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Essays on Opioid Related Issues

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by

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Table of Contents

List of Tables

List of Figures

Abstract

This dissertation contributes to the field of health economics by studying opioid-related topics.

In the first chapter, we notice that strong empirical evidence points towards a significantly higher prevalence of opioid consumption among people receiving disability benefits (DB) than in the general population of the United States. However, no previous research established a causal relationship between the decision to award DB to applicants and their subsequent opioid use. We aim to contribute towards filling this gap. There are channels through which awarding DB may both increase and depress opioid consumption, and thus, ex-ante, the sign of a potential causal relationship is ambiguous. To correct for the treatment endogeneity, since an individual's age at the time of the decision on an application impacts discontinuously at certain age cutoffs the award decision, we employ a fuzzy Regression Discontinuity model with three age cutoffs used for identification. We find that awarding DB increases the likelihood of using opioids by about 27-30 percentage points. This suggests that the positive association between DB receipt and opioid consumption is likely to be causal.

In the second chapter, I find that drug control policies can improve social welfare by curbing substance abuse or overdose. However, some of their potential unintended consequences, like the effects on drug-related crimes, remain underexplored. To the best of my knowledge, the existing literature has not established any causal relationships between drug control policies and domestic violence. As one of the largest state-level policies, the Mandatory Access (MA) Prescription Drug Monitoring Programs (PDMPs) have been shown to be effective at decreasing opioid use. This paper is the first to causally examine whether MA PDMPs impact the prevalence of intimate partner violence. Additionally, it complements the current literature by examining its effects on multiple types of child maltreatment crimes. I employ a difference-in-differences model on offense-level crime data and find that MA PDMPs can significantly decrease the offenses of intimate partner assaults as well as child assault and intimidation. The results are robust under multiple robustness checks.

Chapter 1: The effect of awarding disability benefits on opioid consumption[1]

Andrei Barbos, Minglu Sun

Portions of this chapter have been previously published in Health Economics (2021), 30(11), 2794-2807. The copyright permissions for reuse previously published material in this chapter can be found in Appendix Figure A6.

1. Introduction

Life expectancy in the United States has been growing steadily over the past sixty years. However, this trend has slowed down recently, and even declined for three consecutive years starting from 2014. Several factors—drug overdose, excessive alcohol consumption, suicide, and heart diseases—have been proposed as explanations for this phenomenon, but opioid abuse is regarded by many experts and public officials as the leading factor.[2] According to the Center for Disease Control and Prevention, “130 Americans died every day from an opioid overdose in 2017”,[3] and “among all deaths from drug overdose, approximately 70% involved an opioid in 2018”.[4] Since the opioid crisis has become a significant public health issue, it is important to

[1] We thank helpful comments from Padmaja Ayyagari, Giulia LaMattina, and Gabriel Picone. We also thank the associate editor and two referees for valuable suggestions that improved the analysis and exposition in this paper.
[2] See, for example, Dowell et al. (2017); Muenning et al. (2018); and Haskins (2019).
[3] https://www.cdc.gov/injury/features/prescription-drug-overdose/index.html
[4] https://www.cdc.gov/drugoverdose/epidemic/index.html

understand which groups of individuals are the most vulnerable, and what factors can influence their opioid use. One potential factor, which we investigate in our study, is the receipt of DB.

In the United States individuals can apply for DB, which include financial and medical assistance, from two programs—the Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). As a social insurance, the programs offer help for people who lack the ability to perform past work or who cannot work at all, but questions have been raised whether these programs may be contributing to the opioid epidemic crisis experienced in the United States.

Acute or chronic pain are the main reasons individuals start using opioids. Individuals who suffer from musculoskeletal conditions or low back pain are thus more exposed to the risk of opioids addiction and overdose. Meanwhile, musculoskeletal conditions and low back pain are also the leading causes of disability.[5] Consequently, people with disabilities have a significantly higher prevalence of opioids use than the general population (Morden et al., 2014; King et al., 2016; Gebauer et al., 2019; Ghertner, 2020). The substance abuse rate of people with disabilities has been shown to be two to four times higher than that of the general population.[6] Moreover, more than 40 percent of SSDI beneficiaries take opioids, with 20 percent of them being classified as chronic users.[7] Individuals who qualify for SSDI or SSI also have higher per capita expenditures of opioids (Zhou et al., 2016).

Our study also finds a strong positive correlation between the receipt of DB and use of opioid pain relievers among the individuals included in our dataset. However, while all evidence

[5] https://www.who.int/news-room/fact-sheets/detail/musculoskeletal-conditions
[6] https://ncdj.org/2018/03/when-addiction-opioids-and-disability-meet/
[7] Chronic opioid users received at least 6 prescriptions a year with an average of 13 prescriptions. 20 percent of chronic users were found to take a dose of at least 100 milligrams of a morphine equivalent dose, while ten percent to take 200 milligrams of that dose (https://www.sciencedaily.com/releases/2014/08/140814123612.htm).

points towards a strong association, to the best of our knowledge, no previous research established a causal relationship between the decision to allocate DB to an applicant and their subsequent opioid use. We aim to make a contribution towards filling this gap.

There are several channels through which DB receipt may impact opioid use. First, since DB come with increased access to health insurance,[8] the cost of the opioid drugs and of the doctor visits required for obtaining a prescription may be significantly reduced for DB recipients.[9] Second, since disability beneficiaries can substitute salary income with DB, the absence of regular work requirements may reduce their discipline and concern for the adverse consequences of engaging in opioid abuse. However, there are also forces that may reverse this relationship. For example, people with acute or chronic pain may need to use opioids to alleviate pain during the work hours. Having income without work requirements can reduce the need to alleviate the pain that would otherwise be experienced while executing physically or mentally demanding tasks on a job (our analysis shows, for instance, that DB recipients do reduce their consumption of an opioid substitute, the over-the-counter (OTC) painkillers). The net effect and thus the direction of a potential causal relationship on opioid use is therefore ex ante ambiguous.

Our analysis is performed on cross-sectional data from the 2016 and 2018 waves of the Health and Retirement Study, which among other variables, provide detailed records about Social Security applications and outcomes, while simultaneously, for the first time in all waves, presenting information on the individuals' use of opioids. To obtain more precise counterfactuals, unlike the earlier literature, we use the rejected applicants instead of the general population as

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[8] SSDI beneficiaries are eligible for Medicare after 2 years, while SSI recipients are typically eligible for Medicaid, but SSDI beneficiaries can also apply for Medicaid coverage during the waiting period even if they do not qualify for the program based on income (https://www.disabilitysecrets.com/resources/disability/disability-and-social-security/will-i-still-be). In our dataset a fraction of SSDI beneficiaries do report to have Medicaid coverage.

[9] The cost of opioid treatment, including doctor visits, for three of the most popular drugs was, as collected in 2016, was between $5,980 and $14,112 per year. Source: https://www.drugabuse.gov/publications/research-reports/medications-to-treat-opioid-addiction/how-much-does-opioid-treatment-cost

control group. Therefore, we restrict attention to individuals who applied at least once for DB prior to the date of the interview, and use information on their opioid consumption around the time of the interview.

Being awarded DB is endogenous to opioid use, particularly because opioid use and DB receipt can both be affected by physical disability. To correct the endogeneity of the treatment effect, the identification strategy we employ relies on the fact that both SSDI and SSI application review processes use the individual's age among other medical and vocational factors that determine the application outcome. More specifically, applicants are grouped into four different groups—under age 45, 45-49, 50-54, and 55 and over— and according to the official grid employed in the award decision process, individuals in higher age groups are subject to less stringent requirements, having thus a higher probability of a successful application. This allows using a fuzzy Regression Discontinuity (RD) Design with age as the running variable, and the three age cutoffs (45, 50 and 55) used for identification. We estimate a set of global two-stage regressions, where the age thresholds are used to predict award rate in the first stage, while the causal effect of interest is measured in the second stage. This fuzzy RD approach, first adopted by Van der Klaauw (2002), was also employed by Chen and Van der Klaauw (2008), to investigate the effect of a successful DB application on individuals' labor supply using the same age cutoffs.

Our first stage results confirm that the age groups employed during the application review process do constitute a strong predictor of DB receipt. On the other hand, the key second stage estimate of the causal effect of being awarded DB on opioid consumption indicate that awarding DB increases the likelihood of using opioids by about 27-30 percentage points. We confirm these findings by performing a set of supplementary tests aimed at verifying the validity of the fuzzy RD estimation strategy and the robustness of the results. Our analysis suggests that the clear and widely documented association between the receipt of DB and opioid consumption is likely causal, confirming the concerns about this unintended consequence of the disability insurance programs.

To investigate the mechanism through which DB may impact opioid use, we also run the second stage regression on several other outcome variables. The results indicate that following a positive decision on a DB application, individuals are significantly more likely to be covered by Medicaid, to have insurance pay for prescription medication and less likely to use OTC painkillers. These suggest that a key channel through which DB likely impacts opioid use is the increased access facilitated by the health insurance coverage that comes with DB, which allows DB recipients to substitute the less effective OTC medications for opioids, by lowering the cost of these drugs and of the doctor visits required to have a prescription filled.

The rest of the paper is organized as follows. Section 2 provides a background on SSDI and SSI programs with a focus on the application and appeal process. In Section 3, we describe the data and how we constructed our variables. Section 4 presents our empirical strategy. Section 5 and Section 6 report the summary statistics and the results, including a series of robustness checks, while Section 7 concludes. This paper also has an online appendix. Section A1 from this appendix presents a review of the literature on opioid use and DB from the economics and public health literature. Section A2 presents a series of robustness and placebo tests for our main estimation, while section A3 discusses the effects of awarding DB on other variables of interest with the aim of identifying the most likely channels through which DB impacts opioid use.

2. Background

Both the SSDI and SSI disability benefits programs offer cash benefits and access to health insurance. The key differences between the two programs are given by the factors considered for eligibility and the formula used for computing the level of financial benefits. To be eligible for

SSDI, an individual needs to have earned a certain number of work credits and the financial aid from SSDI is a function of the income and taxes paid before the individual stopped working.[10] On the other hand, SSI is available to low income individuals who do not qualify for SSDI based on work history, as long as their total assets are valued below a certain threshold. [11] The SSI financial assistance is set to provide a minimum level of income that is independent of applicants' past earnings. [12] Both programs are overseen and managed by the Social Security Administration (SSA). Individuals applying for either program face the same application and appeal process and are subjected to the same medical assessment protocols.

The application process consists of an initial application, followed by several rounds of appeals available to an applicant if denied: request for reconsideration, appeal hearing, the appeals council, and the federal appeal. Since our identification strategy relies on features of this application process and the time some of these steps take to reach a decision, we describe it next in detail.

The evaluation of the *initial application* is made in five distinct stages (Lahiri et al, 1995). Failure to meet the requirement from any stage, except for stage three, leads to a denied application. The first stage consists in an earnings screen which most applicants pass. The second stage assesses the severity of applicants' medical impairment. Stage three compares the applicants' medical evidence against minimum standards in terms of the nature and severity of the impairment. Applicants are awarded the disability insurance if they meet the requirements from the first three stages. Otherwise, they are passed to stage four, which evaluates whether the applicant can perform the previous job. If an applicant can carry out the previous job, he is rejected. Otherwise, he is

[10] https://www.disabilitybenefitscenter.org/glossary/cash-benefits
[11] https://www.disabilitysecrets.com/page5-13.html
[12] https://www.ssa.gov/oact/ssir/SSI11/ProgramDescription.html

moved to stage five that measures his capacity to perform other types of work in the economy, by considering vocational factors such as age, education level, and past work experience.

The initial application process described above usually takes three to five months depending on the completeness of an application, on whether the applicants meet medical requirements, on how long it takes for the medical providers to respond to medical record requests, and on how fast SSA can acquire any additional information needed for the decision. Approximately one third of the initial applicants are awarded SSDI or SSI through their first-time application.

If the initial application is rejected, a *reconsideration* (the first appeal step) can be requested, during which the initial application file is reevaluated over the same five stages by a different team of evaluators. With no changes in criteria or rules employed in the evaluation, decision reversals are rare at this step. The reconsideration process normally also takes three to five months.[13]

If the request for reconsideration is rejected, applicants can turn to the second appeal step, and request an *appeal hearing* in front of an administrative law judge. The average waiting time at this point is about 18 months[14]. While the waiting time is significant, the probability of being approved for DB increases dramatically. Around 63% of the applications are successful at the hearing level.[15]

A negative decision at the hearing level can be appealed with the Appeals Council, but fewer than 2% of cases are typically granted benefits at this stage[16]. Finally, the denied applicants can go further to the last level of appeal, the Federal District Court, with even lower rates of success.

[13] https://www.disability-benefits-help.org/blog/timeline-for-disability-claim
[14] https://www.disabilitysecrets.com/disability-reflection-12.html
[15] https://www.disabilitybenefitscenter.org/social-security-disability-application-process
[16] https://www.disabilitybenefitscenter.org/social-security-disability-application-process

In summary, every applicant goes through the initial application with an average of four months. If the initial application is rejected, the applicant can request a reconsideration, but the decision is rarely changed. If one is rejected again, he can proceed to the appeal hearing, where the rate of success increases significantly up to around 63 percent with an 18-month average waiting time, and then to two additional appeal courts where the chances of a decision reversal are very small.

Whenever assessing an applicant's capacity to perform work in the economy, the official grid employed in evaluating a DB application requires that the individual be classified into one of four age groups (under age 45, 45-49, 50-54, and 55 and over), and individuals who are in lower age groups need to meet stricter requirements to qualify for the benefit. This can potentially induce sharp upward jumps in the likelihood of a positive application outcome around these age cutoffs. Chen and Van der Klaauw (2008) found clear discontinuities in the likelihood of being awarded DB at ages 50 and 55, which they employ in their fuzzy RD estimation of the effects of being awarded DB on the labor supply. Our identification strategy, aimed to correct the endogeneity problem of the treatment effect, is built upon using the same discontinuities. Note that while most individuals in our dataset are over the age of 50 at the time of the interview, the relevant age in the first stage of our two-stage estimation is the age at the time of the decision on the DB application. Since there is on average a 7 year gap between the time of the decision and the time of the interview, many applicants in our sample were under 50 at the decision time. Specifically, 21% of them were younger than 45 years old, and 40% were younger than 50 years old. Thus, the fact that the sample is restricted mostly to individuals over the age of 50 at the time of the interview is not a concern for our RD method. However, since the data on opioid consumption is recorded at the time of the interview, it does imply that our insights may be more relevant to the older individuals.

3. Data

We use data from the publicly available Health and Retirement Study (HRS). HRS is a longitudinal survey sponsored by the National Institute on Aging and the SSA. Initially conducted annually, starting with 1998, HRS switched to surveying approximately 20,000 individuals every two years. Aimed at household heads older than 50 and their spouses, it provides information about the health, economic, marital, family, retirement and disability status, as well as about individuals' access to social security benefits, workers' compensation, and veteran benefits. Most relevant for our study, the Social Security section contains information about self-reported application and award decisions for SSDI and SSI, but starting from 2016, also about the individuals' use of opioids.

For each interviewee, the HRS collects the history of SSDI or SSI applications: the date of the first-time SSDI or SSI application, the result of the initial application, individuals' appeal or reapplication decisions, the date and result of the first appeal, the date and result of the last appeal or reapplication, and the date when interviewees started receiving DB. The questions vary slightly across waves, but one can access all answers in the RAND HRS Longitudinal data, which is derived from all waves of the HRS. All Social Security data used in this paper is extracted from the HRS dataset and the RAND HRS dataset.

Sample selection. Our goal is to examine the effect of DB receipt on opioid use by comparing the use of opioid analgesics between the disability beneficiaries and the declined applicants. We define the SSDI and SSI applicants to be the individuals who respond that they have ever applied by the date of the interview and drop the non-applicants. This restricts the validity of our results to a narrower subgroup of the population that can potentially be affected by changes in disability insurance policies, but this is the policy relevant population. We then categorize applicants into the treatment group of disability beneficiaries, who are the individuals who respond that they received DB from either SSDI or SSI at any time prior to the date of the interview, and the control group of individuals who applied, but never received DB.[17]

The key dependent (dummy) variable elicits an individual's use of opioids, and it is constructed based on the responses to the following question: *"Another class of pain medications, called 'opioids', includes such things as Vicodin, OxyContin, codeine, morphine, or similar medications. In the past three months, have you taken any opioid pain medications?"* Because this question only appears in the 2016 and 2018 waves of the HRS dataset, we restrict our analysis to the data from these two waves as a repeated cross section. Since an individual's DB status, opioid consumption, and other characteristics may change between the two waves, individuals appearing in both waves are counted as two separate observations.

To increase the power of the RD design that employs the cutoff ages of 45, 50 and 55, we further restrict our sample to applicants whose ages are between 35 and 65 at the time when they were awarded the DB or had their last application or appeal rejected. We also dropped individuals who are older than 65 at the time of the interview since these individuals are covered by Medicare and our results suggest that the health insurance provided by DB may be a main channel through which DB impact opioid use. As individuals under the age of 50 (2.5% of the sample) are mostly married by the construction of the HRS dataset, and thus constitute a non-random sample of individuals under 50 years old, we also dropped them. As explained in the online appendix, our results are robust to these sample restrictions. Our final sample has 1867 observations in the 2016 wave, and 1298 observations in the 2018 wave, for a total of 3265 observations.

[17] There is no restriction on individuals applying to and receiving benefits from both SSDI and SSI simultaneously. In our study, individuals who applied and/or received benefits from both programs are only accounted once in a wave.

Variables used in the analysis. Besides the variable measuring opioid use defined above, in our analysis we use a set of standard demographic variables and a few additional variables which we describe in the following. The demographic variables capture information about age at time of the interview (denoted "Current Age"), gender, race, ethnicity, education, and marital status. We also use a health-related dummy variable: "Health Problem", created using the question "*Do you have any impairment or health problem that limits the kind or amount of paid work you can do*?". We also employ in our analysis a dummy variable eliciting an individual's OTC painkillers use, which is constructed from the question in the HRS dataset "*In the past three months have you taken any over-the-counter pain medications? Over-the-counter pain medications include such things as Advil, Aleve, Tylenol, aspirin or similar medications*", a dummy variable that elicits if insurance paid for prescription medication, constructed from the question "*Prescription Medications. Did insurance pay for any of that?*", and a dummy variable indicating whether the individual is covered by Medicaid, constructed from the question "*Are you covered by Medicaid*?". We also used two variables eliciting whether the individual and his or her spouse had any type of health insurance at the time of the interview constructed from questions in the RAND HRS dataset that are meant to report whether the individual is covered by different types of health insurance programs. Finally, we constructed a variable "Receive", which elicits if an individual had ever received DB from either the SSI or the SSDI programs. This variable is built upon several questions from the HRS dataset which inquire whether the individual's application has been accepted at the initial review or a step from the appeal process. The variable "Receive" is coded 1 if the answer is "Accepted" to any such question, and 0 otherwise.

Computation of the age at award decision variable. Since the decision on an application for DB is based on the individual's age *at the time the decision is made* (denoted "Decision Age") we need to compute this age from the information available in our dataset, to employ it as the key running variable in our estimation model. The HRS dataset has information on the date of birth of all individuals, and on the date when individuals were awarded the DB if the application was successful, irrespective of whether it was the first-time application, a reapplication, or an appeal. However, the dataset does not have information on the date when applications were rejected. Therefore, while we can calculate the exact age of an individual at the time of a positive decision on an application, we need to impute values for the date of decisions on denied applications so as to compute an individual's age at the time of a rejection. We describe our method next.

First, we classify rejected DB applicants into those for whom the last decision on an application was taken on the initial application and who never reapplied or appealed, and those for whom the last decision was taken during one of the steps in the appeal process. Since the average waiting time for the initial application stage is approximately 4 months,[18] for applicants in the first category, we imputed as the date of the final decision the initial application date, which is available in the HRS dataset, plus 0.3 years.[19]

For the individuals whose initial applications were denied and who continued with the appeals process, but were eventually denied, we do not have information about the precise *step* of the appeals where the applicants stopped, although we do have information on the date when that last appeal step was initiated. To impute a date of the final rejection decision, we need to impute a value for the duration of time before the decision was made at that last appeal step.

[18] https://www.disability-benefits-help.org/blog/timeline-for-disability-claim

[19] Maestas et al. (2013) emphasize the importance of waiting times for the SSDI decisions. One may argue that waiting with limited income from wage earnings might have worsened health and thus result in higher opioid use. However, since all applicants went through the same application/appeal process, this effect is washed out when comparing the treatment and the control groups.

Since the average delay in a decision varies across the different steps of the appeal, we first need to make an assumption on the step of the appeals process where the applicant is likely to have stopped. To this end, we evaluate the incentives of an individual to continue the appeal process by considering the likelihood of a positive decision at the various appeals steps. The starting of the appeal process indicates an eagerness to obtain DB, but since, as described in section 3, hardly any decisions are reverted at the first appeals step (the request for reconsideration), it is unlikely that an individual stops at this step. On the other hand, the acceptance rate in the second step of the appeal process (the appeal hearing), is over 60%, making it very likely that individuals who appeal a rejection decision on the initial application, go to at least this second step. Finally, the further appeal steps – Appeals Council and Federal District Court – have the lowest award rates compared with the previous three steps, as well as a relatively longer average waiting times; making it thus likely that few applicants reach these steps.[20] We thus assume that the appeal hearing is the last step applicants went through if they started their appeal process. To impute a date on the final decision, we also use information on the processing time of this step in the appeals process, which is on average 18 months. Therefore, for individuals rejected during the appeal process, the imputed date of the rejection decision is the date of the last appeal plus 1.5 years.

The use of these imputations lowers the precision of our first stage estimates, but the key results are still statistically significant. As robustness checks, reported in section A3 of the Online Appendix, we estimate the first stage with different imputed values for the delay in the decision on the initial application and on the appeal hearing. The results are similar when the alternative

[20] In fact, when describing the main steps to the SSDI claim process, the official Disability Benefits Help website only lists the details of first three steps: the initial application, the request for reconsideration, and the hearing.

values are near those we employ in our main specification but become statistically insignificant when chosen sufficiently distinct.

4. Empirical strategy

We estimate the effect of being awarded DB on opioid use by comparing opioid use between the treated group (individuals who receive DB), and the control group (the rejected SSDI or SSI applicants). The simplest specification would be an OLS model

$$y_i = \beta + \alpha_i t_i + \theta X + u_i \qquad (1)$$

where y_i is a dummy variable capturing opioid analgesics use, t_i is the treatment variable which is set to equal 1 if individual i received DB, and X is a set of controls. α_i is the parameter of interest, measuring the difference in opioid usage between the treated group and the control group.

However, being awarded DB is endogenous to opioid use, particularly because opioid use and having a disability insurance benefit can both be affected by physical disability. To correct the endogeneity of the treatment effect, following Van der Klaauw (2002) and Chen and Van der Klaauw (2008), we use a two-stage fuzzy RD method. In the first stage, the model is

$$E[t_i|A_i] = \Pr(t_i = 1|A_i) = g(A_i) + \sum_{j=1}^{3} \gamma_j * 1\{A_i \geq \overline{A_j}\} + \alpha X \qquad (2)$$

where A_i is the running variable Decision Age, measuring the age when applicants were awarded or rejected the benefits, and $g(A)$ is a piecewise linear function of age

$$g(A) = \varphi_{00} + \varphi_{01} A + \sum_{j=1}^{3} \varphi_{1j} \left(A - \overline{A_j}\right) 1\{A_i \geq \overline{A_j}\}. \qquad (3)$$

The second term in the right-hand side of equation (2) contains the three age dummies corresponding to the three age cutoffs. The coefficients γ_j capture the discontinuity in the award rate resulting from one of the vocational factors—age. Finally, X is a set of controls which depend on the model and contains a subset of the following variables: individual characteristics (sex, race, ethnicity, education and marital status), and the variable Health Problem, which elicits if the individual has an impairment or health problem that limits the kind or amount of work he or she can do.

By estimating the model in (2) we compute the predicted propensity score of being awarded a benefit, $\hat{E}[t_i|A_i]$. The model employed in the second stage is then

$$y_i = \beta + \delta\hat{E}[t_i|A_i] + k(A_i^c) + \theta X + v_i \qquad (4)$$

where t_i was substituted by the first-stage estimate $\hat{E}[t_i|A_i]$ of the propensity score $E[t_i|A_i]$ and A_i^c is the individual's age at the time of the interview, Current Age. $k(A_i^c)$ is a continuous control function (a first- or second-order polynomial), while *X* is the same set of control variables used in the first stage. δ is the parameter of interest, which captures the causal effect of receiving DB on the likelihood of opioid consumption.

The dummies constructed based on the age cutoffs that we employ in our two-stage estimation are statistically significant in the sample of observations collected in the 2016 wave, and in the combined sample of observations from the 2016 and 2018 waves. They are, however, not significant in the sample created solely from the 2018 wave, which might be due to power issues since we have fewer observations in the 2018 sample. Consequently, we present two sets of estimation results: in the 2016 wave, and in the combined 2016 and 2018 waves.

There are two main concerns when using the RD approach in this study. The first concern is that besides the discontinuities of the award rate at the cutoff ages, other variables may be discontinuous around these cutoffs and thus determine the effects we may observe in the second stage. We address this concern with a series of balancing tests presented in section 6.B. The second concern is a possible nonrandom sorting around the cutoffs, if applicants time their applications or appeals to have decisions on their cases made after the age cutoffs to benefit from the weaker requirements. However, as stated in Chen and Van der Klaauw (2008), while the use of the age cutoffs is in the public domain, the applicants for DB are unaware of the grid used in deciding on an application, or of the use of the age cutoffs. To confirm this, in figures A1 and A2 from the online appendix, we report the results of a density test proposed by McCrary (2008) to check for manipulation of the running variable around these cutoffs.

5. Summary statistics

Table 1.1 presents summary statistics for our restricted samples, constructed from the 2016 wave and the combined 2016 and 2018 waves, respectively, as described in section 3. For each sample, we present separately the summary statistics for the applicants who were awarded DB and for those who were rejected. In terms of demographical variables, applicants who received DB are slightly younger at the time of the decision on the DB application, less likely to be married, and less likely to be of Hispanic ethnicity. Beneficiaries also have a higher probability of experiencing a health problem. DB recipients are also more likely to use opioids, which is consistent with findings from the previous literature, but less likely to use OTC painkillers.

Figure 1.1 shows the DB award rate across different decision ages, using a one-year age bandwidth, in the sample of applicants constructed from the combined 2016 and 2018 waves. The graph provides preliminary evidence for the first stage regression results. As expected from the application approval rules, discontinuities can be observed at the age of 50, 55, and less evidently at 45. Figure A3 from the appendix presents the same information for the 2016 wave.

It is also worth mentioning at this point that the award rate is generally decreasing between the age cutoffs with age at the last decision on an application, especially in the range of 45 to 55 years. This is observed in Figure 1.1 and will also be confirmed later through regression analysis.

While this may appear counterintuitive since health typically worsens with age, a possible explanation of this fact is that individuals may be more likely to apply for DB as they grow older, even when they may not be impaired enough to meet the standards necessary to be awarded these benefits.

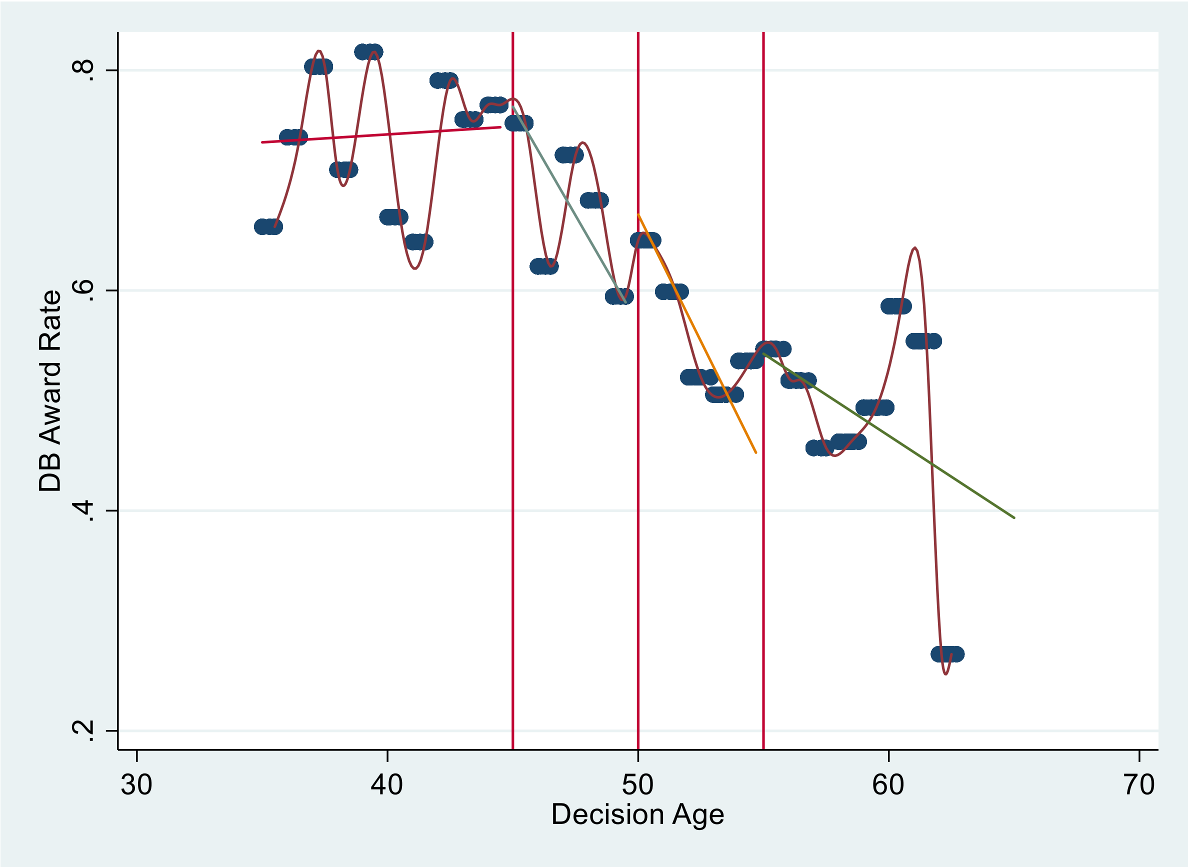

Figure 1.1: Award Rate in the Combined 2016 & 2018 Waves

Table 1.1 Summary Statistics

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	Receive=0 (1)	Receive=1 (2)	Difference (3)	Receive=0 (4)	Receive=1 (5)	Difference (6)
Decision Age	52.787	49.903	-2.884***	52.514	49.635	-2.879***
	(0.237)	(0.210)	(0.321)	(0.178)	(0.155)	(0.239)
Current Age	57.742	57.422	-0.320	58.063	57.871	-0.191
	(0.157)	(0.126)	(0.200)	(0.115)	(0.092)	(0.146)
Female	0.602	0.578	-0.024	0.607	0.585	-0.022
	(0.018)	(0.015)	(0.023)	(0.013)	(0.011)	(0.018)
Hispanic	0.217	0.183	-0.034*	0.216	0.179	-0.038***
	(0.015)	(0.012)	(0.019)	(0.011)	(0.009)	(0.014)
Black	0.382	0.406	0.024	0.383	0.409	0.025
	(0.018)	(0.015)	(0.023)	(0.013)	(0.011)	(0.017)
White	0.446	0.425	-0.021	0.436	0.421	-0.015
	(0.018)	(0.015)	(0.023)	(0.014)	(0.011)	(0.018)
High school	0.469	0.463	-0.005	0.457	0.464	0.008
	(0.018)	(0.015)	(0.023)	(0.014)	(0.011)	(0.018)
College	0.370	0.380	0.009	0.380	0.385	0.004
	(0.017)	(0.015)	(0.023)	(0.013)	(0.011)	(0.017)
Post college	0.024	0.027	0.004	0.025	0.028	0.002
	(0.005)	(0.005)	(0.007)	(0.004)	(0.004)	(0.006)
Married	0.407	0.295	-0.112***	0.410	0.293	-0.117***
	(0.018)	(0.014)	(0.022)	(0.014)	(0.010)	(0.017)
Health problem	0.754	0.911	0.157***	0.793	0.930	0.137***
	(0.016)	(0.009)	(0.017)	(0.011)	(0.006)	(0.012)
Opioid	0.276	0.402	0.125***	0.251	0.378	0.127***
	(0.016)	(0.015)	(0.022)	(0.012)	(0.011)	(0.017)
OTC pain killers	0.681	0.622	-0.059**	0.671	0.622	-0.050***
	(0.017)	(0.015)	(0.022)	(0.013)	(0.011)	(0.017)
Observations	764	1,103		1,363	1,989	

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. The variable Receive takes value 1 if the individual has ever been awarded disability benefits, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

6. Results

In section 6.A we present results eliciting the correlations between DB receipt and opioid consumption, as well as our main estimates from the two-stage model of the causal effect of the DB receipt on opioid consumption. In section 6.B we conduct the balancing tests using scatter plots and regression to support the use of our particular control group as an appropriate counterfactual to the treatment group. In section 6.C we discuss the results from the estimation of the effects of DB receipt on other variables of interest, specifically, the access to Medicaid, the likelihood that insurance paid for some prescription medication, and the OTC pain killers use. Except for the main results, all other results of our analysis are reported in the online appendix.

A. Main results

In each of the two samples we consider in our analysis, we report results from three specifications of the model with different sets of controls. The first column from the panel corresponding to each sample presents the results from a model with the age as the main covariate. The second column includes as controls the individual characteristics. The last column adds the variable Health Problem. Since we use a relatively wide age bandwidth around the age cutoffs, the subsamples of individuals above and below a cutoff, respectively, are slightly unbalanced along these covariates, and thus the dummy variables constructed from the age cutoffs used for identification are correlated with these covariates. Since the covariates may affect the outcome variable (opioid consumption) directly, those dummy variables satisfy the exclusion restriction only once we control for these covariates (Heckman and Vytlacil, 2005; Deuchert and Huber, 2017). [21] However, the results are robust to the inclusion of these covariates. All regressions from the two-stage fuzzy RD model (but not the OLS model, whose results are reported in table 2) include the age-related covariates defined in equation (3), although we do not report the corresponding estimates.

Table 1.2 reports the results from the simple OLS regression defined in equation (1). The results elicit a clear positive correlation among applicants between the receipt of DB and opioid

[21] Adding all these covariates also improves the sample variability of the estimates (Lee and Lemieux, 2010).

use. As expected, the correlation estimate decreases when including the health-related covariate, which controls for some of the underlying factors that induce the association. As the earlier literature (Morden et al., 2014; King et al., 2016; Gebauer et al., 2019) compared the opioid use among the disability beneficiaries relative to the rest of the population, rather than just to the rejected DB applicants, we also report in table A1 from the online appendix the results from the same OLS regression in an expanded sample that includes the non-applicants. The magnitude of the correlation coefficient is larger, indicating unsurprisingly a stronger correlation between the receipt of DB and the opioid use in the general population.

Table 1.2: Correlation between Disability Benefits Award and Opioid Use in the Sample of Disability Benefits Applicants

	(A) 2016 wave			**(B) 2016 & 2018 waves**		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.004	-0.003	-0.003	-0.005**	-0.004*	-0.004**
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Receive	0.124***	0.124***	0.102***	0.127***	0.130***	0.111***
	(0.022)	(0.022)	(0.023)	(0.017)	(0.017)	(0.017)
Year2018				-0.053***	-0.055***	-0.064***
				(0.017)	(0.016)	(0.016)
Individual Characteristics		X	X		X	X
Health problem			X			X
	1,867	1,867	1,867	3,265	3,265	3,265
Observations	0.018	0.028	0.039	0.023	0.034	0.043
R-squared	-0.004	-0.003	-0.003	-0.005**	-0.004*	-0.004**

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the OLS model in equation (1). The dependent variable takes value 1 if the individual took any opioid pain medication during the 3 months prior to the interview date, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table 1.3 presents the first stage estimation results, which elicit significant discontinuities of about 10-12 percentage points in the probability that applicants are awarded DB at the age cutoffs 50 and 55. There is also a positive jump at age 45, but it is not statistically significant.

These results confirm that the age-cutoffs related variables are predictors of the likelihood of a positive decision on the DB application as required for the validity of our two stage estimation method.

Table 1.3: First Stage Results on the Disability Benefits Award

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Decision Age	0.003	0.002	0.001	0.001	0.001	-0.000
	(0.009)	(0.009)	(0.009)	(0.006)	(0.006)	(0.006)
Age45	0.020	0.022	0.027	0.018	0.017	0.028
	(0.067)	(0.067)	(0.065)	(0.050)	(0.049)	(0.048)
Age50	0.111*	0.114*	0.126*	0.099**	0.102**	0.112**
	(0.067)	(0.067)	(0.065)	(0.050)	(0.049)	(0.048)
Age55	0.109**	0.116**	0.115**	0.103**	0.109***	0.107***
	(0.054)	(0.054)	(0.052)	(0.041)	(0.041)	(0.040)
Year2018				0.007	0.008	-0.012
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared	0.049	0.062	0.109	0.049	0.064	0.106

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. Age45 takes value 1 if the individual was 45 years or older at the time of the last decision on a disability benefits application, and 0 otherwise. Age 50 and Age 55 are defined similarly. All regressions are estimated using the model in equation (2). The dependent variable takes value 1 if the individual had ever had a disability benefits application accepted, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table 1.4 reports the second stage results using a linear control function $k(A_i^c)$ in equation (4) (the estimation results using a second order polynomial are presented in table A2 in the appendix). The value of the key coefficient on $\hat{E}[t_i|A_i]$ indicates that awarding DB increases the likelihood that the individual consumes opioids at interview time by about 27-30 percentage points with similar estimates in the two samples. We also estimate a Probit model for the second stage; the results reported in table A3 in the appendix show similar marginal effects. Finally, we also estimate the causal effect with an IV method, where the instrumental variables are three dummy indicators and three interactions of these dummies and the corresponding age gaps determined by the age cutoffs. As shown in table A4, the IV results are similar with that from fuzzy RD, lending credibility to our baseline results.

Table 1.4: Second Stage Results on Opioid Use

	(A) 2016 wave			**(B) 2016 & 2018 waves**		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.002	-0.001	-0.000	-0.002	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$\hat{E}[t_i\|A_i]$	0.272**	0.283**	0.287***	0.274***	0.300***	0.302***
	(0.112)	(0.114)	(0.110)	(0.083)	(0.085)	(0.081)
Year2018				-0.058***	-0.061***	-0.066***
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared	0.005	0.015	0.091	0.009	0.019	0.035

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in Section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in Section 3. All regressions are estimated using the model in equation (4). The dependent variable takes value 1 if the individual took any opioid pain medication during the 3 months prior to the interview date, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Also worth noting in table 1.4 are the negative coefficients on the Year2018 dummy in the combined 2016 and 2018 sample (this dummy takes value 1 if the observation is taken from the 2018 wave), which suggests a significant drop in opioid use among DB applicants between 2016 and 2018. This is likely explained by the fact that starting with 2017 there has been a significant change in the US government's response to the opioid crisis as “the U.S. Department of Health and Human Services declared a public health emergency and announced a 5-Point Strategy to combat the opioid crisis".[22] It is possible that the US government's policy change may have also altered the nature of the relationship between receipt of DB and opioid consumption in the long run, even though our results do not suggest that a change has occurred by 2018 when the second wave of our data was collected. Further research would be needed to investigate this possibility.

B. Balancing and robustness tests

The local continuity of pre-treatment variables around the age cutoffs is essential to allow interpreting the RD coefficients as intended. To validate this condition, we conduct two types of balancing tests. First, we show in figures A4 and A5 from the online appendix scatterplots for eight control variables that are used in regression equation (2). The discontinuities at the three age cutoffs used in the RD estimation the individual characteristics or the health conditions are small. Second, we test if the covariates used in our main model vary discontinuously at the age cutoffs 45, 50 and 55, by using these covariates as dependent variables in regressions similar that from equation (2). As shown in tables A5 and A6 from the online appendix, the coefficients on the age cutoff dummies for most covariates are statistically insignificant, alleviating concerns of other meaningful discontinuities at the age cutoffs.

In section A2 from the online appendix we also present the second stage results for some additional outcome variables which should not be affected by DB as placebo tests for our estimation exercise, and a series of other robustness checks for both stages of our two-step estimation.

[22] https://www.hhs.gov/opioids/about-the-epidemic/index.html

C. Other effects of awarding disability benefits

Apart from the main objective of measuring the effect of the disability insurance programs on opioid consumption, we also ran our estimation model using other outcome variables potentially impacted by these programs. We aim to identify other possible consequences of awarding DB, and by means of this to also potentially better understand the mechanisms through which awarding DB may affect opioid use. The estimations and their results are discussed in more detail in section A3 from the online appendix.

We observe a significant impact of about 50 percentage points on the likelihood that the individual is covered by Medicaid, a roughly equal impact on the likelihood that insurance paid for prescription medication, and a decrease of about 20 percentage points in the likelihood that the individual uses OTC painkillers. In columns (1) and (4) from table A7 in the online appendix, we also report the results of a regression where we add a variable Own Insurance, which captures whether the individual was covered by insurance at the time of the interview. The coefficient on this variable is positive and significant, while the key coefficient on $\hat{E}[t_i|A_i]$ is reduced in magnitude to 25-26 percentage points.[23] These secondary findings suggest as one likely channel for the main effect that we identify, the increased access to opioids through health insurance coverage. This can both lower the cost of opioids, but can also reduce the cost of the doctor visits necessary for prescription medications, allowing individuals who experience pain to substitute the OTC pain killers with the more potent opioid analgesics.

[23] In the same table A7, we also report the results of a regression where we add the spouse's insurance status as a control variable, but this factor does not appear to be a significant driver of opioid consumption.

7. Conclusion

A growing body of evidence suggests that the receipt of SSDI and SSI benefits is positively correlated with opioid consumption (Morden et al., 2014; King et al., 2016; Zhou et al., 2016; Gebauer et al., 2019). However, there is little evidence of a potential causal effect of disability benefits on the use of opioids. In this paper, we aimed to contribute towards filling this gap.

Since in the official grid used in evaluating disability benefits applications, individuals are classified into four age groups, and stricter rules are required for applicants who are in a lower age group, to solve the endogeneity problem, we employed the fuzzy RD design from Van der Klaauw (2002) and Chen and Van der Klaauw (2008), with age at decision time as the running variable. The fuzzy RD design is equivalent to a two stage estimation, where we use age-related variables to predict the award rate for DB in the first stage, and our first stage results show that these age-related variables are valid predictors. We then use the predicted award rate in the second stage in place of the treatment variable in a regression where the dependent variable captures opioid consumption. The key second stage results suggest that a successful disability benefits application increases the likelihood that the individual will consume opioids by about 27-30 percentage points. We confirmed these results with a series of robustness checks for both stages of the estimation.

Overall, our results provide evidence for a positive causal effect of awarding disability benefits on opioid consumption, thus confirming the concerns regarding a possible unintended consequence of the disability benefits programs raised by the evidently higher rates of opioid consumption among their recipients observed by earlier literature. More research needs to be done to study this question, possibly with larger datasets or more powerful predictors that would allow obtaining more precise estimates of this effect. Another direction for future research would be to investigate other outcomes that may be associated with DB award given the increase in opioid consumption, such as a potential substitution away from or towards other more concerning drugs. More research is also needed to confirm that this unintended effect continues to be present after the recent change in the US government's emphasis on the opioid crisis, which appears to have decreased opioid consumption, at least among the disability applicants, but may have also changed to a certain extent the nature of the relationship we investigated in this article. Finally, it is worth keeping in mind that opioids are likely beneficial in certain situations. While the line between the two may be subtle and hard to assess objectively, especially due to the danger that an initial beneficial use of opioids may lead to addiction and abuse in the long term, it would be important in further research to attempt to distinguish opioid abuse from medically valid opioid use. However, overall it is widely recognized that there has been a significant degree of opioid over-consumption in the US recently, and our paper makes a contribution by identifying one factor that increased the use of opioids.

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Chapter 2: The effect of the drug abuse prevention programs on intimate partner violence and child maltreatment[24]

Minglu Sun

1. Introduction

Domestic violence has been a major public health concern due to its meaningful adverse consequences and its prevalence, with statistics showing that 1 in 4 women and 1 in 9 men have suffered from some form of serious domestic violence.[25] The costs of domestic violence exceed 5.8 billion every year in terms of the medical and mental health care services (Department of Health and Human Services 2003), in addition to the economic output losses of their employers, victims' emotional costs, as well as costs incurred by other government services such as criminal justice system. Besides the domestic violence towards the partner, child maltreatment is another serious issue highly co-occurring within households: "65% of adults that abuse their partner also physically and/or sexually abuse their children."[26] Furthermore, children can be harmed emotionally or physically from domestic violence even if they are not the intended targets. In the long run, children who suffered from abuse or were exposed to domestic violence during childhood are more likely to perpetrate domestic violence to their partners. The previous literature has examined multiple socio-economic factors that can contribute to domestic violence, such as gender-specific income, social norms, and financial stress. Prior studies (Crane et al., 2014; El-Bassenl et al., 2007; Smith et al., 2012; Stene et al., 2012; Subodh et al., 2014; Tran et al., 2014;

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[25] https://ncadv.org/STATISTICS
[26] https://violence.chop.edu/domestic-violence-and-child-abuse

Wuest et al., 2008) have also shown that people with substance abuse history are more likely to be engaged in intimate partner violence, but without establishing a causal relationship between them. I aim to fill this gap and investigate whether a policy intervention that decreases access to addictive substances lowers the prevalence of domestic violence and child maltreatment.

Specifically, I focus on the Prescription Drug Monitoring Programs (PDMPs) that were introduced over decades to address the rising opioid crisis in the United States. PDMPs are electronic databases recording patients' prescriptions of controlled substances, including opioids. When first introduced, none of state governments mandated the use of PDMPs, but since the utilization of these databases was low, starting from 2007, most states gradually switched to the Mandatory Access (MA) PDMPs, where checking patients' prescription history is required for healthcare providers at the time when they provide their services. As one of the largest state-level programs to curb the opioid epidemic, MA PDMPs were found to be effective in decreasing the utilization of opioids (Alpert et al., 2020; Buchmueller and Carey, 2018; Neumark and Savych, 2021; Sacks et al., 2021). I investigate whether the implementation of MA PDMPs had a spillover effect on reducing the prevalence of domestic violence.

There are several mechanisms through which a drug control policy could impact domestic violence. On one hand, decreased exposure to opioids after the adoption of MA PDMPs could prevent criminal acts pharmacologically executed under the influence of opioids[27]. In addition, reduced access to opioids may also cease arguments about quitting opioids, lowering the likelihood of triggering domestic violence. On the other hand, sudden restriction on opioids could lead to involuntary withdrawal process, which is strongly related to exaggerated aggressive behaviors. Additionally, MA PDMPs could have caused a substitution of opioids with other illicit drugs that

[27] Moore et al. (2010) show that oxycodone is highly associated with violence-related adverse drug events.

are more likely to induce irritability or physical aggression, such as cocaine and heroin. Apart from channels with straightforward outcomes being mentioned above, decreased access to expensive opioids could relax household financial distress either directly or more importantly, through improved job opportunities. As shown from the prior evidence, the released financial burden may both increase (Bhalotra et al., 2021; Bloch and Rao, 2002; Bobonis et al., 2013) or decrease (Angelucci, 2008; Farmer and Tiefenthaler, 1997; Haushofer et al., 2019; Health et al., 2020; Hidrobo et al., 2016) domestic violence. Overall, the net effect on domestic violence is ex ante ambiguous.

The causal effect is estimated from comparing the variations in domestic violence in the geographical areas exposed to MA PDMPs with those not. I employ a difference-in-differences strategy combined with an event study model, exploiting the implementation of MA PDMPs in a scattered manner across states. The adoption date information of MA PDMPs across different states is extracted from Buchmueller and Carey (2018) and Sacks et al. (2021).

To establish the effect of MA PDMPs on domestic violence, I use the data from the National Incident-Based Reporting System (NIBRS) which offers three major advantages. First, it contains detailed information on several crime categories, including homicide, assault, and sex offense, the three categories that capture physical and sexual violence. Second, NIBRS data enables the identification of violence targeting intimate partners and children, by indicating the relationship between victims and offenders in each crime report. Third, geographic identifiers (police agency) provided in its victim-level raw data offer compilation flexibility to generate city- and state- level statistics.

As a measure of the intensity of partner violence or child maltreatment, I use the number of offenses per 100,000 covered population. To generate a larger reasonable representative unit while maintaining a reasonable statistical power, the victim-level raw data is aggregated to the city level; the sample ended up with 2095 cities from 31 states.

The results of my analysis show that drug abuse prevention programs significantly decreased intimate partner violence as well as child maltreatment. On average, MA PDMPs decreased intimate partner assault by around 27 offenses per 100,000 population, which is equivalent to a decline of 9%. As for child maltreatment, 13.6% fewer children suffered intimidation from their parents or grandparents. To check dynamic impacts over time, I plot the event study for four years before and four years after MA PDMPs. The plots suggest that the decline trends of domestic violence and child maltreatment start precisely upon implementation of the policy, while prior to MA PDMPs, the incidences of domestic violence and child maltreatment share similar trends and magnitude levels among the treated and control states.

As a first robustness check, I overcome the known shortcoming of the DID estimation in the scenario of multiple time periods and variation of treatment timing by applying the method from Callaway and Sant'Anna (2021), which addresses the treatment effect heterogeneity. The results from this alternative model specification are quantitatively similar to those from the standard DID model.

Additionally, I test the sensitivity of the results by controlling state-level dynamic covariates such as basic demographics, unemployment rate, and educational attainment. I apply the "leave-one-out" strategy to check whether the results are driven by certain states. To address the skewed distribution of count variables in the regression, I employ the logarithm transformation of dependent variable. To alleviate the concern that other opioid-related regulations might overlap with MA PDMPs, I control several other policies including pill mill laws, OxyContin reformulation, naloxone access laws as well as Good Samaritan Law. On top of all the robustness checks mentioned above, I estimate the causality in a larger geographical unit—state level. Overall, the baseline results are robust from multiple model specifications.

This paper has several contributions. First, it contributes to the growing literature by suggesting a potential factor that can impact the prevalence of domestic violence. It has been shown that the conditional and unconditional cash transfer programs (Angelucci, 2008; Bobonis et al., 2013; Haushofer et al., 2019), female employment (Anderberg et al., 2016; Erten and Keskin, 2021), lower gender wage gap (Aizer 2010), unilateral divorce laws (Garcia-Ramos, 2021; Stevenson and Wolfers 2006), education (Erten and Keskin, 2018; Erten and Keskin, 2020), social norms (Gonzalez and Rodriguez-Planas, 2020), the role of police demographics (Miller and Segal, 2019) as well as the timing of benefits disbursements (Carr and Packham, 2021) can affect the prevalence of intimate partner violence. Recently, more studies examined the effect of the "lockdown" policy during the Covid-19 Pandemic on domestic violence (Bullinger et al., 2021; Leslie and Wilson, 2020). My paper provides the first evidence of the drug control policy decreasing the level of domestic violence.

Additionally, this paper extends the literature on the spillover effects of drug control policies. The growing literature has studied the spillover effects of the drug abuse prevention programs on the use of more harmful or illegal drugs (Alpert et al., 2018; Evans et al., 2019; Mallatt, 2022), labor supply (Kaestner and Ziedan, 2019), disability duration (Neumark and Savych, 2021), and child welfare (Evans et al., 2020; Gihleb et al., 2019). Most relevant to my paper, two previous literature examines the unintended effect of MA PDMPs on the crime dimension. Dave et al. (2020) study the impact on violence and property crimes, and Mallatt (2022) examines the impact on heroin-related crimes. I contribute to providing the first evidence of MA PDMPs decreasing the incidence of domestic violence.

Finally, even though earlier research has exploited how MA PDMPs impact child welfare, different trends are observed due to the divergence in outcome variables being investigated. Gihleb et al. (2019) find that the MA PDMPs decrease foster care admissions, while Evans et al. (2020) show that MA PDMPs increase child abuse and neglect among counties with higher initial exposure to opioids prior to the intervention of the program. My research complements the literature in several dimensions. For example, child maltreatment with clearly defined crime types (homicide, assault, and sex offense) is analyzed, instead of measuring under a vague category of child abuse. In addition, the treatment effect is estimated from more rural-concentrated geographic locations[28] compared with the relatively urban-focused sample from Evans et al. (2020).

The rest of the paper is organized as follows. In section 2, I provide a more detailed background for MA PDMPs and the relationship between domestic violence, child maltreatment and substance abuse. Section 3 describes data sources and the sample construction. The empirical strategy is presented in section 4; summary statistics and results are shown and discussed in sections 5 and 6, respectively. Section 7 concludes.

2. Background

A. Domestic violence, opioid use, and drug abuse prevention programs

How does domestic violence relate to opioid use? Previous literature only found evidence of correlation: On one hand, people with a history of opioid use are more likely to perpetrate violent behavior towards an intimate partner than those without such history (Crane et al., 2014; El-Bassenl et al., 2007; Subodh et al., 2014). On the other hand, people who suffer from domestic

[28] Kaplan J (2021). National Incident-Based Reporting System (NIBRS) Data: A Practitioner's Guide. https://nibrsbook.com/

violence also have a higher chance of using opioid than the general population. Not only do domestic violence victims turn to opioid for alleviation of physical pain or mental trauma (Smith et al., 2012; Stene et al., 2012; Tran et al., 2014; Wuest et al., 2008), opioid users also have higher chance of being abused by intimate partners. In a word, opioid use is positively correlated with the engagement of domestic violence but to the best of my knowledge, no literature studies the causal relationship between opioid use and domestic violence.

The major obstacle of investigating such topic is the endogeneity. This may be caused by unobserved factors, such as mental health conditions related to both opioid use and domestic violence at the same time. Failing to control them will bias the estimates in this scenario. In addition, endogeneity can come from reverse causality mentioned above, since opioid users might be identified as either crime perpetrators or victims.

This paper circumvents these complications by measuring how the supply shock of opioids, resulting from drug abuse intervention programs, affects domestic violence, specifically, the introduction of the Mandatory Access Prescription Drug Monitoring Programs.

B. Mandatory Access PDMPs

Prescription Drug Monitoring Programs (PDMPs) are statewide electronic databases designed to track the prescribing and dispensing of controlled substances. Being the largest state-level program to combat the opioid overdose epidemic, as of December 2019, PDMPs are legislatively authorized across all states in the US except the state of Missouri[29].

Theoretically, PDMPs help curb the opioid crisis from multiple channels. By listing patients' drug use history, PDMPs enable healthcare providers (physicians or pharmacists) to

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

identify patients at risk of overdose or dangerous co-prescribing and adjust inappropriate prescriptions. In addition, PDMPs can indicate the "doctor shopping" behavior, when a patient intentionally visits different doctors or pharmacies for multiple opioid prescriptions. In reality, however, previous literature (Buchmueller and Carey, 2018; Meara et al., 2016; Grecu et al., 2019) found no evidence that PDMPs reduced opioid use or abuse. Such inefficiency is driven by the low take-up rate of PDMPs when the healthcare providers can voluntarily choose to use them or not, according to Buchmueller and Carey (2018). Since aforementioned PDMPs are voluntary, checking patients' prescription history is not legislatively required by state governments for prescribers or pharmacies.

In 2007, Nevada became the first state requiring prescribers and dispensers to query PDMPs before prescribing controlled substances. This is an upgraded version of optional PDMPs – "Mandatory Access" or the "Must Access" (MA) PDMPs, which more states gradually switched to from the optional PDMPs. I am focusing on MA PDMPs as the drug control policies in this paper, with ten states implementing MA PDMPs in the sample (more information about the policy status across states provided in Figure A1). Under the regulation of MA PDMPs, when failing to query the system or failing to submit prescriptions to PDMPs, healthcare providers will face criminal or civil penalties (Grecu et al., 2019), which not only prevents the doctor shopping behavior but also effectively reduces the prescription of high-dose opioids or even other doses, prompting a general decline trend in all types of prescribed opioids (e.g., OxyContin, Vicodin, Morphine, Methadone, etc.) (Alpert et al., 2020; Buchmueller and Carey, 2018; Neumark and Savych, 2021; Sacks et al., 2021), thus likely to influence opioid use-related domestic violence. Furthermore, the evidence of MA PDMPs decreasing utilization of opioids regardless of patients' drug use history may indicate another important pathway of curtailing the domestic violence related to opioids, since less exposure means fewer chances of turning opioid-naïve people into users.

C. Child maltreatment and drug abuse prevention programs

The adverse effect of drug use on child welfare has been substantially documented in the literature. Exposure to opioids in the womb before birth is more likely to cause the neonatal abstinence syndrome, the conditions when a baby is forced to withdraw from the substances. In addition, the risk of being neglected or maltreated increases for children when their parents abuse opioid.

The effect of substance abuse intervention policies on child maltreatment is hardly studied in the literature and the existing studies have multiple limitations. First, in most of the studies, child maltreatment is indirectly measured by the admissions in foster care system, which only captures a proportion of the effect since the group of children who are removed from the family are a subset of children who suffer from child maltreatment. Second, the inaccessible individual level data makes the previous research rely more on the aggregated county- or state- level summaries. These restrictions make it hard to exploit the precise effect of drug abuse prevention programs on child maltreatment.

3. Data

The primary dataset is the National Incident-Based Reporting System (NIBRS), which collects multiple categories of crimes. Under the administration of the Federal Bureau of Investigation, police agencies report crimes at the incident level with detailed information such as the incident date, location, the number and characteristics of victims and offenders, whether weapons or property are involved in crimes, and more importantly to my study, the relationships between offenders and victims.

The NIBRS raw datasets are structured into six segment levels: administrative, offense, property, victim, offender, and arrestee. The data are stored in multiple single files, which vary in length and layout across segment levels, making it complicated to clean and use directly. As an alternative, the crime data merged, reconstructed, and maintained by the National Archive of Criminal Justice Data (NACJD) is a well-organized version of the NIBRS.

In the cleaned dataset from NACJD, the complete information of NIBRS is reconstructed into four separate files—crime incident, victim, offender, and arrestee. Each file contains full information under its own category (unit of analysis), while the number of records is limited to three for each type, for the rest of the segment levels merged therein. For instance, the victim level file includes all victims, and maximum of three records each for offense, property, offender, and arrestee segments. Such feature of data organization does not harm the completeness of the sample, based on the fact that "99% of NIBRS incidents have three or fewer offense, victim, offender, and arrestee records" (NACJD, 2021).

The goal of this paper is to examine the effect of MA PDMPs on domestic violence and child maltreatment, which theoretically can be counted by the number of victims. Thus, I use the victim-level dataset rather than the other three to guarantee direct measurement of the prevalence and intensity of crimes.

It is also worth mentioning that personal identifications are not provided in the dataset, indicating the probability of some recurring victims suffering from several domestic violence or child maltreatment offenses between 2007 and 2016. In this specific context, the measurement of domestic violence and child maltreatment should be better referred to as the number of offenses instead of the number of victims.

Ideally, all police agencies are consistently reporting crimes, however individual police agency can voluntarily start and stop the process in reality. As the level of domestic violence being quantified with the cumulative number of offenses, the number of active reporting agencies is positively correlated with the prevalence of domestic violence shown from the data. In order to create a balanced sample avoiding unpredictable starting or ceasing reporting from police agencies, I check the reporting history and select those police agencies consistently showed up in the system every month over entire study period.

The selection of study period will need to balance the length of time spectrum and sample representativeness. Longer study period enables the measurement of the long-run effect, while the tradeoff is less qualified police agencies due to miss reporting, leading to a less representative sample. The final study period covers from the fiscal year 2007 to 2016, the last publicly available year in the dataset.

When choosing analysis unit, one major concern of using police agency is that the values of dependent variables most likely distribute in the range of small numbers. To circumvent this issue, I aggregate the raw data to the city level, which ended up with a balanced panel of 2095 cities across 31 states. However, choosing city level impose an immediate challenge on comparing the number of offenses across analysis unit—it is meaningless to use the city total population for weighting, since my sample only picks a subset of police agencies within a city. Nevertheless, one advantage of the NIBRS makes it possible to compare the prevalence of offenses among cities, which is the dynamically reported population covered by each police agency.

I focus on three categories of crimes that best describe the topic of physical and sexual violence in this study: homicide, assault, and sex offense. Since assault and sex offense are more prevalent, their subcategories (simple assault, aggravated assault and intimidation, as well as forcible sex offense and non-forcible sex offense) are also taken into analysis. In this paper, domestic violence is identified as the aforementioned crimes conducted by an intimate partner, specifically, when the relationship between victims and offenders belongs to one of the following: spouse, common-law spouse, boyfriend or girlfriend, homosexual relationship, and ex-spouse. Similarly, child maltreatment is identified when the victim is the child, grandchild, stepchild of perpetrator, or the child of the offender's boyfriend or girlfriend. The prevalence of domestic violence and child maltreatment is measured by the number of offenses reported at the city-year level.

4. Identification strategy

The goal of this paper is to estimate the effect of MA PDMPs on intimate partner violence and child maltreatment. To this aim, exploiting the staggered implementation of MA PDMPs, I employ the difference-in-differences (DID) specification following equation (1).

$$Y_{cst} = \beta MA\_PDMPs_{st} + \gamma_c + \delta_t + \varepsilon_{cst} \qquad (1)$$

Where Y_{cst} represents an outcome such as the number of offenses from intimate partner assault per 100,000 covered residents for city c located in state s and in year t. MA_PDMPs_{st} is a dummy variable which is set to be one if the located state s of city c has MA PDMPs in year t. In our study period, there are 19 states implementing MA PDMPs. γ_c and δ_t are city and year fixed effects, which capture the underlying differences across cities and the common trend on domestic violence or child maltreatment for cities over time, respectively. Since the programs were implemented at the state level, the robust standard errors are clustered at the state level. The parameter of interest is β, which estimates the net impact of MA PDMPs on domestic violence or child maltreatment. As the history of substance use for both offenders and victims cannot be identified from data, I measure the intent-to-treatment effect in this paper.

Recent literature (Goodman-Bacon, 2020; Callaway and Sant'Anna, 2021) has raised the concern of treatment effect heterogeneity with the two-way fixed effects DID model. The major issue under the classical DID specification is that when the already treated units are included in the control group for comparison with newly treated units, they cannot reflect the untreated potential outcome. In other words, the treatment effect dynamics will be included in the parameter of interest, which might bias the results. To address this potential issue in this context, I use the method by Callaway and Sant'Anna (2021) to check the robustness of the results.

Event study The most important requirement of using DID is that the treated units and untreated units would follow parallel trends over time had the exogenous shock never happened. To check whether the assumption holds in this context and to measure the dynamic net effect, I conduct the event study following the equation (2).

$$Y_{cst} = \sum_{j=-4}^{-1} \theta_j D_{s,\tau+j} + \sum_{j=0}^{4} \beta_j D_{s,\tau+j} + \gamma_c + \delta_t + \varepsilon_{cst} \quad (2)$$

Where Y_{cst} is the outcome variables such as the number of offenses from intimate partner assault in city c located in state s and in year t. The city and year fixed effects are also controlled in this specification. The estimation is weighted by the covered population in city c as reported from NIBRS. Since MA PDMPs were implemented at the state level, I cluster robust standard errors at the state level.

While in equation (2), the dummy variable MA_PDMPs_{st} is replaced with a vector of dummies to examine the lead and lag effects of MA PDMPs. The indicator $D_{s,\tau+j}$ equals to one if state s has implemented MA PDMPs j years away from the legislation year τ. The estimation is normalized at the last year without MA PDMPs. Thus, θ_j and β_j represent the difference between the treated and control cities in the outcomeY_{cst}, using the year prior to the implementation used as the reference.

5. Summary statistics

Table 2.1 shows the summary statistics of offenses for both intimate partner violence and child maltreatment, which is stratified by the treatment status of MA PDMPs in the analytic sample. Among 2095 cities in my sample, 1220 cities did not implement MA PDMPs from 2007 through 2016, constituting the control group, shown in column (1). The rest cities adopted MA PDMPs in a staggered manner during the study period. Based on the implementation time, they are further subcategorized into one before-treated group, as shown in column (2), and one after-treated group, as shown in column (3), to make a more direct comparison. Column (4) shows the comparison between the control group and the pre-treated treatment group. Column (5) shows the comparison between the control group and the after-treated treatment group.

The table is separated into two panels showing the average crime offenses from intimate partner violence and child maltreatment per city each year. All statistical numbers are weighted by 100,000 covered population. Table 2.2 shows homicide is the least prevalent crime, with less than 0.5 offenses per 100,000 covered population at the city-year level. Assault and sex offense have a relatively higher offense rate. Thus I check their subcategories, aggregative assault, simple assault and intimidation, as well as forcible sex offense and non-forcible sex offense. As shown in the first three columns, the offense of simple assault accounts for more than 70 percent (60 percent) of total assault offenses from intimate partner violence (child maltreatment). The offenses of sex offense are mainly driven by the forcible sex offense. Instead of looking at the general difference in crimes between the treatment group and control group, I check two differences by comparing offenses of control group with that from pre-treated treatment units and post-treated treatment cities separately. Columns (4) and (5) suggest some differences in the criminal offenses between the three groups when averaging on the city-year level. The causal relationship and the check for the parallel trend in crime offenses are shown in the next section.

Table 2.1 shows some individual characteristics (age, gender, and race) of offenders and victims who are engaged in domestic violence or child maltreatment. Following the same format, the table is stratified by the treatment status for all city-year level units. It is worth mentioning that females and males are roughly equally likely to be victims from intimate partner violence or child maltreatment, while males are more likely to be perpetrators.

Table 2.1 Summary Statistics (Individual Characteristics)

	(1) Control: Never MA PDMPs		(2) Treatment: Before MA PDMPs		(3) Treatment: After MA PDMPs		(4)	(5)
	Mean	SD	Mean	SD	Mean	SD	(2) - (1)	(3) - (1)
Individual characteristics								
Victim								
Age	38.993	3.818	39.920	3.484	40.943	3.688	0.927***	1.950***
Female	0.492	0.059	0.500	0.051	0.501	0.060	0.008***	0.009***
White	0.889	0.167	0.881	0.151	0.890	0.146	-0.008**	0.001
Black	0.095	0.165	0.109	0.150	0.099	0.143	0.014***	0.003
Offender								
Age	33.054	3.354	33.657	3.059	33.944	3.116	0.603***	0.890***
Female	0.228	0.097	0.230	0.087	0.235	0.093	0.002	0.006**
White	0.827	0.220	0.818	0.203	0.831	0.194	-0.010**	0.003
Black	0.148	0.217	0.174	0.203	0.157	0.189	0.026***	0.008
N City-year	12200		6317		2433			

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Column (1) reports the summary statistics for the cities that never adopted MA PDMPs across the study periods. Column (2) and column (3) display the statistics for cities that implemented MA PDMPs between 2007 and 2016, while they are separately by the implementation time.

6. Results

A. Domestic violence

Following the estimation equation (1), the causal relationship between MA PDMPs and domestic violence is shown in Table 2.3. The first three columns show the results on homicide, assault and sex offense. And the rest show the estimation results on their subcategories.

Table 2.2 Summary Statistics (Offense Rate per City-year Level Weighted by 100,000 Covered Population)

	(1) Control: Never MA PDMPs		(2) Treatment: Before MA PDMPs		(3) Treatment: After MA PDMPs		(4)	(5)
	Mean	SD	Mean	SD	Mean	SD	(2) - (1)	(3) - (1)
Intimate partner violence								
Homicide	0.494	2.777	0.460	1.991	0.412	2.057	-0.033	-0.082
Assault	377.316	430.982	421.171	361.144	389.432	330.308	43.855***	12.116
Agg. assault	46.754	72.031	44.579	56.850	49.810	67.775	-2.175*	3.055
Simple assault	289.045	365.683	304.872	272.504	274.456	244.562	15.827**	-14.589
Intimidation	41.517	69.552	71.719	121.129	65.167	110.448	30.203***	23.650***
Sex offense	10.092	22.328	5.852	9.859	6.277	10.838	-4.240***	-3.815***
Forcible	7.742	20.096	4.142	7.727	4.676	8.801	-3.600***	-3.066***
Non-forcible	2.351	8.195	1.711	5.666	1.602	5.792	-0.640***	-0.749***
Child maltreatment								
Homicide	0.260	2.930	0.236	1.797	0.193	1.815	-0.025	-0.067
Assault	61.430	68.841	75.345	79.738	76.443	81.446	13.915***	15.013***
Agg. assault	10.872	21.116	9.897	19.412	11.396	21.161	-0.976**	0.524
Simple assault	46.417	58.005	50.995	53.658	47.243	52.819	4.578***	0.826
Intimidation	4.140	12.348	14.452	44.013	17.804	48.565	10.312***	13.663***
Sex offense	13.165	19.829	8.115	12.936	8.859	13.617	-5.051***	-4.306***
Forcible	11.755	18.316	6.742	11.951	8.042	13.252	-5.013***	-3.714***
Non-forcible	1.410	6.972	1.373	4.878	0.817	3.304	-0.037	-0.593***
N City-year	12200		6317		2433			

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Column (1) reports the summary statistics for the cities that never adopted MA PDMPs across the study periods. Column (2) and column (3) display the statistics for cities that implemented MA PDMPs between 2007 and 2016, while they are separately by the implementation time.

The first column indicates that, on average, MA PDMPs decrease the number of intimate partner homicide by 0.09 offenses per 100,000 covered residents, which is equal to a decline of 18.4 percent. Apart from the impact on homicide, MA PDMPs can decrease 33 offenses of intimate partner assault among every 100,000 covered population per city each year, which corresponds to a decline of 9 percent. When zooming into the subcategories of assaults, I find that the impact on assault is mainly driven by the decreased offenses from simple assault. The impacts from aggregated assault and intimidation are of small magnitude level and not statistically significant, As for the sexual violence from intimate partners, the estimation results do not suggest any effect on sex offense and its subcategories.

Table 2.3 The Effect of MA PDMPs on Intimate Partner Violence (TWFE DID)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.09**	-33.40**	0.08	0.35	-31.77***	-1.98	0.011	0.70
	(0.04)	(15.70)	(0.35)	(4.26)	(10.48)	(5.80)	(0.31)	(0.22)
Mean	0.47	391.90	8.37	46.45	292.10	53.37	6.30	2.07
Percent	-0.184	-0.0852	0.00969	0.00747	-0.109	-0.0371	0.00179	0.0337
p-val	0.0334	0.0417	0.816	0.936	0.00497	0.735	0.971	0.753
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950
R-squared	0.157	0.938	0.529	0.817	0.929	0.838	0.477	0.424

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 covered population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

One primary concern about the two-way fixed effect (TWFE) DID empirical strategy is that the estimators might be biased when the sample has multiple time periods, and there are various treatment timing (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). In this context, the sample covers 31 states over ten years, and MA PDMPs were implemented in a staggered manner. Under this scenario, all observations can be categorized into three groups: the never treated units, the relatively early-treated units, and the relatively late-treated units. To get the average treatment effect on intimate partner violence and child maltreatment, those early treated units will be included in the control group to compare with the late-treated units. However, the estimators from the TWFE DID model can be biased because the early-treated units can not perfectly reflect the path of untreated potential outcomes.

To check the validity of the baseline results from the TWFE DID specification, I apply the method from Callaway and Sant'Anna (2021). Table 2.4 suggests that the average treatment effects from the new method are similar with the baseline results. The drug control policy can significantly decrease the offense of intimate partner assault, which is driven mainly by the decline in the simple assault. Both results are comparable in terms of magnitude and statistical level. However, the impact on homicide is no longer significant, suggesting it is less robust than intimate partner assault.

It is also worth mentioning that Callaway and Sant'Anna method requires excluding the units that were treated before or right in the first year of the study period. This paper covers the ten-year period starting from 2007, and Nevada is the first state implementing MA PDMPs in 2007. Thus, all cities from the state of Nevada are dropped when applying the Callaway and Sant'Anna method, ending up with 20,600 city-year level observations.

Table 2.4 The Effect of MA PDMPs on Intimate Partner Violence (CS Method)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.06	-31.16**	-0.17	-1.76	-31.11***	1.70	-0.04	-0.14
	0.05	12.12	0.31	3.30	7.12	4.30	0.29	0.13
Mean	0.47	391.90	8.37	46.45	292.10	53.37	6.30	2.07
Percent	-0.12	-0.08	-0.02	-0.04	-0.11	0.03	-0.01	-0.07
Observations	20,600	20,600	20,600	20,600	20,600	20,600	20,600	20,600

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

The average treatment effect indicates that MA PDMPs can significantly decrease the offenses of intimate partner assault and simple assault. To examine the impacts dynamically, I plot the event study graphs in Figure 1 for three crimes, homicide, assault and simple assault, using the method proposed by Callaway and Sant'Anna (2021).

Prior to the implementation of MA PDMPs, little systematic differences in the number of offenses between the treatment and control groups are observed, indicating that the parallel trend assumption holds in this context. The trend is shared among all crime categories and not limited to homicide, assault, and simple assault, even though only those three are plotted in Figure 2.1.

As shown in panel (a), the number of homicide offenses drop significantly right after implementing MA PDMPs. The impact lasts for two years, and it shrinks afterward. At the same time, the magnitude of standard errors keeps increasing over time. As shown in Panel (b) and Panel (c), the dynamic impacts on assault and simple assault are similar. First, the number of offenses starts decreasing right after MA PDMP, and it keeps decreasing over the course of four years. Second, the impacts on assault and simple assault are statistically significant for each single post period.

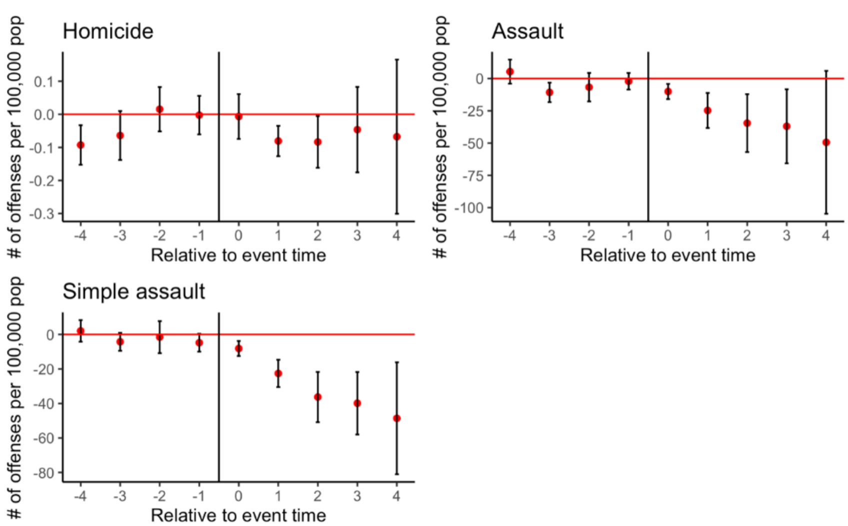

Figure 2.1 Event Study for Intimate Partner Violence from City-level Sample (CS method)

The previous literature (Anderberg et al., 2016) shows that unemployment is a critical factor affecting violent behaviors within households. To capture the pure effect of the drug control policy on domestic violence, ideally, the dynamic unemployment rates among the sampling population should be controlled. However, due to the data limitation, only the state dynamic unemployment rates can be obtained from the U.S. Bureau of Labor Statistics. After controlling for the unemployment rate, the results from the TWFE DID model are shown in Table 2.5. MA PDMPs can decrease the offenses of homicide, assault and simple assault among intimate partners. All coefficient magnitude levels and statistical significant levels are comparable with the baseline results.

Table 2.5 The Effect of MA PDMPs on Intimate Partner Violence (with Unemployment Rate Control)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.09**	-32.69**	0.10	0.19	-30.93***	-1.95	-0.01	0.11
	(0.04)	(15.31)	(0.35)	(4.02)	(10.37)	(5.86)	(0.31)	(0.21)
Mean	0.474	391.9	8.371	46.45	292.1	53.37	6.300	2.071
Percent	-0.182	-0.083	0.012	0.004	-0.106	-0.037	-0.001	0.051
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1) when controlling for state dynamic unemployment rate. Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

In addition, state-level basic demographics, including age, gender, race, ethnicity, and educational achievement[30], are added as the confounding variables in equation (1). Consistent with the baseline results, Table 2.6 shows that MA PDMPs can decrease the intimate partner homicide and assault, while the policy does not have any significant impact on intimate partners' sex offense.

[30] State-level demographics are obtained from the Current Population Survey.

Table 2.6 The Effect of MA PDMPs on Intimate Partner Violence (with Unemployment Rate Control and Other State Dynamic Variables)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.09**	-39.29***	0.00	-1.85	-34.67***	-2.78	-0.01	0.01
	(0.04)	(9.78)	(0.40)	(3.62)	(7.96)	(4.68)	(0.35)	(0.21)
Mean	0.474	391.9	8.371	46.45	292.1	53.37	6.300	2.071
Percent	-0.198	-0.100	0.000	-0.040	-0.119	-0.052	-0.002	0.006
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

The dependent variable measures the total number of offenses for each crime category. As a count variable, the distribution has a long right tail. To limit the influence from extreme outliers, I use the logarithm of total number of offenses plus one as the dependent variable. Showing in Table 2.7, the estimates indicate that the baseline results are robust under the log transformation method.

Table 2.7 The Effect of MA PDMPs on Intimate Partner Violence (Log)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.04**	-0.08**	-0.00	-0.16**	-0.09	-0.03	0.01	0.01
	(0.02)	(0.03)	(0.03)	(0.06)	(0.09)	(0.04)	(0.04)	(0.05)
Mean	0.474	391.9	8.371	46.45	292.1	53.37	6.300	2.071
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Dependent variables are replaced by the logarithm of the number of offenses plus one. Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

As discussed in the Introduction, the impact of MA PDMPs on domestic violence and child maltreatment is most likely to work through the decreased access to opioids. Apart from MA

PDMPs, many other policies can have similar effects on reducing the supply or the usage of opioids, such as OxyContin reformulation, pill mill laws, Naloxone access laws, and Good Samaritan law. OxyContin reformulation is an introduction of an abuse-deterrent formulation of OxyContin. In contrast to the original version of OxyContin which can be easily dissolved and crushed, the reformulated OxyContin, was introduced in 2010, cannot. As a result, it can significantly decrease opioid overdose (Coplan et al., 2016; Sessler et al., 2014). Pill mill laws are statewide regulations on pain management clinics. Through banning these centers from dispensing drugs without medical indicators, the laws can significantly decrease the prescription and usage of opioids (Chang et al., 2018; Lyapustina et al., 2016; Rutkow et al., 2015).

Apart from the regulations that target the supply of opioids directly, some laws were created to decrease preventable overdose deaths. For example, Naloxone can rapidly and effectively reverse an opioid overdose. To increase access and simplify the prescribing procedures of Naloxone, states have gradually adopted the Naloxone access laws. By the end of 2017, all states across the U.S. have implemented the law. However, Naloxone can only reverse the overdose temporarily. The medical intervention is required for follow-up care. In the United States, overdose 911 calls are sent to both emergency medical services and police. Overdose patients and bystanders are less likely to actively seek for help for fear of being arrested for overdose or drug possession. To encourage the use of medical treatment and provide immunity from drug-related arrest or charge, states are passing the Good Samaritan Laws in a staggered manner.

All the laws mentioned above are not controlled in equation (1) since their implementation dates are not perfectly collinear with the date of MA PDMPs. Thus, the estimates from equation (1) should capture the effect of MA PDMPs on domestic violence or child maltreatment. To alleviate the concern that the impact of other opioid-related policies might overlap with MA

PDMPs, additional controls are added[31]. As shown in Table 2.8, the new estimates are consistent with the baseline results in terms of both magnitude and statistical power.

Table 2.8 The Effect of MA PDMPs on Intimate Partner Violence (with Other Policies)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.11**	-34.73**	0.26	-1.64	-31.08**	-2.02	-0.03	0.29
	(0.04)	(14.40)	(0.29)	(3.58)	(10.24)	(5.04)	(0.25)	(0.18)
Mean	0.474	391.9	8.371	46.45	292.1	53.37	6.300	2.071
Percent	-0.233	-0.089	0.031	-0.035	-0.106	-0.038	-0.005	0.142
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

Since the study sample has 2095 cities from 31 states, one may concern that the results are driven by specific states. To alleviate this concern and check the impact from each state, I employ a "leave-one-out" strategy by dropping clustered cities belonging to the same state at each time. Using the intimate partner assault as an example, Table A1 shows that the estimates from the 31 regressions are comparable with the baseline results, indicating that no state has a significantly different impact on intimate partner assault. The results from other dependent variables are the same.

So far, all regressions are analyzed at the city level. Since state-level confounders are controlled in the estimates, the last robustness check was conducted by aggregating the offenses from police agencies to the state level. Not surprisingly, Table 2.9 shows that the state-level estimates are consistent with the city-level baseline results. MA PDMPs can decrease the offenses

[31] The legislation time for pill mill laws follows Mallatt (2021). The implementation dates of Good Samaritan laws and Naloxone access laws are obtained from Prescription Drug Abuse Policy System, pdaps.org.

of assault, especially the simple assault. The effect on homicide is not statistically significant, probably driven by the power issue.

Table 2.9 The Effect of MA PDMPs on Intimate Partner Violence (State-level Sample)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.07	-34.57**	0.09	0.93	-32.71***	-2.78	0.08	0.01
	(0.05)	(15.87)	-0.32	(4.15)	(10.41)	(5.74)	(0.22)	(0.29)
Mean	0.524	421.1	9.677	48.58	324.7	47.77	2.390	7.287
Percent	-0.141	-0.082	0.009	0.019	-0.101	-0.058	0.034	0.001
Observations	310	310	310	310	310	310	310	310

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the state-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

B. Child maltreatment

The second part of the results shows the impact of MA PDMPs on child maltreatment. Following the TWFE DID specification, Table 2.10 indicates that MA PDMPs can significantly decrease the incidence of children suffering from assault by 7.31 offenses per 100,000 population, corresponding to a decline of 10.8 percent. When taking a closer look at the three subcategories of assault, the estimates suggest that the decreased offenses of assault are mainly driven by simple assault and intimidation. Neither homicide nor sex offense is statistically significant in the context of child maltreatment under the traditional DID model.

Table 2.10 The Effect of MA PDMPs on Child Maltreatment (TWEF DID)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.04	-7.31**	-0.55	-0.04	-5.18**	-2.09*	0.18	-0.73
	(0.02)	(3.23)	(0.55)	(0.60)	(2.40)	(1.14)	(0.41)	(0.71)
Mean	0.245	67.37	11.14	10.64	47.89	8.836	9.812	1.330
Percent	-0.146	-0.108	-0.050	-0.004	-0.108	-0.237	0.018	-0.548
p-val	0.113	0.0312	0.326	0.949	0.0387	0.0762	0.671	0.309
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950
R-squared	0.128	0.802	0.533	0.672	0.770	0.766	0.535	0.367

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

As mentioned above, the estimates might be biased when having heterogeneous treatment timing in the TWFE DID model. To alleviate this concern in the context of child maltreatment, the method by Callaway and Sant'Anna (2021) was conducted as a robustness check. As indicated in Table 2.11, a consistent decrease in assault is found. In addition, marginal reductions in homicide and non-forcible sex offense are found.

Table 2.11 The Effect of MA PDMPs on Child Maltreatment (CS Method)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.05*	-6.13***	-0.44	-0.41	-3.74***	-1.99***	0.22	-0.66*
	0.02	1.75	0.27	0.88	1.42	0.52	0.38	0.34
Mean	0.245	67.37	11.14	10.64	47.89	8.836	9.812	1.330
Percent	-0.18	-0.09	-0.04	-0.04	-0.35	-0.04	0.02	-0.07
Observations	20,600	20,600	20,600	20,600	20,600	20,600	20,600	20,600

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

To further examine how MA PDMPs affect child maltreatment dynamically, I plot the event study graphs in Figure 2.2, following the method from Callaway and Sant'Anna (2021). Prior to the adoption of MA PDMPs, little systematic difference is observed in assault, simple assault, intimidation, and non-forcible sex offense, indicating that the parallel trend assumption holds in this context. After MA PDMPs, for assault and simple assault, the number of offenses decreases immediately. Even though little reduction is found at year three after the adoption, the profound reduction goes back four years after the event. For intimidation and non-forcible sex offense, after the implementation of MA PDMPs, the number of offenses keeps decreasing over time.

The series of robustness checks in child maltreatment follow the same format from intimate partner violence. First, I check the validity of baseline results by adding the dynamic state unemployment rate. Showing in Table 2.12, the estimates indicate that MA PDMPs can significantly decrease the occurrence of assault within the domain of child maltreatment. Additionally, the reduction is mainly driven by the decrease in simple assault and intimidation.

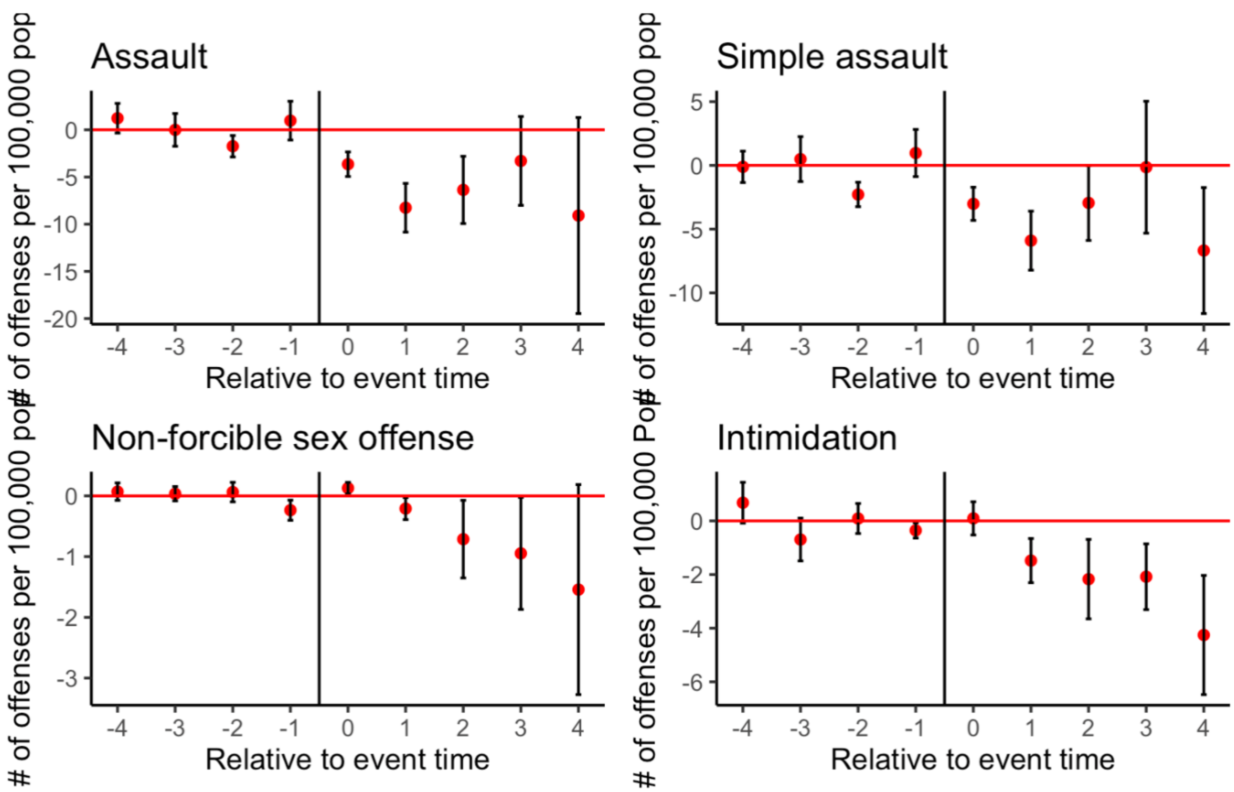

Figure 2.2 Event Study for Child Maltreatment from City-level Sample (CS method)

Apart from the unemployment rate, other state-level demographics are controlled in the regressions, such as age, gender, race, ethnicity, and education. Table 2.13 shows that the new results are comparable with the baseline results both in terms of magnitude level and statistical power.

Table 2.12 The Effect of MA PDMPs on Child Maltreatment (with Unemployment Rate Control)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.03	-7.34**	-0.52	-0.09	-5.14**	-2.12*	0.23	-0.75
	(0.02)	(3.28)	(0.58)	(0.58)	(2.41)	(1.14)	(0.40)	(0.72)
Mean	0.25	67.37	11.14	10.64	47.89	8.84	9.81	1.33
Percent	-0.140	-0.109	-0.047	-0.008	-0.107	-0.240	0.023	-0.562
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1) when controlling for state dynamic unemployment rate. Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

Table 2.13 The effect of MA PDMPs on Child Maltreatment (with Unemployment Rate Control and Other State Dynamic Variables)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.03	-6.47**	-0.48	-0.00	-4.42**	-2.05*	0.26	-0.74
	(0.02)	(2.96)	(0.54)	(0.52)	(1.99)	(1.10)	(0.36)	(0.65)
Mean	0.25	67.37	11.14	10.64	47.89	8.84	9.81	1.33
Percent	-0.111	-0.096	-0.043	-0.000	-0.092	-0.231	0.026	-0.555
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

Second, to address the common concern from the count data, I use the log transformation of the offenses of child maltreatment plus one as the dependent variable. As indicated in Table 2.14, the logarithm regression suggests that the impact of MA PDMPs on assault is robust.

Table 2.14 The effect of MA PDMPs on Child Maltreatment (Log)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.01	-0.08*	-0.04	-0.03	-0.08	-0.08**	0.03	-0.18
	(0.01)	(0.04)	(0.04)	(0.04)	(0.06)	(0.04)	(0.08)	(0.17)
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950
R-squared	0.194	0.734	0.593	0.657	0.714	0.716	0.605	0.447

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

One concern is that the regulation of MA PDMPs on opioids may overlap with other policies, such as pill mill laws, OxyContin regulation, Good Samaritan laws, and Naloxone access laws. To alleviate this concern and capture the impact of MA PDMPs, the opioid-related policies are controlled in the regression. Table 2.15 shows that consistently with the baseline result, MA PDMPs can still decrease the offenses of assault and intimidation.

Table 2.15 The Effect of MA PDMPs on Child Maltreatment (with Other Policies)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.01	-5.07*	-0.24	-0.24	-2.73	-2.10*	0.32	-0.56
	(0.03)	(2.58)	(0.38)	(0.68)	(1.63)	(1.19)	(0.47)	(0.54)
Mean	0.245	67.37	11.14	10.64	47.89	8.836	9.812	1.330
Percent	-0.044	-0.075	-0.022	-0.023	-0.057	-0.238	0.032	-0.417
Observations	20,950	20,950	20,950	20,950	20,950	20,950	20,950	20,950

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

The last robustness check is conducted by aggregating the police agency-level crimes to the state level. The results from Table 2.16 are comparable with the baseline results. Overall, MA

PDMPs have a robust impact on reducing assault and intimidation in the domain of child maltreatment.

Table 2.16 The Effect of MA PDMPs on Child Maltreatment (State-level Sample)

	Crimes			Subcategories of crimes				
Dependent Variable:	Homicide	Assault	Sex offense	Aggravated assault	Simple assault	Intimidation	Forcible sex offense	Non-forcible sex offense
MA_PDMPs	-0.04*	-7.48**	-0.47	-0.07	-5.29**	-2.12*	0.25	-0.73
	(0.02)	(2.96)	(0.49)	(0.59)	(2.19)	(1.11)	(0.47)	(0.71)
Mean	0.283	63.64	12.89	11.45	45.97	6.224	11.68	1.216
Percent	-0.154	-0.118	-0.037	-0.006	-0.115	-0.341	0.022	-0.597
Observations	310	310	310	310	310	310	310	310

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 population at the state-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

7. Conclusion

The consequences of interventions to curb opioid abuse have been well-documented in multiple areas, such as illicit drug use, labor supply, social security programs, as well as child welfare. However, no existing literature examines whether drug control policies can affect domestic violence. I aim to fill the gap in this paper.

Exploiting the exogenous geographical variation in the implementation of MA PDMPs, I employ the TWFE DID method to examine the effect of MA PDMPs on domestic violence and child maltreatment. The estimation results suggest that the drug control policies can significantly decrease 31 offenses of intimate partner assault, which is equivalent to a reduction of 8%. In addition, the programs can decrease seven offenses of assault in the context of child maltreatment, corresponding to a decline of 9%. The results are robust under multiple checks, including the staggered treated DID method by Callaway and Sant'Anna (2021).

This paper has many contributions. First, it contributes to the growing literature by suggesting a new factor that can impact domestic violence. Additionally, this paper extends the literature on investigating the spillover effects of drug control policies. Finally, even though previous research has explored how MA PDMPs impact child welfare, my paper complements the research on child welfare with clearly defined crime types rather than a vague category of child abuse. In addition, the treatment effect on child maltreatment is estimated from a more rural-concentrated sample compared with the previous literature.

More research can be done to generalize the results with a more nationally representative sample. In addition, since this paper only examines the net effect of MA PDMPs on domestic violence, it should be worth disentangling the impacts from positive and negative channels. It is also worth investigating whether the effect continues to be present in the long run as more harmful drugs substitute opioids gradually.

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Appendix A:
The effect of awarding disability benefits on opioid consumption

A1. Literature review

As two of the largest federal aid programs, SSDI and SSI provide financial assistance and health insurance to people who are in need because of a poor health condition. These programs are however also widely claimed to have some unintended effects, especially by disincentivizing labor participation. Previous work (Parsons, 1980; Bound, 1989; Parsons, 1991; Chen and Van der Klauuw, 2008; Maestas et al., 2013) report a statistically and economically significantly deterrent on employment from the receipt of DB. Moreover, Von Wachter et al. (2011) show that SSDI also has a permanently negative effect on employment and earnings among the rejected applicants, which Maestas (2019) claim that might be driven by the erosion of applicants' working capacity due to the considerable long application process with limited earnings. Another potential unintended effect of awarding DB is an increase in opioid use, which we study in this paper.

There is a small, but rapidly growing, empirical literature examining opioid-related questions. One issue that has garnered significant attention in this line of research, and which is particularly relevant for our study, since disability beneficiaries are typically eligible for Medicare or Medicaid, is the relationship between opioid use and health care access or health insurance. Health insurance can increase opioid use by means of the easier access to drugs, but at the same time the improved health from accessible health care can also reduce the need for painkillers. Wettstein (2019) estimates the effect of a health insurance policy change – the gradual introduction of the young adult provision included in the Affordable Care Act (ACA) across states over time –

on opioid-related mortality, and finds that the positive effect of the health insurance expansion dominates. Powell et. al (2020) investigate the impact of the introduction of the Medicare Part D, which provided coverage of prescription drugs, on the opioid related outcomes and find that it increased the state opioid supply, the opioid-related mortality rate, and the opioid abuse treatment admissions. Soni (2018) estimates the price elasticity of the demand for opioids and finds that new customers have a high price elasticity and that OTC painkillers are substitutes for opioids.

In a different vein, other studies examined the relationship between fluctuations of local economic conditions and opioid use. Being unemployed during economic downturns reduces time costs to do exercise or engage in recovery treatment programs while also reducing the need to use opioids to alleviate job induced pain, which can all lead to less opioid use. At the same time, it may also reduce discipline and worsen mental health, which may lead to a higher likelihood of using opioids. Hollingsworth et al. (2017) find that an increasing local unemployment rate leads to a rise in deaths and Emergency Department visits related to opioids and other drugs in state. This is also supported by Zhou et al. (2018) who find that a higher unemployment rate, lower county household income, and lower Gini index typically increase the use of opioid among disabled Medicare beneficiaries.

The public health literature has documented extensively the higher levels of opioid use among disability applicants, as well as beneficiaries. Wu et al. (2019) show that opioids are more commonly used among SSDI applicants than in the general population, particularly among women, individuals aged between 40 and 49, and those with a post-secondary education. Morden et al. (2014) show that the opioid use of under-65-year-old disabled individuals is driven by the illness and injury. Gebauer et al. (2019) use a prospective cohort method to investigate the association between being an SSDI beneficiary and opioid consumption, using a rich set of covariates, but a relatively small sample. The use of opioids is found to be increasing over time, with elevated levels among SSDI beneficiaries, but the difference is not statistically significant, likely due to the small sample. Finally, Ghertner (2020) finds a positive association between changes in county-level disability benefit rates and county-level opioid prescription.

The literature provides thus strong evidence of an association between DB receipt and opioid use, and also suggests several channels through which a causal relationship could be established in either direction. However, to the best of our knowledge, no previous paper investigated this issue employing methods that could plausibly elicit a causal relationship, as we do in our analysis.

There are however several articles that investigated the reverse relationship between opioid use and the likelihood of receiving DB. Park and Powell (2021) found that the recent switch from prescription opioids to illicit opioids led to an increase in the number of DB applications and beneficiaries. Abdulhadi (2019) showed that increased opioid consumption led to an increase both in SSDI applications and awards. Butikofer and Skira (2018) studied the effect of entry and exit of a specific pain medicine on disability pension receipt in Norway, and showed that its introduction decreased the probability of receiving disability pension among women, while the removal increased the probability of receiving disability benefits among both men and women. Franklin et al. (2008) found that more opioid consumption was positively correlated with receiving DB one year later. Savych et al. (2019) found longer period of opioid use resulted in longer duration of receiving temporary DB (workers' compensation) among workers suffering from low back injuries.

A2. Robustness checks and placebo tests

In this section we present a series of robustness checks and placebo tests aimed to verify the validity of both stages of our two-step estimation method.

As discussed in section 3 in the main article, one drawback of the HRS dataset is the unavailability of the precise age when applications were last rejected for individuals who were never awarded the DB. To check whether the age values we imputed are reliable, we perform a robustness test by using slightly different average waiting times to compute the expected date of the last rejection given the data that we have on the time of the start of the corresponding step in the application process. For example, as discussed in section 3, in our main specification model, we assumed an average waiting time for the initial application period of 0.3 years. As robustness checks, instead of adding 0.3 to the start-application age for individuals who were rejected in the initial application period and never appealed or reapplied, we use here 0.4 and 0.5. Similarly, the average waiting time at the appeal hearing step for individuals who were initially rejected and appealed, was assumed to be 1.5 years. We now use 1.6 and 1.7 as alternative values. As shown in tables A8 and A9 from this appendix, the results from the first stage estimation with these alternative values are close in magnitude and statistical power to those from the main specification, showing that the original results are robust to potential small errors in the imputed values. With values imputed further away from those in the main specification, the first stage results gradually become insignificant.

The second type of check we performed is a placebo test for the validation of our age cutoffs for predicting the award rate. As a reminder, DB applicants are sorted into four age groups with three age thresholds 45, 50 and 55, and we find significant discontinuities of the award rate at the age cutoffs 50 and 55. One concern is that the model may be picking up something else that increases the likelihood of a positive award decision around those ages, but not necessarily at the particular age cutoffs that we employed. If such alternative unobserved factors also impacted the opioid consumption directly, we could not interpret our RD results as intended. To alleviate this concern, we run supplementary tests employing several different age cutoffs. The first-stage results are reported in table A10 with age groups created using cutoffs 43, 48 and 53, and age cutoffs 47, 52 and 57, respectively. We generally do not find discontinuities in the award rate using these alternative cutoffs, which confirms that the age cutoffs we employed play an important role in the outcome of the disability benefit application. We also estimated the first stage model by employing age cutoffs closer to the main values: 44, 49 and 54, as well as 46, 51, and 56. We also obtained statistically insignificant coefficients except a significant coefficient at age 54, which is probably due to our imprecision in the imputation of the age at the time of the rejection decision.

The next tests validate the second stage of the two-step procedure, with the first two of these tests verifying the modeling specification. Table A2 allows for a more flexible quadratic function $k(A_i^c)$ in equation (4) describing the second-stage regression model with the opioid consumption as the dependent variable, rather than the linear function that we have employed throughout our prior analysis. The quadratic term is insignificant and the coefficients on $\hat{E}[t_i|A_i]$ are very similar to those from the estimation with a linear specification of $k(A_i^c)$. Table A3 reports the results of a Probit model specification of the second stage, instead of the linear probability model employed in the main specification. The results show again a positive effect of awarding DB on the likelihood of opioid consumption of similar magnitude in terms of marginal effects to those from the main specification. We also tested a Probit specification of the second stage model in regressions with Medicaid coverage, OTC consumption and insurance covering prescription medication as dependent variables and the results are also consistent with those from the model with a linear specification, although we did not report these results in the article.

Tables A11 and A12 report the results of two placebo tests of the second stage model with outcome variables that arguably should not be impacted by the decision to award DB, specifically, whether the individual has any children in school and the father's education. As expected, the results are statistically insignificant.

Our main sample includes the early retired people, the individuals who are receiving at the time of the interview other benefits such as workers' compensation or veterans' benefits, and individuals who received the DB at some time before the date of the interview, but no longer receive it by that date. One possible concern is that these categories may contain individuals whose circumstances, and thus unobservable characteristics, are different than those of the typical DB recipient. We therefore also conduct a robustness check by estimating both stages of our model in a restricted sample where we drop these categories of individuals. The results from the two stages are reported in tables A13 and A14. The coefficients in the first stage are about 10%-50% larger in magnitude, indicating a stronger gap in the likelihood of DB award at the cutoffs, whereas those from the second stage are about 15%-20% smaller. We also estimated the effects on the other outcome variables described in section 6.A. from the main article in this reduced sample and the results are similar although the coefficients are of slightly smaller magnitude. Since in our main analysis we dropped individuals over 65 at interview time, we also ran our model on a sample that also includes these older applicants. The key second stage coefficient in the corresponding unreported results drops in magnitude to about 0.23, but continues to be statistically significant.[32]

[32] Since besides spouses, partners may also have an impact on the outcomes studied in this article, in columns (3) and (6) of table A7 we also report the results of regressions where the control variable Married from the main regressions reported in columns (4) and (8) in table 4 is replaced by control variable Partner, which takes value 1 if the individual

A3. Other effects of awarding disability benefits

Apart from the main objective of measuring the effect of the disability insurance programs on opioid consumption, we also run our estimation model using other outcome variables potentially impacted by these programs. Specifically, we estimate regressions using as outcome variables in the second stage the individuals' Medicaid coverage, the likelihood that insurance paid for prescription medication, and the individuals' use of OTC painkillers. We aim to identify other possible consequences of awarding DB, and by means of this to also potentially better understand the mechanisms through which awarding DB may affect opioid use.

Table A15 reports the estimates of the impact of awarding DB to an applicant on the likelihood that the individual is covered by Medicaid at the time of the interview in 2016 or 2018.[33] As expected, since some disability beneficiaries become eligible for this health insurance program, the results elicit a significant increase in this likelihood by between 39 to 50 percentage points. The insight from the previous table is confirmed by the results presented in table A16 which show the effect of awarding DB on the likelihood that an individual reported that insurance paid for some prescription medications and unveil an effect of comparable magnitude with that on the likelihood that the individual is covered by Medicaid. Consequently, one possible channel through which DB may impact opioid use is through the increased take up of health insurance which facilitates opioid consumption by lowering the cost of the opioids and of the appointments with the doctors that prescribe these medications.

Table A17 further investigates this potential channel and reports the second-stage results with a variable capturing the use of OTC pain killers as outcome. The coefficients on $\hat{E}[t_i|A_i]$ are

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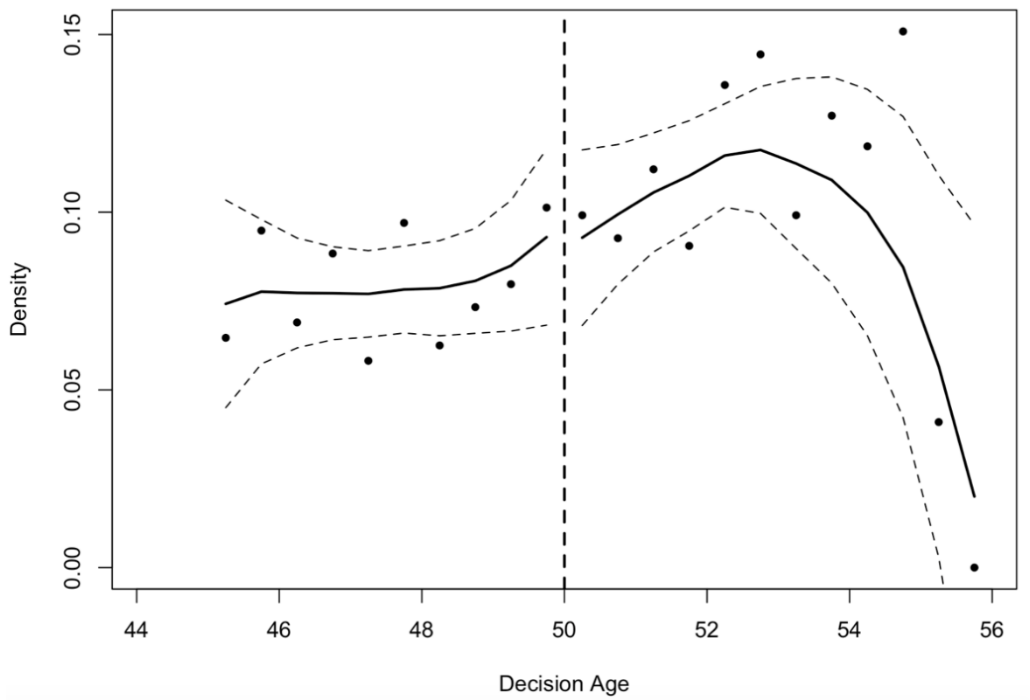
reported that he or she was married or had a partner at the time of the interview, and 0 otherwise. The results are robust to this alternative specification of this control variable.

[33] The sample of observations in this regression is smaller because of missing data on the answer to the Medicaid coverage question.

all negative and statistically significant and, possibly non-coincidentally, of similar magnitude with the corresponding coefficients from table 4 in the main paper. These results suggest that disability beneficiaries may substitute the less effective OTC pain killers with the more potent opioid analgesics because the access to health insurance lowers the cost of the relatively more expensive opioid treatments.



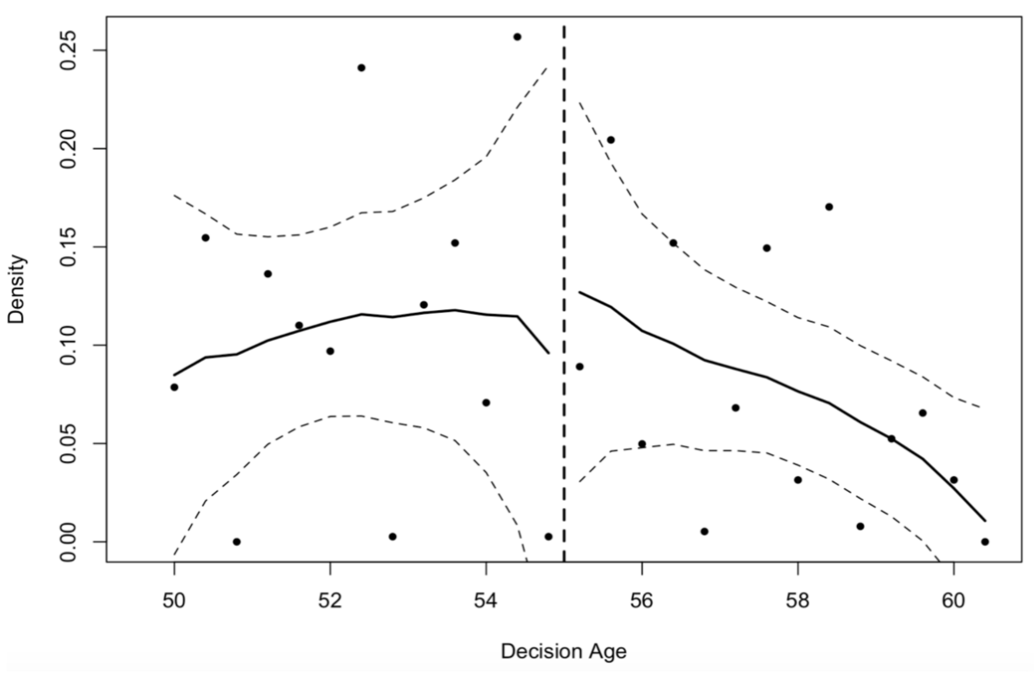


Figure A1: McCrary Density Test in the 2016 Wave

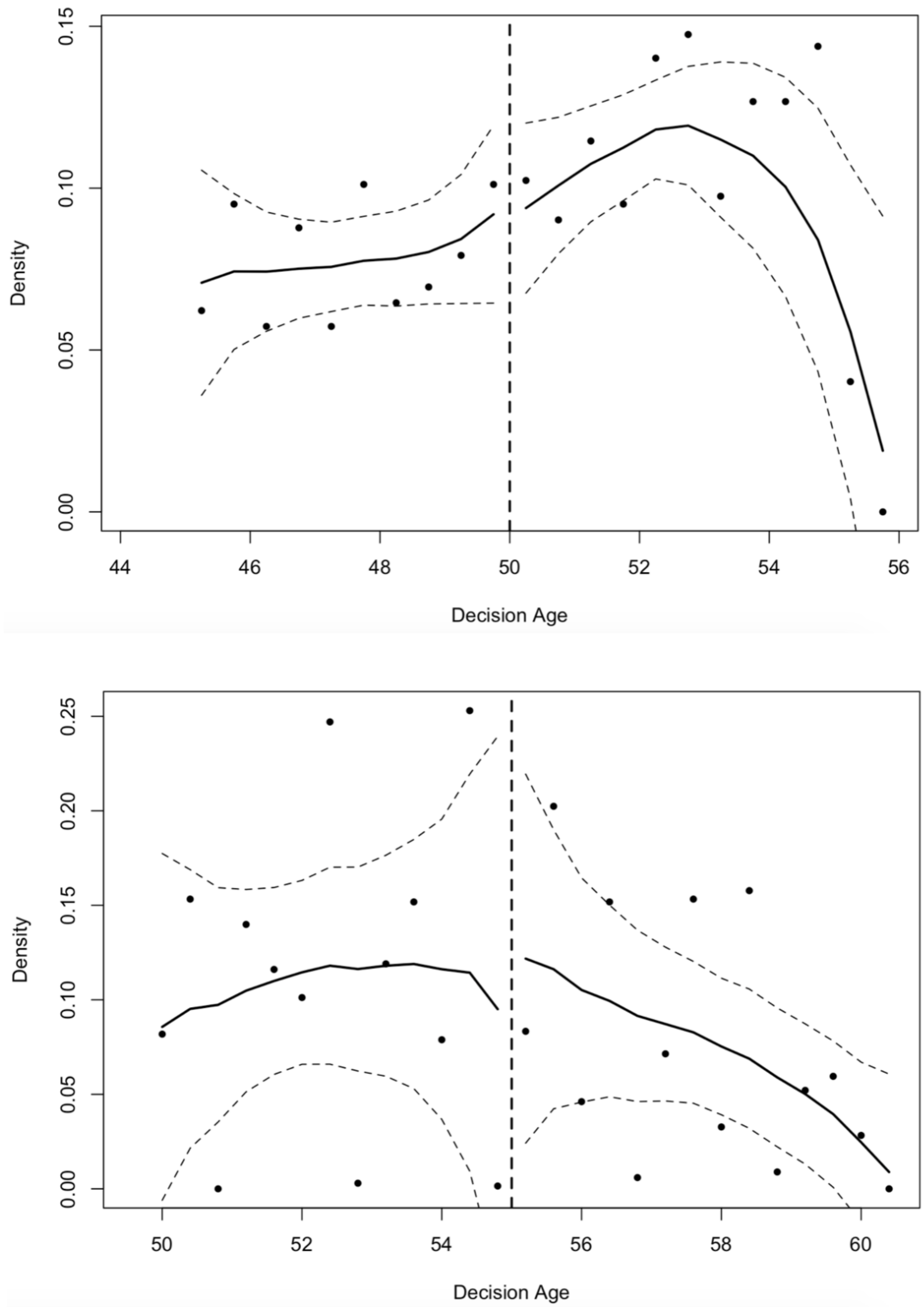


Figure A2: McCrary Density Test in the Combined 2016 & 2018 Waves

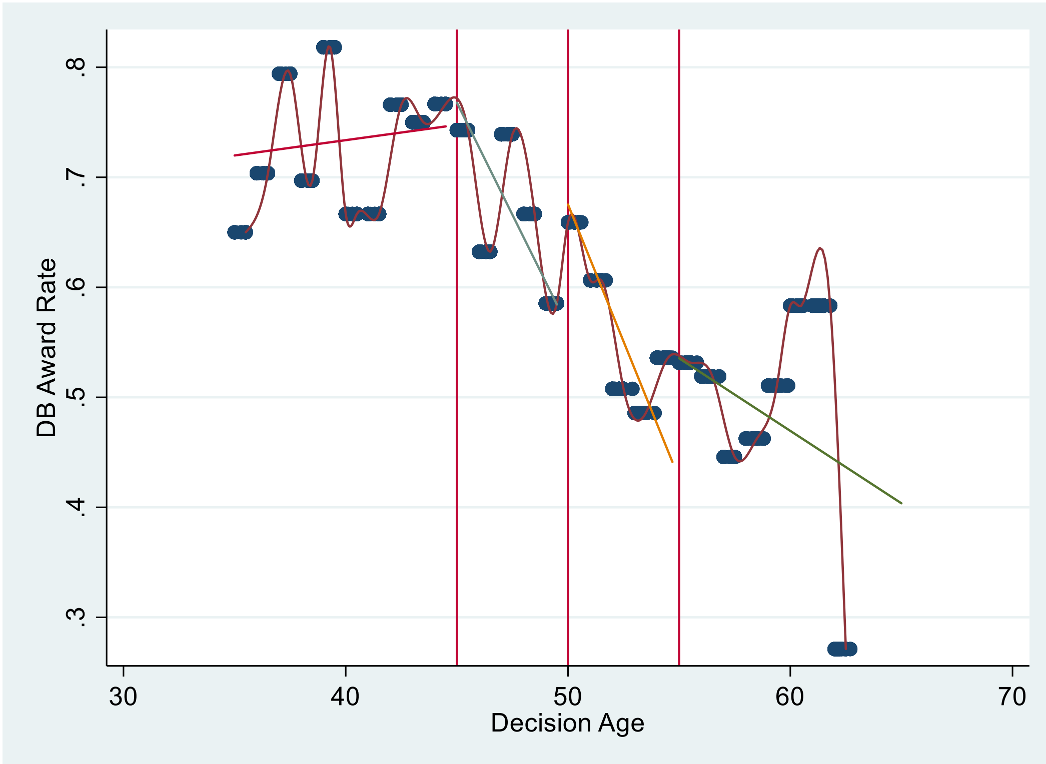


Figure A3: Award Rate in the 2016 Wave

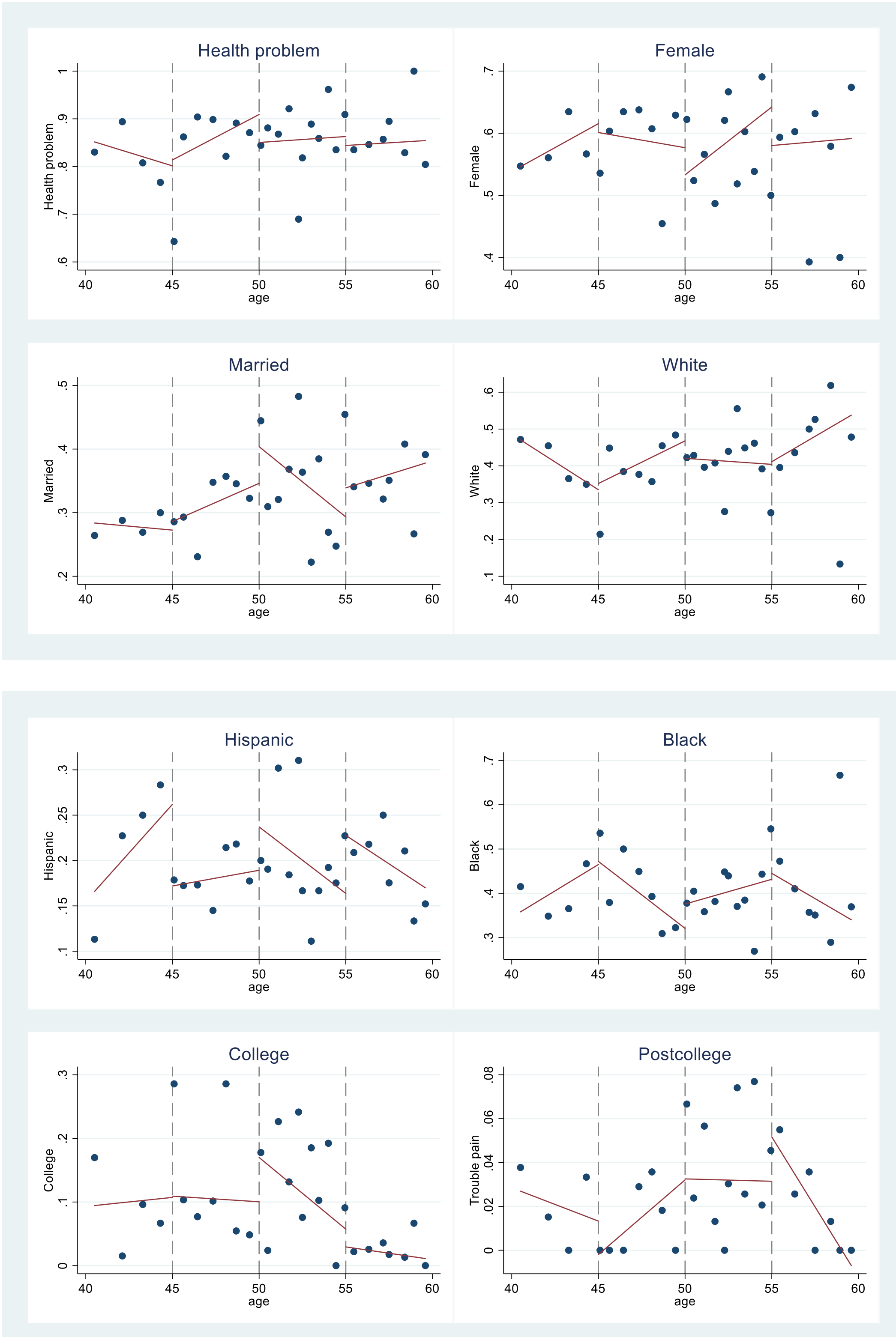


Figure A4: Scatter Plots for Covariates in the 2016 Wave

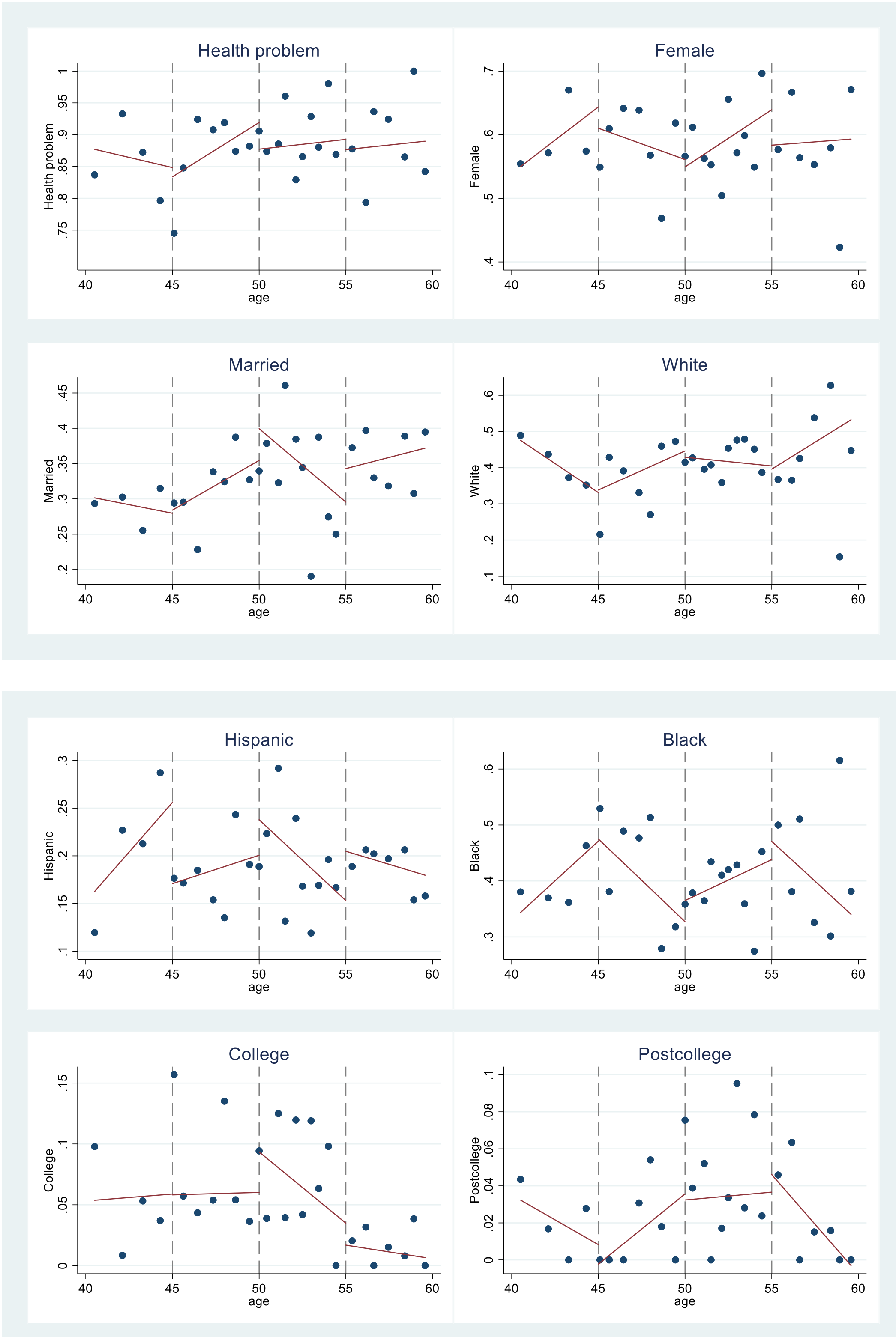


Figure A5: Scatter Plots for Covariates in the Combined 2016 & 2018 Waves

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Table A1: Correlation between Disability Benefits Award and Opioid Use in the General Population

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.000	-0.001***	-0.002***	-0.000**	-0.001***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Receive	0.261***	0.253***	0.154***	0.241***	0.235***	0.145***
	(0.009)	(0.009)	(0.010)	(0.007)	(0.007)	(0.007)
Year2018				-0.019***	-0.031***	-0.040***
				(0.003)	(0.008)	(0.008)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	20,166	20,166	20,166	36,731	36,731	36,731
R-squared	0.041	0.046	0.084	0.039	0.042	0.078

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations includes both DB applicants and non-applicants. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the OLS model in equation (1) from the main article. The dependent variable takes value 1 if the individual took any opioid pain medication during the 3 months prior to the interview date, and 0 otherwise. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A2: Robustness Check for Second Stage Results on Opioid Use by Including Age^2

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	0.002	-0.008	-0.027	0.023	0.015	-0.008
	(0.075)	(0.075)	(0.075)	(0.059)	(0.059)	(0.058)
$\hat{E}[t_i \mid A_i]$	0.272**	0.282**	0.284**	0.276***	0.301***	0.301***
	(0.112)	(0.115)	(0.111)	(0.083)	(0.085)	(0.081)
Current Age^2	-0.000	0.000	0.000	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year2018				-0.058***	-0.061***	-0.066***
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared	0.005	0.015	0.032	0.009	0.019	0.035

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the model in equation (4) from the main article with a quadratic functional form for $k(A_i^c)$. The dependent variable takes value 1 if the individual took any opioid pain medication during the 3 months prior to the interview date, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A3: Probit Specification of the Second Stage on Opioid Use

	(A)2016 wave			(B)2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.002	-0.001	-0.000	-0.002	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$\hat{E}[t_i \mid A_i]$	0.271**	0.283**	0.285***	0.274***	0.300***	0.297***
	(0.111)	(0.113)	(0.109)	(0.082)	(0.084)	(0.080)
Year2018				-0.058***	-0.061***	-0.066***
				(0.017)	(0.017)	(0.016)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The numbers report marginal effects. The variables are defined as presented in section 3. All regressions are estimated using a Probit model. The dependent variable takes value 1 if the individual took any opioid pain medication during the 3 months prior to the interview date, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A4: IV Results on Opioid Use

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.003	-0.002	-0.002	-0.005**	-0.003*	-0.003*
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Receive	0.256***	0.268***	0.271***	0.256***	0.281***	0.284***
	(0.098)	(0.102)	(0.098)	(0.072)	(0.074)	(0.072)
Year2018				-0.055***	-0.058***	-0.064***
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared		0.006	0.010	0.005	0.009	0.012

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the model in equation $y_i = \beta + \alpha_i Receive_i + \theta X + u_i$, where $Receive_i$ is instrumented by three dummy indicators and three interactions of these dummy and corresponding age gaps determined by the age cutoffs 45, 50, and 55, as explained in section A2 of this online appendix. The dependent variable takes value 1 if the individual took any opioid pain medication during the 3 months prior to the interview date, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A5: Balancing Tests in 2016 Wave

VARIABLES	(1) Health problem	(2) Female	(3) Black	(4) Hispanic	(5) White	(6) High school	(7) College	(8) Graduate school	(9) Married
Decision Age	0.000	-0.008	0.007	0.014**	-0.012	-0.008	-0.002	0.002	-0.009
	(0.007)	(0.009)	(0.009)	(0.007)	(0.009)	(0.009)	(0.009)	(0.003)	(0.009)
Age45	-0.010	0.040	0.072	-0.097*	-0.031	0.034	0.009	-0.022	0.018
	(0.050)	(0.069)	(0.068)	(0.055)	(0.069)	(0.070)	(0.068)	(0.022)	(0.066)
Age50	-0.050	-0.033	0.074	0.030	-0.060	-0.103	0.076	0.023	0.049
	(0.050)	(0.069)	(0.068)	(0.055)	(0.069)	(0.069)	(0.068)	(0.022)	(0.066)
Age55	-0.010	-0.063	0.005	0.051	0.015	0.090	-0.018	0.009	0.059
	(0.040)	(0.055)	(0.055)	(0.045)	(0.055)	(0.056)	(0.054)	(0.018)	(0.053)
(Age-45)age45	0.020	-0.000	-0.046**	-0.006	0.040**	0.024	-0.005	0.001	0.024
	(0.015)	(0.020)	(0.020)	(0.016)	(0.020)	(0.020)	(0.020)	(0.006)	(0.019)
(Age-50)age50	-0.022	0.032	0.048**	-0.023	-0.030	-0.026	-0.003	-0.007	-0.039*
	(0.017)	(0.023)	(0.023)	(0.019)	(0.023)	(0.024)	(0.023)	(0.007)	(0.022)
(Age-55)age55	0.002	-0.029*	-0.018	0.016	0.014	0.002	0.015	0.003	0.036**
	(0.012)	(0.017)	(0.017)	(0.014)	(0.017)	(0.017)	(0.016)	(0.005)	(0.016)
Observations	1,867	1,867	1,867	1,867	1,867	1,867	1,867	1,867	1,867
R-squared	0.003	0.003	0.004	0.004	0.007	0.005	0.001	0.004	0.009

Notes: Data is obtained from the 2016 wave of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Age45 takes value 1 if the individual was 45 years or older at the time of the last decision on a disability benefits application, and 0 otherwise. Age 50 and Age 55 are defined similarly. All regressions are estimated using the model in equation (2) from the main article but using as a dependent variable the variable from the corresponding column header and dropping it from the controls. (Age-45)age45 denotes $(A-45)1\{A_i \geq 45\}$; similarly for the other two variables. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A6: Balancing Tests in 2016 & 2018 Waves

VARIABLES	(1) Health problem	(2) Female	(3) Black	(4) Hispanic	(5) White	(6) High school	(7) College	(8) Graduate school	(9) Married
Decision Age	0.003	-0.003	0.007	0.013**	-0.009	-0.010	-0.000	0.001	-0.008
	(0.004)	(0.007)	(0.007)	(0.005)	(0.007)	(0.007)	(0.007)	(0.002)	(0.006)
Age45	-0.032	0.026	0.080	-0.087**	-0.051	0.060	-0.025	-0.020	0.005
	(0.034)	(0.051)	(0.051)	(0.041)	(0.051)	(0.052)	(0.050)	(0.017)	(0.049)
Age50	-0.037	-0.007	0.065	0.023	-0.041	-0.095*	0.066	0.022	0.031
	(0.034)	(0.051)	(0.051)	(0.041)	(0.051)	(0.052)	(0.051)	(0.017)	(0.049)
Age55	-0.007	-0.064	0.025	0.042	-0.002	0.112***	-0.027	0.001	0.053
	(0.028)	(0.042)	(0.042)	(0.034)	(0.042)	(0.042)	(0.041)	(0.014)	(0.040)
(Age-45)age45	0.013	-0.010	-0.045***	-0.004	0.037**	0.023	-0.002	0.002	0.025*
	(0.010)	(0.015)	(0.015)	(0.012)	(0.015)	(0.015)	(0.015)	(0.005)	(0.014)
(Age-50)age50	-0.016	0.035**	0.049***	-0.026*	-0.028	-0.029*	-0.001	-0.006	-0.038**
	(0.012)	(0.017)	(0.017)	(0.014)	(0.017)	(0.018)	(0.017)	(0.006)	(0.017)
(Age-55)age55	-0.001	-0.023*	-0.025*	0.020*	0.016	0.007	0.008	0.004	0.031**
	(0.009)	(0.013)	(0.013)	(0.010)	(0.013)	(0.013)	(0.013)	(0.004)	(0.012)
year2018	0.065***	0.012	0.003	-0.006	-0.011	-0.011	0.016	0.003	0.002
	(0.012)	(0.017)	(0.017)	(0.014)	(0.018)	(0.018)	(0.017)	(0.006)	(0.017)
Observations	3,265	3,265	3,265	3,265	3,265	3,265	3,265	3,265	3,265
R-squared	0.012	0.002	0.005	0.004	0.007	0.007	0.002	0.004	0.006

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Age45 takes value 1 if the individual was 45 years or older at the time of the last decision on a disability benefits application, and 0 otherwise. Age 50 and Age 55 are defined similarly. All regressions are estimated using the model in equation (2) from the main article but using as a dependent variable the variable from the corresponding column header and dropping it from the controls. (Age-45)age45 denotes $(A-45)1\{A_i \geq 45\}$; similarly for the other two variables. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10

Table A7: Additional Second Stage Robustness Checks

	(C) 2016 wave			(D) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.002	-0.001	-0.000	-0.002	-0.005	-0.001
	(0.003)	(0.004)	(0.003)	(0.002)	(0.003)	(0.002)
$\hat{E}[t_i \vert A_i]$	0.248**	0.311**	0.293***	0.262***	0.233**	0.304***
	(0.112)	(0.145)	(0.109)	(0.082)	(0.104)	(0.081)
Own Insurance	0.102***			0.111***		
	(0.035)			(0.027)		
Spouse Insurance		0.035			0.044	
		(0.045)			(0.035)	
Partner			0.032			0.054***
			(0.024)			(0.018)
Individual Characteristics	X	X	X	X	X	X
Health problem	X	X	X	X	X	X
Observations	1864	765	1,867	3,229	1,312	3,265
R-squared	0.036	0.066	0.032	0.039	0.061	0.035

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in Section 3. The variable Receive takes value 1 if the individual has ever been awarded disability benefits, and 0 otherwise. Standard errors are reported in parenthesis. The regressions (1) and (4) are the same as the regressions (3) and (6) from table 4 from the main article with an additional control variable Own Insurance, which takes value 1 if the individual reported that he or she was covered by insurance at the time of the interview, and 0 otherwise. The regressions (2) and (5) are the same as the regressions (3) and (6) from table 4 with an additional control variable Spouse Insurance, which takes value 1 if the individual reported that his or her spouse was covered by insurance at the time of the interview, and 0 otherwise. The sample in these regressions is restricted by the availability of the data on this new variable. The regressions (3) and (6) are the same as the regressions (3) and (6) from table 4 with the control variable Married replaced by control variable Partner, which takes value 1 if the individual reported that he or she was married or had a partner at the time of the interview, and 0 otherwise. *** p<0.01, ** p<0.05, * p<0.10.

Table A8: Robustness Check +0.4 +1.7 of First Stage on the Disability Benefits Award

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Decision Age	0.002	0.001	0.001	0.001	0.000	-0.001
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.006)
Age45	0.036	0.037	0.041	0.035	0.034	0.045
	(0.067)	(0.067)	(0.066)	(0.050)	(0.050)	(0.049)
Age50	0.129**	0.121*	0.132**	0.119**	0.117**	0.126***
	(0.066)	(0.066)	(0.064)	(0.049)	(0.049)	(0.048)
Age55	0.115**	0.123**	0.123**	0.112***	0.116***	0.116***
	(0.053)	(0.053)	(0.051)	(0.040)	(0.040)	(0.039)
Year2018				0.007	-0.007	-0.031
				(0.017)	(0.020)	(0.019)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared	0.050	0.065	0.113	0.051	0.066	0.108

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Age45 takes value 1 if the individual was 45 years or older at the time of the last decision on a disability benefits application, and 0 otherwise. Age 50 and Age 55 are defined similarly. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in Section 3, except for the Age variable. For the Age variable, the delay in the decision on the initial application is imputed to be 0.4 years instead of 0.3 years, while the delay on the decision on the appeal hearing is imputed to be 1.7 years, instead of 1.5 years. All regressions are estimated using the model in equation (2) from the main article. The dependent variable takes value 1 if the individual had ever had a disability benefits application accepted, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A9: Robustness Check +0.5 +1.6 of First Stage on the Disability Benefits Award

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Decision Age	0.001	0.000	-0.000	-0.000	-0.001	-0.002
	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.006)
Age45	0.047	0.049	0.053	0.045	0.044	0.054
	(0.067)	(0.067)	(0.065)	(0.050)	(0.050)	(0.048)
Age50	0.152**	0.154**	0.163**	0.141***	0.143***	0.150***
	(0.066)	(0.066)	(0.064)	(0.049)	(0.049)	(0.048)
Age55	0.132**	0.138***	0.136***	0.128***	0.133***	0.130***
	(0.053)	(0.053)	(0.052)	(0.040)	(0.040)	(0.039)
Year2018				0.007	0.007	-0.013
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared	0.053	0.066	0.113	0.053	0.068	0.109

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Age45 takes value 1 if the individual was 45 years or older at the time of the last decision on a disability benefits application, and 0 otherwise. Age 50 and Age 55 are defined similarly. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in Section 3, except for the Age variable. For the Age variable, the delay in the decision on the initial application is imputed to be 0.5 years instead of 0.3 years, while the delay on the decision on the appeal hearing is imputed to be 1.6 years, instead of 1.5 years. All regressions are estimated using the model in equation (2) from the main article. The dependent variable takes value 1 if the individual had ever had a disability benefits application accepted, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A10: Placebo Test of First Stage on the Disability Benefit Award Employing Different Age Cutoffs

	(A) 2016 wave		(B) 2016 & 2018 waves	
VARIABLES	age cutoffs 43, 48, 53 (1)	age cutoffs 47, 52, 57 (2)	age cutoffs 43, 48, 53 (3)	age cutoffs 47, 52, 57 (4)
Decision Age	-0.004	-0.004	-0.004	-0.006
	(0.013)	(0.006)	(0.010)	(0.005)
Age 45 ± 2	0.073	0.056	0.079	0.059
	(0.078)	(0.062)	(0.058)	(0.046)
Age 50 ± 2	0.068	-0.010	0.093*	0.012
	(0.073)	(0.062)	(0.054)	(0.046)
Age 55 ± 2	0.055	0.004	0.066*	0.001
	(0.052)	(0.062)	(0.039)	(0.047)
Year2018			0.008	0.008
			(0.017)	(0.017)
Individual Characteristics	X	X	X	X
Observations	1,867	1,867	3,265	3,265
R-squared	0.061	0.061	0.063	0.062

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the model in equation (2) from the main article, except that the age cutoffs 45, 50, and 55 are replaced with the age cutoffs specified in the corresponding column headers. The coefficient on variable Age 45± 2 is the coefficient on variable Age 45-2 in columns (1) and (3), and on variable Age 45+2 in columns (2) and (4). Similarly for Age 50± 2 and Age 55± 2. The dependent variable takes value 1 if the individual had ever had a disability benefits application accepted, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A11: Placebo Test of the Second Stage Using the Father's Education as Dependent Variable

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	0.001	-0.021	-0.021	0.005	-0.006	-0.007
	(0.082)	(0.076)	(0.075)	(0.062)	(0.058)	(0.057)
$\hat{E}[t_i\|A_i]$	0.378	-0.484	-0.497	0.172	-0.501	-0.564
	(2.069)	(1.906)	(1.824)	(1.516)	(1.390)	(1.328)
Year2018				0.048	0.009	0.030
				(0.291)	(0.264)	(0.261)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	541	541	541	997	997	997
R-squared	0.000	0.192	0.194	0.000	0.193	0.195

Notes: The estimation details are as in table 4 from the main article. The dependent variable takes value 1 if the individual had any children in school at the time of the interview, and 0 otherwise.

Table A12: Placebo Test of the Second Stage using as Dependent Variable if Individual Had Any Children in School

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.016***	-0.015***	-0.015***	-0.015***	-0.015***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$\hat{E}[t_i \mid A_i]$	-0.014	0.034	0.041	-0.058	-0.019	-0.016
	(0.108)	(0.110)	(0.107)	(0.075)	(0.076)	(0.074)
Year2018				-0.088***	-0.091***	-0.091***
				(0.015)	(0.015)	(0.015)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,566	1,566	1,566	2,769	2,769	2,769
R-squared	0.025	0.047	0.047	0.040	0.061	0.061

Notes: The estimation details are as in table 4 from the main article. The dependent variable takes value 1 if the individual had any children in school at the time of the interview, and 0 otherwise.

Table A13: First Stage Results for Restricted Sample on the Disability Benefits Application Outcome

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Decision Age	0.006	0.004	0.003	0.002	0.001	-0.002
	(0.010)	(0.010)	(0.009)	(0.007)	(0.007)	(0.007)
Age45	0.034	0.037	0.038	0.038	0.036	0.042
	(0.072)	(0.072)	(0.069)	(0.054)	(0.053)	(0.051)
Age50	0.161**	0.160**	0.169**	0.137**	0.139***	0.149***
	(0.073)	(0.073)	(0.070)	(0.054)	(0.054)	(0.052)
Age55	0.113*	0.120**	0.115**	0.117***	0.126***	0.120***
	(0.060)	(0.060)	(0.058)	(0.045)	(0.045)	(0.043)
Year2018				0.013	0.013	-0.006
				(0.019)	(0.019)	(0.018)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,468	1,468	1,468	2,594	2,594	2,594
R-squared	0.047	0.065	0.139	0.049	0.069	0.134

Notes: The estimation details are as in table 3 from the main article, only that the sample is restricted as explained in section A2 of this online appendix to exclude the early retired people, the individuals who are receiving at the time of the interview other benefits such as workers' compensation or veterans' benefits, and individuals who received the DB at some time before the date of the interview, but no longer receive it by that date.

Table A14: Second Stage Results on the Opioid Use with a Restricted Sample

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	0.001	0.002	0.003	0.001	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$\hat{E}[t_i \vert A_i]$	0.215*	0.223*	0.224*	0.239***	0.260***	0.260***
	(0.124)	(0.128)	(0.123)	(0.092)	(0.094)	(0.090)
Year2018				-0.059***	-0.062***	-0.067***
				(0.019)	(0.019)	(0.019)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,468	1,468	1,468	2,594	2,594	2,594
R-squared	0.002	0.018	0.037	0.006	0.017	0.035

Notes: The estimation details are as in table 4 from the main article, only that the sample is restricted as explained in section A3 of this online appendix. To exclude the early retired people, the individuals who are receiving at the time of the interview other benefits such as workers' compensation or veterans' benefits, and individuals who received the DB at some time before the date of the interview, but no longer receive it by that date.

Table A15: Second Stage Results on Medicaid Participation

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	0.003	0.002	0.002	0.005**	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$\hat{E}[t_i\|A_i]$	0.414***	0.404***	0.389***	0.535***	0.521***	0.497***
	(0.106)	(0.109)	(0.105)	(0.085)	(0.087)	(0.084)
Year2018				-0.093***	-0.090***	-0.084***
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	896	896	896	1,733	1,733	1,733
R-squared	0.017	0.024	0.027	0.035	0.044	0.047

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the model in equation (4) from the main article. The dependent variable takes value 1 if the individual is covered by Medicaid at the time of the interview, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A16: Second Stage Results on Insurance Pay for Prescription Medications

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	0.006***	0.006***	0.006***	0.006***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$\hat{E}[t_i \vert A_i]$	0.410***	0.417***	0.410***	0.443***	0.447***	0.437***
	(0.079)	(0.080)	(0.078)	(0.062)	(0.063)	(0.061)
Year2018				-0.052***	-0.053***	-0.048***
				(0.013)	(0.013)	(0.013)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,677	1,677	1,677	2,988	2,988	2,988
R-squared	0.017	0.025	0.029	0.020	0.031	0.036

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the model in equation (4) from the main article. The dependent variable takes value 1 if the individual reported that insurance paid for any prescription medication, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table A17: Second Stage Results on the Use of OTC Pain Killers

	(A) 2016 wave			(B) 2016 & 2018 waves		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Current Age	-0.006**	-0.005*	-0.005*	-0.005**	-0.004*	-0.004*
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$\hat{E}[t_i \mid A_i]$	-0.191*	-0.180	-0.171	-0.228***	-0.211**	-0.202**
	(0.112)	(0.115)	(0.111)	(0.085)	(0.086)	(0.084)
Year2018				-0.001	-0.004	-0.008
				(0.017)	(0.017)	(0.017)
Individual Characteristics		X	X		X	X
Health problem			X			X
Observations	1,867	1,867	1,867	3,265	3,265	3,265
R-squared	0.003	0.015	0.021	0.003	0.021	0.021

Notes: Data is obtained from the 2016 and 2018 waves of the Health and Retirement Study dataset. Sample of observations restricted as described in section 3. Panel (A) reports results from the data in 2016 wave. Panel (B) reports results from the data in the combined 2016 and 2018 waves. The variables are defined as presented in section 3. All regressions are estimated using the model in equation (4) from the main article. The dependent variable takes value 1 if the individual took any over-the-counter pain medication during the 3 months prior to the interview date, and 0 otherwise. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Appendix B:
The effect of the drug abuse prevention programs on intimate partner violence and child maltreatment

Table B1 The Effect of MA PDMPs on Intimate Partner Assault from Leave-one-out Strategy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Excluded State FIPS Code	1	4	5	8	9	10	16	17	19	20
PDMP	-33.41**	-33.32**	-33.09**	-28.00*	-34.07**	-27.27*	-33.22**	-33.30**	-32.83*	-36.41**
	(15.72)	(15.72)	(16.03)	(15.03)	(16.18)	(14.71)	(15.93)	(15.74)	(16.08)	(15.71)
Mean	391.9	392	401.2	389.3	396.5	387.3	393.1	391.5	399.1	385.9
Percent	-0.085	-0.085	-0.083	-0.072	-0.086	-0.070	-0.085	-0.085	-0.082	-0.094
Observations	20,940	20,930	19,800	20,320	20,330	20,670	20,390	20,940	19,850	19,660

Note: Data is obtained from 2007—2016 NIBRS. The variables as well as sample selection are described in Section 3. Crimes are measured using the number of offenses per 100,000 covered population at the city-year level. Regressions are estimated using the DID model in Equation (1). Robust clustered standard errors are reported in parenthesis.
***p < 0.01, **p < 0.05, *p < 0.10.

Table B1 The effect of MA PDMPs on intimate partner assault from leave-one-out strategy (continued)

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Excluded State FIPS Code	21	22	23	25	26	30	32	33	38	39
PDMP	-33.34**	-34.58**	-33.67**	-37.75**	-30.06*	-32.41**	-33.90**	-33.44**	-32.54**	-41.23**
	(15.77)	(15.92)	(15.74)	(17.16)	(16.76)	(15.78)	(15.96)	(15.84)	(15.75)	(16.94)
Mean	392	391.6	392	406.4	404.1	392.3	394.2	395.5	392.7	395.4
Percent	-0.085	-0.088	-0.086	-0.093	-0.074	-0.083	-0.086	-0.085	-0.083	-0.104
Observations	20,900	20,860	20,860	19,130	18,280	20,480	20,600	19,980	20,730	20,900

Table B1 The effect of MA PDMPs on intimate partner assault from leave-one-out strategy (continued)

	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)
Excluded State FIPS Code	41	44	45	46	47	48	49	51	53	54	55
PDMP	-33.32**	-33.09**	-28.00*	-34.07**	-27.27*	-33.22**	-33.30**	-32.83*	-36.41**	-33.34**	-34.58**
	(15.72)	(16.03)	(15.03)	(16.18)	(14.71)	(15.93)	(15.74)	(16.08)	(15.71)	(15.77)	(15.92)
Mean	392	401.2	389.3	396.5	387.3	393.1	391.5	399.1	385.9	392	391.6
Percent	-0.085	-0.083	-0.072	-0.086	-0.070	-0.085	-0.085	-0.082	-0.094	-0.085	-0.088
Observations	20,930	19,800	20,320	20,330	20,670	20,390	20,940	19,850	19,980	20,900	20,860