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Abstract

We examine how the availability of prescription opioids affects labor market activity and household economic well-being. While greater access to opioids may lead people to substance use disorders and negative economic consequences, appropriate pain medication may allow some individuals to effectively participate in the labor market. We study prescription drug monitoring programs (PDMPs), which were designed to curb inappropriate opioid prescribing and assess how these policies affected labor force attachment and credit outcomes. We use variation across states in the timing of implementation of PDMPs and recent methods developed for difference-in-difference event study designs with multiple time periods. In line with previous work, we find that PDMPs lead to a decline in opioid prescribing rates. Although we find that these reductions in opioid supply have no clear effect on measures of labor force activity in our pooled sample, we see some suggestive evidence of negative effects on labor force attachment in states where there is less scope for substitution to illicit drugs. We also show that PDMPs lead to a decline in credit scores and increases in the number and amount of third-party debt collections, and that these effects are more pronounced in states lacking an illicit market. These findings suggest that some individuals are likely negatively affected by the lack of access to pain medication due to the PDMP laws.

Keywords: Opioids, prescription drug monitoring programs, labor force participation, disability insurance, collections, credit scores

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1. Introduction

Prescription opioid abuse continues to be a major public health concern in the U.S. even as the nature of the opioid epidemic has shifted towards illicitly manufactured synthetic drugs like fentanyl, often serving as gateway drugs initiating trajectories of substance use (Jones, 2013; Muhuri, Gfroerer & Davies, 2013).[1] Over 16,000 individuals died from overdoses involving prescription opioids in 2020; this represented an increase of over 16 percent compared to 2019. (CDC, 2022). Some have raised concerns that the opioid "epidemic" has had important economic effects and has contributed to the decline in labor force participation in recent decades (Hollingsworth et. al, 2017; and Krueger, 2018). Indeed, research suggests that areas with more prescribers per capita, and where relatively more opioid pain medication is prescribed, have lower rates of labor force participation (Ruhm, 2018; Krueger, 2018). At the same time, however, there is a prominent literature that has suggested that causality may run in the other direction, namely that poor economic conditions have set the stage for the opioid epidemic, which has led to "deaths of despair".[2]

Thus far, there is only limited and mixed evidence on the *causal* effects of opioid prescriptions on labor market outcomes, and no evidence that we are aware of on household financial outcomes. Using quasi-experimental methods, a few recent studies examine whether greater availability of prescription opioids has led to lower rates of labor force engagement. Harris et. al (2019) use variation in the concentration of high-volume opioid prescribers to the Medicare population as an instrument for the supply of prescription opioids at the county level. They find that a 10 percent increase in prescriptions causes a 0.56 percentage point reduction in labor force participation. Aliprantis et al (2019) also find declines in labor force participation in high prescription counties using differences in differences and using a control group of counties with

[1] Nearly 80% of recent heroin misusers report using prescription opioids nonmedically before initiating heroin use (Jones, 2013; Muhuri, Gfroerer & Davies, 2013). Results from the 2017 National Survey on Drug Use and Health indicate that nearly 93% of opioid misusers exclusively misuse prescription opioids and only 3% exclusively misuse heroin. But overdose mortality rate is much higher among heroin users. About 63% of heroin users also misuse prescription opioids (SAMHSA, 2018).

[2] For example, economic downturns may lead to declines in mental health (Ruhm, 2015; Charles and DeCicca, 2008; Modrek et al., 2015). This in turn could lead to high rates of addiction (Carpenter et. al, 2017) and deaths of despair" (Case and Deaton, 2015). Some research has argued that these effects are concentrated among working-age white males with low educational attainment (Case and Deaton, 2015; Hollingsworth et. al, 2017; Carpenter et. al, 2017). Case and Deaton (2017) also attribute growing addiction to factors like increasing instability in marriages and jobs, stagnation of real wages, and the decline of organized religion. Pierce and Schott (2020) and Venkataramani et al (2020) also link job loss to opioid abuse using quasi-experimental research designs.

similar economic characteristics. In contrast, Currie and Schnell (2018), using a very similar approach to Harris et al. (2019), find a small, *positive* effect of opioids on employment to-population ratios for women, but no relationship for men. Currie and Schnell interpret their findings as supportive of the important analgesic role of prescription opioids as it allows some women to continue to work who would otherwise leave the labor force.

Notably, these studies have only utilized variation during the run-up of the opioid crisis and have not considered how *reductions in opioid prescribing behavior might affect labor market outcomes*. This is important for at least two reasons. First, it is possible that there are important asymmetries in how greater versus lesser availability of prescription drugs may affect the labor market. This might be the case, for example, because it may not be so easy to reverse addictive behavior as it is to initiate it. Second, this is important from a policy perspective as federal and state agencies have responded to the opioid epidemic by implementing several regulations to limit the excessive supply of prescription opioids for misuse.

One particularly important policy effort is the establishment and tightening of statewide prescription drug monitoring programs (PDMPs). These laws are designed to reduce inappropriate prescribing of opioids and decrease the initiation and sustenance of opioid use disorders.[3] So called 'must-access' PDMPs mandate that providers review the patient's previous history of opioid prescriptions to help identify patients at "high risk" for suspected misuse, diversion, and doctor shopping before any new opioid prescribing can occur. Several studies suggest that these laws are particularly effective in reducing opioid prescriptions and non-medical opioid use (Buchmueller and Carey, 2018; Meinhofer, 2018; Sacks et. al, 2019, Patrick et. al, 2016; Wen et. al, 2019).

It is not obvious, *a priori*, whether reductions in opioid prescriptions from PDMPs should lead to increases or decreases in aggregate labor market participation as there are multiple channels that could be affected. First, the decline in supply of prescription opioids could lead non-medical users to seek treatment for substance use disorders (SUDs), which in turn could lead them to return to the labor market. Dave et. al, 2018, find some evidence in support of this mechanism among young adults (ages 18-24), who have had the highest rates of

[3]Other notable regulations aimed to reduce excessive opioid prescribing include the 2010 deterrence reformulation of the frequently misused prescription opiate Oxycontin, day limit laws that restrict the length of initial prescriptions and up-scheduling of frequently misused drugs, like hydrocodone, by the Drug Enforcement Agency to control prescribing.

substance use disorders.[4] Considering labor market impacts of other policy actions that reduce the supply of prescription opioids, Beheshti (2022) reports an 0.8% increase in labor force participation rates following a 22% decline in distribution of hydrocodone, the most prescribed opioid and comprising roughly 46% of total US opioid sales since 2012 (IMS Health, 2012), following tighter federal Controlled Substance Act restrictions on its prescribing. Alpert et al (2022) find that assignment to a high-intensity opioid prescribing physician among active-duty members of the military led to behavioral problems and lower job performance and productivity and higher separations. This literature would suggest that PDMPs could improve labor market participation.

Second, the decline in prescription opioid supply could lead some users to substitute toward more dangerous illicit drugs like heroin and fentanyl. Recent studies find evidence of such behavior (Alpert, Powell and Pacula, 2018; Evans, Lieber and Power, 2019, Park and Powell, 2021; Gupta, Al Achkar and Ray, 2021). The possibility of substitution is particularly compelling given the lack of adequate access to SUD treatments (Jones et. al, 2015; Rosenblatt et. al, 2015). For those users who switch to more dangerous drugs after PDMP laws are enacted, we might expect to see no change in labor force participation, or perhaps a reduction in active participation.

Third, stricter opioid prescribing laws may unintendedly restrict access to adequate pain management among legitimate medical users, forcing them to withdraw from active engagement in the labor market. Indeed, previous studies have found declines in labor force participation rates following the market withdrawal of a widely used pain medication (Garthwaite, 2012; Butikofer and Skira 2017). Similarly, Nicholas and Maclean (2019) found that legalization of medical marijuana for pain management among the near elderly led to greater labor force participation. On the other hand, this negative effect could be alleviated to some extent through access to opioid drugs on the illicit market. Thus, we may see heterogeneity in effects depending on the degree to which workers are able to substitute if they lose access to prescription opioids.

Therefore, given the multiple channels by which PDMPs could affect labor market activity and the possibility that the effects could go in different directions for different populations, the aggregate effect of PDMPs on the labor market is ultimately an empirical question.

[4] Specifically, Dave et al (2018) find that mandatory-PDMPs are followed by an immediate, initial increase in the uptake of treatment for SUDs, and then a long-term significant reduction in treatment. The authors interpret the subsequent decline in treatment rates as evidence of the success of PDMPs in lowering long-run rate of opioid use disorders requiring treatment.

Similarly, there may be multiple ways in which these laws could affect the financial situation of families. If these laws reduce SUDs then they could contribute to improving the economic well-being of families both by improving labor market outcomes and by removing the negative financial consequences that might be associated with SUDs. Alternatively, if they reduce medically necessary access to opioid drugs or lead some users to shift to more illicit and dangerous drugs then these laws could contribute to worse financial outcomes in the aggregate. Again, this is ultimately an empirical question.

Our study uses variation across states in the timing of the implementation of must access PDMPs based on a difference-in-difference estimator introduced by Callaway and Sant'anna (2021). We first confirm that these laws have in fact, led to reduced opioid prescribing using data from the Center for Disease Control (CDC). Specifically, we find that five years after state PDMP laws are enacted, there is a greater than 10 percent decline in opioid prescribing. This is consistent with previous work.

We then turn to examining measures of labor market engagement for the working-age population. We use four main measures: the employment to population ratio, the labor force participation rate, the unemployment rate, and usual hours weekly hours worked. These are state-level measures culled from the Current Population Survey (CPS) covering 2009 to 2019. When we look at our samples that combine all states, we do not find any meaningful effects on any of these outcomes either for the full sample or for subsamples defined by gender or ageI groups. However, when we stratify our samples by a proxy for the presence of an illicit market for opioid drugs, we see highly suggestive evidence of a negative effect on labor market participation in the states with less developed illicit drug markets, where there is presumably much less scope for substitution to other forms of opioids.

We also evaluate the effects of PDMP laws on the recipiency of disability insurance programs provided by the Social Security Administration (SSA). Specifically, we examine supplemental security income (SSI) and disability insurance (SSDI) and the share of social security disability awards for musculoskeletal diseases (SSDImd). We again do not find any clear evidence of changes related to PDMP laws.

Finally, we turn to financial outcomes using credit bureau data from Equifax™. We find that after the introduction of PDMP laws there is a statistically significant and economically meaningful deterioration in some credit outcomes including credit scores and the number and amount of debt sent to third party collection agencies. Consistent with our labor market results, we also find that these negative effects are more pronounced in states with less-developed illicit markets where there is less opportunity to switch to non-prescription opioids. We also find an increase in credit card balances past due and total balances past due in states with low illicit market activity compared to states to high illicit market activity.

Overall, our results suggest that PDMP policies reduced opioid prescribing and led to some worsening of financial outcomes while having no significant effects on labor market attachment. These results could reflect differing patterns of effects on different subsets of the population and that on net, the effects on the labor markets were offsetting, while the effects on credit outcomes were worsened by especially poor outcomes for some individuals. We gain some insight into the mechanisms at play by comparing the magnitudes of effects in states where there was less cope for substitution to illicit drugs to those where there was more a more developed illicit market. Here, we find suggestive evidence of a worsening in labor market attachment in the states with a less developed illicit market where the scope for substitution was less. We also find a sharper decline in several credit outcomes in these states.

A clear implication for policy is that even if excessive opioid prescribing caused reduced individual labor market participation in the past, PDMP laws targeting inappropriate opioid prescribing do not appear to have led to greater labor market engagement and may have in fact, reduced it due to the inability of individuals to obtain adequate pain relief. Furthermore, PDMP laws appear to have negative repercussions on credit outcomes for some individuals, and these effects are especially large in areas where there are no substitutes for prescription opioids. Given the dire consequences of the use of illegal substitutes to prescription opioids, there is a clear need for policy to create safe and easier access to adequate pain medication for pain patients as well as treatments for people with substance use disorders.

2. Data

We use several different data sources for our analysis. To measure the state-level opioid prescribing rate we use the number of opioid prescriptions per 100 persons from the Center of Disease Control.[5] To measure labor market attachment we use four different measures – employment to population ratio (EPOP), labor force participation rate (LFP), unemployment rate

[5] Source https://www.cdc.gov/drugoverdose/maps/rxrate-maps.html

(UR) and hours worked last week (HOURS)[6] – from the Current Population Survey (CPS) of the Bureau of Labor Statistics, for the years 2009Q4-2019Q4 for individuals aged 25 to 64.[7] All measures are seasonally adjusted using X12 ARIMA developed by the US Census Bureau and the unit of observation is state-year-quarter. Finally, we use awards of Supplemental Security Insurance (SSI), Social Security Disability Insurance (SSDI) and the share of social security disability awards for musculoskeletal diseases (SSDImd) (range 0-100 percent) from the Social Security Administration to study changes in disability insurance following the decline in opioid prescribing.[8]

We provide the details of funding, application process and eligibility conditions for the SSI and SSDI programs in Appendix Table 1. Broadly, SSI is the primary source of disability insurance for low-income, low-asset individuals who have either never worked or who have not earned enough work credits to qualify for SSDI. SSI support is limited to a maximum of $771 per month and disqualifies recipients from performing any work. SSDI recipients receive a much more generous average pay out of $2861 per month and is available to workers who have accumulated sufficient number of work credits. Number of credits needed depends on age and accrue each year based on some threshold of income earned over the year, which is recalculated annually. Recipients can earn up to 4 credits in a year. The 2019 thresholds are $1360 for 1 credit, and $5440 for 4 credits. To be considered SSDI applicants must provide a detailed medical history and a record of every job they have had for the past 15 years. Together, SSI and SSDI capture all federal disability insurance benefits available to the full spectrum of individuals across all ages, work histories and socioeconomic status. As we are interested in work transitions in response to changes in opioid prescribing SSDI awards may be a more appropriate measure of disability rates. Finally, musculoskeletal disorders are associated with high rates of opioid use for pain management.[9] Thus, we expect changes in prescription opioid use to be particularly correlated to the share of SSDI awards granted for musculoskeletal disorders.

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[6] Results are insensitive to using 'usual hours worked', also from the CPS, as an alternative measure of labor market participation at the intensive margin. We chose 25 to 64 to be consistent with our credit market outcomes, the patterns are similar if we look at 18 to 64 year olds.

[7] Source https://cps.ipums.org/cps/. Using 18 to 64 year olds has little impact on our results, we chose to use 25 to 64 year olds to be consistent with our credit market sample, where using individuals under 25 would be highly selective.

[8] Source https://www.ssa.gov/policy/docs/statcomps/ssi_sc/ and https://www.ssa.gov/policy/docs/statcomps/di_asr/index.html

[9] Prescription opioids accounted for 18.8% of medication prescriptions for musculoskeletal chronic low back pain, over three quarters of which were for long-term use (Shmagel, Ngo and Foley, 2018). Opioid naïve patients treated for musculoskeletal pain with prescription opioids have elevated risk of

Our credit outcomes come from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax™ (CCP) credit bureau data. The CCP is a quarterly database containing a nationally representative 5% sample of credit applicants. We limit our sample to 25- to 64-year-olds observed between 2009 Q4 and 2019 Q4. We examined the following broad based measures of financial health from the CCP: the total amount of debt (excluding mortgage debt); the total amount of debt at least 30 days past due; credit card debt; credit card debt past due; the number of new third party collections (all types, including medical) in the last 12 months; the total balance of collections in the last 12 months; bankruptcy in past 24 months; and the Equifax Risk Score™ which we sometimes refer to as a "credit score".[10]

The time series of adoption dates of must-access prescription drug monitoring program laws (PDMPs) in each of the fifty US states was constructed using four established sources – Prescription Drug Abuse Policy System (pdaps.org), National Alliance for Model State Drug Laws (namsdl.org), PDMP Training and Technical Assistance Center (pdmpassist.org) and MonQcle (monqcle.com) – and are presented in Appendix Table 2. All dates were confirmed from state legislative documentations accessible from respective state PDMP sites and have been used in prior literature (Sacks et. al, 2021; Gupta et. al, 2020).

Finally, for some of our analysis we divide states by whether or not they appear to have a developed illicit opioid drug market prior to the enactment of their PDMP laws. States with a developed illicit market potentially allow individuals to substitute away from prescription opioid drugs in response to reductions in prescription opioid supply following mandatory state PDMPs. We proxy for the size of the illicit opioid market in a state by taking the ratio of the count of deaths due to illicit or synthetic opioids to deaths from prescription opioids over the period from 2007 to 2011 which is before the enactment of any PDMP laws. We obtained these counts from the CDC Multiple Cause of Death Data.[11]

Recent studies have shown that the inclusion of early and late adopters in difference-in-difference event studies in the presence of heterogeneous treatment effects, can bias results (e.g. Goodman-Bacon, 2018). This is likely less of a concern in our setting since most of our comparisons utilize states that are "never treated". Nevertheless, we directly address this by using

chronic opioid use (i.e., ≥10 prescriptions or ≥120 days supply between 91 and 365 days after the initial musculoskeletal pain diagnosis) (Moshfegh, George and Sun, 2018; George and Goode, 2020).

[10] Our choice of which variables to consider from the CCP follows that of Hu et al (2018)

[11] Multiple Cause of Death (MCOD) vital statistics data from the National Center for Health Statistics (NCHS) https://wonder.cdc.gov/mcd.html .

an estimator from Callaway and Sant'anna (2021) which is specifically designed to eliminate such biases in an efficient way.[12] We also only use PDMP mandates adopted during the June 2012-June 2016 period to ensure that we have a balanced panel of states that are observed for at least 6 quarters prior to, and 6 quarters post mandatory PDMP adoption. For most of the analysis, our observation window is 2009-2017.[13]

To get a sense of the raw data before we turn to our statistical models, Figure 1 plots the trends in prescriptions as well as for our labor market and disability insurance measures centered at the time of mandatory PDMP adoption for the subset of the states that adopted mandatory PDMPs. What is most notable is that there is a pronounced drop in prescriptions per 100 people over the next few years after PDMP adoption. Turning to labor market outcomes, there is an increase in LFP and EPOP in the quarter after adoption of the PDMPs (t= 0) but this increase is highly transitory and within the normal range of typical fluctuations. For UR there is a continuation of a downward trend that does not appear to be impacted by the state adoption of mandatory PDMPs. We also find no change in HOURS in response to the programs. Finally, we note that the rate of SSI recipients slightly declined throughout this period while the rate of SSDI recipients marginally increased. These trends appear to be unaffected by the state adoptions of mandatory PDMPs.

3. Empirical strategy

We estimate an event study model exploiting variation in must-access PDMPs across states and over time to estimate changes in opioid prescribing, rates of labor market participation and disability insurance and credit scores. Under the identifying assumption of equality of outcomes between treated and control groups in the pre-treatment period, the event study identifies the dynamic response of opioid prescribing, labor market participationlabor market participation rates and financial health measures to mandatory PDMP laws. The event study is specified as follows:

[12] Specifically, we use the CSDID estimator in STATA. For the last column of graphs in Figures 5 and 8 where we estimate the difference in effects between states with and without a developed illicit market, we bootstrap the standard errors since CSDID does not produce standard errors for this design. Specifically, we used 1000 iterations using random weights drawn from an exponential distribution. The use of random weights rather than random samples is based on Xu et al (2020).

[13] There are slight variations in the observation windows across the different analyses as described in Table 1.

$$Y_{st} = \sum \beta_{t(Must\ Access\ in\ Effect\ During\ T)_{st}} + X_{st}\delta + \gamma_s + \tau_t + \varepsilon_{st} \qquad (1)$$

Y_{st} represents our outcome variable in state s in a quarter, t. The key independent variable is an indicator for whether the state had a mandatory access PDMP operating in that quarter ($Must\ Access\ in\ Effect\ During\ T_{st}$). All specifications also include state fixed effects (γ_s) and year-quarter fixed effects (τ_t). Regressions are weighted by state population (or by population in the relevant age-subpopulation group). Heteroskedasticity robust standard errors are clustered at the state level. The quarter prior to the implementation of mandatory PDMPs, i.e. $t = -1$, is the reference period. Finally, the coefficients of ($Must\ Access\ in\ Effect\ During\ T_{st}$) for t<0 are used to test the underlying identifying assumption of equality of labor market participation rates in the treated and never-treated control states in the period prior to mandatory PDMPs. Under the identifying assumption, the coefficients of ($Must\ Access\ in\ Effect\ During\ T_{st}$) for $t \geq 0$ capture the dynamic causal effect of the state adoption of mandatory PDMPs on the rates of opioid prescribing, labor market participation, disability insurance and credit outcomes.

4. Results

We present our event study results of the impact of mandatory PDMPs graphically, in Figures 2 through 8 for our various outcomes. We note that for nearly all our outcomes and for the different subpopulations considered, states that adopted mandatory PDMPs appear to be no different statistically, from the states that did not implement these programs in the pre-treatment period, which supports the credibility of our identifying parallel pre-trend assumption.

4.1 Opioid Prescribing

We first consider the effect of the mandatory PDMPs on rates of opioid prescribing, Figure 2 plots the adjusted difference (red dot) and its 95% confidence interval (in error bars) in the yearly number of dispensed opioid prescriptions per 100 persons up to three years (or more) prior to and 5 years post state adoption of mandatory for the full population. There is a notable decline that is statistically significant in the first two years after the laws are enacted. In the subsequent three years, prescriptions appear to be similar to the pre-period but drop sharply five years after the PDMP laws are enacted. Overall, PDMPs are associated with a 2-5 percent decline in annual prescription rates. The finding of significant reductions in opioid prescribing in response to mandatory PDMPs is not new to the literature.[14] However, this replication exercise supports our research design, including the set of states mandatory PDMP laws examined and the study window.

4.2 Labor Market Attachment

Figure 3 shows the effects of mandatory PDMPs on our state-level quarterly measures of labor market attachment.[15] For this analysis we show the six or more quarters prior to and post state adoption of mandatory PDMPs. In line with the descriptive analysis presented in Section 3, we find little evidence of effects of mandatory PDMPs on measures of labor market participation at either the extensive or intensive margin. In Figure 3, we see a brief uptick in the employment to population ratio and the labor force participation rate in the quarter immediately after PDMPs are enacted but thereafter the estimates fluctuate around zero. On the other hand, we do find some suggestive evidence of an increase in the unemployment rate and a decline in HOURS in some quarters in the post-period. For example, in the periods 3 to 5 quarters after PDMPs are enacted, the UR is about 0.005 higher (off of a base rate of around 0.06). However, these effects tend to fluctuate quite a bit and are not consistent cross outcomes. Overall, we conclude that there is no clear finding of a change in labor market attachment when we consider all states pooled together.

We next consider whether these effects differ by population subgroups. For example, Currie and Schnell (2018) find that greater access to opioid prescriptions led to an increase in the employment to population ratio for women. This might suggest the possibility that we would see declines in labor market activity for women after PDMP laws restrict access. In Figure 4 we produce separate estimates for men and women. We again see only a few statistically significant effects with some suggestive evidence that if anything, perhaps men's labor market attachment

[14] In addition to the economics literature cited above, a body of medical and health services research literature has also examined the association between state PDMP adoptions and opioid prescribing (Baehren et al. 2010; Brady et al. 2014; Chang et al. 2016; Green, Zaller, Rich, Bowman & Friedmann 2011; Lin, Wang, Boyd, Simoni - Wastila & Buu 2018; Meara et al. 2016; Rasubala, Pernapati, Velasquez, Burk & Ren 2015; Reisman, Shenoy, Atherly & Flowers 2009; Ringwalt, Garrettson & Alexandridis 2015; Simeone & Holland 2006). These studies vary in the type of PDMP characteristics they evaluate (e.g., implementation or mandatory access), observation windows, sup - population of patients (e.g., Medicaid only), and providers (e.g., emergency department physicians) considered. Although these studies generally rely on pre-post comparisons or use small or single site study samples and thus may not be causally interpretable, many of them also report significant declines in opioid prescribing associated with mandatory PDMP regulations.

[15] We also separately examined hours worked in primary or secondary jobs and the results were broadly similar.

was more negatively impacted than women's. in response to reduced supply of prescription opioids following mandatory state PDMPs. For men, EPOP, LFP and HOURS appear to be generally lower in the post period compared to the pre-period, while UR is consistently higher. In contrast, for women, there does not appear to be any meaningful change from the pre- to post-period. However, these gender differences are generally small and are not statistically significant. We also consider differences among age groups by comparing those 25-39, 40-54 and 55-64 years old (see Appendix Figure 1). We again find no significant or meaningful effects.

4.3 Labor Market Attachment in High vs Low Illicit Market States

As we noted earlier, it is plausible that the effects of PDMP laws on labor market activity could differ by whether individuals have the ability to substitute prescription opioid drugs with non-prescription drugs that are available on the illicit market. The enactment of PDMP laws could lead to worse outcomes if they lead to greater substance misuse due to a shift towards un-regulated drugs. For instance, Gupta, Al Achkar and Ray (2021) find significant reductions in opioid prescribing and prescription opioid involved overdose mortality following Kentucky's 2012 adoption of mandatory PDMP, using Indiana as a control state. However, the authors find that the decline in prescription-opioid-involved mortality was offset by an increase in illicit-drug mortality, suggesting complete substitution and resulting in no net change in total drug-overdose mortality in Kentucky following its mandatory PDMP. Alternatively, for medical users who need access to opioids for pain relief in order to work, access to the illicit market could facilitate greater labor market activity.

To address this empirically, we divide states by whether or not they had below median or above median level of illicit market activity in their state prior to enactment of PDMP laws. The results of this exercise are shown in Figure 5. For each of our labor market outcomes we show the event study figure for low illicit market states, high illicit market states and for the difference between them (low minus high illicit market activity).

Figure 5 strongly suggests that there was a sharp difference in the effects of PDMP laws on labor market attachment by the pre-existing degree of an illicit market. This is perhaps most clearly seen for the unemployment rate. In states with low illicit market activity there is a statistically significant increase in the unemployment rate by between 1 and 2 percentage points from 1 to 6 quarters after the PDMP laws were enacted. In contrast, we see absolutely no rise in the unemployment rate in the states with a large illicit market. This is highly consistent with the hypothesis that individuals who were able to substitute towards illicit opioids for pain management may have been better able to remain active in the labor market whereas individuals who were unable to substitute found themselves without a substitute and were more likely to become unemployed. We further see that the difference in the unemployment rate between the two groups of states is also statistically significant and is in the order of 1 percentage point or more.

We also see strongly suggestive evidence of a decline in the employment to population rate of around 1 percentage point in the "low illicit" states in the 6 quarters after PDMP laws, but no evidence of such a decline in the "high illicit" states. We also find that the difference in EPOP between these groups of states is statistically significant in 4 of the 6 quarters in the post period. A somewhat similar but noisier pattern is also evident with LFP and HOURS.

4.4 Effects on Disability Insurance

Our next set of results, shown in Figure 6, examine changes in disability insurance rates – SSI, SSDI and share of SSDImd. While states that adopted mandatory PDMPs have statistically similar rates of SSI and SSDImd to states that did not in in the pre-treatment period, we do note that the treated states had lower rates of SSDI. Also, in line with the descriptive analysis presented in Section 3 above, mandatory PDMPs are followed with a gradual decline in rates of SSI and a gradual increase in rates of SSDI and particularly share of SSDImd. However, these effects are often small (0-10 percent relative to pre-treatment mean), imprecisely estimated, particularly as we move further away from the time of mandatory PDMP adoption, and are not statistically significant at conventional levels.

4.5 Effects on Credit Market Outcomes

Our last set of results consider changes in financial credit outcomes and are shown in Figure 7. Starting with the first column, we begin by showing that shortly after the enactment of mandatory PDMP laws we begin to observe an increase in the number of collections that are referred to third party collection agencies over the past 12 months. At its peak, about 9 quarters after PDMP (not shown), the effect is about 0.05 additional collections which represents an increase of around 12 percent relative to the baseline mean of 0.43 in the untreated states. The next figure in the first column shows the effects on the dollar amounts of these collections and indicates that total collections over the past year increased by about $30 following PDMP laws, which represents an effect size of about 11 percent relative to the baseline mean of $285 in the control states. Both figures are also reassuring in that they show no evidence of a pre-trend, which provides further evidence in support of our identifying assumption.

The next figure shows that there appear to be no effects on bankruptcy rates over the prior two years, even once we look out a full 13 quarters or 3 and a quarter years after mandatory PDMP laws have been enacted. One possible explanation is that this is because bankruptcy is an extreme outcome that perhaps only affects a small portion of individuals who are less likely to be affected by opioid addiction. The bottom figure in the first column shows that there are clear and statistically significant effects on credit scores that steadily emerge in the first two years after PDMP laws. Our estimates suggest that 2 to 3 years after the laws, there is a decline of about two points in the Equifax Risk Score™. While this effect is statistically significant, it is relatively small in magnitude representing just a 0.3 percent effect relative to the mean score of about 674 points in the untreated states.

The last four charts in the second column of Figure 7 show that we do not find statistically significant effects on total balances, credit card debt as well as balances of each type that are past due. Total balances are a somewhat ambiguous outcome from a welfare standpoint as higher balances could reflect greater consumption or a potential for unsustainable debt. Total balances past due are more clearly a negative outcome and here the point estimates do indicate that after PDMP laws a greater fraction of individuals are delinquent on payments but the event study coefficients are not statistically significant.

4.6 Credit Market Outcomes in High vs Low Illicit Market States

Finally, as we did with the labor market analysis, we conducted a parallel analysis with the credit market outcomes where we again divided our sample into states either with or without an already developed illicit market and examined the difference in effects across these two state groups. These results are shown in Figure 8. For several outcomes, we now see much more pronounced negative effects in the "low illicit" states, where substitution to non-prescription opioid drugs is constrained. For example, we now find that credit card balances past due and total balances past due rose by more than in the "high-illicit" states and that these differences are statistically significant. For other outcomes such as the number and amount of 3rd party collections, there is perhaps a slightly larger increase in the "low-illicit" states, but the differences are quite small and statistically insignificant. Overall, however, these differences are consistent with our findings on the labor market and highlight how the PDMP laws were particularly impactful on households in states that lacked an alternative to prescription opioids.

4.7 Other Sensitivity Checks

We also consider whether the aggregate effects may mask some other important differences across states. First, we explored whether the response to mandatory PDMPs differs by baseline opioid prescribing rates. Specifically, we estimated our model separately for states with above and below median baseline opioid prescribing rates.[16] We found that there were no significant effects of mandatory PDMPs in either group for our labor market outcomes and we also found very similar patterns for both groups for the financial outcomes where we had found significant effects. In addition, we explored whether the labor market effects differed by baseline unemployment rates and again found no differences. We also found that our estimates are not sensitive to whether we weight by population. We show the unweighted results for labor market outcomes in Appendix Figure 3 and for credit outcomes in Appendix Figure 4.

5. Conclusion

Our study examines the effects of mandatory PDMP laws on several important and policy relevant outcomes. In line with earlier studies, we find that mandatory PDMPs reduce opioid prescribing. Using our full sample of states, our results suggest that while these laws did not appear to affect labor market attachment, they had an adverse impact on some financial outcomes including credit scores and the number and the amounts of debts send to third party collection agencies. However, we suspect that these effects may mask differing effects on different subpopulations. While we cannot identify the precise mechanisms underlying our results, our finding is consistent with the possibility that while some individuals may have benefitted from these laws, others may have been negatively impacted if they needed access to pain relief in order to remain active in the labor market. Therefore, well-intentioned policies aimed at reducing inappropriate opioid prescribing may in fact unintendedly create further barriers in access to adequate pain management.

In the case of the labor market, the effects may have been largely offsetting, but in the case of the credit market, it may be that there were much larger negative effects on the subpopulation who could not substitute away from prescription opioids. Indeed, when we break down our sample into states with a well-developed illicit market versus those without, we find that suggestive evidence of negative effects on labor market attachment and clear evidence of worse

[16] We classified states using the CDC opioid prescribing rate data from 2009, the first year the data is available.

credit outcomes in states where there was less scope for substitution. Of course, the costs of substitution towards illicit substances such as fentanyl is high and can be tragic, highlighting the need for access to safe forms of pain medication.

There are a few caveats to our analysis. First, we cannot directly identify individuals who were tapered off prescription opioid therapy following mandatory PDMPs and their subsequent labor market response, our findings are necessarily based on aggregate effects that could involve both positive and negative effects that perhaps “wash out” in the balance. Second, we do not include states that have more recently adopted mandatory PDMP laws as the follow up period is shorter and likely to be confounded with the Covid-19 pandemic. Similarly, some of the very early mandatory PDMP adopters are excluded from the sample as they have a very short pre-treatment period. Although unlikely, it could be that the effect of mandatory PDMPs on labor market participation may emerge only later, particularly if the programs cause non-medical prescription opioid users to seek substance use disorder treatment and subsequently re-enter active labor. However, recent literature finds that admissions to substance use treatment centers consistently *decline* after a brief initial increase following mandatory PDMPs (Dave et. al, 2018). The authors interpret their finding as evidence of long-term decline in rates of prescription opioid use disorders following mandatory PDMPs. Future researchers equipped with longitudinal data that directly links individuals who use opioid drugs to their labor market and credit market outcomes, may be able to verify these findings.

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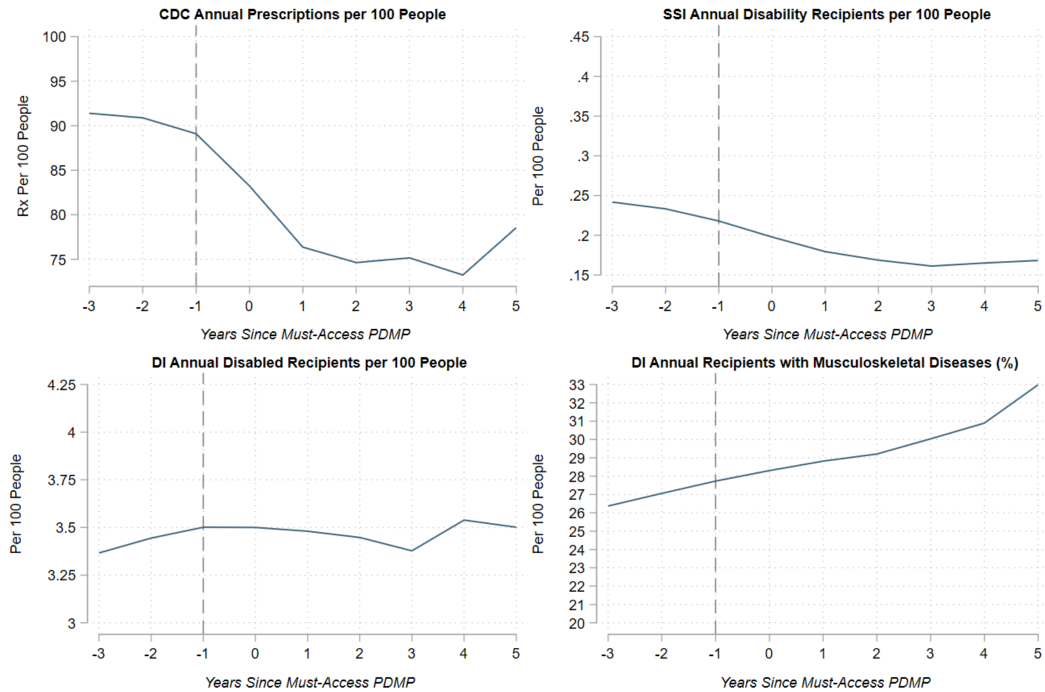
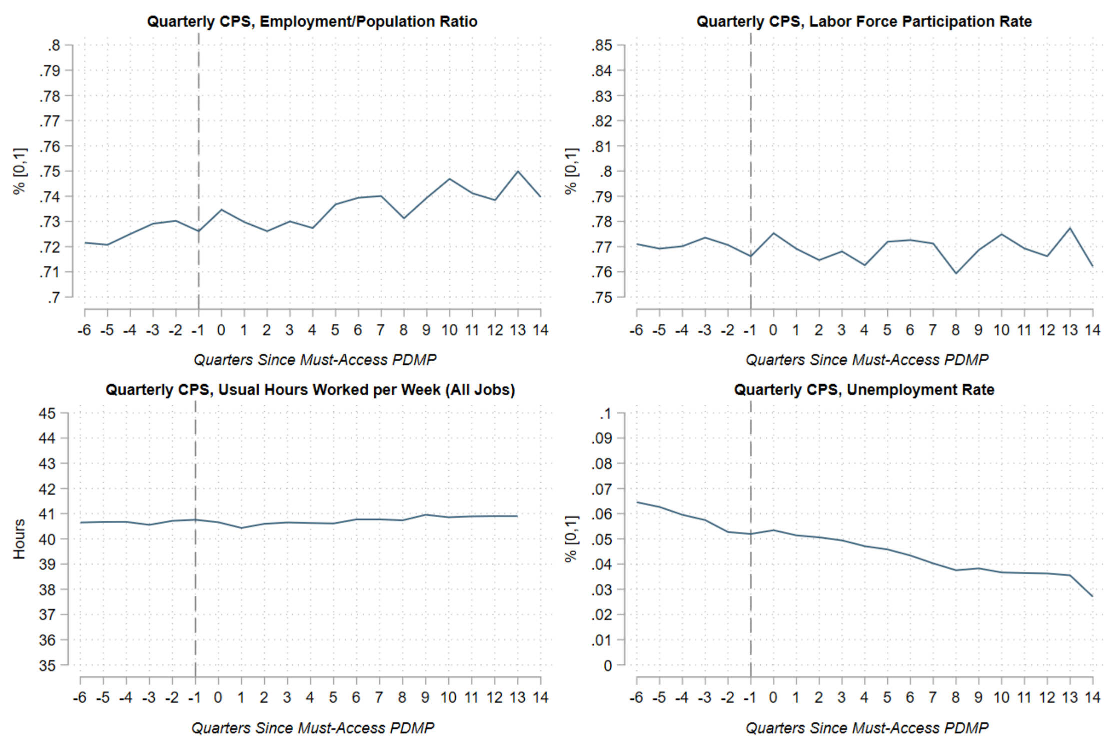
Figure 1: Non-parametric graphical evidence of the effect of mandatory PDMP adoption on labor market participation and disability insurance rates (2009-2017).

Panel A: Prescriptions and Disability Measures



Panel B: Labor Market Outcomes



Panel C: Credit Market Outcomes

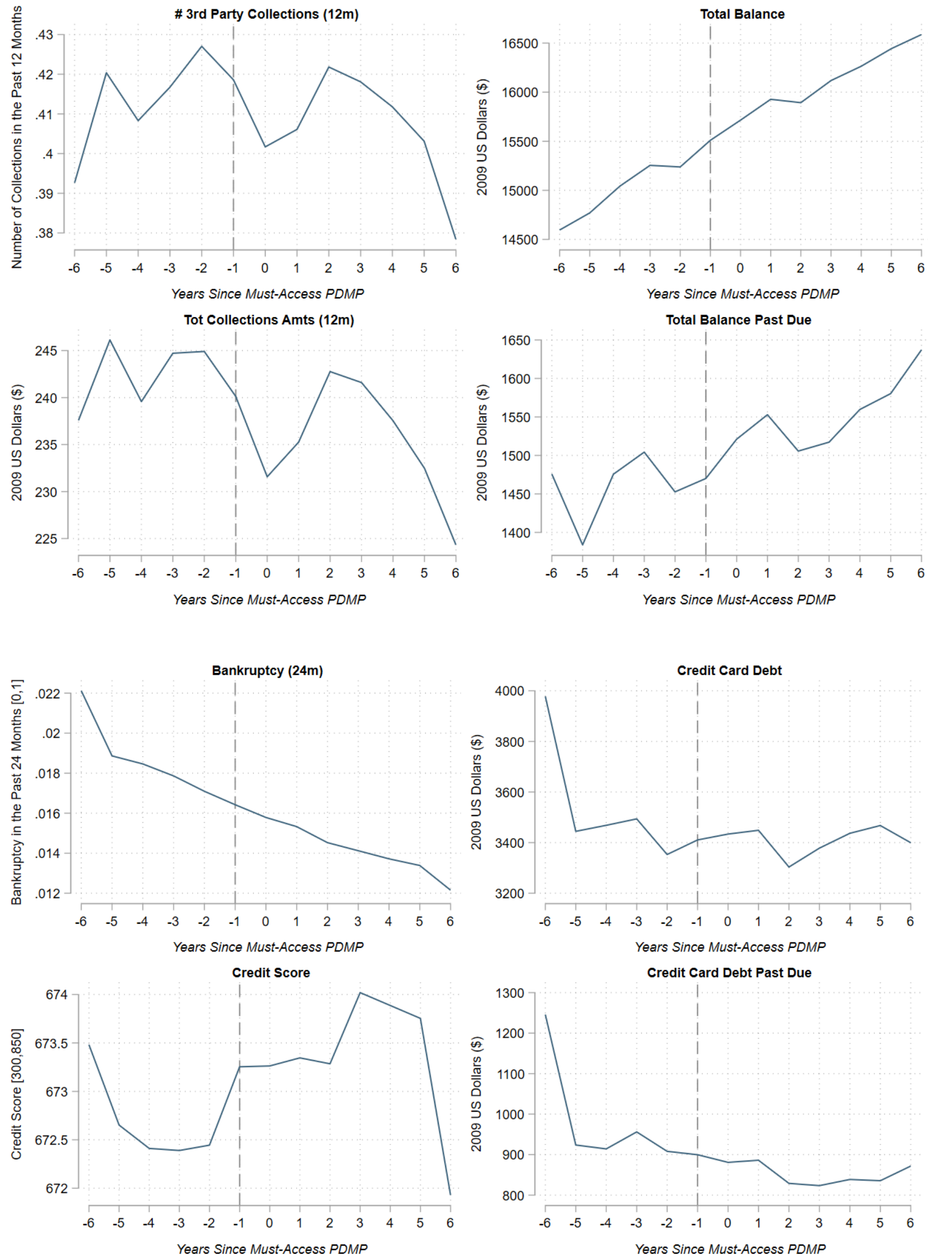


Figure notes: Authors calculation of unadjusted trends in opioid prescribing, labor market attachment, SSI, SSDI and credit scores, relative to timing of state adoption of mandatory PDMPs. Rate of opioid prescribing in state is measured using (1) number of opioid prescriptions per 100 persons from the Center of Disease Control. Four measures of labor market attachment considered include – (2) employment to population ratio (EPOP), (3) labor force participation rate (LFP), (4) unemployment rate (UR) and (5) hours worked last week (HOURS) – from the Current Population Survey (CPS) of the Bureau of Labor Statistics, for the years 2009Q4-2019Q4. The three measures of disability insurance include (6) Supplemental Security Insurance (SSI), (7) Social Security Disability Insurance (SSDI) and (8) the share of social security disability awards for musculoskeletal diseases (SSDImd) (range 0-100 percent) from the Social Security Administration. Refer Data section of manuscript for sources of each data and variable construction.

Figure 2: Effects of mandatory PDMPs on the number of opioid prescriptions per 100 persons (2008-2016).

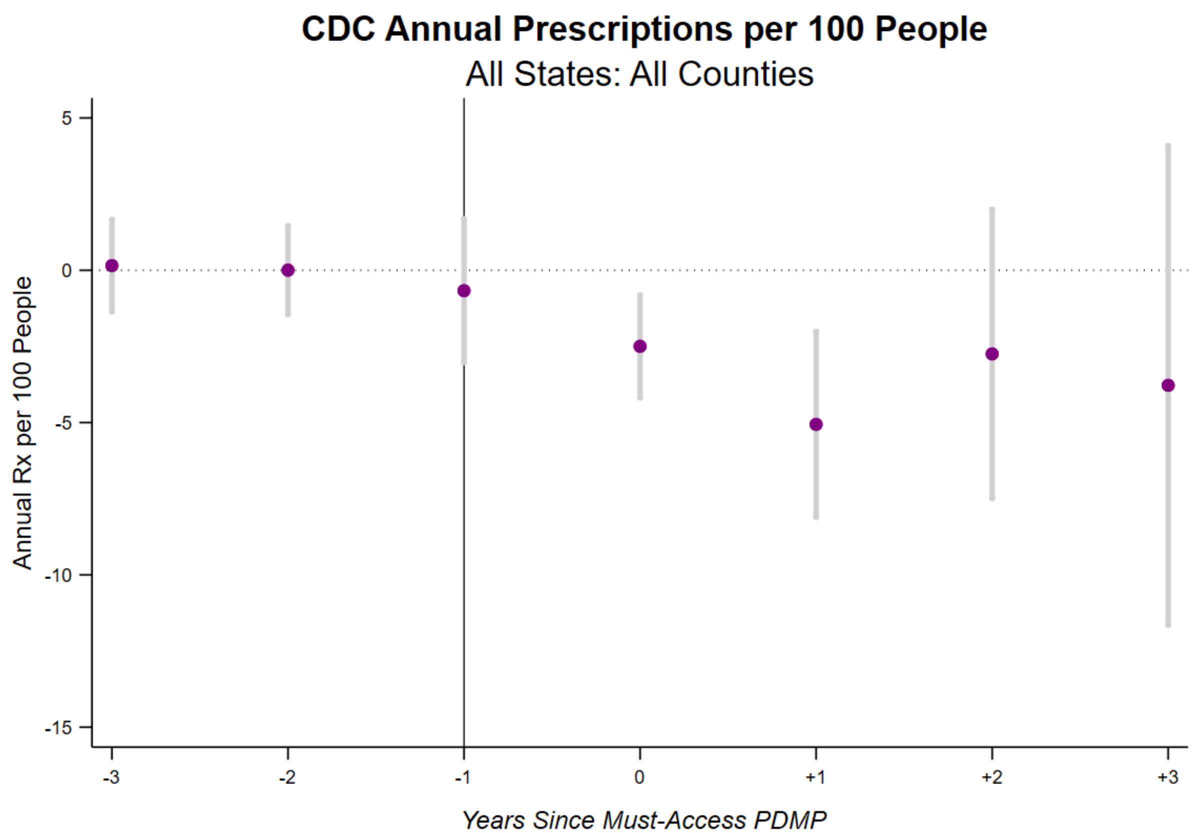


Figure notes: Authors' calculations based on the CDC annual data of the number of opioid prescriptions per 100 persons during 2008-2016. The DV is the annual growth rate of opioid prescriptions per 100 persons. The figure presents the adjusted difference (red dot) and its 95% confidence interval (in error bars) in the number of dispended opioid prescriptions per 100 persons in year's prior to/post-state adoption of mandatory PDMPs. Figure presents Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level.

Figure 3: Effects of mandatory PDMPs on state-level labor market participation rates.

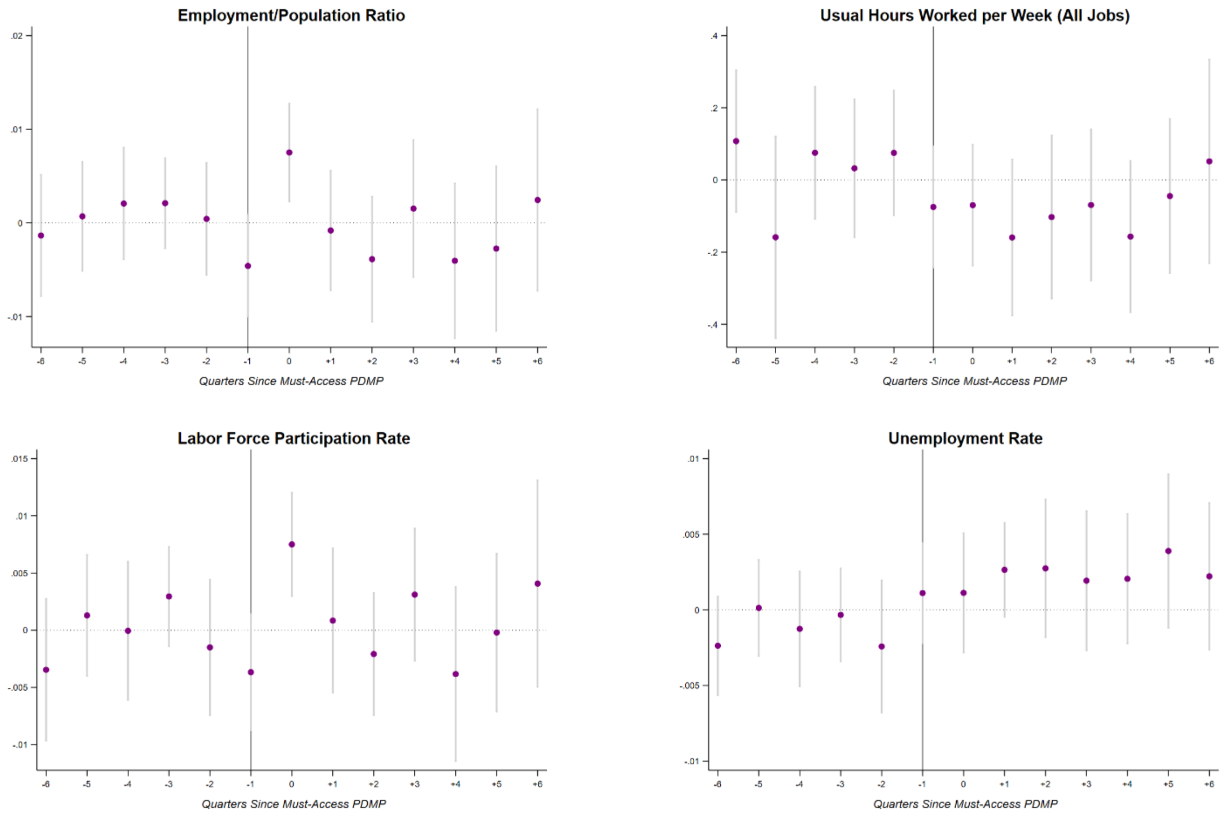


Figure notes: Authors' calculations based on Current Population Survey (CPS) of the Bureau of Labor Statistics, downloaded from IPUMS for the year's 2009Q4-2019Q4. Each row is a separate DV capturing alternative measures of labor market participation rate at the extensive margin – EP, LFP and UR. The figure presents adjusted changes in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs along with 95% confidence intervals (in error bars) in the. Figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level.

Figure 4: Effects of mandatory PDMPs on state-level labor market participation rates by Gender.

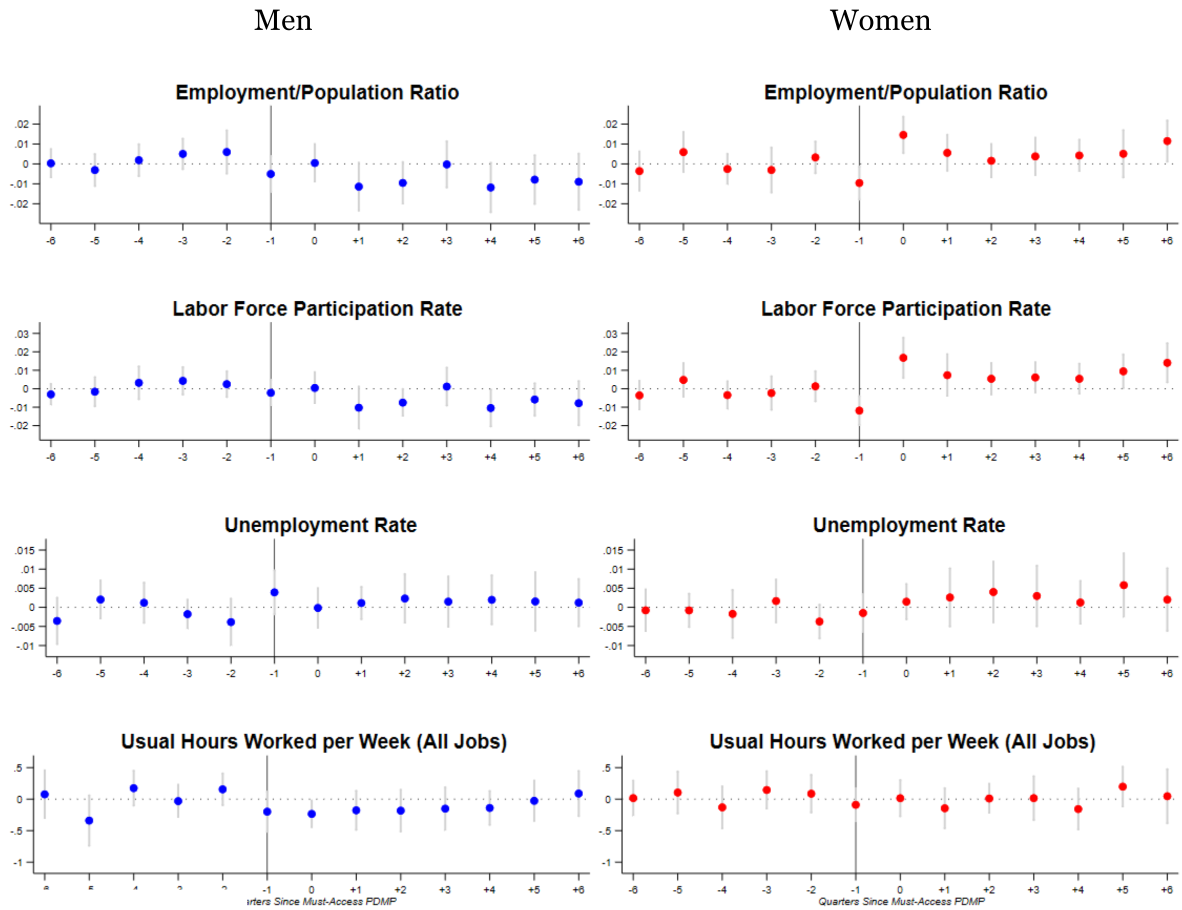


Figure notes: Authors' calculations based on Current Population Survey (CPS) of the Bureau of Labor Statistics, downloaded from IPUMS for the year's 2009Q4-2019Q4. Each row is a separate DV capturing alternative measures of labor market participation rate at the extensive margin – EP, LFP and UR. The figures present adjusted changes in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs difference for subpopulations stratified by gender (purple dot for all, blue dot for men and red dot for women) along with 95% confidence intervals (in error bars). Figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level.

Figure 5: Effects of mandatory PDMPs on state-level labor market participation rates by states with low versus high illicit market activity.

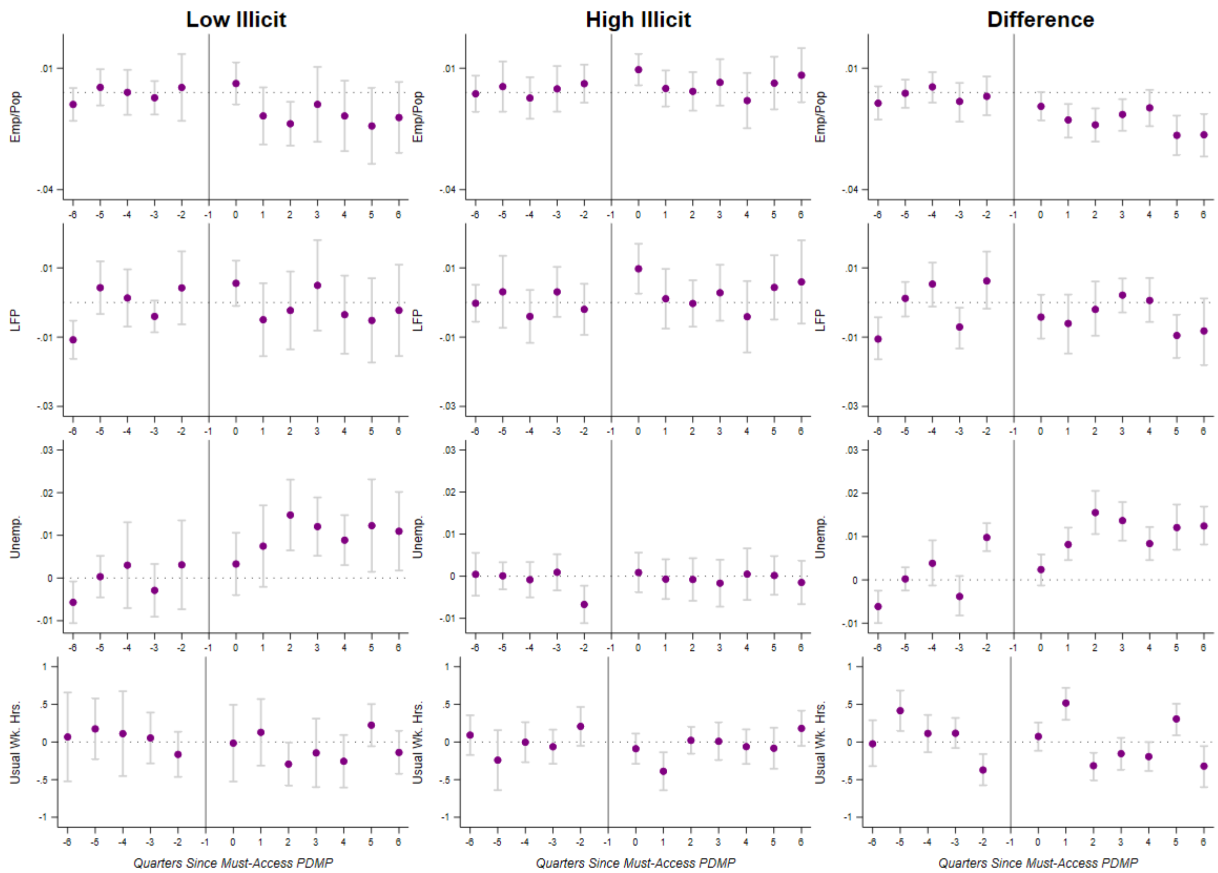


Figure notes: Authors' calculations based on Current Population Survey (CPS) of the Bureau of Labor Statistics, downloaded from IPUMS for the year's 2009Q4-2019Q4. Each row is a separate DV capturing alternative measures of labor market participation rate at the extensive margin — EP, LFP and UR. The figures present adjusted changes in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs difference for subpopulations stratified by states with low or high levels of illicit market activity (below/above median baseline illicit-drug involved overdose mortality from CDC Wonder MCOD data) along with 95% confidence intervals (in error bars). Figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level. Standard errors of the incremental effect of mandatory PDMP laws in states with high illicit market activity, relative to states with low illicit activity ("difference" column 3) are calculated through bootstrapping. See text for more details.

Figure 6. Impact of mandatory PDMPs on annual awards of Social Security Insurance (SSI), Social Security Disability Insurance (SSDI) and share of SSDI related to musculoskeletal diseases. Social Security Administration data, 2008-2017.

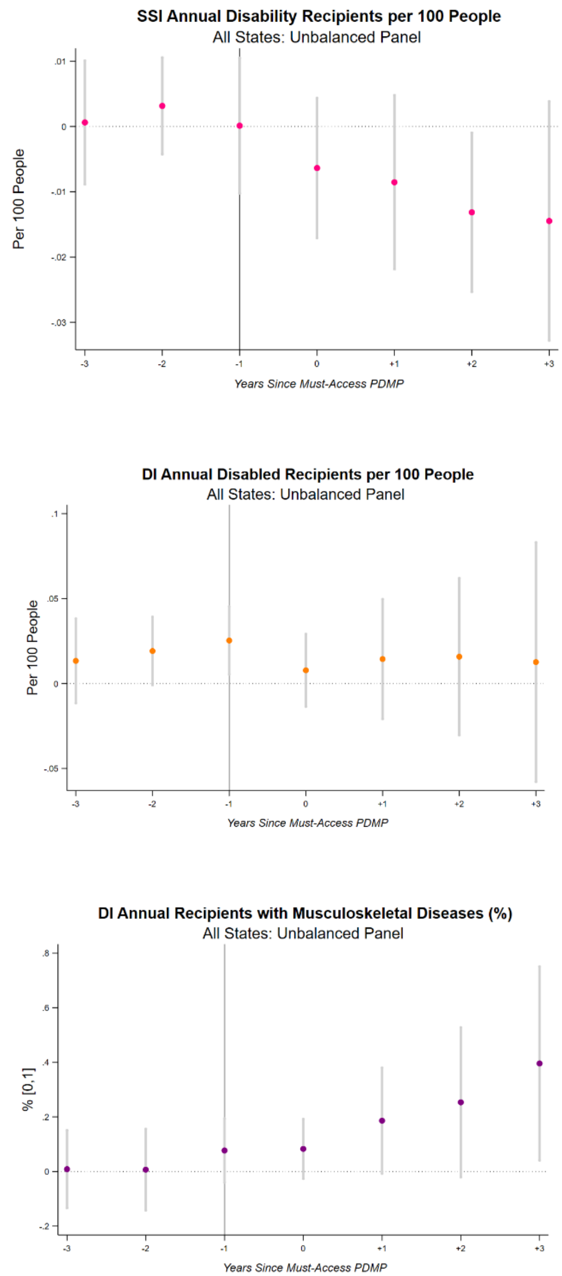


Figure notes: Authors' calculations based on Social Security Administration data on annual awards of SSI, SSDI and share of SSDI related to musculoskeletal disease, 2008-2017. The figure presents Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level.

Figure 7. Effects of mandatory PDMPs on financial outcomes.

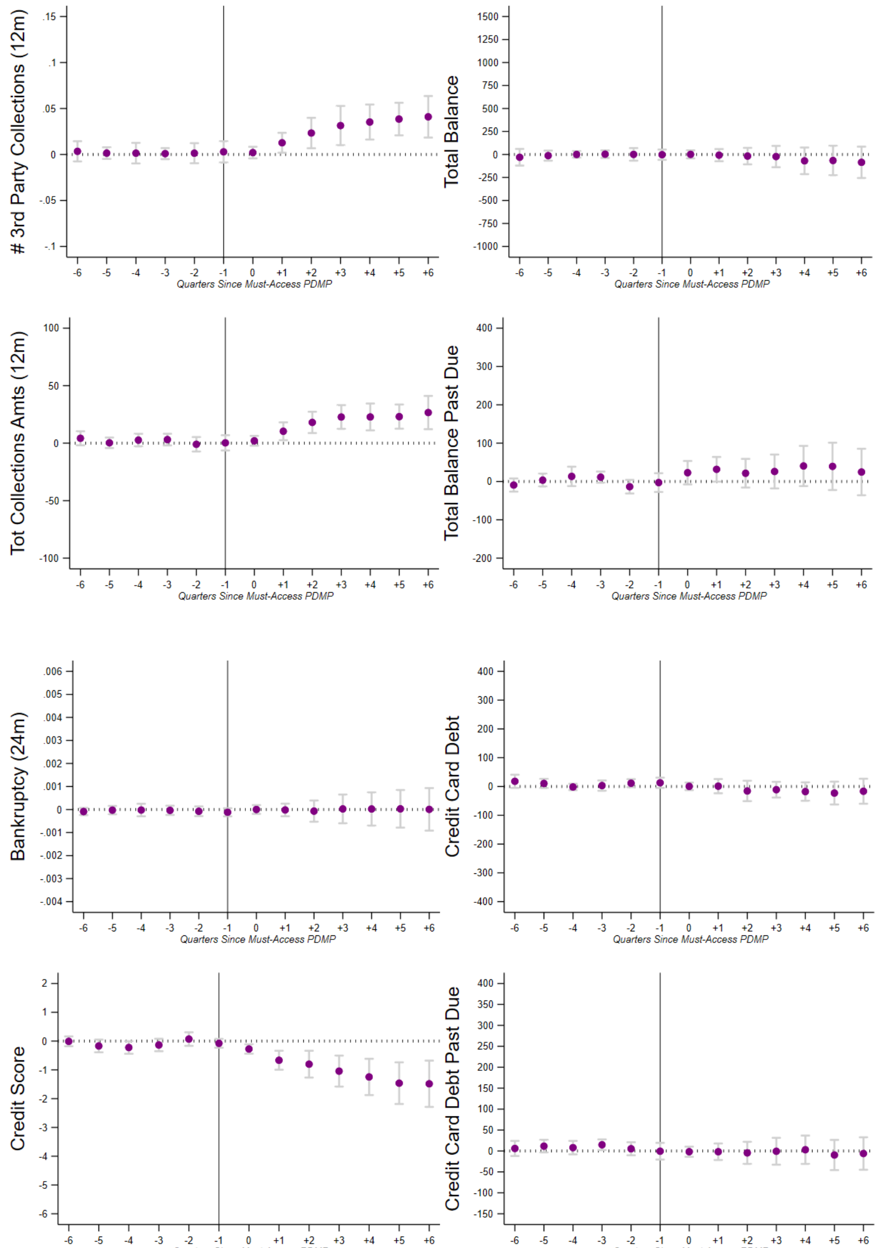


Figure notes: Authors' calculations based on the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) data. The figure presents the differences in each financial outcome and its 95% confidence interval (in error bars) in quarter's prior to/post state adoption of mandatory PDMPs. Data includes individuals 25 to 64 years old. The figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level.

Figure 8. Effects of mandatory PDMPs on selected financial outcomes by states with high versus low illicit market activity.

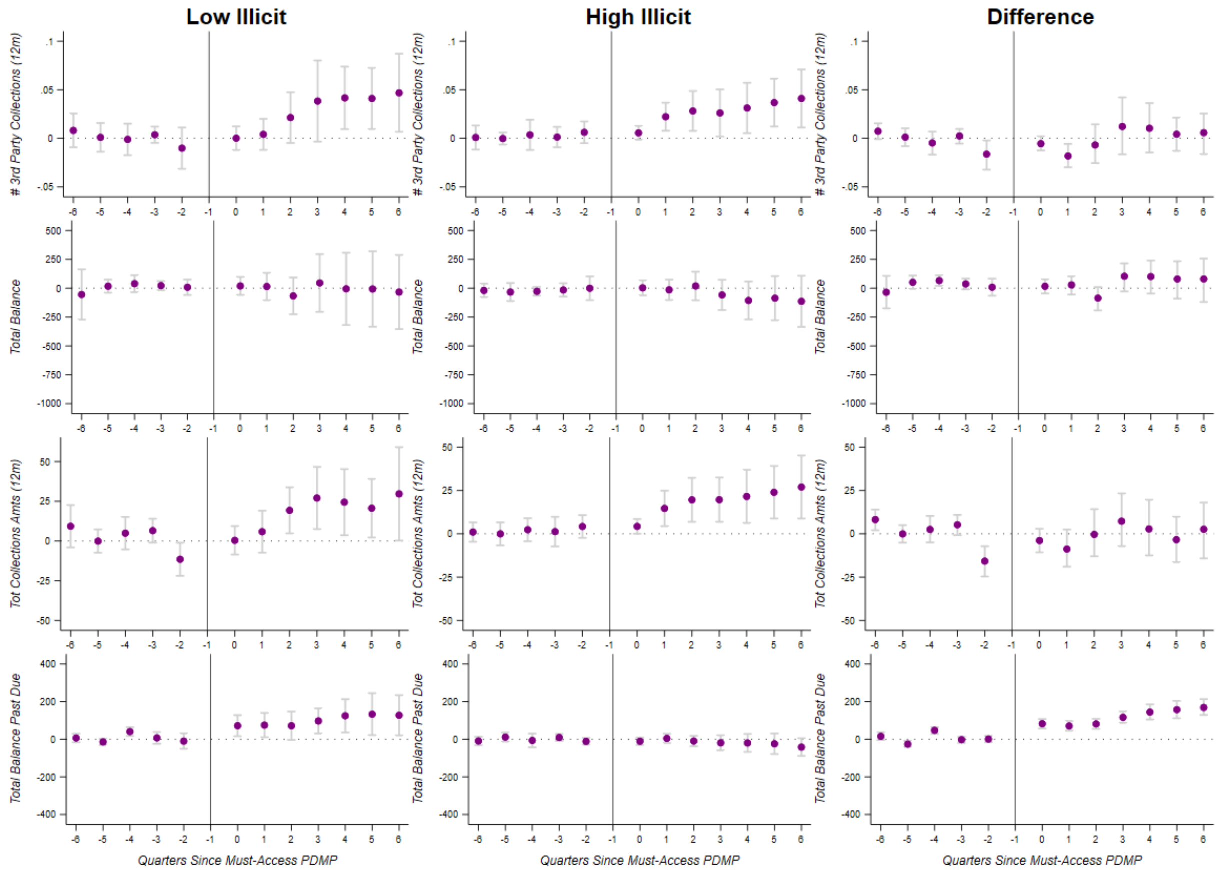


Figure notes: Authors' calculations based on the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) data. The figure presents the differences in each financial outcome and its 95% confidence interval (in error bars) in quarter's prior to/post state adoption of mandatory PDMPs. Data includes individuals 25 to 64 years old. The figures present adjusted changes in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs difference for subpopulations stratified by states with low or high levels of illicit market activity (below/above median baseline illicit-drug involved overdose mortality from CDC Wonder MCOD data) along with 95% confidence intervals (in error bars). Figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level. Standard errors of the incremental effect of mandatory PDMP laws in states with high illicit market activity, relative to states with low illicit activity ("difference" column 3) are calculated through bootstrapping.

Figure 8 (continued). Effects of mandatory PDMPs on selected financial outcomes by states with high versus low illicit market activity.

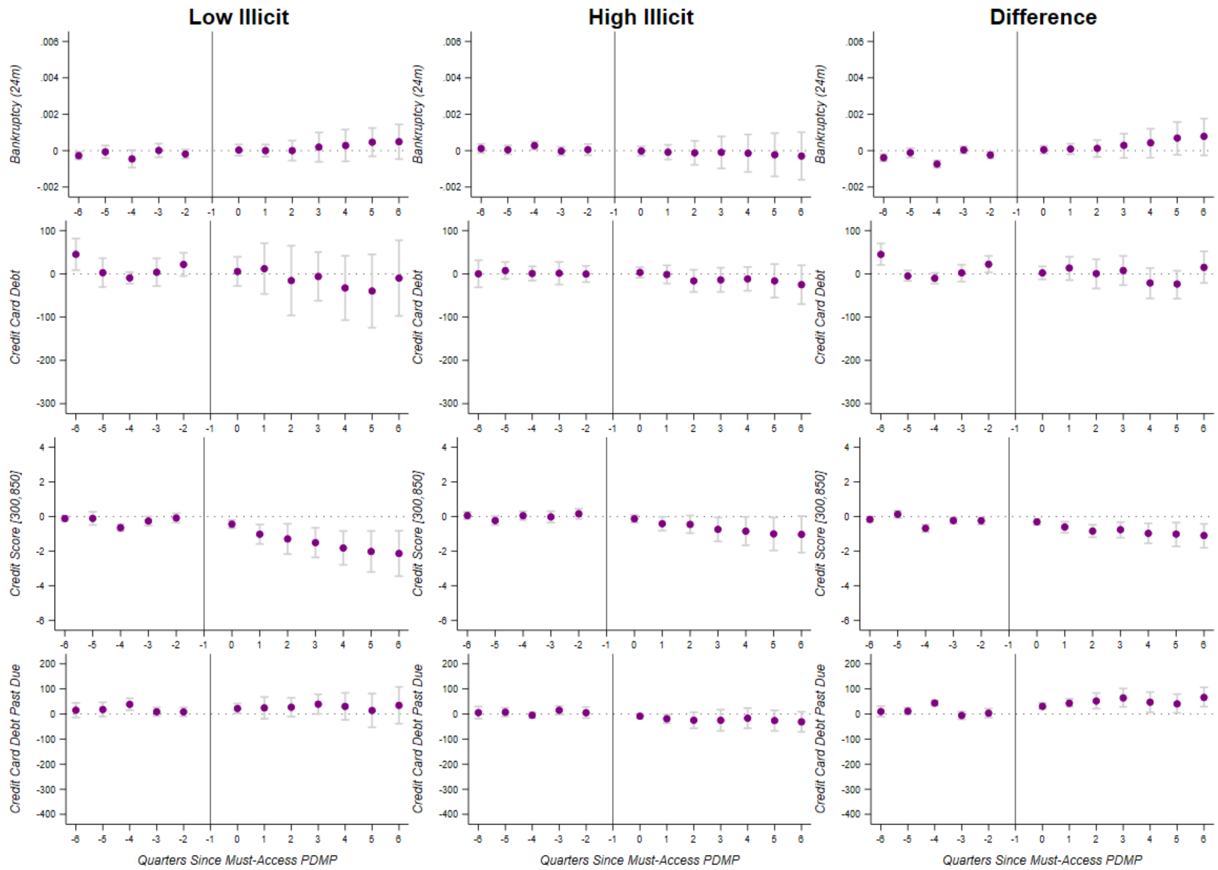


Figure notes: Authors' calculations based on the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) data. The figure presents the differences in each financial outcome and its 95% confidence interval (in error bars) in quarter's prior to/post state adoption of mandatory PDMPs. Data includes individuals 25 to 64 years old. The figures present adjusted changes in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs difference for subpopulations stratified by states with low or high levels of illicit market activity (below/above median baseline illicit-drug involved overdose mortality from CDC Wonder MCOD data) along with 95% confidence intervals (in error bars). Figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level. Standard errors of the incremental effect of mandatory PDMP laws in states with high illicit market activity, relative to states with low illicit activity ("difference" column 3) are calculated through bootstrapping.

Table 1. Estimation **s**ample set up and observation window of analysis.

Sample	Observation Window	Treated States	Control States
CDC Rx Rates	2008-2016 ➔ Only through 2016 due to data availability	CT, GA, IN, KY, MA, NH, NJ, NM, NY, OH, OK, RI, TN, VT, VA, WV	AL, AK, AZ, AR, CA, CO, DE, FL, HI, ID, IL, IA, KS, LA, ME, MD, MI, MN, MS, MO, MT, NE, NV, NC, ND, OR, PA, SC, SD, TX, UT, WA, WI, WY
CPS – all geography	2009Q4-2017Q4	CT, GA, IN, KY, MA, NH, NJ, NM, NY, OH, OK, RI, TN, VT, VA, WV	AL, AK, AZ, AR, CA, CO, DE, FL, HI, ID, IL, IA, KS, LA, ME, MD, MI, MN, MS, MO, MT, NE, NV, NC, ND, OR, PA, SC, SD, TX, UT, WA, WI, WY
CPS – urban	2010Q1-2017Q3	GA, IN, KY, NJ, NM, NY, OH, TN, VA	AL, AZ, CA, CO, DE, FL, HI, IL, IA, KS, LA, MD, MI, MN, MO, MT, NV, NC, OR, PA, SC, TX, WA, WI
CPS – rural	2009Q4-2017Q4	CT, GA, IN, KY, MA, NH, NJ, NM, NY, OH, OK, RI, TN, VT, VA, WV	AL, AK, AZ, AR, CA, CO, DE, FL, HI, ID, IL, IA, KS, LA, ME, MD, MI, MN, MS, MO, MT, NE, NV, NC, ND, OR, PA, SC, SD, TX, UT, WA, WI, WY
SSI/SSDI	2008-2017	CT, GA, IN, KY, MA, NH, NJ, NM, NY, OH, OK, RI, TN, VA, VT, WV	AK, AL, AR, AZ, CA, CO, DE, FL, HI, IA, ID, IL, KS, LA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NV, OR, PA, SC, SD, TX, UT, WA, WI, WY

Table notes: The treated and control states are the same in each analysis except for the urban and rural cuts.

Appendix

Appendix Table 1.

	SSI	SSDI
Who pays?	Funded by U.S. Treasury general fund taxes (NOT the Social Security trust fund), which are split 50/50 between employers and employees	Payroll taxes (FICA Social Security taxes), which is split 50/50 between employers and employees
Limits on payouts	$771 per month for individuals, $1157 for couples (number adjusted annually if there is a Social Security cost-of-living adjustment)	$2861 per month
Eligibility thresholds	Available to low-income individuals who have either never worked or who haven't earned enough work credits to qualify for SSDI. Must have less than $2000 in assets (or $3000 for a couple) and a very limited income.	Available to workers who have accumulated a sufficient number of work credits. Number of credits needed depends on age. Credits accrue each year based on some threshhold of income earned over the year, which is recalculated annually. You can earn up to 4 credits in a year. The 2019 threshholds are $1360 for 1 credit, and $5440 for 4 credits. These are then used to assess the "recent work test" and "duration of work test". The recent work test assesses if you've worked for a sufficient portion of the time right before you became disabled (usually a 10-year window). The duration of work test assesses whether you've worked long enough given your age (but the working years could come from any period in your life, not just the last 10 years).
Application process	Have to prove that your disability makes it impossible for you to perform any work, including both work you have done before and any work for which you could otherwise be trained. Also need to prove financial need.	Need to provide a detailed medical history and a record of every job you've had for the past 15 years.
Approval process	A local SSA field offices and state agencies (Disability Determination Services--DDS) determine if you qualify. There is an appeals process if your claim doesn't go through. Once approved, benefits begin on the first of the month when you first submit your application.	A local SSA field offices and state agencies (Disability Determination Services--DDS) determine if you have a disability that bars you from work. There is an appeals process if you are not approved the first time through. There is a 5 month waiting period for benefits.
Benefits	Number listed above under "limits on payouts" is the monthly federal maximum. Any income you receive in the month (minus some exclusions) can be subtracted. State supplements may be added onto the monthly federal payment.	Exact benefit rate determined based off of your average lifetime earnings before your disability began. No consideration given to severity of disability or current income. Benefits can go to your family members (considered dependents).
Other	If individual also recieves SSDI, this will be factored in as income received in the month such that between SSDI and SSI, the individual will get $771 maximum in benefits.	After receiving SSDI for two years, a disabled person will become eligible for Medicare. Approximately 70% of Social Security Disability claims fail on the first attempt.

Sources:

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https://www.disability-benefits-help.org/faq/how-much-to-earn-to-qualify

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https://www.ssa.gov/ssi/text-understanding-ssi.htm

https://www.ssa.gov/disability/determination.htm

Appendix Table 2: Age-by-Gender Sample Selection

State	Gender/Age Samples		Urba		Rur		PDMP Date
	Control	Treate	Control	Treated	Control	Treated	
Alabama	x		x		x		
Alaska	x				x		
Arizona	x		x		x		
Arkansas	x				x		
California	x		x		x		
Colorado	x		x		x		
Connecticut		x				x	10/1/2015
Delaware	x		x		x		
DC							
Florida	x		x		x		
Georgia		x		x		x	7/13/2013
Hawaii	x		x		x		
Idaho	x				x		
Illinois	x		x		x		
Indiana		x		x		x	7/1/2014
Iowa	x		x		x		
Kansas	x		x		x		
Kentucky		x		x		x	7/20/2012
Louisiana	x		x		x		
Maine	x				x		
Maryland	x		x		x		
Massachusetts		x				x	7/1/2014
Michigan	x		x		x		
Minnesota	x		x		x		
Mississippi	x				x		
Missouri	x		x		x		
Montana	x		x		x		
Nebraska	x				x		
Nevada*	x		x		x		1/1/2018*
New Hampshire		x				x	1/21/2016
New Jersey		x		x		x	11/1/2015
New Mexico		x		x		x	9/28/2012
New York		x		x		x	8/27/2013
North Carolina	x		x		x		
North Dakota	x				x		
Ohio		x		x		x	12/31/2015
Oklahoma		x				x	11/1/2015
Oregon	x		x		x		
Pennsylvania	x		x		x		
Rhode Island		x				x	6/28/2016
South Carolina	x		x		x		
South Dakota	x				x		
Tennessee		x		x		x	7/1/2013
Texas	x		x		x		
Utah	x				x		
Vermont		x				x	10/15/2013
Virginia		x		x		x	7/1/2015
Washington	x		x		x		
West Virginia		x				x	6/8/2012
Wisconsin	x		x		x		
Wyoming	x				x		
Count	34	16	24	9	34	16	17

*Note: Since Nevada's PDMP law takes effect in 2018, it is effectively untreated for the whole observation window, so it is included as a full control state.

Appendix Figure 1: Impact of mandatory PDMPs on state-level labor market participation rates by Age.

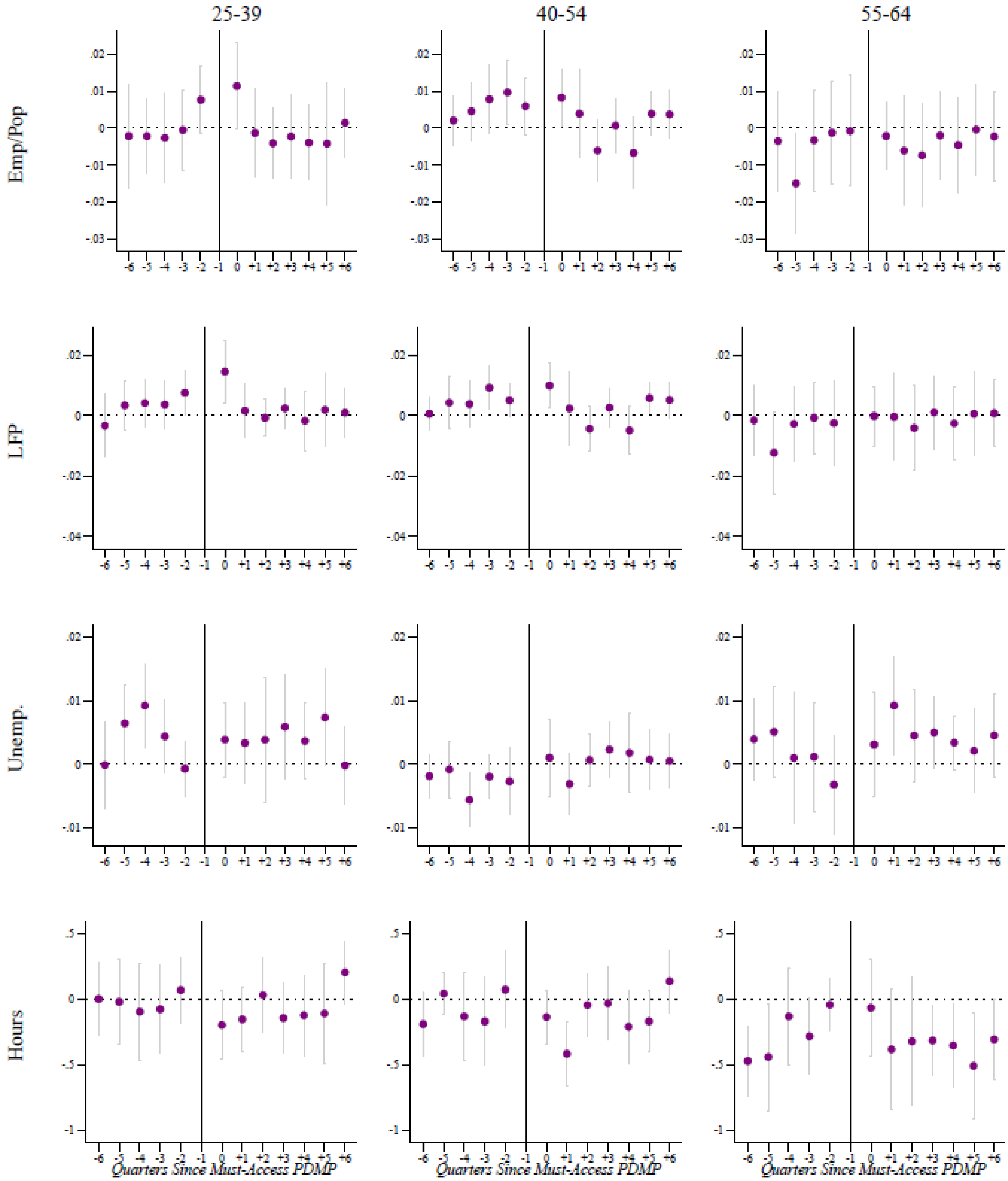
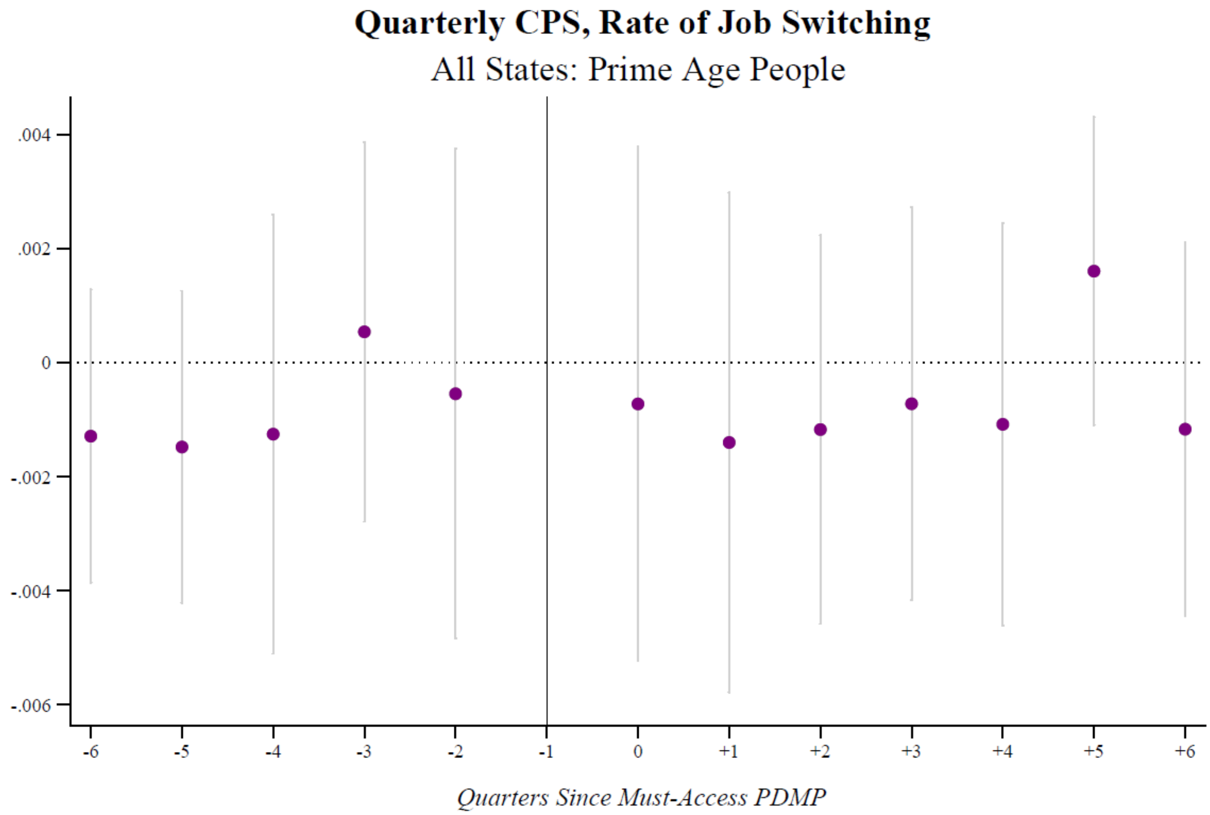


Figure notes: Authors' calculations based on Current Population Survey (CPS) of the Bureau of Labor Statistics, downloaded from IPUMS for the year's 2009Q4-2019Q4. Each row is a separate DV capturing alternative measures of labor market participation rate at the extensive margin — EP, LFP and UR. The figures present adjusted changes in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs difference for subpopulations stratified by age (ages 25-39, 40-54, and 55-64 years) along with 95% confidence intervals (in error bars). Figures present Callaway and Sant'anna (2021) based difference-in-different event study estimates for staggered adoption of state mandatory PDMPs in multiple time periods along with 95% confidence interval (in error bars) in each outcome in the year's prior to/post state adoption of mandatory PDMPs Regressions include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered at state-level.

Appendix Figure 2: Impact of mandatory PDMPs on month-to-month job switching.



Note: *Weighted*

Appendix Figure 3: Effects of mandatory PDMPs on state-level labor market participation rates **(unweighted)**.

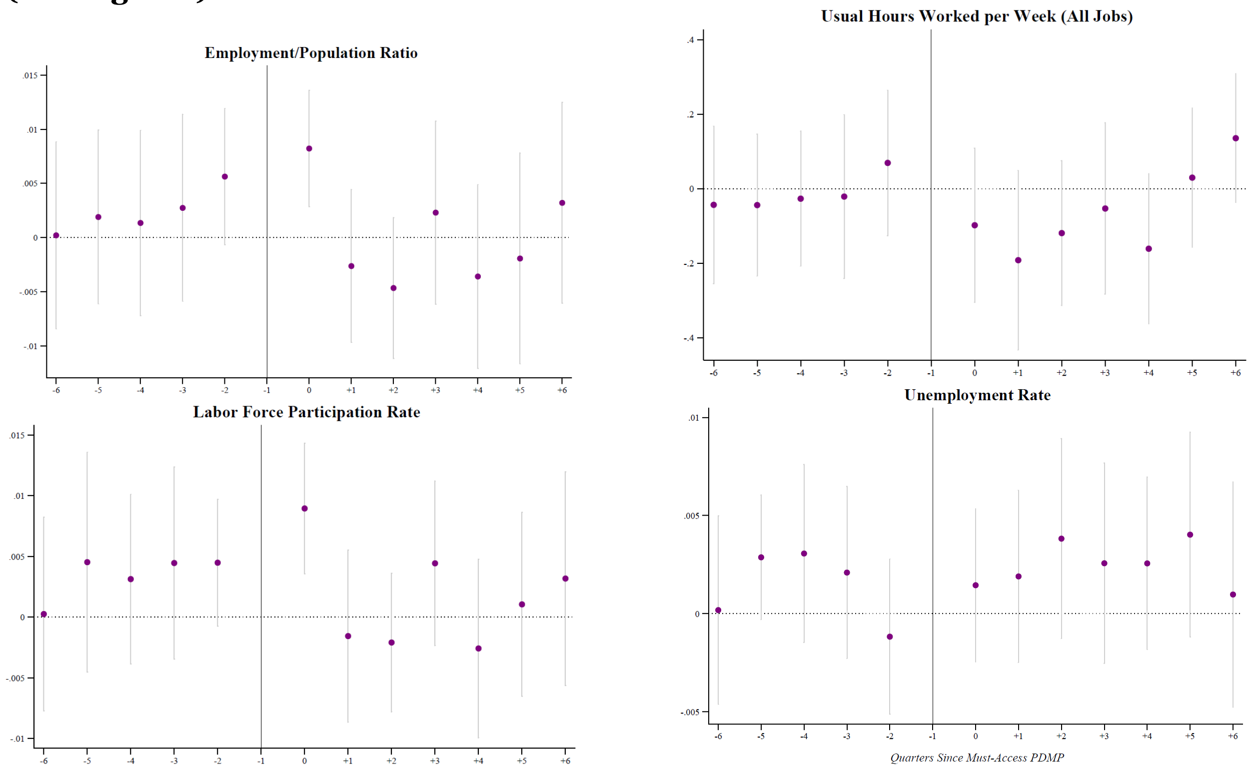


Figure notes: Authors' calculations based on Current Population Survey (CPS) of the Bureau of Labor Statistics, downloaded from IPUMS for the year's 2009Q4-2019Q4. Each row is a separate DV capturing alternative measures of labor market participation rate at the extensive margin — EP, LFP and UR. The figure presents the adjusted difference for subpopulations stratified by age (separate columns for ages 25-39, 40-54 and 55-64 years) and gender (purple dot for all, blue dot for men and red dot for women) and its 95% confidence interval (in error bars) in the rate of labor market participation in quarters prior to/post state adoption of mandatory PDMPs. All models include controls for state fixed effects and calendar quarter-year fixed effects. Robust standard errors, clustered on state.

Appendix Figure 4. Effects of mandatory PDMPs on financial outcomes **(Unweighted)**.

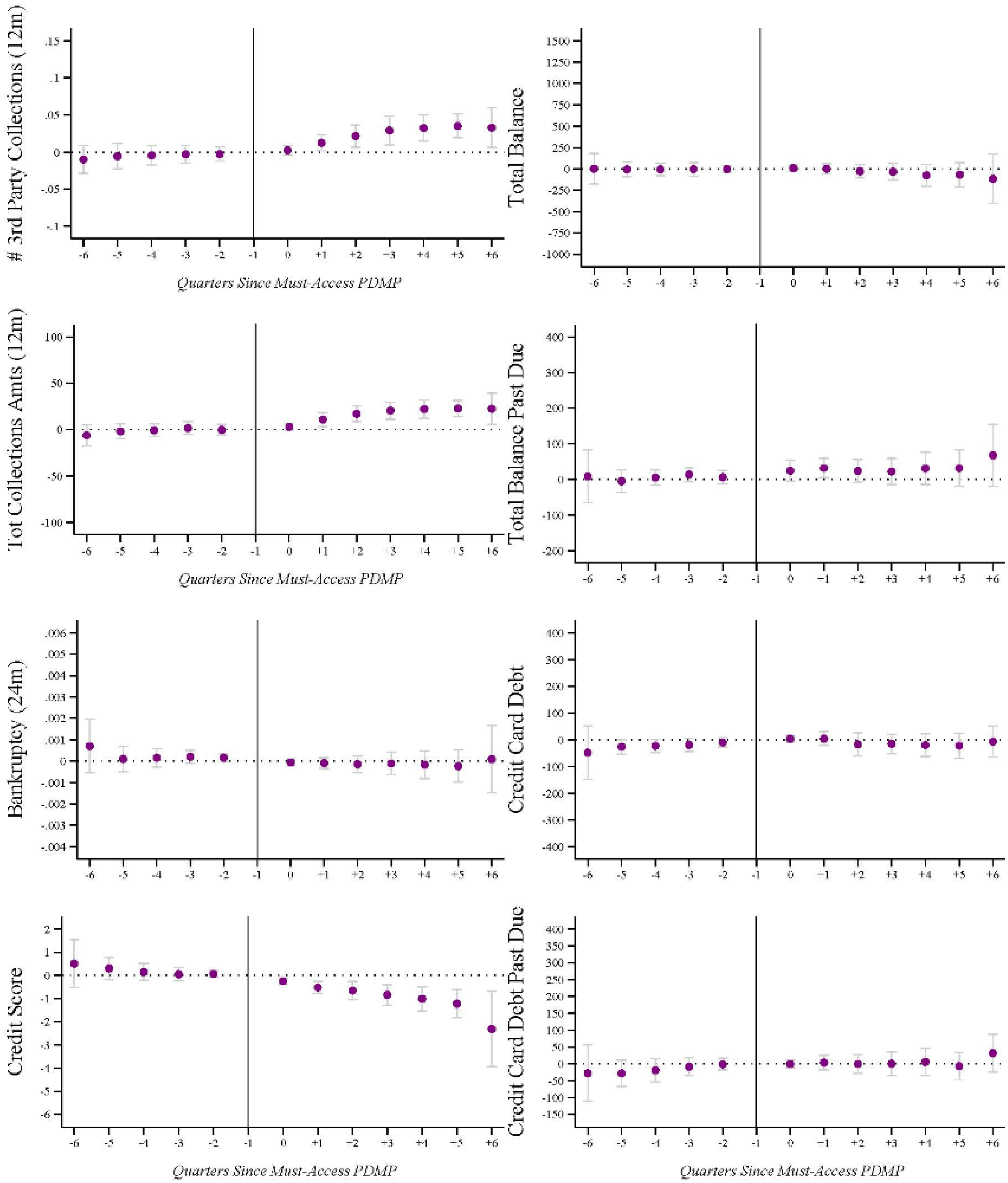


Figure notes: Authors' calculations based on the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) data. The figure presents the differences in each financial outcome and its 95% confidence interval (in error bars) in quarter's prior to/post state adoption of mandatory PDMPs. Data includes individuals 25 to 64 years old. All models include controls for state fixed effects and calendar year fixed effects. Robust standard errors, clustered on state.