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ABSTRACT

This article presents estimates of the effects of state prescription opioid policies on prescription opioid sales, mortality and socioeconomic outcomes of adults. Our analysis highlights that most prescription opioid use is medically prescribed and that curtailing such use may have adverse effects on wellbeing. We also emphasize that there are significant differences in prescription opioid use and mis-use across demographic groups that may cause state policies to have heterogeneous effects. Results indicate that state policies reduced prescription opioid sales by between 5% and 20% depending on the policy and type of prescription opioid. State "pill mill" laws have been particularly effective at reducing prescription opioid sales. The reductions in prescription opioid sales associated with state policies, however, were not associated with significant changes in mortality or socioeconomic outcomes.

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Introduction

The opioid "epidemic" is one of the most pressing public health issues for local, state and federal policymakers and its consequences have been widely documented. The "epidemic" is often characterized by the rise of prescription opioid use. Between 1992 and 2011, the number of opioid prescriptions in the U.S. increased nearly three-fold from approximately 75 million annually to 220 million annually (Manchikanti et al. 2017). At its peak in 2010-2012, the Opioid prescription rate was 80 per 100 persons in the U.S., although only about 20% of the population had one or more prescriptions (CDC 2017). Since 2010, the opioid prescription rate declined to 59 per 100 persons in 2017 (CDC 2018). The second prominent fact used to characterize the opioid "epidemic" is the rise in prescription opioid-related mortality. Between 1999 and 2010, the rate of prescription opioid overdose deaths increased from just over 1 per 100,000 to just over 5 per 100,000 and remained at around 5 per 100,000 through 2016 (CDC 2018). Finally, the rise in non-prescription opioid (e.g., heroin and fentanyl) deaths are also often included to document the "epidemic". The rate of non-prescription opioid (heroin and fentanyl combined) deaths increased from approximately 1 per 100,000 in 1999 to 2 per 100,000 in 2010. After this date, non-prescription opioid deaths began to increase markedly rising to over 10 per 100,000 by 2016 (CDC 2018).

While the sheer magnitude of opioid prescriptions and the mortality consequences of the opioid "epidemic" have garnered most of the research and public policy attention, the rise in prescription opioid use may have had other consequences. For example, there have been a few studies of the effect of opioid use on employment, although evidence from these few studies remains mixed (Currie et al. 2018; Krueger 2017; Harris et al. 2017). Other outcomes that may be plausibly affected by opioid use, both medical and non-medical use, include marriage, earnings and health (Duenas et al. 2016; Gustavsson et al. 2012; Turk et al. 2016). There have been no studies of the effect of prescription opioid use on these outcomes. In this article, we add to this limited literature. We exploit plausibly exogenous variation in prescription opioid use caused by states' adoption of Prescription Drug Monitoring Programs (PDMPs) and "pill mill" statutes. We show that the adoption of PDMPs, particularly PDMP's that have been characterized as "modern", and the adoption of "pill mill" legislation reduced prescription opioid sales by between 5% to 20%, although there is some heterogeneity of estimates that we describe later. This evidence is consistent with several prior studies.[1] The variation in prescription opioid sales, and presumably opioid use, caused by the state policies provides exogenous variation in prescription opioid use that we use to assess the effects of prescription opioid use on socioeconomic outcomes. We also estimate the effect of states policies on mortality.

[1] See Finley et al. (2017) for a review. The evidence of the effect of PDMPs on opioid prescriptions is somewhat mixed. We present evidence below on their effectiveness and review other studies that show similar findings.

An important contribution of our analysis is the stratification of the sample by age and gender. This stratification is motivated by evidence suggesting that most prescription opioid use is medical and that rates of non-medical use, and the ratio of non-medical to medical use of prescription opioids differs significantly by age, gender and to a lesser extent education (see Table 1). For example, approximately 16% of females ages 50 to 64 had a prescription opioid in 2002-2006, but only 2% reported non-medical use, and about 1% reported heroin use in the past year. These figures suggest that this group of females has a relatively high rate of prescription opioid use that is almost all medically prescribed. There is little purposeful misuse of prescription opioids, or use of illegal opioids, among this demographic group. In contrast, among men ages 26 to 34, only 9% had a medical prescription for opioids in 2002-2006, but 9% also reported non-medical use, and 3% reported past year heroin use. For this group, much of prescription opioid use is misuse and this group has a relatively high rate of illegal drug use. Given these differences in opioid use, it is likely that that changes in prescription opioid use due to state policies had different effects on mortality and socioeconomic outcomes of these demographic groups. We develop this point in more detail below.

Results of our analysis indicate that state implementation of a “modern” PDMP is associated with modest decreases in opioid sales of between 5% and 10%, although estimates are not always statistically significant. Pill mill laws are more strongly associated with decreased opioid sales; adoption of such statutes is associated with a decrease in opioid sales of between 10% and 20% and estimates are highly significant. We also showed that the effects of these two states policies are larger in urban areas.

The reductions in prescription opioid sales associated with adoption of a “modern” PDMP and a “pill mill” law were not associated with moderate to large effects on mortality or socioeconomic outcomes. There was limited evidence that “pill mill” laws reduced drug-related mortality among young males, which is consistent with this group having the highest rates of prescription opioid mis-use. However, estimates were not statistically significant though large (25%). We also found that adoption of a “modern” PDMP decreased earnings (2% to 5%) and the adoption of a “pill mill” law increased earnings (2% to 6%) among young persons (ages 18 to 34), but the statistical significance of these estimates was marginal. Overall, while state policies have had a significant effect on prescription opioid sales, the impact of this decline in opioid prescriptions and these policies on mortality and socioeconomic outcomes has been insignificant. We discuss possible explanations of this finding in our conclusion.

State Responses to Opioid Epidemic

Many states have responded to the opioid epidemic by enacting a variety of laws and policies related to controlling and monitoring opioid prescribing behavior. The most prominent state response has been the enactment, refinement and strengthening of Prescription Drug Monitoring Programs (PDMPs).

PDMPs are widely seen as one of the most effective policies to deter opioid abuse.[2] While PDMPs have been in existence for many years, with California establishing the first in 1939, there has been substantial activity in recent years to bolster the effectiveness of PDMPs.[3] Between 2000 and 2010, 25 PDMPs were established. In addition, newly established PDMPs and upgrades to existing PDMPs differ from earlier PDMPs in that they are fully electronic, more accessible to physicians, pharmacists and other pertinent parties, and often include requirements for mandatory use.

To characterize state prescription opioid policies, we reviewed the range of policies and dates of implementation used in prior studies with particular attention to information in Horwitz et al. (2018). Our goal was to accurately identify the timing of implementation of a policy and to classify in a parsimonious way main elements of state opioid policies. Based on our review, we chose six measures: five mutually exclusive categories of PDMPs and an indicator for whether a state had a "pill mill" law. The reference category for PDMP classification was a state with no PDMP. The most basic PDMP category identifies the date PDMP legislation was enacted. If enactment was contingent on the availability of funding, we used the date funding became available as the implementation date (Horwitz el al 2018). The next PDMP category identifies the date that an electronic PDMP was implemented. If the original PDMP that was enacted was also electronic, the former date is used. Electronic systems are not paper-based and allow the prescriber to transmit the prescription information electronically to the state authority (Manchikanti, Brown and Singh 2002). When the PDMP became accessible to any authorized user (e.g., physician, pharmacist, or member of law enforcement), we classified the PDMP as "modern" and used the date the modern PDMP was implemented. If the state implemented an electronic and modern system at same time then we used that date to classify the state as having a "modern" PDMP. The last category of PDMP represents the month and year that querying the (modern) PDMP database became mandated (Buchmueller and Carey 2018). A mandated PDMP requires prescribers to check the state medication history database before prescribing controlled substances. In choosing the implementation dates for each PDMP category we verified the dates and followed Horwitz et al. (2018). If the date of implementation is mid-year, we use fractional time periods. For example, a state that has an electronic PDMP for half the year and a "modern" PDMP for half the year, a value 0.5 was assigned to each of these categories in that year.

Despite the prominence of PDMPS, states have also taken other steps to control prescription opioid use. One of the most important of these policies is regulations on "pill mills" (pain management clinics).

[2] https://www.cdc.gov/drugoverdose/pdmp/states.html, last accessed May 14, 2019.
[3] http://www.pdmpassist.org/pdf/state_survey_comparisons_TAG_final_20161214_revised.pdf, last accessed May 14, 2019.

Pill Mill laws target prescribers who account for a disproportionate share of opioid prescribing. Pill Mill laws include legal provisions establishing state inspection authority or specific training requirements for Pill Mill owners or associated physicians[4]. These laws are associated with a decrease in the number of pain management clinics (Gau et al 2017). In choosing the dates that best reflect when Pill Mill laws were activated, we followed Buchmueller and Carey (2018) and Malllatt (2017).[5]

Figure 1 (and Appendix Figure 1) shows changes over time in state PDMPs using a classification of PDMPs that we have adopted. As Figure 1 shows, there is variation over time within states in both the extensive margin, reflected in the creation of electronic PDMPs, and at the intensive margin, reflected in significant changes in the structure of PDMPs as reflected in the growth of what we refer to as "modern" PDMPs. Appendix Figure 1 shows similar information, but for an expanded classification of PDMPs.

Evidence of the Effectiveness of Prescription Drug Monitoring Programs

As noted, PDMPs are widely viewed as an effective tool to combat opioid abuse. Supporting this view is evidence from an existing literature. A recent, comprehensive literature review by Weiner et al. (2017) concluded that PDMPs have effectively reduced opioid prescribing emphasizing the point that it is particular features of a PDMP, such as mandatory use and greater integration of the PDMP into electronic health records that are particular effective.

There are several, quasi-experimental studies of the effect of PDMPs on opioid prescriptions.[6] Bao et al. (2016) is a good example. It examined the effect of PDMPs on physician prescribing behavior using data from the National Ambulatory Medical Care Survey from 2001 to 2010 and a difference-in-differences (pre- and post-test with comparison group) research design. This study exploited the significant increase in state PDMPs during this period (see Figure 1). It found that the implementation of a PDMP was associated with a 33% decline in opioid prescriptions. Dowell et al. (2016) found similar

[4] https://www.cdc.gov/phlp/docs/menu-pmcr.pdf contains a description of the scope of various Pill Mill laws across states.

[5] There are other state policies that we do not to include in our analysis (e.g., ID laws and quantity limits). To the extent PDMPs and "pill mill" laws are coincident with these other policies, then estimates of the effect of PDMPs and "pill mill" laws will include the effect of these programs. However, since these laws also are intended to reduce prescription opioid use, the estimates we obtain below on the effects of these state policies on mortality and socioeconomic outcomes still reflect the effect of decrease in prescription opioid use.

[6] We focus on quasi-experimental studies. We do not review other types of studies because of the weak causal analysis frameworks. We describe two of the more comprehensive, observational studies in this note. Brady (2014) conducted a national study of effects of PDMPs on opioid prescriptions from 1999 to 2008 and found no statistically significant effect. However, this study did not differentiate between PDMP types and did not include state fixed effects. Reisman et al. (2009) reported results from a time-series comparison between states that had PDMPs and states that did not have PDMPs during the period between 1997 and 2003. PDMPs states experienced slower growth in oxycodone and hydrocodone sales.

results using a slightly later period of analysis, 2006 to 2013, and data from the IMS National Prescription Audit, which tracks prescriptions dispensed by pharmacies.

Rutkow et al. (2015) conducted a case study of the implementation of a PDMP in Florida in 2011. The authors examined pre-to-post PDMP changes in the prescribing behavior and opioid use of a closed panel (i.e., no compositional change) of physicians, pharmacies and patients. The authors used Georgia as a comparison. Results from the study indicated that Florida's PDMP decreased opioid prescriptions by between 2% and 6% within 12 months.

Two studies of PDMPs used Medicare data and samples of elderly: Moyo et al. (2017) and Buchmueller and Carey (2017). These studies used a difference-in-differences research design and data between 2007 and 2012-13. Buchmueller and Carey reported that must-access PDMPs were associated with modest (2% to 3%) reductions in prescription opioid use. Moyo et al. (2017) reported mixed evidence, but found that the total quantity (in weight) of opioid prescription declined by approximately 5%, but that other measures of prescription opioid use did not decrease. An important finding in Buchmueller and Carey (2017) is that it is mainly the required use mandate of a PDMP that causes the decline in opioid use. Patrick et al. (2016), Birk and Waddell (2017) and Grecu et al. (2019) also emphasize the importance of focusing on specific aspects of PDMPs. All three of these studies reported that PDMPs are associated with fewer serious opioid-related incidents such as treatment admissions and mortality.

The upshot of this brief review is that there is significant evidence that PDMPs have reduced opioid prescriptions. We provide additional evidence of the effectiveness of PDMPs below. From a research point of view, the within-state, time variation in the creation (extensive margin) and design (intensive margin) of PDMPs, and their apparent effectiveness, facilitates the use of quasi-experimental methods to study the consequences of PDMPs on the use of prescription opioids, mortality, and on socioeconomic outcomes plausibly affected by prescription opioids. This is the overarching objective of this article.

Conceptual Model: Opioid Use versus Opioid Abuse and Implications

A distinguishing feature of prescription opioid use is that it has both therapeutic and consumption value (euphoria[7]). The therapeutic use of prescription opioids is primarily for pain relief, which if untreated can lead to anxiety, depression, functional limitations and increased health care costs (Fishbain et al. 1986; American Geriatric Association 2002; Bair et al. 2003; Chou et al. 2011). The therapeutic use of prescription opioids is generally health improving. The use of prescription opioids for its consumption (euphoria) value is plausibly health decreasing, particularly if it leads to addiction and heavy use. These

[7] We use the term "euphoria" coined by Stigler and Becker (1977) to describe the good associated with use of opioids for pleasure.

fundamentally different uses of prescription opioids need to be incorporated into the conceptual model, particularly given our interest in assessing the effect of changes in prescription opioid use on mortality and socioeconomic outcomes. A second aspect of prescription opioids is that there are close substitutes for its use, particularly its use as a consumption good (i.e., in the production of euphoria). Heroin and synthetic opioids are often used to achieve the same euphoric feeling, as non-medical use of prescription opioids. There are also substitutes for the therapeutic use of opioids, such as acetaminophen. Given the availability of close substitutes, changes in use of prescription opioids will cause changes, perhaps large, in the use of these substitutes.

In this article, we examine the effect of changes in prescription opioid use brought forth by state prescription opioid control polices, specifically PDMPs and pill mill statutes. As we show, and others have shown, adoption of PDMPs and pill mill statutes have caused a decrease in prescription opioids. This decrease in prescription opioid use will plausibly affect health and socioeconomic outcomes. However, the effects of a decrease in prescription opioid use on health and socioeconomic outcomes are ambiguous, and depends importantly on the likelihood that the consumer is using prescription opioids for therapeutic purposes, as consumption, or for both therapeutic and consumption purposes.

Consider the person whose only use of prescription opioids is for medical purposes. A policy-induced decrease in prescription opioids for a person who only uses prescription opioids for therapeutic purposes is likely to adversely affect health. This person will have less access to pain relief and less pain relief can have potentially serious effects on health, as noted earlier. In turn, worse health, for example, as manifested in anxiety and depression, can have effects on socioeconomic outcomes, such as labor market outcomes. If, however, the policy-induced decrease in prescription opioids affects primarily opioid use that had relatively low-health benefits, for example, because of moral hazard associated with health insurance, then the adverse health consequences of the policy-induced decrease in opioids may be less substantial. The use of therapeutic substitutes may also lessen the consequences of decreased access to prescription opioids.

Now consider a person whose only use of prescription opioids is for consumption (non-medical) purposes. For this person, a policy-induced decrease in prescription opioids will improve health all else equal. However, all else is not equal because of the availability of close substitutes for non-medical use of prescription opioids. The decrease in prescription opioid use that was for consumption (i.e., mis-use) will increase the use of substitutes, such as heroin, that have arguably more serious adverse health effects because of the uncertain quality (adulteration) of these substitutes (e.g., heroin). Therefore, for this person health may improve or worsen. And while the decrease in mis-use of prescription opioids may decrease opioid treatment admissions and prescription opioid use disorder, the increase in use of illegal opioids will offset these effects. Evidence presented in Grecu et al. (2019) and Evans et al. (2019) is consistent with this argument. Grecu et al. (2019) showed that PDMPs decreased admissions for treatment of opioid use disorder and Evans et al. (2019) reported that decreased access to easy-to-abuse oxycodone increased heroin (fentanyl)-related mortality.

The last case is for the person who uses prescription opioids for therapeutic reasons and as consumption. For this person, a policy-induced decrease in prescription opioids will have ambiguous effects on health and socioeconomic outcomes.

To summarize, the conceptual model predicts different effects of a policy-induced decrease in prescription opioids depending on whether a person uses prescriptions opioids for therapeutic purposes, consumption, or both. For those whose only use is therapeutic, health and health-related outcomes may worsen.[8] For others, health may improve or worsen. Ideally, our empirical analyses would stratify by these categories of prescription opioid users. However, there is little data at the individual level that has this information as well as data on health and socioeconomic outcomes.

The figures in Table 1 provide some insight into this issue. Table 1 shows means of prescription opioid use, mis-use of prescription opioids, and use of heroin by demographic groups. There are marked differences. Younger people have relatively low rates of prescription opioid use and relatively high rates of mis-use of prescription opioids. Older people have relatively high rates of prescription opioid use and relatively low rates of mis-use of prescription opioids. Men tend to have higher rates of mis-use of prescription opioids than women and women have higher rates of prescription opioid use than men. Surprisingly, there is not a strong gradient in prescription opioid use and mis-use by education, although more educated persons reported less prescription opioid use and less mis-use of prescription opioids.

Given the differences in the types of use of prescription opioids by these demographic groups, we may expect a policy-induced decrease in prescription opioids to have different effects across these groups. All else equal, we expect the health of older persons, particularly women, to be adversely affected by the policy change. Of course, if the main effect of the policy was to decrease low-value prescription opioid use and to increase use of substitutes, then health may remain relatively unchanged for these groups. If health does worsen, we may see subsequent changes in socioeconomic outcomes such as employment. On the other hand, the policy-induced decrease in prescription opioids may decrease mis-use of prescription opioids among young persons, particularly men, and increase use of substitutes such as heroin. It is likely, however, that the net effect will be an improvement in health because only a small fraction of young people (men) who misuse prescription opioids use heroin and, while there may be substitution, it is

[8] Mortality may decrease for this group because of fewer accidental overdoses among those using prescription opioids for therapeutic reasons.

unlikely that it will be sufficient to offset the decrease in mis-use of prescription drugs.[9] An improvement in health will be likely to have beneficial effects on socioeconomic outcomes, such as employment and earnings. Some evidence that this prediction is likely to hold is presented in Grecu et al. (2019) who found that PDMPs reduced admissions for treatment of prescription opioid use, and that there was a strong age gradient in the effect of PDMPs on admissions for treatment.

To summarize, our hypothesis is that a policy-induced decrease in prescription opioids, for example, from states' adoption of PDMPs, will adversely affect health of demographic groups (e.g., older females) that have high rates of medically prescribed opioids. In turn, this decrease in health will adversely affect socioeconomic outcomes. For groups that have high rates of mis-use of prescription opioids, a policy-induced increase in prescription opioids will be health improving and the improvement in health will have subsequent salutary effects. An important caveat to these hypotheses is that they assume that PDMPs have similar effects on prescription opioid use (i.e., the first stage) across demographic groups. This may not be the case. If not, then the predictions described above may not hold. For example, PDMPs may decrease prescription opioid use the most among those who have high rates of medically prescribed opioid use, such as older women. If this is the case, then the predictions noted above are plausible. In contrast, among young men who have high rates of non-medical use of prescription opioids and who presumably obtain prescription opioids through diversion and in the black market, PDMPs may have less effect on their use of prescription opioids. If so, then PDMPs may have little effect on the health and socioeconomic outcomes of this group. The bottom line is that the reduced form effect we estimate consists of the effect of PDMPs on prescription opioid use and the effect of prescription opioid use on outcomes. The demographic differences observed in Table 1 and our hypotheses stemming from those differences focus on the second relationship—the effect of prescription opioids on health and socioeconomic outcomes.

Research Design

We aim to answer several research questions. First, we obtain estimates of the effect of PDMPs on prescription opioid sales. To accomplish this objective, we use data on retails sales of prescription opioids available from the Drug Enforcement Agency (DEA) of the Department of Justice. The DEA information is from the Automated Reports and Consolidated Ordering System (ARCOS).[10] We describe these data in detail below, but briefly, the DEA collects and makes publicly available information about the number of

[9] Note that mortality may rise, as Evans et al. (2019) find. But mortality is a limited measure of health and is most related to abuse. The conceptual model does not rule this out. We argue that, on average, health will be improved because substitution is likely to be much less than one-for-one.
[10] See: https://www.deadiversion.usdoj.gov/arcos/retail_drug_summary/, last accessed May 2, 2019

retail sales of prescription opioids at the 3-digit zip code and state levels. To these data, we merge information on state PDMPs by state and year.

Using the ARCOS data at the state level, we estimate the following difference-in-difference regression model:

$$OPIOIDS_{jt} = \alpha_j + \delta_t + \sum_{k=1}^{3} \beta_k PDMP_{kjt} + \sum_{m=1}^{M} \lambda_m AGE_{mjt} + e_{jt}$$

$$(1) \quad j = 1, \ldots, 51$$
$$t = 2002, \ldots, 2016$$

In equation (1), the dependent variable is the quantity of retail prescription opioid sales (*OPIOIDS*) in state j and year t. The amount of prescription use depends on state fixed effects (α_j), year fixed effects (δ_t), and three policy indicators for whether the state has an electronic PDMP, a "modern" PDMP and a "pill mill" statute. We described the rationale for our classification of state policies earlier. Electronic PDMP and modern PDMP are mutually exclusive categories and the reference category are states with no PDMP, or no electronic PDMP. Prescription opioid sales in the state are measured in terms of milligram morphine equivalents, and we use two different measurements: log total milligram morphine equivalent grams (MEG) or per-capita MEG grams. To construct the per-capita measure, we use the state population. In models that use log total MEG equivalents, we include the log of state population in 5-year age categories (*AGE*) as explanatory variables. In models that use the per-capita MEG, we include the share of state population in 5-year age categories. We use the 5-year age categories to allow the effect of population to have different associations with prescription opioid use, which is consistent with the different rates of prescription opioid use by age. We also estimate equation (1) using only the combined sales for two of the most frequent opioid prescriptions: oxycodone and hydrocodone.

To explore whether there are heterogeneous effects of PDMPs on prescription opioid sales, we stratify the sample by urban-rural status. We use U.S. Census definitions to identify counties that are mostly urban, mostly rural and rural (Ratcliffe et al. 2016). We then aggregate these counties to form state-level aggregates. This urban-rural stratification is motivated by media reports that rural areas have been particularly affected by the opioid epidemic and documented high rates of opioid-related deaths in some rural states such as Kentucky, Maine, West Virginia and New Mexico, although there are relatively low rates of opioid deaths in other rural states such as the plains states.[11]

Our second objective is to obtain estimates of the effects of state policies on mortality and socioeconomic outcomes. Foreshadowing our results, we find that states policies (PDMPs and pill mill statutes) decrease opioid prescription sales by between 5% and 20% depending on the policy (e.g., pill

[11] See https://www.drugabuse.gov/drugs-abuse/opioids/opioid-summaries-by-state, website last accessed May 14, 2019. Also, ARCOS data does not indicate that rural states have high per-capita opioid prescription sales.

mill) and type of prescription (e.g., oxycodone and hydrocodone). This evidence motivates the analysis of state policies on mortality and socioeconomic outcomes. The outcomes we analyze are death rates, overall and for drug-related deaths, and the following socioeconomic outcomes: employment, weeks worked per year, personal earnings, receipt of social welfare cash benefits, marital status and a measure of disability. The information on mortality comes from vital statistics on deaths collected by the National Center for Health Statistics (NCHS) and for socioeconomic variables we use individual-level data from American Community Surveys (ACS). We describe these data in more detail below.

For the analyses of mortality and socioeconomic outcomes, we obtain separate estimates for eight demographic groups stratified by age (18-25, 26-34, 35-49, 50-64) and gender. This stratification is motivated by the evidence in Table 1 that shows significant differences in prescription opioid use and mis-use by these groups. The regression models used to analyze these outcomes are:

$$(2)\quad MORTALITY_{jt} = \tilde{\alpha}_j + \tilde{\delta}_t + \sum_{k=1}^{3} \tilde{\beta}_k PDMP_{kj(t-1)} + u_{jt}$$

$$(3)\quad SOCIOECONOMIC_{ijt} = \breve{\alpha}_j + \breve{\delta}_t + \sum_{k=1}^{3} \breve{\beta}_k PDMP_{kj(t-1)} + \breve{\lambda} DEMOG_{ijt} + v_{ijt}$$

There are a couple of differences between equation (1), and equations (2) and (3) that merit mention. First, we do not include the population, or population share, variables in equations (2) and (3). For mortality, the data are aggregated by age and gender as indicated, and the basic model includes just state and year fixed effects. The NCHS mortality information is not reported for groups with less than 10 deaths per year, and so disaggregating further, for example, by each year of age and including year of age dummy variables in equation (2), would lead to a substantial number of missing cells.[12] For socioeconomic outcomes, we include dummy variable indicators of each year of age, and race/ethnicity (non-Hispanic black, non-Hispanic white, non-Hispanic other, and Hispanic). Second, we lag the state policy indicators by one year. We expect some time to elapse between the time state policies are adopted, prescription opioid use decreases, and health and socioeconomic outcomes are realized.

We also obtain estimates of equations (2) and (3) stratified by urban-rural status. For the mortality analysis, we use the NCHS definitions of urban-rural that are used to report deaths.[13] In the ACS, we use

[12] To assess whether adding more detailed demographic controls mattered, we re-estimated equation (2) using data disaggregated by year of age and race/ethnicity. We included year of age dummy variables and dummy variables for race/ethnicity. Estimates, which are available from authors, are very similar to those reported in Table 4. This is not surprising because there is very little variation over time in the age and race/ethnicity distributions with the broader demographic cells used in the analysis of Table 4.

[13] The 2013 NCHS definitions are: large central metro counties in MSA of 1 million population; large fringe metro counties in MSA of 1 million or more population that do not qualify as large central Medium metro counties in MSA of 250,000-999,999 population.; small metro counties are counties in MSAs of less than 250,000 population;

the Census definition of metropolitan and non-metropolitan areas, although this variable is only available in 2006 onward. As we show below, the effect of state policies on prescription opioid sales differs across urban and rural places. This evidence supports the stratifications of the sample. However, we emphasize that the reduced form effects reflect differences in the effects of state policies on prescription opioid sales and the effect of prescription opioid use on outcomes. Therefore, it is not necessarily the case that reduced form estimates will align with the "first stage" estimates of the effect of state policies on prescription opioid sales.

Threats to Validity

The difference-in-differences research design represented by equations (1) through (3) is valid under the assumption that, in the absence of the adoption of PDMPs and pill mill statutes, the pre-to-post policy changes in opioid sales, mortality and socioeconomic outcomes would be the same. To assess the likely validity of this assumption, we do the following. First, we estimate a model that examines whether there are differences in trends in opioid sales, mortality and socioeconomic outcomes in periods prior to the adoption of state policies. This model is sometimes referred to as an "event-study" specification because it tracks the difference in outcomes in periods up to and post the event, which in this case is the adoption of state policies. Evidence of a valid design is that there is no divergence between the trends in outcomes (opioid sales, mortality, socioeconomic outcomes) in periods prior to the adoption of state policies. We report these results later in the article, but note here that evidence from this analysis is strongly consistent with a valid design.

Second, we include additional control variables that adjust for potentially different trends in opioid sales, mortality and socioeconomic outcomes that are related to differences in baseline opioid sales. The intuition of this approach is that if states have different baseline levels of opioid sales, then they may have different trends in opioid sales (and in turn mortality and socioeconomic outcomes). Further, if the different level of baseline sales, or different trends, in opioid sales are correlated with states' adoption of policies, adding these controls will adjust for this possibility. Specifically, we construct a set of dummy variables indicating the level of baseline sales of opioids in a state. We created six categories (sextiles). We interact these dummy variables with year dummy variables to allow for different year effects for different levels of baseline opioid sales.[14]

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and nonmetropolitan counties. We aggregate counties into urban and rural where rural counties are those classified as nonmetropolitan by NCHS and all others are included in urban.
https://www.cdc.gov/nchs/data_access/urban_rural.htm#2013_Urban-Rural_Classification_Scheme_for_Counties, website last accessed May 14, 2019.

[14] We define these six categories for each sample. For state analysis (e.g., Table 2 below), we use baseline opioid sales in the state to define the categories. For urban-rural samples, we define the six categories using baseline opioid sales in the counties included in the analysis, for example, urban counties.

Finally, we add controls for state-specific, linear time trends to the model. These variables allow for differential time trends for each state, but constrain that trend to be linear. While there is some concern that the inclusion of such trends may be "over fitting" and partly measuring the treatment effect if treatment effects are time-varying, the inclusion of such trends remains useful as an assessment of the potential for there to be an omitted variable problem.[15]

We report the results obtained from these augmented regression models below, but note here that the evidence from these analyses generally supports the validity of the difference-in-differences research design.

Stratification by Age and Gender

The conceptual model and figures in Table 1 suggest that the effects of state policies to control prescription opioid use will differ by age and gender, and less so by education. Ideally, we would like to stratify all analyses on these characteristics. However, the ARCOS information on prescription opioid sales does not have information on opioid sales by demographic characteristics, but only by geography and type of prescription (e.g., oxycodone). The NCHS and ACS surveys provide demographic characteristics, and for the analyses of the effect of state policies on mortality and socioeconomic outcomes, we obtain estimates for different demographic groups stratified by age and gender, and in some analyses that use the ACS by age, gender and education. Specifically, we divide the sample into eight groups using age (18-25, 26-34, 35-49, 50-64) and gender. These stratifications are consistent with figures in Table 1 that show different levels of use and mis-use of prescription opioids.

Data

We used several data sources in our analyses. To estimate the effects of state opioid prescription laws on opioid prescribing, we used data on prescription opioid sales from the DEA's Automation of Reports and Consolidated Orders System (ARCOS). We combined these data with information gleaned from the literature on the timing and nature of state prescription opioid policies, which we have previously described. For the analysis of the effect of state policies on mortality, we used data from the National Center for Health Statistics (NCHS) on deaths and causes of death. Again, we combined these data with the information on state opioid policies. To estimate the effect of state policies on socioeconomic outcomes, we used data from the American Community Survey (ACS), which reports individual-level

[15] See Goodman-Bacon (2019).

information about demographic characteristics and socioeconomic outcomes. All analyses spanned the period 2002-2016.

Prescription Opioid Sales

The Controlled Substance Act of 1970 requires all manufacturers and distributors to report their transactions and deliveries of all Scheduled II-V substances to the Attorney General. The data system to accommodate the Controlled Substance Act reporting requirement is the DEA's Automation of Reports and Consolidated Orders System (ARCOS). ARCOS data are publicly available and we used data from 2002 to 2016. We did not use earlier years because of potential reporting problems. For example, in 2000 only two opioids were reported in ARCOS—Hydrocodone and Oxycodone—and in 2001 California had a huge discrepancy in the total opioid grams reported vis-à-vis 2001.

ARCOS reports total grams of retail prescription opioids sales per quarter per drug (i.e., active ingredient) at the 3-digit zip code level. ARCOS reports sales of all schedule II-V substances. We focus on schedule II drugs, which include almost all prescription opioids. In our analysis, we use the top 14 most frequently retailed schedule II opioids (Codeine, Dihydrocodeine, Hydrocodone, Hydromorphone, levorphanol, Meperidine pethidine, Morphine, Oxycodone, Oxymorphone, Opium Powdered, Alfentanil, Remifentanil, Sufentanil base, Tapentadol).[16] Notably, we exclude Fentanyl, Methadone and Buprenorphine. We exclude these drugs because Methadone and Buprenorphine are prescribed for the management of opioid-dependent individuals, and are therefore fundamentally different clinically than the other prescription opioids. We exclude Fentanyl because it is sold primarily in patch and pill form. The Fentanyl patch is more potent than the pill and has a different absorption mechanism. We are unable to distinguish the type of Fentanyl in the ARCOS data and to convert it to a common dosage unit (see below).

ARCOS reports quarterly drug grams at the 3- digit zip code level (known as Report 1 in ARCOS). We used these data because we wanted to construct sub-state measures of opioid sales for analyses stratified by urban-rural status. To generate county level data, we aggregated the 3-digit zip code level data to the county level. Specifically, to convert to counties, the 3- digit zip codes were first converted to five-digit zip codes by distributing the share of opioid sales across the appropriate zip codes based on population proportions. This assumes that the distribution of prescription opioids follows the same distribution as the population. Zip codes were then converted to counties using a zip code-county crosswalk provided by the Department of Housing and Urban Development. To check our calculation, we then aggregated the county data to the state and compared it to AROCS data reported at state level

[16] The 14 opioids we selected represent over 99% of all opioids other than Fentanyl, Methadone and Buprenorphine.

(ARCOS 2). Our estimates constructed from ARCOS 1 reports matched nearly perfectly to the estimates in ARCOS 2.

There are some limitations of ARCOS. The data over-represent the amounts of prescription opioids that are distributed for human consumption because they include prescriptions used for veterinary purposes. Additionally, these data may over-represent amounts dispensed or consumed by patients because they include amounts re-ordered to replace drugs stolen from pharmacies or other retail-level dispensers, and amounts distributed to the retail level that were not actually dispensed or consumed by patients in the same year.

Mortality Data

Mortality data come from the Centers for Disease Control WONDER database, which is based on vital statistics system maintained by the National Center for Health Statistics (NCHS) for years 2002 to 2016. The system provides the overall and cause-specific annual mortality rates (per 100,000) by state and a limited number of demographic characteristics (e.g., age and gender). One limitation of the publicly available mortality data is that it suppresses annual death counts for groups with less than 10 deaths. The sample of analysis contains all U.S. adults ages 18 to 64, which we stratify by age and gender.

We use two mortality rates: all-cause and drug-related causes, which include ICD-10 codes of X40–X44 (drug poisonings, unintentional), X60–X64 (drug poisonings, suicide), X85 (drug poisonings, homicide), and Y10–Y14 (drug poisonings, undetermined intent).

American Community Survey

The American Community Survey (ACS) collects information on approximately three million people each year covering over 92% of the U.S. population. The survey is conducted on a monthly basis throughout the year and combined into an annual file. We limit the sample to non-disabled, adults between the ages of 18 and 64. The ACS collects a wide range of information on individuals and household. The outcomes we use are: employment at the time of interview, weeks worked in the last year, earnings in the last year, receipt of social assistance income (TANF and SSI), disability, and demographic characteristics (age, gender, race, marital status and education).

Results

Effects of State Policies on Prescription Opioid Sales

We begin the discussion with results from the event-study specification. To estimate this model, we drop the indicator that the state has an electronic PDMP and focus on the modern category of PDMPs and whether a state has a “pill mill” statute. We drop the electronic PDMP category for two reasons. The staggered timing of the PDMP policies and the mutual exclusivity of the two PDMP categories renders the time-to-event indicators mechanically related to each other, difficult to construct conceptually and therefore difficult to interpret. For example, once the state adopts a modern PDMP, the indicator for the presence of an electronic PDMP (not modern) is set equal to 0 despite the fact that it may be the first, second, third, or fourth or more year post the initial adoption of the electronic PDMP. The other reason we drop the electronic PDMP indicator variable for this analysis is because we rarely find a statistically significant or economically meaningful effect of electronic PDMPs on prescription opioid sales.[17]

Given these considerations, the event-study specification we use is the following:

$$(4)\, OPIOIDS_{jt} = \alpha_j + \delta_t + \sum_{k=-4}^{4} \beta_k MODERN_{kjt} + \sum_{k=-4}^{4} \gamma_k PILL\, MILL_{kjt} + \sum_{m=1}^{M} \lambda_m AGE_{mjt} + e_{jt}$$

In equation (4), we allow the effect of adopting a modern PDMP and a pill mill statute to differ by the timing of the policy: from four years prior to adoption to four years after adoption. Years outside of this span are included in the limit categories (-4, 4). We estimate equation (4) using the basic model that includes year and state fixed effects and the age-category variables, and the extended model that adds baseline opioid sales interacted with year effects. We estimate these two models for both dependent variables: log total MEG and per-capita MEG.

The results of the event-study specification are presented in Figures 2 and 3 and Appendix Tables 1 and 2. Figure 2 shows results for all opioids and for oxycodone and hydrocodone when sales are measured in log total MEG. Figure 3 shows similar results when sales are measured as per-capita MEG. It is clear in the figures that results from the two model specifications are similar, so, we focus the discussion on results from the model that includes baseline opioid prescriptions interacted with year effects. As can be seen in Figures 2 and 3, almost all of the coefficient estimates on the pre-policy adoption indicators are statistically insignificant. Among the 48 pre-adoption indicators shown in Figures 2 and 3, only two are statistically significant (one only marginally so). In addition, results from F-tests of the joint significance of the pre-adoption coefficient estimates, which are reported in Appendix Tables 1 and 2, never reject the null hypothesis that estimates are jointly equal to zero. P-values of these tests range from 0.08 to 0.44 with most above 0.25. Overall, the evidence in Figures 2 and 3 and Appendix Tables 1 and 2 strongly support the validity of the difference-in-differences research design.

Also evident in Figures 2 and 3 is the noticeable decline in opioid sales post-adoption of the two state policies, particularly pill mill statutes. Estimates in Appendix Table 1 indicate that adoption of a pill mill statute reduced all opioid sales by 6% in the year of adoption, which may have occurred mid-year, with a

[17] As we report below, only two (out of 34 possible) estimates of the effect of an electronic PDMP are significant. Both estimates are from analyses that use sub-samples of the full sample and from the specification that includes state-specific trends.

growing decline in sales reaching a peak of 25% three years after. Similar results are found for oxycodone and hydrocodone, but estimates are slightly larger (-32% by year 4). Estimates in Figures 2 and 3 and Appendix Tables 1 and 2 pertaining to adoption of a modern PDMP also show a significant decrease in all opioid sales of approximately 3% in the year of adoption increasing to around 6% after one year and then remaining relatively constant. Similar effects of adoption of a modern PDMP are found for oxycodone and hydrocodone.

The next set of estimates are from a standard difference-in-differences model that examines the pre-to-post policy adoption changes in opioid sales combining pre- and post-adoption years into two periods. In these models, we also include the indicator for an electronic PDMP. Estimates are presented in Table 2. The top panel of Table 2 reports estimates for all opioid sales and the bottom panel reports estimates for oxycodone and hydrocodone. The left panel of Table 2 reports estimates for opioid sales measured as total log MEG and the right panel reports estimates for opioid sales measured as per-capita MEG grams. In each of the four panels, we report estimates for three model specifications: basic model that includes state and year fixed effects, and age controls; a second model that adds interactions between baseline opioid sales and year effects; and a third model that adds state-specific, linear trends.

Our discussion of Table 2 results focuses on models that use log total MEG sales, as results from models that use per-capita MEG sales are quite similar and it is easier to interpret the estimates from models that use log sales because they represent percentage (relative) effects. Estimates in the top, left panel indicate that adoption of a modern PDMP is associated with a modest, statistically insignificant decrease in opioid sales of between 2% to 6%. Pill mill laws, however, are associated with significant decreases in opioid sales of between 12% and 18%. As noted earlier, implementation of an electronic PDMP has no significant or economically meaningful effects on opioid sales. Estimates in the bottom left panel of Table 2 indicate similar effects of state policies on sales of oxycodone and hydrocodone. Effect sizes for these opioids tend to be larger. Estimates indicate adoption of a modern PDMP decreased sales of oxycodone and hydrocodone by 6% to 8%. These estimates are only marginally significant. And estimates suggest that pill mill laws reduced oxycodone and hydrocodone sales by 15% to 20%.

Estimates in Table 2 are relatively stable across specifications, which is consistent with earlier results from the event-study analysis. Standard errors of estimates related to pill mill laws increase substantially when state-trends are included reflecting the fact that most pill mill legislation came toward the end of the period. However, as observed in Figures 2 and 3, effects of pill mill statutes tended to grow over time, and the inclusion of state-trends may be a case of “over-fitting” where the state-specific trends are measuring these time-varying effects (Goodman-Bacon 2019).

We also assessed whether it was appropriate to use a more detailed classification of PDMP policies. Appendix Table 3 reports estimates analogous to those in Table 2, but with an expanded classification of

PDMP policies. In this analysis, we use four indicators to classify PDMPs: whether a state had a PDMP; whether it was electronic; whether it was modern; and whether it was modern and had a mandate to use. Results from these models are similar to those reported in Table 2 and are consistent with our more parsimonious classification. For example, whether a state has a mandate to use the PDMP in addition to having what we refer to as a modern PDMP has no additional impact on opioid sales.

Next, we present estimates of the effect of PDMPs and pill mill statutes on opioid sales for samples of places stratified by whether it is urban or rural. The unit of observation in these analyses are states, but consist of a selected number of counties within each state. We stratified counties into three groups based on the U.S. Census definitions of counties that are mostly urban, mostly rural and rural and then aggregated counties to the state level (Ratcliffe et al. 2016). Estimates from these models are presented in Table 3 and are based on the most extensive model specification that includes state-specific trends.[18]

Estimates in Table 3 reveal a noticeable pattern. The effects of PDMP policies and pill mill statutes are concentrated in urban counties. In urban counties, the presence of a modern PDMP is associated with approximately a 5% decrease in all opioid sales and a 9% decrease in sales of oxycodone and hydrocodone, although these estimates are not always significant. Pill mill laws are significantly associated with opioid sales with around a 17% decrease in all opioid sales and a 18% decrease in sales of oxycodone and hydrocodone in urban counties.

Overall, evidence presented in Figures 2 and 3, and Tables 2 and 3, suggest that state policies have significantly reduced opioid sales. This finding is similar to some previous studies (Bao et al. 2016; Dowell et al. 2016; Buchmueller and Carey 2017; Moyo et al. 2017). The evidence is most apparent for the adoption of pill mill laws. These laws are associated with significant reductions in opioid sales of approximately 10% to 20% depending on the model specification and measure of opioid prescriptions. Effects of these laws are concentrated in urban counties. Adoption of a modern PDMP is also associated with a reduction in opioid sales, but effect sizes are smaller (e.g., 5% to 10%) and are only marginally significant.

In the next section, we examine how these policies affect mortality and socioeconomic outcomes. While this is a reduced form approach, the evidence above suggests that these policies have affected opioid prescribing. Therefore, it is plausible that these laws will affect behaviors affected by opioid use, such as mortality, employment and earnings. We also note that our analysis of the effect of PDMPs and pill mill laws on opioid sales was at an aggregate level. We were unable to identify whether these laws had different effects on different demographic groups, which may be expected by the evidence in Table 1 related to differences in medical and mis-use of prescription opioids. Therefore, the aggregate effects

[18] Appendix Table 4 presents estimates from a model that excludes state-specific trends. Results are largely similar to those presented in Table 3.

presented above may mask substantial heterogeneity of the effects of these policies on prescription opioid use across groups.

Effects of State Policies on Mortality

To conserve on space, we present a limited number of results from analyses of the effect of state prescription opioid policies on mortality. Specifically, we present estimates from models that include state and year fixed effects and interactions between baseline opioid sales (at state level) and year effects. Results from other model specifications are largely similar and presented in the Appendix Tables 5 and 6 and referred to in text.

Table 4 presents estimates of the effects of state polices on all-cause mortality for the eight demographic groups stratified by age and gender shown in Table 1. Coefficient estimates on the indicator of electronic PDMP are almost never significant and always very small (<1% of mean). Only one estimate pertaining to the presence of a modern PDMP is statistically significant at 5% level. The rest of the estimates are insignificant and quite small (<1%). The statistically significant estimate is for the sample of females ages 18 to 25. It indicates that adoption of a modern PDMP reduced all-cause mortality by approximately 2 per 100,000 females, or about 4%. However, the analogous estimate in Appendix Table 6, which is from a model that includes state-specific trends, is smaller (-1.4) and marginally significant. For pill mill laws, only one estimate is statistically significant. Among women ages 50 to 64, pill mill laws are associated with a 24 per 100,000, or approximately 4%, increase in all-cause mortality. In this case too, the analogous estimate in Appendix Table 6 is much smaller and not statistically significant. Other estimates in Table 4 pertaining to pill mill laws are not statistically significant. For males younger than age 35, pill mill laws are associated with approximately a 4% decrease in all-cause mortality, which is not significant, but similar estimates in Appendix Table 6 are smaller.

As already noted, similar results as those reported in Table 4 are obtained from other model specifications and presented in Appendix Tables 5 and 6. In Appendix Table 7, we report estimates from the event-study specification analogous to the model used in Table 4. Overall, event study estimates reveal that, for most samples, the difference-in-differences design appears valid. The exception is for persons ages 50 to 64; for this group there appears to be some divergence in mortality (pre) trends between states that did and did not adopt policies. Estimates in Appendix Table 7 also show more clearly the variability of estimates and are less suggestive of a true effect even in the cases where the estimate in Table 4 was statistically significant. For example, among women ages 50 to 64, the event-study estimates related to pill mill laws show no evidence of an effect on mortality.

Table 5 reports estimates of the effect of state polices on drug-related mortality. The causes of mortality included in this analysis are listed in the notes to the table and are several types of drug poisonings. In this case, there is generally little evidence of a statistically significant effect of PDMPs or pill mill laws on mortality. Among young males (ages 18 to 34), pill mill laws are associated with a substantial decline in drug-related mortality of approximately 25%. This is a plausible effect size for this sample. Approximately 12,000 (ages 26-34) to 15,000 (ages 18-25) per 100,000 young men mis-use prescription opioids and/or use heroin (see Table 1). Drug-related deaths for these men are between 17 (ages 18-25) and 28 (ages 26-34) per 100,000. If 20% of these men stopped mis-using prescription opioids because of a pill mill law, and we apply the same rate of mortality as for the sample average, then it implies a decrease of 3.4 deaths per 100,000 for men ages 18 to 25 and 5.6 deaths per 100,000 for men ages 26 to 34. Estimates of the effect of pill mill laws in Table 5 are approximately 4 deaths per 1000,000. Standard errors of estimates of the effect of pill mill laws for these two age groups are a little too large to detect effect sizes of this magnitude, but if the death rate was above average for those who were induced to curtail mis-use by pill mill statutes, then there would likely be sufficient power (5%) to detect an effect of this size. While these calculations are approximate, they indicate that the analysis is adequately powered to detect moderate effect sizes. In addition, the pattern of results is consistent with the conceptual model described earlier. Young men have relatively high rates of mis-use of prescription opioids and the decrease in opioid sales (use) caused by pill mill laws are expected to reduce this mis-use and the consequences of that mis use. The mortality declines for young men suggested by the estimates in Tables 4 and 5 are consistent with this prediction, but we note, again, estimates are not statistically significant.

In Appendix Tables 8 and 9, we present estimates of the effects of PDMPs and pill mill laws on all-cause mortality by urban-rural status. Estimates are very similar to those in Table 4 and there is little evidence of systematic differences in the effects of state policies on mortality by urban-rural status. Few estimates in these tables are statistically significant and most are small (<5% of mean). Among males younger than 35, pill mill laws are associated with a decrease in all-cause mortality of between approximately 3% and 5% depending on sample (urban-rural) and age group (18-25, 26-34), but these effects are not statistically significant and similar to those presented in Table 4.

Our findings that PDMPs and pill mill laws have small to no significant effect on mortality are consistent with some prior studies, although the literature remains relatively sparse. We also note that our analysis and other similar analyses are under powered to detect reliably small effect sizes. Grecu et al (2019) reported that PDMPs were associated with a 25% decline in opioid-related mortality among persons ages 18 to 24; we found a similar sized effect for males in our analysis, but our estimate was not statistically significant. In addition, Grecu et al. (2019) reported no significant change in all-cause mortality for those ages 18 to 24 and no effects of PDMPs on mortality for other groups. Paulozzi et al. (2011) compared state level opioid sales (ARCOS) and mortality in PDMP and non-PDMP states between 1999 and 2005 and found that PDMPs were not associated with declines in either opioid sales or morality. Similarly, Li (2014) compared states with and without PDMPs from 1999 to 2008 and found no difference in drug overdose mortality.

Effects of State Policies on Socioeconomic Outcomes

Prescription opioid use and mis-use may affect socioeconomic outcomes because of the effects of prescription opioids on health (e.g., pain relief) and through consumption pathways, for example, mis-use and the adverse consequences of such use. We have shown that state prescription opioid control policies, particularly pill mill laws, decreased opioid sales substantially and presumably opioid use too. Therefore, it is plausible that state policies have affected socioeconomic outcomes, such as employment and earnings. We present evidence on this issue next.

Table 6 presents estimates of the effects of (lagged) state policies on the probability of being employed at the time of survey. The table shows results for the eight demographic groups and for three model specifications in different panels (top, middle and bottom). Before describing specific estimates, we note the following. All estimates in the table have magnitudes that are 2% or less of the mean, and most are less than 1% of the mean. Second, standard errors of estimates are small enough to detect reliably effect sizes that are approximately 1.5% or larger. This information is important because plausible effect sizes will be small. For example, we have shown that state policies decrease prescription opioid sales, and presumably use, by 5% to 20% depending on the policy and types of opioids. Decreases may be larger (smaller) for some groups because this is an average effect among all adults. We have also shown that prescription opioid use and mis-use differs significantly by demographic groups and that prescription opioid use (mis-use) is non-trivial. Among young men, prescription opioid mis-use is relatively high (14%) while for older women medically prescribed opioid use is relatively high (16%). These figures also represent use and mis-use of prescription opioids from a period, 2002-2006, in the beginning of our analysis period and when prescription opioid use had not reached its peak (2010-2011).[19] Finally, these figures are self-reported and may underestimate prescription opioid use, particularly mis-use. Given these numbers on use of prescription opioids, a 10% to 20% decrease in prescription opioid use, which is consistent with the adoption of PDMPs and pill mill laws, would imply that approximately 1.5% to 3% of young men would not mis-use prescription opioids. As noted, we are able to detect a change in employment of approximately 1.5%. So, if the effect of prescription opioid mis-use on employment was moderate to large, we would be able to detect such effects. Of course, this is a

[19] These figures are self-reported and may be under reported, particularly mis-use, because of the sensitive nature of the information.

rough approximation, but it shows that we can detect reliably moderate to large treatment (on treated) effects for this outcome.

Returning to the estimates in Table 6, our reading of the evidence is that there are few patterns that indicate an effect that is statistically significant or economically important. For example, consider estimates for young people (ages 18 to 34). Several of the estimates of the effects of a modern PDMP are statistically significant and estimates are between -0.012 and -0.005. However, estimates of the effect of a pill mill are mostly positive despite the fact that both policies decreased prescription opioid sales. While these two policies may have affected different dimensions of prescription opioid use and, therefore, may have had different effects on employment, the diverging estimates are a source of uncertainty. In addition, estimates vary somewhat with model specification, which is another source of uncertainty. Overall, the evidence in Table 6 indicates that state policies had no moderate to large effects on employment, but we cannot rule out small effects. In Appendix Tables 10 and 11, we present similar estimates for two sub-groups: persons living in urban areas (only for years 2006-2016) and persons with a high school education or less. Estimates and conclusions from those analyses are similar to those in Table 6.

Table 7 presents estimates of the effects of state prescription opioid policies on whether a person worked full-year, which is defined as 48 or more weeks in the last 12 months. Much of the description of results in Table 6 apply here too. Effects sizes are small—almost always less than 1% of mean—and standard errors are small enough to detect reliably moderate to large treatment effects. However, we do not see a consistent pattern among estimates that would suggest that state policies had a moderate to large effect on full-year work. Similar estimates are found for sub-samples restricted to those living in urban areas or for those with a high school education or less (Appendix Tables 12 and 13, respectively).

The next outcome we examined was earned income in past 12 months, which is measured in thousands of 2010 dollars and includes people with zero earnings. Estimates of the effects of state policies on this outcome are shown in Table 8. Few estimates are statistically significant. However, there is one pattern among the estimates that is readily identifiable: adoption of a modern PDMP is always associated with a decrease in earned income and the adoption of a pill mil statute is almost always associated with an increase in earned income. This pattern is more pronounced (e.g., larger relative effects) among younger cohorts than older cohorts and among males than females. For example, adoption of a modern PDMP is associated with between a $238 to $539 decrease in earned income among males ages 18 to 25; these are relatively modest effects relative to the mean—between 2% to 5%. Adoption of a pill mill law has the opposite effect for this demographic group: an increase in earnings of between $273 (2%) and $734 (6%).

Notably both policies were associated with a decrease in opioid sales. However, it may be the case that pill mill policies reduced mis-use of opioids and this resulted in an improvement in earnings (and employment and weeks worked, see Tables 6 and 7), while adoption of a modern PDMP reduced medically indicated use. The pattern of results just described is even more pronounced among a sample of low-educated persons (see Appendix Table 15).[20] In this sample, many of the estimates are statistically significant, particularly for younger cohorts, and slightly larger in magnitude. Whether our speculation as to the different effects of PDMPs and pill mill policies is valid is an issue for further research.

We next present results of the effect of state opioid policies on receipt of cash social assistance: TANF or SSI.[21] Estimates are in Table 9. The mean of the dependent variable is quite small (2% to 5%) for this outcome and estimates reflect this and are also small in absolute size. Nevertheless, we see little evidence of an effect of state policies on this outcome. The one consistent finding is related to adoption of pill mill laws; among women ages 50 to 64, adoption of a pill mill law is associated with a 0.3 percentage point (6%) increase in receipt of cash assistance. While this is a statistically significant, it does not align with other estimates, for example, related to labor market. Overall, we see little evidence in Table 9 that state policies were associated with a significant or meaningful change in receipt of cash assistance.[22]

Table 10 presents estimates of the effect of state policies on the probability of being married and living with a spouse. As was the case with other outcomes, estimates in Table 10 are quite small—always less than 2% of the mean. Estimates are also relatively precise with standard errors that can detect reliably effect sizes of approximately 0.5 percentage points. The one noticeable pattern among estimates in Table 10 is that pill mill laws are associated with a significant decrease in the probability of being married, except for estimates in the bottom panel from models that include state-specific trends. Estimates remain small, however, and are approximately 1% of mean.

The last set of results is from an analysis of the effect of PDMPs and pill mill laws on the probability of having a cognitive or ambulatory problem, for example, difficulty learning or making decisions because of a physical, mental or emotional condition, or difficulty with basic physical activities. Estimates in Table 11 are mostly insignificant and small, and there are few noticeable patterns that would suggest an effect of PDMPs or pill mill laws on these health-related problems.

Conclusion

The opioid "epidemic" is a major public health problem, primarily because of the adverse consequences opioid mis-use (abuse) has on health, particularly mortality. In response to this epidemic, states have undertaken a variety of policies to stem opioid use, particularly prescription opioid use. The

[20] Among those in urban areas (2006-2016 period), the pattern described in text is not as evident (see Appendix Table 14).
[21] We do not examine Medicaid, which may be a likely consequence of opioid use, because prior to 2008, the ACS does not include such information.
[22] Estimates from analyses stratified by urban-rural area and by education are available on request. Estimates from these analyses do not differ qualitatively from those reported in text. This is also the case for the next two outcomes: probability of being married and probability of having a cognitive or physical disability.

most prominent policies are PDMPs and “pill mill” statutes. In this article, we showed that PDMPs and ‘pill mill” laws significantly reduced prescription opioid sales by between 5% and 20% depending on the policy and the type of opioid prescription. “Pill mill” laws have been particularly effective reducing prescription opioid sales by between 10% and 20%, but “modern” PDMPs have also significantly reduced prescription opioid sales, although by a more modest amount (5% to 10%).

While the mortality consequences of prescription opioid use garner the most public attention, and rightfully so given the value of life, prescription opioid use may affect other aspects of life that determine wellbeing, such as employment, earnings and marriage. There has been little study of this issue. In this article we provide evidence on this research question. We also incorporate the fact that most prescription opioid use is medically prescribed and arguably clinically indicated, and that the effect of reductions in prescription opioids may have different effects depending on whether prescription opioid use is medically prescribed or mis-used. We identify demographic groups that have very different profiles of prescription opioid use in terms of whether it is mis-use or medically prescribed use, and we examine these groups separately.

Results suggest that the reductions in opioid prescriptions associated with PDMPs and pill mill laws had relatively little effect on mortality. We found suggestive evidence that “pill mill” laws reduced drug-related mortality among young males, which is consistent with this group having the highest rates of prescription opioid mis-use. However, estimates were not statistically significant though large (25%). For most socioeconomic outcomes, we found little evidence that the reductions in opioid sales due to adoption of a “modern” PDMP or of a “pill mill” law had moderate to large (treatment) effects on socioeconomic outcomes. The most consistent finding was that adoption of a “modern” PDMP decreased earnings and the adoption of a “pill mill” law increased earnings among young persons (ages 18 to 34), particularly males. However, the statistical significance of these estimates was marginal, and there is some degree of uncertainty across estimates as to whether this is a true effect. Why these policies had opposite effects, if in fact they did, despite both decreasing opioid sales is a question for possible further inquiry.

Finally, in terms of population wellbeing, PDMPs and “pill mill” laws had relatively few benefits, as shown in this article. The proportion of the population that mis-uses prescription opioids is relatively small and the proportion with a prescription opioid abuse disorder is even smaller—less than 1% of the population (McCance-Katz 2018). Therefore, the likely reduction in prescription opioid mis-use brought forth by state policies will have small although potentially important effects, for example, a decrease in mortality (although some evidence on mortality is inconsistent with hypothesis). In contrast, a relatively large share of the population uses medically prescribed prescription opioids and limits their use to medical purposes. Reductions in medically prescribed opioids may not have all positive benefits. Indeed, there has been a growing concern that the pendulum has swung too far and that appropriate prescription opioid use is being curtailed (Dowell et al. 2019; Bohnert et al. 2019; Kroenke et al. 2018). If so, then curtailing such prescription use through PDMPs and "pill mill" laws will affect a relatively large share of the population and may have adverse consequences on wellbeing, although we found little evidence to support this hypothesis.

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Table 1. Mean Illicit Drug Use by Age, Education and Gender, 2002-2006

	Age 18-25			Age 26-34			Age 35-49			Age 50-64		
Males	LTHS	HS	>HS	LTHS	HS	>HS	LTHS	HS	>HS	LTHS	HS	>HS
Any Non-medical Use Pain Reliever Past Year	0.144	0.141	0.121	0.094	0.093	0.068	0.047	0.048	0.041	0.037	0.025	0.021
Any Medical Use Pain Reliever Past Year	0.061	0.067	0.059	0.073	0.085	0.086	0.115	0.121	0.098	0.139	0.142	0.129
Any Heroin Use Past Year	0.026	0.020	0.013	0.042	0.032	0.018	0.051	0.034	0.020	0.028	0.025	0.034
Heroin or Non-medical Pain Reliever Past Year	0.151	0.148	0.124	0.121	0.107	0.079	0.089	0.076	0.058	0.059	0.042	0.051
Any Illicit Drug Not Marijuana Past Year	0.232	0.220	0.200	0.147	0.147	0.116	0.096	0.094	0.077	0.070	0.044	0.035
Females												
Any Non-medical Use Pain Reliever Past Year	0.124	0.107	0.103	0.055	0.058	0.052	0.057	0.043	0.032	0.021	0.015	0.015
Any Medical Use Pain Reliever Past Year	0.120	0.131	0.100	0.143	0.147	0.133	0.155	0.158	0.145	0.181	0.157	0.148
Any Heroin Use Past Year	0.020	0.018	0.010	0.016	0.019	0.010	0.029	0.012	0.011	0.008	0.010	0.011
Heroin or Non-medical Pain Reliever Past Year	0.132	0.110	0.107	0.069	0.071	0.059	0.076	0.052	0.042	0.029	0.025	0.024
Any Illicit Drug Not Marijuana Past Year	0.185	0.172	0.170	0.093	0.095	0.093	0.099	0.069	0.060	0.029	0.024	0.024

Notes – All means except medical use of pain relievers are estimated using data from the 2002 to 2006 National Survey on Drug Use and Health. Mean medical use of pain relievers is from Medical Expenditure Survey from 2002 to 2006.

Table 2. Estimates of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Retail Opioid Prescriptions, 2002-2016

	Log Grams MEG			**Per-capita MEG Grams**		
All Opioids	(1)	(2)	(3)	(4)	(5)	(6)
Electronic PDMP	-0.010	0.012	0.016	-0.040	-0.004	0.000
	(0.030)	(0.024)	(0.021)	(0.030)	(0.021)	(0.017)
Modern PDMP	-0.060	-0.044	-0.023	-0.080*	-0.049	-0.044
	(0.050)	(0.037)	(0.032)	(0.040)	(0.032)	(0.031)
Pill Mill	-0.120**	-0.126**	-0.176**	-0.070*	-0.084**	-0.125**
	(0.030)	(0.033)	(0.050)	(0.040)	(0.029)	(0.042)
P-value of F-Test of Joint Significance	0.00	0.00	0.01	0.11	0.02	0.03
Mean of Dependent Variable in 2002				0.39	0.39	0.39
Hydrocodone/Oxycodone						
Electronic PDMP	-0.000	0.015	-0.013	-0.030	-0.009	-0.023
	(0.040)	(0.023)	(0.025)	(0.030)	(0.019)	(0.018)
Modern PDMP	-0.080	-0.067*	-0.073*	-0.070*	-0.048*	-0.051*
	(0.060)	(0.036)	(0.040)	(0.040)	(0.026)	(0.030)
Pill Mill	-0.200**	-0.149**	-0.150*	-0.080**	-0.083**	-0.082*
	(0.040)	(0.040)	(0.081)	(0.040)	(0.029)	(0.045)
P-value of F-Test of Joint Significance	0.00	0.00	0.02	0.11	0.02	0.18
Mean of Dependent Variable in 2002				0.25	0.25	0.25
Baseline Opioid * Year Effects	No	Yes	Yes	No	Yes	Yes
State Linear Trend	No	No	Yes	No	No	Yes
Number of Observations	765	765	765	765	765	765

Notes: We exclude prescriptions for methadone, buprenorphine and fentanyl. The unit of observation is the state-year. Regressions using log MEG grams include state fixed effects, year fixed effects, and the natural logarithm of state population in four-year age categories for ages 15 and older. Regressions using per-capita MEG grams include state fixed effects, year fixed effects and the share of the state population in four-year age categories for ages 15 and older. All regressions are weighted by state population. Standard errors in parentheses. Standard errors were constructed allowing for non-independence (clustering) within state. *0.05< p-value ≤ 0.10, ** p-value ≤ 0.05

Table 3. Estimates of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Retail Opioid Prescriptions, By Urban/Rural Status and By Baseline Opioid Prescriptions
2002-2016

	Log Grams MEG			
All Opioids	All Counties	Urban Counties	Mostly Rural	Rural Counties
Electronic PDMP	0.016	0.007	0.054**	0.019
	(0.021)	(0.026)	(0.024)	(0.019)
Modern PDMP	-0.023	-0.046	0.052*	-0.001
	(0.032)	(0.043)	(0.027)	(0.027)
Pill Mill	-0.176**	-0.166**	-0.062	0.011
	(0.050)	(0.056)	(0.040)	(0.031)
P-value of F-Test of Joint Significance	0.01	0.02	0.08	0.44
Hydrocodone/Oxycodone				
Electronic PDMP	-0.013	-0.005	0.024	0.036**
	(0.025)	(0.024)	(0.017)	(0.017)
Modern PDMP	-0.073*	-0.092*	0.001	0.024
	(0.040)	(0.047)	(0.019)	(0.021)
Pill Mill	-0.150*	-0.182**	0.022	0.079**
	(0.081)	(0.083)	(0.052)	(0.029)
P-value of F-Test of Joint Significance	0.01	0.03	0.98	0.01
Baseline Opioid * Year Effects	Yes	Yes	Yes	Yes
State Linear Trend	Yes	Yes	Yes	Yes
Number of Observations	765	764	675	615

Notes: We exclude prescriptions for methadone, buprenorphine and fentanyl. The unit of observation is the state-year. Regressions using log MEG grams include state fixed effects, year fixed effects, and the natural logarithm of state population in four-year age categories for ages 15 and older. All regressions are weighted by state population. Standard errors in parentheses. Standard errors were constructed allowing for non-independence (clustering) within state. * 0.01< p-value ≤ 0.05, ** p-value ≤ 0.01

Table 4: Estimates of Effects of State Polices on Mortality Rates from All Causes, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Electronic PDMP, Lag 1	-0.986	-0.401	-0.988	1.364	-1.074	-0.799	-5.755	-8.672
	(0.800)	(2.324)	(1.136)	(2.323)	(2.102)	(3.136)	(4.508)	(6.421)
Modern PDMP, Lag 1	-2.085**	-2.121	-0.413	-1.707	1.635	0.906	-7.207*	-2.751
	(0.872)	(3.500)	(1.798)	(3.815)	(3.360)	(5.390)	(4.003)	(7.013)
Pill Mill, Lag 1	-1.814	-4.560	-1.543	-7.368	2.130	-7.508	24.337**	19.802
	(2.009)	(3.935)	(2.870)	(5.621)	(5.435)	(8.522)	(7.322)	(13.672)
Mean of Dependent Variable	45.436	128.644	68.273	150.314	178.158	294.529	559.771	921.273
Number of Observations	763	765	765	765	765	765	765	765

Notes: The unit of observation is the state-year mortality rate (per 100,000) for the demographic groups listed in the tables. All regression models include state and year fixed effects, interactions between baseline opioid sales and year effects and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. ** $p < 0.05$

Table 5: Estimates of Effects of State Polices on Mortality Rates from Drug-related Causes, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Electronic PDMP, Lag 1	0.486	1.180	0.359	1.421	0.597	1.263	-0.585	-0.261
	(0.554)	(1.277)	(1.128)	(2.394)	(1.087)	(2.197)	(0.622)	(1.135)
Modern PDMP, Lag 1	-0.218	0.363	-0.610	-1.851	0.136	-0.633	-0.794	-0.933
	(0.578)	(1.228)	(1.317)	(2.428)	(1.380)	(2.333)	(0.682)	(1.348)
Pill Mill, Lag 1	-0.687	-4.159	-0.093	-3.990	0.265	0.315	0.322	-0.577
	(1.194)	(2.532)	(2.065)	(4.284)	(1.979)	(3.953)	(0.962)	(1.781)
Mean of Dependent Variable	6.635	17.423	12.568	27.985	19.430	31.052	16.315	24.042
Number of Observations	471	615	590	674	729	739	684	706

Notes: See notes to Table 4. Drug-related causes include underlying-cause-of-death ICD-10 code of X40–X44 (drug poisonings, unintentional), X60–X64 (drug poisonings, suicide), X85 (drug poisonings, homicide), and Y10–Y14 (drug poisonings, undetermined intent).

Table 6: Estimates of Effects of State Polices on Probability of Being Employed at Time of Survey, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.004	-0.002	-0.004	-0.002	0.001	0.002	0.000	0.001
	(0.004)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.005	-0.012**	-0.004	-0.005	-0.002	-0.001	-0.004**	-0.003
	(0.004)	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Pill Mill, Lag 1	0.007	0.010	0.000	-0.004	0.001	-0.001	0.002	0.003
	(0.007)	(0.009)	(0.004)	(0.007)	(0.003)	(0.005)	(0.003)	(0.003)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.005	-0.005	-0.004	-0.004	0.001	0.001	-0.001	-0.001
	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.004	-0.013**	-0.008**	-0.007*	-0.003	-0.003	-0.004**	-0.004
	(0.005)	(0.006)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.007	0.008	0.003	-0.003	0.001	-0.000	0.001	0.002
	(0.006)	(0.008)	(0.004)	(0.006)	(0.002)	(0.004)	(0.002)	(0.003)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.003	0.004	-0.002	-0.001	0.001	0.002	-0.001	-0.000
	(0.003)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.002	-0.007	-0.005	-0.003	-0.001	-0.001	-0.001	-0.002
	(0.004)	(0.006)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.011**	0.011	0.009**	0.009*	0.004	0.007*	0.000	0.001
	(0.005)	(0.007)	(0.002)	(0.005)	(0.003)	(0.003)	(0.002)	(0.003)
Mean of Dependent Variable	0.601	0.620	0.699	0.821	0.714	0.835	0.608	0.705
Number of Observations	23,252	23,450	25,907	26,114	42,943	43,203	41,727	41,670

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Table 7: Estimates of Effects of State Polices on Probability of Working 48 or more Weeks Past 12 Months, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.005	-0.005	-0.005*	-0.003	0.000	0.002	0.000	-0.000
	(0.004)	(0.005)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.005	-0.014**	-0.006	-0.008*	-0.003	-0.002	-0.003	-0.005*
	(0.004)	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.015**	0.013	0.004	-0.000	0.003	0.001	0.006**	0.004
	(0.007)	(0.009)	(0.006)	(0.008)	(0.004)	(0.006)	(0.003)	(0.004)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.006	-0.008*	-0.006**	-0.006	-0.001	0.001	-0.001	-0.002
	(0.004)	(0.005)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.003	-0.014**	-0.007**	-0.012**	-0.003	-0.003	-0.003	-0.006*
	(0.005)	(0.005)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.014**	0.011	0.006	0.000	0.003	0.001	0.005**	0.003
	(0.006)	(0.008)	(0.005)	(0.007)	(0.003)	(0.005)	(0.002)	(0.004)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.002	0.001	-0.001	-0.003	-0.000	0.002	-0.000	-0.001
	(0.003)	(0.005)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.004	-0.010*	-0.003	-0.008	-0.001	-0.002	-0.000	-0.004
	(0.004)	(0.005)	(0.003)	(0.005)	(0.002)	(0.004)	(0.002)	(0.003)
Pill Mill, Lag 1	0.006	0.007	0.008**	0.006	0.002	0.006*	0.003	-0.000
	(0.005)	(0.007)	(0.003)	(0.005)	(0.003)	(0.004)	(0.002)	(0.004)
Mean of Dependent Variable	0.463	0.488	0.636	0.776	0.661	0.803	0.569	0.675
Number of Observations	23,252	23,450	25,907	26,114	42,943	43,203	41,727	41,670

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Table 8: Estimates of Effects of State Polices on Earned Income (2010 dollars in $1,000) in Past 12 Months, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.202	-0.182	-0.163	-0.503	0.226	0.094	0.133	-0.034
	(0.152)	(0.249)	(0.232)	(0.481)	(0.249)	(0.492)	(0.178)	(0.437)
Modern PDMP, Lag 1	-0.229	-0.520**	-0.386	-0.630	-0.378	-0.261	-0.224	-0.530
	(0.147)	(0.198)	(0.257)	(0.404)	(0.240)	(0.399)	(0.247)	(0.389)
Pill Mill, Lag 1	0.418	0.734*	0.004	0.689	-0.506	-0.146	-0.060	0.370
	(0.255)	(0.404)	(0.364)	(0.878)	(0.416)	(0.929)	(0.316)	(0.865)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.223	-0.262	-0.193	-0.630	0.229	-0.017	0.118	-0.138
	(0.137)	(0.234)	(0.190)	(0.448)	(0.190)	(0.402)	(0.146)	(0.369)
Modern PDMP, Lag 1	-0.160	-0.539**	-0.444	-0.604	-0.437**	-0.459	-0.221	-0.491
	(0.134)	(0.232)	(0.270)	(0.432)	(0.211)	(0.425)	(0.220)	(0.402)
Pill Mill, Lag 1	0.372	0.745*	0.120	0.833	-0.418	0.058	-0.063	0.123
	(0.236)	(0.371)	(0.316)	(0.762)	(0.303)	(0.692)	(0.274)	(0.636)
Model with State-specific Trends								
Electronic PDMP, Lag 1	-0.006	0.130	-0.140	-0.202	0.042	-0.125	0.049	0.095
	(0.096)	(0.180)	(0.168)	(0.283)	(0.165)	(0.272)	(0.139)	(0.262)
Modern PDMP, Lag 1	-0.134	-0.238	-0.226	-0.185	-0.249	-0.191	-0.159	-0.317
	(0.141)	(0.235)	(0.260)	(0.417)	(0.205)	(0.384)	(0.186)	(0.413)
Pill Mill, Lag 1	0.126	0.273	0.478*	0.964**	0.360	0.977**	0.421*	0.321
	(0.171)	(0.360)	(0.266)	(0.370)	(0.287)	(0.396)	(0.213)	(0.460)
Mean of Dependent Variable	9.228	11.871	23.135	35.107	28.960	52.624	24.965	47.446
Number of Observations	23,252	23,450	25,907	26,114	42,943	43,203	41,727	41,670

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** p <= 0.05

Table 9: Estimates of Effects of State Polices on Probability of Receipt of Any Public Assistance Past 12 Months, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	0.001	0.001	0.001	0.000	0.001	0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Modern PDMP, Lag 1	0.002	0.000	0.002	0.001	0.001	0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pill Mill, Lag 1	0.000	-0.001	0.003	-0.000	0.001	0.001	0.003*	0.000
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	0.001	0.001*	0.001	0.000	0.001	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Modern PDMP, Lag 1	0.001	0.000	0.003*	0.000	0.002	0.001*	-0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pill Mill, Lag 1	0.001	-0.001	0.003	0.000	0.001	0.001	0.003**	0.000
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Model with State-specific Trends								
Electronic PDMP, Lag 1	-0.000	0.001	0.002	-0.001	0.001*	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Modern PDMP, Lag 1	-0.000	0.000	0.004**	-0.000	0.002*	0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pill Mill, Lag 1	0.001	-0.000	0.000	-0.000	0.002	0.001	0.002**	-0.001
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Mean of Dependent Variable	0.035	0.020	0.048	0.025	0.044	0.029	0.051	0.042
Number of Observations	23,252	23,450	25,907	26,114	42,943	43,203	41,727	41,670

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). Public assistance incudes welfare and supplemental security income. All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** p <= 0.05

Table 10: Estimates of Effects of State Polices on Probability of Married and Living with Spouse, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	0.006*	0.006**	0.000	0.004*	0.001	0.002	-0.000	0.002
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
Modern PDMP, Lag 1	-0.002	-0.000	-0.002	0.001	-0.001	0.003	0.001	0.005**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Pill Mill, Lag 1	-0.006	-0.006*	-0.003	-0.005	-0.007	-0.010**	-0.012**	-0.008*
	(0.004)	(0.003)	(0.006)	(0.004)	(0.005)	(0.003)	(0.005)	(0.005)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	0.007	0.007**	-0.000	0.004*	0.002	0.003*	-0.000	0.002*
	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Modern PDMP, Lag 1	-0.002	-0.001	-0.000	0.000	-0.001	0.003	-0.000	0.003
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
Pill Mill, Lag 1	-0.008*	-0.006*	-0.004	-0.007**	-0.008**	-0.011**	-0.011**	-0.007**
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.001	0.002	0.000	0.001	0.001	0.002	-0.000	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Modern PDMP, Lag 1	-0.001	-0.002	0.001	0.001	0.001	0.004	0.001	0.005**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Pill Mill, Lag 1	0.000	0.002	0.001	0.005	0.001	0.005**	-0.005	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Mean of Dependent Variable	0.134	0.080	0.488	0.420	0.610	0.610	0.600	0.666
Number of Observations	23,252	23,450	25,907	26,114	42,943	43,203	41,727	41,670

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** p <= 0.05

Table 11: Estimates of Effects of State Polices on Probability of Any Cognitive or Ambulatory Difficulty, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.000	-0.001	-0.001	-0.001	-0.001	-0.002	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.001	0.001	-0.001	-0.001	-0.001	-0.001	-0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Pill Mill, Lag 1	-0.001	-0.002**	-0.000	-0.001	-0.002	-0.000	-0.003	-0.006**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.001	0.003**	-0.001	-0.000	0.000	0.000	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Pill Mill, Lag 1	-0.001	-0.003**	-0.001	-0.001	-0.003*	-0.001	-0.004**	-0.006**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.000	-0.000	0.001	0.000	0.001	-0.000	0.004**	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Modern PDMP, Lag 1	-0.001	0.004**	-0.000	0.001	0.002*	0.002	0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Pill Mill, Lag 1	0.002	-0.002	-0.001	-0.002	0.003**	0.001	0.003	-0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
Mean of Dependent Variable	0.040	0.054	0.045	0.051	0.079	0.074	0.151	0.139
Number of Observations	23,252	23,450	25,907	26,114	42,943	43,203	41,727	41,670

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Appendix Table 1. Event Study Estimates of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Logarithm Retail Opioid Prescriptions 2002-2016

	All Opioids		Hydrocodone/Oxycodone	
Log Grams MEG	All Counties	All Counties	All Counties	All Counties
Pill Mill				
Period t = - 4	-0.089	-0.063	-0.080	-0.100
	(0.095)	(0.071)	(0.108)	(0.095)
Period t = - 3	-0.046	-0.027	-0.035	-0.042
	(0.049)	(0.051)	(0.058)	(0.058)
Period t = - 2	-0.015	0.0017	0.0032	-0.0025
	(0.029)	(0.033)	(0.045)	(0.046)
Period t = 0	-0.071	-0.058*	-0.090*	-0.057
	(0.045)	(0.029)	(0.049)	(0.041)
Period t = 1	-0.178**	-0.162**	-0.247**	-0.200**
	(0.088)	(0.059)	(0.121)	(0.085)
Period t = 2	-0.243**	-0.225**	-0.324**	-0.259**
	(0.107)	(0.071)	(0.151)	(0.111)
Period t = 3	-0.276**	-0.246**	-0.339**	-0.275**
	(0.099)	(0.061)	(0.138)	(0.095)
Period t = 4	-0.202*	-0.197**	-0.333**	-0.320**
	(0.101)	(0.069)	(0.107)	(0.085)
P-value of F-Test of Joint Significance for Pre-policy Coefficients	0.577	0.289	0.08	0.426
Modern PDMP				
Period t = - 4	-0.057	-0.051*	-0.082*	-0.078**
	(0.038)	(0.030)	(0.042)	(0.037)
Period t = - 3	-0.036	-0.037*	-0.042	-0.023
	(0.025)	(0.021)	(0.03)	(0.024)
Period t = - 2	-0.029*	-0.023	-0.03	-0.005
	(0.015)	(0.014)	(0.018)	(0.014)
Period t = 0	-0.036**	-0.032*	-0.047**	-0.051**
	(0.017)	(0.016)	(0.017)	(0.016)
Period t = 1	-0.073**	-0.059**	-0.080**	-0.075**
	(0.026)	(0.022)	(0.034)	(0.026)
Period t = 2	-0.064*	-0.068**	-0.084**	-0.077**
	(0.032)	(0.030)	(0.040)	(0.030)
Period t = 3	-0.045	-0.062	-0.085**	-0.065**
	(0.041)	(0.039)	(0.039)	(0.030)
Period t = 4	-0.048	-0.065	-0.071*	-0.071*
	(0.050)	(0.048)	(0.041)	(0.041)
P-value of F-Test of Joint Significance for Pre-policy Coefficients	0.134	0.323	0.129	0.121
Baseline Opioid * Year Effects	No	Yes	No	Yes
State Linear Trend	No	No	No	No
Number of Observations	765	765	765	765

Notes: We exclude prescriptions for methadone, buprenorphine and fentanyl. The unit of observation is the state-year. Regressions include state fixed effects, year fixed effects, the natural logarithm of state population in four-year age categories for ages 15 and older, indicators for the time sense pill mill legislation was passed (up to four lags and four post periods) and indicators for the time sense a modern operational or modern operational and mandated PDMP was available (up to four lags and four post periods). All regressions are weighted by state population. Standard errors in parentheses. Standard errors were constructed allowing for non-independence (clustering) within state. Figure 2 presents these coefficients graphically. * 0.05 < p-value ≤ 0.10 ** p-value ≤ 0.05

Appendix Table 2. Event Study Estimates of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Per Capita Retail Opioid Prescriptions 2002-2016

	All Opioids		Hydrocodone/Oxycodone	
Per Capita MEG Grams	All Counties	All Counties	All Counties	All Counties
Pill Mill				
Period t = - 4	-0.182	-0.077	-0.172	-0.111
	(0.121)	(0.074)	(0.115)	(0.095)
Period t = - 3	-0.088	-0.031	-0.089	-0.050
	(0.072)	(0.047)	(0.072)	(0.058)
Period t = - 2	-0.058	-0.0038	-0.054	-0.008
	(0.042)	(0.030)	(0.047)	(0.046)
Period t = 0	-0.068	-0.052*	-0.073	-0.054
	(0.057)	(0.028)	(0.056)	(0.040)
Period t = 1	-0.189*	-0.157**	-0.194*	-0.192**
	(0.107)	(0.055)	(0.116)	(0.085)
Period t = 2	-0.249**	-0.214**	-0.241*	-0.243**
	(0.114)	(0.065)	(0.124)	(0.110)
Period t = 3	-0.261**	-0.226**	-0.242**	-0.260**
	(0.104)	(0.055)	(0.110)	(0.091)
Period t = 4	-0.222**	-0.182**	-0.240**	-0.329**
	(0.0978)	(0.063)	(0.092)	(0.08)
P-value of F-Test of Joint Significance for Pre-policy Coefficients	0.198	0.337	0.322	0.344
Modern PDMP				
Period t = - 4	-0.036	-0.044	-0.042	-0.064
	(0.035)	(0.032)	(0.031)	(0.039)
Period t = - 3	-0.037	-0.034	-0.036	-0.020
	(0.027)	(0.021)	(0.026)	(0.024)
Period t = - 2	-0.027*	-0.023	-0.024*	-0.004
	(0.015)	(0.015)	(0.014)	(0.014)
Period t = 0	-0.033**	-0.034**	-0.034**	-0.050**
	(0.016)	(0.016)	(0.015)	(0.016)
Period t = 1	-0.066**	-0.063**	-0.061**	-0.075**
	(0.028)	(0.021)	(0.029)	(0.026)
Period t = 2	-0.064**	-0.074**	-0.065**	-0.077**
	(0.031)	(0.029)	(0.030)	(0.029)
Period t = 3	-0.050	-0.068*	-0.060**	-0.063**
	(0.034)	(0.038)	(0.027)	(0.029)
Period t = 4	-0.044	-0.066	-0.047	-0.064
	(0.043)	(0.048)	(0.028)	(0.044)
P-value of F-Test of Joint Significance for Pre-policy Coefficients	0.343	0.435	0.392	0.380
Mean Dependent Variable in 2002	0.39	0.39	0.24	0.24
Baseline Opioid * Year Effects	No	Yes	No	Yes
State Linear Trend	No	No	No	No
Number of Observations	765	765	765	765

Notes: We exclude prescriptions for methadone, buprenorphine and fentanyl. The unit of observation is the state-year. Regressions include state fixed effects, year fixed effects and the share of the state population in four-year age categories for ages 15 and older, indicators for the time sense pill mill legislation was passed (up to four lags and four post periods) and indicators for the time sense a modern operational or modern operational and mandated PDMP was available (up to four lags and four post periods). All regressions are weighted by state population. Standard errors in parentheses. Standard errors were constructed allowing for non-independence (clustering) within state. Figure 3 presents these coefficients graphically. * 0.05 < p-value ≤ 0.10 ** p-value ≤ 0.05

Appendix Table 3. Estimates of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Retail Opioid Prescriptions, Expanded Classification of PDMPs
2002-2016

	Log Grams MEG		
All Opioids	(1)	(2)	(3)
PDMP Enacted	0.100**	0.095**	0.034
	(0.038)	(0.042)	(0.049)
Electronic PDMP	0.014	0.033	0.024
	(0.034)	(0.027)	(0.020)
Modern PDMP	-0.033	-0.020	-0.015
	(0.049)	(0.038)	(0.033)
Modern PDMP-Mandate	-0.020	-0.039	-0.004
	(0.054)	(0.048)	(0.057)
Pill Mill	-0.138**	-0.130**	-0.180**
	(0.042)	(0.035)	(0.055)
P-value of F-Test of Joint Significance	0.00	0.00	0.02
Hydrocodone/Oxycodone			
PDMP Enacted	0.025	0.015	-0.053
	(0.034)	(0.038)	(0.048)
Electronic PDMP	0.006	0.023	-0.020
	(0.032)	(0.023)	(0.024)
Modern PDMP	-0.074	-0.064	-0.081*
	(0.056)	(0.035)	(0.041)
Modern PDMP-Mandate	-0.040	-0.014	-0.044
	(0.059)	(0.040)	(0.059)
Pill Mill	-0.220**	-0.165**	-0.156*
	(0.052)	(0.042)	(0.085)
P-value of F-Test of Joint Significance	0.00	0.00	0.04
Baseline Opioid * Year Effects	No	Yes	Yes
State Linear Trend	No	No	Yes
Number of Observations	765	765	765

See notes to Table 1.

Appendix Table 4. Estimates of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Retail Opioid Prescriptions, By Urban/Rural Status and By Baseline Opioid Prescriptions, 2002-2016
Model Specification: State and year fixed effects; age controls; and interactions between baseline opioid sales and year

	Log Grams MEG			
All Opioids	All Counties	Urban Counties	Mostly Rural	Rural Counties
Electronic PDMP	0.012 (0.024)	0.003 (0.028)	0.042* (0.021)	0.045 (0.031)
Modern PDMP	-0.044 (0.037)	-0.056 (0.037)	0.060** (0.025)	0.049 (0.041)
Pill Mill	-0.126** (0.033)	-0.083** (0.034)	-0.089** (0.031)	-0.042 (0.038)
P-value of F-Test of Joint Significance	0.00	0.05	0.01	0.30
Hydrocodone/Oxycodone				
Electronic PDMP	0.015 (0.023)	0.006 (0.026)	0.041 (0.031)	0.073** (0.024)
Modern PDMP	-0.067* (0.036)	-0.086** (0.037)	0.051* (0.028)	0.097** (0.032)
Pill Mill	-0.149** (0.040)	-0.178** (0.035)	-0.108** (0.049)	-0.054 (0.043)
P-value of F-Test of Joint Significance	0.02	0.00	0.09	0.01
Baseline Opioid * Year Effects	Yes	Yes	Yes	Yes
State Linear Trend	No	No	No	No
Number of Observations	765	764	675	615

See notes to Table 3.

Appendix Table 5. Estimates of Effects of State Polices on Mortality Rates from All Causes, 2002 to 2016
Basic Model Specification

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Electronic PDMP, Lag 1	-0.455	0.073	-0.593	2.031	-0.936	0.789	-5.411	-7.970
	(0.885)	(2.359)	(1.605)	(2.843)	(2.777)	(3.535)	(5.675)	(8.366)
Modern PDMP, Lag 1	-2.073**	-2.065	-2.357	-4.354	-1.035	-1.589	-10.05*	-8.323
	(0.648)	(2.578)	(2.100)	(3.391)	(3.683)	(4.925)	(5.368)	(9.122)
Pill Mill, Lag 1	-1.505	-4.208	-0.906	-5.797	2.252	-7.005	25.31**	19.663
	(2.007)	(4.089)	(3.745)	(7.169)	(6.366)	(9.664)	(10.574)	(16.022)
Mean of Dependent Variable	45.436	128.644	68.273	150.314	178.158	294.529	559.771	921.273
Number of Observations	763	765	765	765	765	765	765	765

Notes: The unit of observation is the state-year mortality rate for the demographic groups listed in the tables. All regression models include state and year fixed effects and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. ** $p < 0.05$

Appendix Table 6. Estimates of Effects of State Polices on Mortality Rates from All Causes, 2002 to 2016
Model with State-specific Trends

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males		Females	Males	Females
Electronic PDMP, Lag 1	-0.632	2.926	0.122	1.959	1.610	1.186	-1.994	-0.135
	(0.742)	(1.816)	(0.900)	(1.796)	(1.122)	(2.505)	(2.530)	(3.292)
Modern PDMP, Lag 1	-1.443*	0.980	0.369	0.096	3.604**	3.146	-6.432**	-0.897
	(0.798)	(2.573)	(1.197)	(2.458)	(1.541)	(2.574)	(2.389)	(3.718)
Pill Mill, Lag 1	1.178	-0.372	0.248	-4.438	1.249	-4.735	6.325*	1.179
	(1.516)	(3.583)	(1.675)	(4.594)	(1.835)	(3.072)	(3.359)	(8.761)
Mean of Dependent Variable	45.436	128.644	68.273	150.314	178.158	294.529	559.771	921.273
Number of Observations	763	765	765	765	765	765	765	765

Appendix Table 7: Event-study Estimates of Effects of State Polices on All-cause Mortality Rates, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Modern PDMP, t-4	2.998**	3.461	2.747	3.822	3.725	7.939	11.160**	11.820
	(1.355)	(3.922)	(2.014)	(4.226)	(4.026)	(6.101)	(3.751)	(7.369)
Modern PDMP, t-3	0.937	0.965	1.285	3.934	-0.836	0.143	3.395	1.550
	(1.167)	(2.965)	(1.587)	(3.172)	(2.376)	(3.547)	(3.235)	(5.387)
Modern PDMP, t-2	1.672	-0.599	2.041	-0.257	-0.211	0.442	7.898**	7.864*
	(1.264)	(1.874)	(1.356)	(2.000)	(1.653)	(2.408)	(2.879)	(4.063)
Modern PDMP, t	-0.945	0.011	1.149	0.261	0.417	1.259	-3.875	2.738
	(1.011)	(1.807)	(1.464)	(2.111)	(1.528)	(2.363)	(2.430)	(4.121)
Modern PDMP, t+1	0.325	0.411	0.913	0.440	2.816	1.422	-0.355	2.282
	(0.957)	(2.558)	(1.369)	(2.165)	(2.041)	(2.659)	(2.879)	(3.958)
Modern PDMP, t+2	0.380	-1.014	0.904	-2.363	2.912	3.948	-1.148	4.856
	(1.291)	(3.048)	(1.536)	(2.942)	(2.807)	(4.126)	(2.547)	(4.132)
Modern PDMP, t+3	0.158	0.444	3.221	1.589	2.507	7.578	0.156	3.972
	(1.270)	(3.670)	(2.071)	(4.741)	(3.204)	(4.952)	(3.735)	(6.293)
Modern PDMP, t+4	0.303	0.412	5.881**	3.017	10.691**	12.509	12.478**	27.175**
	(1.884)	(4.921)	(2.495)	(5.546)	(5.003)	(8.172)	(5.217)	(8.506)
Pill Mill, t-4	1.341	1.867	-0.134	-3.961	-5.399	-4.792	-27.725**	-17.273
	(2.950)	(7.371)	(2.934)	(5.709)	(6.518)	(9.033)	(6.114)	(12.881)
Pill Mill, t-3	-2.742	3.258	3.525	3.790	-3.277	-0.816	-8.957	-4.435
	(1.916)	(5.761)	(2.949)	(3.534)	(3.474)	(3.628)	(6.336)	(8.021)
Pill Mill, t-2	0.861	1.841	3.786**	3.441	-1.306	-1.483	-3.491	3.863
	(1.733)	(3.605)	(1.351)	(3.221)	(3.855)	(4.294)	(4.798)	(6.222)
Pill Mill, t	-0.804	-1.802	1.491	-7.661**	-0.989	-8.073**	5.677	4.223
	(1.894)	(3.447)	(1.636)	(3.201)	(2.623)	(3.788)	(5.129)	(6.756)
Pill Mill, t+1	0.554	-3.842	-0.703	-4.979	-1.713	-6.388	5.075	3.697
	(1.803)	(2.998)	(1.701)	(5.044)	(3.123)	(4.047)	(4.972)	(9.376)
Pill Mill, t+2	-0.106	2.079	-0.095	-8.211**	-2.259	-7.207	2.474	13.967
	(2.124)	(3.829)	(2.181)	(3.570)	(2.920)	(5.940)	(6.534)	(9.180)
Pill Mill, t+3	-2.106	3.959	-0.857	-2.441	1.639	-8.598	6.701	10.061
	(1.758)	(6.277)	(2.885)	(4.090)	(4.361)	(5.171)	(5.943)	(10.841)
Pill Mill, t+4	-1.662	-7.981	-1.522	-15.357**	-1.970	-14.847*	14.302**	18.634**
	(1.502)	(7.415)	(3.401)	(7.040)	(4.733)	(7.558)	(6.736)	(8.018)
Mean of Dependent Variable	45.436	128.644	68.273	150.314	178.158	294.529	559.771	921.273
p-value, F-test Modern	0.095	0.672	0.365	0.337	0.327	0.260	0.003	0.026
p-value, F-test Pill Mill	0.010	0.925	0.020	0.187	0.487	0.960	0.000	0.034
Number of Observations	763	765	765	765	765	765	765	765

See notes to Table 4. F-test is a test that pre-policy interaction effects are jointly zero.

Appendix Table 8

Estimates of Effects of State Polices on Mortality Rates from All Causes in Urban Areas, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Electronic PDMP, Lag 1	-1.066	-0.548	-1.117	1.273	-0.833	-0.953	-4.236	-6.313
	(0.949)	(2.539)	(1.154)	(2.486)	(1.978)	(3.129)	(3.865)	(5.644)
Modern PDMP, Lag 1	-2.714**	-1.909	-0.693	-2.066	2.493	2.148	-6.249	3.464
	(0.988)	(4.065)	(2.017)	(4.230)	(2.868)	(5.368)	(3.787)	(6.376)
Pill Mill, Lag 1	-1.737	-3.921	-1.272	-7.346	1.506	-8.512	21.474**	18.242
	(2.066)	(4.514)	(2.848)	(6.069)	(4.502)	(8.143)	(5.979)	(11.218)
Mean of Dependent Variable	45.436	128.644	68.273	150.314	178.158	294.529	559.771	921.273
Number of Observations	705	761	723	759	765	765	765	765

See notes to Table 4. Urban areas are defined as being one of the four metropolitan categories (large central metro, large fringe metro, medium metro, and small metro) under the 2013 NCHS Urban–Rural Classification Scheme. Sample sizes change because units of observation with less than 10 deaths are not reported publicly.

Appendix Table 9

Estimates of Effects of State Polices on Mortality Rates from All Causes in Rural Areas, 2002 to 2016

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Females	Males	Females	Males	Females	Males	Females	Males
Electronic PDMP, Lag 1	-2.314	-0.728	-0.568	-0.338	-1.457	0.578	-8.856	-11.414
	(1.674)	(3.159)	(2.417)	(4.187)	(3.591)	(4.453)	(6.519)	(8.144)
Modern PDMP, Lag 1	-1.915	-5.756*	1.396	-2.780	0.490	-3.166	0.632	-7.519
	(2.008)	(3.406)	(2.970)	(4.965)	(4.714)	(5.896)	(5.987)	(8.882)
Pill Mill, Lag 1	-0.539	-6.217	-5.497	-6.876	0.788	-2.289	34.104**	23.791*
	(3.165)	(4.685)	(3.659)	(7.358)	(6.470)	(9.217)	(6.859)	(12.776)
Mean of Dependent Variable	45.436	128.644	68.273	150.314	178.158	294.529	559.771	921.273
Number of Observations	593	683	635	684	705	705	705	705

See notes to Table 4. Rural areas are defined as being one of the two nonmetropolitan categories (micropolitan, noncore) under the 2013 NCHS Urban–Rural Classification Scheme.

Appendix Table 10: Estimates of Effects of State Polices on Probability of Being Employed at Time of Survey, 2006 to 2016
Sample of Persons in Urban Areas

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.004	-0.002	-0.004*	-0.001	-0.001	0.001	0.001	0.000
	(0.005)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.006	-0.013**	-0.003	-0.003	-0.003	-0.001	-0.002	-0.004
	(0.005)	(0.006)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.006	0.009	0.003	0.002	0.000	0.001	-0.000	0.003
	(0.007)	(0.007)	(0.003)	(0.006)	(0.004)	(0.003)	(0.003)	(0.002)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.005	-0.004	-0.002	0.001	0.001	0.000	0.003*	0.001
	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.003	-0.008	-0.001	0.002	0.000	-0.000	0.001	-0.001
	(0.005)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.006	0.006	0.003	0.005	0.001	0.002	0.002	0.005*
	(0.005)	(0.005)	(0.003)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.004	0.002	-0.001	0.001	-0.000	-0.001	0.003	0.000
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Modern PDMP, Lag 1	0.004	0.000	0.000	0.003	0.000	-0.001	0.001	-0.001
	(0.006)	(0.005)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Pill Mill, Lag 1	0.006	-0.001	0.004	0.007	0.001	0.009**	0.004	0.001
	(0.006)	(0.005)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)
Mean of Dependent Variable	0.598	0.606	0.713	0.824	0.718	0.842	0.622	0.718
Number of Observations	15,893	15,992	17,750	17,855	29,291	29,478	28,525	28,505

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Appendix Table 11: Estimates of Effects of State Polices on Probability of Being Employed at Time of Survey, 2002 to 2016
Sample of Persons with 12 or Fewer Years of Education

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.007	-0.003	-0.011*	-0.005	0.001	0.004	-0.002	0.001
	(0.004)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Modern PDMP, Lag 1	-0.011*	-0.014**	-0.006	-0.006	-0.001	0.004	-0.004	-0.001
	(0.005)	(0.006)	(0.007)	(0.007)	(0.004)	(0.006)	(0.004)	(0.003)
Pill Mill, Lag 1	0.009	0.014	0.005	0.004	0.001	0.004	0.001	0.005
	(0.010)	(0.010)	(0.008)	(0.009)	(0.007)	(0.009)	(0.005)	(0.005)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.009*	-0.007	-0.013**	-0.007	0.002	0.003	-0.004	-0.001
	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.004)
Modern PDMP, Lag 1	-0.008	-0.016**	-0.012**	-0.011*	-0.003	-0.000	-0.006**	-0.004
	(0.006)	(0.007)	(0.004)	(0.006)	(0.003)	(0.005)	(0.003)	(0.004)
Pill Mill, Lag 1	0.007	0.014	0.010	0.006	0.002	0.006	0.002	0.005
	(0.009)	(0.008)	(0.006)	(0.008)	(0.005)	(0.006)	(0.003)	(0.005)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.002	0.003	-0.005	0.002	0.002	0.009**	-0.003	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)
Modern PDMP, Lag 1	-0.006	-0.010	-0.005	-0.003	-0.003	0.003	-0.003	0.000
	(0.006)	(0.007)	(0.004)	(0.006)	(0.003)	(0.005)	(0.003)	(0.004)
Pill Mill, Lag 1	0.012*	0.009	0.019**	0.014*	0.003	0.009*	-0.006	0.001
	(0.007)	(0.010)	(0.004)	(0.008)	(0.004)	(0.005)	(0.004)	(0.004)
Mean of Dependent Variable	0.502	0.584	0.556	0.749	0.618	0.761	0.510	0.620
Number of Observations	21,828	22,402	23,800	24,342	39,767	40,281	38,828	38,381

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** p <= 0.05

Appendix Table 12: Estimates of Effects of State Polices on Probability of Working 48 or more Weeks Past 12 Months, 2006 to 2016
Sample of Persons in Urban Areas

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.004	-0.007	-0.006**	-0.001	-0.002	0.000	0.000	-0.001
	(0.004)	(0.005)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Modern PDMP, Lag 1	-0.007	-0.017**	-0.005	-0.005	-0.004	-0.004	-0.003	-0.006*
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)
Pill Mill, Lag 1	0.011	0.009	0.005	0.003	0.002	-0.000	0.005	0.003
	(0.007)	(0.008)	(0.004)	(0.006)	(0.005)	(0.004)	(0.003)	(0.003)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.004	-0.005	-0.004	0.002	-0.001	0.000	0.003*	-0.001
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Modern PDMP, Lag 1	-0.004	-0.009**	-0.003	0.002	-0.001	-0.001	0.001	-0.003
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Pill Mill, Lag 1	0.010**	0.010**	0.004	0.005	0.003	0.000	0.007**	0.005**
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.001)	(0.002)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.006*	0.004	-0.001	0.001	-0.001	-0.002	0.004*	-0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
Modern PDMP, Lag 1	0.003	-0.000	-0.002	0.002	-0.000	-0.002	0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Pill Mill, Lag 1	0.002	-0.001	0.005	0.005	0.003	0.006	0.007*	-0.001
	(0.007)	(0.006)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Mean of Dependent Variable	0.460	0.474	0.650	0.775	0.665	0.807	0.583	0.686
Number of Observations	15,893	15,992	17,750	17,855	29,291	29,478	28,525	28,505

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** p <= 0.05

Appendix Table 13: Estimates of Effects of State Polices on Probability of Working 48 or more Weeks Past 12 Months, 2002 to 2016
Sample of Persons with 12 or Fewer Years of Education

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.005	-0.005	-0.012**	-0.005	-0.001	0.003	-0.003	0.000
	(0.004)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)
Modern PDMP, Lag 1	-0.006	-0.015**	-0.009	-0.009	-0.002	-0.000	-0.004	-0.004
	(0.005)	(0.006)	(0.007)	(0.008)	(0.004)	(0.006)	(0.003)	(0.004)
Pill Mill, Lag 1	0.016*	0.020*	0.008	0.012	0.004	0.008	0.004	0.007
	(0.009)	(0.012)	(0.010)	(0.011)	(0.008)	(0.009)	(0.005)	(0.007)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.007	-0.008	-0.015**	-0.007	-0.001	0.002	-0.005*	-0.002
	(0.004)	(0.006)	(0.005)	(0.007)	(0.003)	(0.005)	(0.003)	(0.004)
Modern PDMP, Lag 1	-0.004	-0.015**	-0.013**	-0.016**	-0.003	-0.002	-0.006**	-0.007
	(0.005)	(0.006)	(0.005)	(0.007)	(0.004)	(0.005)	(0.003)	(0.004)
Pill Mill, Lag 1	0.014*	0.018*	0.010	0.014	0.004	0.010	0.006**	0.006
	(0.008)	(0.010)	(0.008)	(0.010)	(0.006)	(0.007)	(0.003)	(0.006)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.003	-0.000	-0.005	0.003	0.000	0.008**	-0.004	0.002
	(0.004)	(0.006)	(0.005)	(0.006)	(0.002)	(0.004)	(0.003)	(0.004)
Modern PDMP, Lag 1	-0.005	-0.012*	-0.006	-0.009	-0.003	0.002	-0.004	-0.001
	(0.005)	(0.007)	(0.005)	(0.007)	(0.003)	(0.006)	(0.002)	(0.004)
Pill Mill, Lag 1	0.010	0.010	0.009**	0.011	-0.000	0.008	-0.005	-0.002
	(0.006)	(0.010)	(0.004)	(0.009)	(0.004)	(0.006)	(0.003)	(0.005)
Mean of Dependent Variable	0.372	0.459	0.493	0.698	0.570	0.722	0.479	0.590
Number of Observations	21,828	22,402	23,800	24,342	39,767	40,281	38,828	38,381

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Appendix Table 14: Estimates of Effects of State Polices on Earned Income (2010 dollars in $1,000) in Past 12 Months, 2006 to 2016
Sample of Persons in Urban Areas

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.268	-0.215	-0.340	-0.627	0.003	-0.072	0.184	-0.160
	(0.170)	(0.249)	(0.204)	(0.511)	(0.259)	(0.478)	(0.188)	(0.395)
Modern PDMP, Lag 1	-0.374**	-0.578**	-0.374	-0.769	-0.429	-0.265	-0.221	-0.851**
	(0.155)	(0.216)	(0.252)	(0.460)	(0.297)	(0.340)	(0.303)	(0.413)
Pill Mill, Lag 1	0.294	0.357	0.131	0.626	-0.362	-0.087	0.036	0.285
	(0.260)	(0.384)	(0.390)	(0.938)	(0.496)	(0.882)	(0.377)	(0.752)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.171	-0.090	-0.190	-0.318	0.394*	0.195	0.574**	0.172
	(0.155)	(0.167)	(0.199)	(0.365)	(0.234)	(0.401)	(0.194)	(0.377)
Modern PDMP, Lag 1	-0.194	-0.210	-0.096	-0.174	0.032	0.240	0.267	-0.442
	(0.146)	(0.183)	(0.264)	(0.442)	(0.290)	(0.411)	(0.225)	(0.514)
Pill Mill, Lag 1	0.237	0.563**	0.102	0.988*	-0.120	0.398	0.339*	0.750*
	(0.179)	(0.204)	(0.285)	(0.574)	(0.301)	(0.470)	(0.190)	(0.438)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.061	0.155	-0.182	-0.433**	0.107	-0.223	0.403**	-0.273
	(0.129)	(0.127)	(0.172)	(0.200)	(0.201)	(0.331)	(0.162)	(0.302)
Modern PDMP, Lag 1	-0.003	0.148	-0.120	-0.274	-0.140	0.205	0.250	-0.737*
	(0.112)	(0.122)	(0.208)	(0.246)	(0.280)	(0.270)	(0.211)	(0.400)
Pill Mill, Lag 1	-0.012	0.145	0.385**	0.827**	0.407	1.141**	0.839**	0.981*
	(0.251)	(0.227)	(0.185)	(0.315)	(0.274)	(0.476)	(0.334)	(0.499)
Mean of Dependent Variable	9.224	11.334	24.522	35.440	31.033	55.458	27.276	50.990
Number of Observations	15,893	15,992	17,750	17,855	29,291	29,478	28,525	28,505

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Appendix Table 15: Estimates of Effects of State Polices on Earned Income (2010 dollars in $1,000) in Past 12 Months, 2002 to 2016
Sample of Persons with 12 or Fewer Years of Education

	Age 18-25		Age 26-34		Age 35-49		Age 50-64	
	Females	Males	Females	Males	Females	Males	Females	Males
Basic Model								
Electronic PDMP, Lag 1	-0.145	-0.363	-0.257	-0.772*	-0.065	-0.128	-0.108	-0.299
	(0.132)	(0.250)	(0.208)	(0.411)	(0.174)	(0.403)	(0.166)	(0.386)
Modern PDMP, Lag 1	-0.263**	-0.596**	-0.284	-0.750**	-0.309*	-0.267	-0.323**	-0.532*
	(0.124)	(0.191)	(0.242)	(0.323)	(0.183)	(0.374)	(0.121)	(0.302)
Pill Mill, Lag 1	0.443*	0.837**	0.328	0.918	0.124	1.040	0.107	0.686
	(0.254)	(0.403)	(0.345)	(0.735)	(0.280)	(0.752)	(0.194)	(0.646)
Model with Baseline Opioid Interactions								
Electronic PDMP, Lag 1	-0.173	-0.422*	-0.313*	-0.865**	-0.068	-0.210	-0.153	-0.334
	(0.121)	(0.239)	(0.173)	(0.425)	(0.147)	(0.398)	(0.115)	(0.330)
Modern PDMP, Lag 1	-0.184	-0.690**	-0.394**	-0.911**	-0.338**	-0.273	-0.451**	-0.647*
	(0.122)	(0.211)	(0.182)	(0.366)	(0.165)	(0.382)	(0.125)	(0.323)
Pill Mill, Lag 1	0.390*	0.920**	0.438*	1.137*	0.162	1.193*	0.165	0.645
	(0.218)	(0.360)	(0.256)	(0.676)	(0.263)	(0.624)	(0.152)	(0.473)
Model with State-specific Trends								
Electronic PDMP, Lag 1	0.042	-0.020	-0.013	-0.077	-0.096	0.146	-0.092	-0.132
	(0.106)	(0.183)	(0.122)	(0.310)	(0.147)	(0.279)	(0.125)	(0.263)
Modern PDMP, Lag 1	-0.151	-0.439**	-0.223	-0.362	-0.285	0.007	-0.370**	-0.324
	(0.115)	(0.204)	(0.156)	(0.334)	(0.185)	(0.356)	(0.136)	(0.405)
Pill Mill, Lag 1	0.337**	0.285	0.388**	0.938**	0.137	0.529	-0.141	0.355
	(0.140)	(0.358)	(0.172)	(0.289)	(0.284)	(0.494)	(0.168)	(0.378)
Mean of Dependent Variable	6.298	10.297	11.634	22.988	15.748	30.252	13.779	26.157
Number of Observations	21,828	22,402	23,800	24,342	39,767	40,281	38,828	38,381

Notes: The unit of observation is the state-year-group (age-by-race/ethnicity). All regression models include state and year fixed effects, dummy variables for each year of age and dummy variables for race/ethnicity (non-Hispanic white, non-Hispanic black, Hispanic, and non-Hispanic other), and the state policies indicated in table. Standard errors have been constructed allowing for non-independence of observations within a state. * 0.05 < p-value<=0.10, ** *p* <= 0.05

Figure 1. Frequency of PDMP Policies Across all 51 States over Time

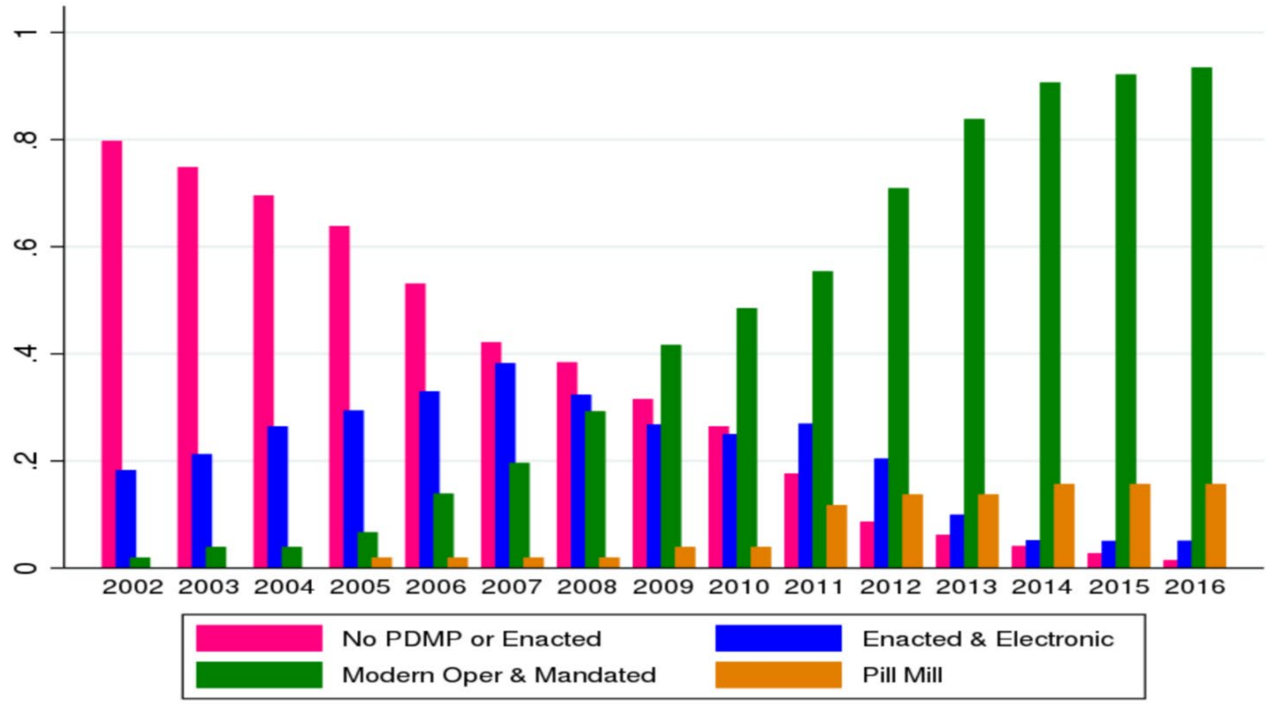
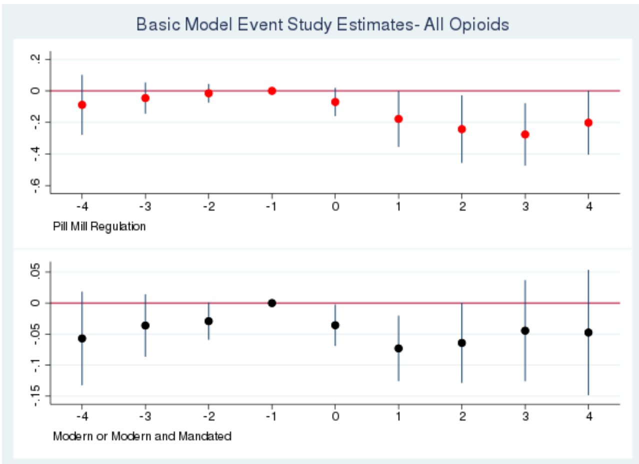
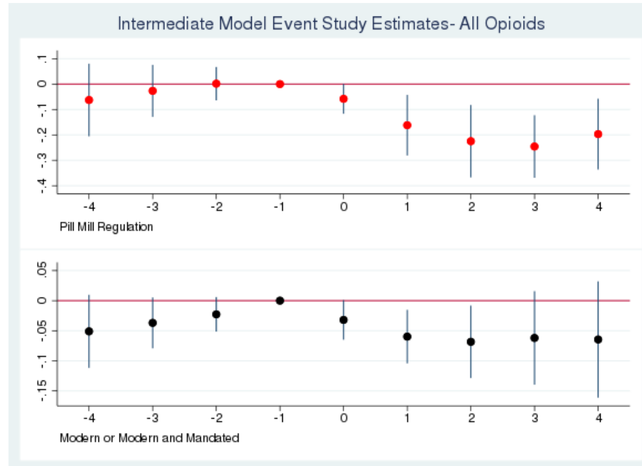


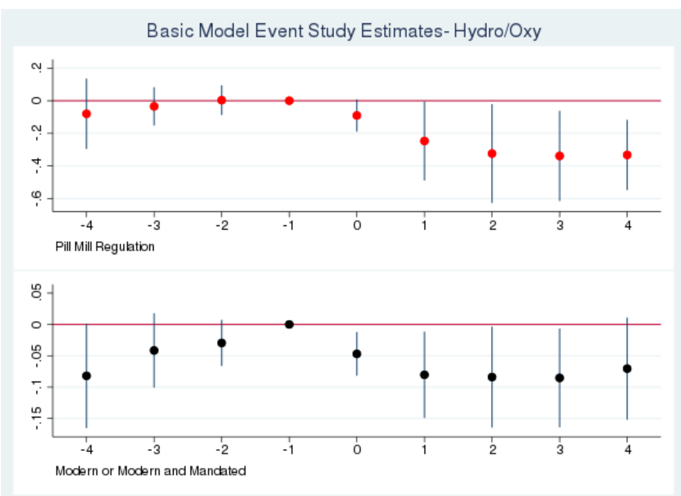
Figure 2. Event Study Coefficients of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Logarithm Retail Opioid Prescriptions 2002-2016

A. All Drugs





B. Hydrocodone/Oxycodone



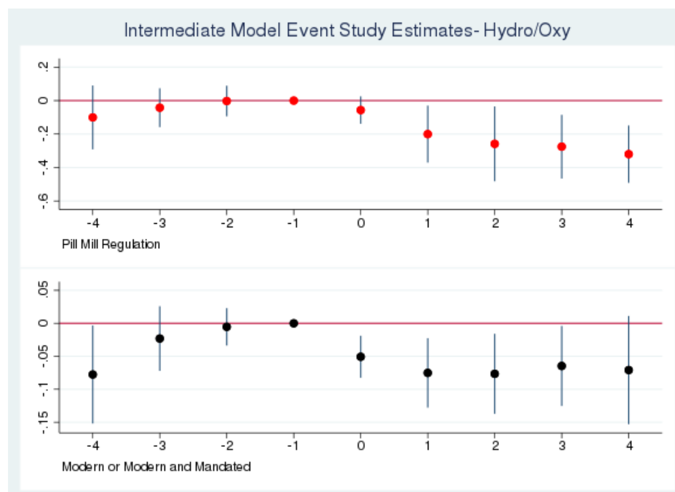
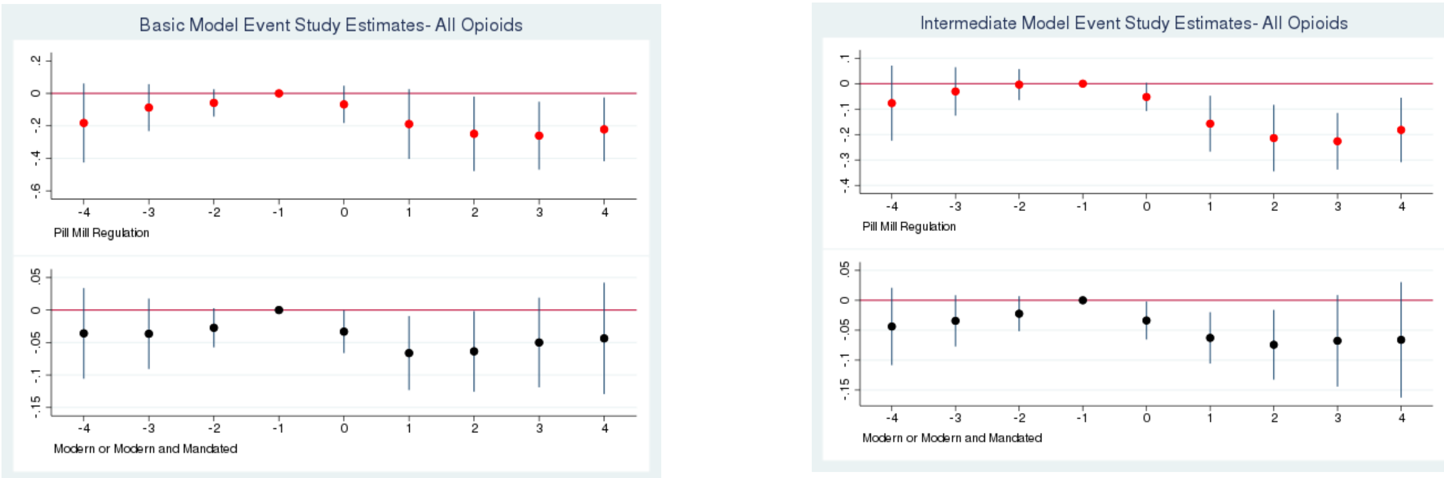
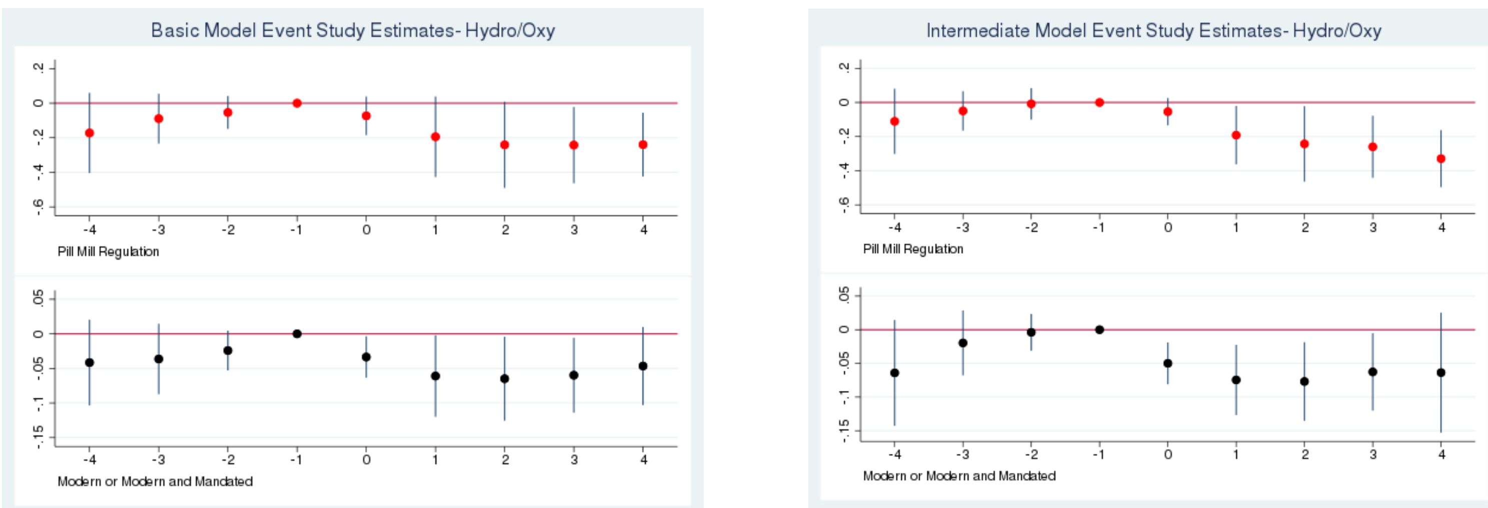


Figure 3. Event Study Coefficients of the Effect of Prescription Drug Monitoring Programs (PDMPs) on Per Capita Retail Opioid Prescriptions 2002-2016

A. All Drugs



B. Hydrocodone/Oxycodone



Appendix Figure 1. Frequency of PDMP Policies Across all 51 States over Time