priori indeterminate. The overall effects depend on the extent of potential substitution into other illicit drugs vs. the overall reduction in the pool of addicts. The various 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). 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 in the healthcare system 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 misuse 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

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[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. 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.

We use measures of crime using data spanning 2003-2017 from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR) monthly files[19], 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, 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).

For our primary analysis, we use the Offenses Known and Clearances by Arrests segments of the UCR. 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 (or the victim). 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.[21] Second, arrest data include information on the demographics of the offender, which allows us to determine whether the

[19] Kaplan (2019) compiled the offenses, arrests and homicide UCR datasets in ICPSR. While the data on offenses known is available for 2017, the arrest data is only available until 2016.
[20] This is important and provides a validation check, given the inherent difficulties in measuring crime, a limitation not unique to our study.
[21] 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.

propensity to commit crime changed in response to PDMP implementation differentially by age. Since young adults are more likely to engage in criminal activity in general and also the most likely to adjust their opioid use patterns in response to the implementation of PDMPs (Grecu et al. 2019), 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. Because of the heterogeneity in the reliability of reporting across agencies and the fact that a single non-reporting agency may account for a substantial fraction of crime for a given geographical area, we follow the crime literature and focus on agencies that reported crimes consistently in all 12 months of the year, every year (Maltz and Targonski, 2002).

3.2 Prescription Drug Monitoring Programs

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.

To model the impact of PDMP legislation on crime we follow the literature and use dates on which a state's PDMP became operational derived from the Prescription Drug Abuse Policy System (PDAPS)[22] and dates of implementation of mandatory-access provisions. Mandatory-access provisions are stronger statutes that required all licensed prescribers and dispensers to

[22] http://pdaps.org/datasets/prescription-monitoring-program-laws-1408223416-1502818373

register on the PDMP and to query the PDMP prior to prescribing and dispensing controlled substances. We note that there is some heterogeneity across states in terms of mandatory access. For instance, Kentucky 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 controlled substances. In contrast, some states mandate access in limited circumstances or do not mandate access for all providers. For instance, Georgia only requires that physicians practicing at a pain clinic regularly check the PDMP on all new and existing patients and Florida only requires providers to check the database prior to prescribing but does not require dispensers to check the database prior to dispensing.[23]

Thus, there is some heterogeneity in the PDMP definitions derived from the PDAPS. In supplementary analyses, we also model the impact of PDMPs, based on an alternate dimension and dates that have been highlighted in a recent study (Horwitz et al. 2018). Horwitz et al. (2018) contend that a salient consideration when modeling the impact of PDMPs is to assess effectiveness relative to when a state's full modern, electronic PDMP system became operational and became directly accessible to all users (providers, law enforcement). They carefully assemble a legal database and report the dates based on these criteria, and further show these PDMPs to be negatively associated with measures of "doctor shopping".

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

[23] According to PDAPS, eighteen states have mandated access defined as "Does the state require prescribers to check the PDMP before prescribing controlled substances?".

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 user to administer the opioid antagonist in case of an overdose (Rees et al. 2019). 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. 2019). 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.[24] Finally, we control for policies pertaining to marijuana legalization, marijuana decriminalization, medical marijuana, beer taxes, and whether the state has a 0.08 blood alcohol content (BAC) per se limit law.

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 2017. Specifically, we control for the natural logarithm of the number of officers in the police force per 100,000 residents. We also control for 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).[25] In particular, we construct

[24] https://www.networkforphl.org/_asset/qz5pvn/network-naloxone-10-4.pdf
[25] https://wonder.cdc.gov/controller/datarequest/D9

the share of the population composed by minors, individuals ages 18-25, and males ages 18-25 years, as well as the overall share of males. Additionally, we control for income per capita and seasonally adjusted unemployment rates, which were obtained from the U.S. Bureau of Labor Statistics and the County Business Patterns (CBP), and account for shifts in the state's economy.[26] Finally, we control for the poverty rate, and the share of residents with a college degree, some college, high school, less than high school.[27]

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 misuse, 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:

$$Y_{jst} = \alpha_0 + \beta_0 PDMP_{st} + \beta_1 PDMP_{st} * MA_{st} + \delta X_{st} + \gamma_t + \gamma_j + \varepsilon_{jst} \qquad (1)$$

Equation (1) can be interpreted as a reduced-form crime supply function. The analysis is at the agency-year level j, and the outcome (denoted by Y_{jst}) represents the natural logarithm of the rate of offenses known to police per 100,000 residents in a given agency *j*, in state *s* and year t.[28] Models are estimated for all Part 1 crimes, and separately for violent and property crimes. The variable $PDMP_{st}$ 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

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

[27] 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.

[28] We add one to the counts before computing the rate in order to avoid dropping the agency-year observations with zero counts.

monitoring program and implemented stricter 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.

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 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, BAC laws, beer taxes), police composition (number of officers in the police force) and other socioeconomic variables (income per capita, unemployment rate, poverty rate 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 also present estimates from models that include treatment-specific linear trends ($MA_s * t$), to account for the possibility that states which ever-adopted enhanced provisions to their PDMPs may be systematically different than the non-adopting states, and models that include state-specific linear trends ($\gamma_s * t$), to account for unmeasured systematic time-varying confounding factors across all states (e.g. policing behavior, funds allocated to policing, funds allocated to education, among others). These controls account to some degree for systematic differential trends across implementation vs. non-implementation states prior to the policy. Given that the analysis is performed at the agency-year level, including state-specific trends is salient as most potential confounders such as allocation of public funds or implementation of police training tactics would be implemented at the state and not at the agency level.[29] 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. First, in addition to evaluating the effects on aggregated counts of all Part 1 crimes (per 100,000 residents), which implicitly assigns equal severity to each offense, we also evaluate effects on cost-weighted crime following Chalfin and McCrary (2018). The latter provides an estimate of the policy on the expected cost of crime based on a weighted aggregate of crime counts, with weights equal to the cost of each type of crime.[30] This approach explicitly places a larger weight on more costly crimes, and typically violent crimes are more costly than property crimes, given the high victim and societal costs of the former. Second, since aggregated crime may mask nuanced changes in relatively infrequent crimes, we also explicitly assess effects of PDMP policy separately on each offense type. Specifically, the following crimes are evaluated: homicide (which combines murder and manslaughter), rape, robbery, assault, burglary, larceny, and motor vehicle theft.

Third, 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 and victim's age is not contained in the UCR Offenses Known Segment, we can observe the offender's and

[29] Previous crime literature exploits variation at the county or city level to evaluate the effect on crime or substance use at the agency level include a state by year fixed effect as the main specification in order to control for potential confounders such as the allocation of public funds on policing, education, changes in policing tactics and training, or socioeconomic conditions that usually vary at the state level (Bondurant, Lindo and Swensen, 2019; Swensen, 2015). Because PDMPs vary at the state-year level, we cannot include a state by year fixed effects, but a state-specific linear trend would take into account these confounders that vary linearly. Also, note that the treatment-specific linear trends are nested within the state-specific linear trends.

[30] Estimating the effect on cost-adjusted violent and property crimes, or the expected cost of crime, as presented in Chalfin and McCrary (2018), takes into account that a policy that prevents a small amount of more socially costly crimes such as homicide could be more cost-effective than a policy that prevents a large amount of less costly crimes such as burglary.

victim's age and gender with respect to homicide incidents using the UCR Supplement of Homicide Report (SHR). In particular, we examine the following dependent variables: the rate of homicides where the offender (victim) was between the ages of 18-39 and 40 and over.[31]

Fourth, we further exploit the SHR to evaluate whether the rate of homicides that involved a firearm or a knife changed in response to PDMP implementation. Illicit drug markets are more likely to involve interactions and networks prone to violence. In particular, studies of 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). If PDMPs impact interactions with illicit drug markets, violent crime, and homicides in particular, it is possible that the strongest impact among offenders may be among young adults - the group whose opioid misuse and adverse health events are most impacted by mandatory PDMPs (Grecu et al. 2019).

Fifth, we also use information on the offender's age from 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 for adults 18-30 and over 40. 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.

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;

[31] The age of the victim is missing only in 1.3 % of the incidents while the age of the offender is missing for 35% of the incidents and therefore the results pertaining the effects on demographics of the offender must be interpreted with caution.

Colman and Dave 2018). We conduct a fully-specified conditional event study based on the following specification.

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

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 and we estimate this event study using both the voluntary PDMP implementation dates as well as the mandatory access dates from PDAPS. 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. We normalize β^{-1} to zero and therefore all parameters β^k for k between -4 and 4 should be interpreted as the policy effect on crime relative to the year prior to implementation. We also impose endpoint restrictions for periods at least five years away from the year of implementation, which prevents us from assigning unequal weight to states that enacted PDMP particularly early or particularly late given the unbalanced sample.

The event study framework serves 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, the event study 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 et al. (2019) show that, while mandatory PDMPs 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, partly because there may be lags in the disruption to supply due to stockpiling, or because it may take time for the total pool of addicts to decrease. Furthermore, even if access to Rx opioids is disrupted, alternate sources may substitute over time or there may be substitution into heroin and other illicit drugs (synthetic opioids) in the shorter or longer term. 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 300 times, each time using a different set of placebo indicators. Once the estimation is complete, all 300 placebo coefficients are plotted and compared with the results of our primary DD analysis.

5. Results

5.1. Summary Statistics

Due to the fact that some agencies do not report offenses every year, decreases in crime could be driven by actual decreases in crime or could be driven by agencies not reporting crimes on that year. In order to avoid that problem, our main analysis is restricted to the 9,136 agencies that report crimes in all years between 2003 and 2017 and that report offenses all 12 months each year between 2003 and 2017.[32]

Panel A of Table 1 presents the summary statistics of offenses known to police for all agencies in the sample (columns 1-2), and agencies that reported all 12 months and reported data each year between columns 2003-2017 (columns 3-4). In terms of crime rates for the entire

[32] We also estimate the model restricting to agency-year cells that report offenses all 12 months regardless of whether they do so for all years between 2003 and 2017. We further analyze offenses known to police restricting the agencies to those that report crimes all years between 2003 and 2016 and that belong to cities with at least 10,000 residents and the results are similar. The last restriction excludes cities with population smaller than 10,000, MSAs and non-MSAs. These results will be provided upon request.

sample, overall there were approximately 2,240 crimes per 100,000 residents with 212 (9%) of these constituting violent crimes and the other 2,027 (90%) being property crimes. The violent crime with the highest crime rate is aggravated assault with 152 incidents per 100,000 residents while the least frequent violent crime is homicide with 2.4 incidents per 100,000 residents. Among property crimes, the most prevalent property crime is larceny with 1,487 incidents per 100,000 residents while the least prevalent property crime is motor vehicle theft with 129 incidents per 100,000 residents. While the means for the subsample we use in the main analysis are expectedly higher due to more complete information on all reporting agencies, the relative shares of violent and property crime in total crime, and the shares of the specific offenses in violent and property crime, remain largely unaffected.

Panel B presents summary statistics for the Supplementary Homicide Reports (SHR), where the first two columns correspond to the entire sample and the last two columns correspond to the subsample used in this study. Because the original SHR dataset reports homicides at the incident level, agencies only appear in the dataset as long as they reported a homicide and therefore the SHR is potentially restricted to agencies where homicide is more prevalent. A missing agency-year can occur either because the data are missing or because there were zero homicides during that period. In order to avoid this issue, we restrict the analysis to agencies that appear in the data every year, and hence report at least one homicide every year throughout the period studied. This is reflected in the fact that the number of homicides that occurred with a firearm are much larger in the subsample used.[33]

[33] We restrict the SHR analysis to agencies that correspond to cities with a population of at least 10,000 that reports data every year instead of restricting the analysis to agencies that report all 12 months because the latter would imply restricting the analysis to agencies that reported at least one homicide each month. The discrepancies between the murder rate and the murder rate conditional on any given age group occurs because not every incident reports demographics of the victim. As we mentioned earlier, demographics of the offender are largely missing.

Finally, Panel C presents summary statistics of the arrest dataset. Patterns of arrest rates (e.g. property crime arrests are more prevalent than violent crime arrests, murder is the least prevalent violent crime, among others) remain unchanged when we restrict the sample to agencies that report all 12 months and report crimes every year in the observed period. In addition, the patterns of arrest rates follow closely the patterns we observed among offenses known to police.

5.2 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 natural log of the number of offenses per 100,000 residents. We also report estimates for the impact of mandatory access PDMPs, relative to no PDMP, using a model with mutually exclusive categories for voluntary and mandated PDMP. We present estimates for total Part I offenses, and then separately for violent and property offenses. Panel A presents the effects for aggregated crime counts, and Panel B presents effects on cost-weight crime counts, explicitly weighting each crime type by its total societal cost following Chalfin and McCrary (2018).

For each crime outcome, we estimate the baseline model, and then progressively add the treatment-specific trends and the state-specific trends in order to control for potential confounders and policies that likely vary at the state level and the less-than-perfect nature of the natural experiment. Our preferred estimates are the ones that include these trends, though it is reassuring that estimates are not largely sensitive to these controls or how we control for these trends (state-specific or treatment-specific). We present estimates from an event study framework later to more explicitly assess the parallel trends assumption and effect dynamics.

The estimates in Table 2 suggest three main findings. First, there is little 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 merely having an operational PDMP without any mandate on providers to query the databases has not been effective in reducing Rx drug or opioid misuse (Haegerich et al. 2014; Buchmueller and Carey 2018; Grecu, Dave and Saffer 2019). Second, we do find evidence that mandatory-access PDMPs significantly decrease crime. Specifically, mandatory PDMPs are found to significantly reduce overall crime by about 5-6%, relative to voluntary PDMPs. Since virtually all states have an operational PDMP currently, these "add-on" effects are policy-relevant given that they inform what may happen if these states enhanced and mandated use of these systems. The total effect of moving from no PDMP of any kind to a fully mandated PDMP is significant and implies about a 7-8% reduction in total crime (based on models that control for trends).

Third, results largely hold when offenses are disaggregated into property and violent crime – voluntary PDMPs do not significantly reduce crime and mandatory access PDMPs do. However, some of these effects are imprecisely estimated. In general, these estimates imply about a 4-5% reduction in property and violent crime as a result of stricter PDMPs. Results in Panel B are largely similar and indicate a comparable reduction in total expected crime costs, driven by both a reduction in violent and property crimes. In Table 3 crimes are disaggregated into specific types of offenses (homicide, rape, robbery, assault, burglary, larceny, and motor vehicle (MV) theft). Similar to Table 2 – voluntary PDMPs are generally found to have no significant effect on any of the offense types. In contrast, mandatory-access PDMPs have a robust and significant negative effect on both assault and burglary. Also, while results are sensitive to model specification and disaggregated crime data are more subject to noise, we also find a negative effect of mandatory-access PDMPs on homicide, robbery, and motor vehicle theft. Generally, mandatory-access PDMPs are found to reduce offenses on the order of about 5-10%.

A limitation of offenses known segment of the UCR is that it does not contain information about the age of either the offender or the victim. There are several reasons to use the other UCR segments that contain demographics about the offender and victim. 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 dependence on pain relievers, though misuse has also been increasing among older adults.[34] Third, prior work has shown that young adults, and in particular young-adult males, have experienced the largest decrease in opioid misuse and related mortality as a result of mandatory access PDMPs (Grecu et al. 2019).[35]

To incorporate information about the offender and victim Appendix Tables A1 and A2 present estimates from FBI UCR arrest rates, which are commonly used in the crime literature (see for instance, Corman et al. 2014, 2017). It is validating that these estimates are largely consistent with those from the Offenses Known Segment. They consistently show that mandatory PDMPs are associated with a reduction in total arrests and are driven by both a reduction in violent and property crime arrests. The effect magnitudes are also similar to those reported in Table 2, implying a decline on the order of about 5-6%. Decomposing these effects

[34] 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.
[35] We also estimate the effect of PDMP with an alternative subsample of agencies that report crimes during all 12 months every year of the study and explore with defining the dependent variable as the log or as the log plus one in order to avoid agencies with zero counts and the results remain unchanged. See Table A2 and A3.

into specific crime types, we find a significant reduction in assault, burglary, and motor vehicle theft, particularly among young adults (ages 18-39). There is also a suggestive decline in robbery, assaults, burglary, and motor vehicle theft (5-13%).[36]

5.3 Homicide

Given that we find significant effects of PDMPs on homicide, we further evaluate whether this effect is also reflected in data from the UCR Homicide Supplements. Based on the Homicide Supplements we are further able to evaluate the extent to which the effect of PDMPs on homicides is driven by a particular demographic group among offenders (gender and age), to what extent it happens to a particular demographic group of victims, and whether the effects are driven by homicides involving a particular weapon or firearms. For consistency, we aggregate the data at the agency-year level.

Table 4 presents the results of the DD model estimated with the UCR Homicide Supplements where the dependent variable is the natural log of the number of homicides per 100,000 residents, conditioned on demographics. Results are broadly in line with the results in Table 3 in that voluntary PDMPs are not found to have a statistically significant impact on homicides, and mandatory-access PDMPs are found to have a significant impact.
Columns 2-3 of Table 4 report the effects of PDMPs on weapon used. The results indicate that mandated PDMPs decreased the rate of homicides that occurred with a firearm by approximately 7-14%. If stricter PDMP regulations affects the circumstances under which individuals access the illicit drug market, wherein interactions are particularly more likely to involve guns, one

[36] 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.

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).[37] The reduction in homicides, and in particular homicides involving handguns, suggest that overall, PDMPs may not have increased interactions with the illicit drug market. This is prima facie consistent with the prior literature that found that these interventions resulted in a net decrease in opioid misuse and related health consequences.

Columns 4-5 evaluate the impact of PDMPs on homicides of victims conditioned on age. Results indicate that mandatory access PDMPs reduced the number of homicides where the victim was between ages 18-39 by 7-13% and the number of homicides where the victim was over 40 by 8%.[38] Both of these effects are statistically significant at conventional levels. The last two columns present evidence that the number of homicides committed by individuals between the ages of 18-39 decreased by about 9%, with a similar magnitude of effect among older offenders.[39] As noted previously, the effects on the offender's demographics must be interpreted with caution because the demographics of the offender are largely missing.

Table 5 presents the effects of PDMP on the demographics of the victim (Panel A) and offender by gender (Panel B) and can be summarized as follows: There is a decrease in the rate of male homicide victims of 6-11% and this is driven mostly by a decrease in homicide rate of 18-39 year old men of approximately 6-12%. There is a decrease in the rate of female homicide

[37] 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).
[38] The effect on victimization of older adults is not statistically different from that for younger victims.
[39] Results a close to zero for models estimated with no time trends and the results for offenders over 40 are less precisely estimated.

victims of 14%, driven by larger effects among older females (40+). As indicated by the means reported in the table, homicide is a crime where the offender and victim are typically male. Hence, even if relative effects for female victims are similar, 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 victims (ages 18-39).

Alternately we can also turn to arrests to identify whether the effects are different among younger or older adult offenders. Appendix Table A1 presents evidence that the decrease in overall and property crimes are somewhat higher among 18-39 year olds than among individuals over age 40. On the other hand, the effect of PDMP on violent crimes are generally similar in magnitude among younger or older adults. It should be noted that, though these relative effects are more or less similar, the effect sizes imply substantially larger reductions in the total number of crimes committed by young adult offenders (given that young adults commit more crimes than older adults).

5.4 Event Study and Timing of the Effects

We visually present the event study in Figures 1-4. Specifically, Figure 1 presents the coefficients β^j from Equation (2) corresponding to dates of mandated access implementation, for total Part 1 crimes, and Figures 2 and 3 present the corresponding estimates for violent and property crime rates respectively. Figure 4 separately presents the event study coefficients using the dates of voluntary PDMP implementation.[40]

Our event-study results underscore four points, all of which instill a degree of confidence to our estimates. First, there is consistent, dynamic evidence that voluntary PDMPs did not

[40] 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. Furthermore, dynamics in the effect magnitudes (shorter vs. longer term effects, for instance) may also capture differential effects across early vs. later adopters (Rees et al. 2019), and estimates should be interpreted with care.

impact crime rates in any significant manner (Figure 4). Given that most of the literature has found little to no first-order effects of these discretionary programs on opioid misuse, 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 mandatory PDMP. This suggests that PDMPs were not endogenously implemented in response to changes in crime trends.

Third, figures 1-3 suggest that a reduction in crime materializes after the implementation of the mandatory access PDMPs. These results are also reflected in the expected cost of crime.[41] Fourth, there are important dynamics in the treatment effects. For violent crime, the post-policy effects persist up to our window of observation (4 years); there also appear to be lagged effects of the policy such that the effects on violent crime get stronger 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. 2019).[42]

For property crime, the post-policy effects are negative for a while but then tend to rebound back to pre-policy levels by the last year of our observation window. This is consistent

[41] Figure A1 presents the coefficient corresponding to the event study using the dates of modern fully accessible PDMP systems from Horwitz et al (2018) and the results remained similar. Since the dates presented in Horwitz et al (2018) are not as recent as those of the PDAPS mandated access, we show a longer event study when using those dates, where we estimate dynamic effects within a five-year period and impose endpoint restrictions where $\beta^{k} = \bar{\beta}$ if $t \geq 6$ and $\beta^{k} = \underline{\beta}$ if $t \leq 6$. The endpoint restrictions prevent us from assigning unequal weight to cities that enacted their PDMP particularly earlier or later given that the sample is unbalanced in event time (Kline, 2014; McCrary, 2007).

[42] 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; indicating that an average post-policy effect may mask important dynamics in the presence of drug transitions.

with Dobkin and Nicosia (2009) who also found that changes in prices and misuse indicators related to supply-side interventions, albeit for methamphetamines, were transient and returned to pre-intervention levels within two years. In terms of non-cost adjusted crime rates, the transient effects for property crime dominate those for violent crime and hence Figure 1 also suggests that total crime may revert back within four years. However, when one accounts for the relative severity of violent crime, cost-adjusted crime rates (Figure 1) – where violent crime carries a larger weight - continue to show a sustained decline. The discrepancies between crime rates and cost-adjusted crime rates arise because the more socially costly crimes are the least prevalent. While a very small change in crime would be unlikely to noticeably change crime rates, the cost-adjusted measure would capture it if these crimes are costly.

5.5 Robustness Checks and Falsification Diagnostics

<u>Drug Arrests</u>

One advantage of the arrest 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. Previous work has reported strong net decreases in opioid misuse among younger adults as a result of PDMPs (Grecu et al. 2019). Hence, if there is a decrease in Rx opioid misuse, 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. Drug-related arrest data are subject to measurement error and the effects of PDMP on drug-related arrest rates are largely imprecise.

Nevertheless, Appendix Table A3 presents some evidence of a decrease in arrests related to drugs offenses in general. This decrease is primarily driven by synthetic drugs (manufactured addicting narcotics such as Demerol and methadone) and by other non-narcotic drugs (e.g. barbiturates and Benzedrine). Previous literature has found a diversion effect from opioids to heroin, but our specification does not have sufficient power even though the effects are positive and suggestive of an increase of about 5% among younger adults. 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 misuse (Liang et al. 2018; Bachhuber et al. 2014). Consistent with this literature, we find a positive effect on marijuana-related arrests (on the order of 6%); however, this effect is statistically insignificant at the conventional level and not consistent across the trend controls.[43]

Falsification Diagnostics

As discussed in the previous section, we perform a placebo check similar to the randomization inference outlined in Abadie and Gardeazabal (2003) in order to evaluate where our results fare relative to a placebo analysis where agency-year indicators for whether mandated access is active are re-shuffled and randomly assigned. For this falsification exercise, we estimate equation (1) with an iteration-specific "shuffled" or placebo pseudo-PDMP indicator and repeat this process 300 times. Figures 5 presents the coefficients of these iterations for the 300 placebo parameters alongside the actual main policy effect and visual inspection suggest that the estimated effect of PDMPs is considerably different from the placebo estimates for the total crime rates as well as for the cost-adjusted crime rates. Figure A2 presents the coefficients of a similar exercise using the dates presented in Horwitz et al (2018) and the results remain similar.

Other Specification Checks

We implemented the following additional checks to verify that our main results are robust to alternate specifications and adjustments for sampling issues, and to assess and these

[43] We further examine the relationship between drug-related arrests and PDMP implementation using the dates from Horwitz et al (2018) and those results present evidence of a decrease in arrests related to "other drug" (e.g. Barbiturates and Benzedrine) that is statistically significant at the conventional level.

results are available upon request. First, we alternately specify the outcome as the crime rate or the natural log of the count of offenses (or arrests). Second, we estimate models using inverse hyperbolic sine transformation (that can account for zero cell counts without having to add one in the log models), fixed effects Poisson and negative binomial specifications. Third, we evaluate the sensitivity of our results to weighting. In our main analyses, we weight all models by the agency population, which 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. Our coefficient estimates, patterns of results, and general conclusions are not materially affected by unweighting.

Fourth, we aggregated up all crime data to the state-year level, and re-estimated all specifications. Aggregation did not materially impact our results. Finally, in our main analyses we restricted the analyses to those agencies that consistently reported their crime statistics for every month of our sample period (that is, reported over all 180 months over our 2003-2017 sample period). As a robustness check we instead use agency-year cells that consistently reported over all 12 months in a given year, yielding an unbalanced panel of agencies (that still nevertheless reported consistently over all 12 months in the given calendar year for which we included their data). All of our estimates, in terms of signs, magnitudes, and statistical significance, remain robust in this expanded sample.

5.6 Alternate PDMP Measures

The bulk of the recent literature that has evaluated the effectiveness of PDMPs (for instance, Grecu et al. 2019; Buchmueller et al. 2018; Ali et al. 2017) stress the importance of differentiating mandatory access provisions to PDMPs. These studies find strong evidence that mandatory provisions have effectively reduced measures of opioid misuse. In recent work, Horwitz et al. (2018) stress another important dimension of such programs, notably when a state's full modern, electronic PDMP system became operational and became directly accessible to users (all key providers, law enforcement). They show that this aspect of PDMP deployment is significantly and negatively associated with doctor shopping and negatively associated with the dispensing of Rx opioids.

Given this evidence of a "first-stage", that such PDMPs appear to have reduced Rx opioid misuse, we also assess whether this dimension produces declines in crime consistent with mandatory access provisions. Appendix Table A4 and A5 utilize these alternate dates of modern, electronic, and fully accessible PDMP deployment from Horwitz et al. (2018) to assess effects on crime (based on both offenses known and the arrests). It is validating that these estimates largely confirm our previously discussed findings; they indicate a significant reduction in total crime as well as in violent and property crime, on the order of about 4%. Also, figure A1 graphically presents estimates from the event study analysis of PDMP deployment using the dates presented in Horwitz et al (2018). Reassuringly, the lead effects are insignificant and close to 0 in magnitude, suggestive of parallel pre-policy trends between the treated and control states. Furthermore, where there are reductions in crime, they materialize only after the deployment of the modern and fully accessible PDMP, with dynamics consistent with those discussed above with respect to mandatory PDMPs.

5.7 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 property crime. The effect magnitudes indicate about a 5% reduction in the total number of offenses overall, and specifically a 7% reduction in the number arrests among young adults. These are reduced-form estimates that directly link 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 misuse from the literature to impute an "implied instrumental variables" (IV) estimate of the structural effect of shifts in Rx drug misuse on crime.[44] Specifically, Grecu, Dave and Saffer (2018), based on similar DD and event-study specifications, find robust evidence that mandatory PDMPs reduced Rx opioid misuse 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 misuse is about 0.2 for young adults.[45] This effect in line with the literature relating other substances (heavy alcohol use, crack cocaine) to crime (Grogger and Willis 2000; Carpenter 2007; Fryer et al. 2013).

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 misuse. In 2017, about 2.5 million adults (ages 18-25) misused opioid pain relievers (based on the NSDUH), and law enforcement made about 2.21 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 misusers by about 750,000 and decreased total arrests among young adults by about 154,700. This indicates that for every 5 or so fewer Rx drug misusers, about one arrest appears to have been averted. Thus, the marginal effect of Rx opioid misuse on arrests is also about 0.2 (154,700 / 750,000). This compares to an average probability of an arrest relative to

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[44] 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)

[45] The reduced-form effect of the policy on crime is about 7% and the reduced-form of the policy on Rx opioid abuse is about 30% (26-32%), among young adults. Thus, the implied IV elasticity, akin to a Wald estimate, is: (-0.07 / -0.30) or 0.2.

having ever misused opioids, of about 0.31.[46] Hence, the marginal probability implied by our estimates is reasonable and "in the ball park"; that it is somewhat smaller than the average probability may imply a concave crime production function with respect to Rx opioid misuse.

While these imputed estimates help to frame the potential importance of PDMPs in affecting crime, help derive a structural causal effect of Rx drug misuse 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 misuse on crime assumes that shifts in Rx opioid misuse 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 misuser 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 misuse and also consistent with descriptive data on the percent of opioid misusers who are arrested.

6. Conclusion

The misuse of opioids in the United States has quadrupled in the last 15 years and has reached epidemic proportions. In an attempt to mitigate opioid misuse 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-

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[46] 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.

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 misuse and adverse health consequences, though there may also be unintended costs due to the possibility that individuals 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 stricter and well-deployed 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 about 5%, which is driven by reductions in violent crimes (4%), specifically homicides, as well as property crime (5%), specifically burglary and motor vehicle theft.[47] 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

[47] The 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).

policies, such as increasing the size of the police force by approximately 10% (Evans and Owens 2007; Chalfin and McCrary 2007)[48].

The reduction in violent crime, and in particular homicides, may reflect the pharmacologic and systemic effects linking substance misuse to crime. Prior work has found that mandatory PDMPs have led to robust reductions in opioid misuse 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 misuser, 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.[49]

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,

[48] To put this in cotext, given the cost-weighted crime elasticity of -0.21 to -0.47 of police and crime (Chalfin and McCrary, 2017), the 4%-6% decrease in the expected cost of total crime driven by the implementation of mandated access PDMP has an effect comparable to a 10% increase in the police force. Given the high cost of expanding policing services as the annual salary of a police officer in 2018 is $65,400 as estimated by the Bureau of Labor statistics (last accessed October 1st, 2019), expanding PDMP to mandated access PDMP may be a cost-effective tool to fight crime.

[49] 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.

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 2017, 18 states, representing 33% of the population had required that providers must use the PDMP prior to prescribing 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 33% coverage rate to universal coverage across the U.S. could reduce violent crime by about 3.4% or by about 42,408 offenses.[50] This would result in economic cost savings of up to $9.8 billion annually.[51] 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.

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[50] Table 2 and Appendix Table A1 reported that mandatory PDMP provisions reduce violent crime by between 4% and 6%, or on average 5%. Expanding coverage by 67% (from 33% to universal coverage) would therefore result in approximately (0.67*5) 3.4% reduction in violent crime. In 2017, the FBI reported 1,247,321 violent Part I crimes (see https://ucr.fbi.gov/crime-in-the-u.s/2017/crime-in-the-u.s.-2017/topic-pages/tables/table-1). Thus, a 3.4% reduction translates into 42,408 fewer offenses. Since property crime might be rebounding by the end of our observation window, we do not include these in our calculations.

[51] 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 2017 dollars yields the total cost of a violent offense as $230,205.

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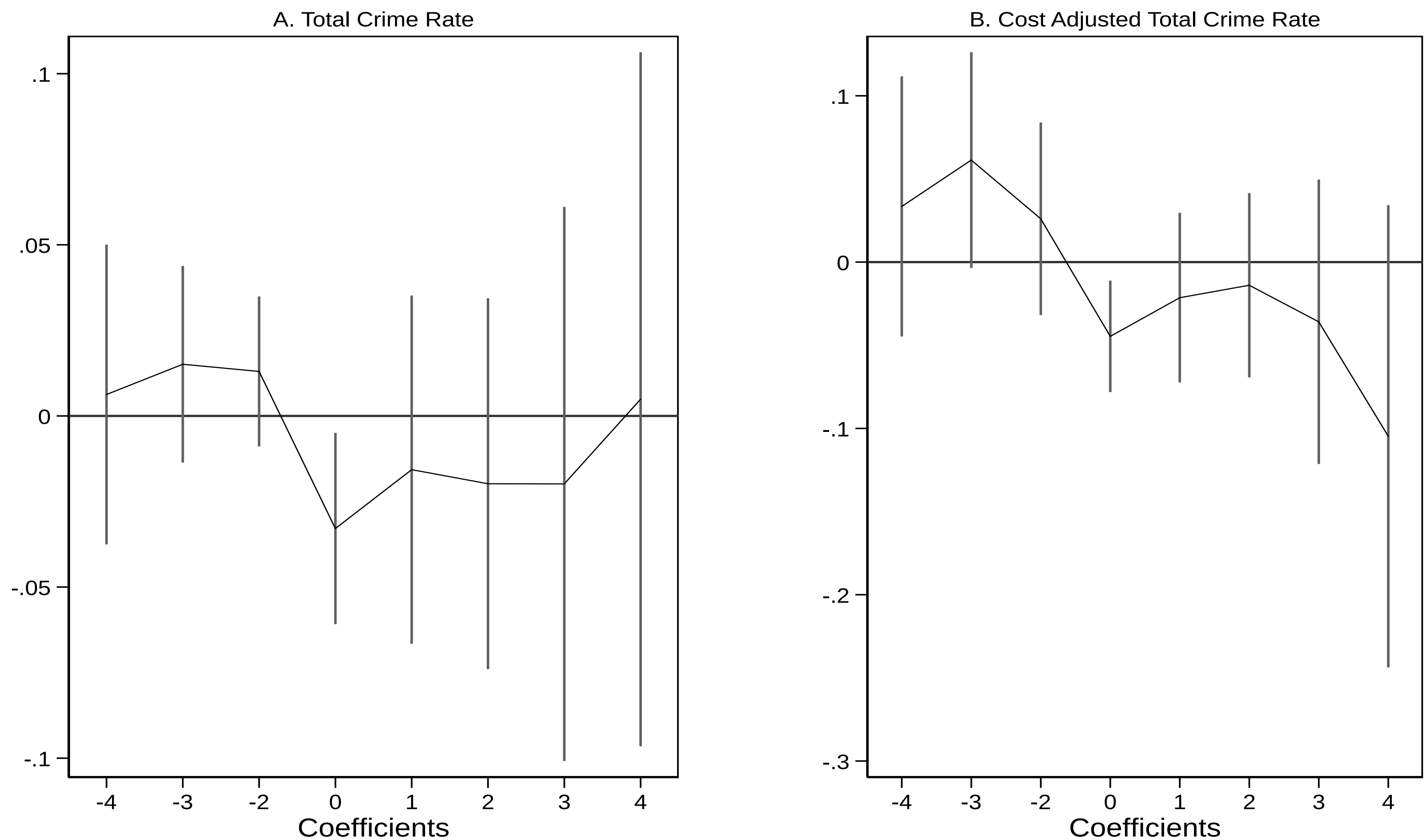
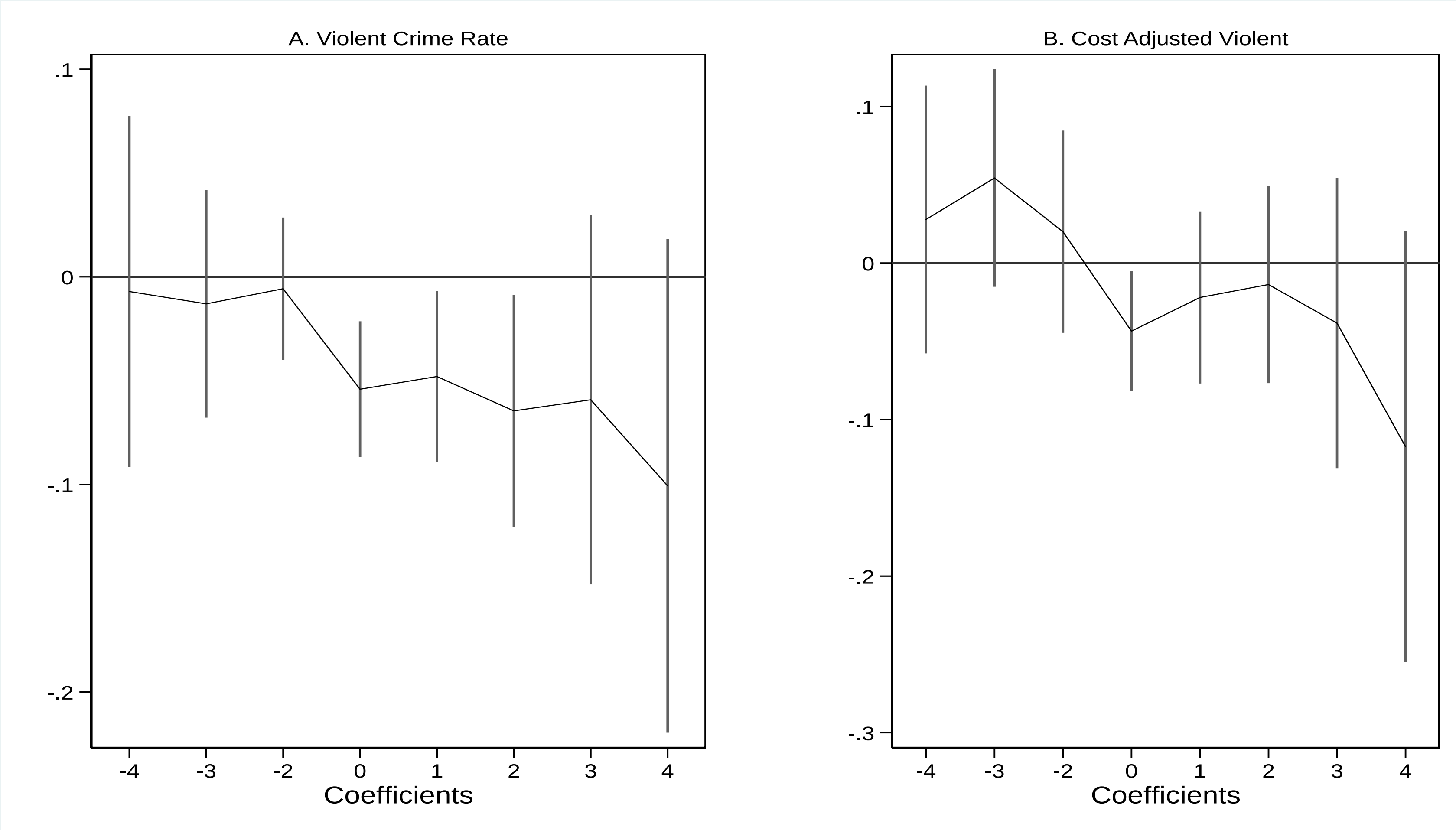
Figure 1: Dynamic Effects of MA PDMP on Total Crime



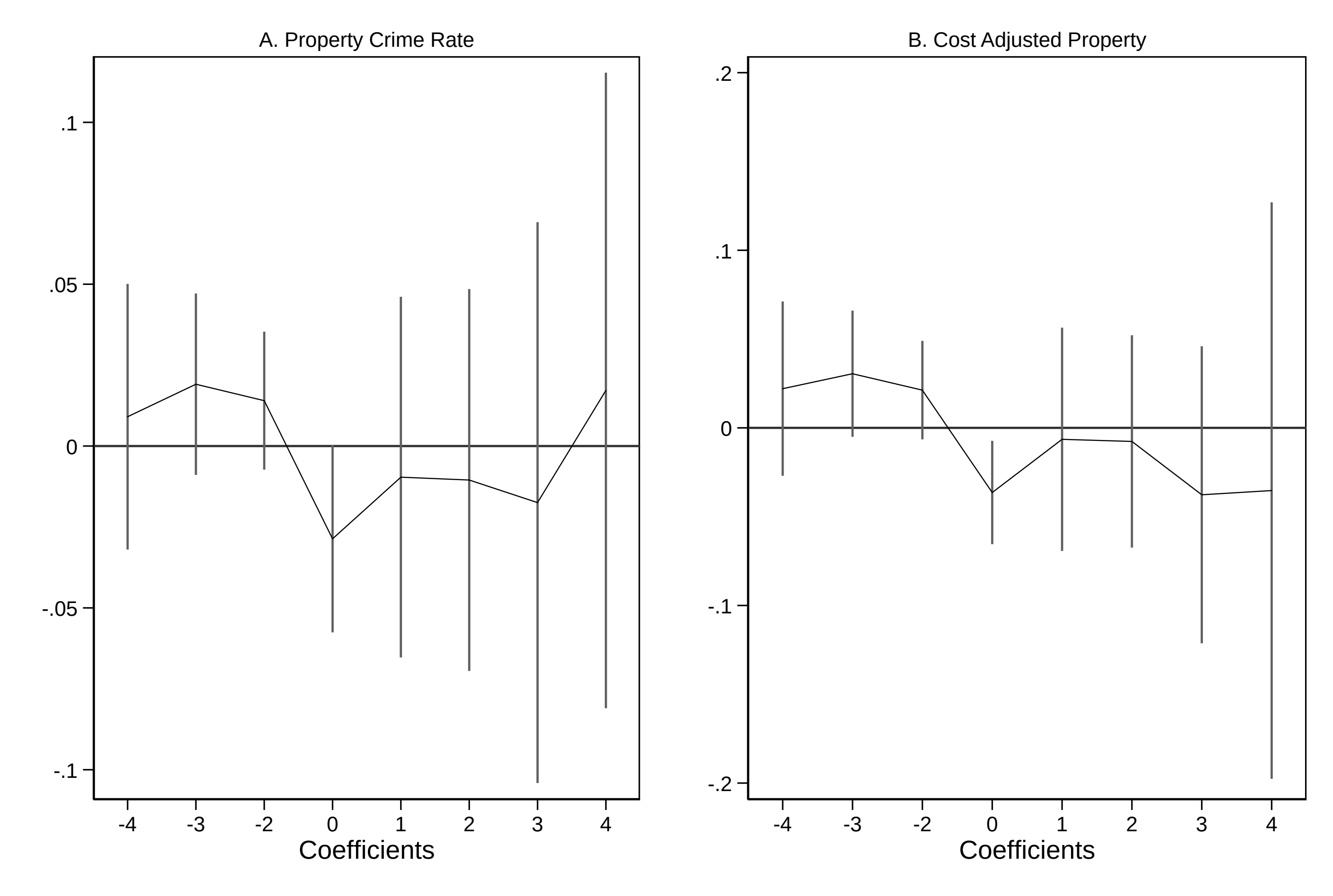
Notes: This event study uses the PDAPS dates of mandatory access implementation. The outcome is total crime rates (crimes per 100,000 residents) and cost-adjusted total crime rates. The coefficient corresponding to the year prior to the implementation (t=-1) of mandated access PDMP is normalized to zero.

Figure 2: Dynamic Effects of MA PDMP on Violent Crime



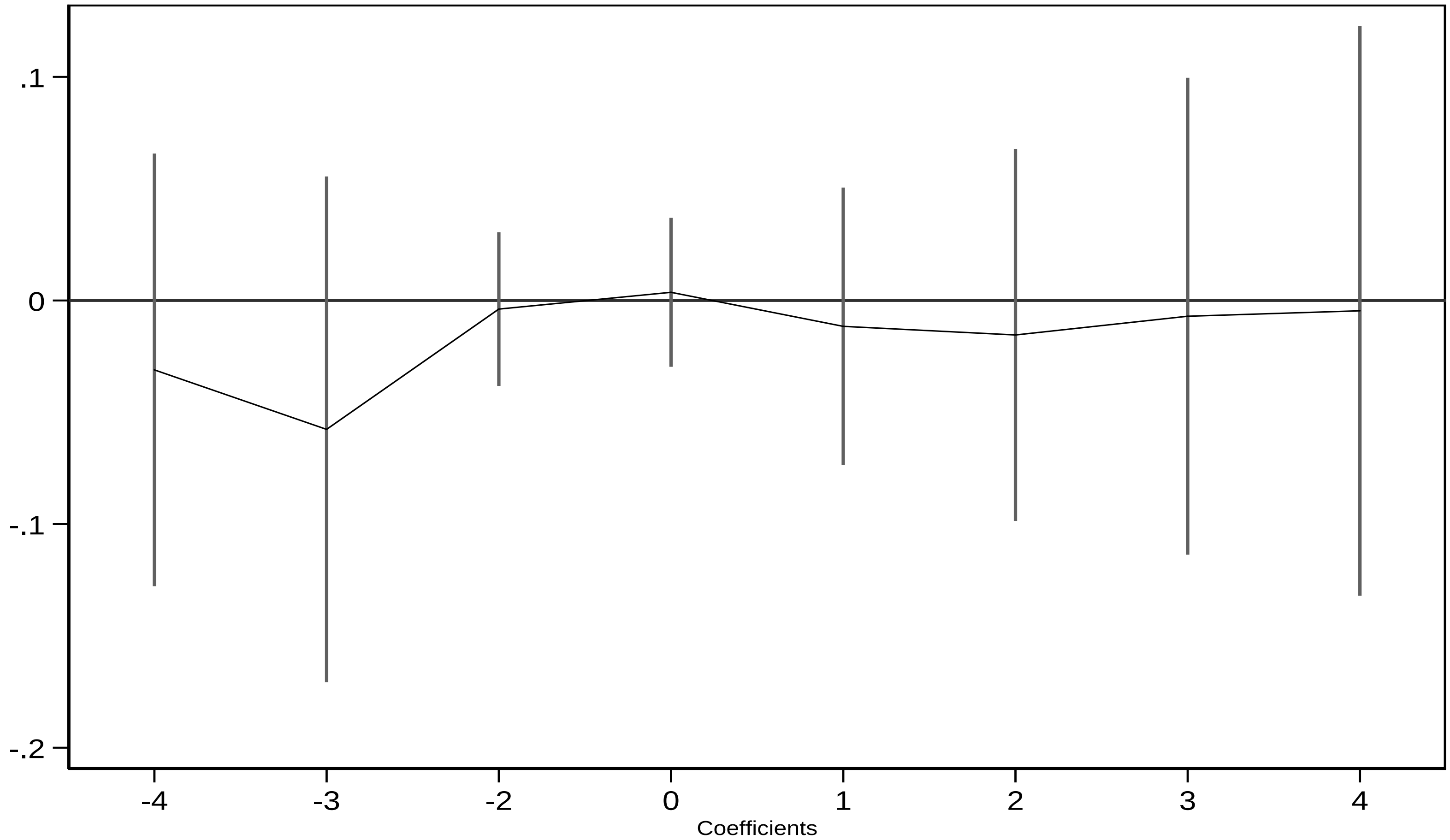
Notes: This event study uses the PDAPS dates of mandatory access implementation. The outcome is violent crime rates (crimes per 100,000 residents) and cost-adjusted violent crime rates. The coefficient corresponding to the year prior to the implementation (t=-1) of mandated access PDMP is normalized to zero.

Figure 3: Dynamic Effects of MA PDMP on Property Crime



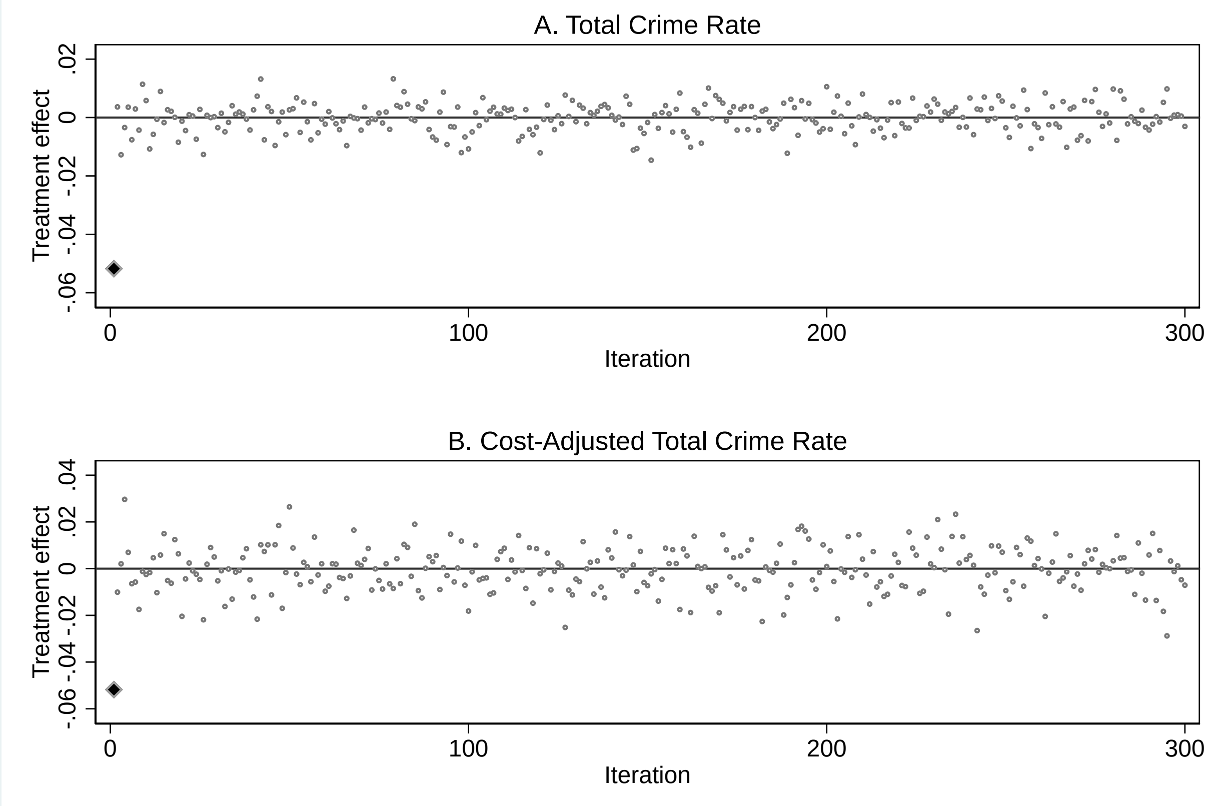
Notes: This event study uses the PDAPS dates of mandatory access implementation. The outcome is violent crime rates (crimes per 100,000 residents) and cost-adjusted violent crime rates. The coefficient corresponding to the year prior to the implementation (t=-1) of mandated access PDMP is normalized to zero.

Figure 4: Dynamic Effects of Voluntary PDMP on Total Crime



Notes: This event study uses the PDAPS dates of voluntary implementation. The outcome is total crime rates (crimes per 100,000 residents). The coefficient corresponding to the year prior to the implementation (t=-1) of voluntary PDMP is normalized to zero.

Figure 5: Randomization Inference, Mandated Access



Notes: This randomization inference exercise estimates the DD specification 300 times after "re-shuffling" the indicator for whether a state has a mandatory access PDMP and estimates the effect of a mandated access PDMP relative to a voluntary PDMP.

Table 1: Summary Statistics, Crime Rate per 100,000 Residents

	Full Sample		Subsample Reports 12 Months	
	Mean	**SD**	**Mean**	**SD**
Panel A: Offenses Known (per 100,000 population covered)				
Total	2,240.07	(27,420.41)	3,253.86	(39,638.11)
Violent	212.77	(635.58)	298.90	(751.33)
Property	2,027.33	(27,149.56)	2,954.97	(39,269.62)
Homicide	2.39	(14.90)	3.25	(14.67)
Rape	19.75	(46.67)	27.59	(43.16)
Robbery	38.02	(287.74)	58.13	(407.75)
Assault	152.82	(417.94)	210.19	(402.53)
Burglary	410.50	(779.26)	562.46	(976.06)
Larceny	1,487.74	(26,643.28)	2,193.23	(38,571.12)
MV Theft	129.09	(1,355.15)	199.28	(1,939.42)
Agencies	22,779		9,136	
Observations	243,986		115,892	
Panel B: Homicide Circumstances (per 100,000 population covered)				
	Full Sample		**City Pop>10,000**	
	Mean	**SD**	**Mean**	**SD**
Murders	11.82	(27.02)	10.56	(9.79)
Weapon Firearm	7.18	(24.28)	7.83	(8.53)
Weapon Knife	1.74	(7.27)	1.10	(1.31)
Victim 18-39	6.18	(20.95)	6.95	(7.15)
Victim Over 40	4.32	(13.15)	2.61	(2.66)
Offender 18-39	5.88	(14.12)	4.65	(4.12)
Offender Over 40	3.05	(18.24)	1.14	(1.32)
Victim Male	8.41	(24.26)	8.83	(8.81)
Victim Female	3.36	(12.42)	1.72	(1.81)
Victim Male 18-39	4.76	(19.31)	6.09	(6.61)
Victim Male Over 40	2.84	(11.16)	1.98	(2.26)
Victim Female 18-39	1.40	(8.13)	0.85	(1.22)
Victim Female Over 40	1.47	(7.23)	0.62	(0.96)
Agencies	8,988		316	
Observations	39,069		4,740	

Table 1 (Continued) : Summary Statistics, Crime Rate per 100,000 Residents

	Full Sample		Subsample Reports 12 Months	
	Mean	**SD**	**Mean**	**SD**
Panel C: Arrests (per 100,000 population covered)				
Total	866.27	(16,742.59)	807.01	(3,038.45)
Violent	147.61	(3,333.44)	171.00	(543.45)
Property	718.66	(13,448.34)	636.01	(2,600.73)
Murder	2.49	(24.15)	2.65	(15.15)
Rape	7.10	(22.09)	7.70	(16.77)
Robbery	20.07	(117.09)	26.79	(155.91)
Assault	117.95	(3,316.69)	133.86	(396.72)
Burglary	93.31	(573.89)	109.80	(714.87)
Larceny	594.45	(6,943.70)	493.76	(1,764.37)
MV Theft	30.91	(6,600.40)	32.45	(339.83)
Total 18-39	416.53	(1,640.12)	498.71	(2,063.25)
Violent 18-39	94.82	(358.51)	110.85	(393.01)
Property 18-39	321.71	(1,361.07)	387.86	(1,737.45)
Total 40 Plus	162.36	(3,332.54)	156.01	(700.40)
Violent 40 Plus	65.00	(3,299.19)	38.84	(137.83)
Property 40 Plus	97.35	(423.49)	117.18	(600.08)
N Agencies	22,640		3,987	

Notes: The first two columns present summary statistics for the entire sample. The last two columns present summary statistics for the subsample used in this analysis. The analysis using offenses known to police and arrest file are restricted to agencies that reported crimes all 12 months while the Supplementary Homicide Report analysis uses agencies that correspond to a city of at least 10,000 residents and that report at least one homicide in each year of study. Offenses known data is available until 2017 and arrest data is available until 2016.

Table 2: The Effect of PDMP on Total, Violent and Property Crimes

	FBI UCR Offenses Known								
	Total			Violent			Property		
Panel A: Ln Crime Rates									
PDMP	-0.029**	-0.023	-0.022	-0.040	-0.042	-0.000	-0.025	-0.018	-0.026
	(0.014)	(0.015)	(0.020)	(0.031)	(0.031)	(0.019)	(0.015)	(0.015)	(0.021)
MA	-0.022	-0.052*	-0.057*	-0.058**	-0.044	-0.040*	-0.016	-0.049	-0.053*
	(0.025)	(0.029)	(0.029)	(0.028)	(0.029)	(0.023)	(0.026)	(0.030)	(0.029)
Observations	115,892	115,892	115,892	115,891	115,891	115,891	115,895	115,895	115,895
Total MA Effect	-0.052*	-0.075**	-0.078**	-0.097***	-0.086**	-0.041	-0.041	-0.066*	-0.079**
	(0.028)	(0.031)	(0.034)	(0.034)	(0.034)	(0.027)	(0.031)	(0.034)	(0.036)
Panel B: Ln Cost-Adjusted Crime Rates									
PDMP	-0.024	-0.026	0.023	-0.019	-0.022	0.026	-0.030	-0.031	-0.020
	(0.022)	(0.024)	(0.019)	(0.025)	(0.027)	(0.021)	(0.023)	(0.023)	(0.022)
MA	-0.064**	-0.052*	-0.039	-0.061**	-0.045	-0.030	-0.038	-0.035	-0.045
	(0.028)	(0.029)	(0.027)	(0.030)	(0.032)	(0.029)	(0.026)	(0.028)	(0.030)
Observations	115,882	115,882	115,882	115,871	115,871	115,871	115,895	115,895	115,895
Total MA Effect	-0.087**	-0.078**	-0.016	-0.080**	-0.068*	-0.004	-0.069*	-0.066*	-0.065*
	(0.034)	(0.031)	(0.028)	(0.037)	(0.032)	(0.028)	(0.034)	(0.036)	(0.036)
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	N	Y	N	N	Y	N	N	Y
Treatment Trend	N	Y	N	N	Y	N	N	Y	N
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: Models weighted by population agency. Standard erros are clustered by state and reported in parentheses. All models controls for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table 3: The Effect of PDMP on Crime Categories, Ln Crime Rate

	FBI UCR Offenses Known		
Panel A: Homicide			
PDMP	-0.005	-0.004	0.024
	(0.017)	(0.019)	(0.017)
MA	-0.048*	-0.052**	-0.034
	(0.027)	(0.024)	(0.021)
Observations	115,891	115,891	115,891
Total MA Effect	-0.053	-0.056*	-0.010
	(0.034)	(0.029)	(0.023)
Panel B: Rape			
PDMP	-0.152	-0.170	0.001
	(0.107)	(0.125)	(0.046)
MA	-0.309	-0.224	-0.098
	(0.252)	(0.175)	(0.071)
Observations	115,833	115,833	115,833
Total MA Effect	-0.461	-0.394	-0.096
	(0.348)	(0.283)	(0.062)
Panel C: Robbery			
PDMP	-0.016	-0.023	0.010
	(0.029)	(0.029)	(0.016)
MA	-0.057*	-0.025	-0.026
	(0.032)	(0.033)	(0.027)
Observations	115,889	115,889	115,889
Total MA Effect	-0.073**	-0.048	-0.015
	(0.035)	(0.033)	(0.027)
Panel D: Assault			
PDMP	-0.035	-0.033	-0.012
	(0.036)	(0.036)	(0.023)
MA	-0.047	-0.061*	-0.054*
	(0.036)	(0.036)	(0.030)
Observations	115,886	115,886	115,886
Total MA Effect	-0.083*	-0.093*	-0.066*
	(0.046)	(0.047)	(0.036)
Agency FE	Y	Y	Y
Year FE	Y	Y	Y
State Trend	N	N	Y
Treatment Trend	N	Y	N
Weight Agency Pop	Y	Y	Y
Cluster State	Y	Y	Y

(Continued) Table 3: The Effect of PDMP on Crime Categories , Ln Crime Rate

	FBI UCR Offenses Known		
Panel E: Burglary			
PDMP	-0.035	-0.029	-0.013
	(0.023)	(0.023)	(0.023)
MA	-0.052	-0.081**	-0.091**
	(0.036)	(0.038)	(0.039)
Observations	115,895	115,895	115,895
Total MA Effect	-0.087**	-0.110***	-0.105**
	(0.039)	(0.041)	(0.045)
Panel F: Larceny			
PDMP	-0.041	-0.036	-0.096
	(0.035)	(0.035)	(0.070)
MA	-0.017	-0.040	-0.016
	(0.039)	(0.051)	(0.044)
Observations	115,895	115,895	115,895
Total MA Effect	-0.057	-0.076	-0.112
	(0.067)	(0.073)	(0.088)
Panel G: MV Theft			
PDMP	-0.060	-0.076**	-0.040
	(0.040)	(0.038)	(0.029)
MA	-0.103***	-0.024	-0.037
	(0.037)	(0.030)	(0.031)
Observations	115,894	115,894	115,894
Total MA Effect	-0.163***	-0.100**	-0.078
	(0.054)	(0.049)	(0.047)
Agency FE	Y	Y	Y
Year FE	Y	Y	Y
State Trend	N	N	Y
Treatment Trend	N	Y	N
Weight Agency Pop	Y	Y	Y
Cluster State	Y	Y	Y

Notes: Models weighted by population agency. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table 4: The Effect of PDMP on Homicide Circumstances

	FBI UCR Supplementary Homicide Reports						
	Murders	**Weapon Firearms**	**Weapon Knife**	**Victim 18-39**	**Victim Over 40**	**Offender 18-39**	**Offender Over 40**
Panel A: No Trend							
PDMP	-0.025	-0.012	0.041	-0.019	-0.028	-0.026	-0.068
	(0.035)	(0.047)	(0.045)	(0.047)	(0.039)	(0.049)	(0.041)
MA	-0.116**	-0.126**	-0.037	-0.129**	-0.085**	-0.002	-0.029
	(0.047)	(0.058)	(0.030)	(0.055)	(0.033)	(0.051)	(0.056)
Total MA Effect	-0.141**	-0.138	0.004	-0.148*	-0.113*	-0.028	-0.097
	(0.067)	(0.086)	(0.050)	(0.083)	(0.056)	(0.081)	(0.080)
Panel C: Treatment Trend							
PDMP	-0.024	-0.009	0.038	-0.019	-0.028	-0.007	-0.056
	(0.037)	(0.049)	(0.047)	(0.047)	(0.043)	(0.049)	(0.042)
MA	-0.122***	-0.138**	-0.023	-0.127**	-0.084**	-0.086*	-0.081
	(0.042)	(0.051)	(0.045)	(0.052)	(0.040)	(0.047)	(0.059)
Total MA Effect	-0.145**	-0.147*	0.015	-0.146*	-0.112**	-0.093	-0.137*
	(0.061)	(0.080)	(0.051)	(0.082)	(0.051)	(0.073)	(0.074)
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

(Continued) Table 4: The Effect of PDMP on Homicide Circumstances

	FBI UCR Supplementary Homicide Reports						
	Murders	Weapon Firearms	Weapon Knife	Victim 18-39	Victim Over 40	Offender 18-39	Offender Over 40
Panel B: State Trend							
PDMP	0.018	0.034	0.066	0.020	-0.008	0.017	-0.041
	(0.033)	(0.048)	(0.059)	(0.039)	(0.046)	(0.055)	(0.060)
MA	-0.075**	-0.074	-0.028	-0.069*	-0.083*	-0.088**	-0.133**
	(0.034)	(0.045)	(0.048)	(0.039)	(0.045)	(0.039)	(0.053)
Total MA Effect	-0.058	-0.040	0.038	-0.049	-0.091	-0.071	-0.174**
	(0.050)	(0.068)	(0.057)	(0.060)	(0.066)	(0.070)	(0.078)
Observations	4,740	4,740	4,740	4,740	4,740	4,740	4,740
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

Notes: Models weighted by population agency. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table 5 : The Effect of PDMP on Demographic Composition of Homicide Victim and Offenders

	FBI UCR Supplementary Homicide Reports								
Panel A: Male Victim									
	Male Victim			**Male Victim 18-39**			**Male Victim Over 40**		
PDMP	-0.022	-0.020	0.048	-0.019	-0.019	0.039	-0.022	-0.023	0.044
	(0.041)	(0.042)	(0.040)	(0.049)	(0.049)	(0.044)	(0.037)	(0.040)	(0.042)
MA	-0.102**	-0.111**	-0.063*	-0.120**	-0.121**	-0.062	-0.057	-0.053	-0.047
	(0.048)	(0.041)	(0.034)	(0.054)	(0.051)	(0.043)	(0.036)	(0.050)	(0.056)
Total MA Effect	-0.124*	-0.131*	-0.015	-0.140	-0.140*	-0.023	-0.079	-0.076	-0.004
	(0.071)	(0.066)	(0.052)	(0.084)	(0.082)	(0.062)	(0.056)	(0.057)	(0.070)
Panel B: Female Victim									
	Female Victim			**Female Victim 18-39**			**Female Victim Over 40**		
PDMP	-0.007	-0.004	-0.046	-0.016	-0.023	-0.030	-0.002	0.006	-0.089
	(0.035)	(0.039)	(0.047)	(0.047)	(0.046)	(0.058)	(0.051)	(0.056)	(0.061)
MA	-0.144**	-0.160**	-0.141**	-0.130+	-0.100	-0.075	-0.097	-0.133**	-0.167***
	(0.056)	(0.066)	(0.061)	(0.065)	(0.075)	(0.064)	(0.063)	(0.062)	(0.061)
Total MA Effect	-0.151**	-0.163**	-0.187**	-0.145	-0.122	-0.105	-0.099	-0.127	-0.256***
	(0.073)	(0.073)	(0.086)	(0.086)	(0.094)	(0.105)	(0.098)	(0.087)	(0.085)
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	N	Y	N	N	Y	N	N	Y
Treatment Trend	N	Y	N	N	Y	N	N	Y	N
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

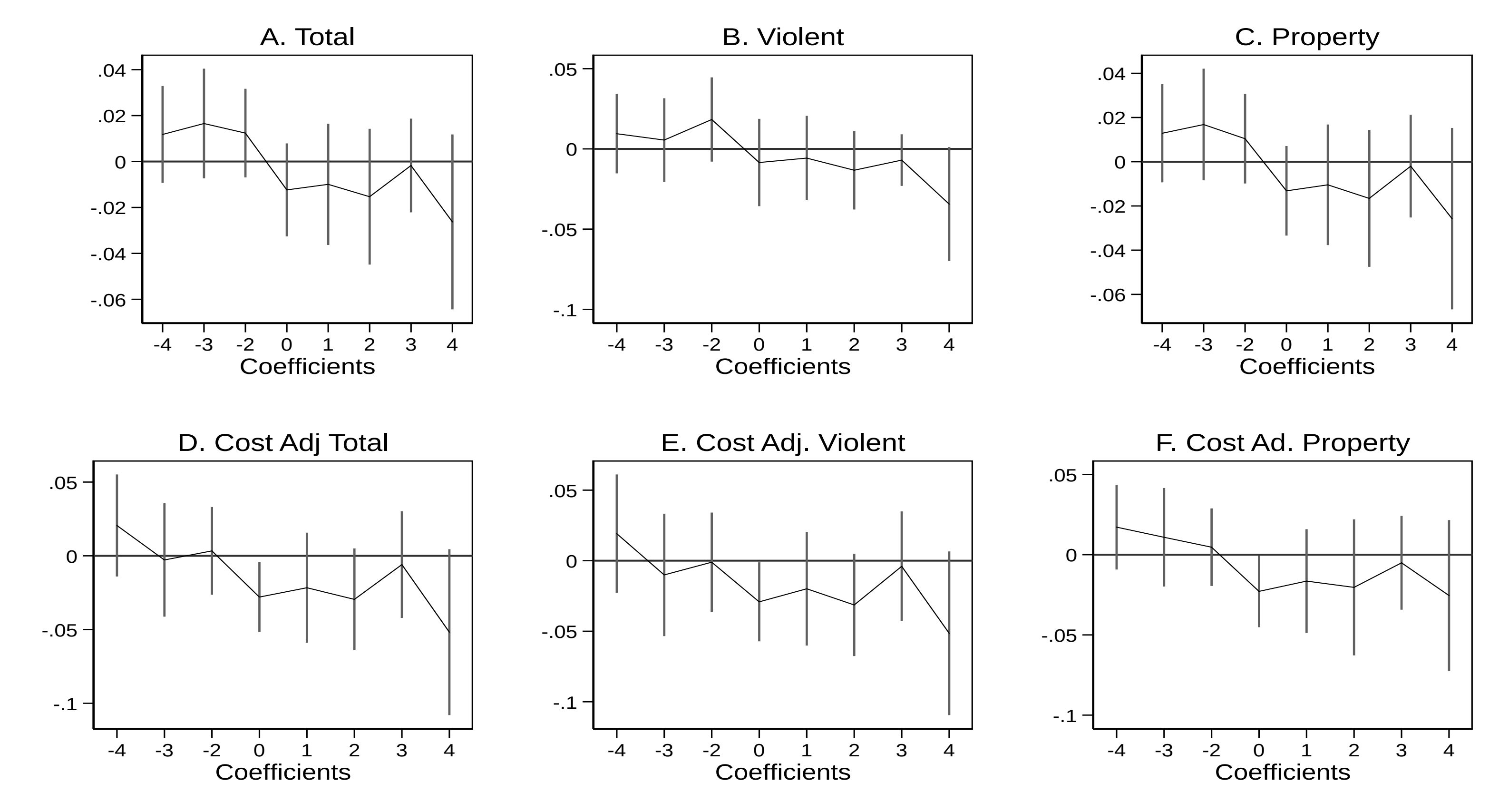
(Continued) Table 5 : The Effect of PDMP on Demographic Composition of Homicide Victim and Offenders

	FBI UCR Supplementary Homicide Reports								
Panel C: Male Offender									
	Male Offender			**Male Offender 18-39**			**Male Over 40**		
PDMP	-0.045	-0.034	-0.014	-0.025	-0.007	0.015	-0.064	-0.053	-0.043
	(0.046)	(0.048)	(0.042)	(0.051)	(0.051)	(0.053)	(0.039)	(0.039)	(0.060)
MA	-0.070	-0.121***	-0.118***	-0.019	-0.100**	-0.095**	-0.026	-0.078	-0.135**
	(0.046)	(0.042)	(0.036)	(0.049)	(0.046)	(0.039)	(0.058)	(0.065)	(0.058)
Total MA Effect	-0.115*	-0.155**	-0.133**	-0.044	-0.107	-0.079	-0.090	-0.130	-0.179**
	(0.066)	(0.059)	(0.060)	(0.080)	(0.072)	(0.068)	(0.081)	(0.079)	(0.087)
Panel D: Female Offender									
	Female Offender			**Female Offender18-39**			**Female Offender Over 40**		
PDMP	-0.018	-0.014	0.009	-0.023	-0.009	0.022	-0.066	-0.057	-0.059
	(0.080)	(0.081)	(0.090)	(0.085)	(0.088)	(0.110)	(0.054)	(0.056)	(0.072)
MA	0.059	0.041	0.001	0.123*	0.059	0.003	-0.018	-0.056	-0.052
	(0.057)	(0.074)	(0.072)	(0.070)	(0.098)	(0.091)	(0.047)	(0.071)	(0.074)
Total MA Effect	0.041	0.027	0.011	0.099	0.050	0.025	-0.084	-0.113	-0.111
	(0.115)	(0.119)	(0.120)	(0.133)	(0.139)	(0.134)	(0.068)	(0.077)	(0.098)
Observations	4,740	4,740	4,740	4,740	4,740	4,740	4,740	4,740	4,740
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	N	Y	N	N	Y	N	N	Y
Treatment Trend	N	Y	N	N	Y	N	N	Y	N
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Models weighted by population agency. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Figure A1: Dynamic Effects of Modern Fully Accessible PDMP

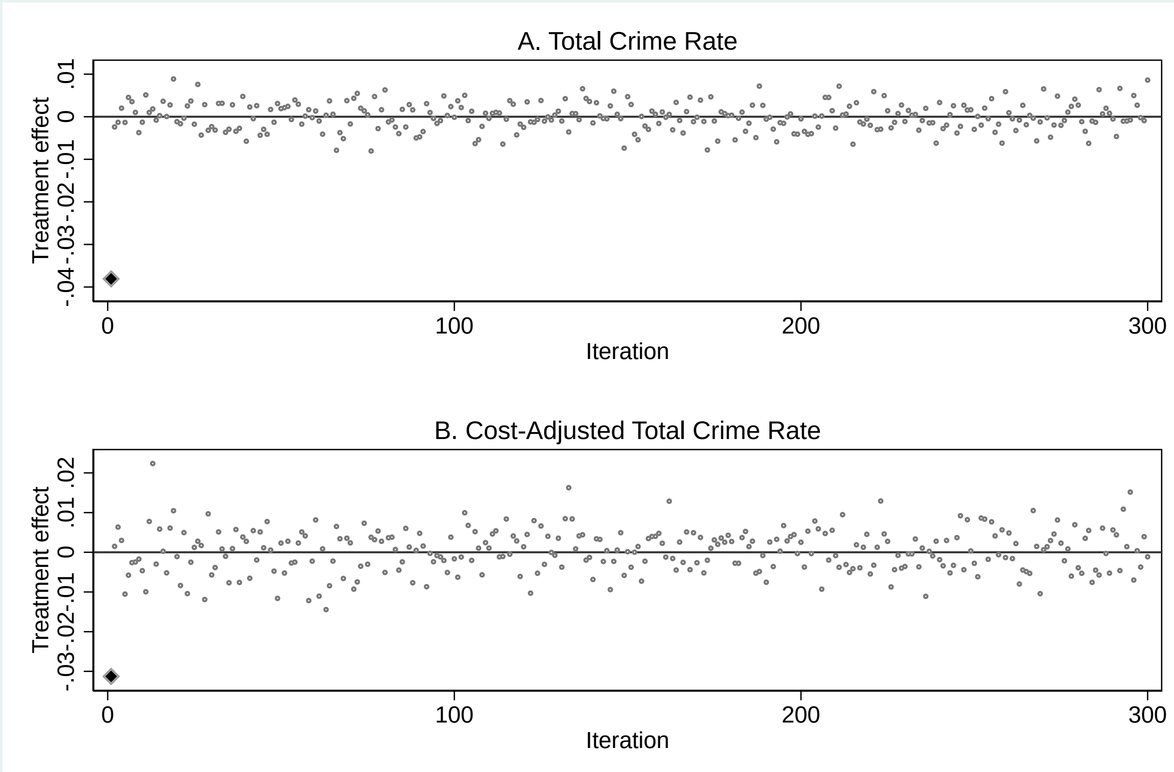
Using Horwitz et al (2018)



Notes: This event study uses the dates of implementation from Horwitz et al (2018). The outcomes are total, violent and property crime rates (crimes per 100,000 residents) and cost-adjusted total, violent, and property crime rates. The coefficient corresponding to the year prior to the implementation (t=-1) of PDMP as defined in Horwitz et al (2018) is normalized to zero.

Figure A2: Randomization Inference, Modern Fully Accessible PDMP Dates

Using Horwitz et al (2018) dates



Notes: This randomization inference exercise estimates the DD specification 300 times after "re-shuffling" the indicator for whether a state has a PDMP and estimates the total effect of PDMP implementation as defined in Horwitz et al (2018)

Table A1: The Effect of PDMP on Total, Violent and Property Ln Arrest Rates

	Total			Violent			Property		
Panel A: All Ages									
PDMP	-0.065**	-0.043	-0.055**	-0.065	-0.052	-0.030	-0.061*	-0.037	-0.066**
	(0.028)	(0.030)	(0.024)	(0.039)	(0.042)	(0.026)	(0.032)	(0.031)	(0.028)
MA	0.023	-0.084***	-0.058***	-0.046	-0.113****	-0.060***	0.054	-0.066*	-0.052**
	(0.035)	(0.030)	(0.019)	(0.040)	(0.024)	(0.018)	(0.038)	(0.035)	(0.023)
Total MA Effect	-0.042	-0.128***	-0.113****	-0.111**	-0.165****	-0.090****	-0.007	-0.103***	-0.118****
	(0.049)	(0.041)	(0.029)	(0.060)	(0.047)	(0.026)	(0.050)	(0.045)	(0.033)
Panel B: Ages 18-39									
PDMP	-0.046	-0.026	-0.048*	-0.053	-0.040	-0.026	-0.046	-0.023	-0.060+
	(0.029)	(0.031)	(0.026)	(0.038)	(0.041)	(0.027)	(0.031)	(0.031)	(0.033)
MA	-0.001	-0.105***	-0.070***	-0.053	-0.115****	-0.061***	0.021	-0.093*	-0.069*
	(0.038)	(0.034)	(0.025)	(0.039)	(0.024)	(0.018)	(0.041)	(0.040)	(0.029)
Total MA Effect	-0.048	-0.130***	-0.118***	-0.106*	-0.155***	-0.087***	-0.025	-0.116***	-0.129****
	(0.053)	(0.044)	(0.033)	(0.059)	(0.046)	(0.027)	(0.054)	(0.049)	(0.040)
Panel C: Age 40 and Over									
PDMP	-0.041	-0.026	-0.042	-0.068*	-0.053	-0.028	-0.033	-0.019	-0.050+
	(0.029)	(0.032)	(0.025)	(0.040)	(0.043)	(0.027)	(0.032)	(0.034)	(0.026)
MA	0.011	-0.066**	-0.055**	-0.034	-0.111****	-0.074***	0.033	-0.035	-0.040+
	(0.036)	(0.029)	(0.021)	(0.041)	(0.028)	(0.023)	(0.042)	(0.034)	(0.024)
Total MA Effect	-0.030	-0.091**	-0.097***	-0.103	-0.164***	-0.102***	0.000	-0.055	-0.090***
	(0.053)	(0.041)	(0.029)	(0.065)	(0.055)	(0.032)	(0.055)	(0.045)	(0.031)
Observations	51,513	51,513	51,513	51,513	51,513	51,513	51,513	51,513	51,513
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	N	Y	N	N	Y	N	N	Y
Treatment Trend	N	Y	N	N	Y	N	N	Y	N
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Models weighted by population agency. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table A2: The Effect of PDMP on Ln Arrest Rates, Crime Categories

	FBI UCR Arrest Rate						
	Murder	**Rape**	**Robbery**	**Assault**	**Burglary**	**Larceny**	**MV Theft**
Panel A: All Ages, No Trend							
PDMP	0.024	-0.088**	-0.006	-0.092*	-0.064*	-0.076*	0.031
	(0.023)	(0.040)	(0.025)	(0.047)	(0.035)	(0.038)	(0.057)
MA	-0.035	0.005	-0.057+	-0.028	-0.032	0.089*	-0.059
	(0.043)	(0.055)	(0.032)	(0.042)	(0.044)	(0.039)	(0.066)
Total MA Effect	-0.001	-0.083	-0.055	-0.119*	-0.095	0.027	-0.019
	(0.043)	(0.070)	(0.044)	(0.067)	(0.065)	(0.056)	(0.080)
Panel B: All Ages, Treated State Trend							
PDMP	0.027	-0.072*	-0.000	-0.076	-0.049	-0.054	0.054
	(0.023)	(0.042)	(0.025)	(0.051)	(0.033)	(0.037)	(0.057)
MA	-0.053	-0.078	-0.084***	-0.113****	-0.109*	-0.028	-0.176**
	(0.042)	(0.054)	(0.030)	(0.029)	(0.055)	(0.037)	(0.072)
Total MA Effect	-0.018	-0.148**	-0.077*	-0.186***	-0.157**	-0.071	-0.121
	(0.039)	(0.065)	(0.040)	(0.057)	(0.075)	(0.044)	(0.087)
Panel C: All Ages, State Trend							
PDMP	-0.006	-0.085*	0.013	-0.066	-0.043	-0.083***	-0.048
	(0.046)	(0.045)	(0.023)	(0.049)	(0.031)	(0.030)	(0.045)
MA	-0.032	-0.053	-0.029	-0.057**	-0.093**	-0.000	-0.128**
	(0.045)	(0.060)	(0.032)	(0.025)	(0.044)	(0.026)	(0.053)
Total MA Effect	-0.025	-0.136**	-0.012	-0.123***	-0.140**	-0.088**	-0.172**
	(0.056)	(0.065)	(0.042)	(0.042)	(0.060)	(0.034)	(0.065)
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

(Continued) Table A2: The Effect of PDMP on Ln Arrest Rates, Crime Categories

	FBI UCR Arrest Rate						
	Murder	Rape	Robbery	Assault	Burglary	Larceny	MV Theft
Panel D: Age 18-39, No trend							
PDMP	0.019	-0.096**	-0.000	-0.082*	-0.041	-0.067*	0.067
	(0.024)	(0.039)	(0.024)	(0.047)	(0.036)	(0.037)	(0.056)
MA	-0.034	0.026	-0.064**	-0.033	-0.038	0.048	-0.106
	(0.045)	(0.048)	(0.030)	(0.043)	(0.050)	(0.041)	(0.080)
Total MA Effect	-0.004	-0.066	-0.058	-0.115*	-0.076	-0.004	-0.029
	(0.044)	(0.064)	(0.043)	(0.067)	(0.072)	(0.057)	(0.081)
Panel E: 18-39, Treated State Trend							
PDMP	0.025	-0.081*	0.004	-0.066	-0.026	-0.046	0.090
	(0.024)	(0.041)	(0.025)	(0.052)	(0.034)	(0.038)	(0.056)
MA	-0.062	-0.048	-0.086***	-0.115****	-0.112*	-0.059	-0.226**
	(0.042)	(0.041)	(0.028)	(0.030)	(0.061)	(0.039)	(0.086)
Total MA Effect	-0.030	-0.125**	-0.075*	-0.178***	-0.135	-0.095**	-0.131
	(0.040)	(0.056)	(0.039)	(0.056)	(0.081)	(0.045)	(0.086)
Panel F:18-39, State Trend							
PDMP	0.014	-0.095**	0.023	-0.059	-0.038	-0.075**	-0.005
	(0.054)	(0.047)	(0.022)	(0.049)	(0.035)	(0.033)	(0.058)
MA	-0.048	-0.025	-0.042	-0.053*	-0.095*	-0.019	-0.169**
	(0.043)	(0.044)	(0.030)	(0.027)	(0.051)	(0.031)	(0.065)
Total MA Effect	-0.023	-0.122**	-0.014	-0.112**	-0.135*	-0.100**	-0.166**
	(0.061)	(0.056)	(0.039)	(0.043)	(0.069)	(0.037)	(0.076)
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

(Continued) Table A2: The Effect of PDMP on Ln Arrest Rates, Crime Categories

	FBI UCR Arrest Rate						
	Murder	Rape	Robbery	Assault	Burglary	Larceny	MV Theft
Panel G: Age Over 40, No Trend							
PDMP	0.021	-0.043	0.019	-0.085*	-0.013	-0.044	0.045
	(0.016)	(0.030)	(0.027)	(0.046)	(0.030)	(0.038)	(0.047)
MA	0.005	0.001	-0.055	-0.020	-0.010	0.069	-0.152*
	(0.024)	(0.048)	(0.034)	(0.043)	(0.045)	(0.046)	(0.079)
Total MA Effect	0.028	-0.049	-0.030	-0.100	-0.029	0.036	-0.091
	(0.029)	(0.061)	(0.054)	(0.068)	(0.064)	(0.061)	(0.076)
Panel H: Age Over 40 Treated State Trend							
PDMP	0.021	-0.025	0.025	-0.068	-0.011	-0.030	0.061
	(0.017)	(0.030)	(0.027)	(0.049)	(0.030)	(0.039)	(0.048)
MA	0.001	-0.090*	-0.086**	-0.109***	-0.022	-0.004	-0.233**
	(0.031)	(0.048)	(0.036)	(0.032)	(0.049)	(0.041)	(0.093)
Total MA Effect	0.027	-0.117*	-0.052	-0.172***	-0.037	-0.024	-0.159*
	(0.030)	(0.059)	(0.051)	(0.062)	(0.066)	(0.049)	(0.085)
Panel I: Age Over 40, State Trend							
PDMP	-0.027	-0.008	0.054	-0.070	-0.013	-0.064**	0.009
	(0.029)	(0.031)	(0.033)	(0.045)	(0.025)	(0.028)	(0.052)
MA	0.021	-0.081*	-0.074**	-0.064**	-0.033	0.003	-0.192***
	(0.033)	(0.044)	(0.036)	(0.026)	(0.041)	(0.031)	(0.069)
Total MA Effect	0.000	-0.085	-0.022	-0.133***	-0.052	-0.066*	-0.170**
	(0.031)	(0.054)	(0.056)	(0.046)	(0.053)	(0.036)	(0.065)
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

Notes: Models weighted by agency's population. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table A3: The Effect of PDMP on Drug Arrest Rates

	FBI UCR Arrest Rate								
	All Ages			Age 18-39			Age Over 40		
Panel A: Total									
PDMP	-0.123	-0.104	-0.045	-0.111	-0.088	-0.032	-0.167**	-0.158**	-0.071
	(0.073)	(0.078)	(0.064)	(0.076)	(0.080)	(0.064)	(0.063)	(0.067)	(0.062)
MA	-0.010	-0.107	-0.088	0.001	-0.115	-0.095	-0.034	-0.084	-0.065
	(0.076)	(0.111)	(0.083)	(0.077)	(0.114)	(0.085)	(0.066)	(0.090)	(0.073)
Total MA Effect	-0.102	-0.187	-0.142	-0.078	-0.178	-0.134	-0.183+	-0.226**	-0.143
	(0.112)	(0.128)	(0.107)	(0.113)	(0.132)	(0.110)	(0.098)	(0.105)	(0.098)
Panel B: Marijuana									
PDMP	-0.019	0.009	-0.040	-0.003	0.028	-0.016	-0.091	-0.065	-0.048
	(0.105)	(0.112)	(0.084)	(0.108)	(0.116)	(0.088)	(0.083)	(0.091)	(0.077)
MA	0.075	-0.068	0.036	0.109	-0.049	0.059	0.100	-0.033	0.057
	(0.111)	(0.144)	(0.120)	(0.113)	(0.147)	(0.125)	(0.090)	(0.125)	(0.108)
Total MA Effect	0.114	-0.014	-0.004	0.169	0.029	0.045	0.052	-0.062	0.014
	(0.172)	(0.183)	(0.164)	(0.178)	(0.187)	(0.172)	(0.136)	(0.150)	(0.148)
Panel C: Other Drug									
PDMP	-0.155*	-0.122	-0.145	-0.130	-0.095	-0.094	-0.197***	-0.175***	-0.172**
	(0.088)	(0.087)	(0.088)	(0.093)	(0.093)	(0.089)	(0.063)	(0.061)	(0.074)
MA	0.044	-0.124	-0.123	0.044	-0.139	-0.143	0.045	-0.065	-0.044
	(0.085)	(0.123)	(0.102)	(0.085)	(0.122)	(0.099)	(0.085)	(0.119)	(0.094)
Total MA Effect	-0.078	-0.208	-0.260*	-0.051	-0.192	-0.227	-0.130	-0.213*	-0.212
	(0.113)	(0.138)	(0.142)	(0.115)	(0.139)	(0.142)	(0.097)	(0.122)	(0.127)
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	N	Y	N	N	Y	N	N	Y
Treatment Trend	N	Y	N	N	Y	N	N	Y	N
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

(Continued) Table A3: The Effect of PDMP on Drug Arrest Rates

	FBI UCR Arrest Rate								
	All Ages			Age 18-39			Age Over 40		
Panel D: Heroin and Coke									
PDMP	-0.034	-0.052	-0.037	-0.036	-0.052	-0.019	-0.065	-0.080	-0.042
	(0.103)	(0.100)	(0.065)	(0.096)	(0.094)	(0.074)	(0.079)	(0.076)	(0.077)
MA	-0.135	-0.044	0.028	-0.081	0.005	0.052	-0.062	0.015	0.040
	(0.114)	(0.147)	(0.210)	(0.122)	(0.178)	(0.217)	(0.104)	(0.154)	(0.188)
Total MA Effect	-0.158	-0.075	-0.014	-0.106	-0.027	0.034	-0.130	-0.056	-0.001
	(0.166)	(0.193)	(0.226)	(0.176)	(0.217)	(0.251)	(0.145)	(0.185)	(0.221)
Panel D: Synthetic Drugs									
PDMP	0.041	0.058	0.058	0.012	0.023	0.068	-0.039	-0.027	0.060
	(0.116)	(0.114)	(0.098)	(0.115)	(0.114)	(0.093)	(0.079)	(0.081)	(0.077)
MA	-0.167	-0.254+	-0.142	-0.140	-0.197	-0.116	-0.130	-0.192+	-0.126
	(0.125)	(0.137)	(0.149)	(0.123)	(0.137)	(0.143)	(0.104)	(0.108)	(0.106)
Total MA Effect	-0.104	-0.172	-0.074	-0.115	-0.149	-0.034	-0.164	-0.202	-0.046
	(0.153)	(0.171)	(0.156)	(0.154)	(0.172)	(0.155)	(0.121)	(0.124)	(0.114)
Observations	51,353	51,353	51,353	51,353	51,353	51,353	51,354	51,354	51,354
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	N	Y	N	N	Y	N	N	Y
Treatment Trend	N	Y	N	N	Y	N	N	Y	N
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: Models weighted by agency's population. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table A4: The Effect of PDMP on Total, Violent and Property Crime (2003-2017)

	Modern, Fully Accessible PDMP Systems (Horwitz et al, 2018)								
	Crime Rates (2003-2017)			Cost-Adjusted Crime Rates (2003-2017)			Ln Arrest Rate (2003-2016)		
	Total	Violent	Property	Total	Violent	Property	Total	Violent	Property
Panel A: PDPAS, no trend									
PDMP Horwitz	-0.038***	-0.031**	-0.039**	-0.032**	-0.029	-0.037**	-0.034*	-0.031	-0.033
	(0.014)	(0.012)	(0.015)	(0.015)	(0.017)	(0.017)	(0.018)	(0.021)	(0.020)
Observations	115,892	115,891	115,895	115,882	115,871	115,895	51,513	51,513	51,513
Mean Pre-MA	3778	485.4	3294	633.3	586.9	46.51	753.7	206.5	547.1
Panel C: Treated State-Trend									
PDMP Horwitz	-0.038***	-0.030**	-0.039***	-0.031**	-0.028	-0.036**	-0.041***	-0.035*	-0.041**
	(0.013)	(0.012)	(0.015)	(0.015)	(0.017)	(0.017)	(0.015)	(0.020)	(0.016)
Observations	115,892	115,891	115,895	115,882	115,871	115,895	51,513	51,513	51,513
Mean Pre-MA	3778	485.4	3294	633.3	586.9	46.51	753.7	206.5	547.1
Panel B: PDPAS , trend									
PDMP Horwitz	-0.036***	-0.022**	-0.038***	-0.020	-0.016	-0.032**	-0.041***	-0.038***	-0.042***
	(0.012)	(0.008)	(0.013)	(0.012)	(0.014)	(0.014)	(0.013)	(0.013)	(0.015)
Observations	115,892	115,891	115,895	115,882	115,871	115,895	51,513	51,513	51,513
Mean Pre-MA	3778	485.4	3294	633.3	586.9	46.51	753.7	206.5	547.1
Agency FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trend	N	Y	N	N	Y	N	N	Y	N
Treatment Trend	N	N	Y	N	N	Y	N	N	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Models weighted by agency's population. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.10

Table A5: The Effect of PDMP on Crime Categories, FBI UCR Offenses Known and Arrests

	Modern, Fully Accessible PDMP Systems (Horwitz et al, 2018)						
	Homicide	**Rape**	**Robbery**	**Assault**	**Burglary**	**Larceny**	**MV Theft**
Panel A: Offenses Known, No Trend (2003-2017)							
PDMP Horwitz	-0.034**	-0.012	-0.028*	-0.039***	-0.033	-0.060**	-0.044*
	(0.013)	(0.046)	(0.017)	(0.014)	(0.023)	(0.027)	(0.025)
Observations	115,891	115,833	115,889	115,886	115,895	115,895	115,894
Mean Pre-MA	5.882	29.09	154.8	296	706.5	2222	365.4
Panel C: Offenses Known, Treatment Trend (2003-2017)							
PDMP Horwitz	-0.033**	-0.008	-0.027	-0.038***	-0.033	-0.061**	-0.0428*
	(0.013)	(0.045)	(0.016)	(0.014)	(0.023)	(0.026)	(0.024)
Observations	115,891	115,833	115,889	115,886	115,895	115,895	115,894
Mean Pre-MA	5.882	29.09	154.8	296	706.5	2222	365.4
Panel B: Offenses Known, State-Trend (2003-2017)							
PDMP Horwitz	-0.025**	-0.016	-0.019*	-0.035***	-0.024	-0.066**	-0.030*
	(0.011)	(0.044)	(0.011)	(0.011)	(0.023)	(0.027)	(0.016)
Observations	115,891	115,833	115,889	115,886	115,895	115,895	115,894
Mean Pre-MA	5.882	29.09	154.8	296	706.5	2222	365.4
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

(Continued) Table A5: The Effect of PDMP on Crime Categories, FBI UCR Offenses Known and Arrests

	Modern, Fully Accessible PDMP Systems (Horwitz et al, 2018)						
	Homicide	**Rape**	**Robbery**	**Assault**	**Burglary**	**Larceny**	**MV Theft**
Panel D: Arrests, No Trend (2003-2016)							
PDMP Horwitz	0.005	-0.028	0.013	-0.053**	0.011	-0.037*	-0.057
	(0.018)	(0.035)	(0.021)	(0.025)	(0.028)	(0.022)	(0.042)
Observations	51,513	51,513	51,513	51,513	51,513	51,513	51,513
Mean Pre-MA	4.099	8.171	42.17	152.1	98.18	410.8	38.11
Panel F: Arrests, Treatment Trend (2003-2016)							
PDMP Horwitz	0.005	-0.034	0.013	-0.059*	0.007	-0.046***	-0.063
	(0.018)	(0.035)	(0.020)	(0.025)	(0.026)	(0.017)	(0.042)
Observations	51,513	51,513	51,513	51,513	51,513	51,513	51,513
Mean Pre-MA	4.099	8.171	42.17	152.1	98.18	410.8	38.11
Panel E: Arrests, State-Trend (2003-2016)							
PDMP Horwitz	-0.006	-0.065**	0.011	-0.063****	-0.005	-0.044**	-0.124****
	(0.020)	(0.025)	(0.025)	(0.016)	(0.029)	(0.017)	(0.028)
Observations	51,513	51,513	51,513	51,513	51,513	51,513	51,513
Mean Pre-MA	4.099	8.171	42.17	152.1	98.18	410.8	38.11
Agency FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Weight Agency Pop	Y	Y	Y	Y	Y	Y	Y
Cluster State	Y	Y	Y	Y	Y	Y	Y

Notes: Models weighted by agency's population. Standard errors are clustered by state, and reported in parentheses. All models control for demographic factors (% minors, % age 18-25, %males age 18-25, %males), drug and alcohol policies (ID laws, PER laws, Naloxone laws, Good Samaritan laws, marijuana decriminalization, marijuana legalization, medical marijuana laws, BAC laws, beer taxes), police composition (ln officers per 100,000 residents) and other socioeconomic variables (income per capita, unemployment rate, poverty rate and share of residents that have a college degree, some college, high school, and less than high school). **** p-value≤0.001, *** p-value≤0.01, * *p-value≤0.05, *p-value<0.1