Is the Rise in Illicit Opioids Affecting Labor Supply and Disability Claiming Rates?[*]

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Abstract: This paper studies how the transition of the opioid crisis from misuse of prescription opioids to misuse of illicit opioids, such as heroin and fentanyl, is altering labor supply and disability claiming rates. There is widespread interest in understanding the broader effects of the opioid crisis beyond overdoses, especially the effects on labor supply, social insurance programs, and economic conditions. We study the effects of the reformulation of OxyContin, a substantial reduction in the supply of abusable opioids, as a primary driver of the transition from prescription opioids to illicit drug markets. This shock differentially affected areas with high rates of non-medical OxyContin misuse, and we study the relationship between pre-reformulation OxyContin misuse with labor and disability outcomes over time. We find that the transition to heroin and fentanyl had little effect on traditional labor supply measures such as labor force participation rates, but that these aggregate metrics mask important behavioral changes for those on the margins of applying for disability benefits. We estimate statistically significant and meaningful increases in disability applications based on prior OxyContin misuse rates. This increase leads to a higher rate of favorable determinations in the population, suggesting downstream effects on the disability insurance system. We do not observe similar effects associated with non-medical pain reliever misuse more generally, implying that the change in disability rates is uniquely associated with OxyContin misuse at the time of reformulation.

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1. Introduction

The opioid crisis in the United States is the worst drug overdose epidemic in the country's history (Kolodny et al., 2015) and a national emergency. In 2017 alone, there were over 70,000 drug overdoses; almost 70% of these involved opioids (Scholl et al., 2019). While we often focus on the rate of fatal overdoses when discussing the opioid crisis, there is widespread interest in understanding its broader effects and there is evidence that it is permeating countless facets of individual, household, and national well-being. Policymakers and researchers have expressed considerable interest in evaluating the economic and labor supply consequences of the opioid crisis.[1] This relationship may be quite strong. The majority of employers report incidents related to prescription drug misuse[2] and, anecdotally, many have stated that it is difficult to find job applicants given the current levels of abuse.[3]

Furthering difficulties in quantifying the labor supply effects, the opioid crisis has evolved over time. Before 2010, the "first wave" was driven primarily by misuse of prescription opioids, such as OxyContin. In 2010, a pivotal transformation produced a heroin epidemic -- the second wave -- which transitioned into the third wave around 2013, an illicit fentanyl crisis. Recent research has suggested that this transformation from prescription to illicitly-produced opioids was driven by the reformulation of OxyContin. In 2010, Purdue Pharma introduced an abuse-deterrent version of OxyContin, replacing the original formulation. This replacement represented a substantial shock to the availability of abusable opioids as OxyContin was often the "drug of choice" for prescribers and for non-medical users (Cicero et al., 2005). The implications of this shock were substantial. Prior research has shown that states with higher rates of non-medical use of OxyContin experienced disproportionate growth in heroin overdose rates (Alpert et al., 2018) and hepatitis C rates (Powell et al., 2019).

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[1] There have been Congressional hearings on the economic effects specifically: see https://www.govinfo.gov/content/pkg/CHRG-115shrg26119/html/CHRG-115shrg26119.htm, last accessed July 8, 2019. There have also been broader policy discussions: see https://thehill.com/opinion/finance/392294-the-severe-economic-costs-of-the-us-opioid-crisis, last accessed July 8, 2019

[2] See https://www.nsc.org/Portals/0/Documents/NewsDocuments/2017/Media-Briefing-National-Employer-Drug-Survey-Results.pdf, last accessed July 5, 2019

[3] See https://khn.org/news/workers-overdose-on-the-job-and-employers-struggle-to-respond/, https://www.cnbc.com/2017/09/01/struggling-to-find-workers-employers-offer-opioid-addicts-a-chance.html, and https://www.bloomberg.com/news/articles/2017-09-20/overdosing-on-the-job-opioid-crisis-spills-into-the-workplace

The wide-ranging effects of the opioid crisis, such as the implications on the labor market and social insurance programs, are only beginning to be understood. An emerging literature analyzes the effects of opioid availability and overprescribing on the labor market. Krueger (2017) suggests that the rise in opioid prescribing since 2000 can explain a large share of the decline in labor force participation among men over that time period. Aliprantis et al. (2019) adopt a similar empirical approach and conclude that 44% of the drop in male labor force participation between 2001 and 2015 can be attributed to the rise in opioid prescribing.

Implementing a different strategy, Currie et al. (2019) instrument working age opioid prescribing rates with those for older ages in the same county, assuming that opioid prescribing rates for the elderly are unrelated to labor market conditions. They find that higher rates of opioid prescribing improve labor market outcomes for women, suggesting a positive effect due to the pain management benefits of opioids. Focusing more on the overprescribing dimension, Harris et al. (2019) find that areas with more high-volume prescribers have lower labor force participation rates.

This developing literature measures opioid exposure in terms of geographic prescribing rates. In contrast, this paper studies the labor supply effects of the transition of the opioid crisis from prescription opioids to illicit opioids – heroin and fentanyl – which are not measured in prescriptions. This emphasis is likely more relevant to the current state of the epidemic and informs our knowledge about how we should expect these outcomes to evolve as heroin and illicit fentanyl continue to flood many local drug markets. More broadly, there is significant interest in understanding the labor consequences of illicit drug use and expansion of illicit drug markets, a difficult empirical topic given that drug markets are often potentially tied to local economic conditions.

In this paper, we study the ramifications of the reformulation of OxyContin on labor market outcomes and applications for disability benefits. Given the dramatic consequences of the transformation represented by reformulation, it is possible that the subsequent market-wide substitution to heroin and fentanyl altered labor force participation rates and, importantly, disability application rates. Alpert et al. (2018) attribute the vast majority of the rise in heroin overdoses since 2010 to reformulation, showing that states with higher rates of OxyContin misuse prior to reformulation experienced disproportionately sharper growth in heroin overdoses beginning immediately after reformulation. This work also finds related evidence that the subsequent rise in synthetic opioid deaths, such as those involving fentanyl, can also be attributed to the reformulation

of OxyContin. As the supply of abusable prescriptions opioids decreased, people substituted to illicit markets and these markets grew disproportionately in areas where OxyContin misuse was more prevalent.

Given the existing evidence that OxyContin reformulated induced a significant transformation in the opioid crisis, it is important to understand the labor market consequences of this transition. More generally, the labor productivity costs of illicit drug use have been estimated to be large. Jiang et al. (2017) estimate that heroin use disorder alone in the United States results in over $5 billion of productivity losses annually. This paper explores the direction and magnitude of effects resulting from a broad market-wide substitution from prescription opioids to illicit opioids resulting from a plausibly exogenous shock to the legal availability of abusable prescription opioids.

We use the Current Population Study (CPS) to construct measures of labor force participation and employment. We also rely on data from the Social Security Administration (SSA) concerning applications and determinations for disability benefits through Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). We study these disability-related measures extensively for a few reasons. First, disability applications represent an informative dimension of labor supply that is potentially not captured by employment rates, labor force participation rates, and other traditional labor market measures. Changes in SSDI application rates potentially reflect an important indicator of the labor market for the population most affected by reformulation that would be masked by relying on more traditional labor supply metrics. Misuse of medical and illicit opioids possibly puts people at risk of permanently dropping out of the labor force, making SSDI applications an especially useful proxy for labor force attachment in this context.

Second, there is significant policy interest in understanding the drivers behind increases in SSDI applications and enrollment, especially when these mechanisms are due to unforeseen changes in labor force attachment among the population and not predictable changes due to demographic shifts and economic conditions. SSDI represents 4% of the federal budget at $143 billion.[4] Broad shifts in labor market attachment can have meaningful effects on federal government expenditures. This analysis relates to the literature on explaining drivers in changes in disability beneficiary rates.

[4] https://www.cbpp.org/research/retirement-security/policy-basics-social-security-disability-insurance, last accessed July 6, 2019

Several papers have found that worsening local economic conditions predict increases in disability payments (Autor and Duggan, 2003; Black et al. 2002; Autor et al., 2013; Charles et al., 2018). However, there is little work exploring how the growth of illicit drug markets affect enrollment in disability insurance programs.

At an individual-level, substitution from OxyContin to heroin could be detrimental to an individual's ability to operate in the labor force given heroin's additional potency and its associated health risks (e.g., due to injection use). In addition, acquiring heroin may introduce individuals to illicit markets, which could have independent harmful consequences such as exposing them to criminal behavior or increasing their odds of victimization. On the other hand, prior work found that heroin overdoses increased due to reformulation with some (but limited) evidence of reductions in (non-heroin) opioid-related overdoses. Overdoses, however, do not perfectly reflect changes in misuse given the additional potency of heroin. If reformulation induced some to switch to heroin (and they became more likely to overdose) and others to stop misusing opioids entirely, then we could observe beneficial outcomes on non-mortality dimensions.

At a market level, a geographic-wide transition from prescriptions opioids to heroin may cause illicit drug markets to expand which could potentially have broader economic consequences. This expansion could alter labor markets, induce crime, and systematically change population health. Our analysis should pick up the net effects of these individual- and market-level responses.

We study changes in labor supply and disability outcomes in states with higher rates of OxyContin misuse prior to OxyContin reformulation relative to those with lower rates, leveraging the findings of prior work that this variable strongly predicts transitions to illicit opioids. Our primary empirical strategy traces out the relationship between OxyContin misuse and these outcomes in each year, conditioning on state and time fixed effects, while also accounting for the independent effects of pre-reformulation pain reliever misuse rates more generally. Over this time period, many federal, state, and local policies targeting opioid use and misuse were implemented, and it is important to account for the independent and potentially non-random effects of these policies. We exploit the fact that OxyContin reformulation targeted OxyContin specifically, while other policies generally affect opioid misuse more broadly, by separately accounting for non-medical pain reliever use.

We observe little effect of reformulation on employment-to-population ratios or labor force participation. However, our estimates suggest meaningful effects for disability applications and favorable determinations, implying that reformulation induced more people to apply for disability benefits and many of these people were found to meet the SSA's disability criteria. This relationship is unique to OxyContin misuse as we do not observe similar increases associated with pain reliever misuse more generally. The differential rise in disability applications begins immediately after reformulation and there is little evidence of any systematic pre-existing trends. We also find that our results are robust to the years included in the analysis, selecting only on states in the West to avoid possible confounding effects with the Florida pill mills, conditioning on other policies targeting opioid use and misuse, and functional form assumptions. We further find that the results are not driven by the Great Recession, implying that our time fixed effects are adequately accounting for its effects or, more appropriately, that pre-reformulation non-medical use does not predict the areas which were disproportionately harmed by the Great Recession.

Our results imply that a state with a one standard deviation higher rate of non-medical OxyContin use prior to reformulation experienced a 5% increase in disability applications after 2010. The rise in applications did not lead to a drop in the rate of favorable determinations for those applying. In fact, we find that the favorable determination rate – both as a function of the eligible adult population and as a function of the number of applicants – disproportionately rose post-reformulation in the most affected states. While substance use disorders alone cannot be used as qualifying conditions for disability benefits, these results suggest that growth in illicit drug markets alter the labor market capabilities of those with other disabling conditions or worsen economic conditions for this population. While the literature has provided evidence that economic conditions can drive applications and successful determinations for disability benefits, this paper demonstrates that general equilibrium shocks to illicit drug markets can also affect demand for disability insurance.

We discuss these interactions further in the next section while also providing additional background on the reformulation of OxyContin. We introduce the Data in Section 3 followed by the empirical strategy in Section 4. Results are presented in Section 5, and we conclude in Section 6.

2. Background

2.1 Disability Insurance in the United States

The Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) programs are the largest of several federal programs that provide monetary and non-monetary -- such as health insurance -- support to people with disabilities in the United States. In December 2017, 12.7 million people ages 18-64 received disability benefits – 62% received SSDI benefits only, 28% received SSI benefits only, and 10% received benefits from both SSDI and SSI.[5] SSDI is generally available to individuals with a sufficient work history while SSI is primarily targeted to individuals with little work experience and is subject to asset thresholds. Children are also eligible for SSI and evaluated based on medically determinable physical and mental impairments.

When it started in 1956, SSDI provided benefits to people with disabling conditions including those unable to work because of substance use disorders. SSI, introduced in the Social Security Amendments of 1972, defined disabling conditions using similar guidelines. However, in 1997, with the introduction of public law 104-121, substance use disorder beneficiaries became disqualified unless they also qualified under different disability requirements (Gresenz et al. 1998, Noblitt and Noblitt 2012). Since 1997, substance use disorder has not been a qualifying condition for disability benefits.[6] However, substance use also does not *disqualify* individuals from receiving benefits and potentially exacerbates other disabilities or reduces labor market opportunities, inducing individuals to apply and qualify for benefits. Consequently, it is plausible that shocks to substance abuse prevalence could alter disability claiming rates.

To apply for SSDI, an individual initially files an application at a Social Security office. The office first determines if the applicant meets basic requirements such as age and work credits for Social Security disability benefits. If these conditions are met, then the application is reviewed by the state Disability Determination Services to determine whether the individual is disabled under Social Security guidelines. Denied individuals can appeal these decisions. Individuals receiving favorable determinations will receive cash benefits, which are a function of prior earnings, and access to health insurance through Medicare. The process for SSI is similar, though the eligibility requirements regarding work history and asset requirements are different. Monthly SSI payments are based on a federal benefit rate and medical coverage is typically provided through State Medicaid programs.

[5] See Chart 12: https://www.ssa.gov/policy/docs/statcomps/di_asr/2017/di_asr17.pdf, last accessed June 2, 2019
[6] Moore (2015) studied the long-term effects of disqualification for those affected by this reform.

Thus, SSDI and SSI applications for the 18-64 age group potentially reflect a combination of willingness and ability to work among the working age population. Given the shock studied in this paper, we also view changes in the rate of favorable determinations as possibly signaling changes in population health (though other interpretations are possible and discussed below). For reasons discussed in the introduction above, it is not clear in principle which direction reformulation should affect disability claiming rates. Empirical evidence is needed.

2.2 OxyContin Reformulation

OxyContin was introduced in 1996 by Purdue Pharma. It is a brand-name drug for the extended-release formulation of oxycodone, a semi-synthetic opioid, similar to morphine, used for the management of acute and chronic pain. The key innovation of OxyContin was its long-acting formula which provided 12 hours of continuous pain relief, significantly improving the quality and ease of pain management over previous drugs. However, crushing or dissolving the pill causes the high dose of oxycodone, which is intended to be released slowly over 12 hours, to be delivered all at once. This property made OxyContin especially easy to abuse. Individuals who intended to abuse OxyContin could chew, snort or inject the crushed pill for maximum euphoric effects. This method of abuse is arguably the most dangerous, as this high level of potency comes with a heightened risk for addiction and overdose death.[7]

OxyContin was one of the highest selling prescription drugs in the U.S. with more than $3 billion in annual sales in 2010 (Bartholow, 2011). The increased market presence of OxyContin led to high levels of diversion to non-medical use, making it one of the leading drugs of abuse (Cicero et al., 2005).[8] Many experts have implicated OxyContin as a key cause of the opioid epidemic (e.g., Kolodny et al., 2015), suggesting that its removal or reformulation could also have large effects.

In April 2010, Purdue Pharma introduced a reformulated version of OxyContin which was designed to make the drug more difficult to abuse. The abuse-deterrent version uses physicochemical barriers to make the pill hard to break, crush, or dissolve. This increases the costs of misusing OxyContin while maintaining the medical benefits of the drug. It is not "abuse-proof" as the reformulated version can still be abused orally (i.e., taking higher doses than prescribed) and

[7] Ironically, the time-released aspect of OxyContin led FDA officials to initially believe that OxyContin would be less attractive to abusers since absorption of the drug would be delayed. The original product label included the false statement that OxyContin had a lower potential for abuse. This claim was central to the marketing campaign.
[8] OxyContin was the first drug targeted for monitoring by the DEA by its brand name, specifically (GAO, 2003).

some users have even found ways to counteract the abuse-deterrent properties of the new version.[9] However, it has been shown to decrease abuse rates. It was the first drug product to ever receive an "abuse-deterrent" designation from the FDA.[10] In August 2010, Purdue Pharma stopped distributing the original formulation of OxyContin to pharmacies.

Recent research has shown that the replacement of the original formulation with the abuse-deterrent version increased heroin overdose rates (Alpert et al., 2018; Evans et al., 2019), other harms such as heroin-specific treatment admissions (Alpert et al., 2018), and acute hepatitis C infection rates (Powell et al., 2019). In addition, Schnell (2017) shows that some physicians reduced prescribing rates of OxyContin after it became more difficult to abuse and uses this information to study how physicians alter their behavior given the illicit secondary market for prescribed opioids.

Prior research has shown that reformulation reduced misuse of OxyContin specifically. In Cicero and Ellis (2015), a small sample (N=153) of OxyContin users were asked how they responded to the reformulation: 33% indicated that they replaced OxyContin with other drugs. Of this group, 70% reported that they had switched to heroin and a smaller proportion switched to other opioids (primarily other forms of oxycodone). Only 3% reported that they stopped abusing drugs altogether. Alpert et al. (2018) found a reduction in OxyContin misuse overall in the NSDUH of about 40% as well as reductions in the legal oxycodone supply. However, Alpert et al. (2018) find little evidence that reformulation reduced overdoses overall. When studying all opioid overdoses as the outcome, they estimate positive (though statistically insignificant) effects, suggesting that any decline in overdoses involving prescription opioids was offset and maybe overwhelmed by the rise in overdoses involving illicit opioids. However, this does not necessarily mean that underlying opioid misuse rates remained the same or increased, indicating scope for potential positive effects on other dimensions such as labor supply. However, the survey evidence presented in Cicero and Ellis (2015) casts some doubts on this possibility.

Given the widespread interest in understanding the labor supply and health implications of the ongoing opioid crisis, we add to these literatures by studying labor supply and disability responses to OxyContin reformulation, a major transition in the opioid crisis which is still impacting

[9] Highly sophisticated methods were shared on websites for how to counteract the abuse-deterrent properties of the drug involving baking, freezing, or soaking the pill in solvents (Goodnough and Zezima, 2011; Becker and Fiellin, 2017). Cicero and Ellis (2015) noted that the significant time effort required should deter use of these methods.
[10] This reformulated version received an official "abuse-deterrent" designation from the FDA in April 2013.

it today. Did the widespread shift from prescription to illicit opioid markets hurt labor markets and increase disability claiming?

3. Data

Our analysis will first explore traditional labor outcomes. We constructed labor supply outcomes using the monthly Current Population Study (CPS), relying primarily on state-level measures of employment and labor force participation for 2001-2015. We select these years for consistency with the disability data source described below. We will rely on these measures from the CPS for the 18-64 age group, also selected to remain consistent with the disability analysis, though we will also estimate our models for the 25-54 age group to focus more on the traditional "working age" population.

First, we will study the fraction employed. Second, we will study the fraction not in the labor force. While correlated, these two measures together provide information about whether there are changes in how many individuals are working and whether any changes in this measure reflect exits from the labor force. We aggregated all CPS measures to the annual level.

To construct state-level measures of disability claiming behavior, we used Social Security Administration (SSA) Fiscal Year Disability Claim Data. We will focus on the adult population, but we will also show results for children. These variables are available for calendar years[11] 2001-2015 at the annual level. We will study a range of outcomes. While disability applications and determinations can often take months and even years, the data benchmark each applicant to the *calendar* year in which he or she applied. Thus, when we study the fraction of applicants in a year receiving favorable determinations, this fraction is constructed by year of application, regardless of when the determination is made. This construction is fortunate because we are interested in how the pool of applicants changed post-reformulation.[12]

Our primary measure is the percentage of eligible adults who apply for disability benefits. "Eligible adults" are defined by the SSA as individuals ages 18-64 not already receiving disability benefits.[13] The data that we are using do not distinguish between SSI and SSDI benefits, but the

[11] Despite the title of the data set including "fiscal year," the reported outcomes used in this paper relate to calendar years.

[12] For this reason, we do not study outcomes related to the current number of beneficiaries since it is less clear when we should begin to see effects for outcomes not benchmarked to the original application date. The outcomes used in the paper provide a rich picture of initial behavioral responses and downstream consequences.

[13] Results in this paper are similar if we use the percentage of adults ages 18-64 applying for disability benefits.

vast majority of these applicants are applying for SSDI. We will briefly study child applicants in our analysis, and those applications are primarily for SSI. Our focus on applicants is because applying for SSDI reflects a need or desire to exit the labor force and receive disability benefits.

Figure 1 shows the time series trends in both the percentage of the 18-64 population working and the percentage of eligible adults applying for disability benefits. Notably, there are large changes in both of these measures during the Great Recession, followed by gradual and only partial convergence to the original values by 2015.

We will also study the percentage of eligible adults in the population receiving a favorable determination. This measure reflects both the rate of applying for disability benefits and the frequency with which the state agency reviewing the case determines that the claimant meets the disability criteria. It measures the determination for the initial claim only as other eligibility criteria – such as work history and earnings – are also necessary before the individuals qualifies for benefits. However, this metric is useful for understanding whether the types of claims are changing systematically over time. It is also informative about the downstream implications of any changes in the number of applicants. If there are additional applicants, but they rarely receive favorable determinations, then it is less likely that we would observe longer-term implications on SSDI enrollment. We will also directly study the percentage of *applicants* receiving a favorable determination with the motivation of trying to observe how reformulation affected the health of the applicants. The applicant pool may become less healthy because of the transition to illicit opioids, but the "new" applicants may also potentially be healthier on average since they were on the margins of applying. The net effect is an empirical question.

It is also possible that state agencies respond to the changing environment in a manner which alters their determination decisions. We cannot rule out this possibility, but we note that the agents making these decisions have little incentive to alter their decision-making process due to economic conditions or changes in population health. We interpret our results about the fraction of applicants receiving favorable determinations as indicating changes in the health of the applicants, but we cannot rule out systematic reviewer-side behavioral changes.

To measure non-medical use of OxyContin and pain relievers, we used state-level data from the National Survey on Drug Use and Health (NSDUH). The NSDUH, sponsored by SAMHSA, is a nationally representative household survey of individuals ages 12 and older and is the largest

annual survey collecting information on substance use in the U.S. The survey provides information on "non-medical OxyContin use" within the past year beginning in 2004 as well as "non-medical pain reliever use." These variables provide the cross-state variation in pre-reformulation OxyContin misuse to create our main explanatory variable which predicts changes in behavior. Alpert et al. (2018) found that non-medical OxyContin misuse was highly correlated with measures of oxycodone supply and OxyContin prescriptions in verified claims data. The benefit of this metric over using these alternative measures directly is that the NSDUH measure specifies both "OxyContin" and "misuse," and substitution to heroin due to reformulation requires the interaction of those two properties. This non-medical use variable has been shown to strongly predict differential growth in heroin and synthetic opioid mortality.

In the NSDUH, non-medical use is defined as use by individuals who either (a) were not originally prescribed the medication or (b) use such medications "only for the experience or feeling they caused."[14] Given the sensitive nature of pain reliever misuse, NSDUH provides respondents with a highly private and confidential method for responding to questions in effort to increase honest reporting.[15] Nevertheless, self-reported data on drug use is subject to some under-reporting error. We combine the 2004-2009 surveys when constructing the non-medical use variables to reduce measurement error concerns. We select these years because they precede the 2010 reformulation and are therefore untreated. Appendix Figure 1 provides a map of OxyContin misuse rates by state. While there is some evidence of regional clusters, there is surprising amounts of variation throughout the country.

In Table 1, we show summary statistics based on initial OxyContin misuse for 2004-2009, dividing the sample into "above median" and "below median" states. Alpert et al. (2018) showed that OxyContin misuse rates were uncorrelated with heroin overdose rates prior to reformulation. Similarly, we observe little evidence of pre-reformulation labor outcome differences based on OxyContin misuse. On almost all labor and disability-related outcomes, the two groups of states

[14] Specifically, the respondent is shown cards with the names of different types of pain relievers (including OxyContin) and photos of the pills. They are asked to identify "which of the pain relievers…have you used when they were not prescribed for you or that you took only for the experience or feeling they caused?" This section of the questionnaire is preceded by the following introduction, which further emphasizes non-medical use: "Now we have some questions about drugs that people are supposed to take only if they have a prescription from a doctor. We are only interested in your use of a drug if the drug was not prescribed for you, or if you took the drug only for the experience or feeling it caused."

[15] NSDUH collects data using audio computer-assisted self-interviewing (ACASI) in which respondents read or listen to the questions on headphones and respond using a NSDUH laptop computer, rather than to an interviewer.

have nearly-identical rates before 2010. For demographics, we find that the high misuse rates are smaller in terms of population and a higher proportion of the population is white. Age and education shares are relatively similar though.

Our analysis adopts an event study framework, discussed in the next section. Event studies are designed to help evaluate for pre-existing trends but, notably, we also have similar pre-existing levels for our outcomes. This quality is an important feature for difference-in-differences designs according to recent work (Kahn-Lang and Lang, 2019).

4. Empirical Strategy

We adopt an event study empirical design, which estimates the relationship between initial OxyContin misuse and labor/disability outcomes in each year, normalized to 0 in 2009. The specification is

$$(1) \qquad Y_{st} = \alpha_s + \gamma_{rt} + \delta_t \times OxyRate_s^{Pre} + \theta_t \times PainRelieverRate_s^{Pre} + X'_{st}\varphi + \varepsilon_{st},$$

where Y_{st} is the labor supply or disability outcome in state s and year t; $OxyRate_s^{Pre}$ represents the fixed OxyContin misuse rate in state s in the pre-reformulation period (2004-2009). $PainRelieverRate_s^{Pre}$ represents the pain reliever misuse rate in state s in the pre-reformulation period. Alpert et al. (2018) found evidence of relative *reductions* in heroin overdoses associated with the more general pain reliever misuse variable, consistent with systematic adoption of policies to reduce opioid-related harms in high misuse states. We include the pain reliever misuse variables to account for outcome changes related to pain reliever use more generally and isolate the changes which are unique to OxyContin misuse. We also include a set of time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. The specification also includes state and time-region fixed effects to account for fixed differences across state and regional secular trends in labor outcomes. We define "region" here simply as East and West and permit the time fixed effects to vary on this dimension because of the different evolution of the opioid crisis in these areas during our time period (Abouk et al., 2019).

This specification permits us to transparently trace out the trajectory of the relationship between OxyContin misuse and the outcomes over time. We will test for pre-existing trends while also studying the timing of any effect given the possibility of lagged effects in this context. We will plot the δ_t estimates with 95% confidence intervals, adjusted for state-level clustering. We will report joint significance of the 2011-2015 estimates as a test of a differential post-reformulation non-zero effect. The null hypothesis is that each of the estimates is equal to zero.

As one test of pre-existing trends, we will also report joint significance of the 2005-2008 estimates. These tests are comparisons of those estimates to the omitted 2009 variable. We exclude 2010 from both the pre- and post-reformulation tests since it is partially-treated. The event studies themselves plot estimates back to 2001 so that we can observe longer pre-trends, but we think that a focus on the 2005-2008 period (relative to 2009) for a formal test of pre-existing trends is more relevant.

In addition to the event studies, we will also summarize the post-reformulation effects using a difference-in-differences design. We rely on the event study estimates to provide evidence about the appropriateness of this approach. For this specification, we interact both misuse variables with an indicator equal to 1 in the "post" period, defined as after 2011. The specification is

$$(2)\quad Y_{st} = \alpha_s + \gamma_{rt} + \delta\, OxyRate_s^{Pre} \times Post_t + \theta\, PainRelieverRate_s^{Pre} \times Post_t + X_{st}'\varphi + \varepsilon_{st}.$$

We will use 2007-2015 data when estimating this specification, excluding 2010 since it is a partially-treated year, though we will also provide estimates including additional years.

5. Results

5.1. OxyContin Misuse

In Appendix Figures 2 and 3, we show that reformulation induced larger reductions in OxyContin misuse in high OxyContin misuse states. Appendix Figure 2 divides states into 4 groups based on initial misuse rates and shows that the decline in OxyContin misuse has a monotonic relationship across these quartiles. For Appendix Figure 3, we estimate an event study with

OxyContin misuse by 2-year wave as the outcome,[16] similar to a "first stage" relationship for the available data. We estimate that the 2012-2013 estimate is smaller and statistically different than the pre-reformulation estimates. These results show that the states with higher rates of non-medical OxyContin use prior to reformulation were most impacted by reformulation and experienced the sharpest declines in OxyContin misuse.

5.2. Labor Supply

Using CPS data, we first flexibly evaluate the temporal relationship between OxyContin misuse and the fraction of the population working and the fraction of the population not in the labor force. These estimates are presented in Figure 2. There is little evidence of any systematic trends prior to 2010 based on OxyContin misuse. In 2010 and 2011, the share working increases disproportionately in high misuse states. In fact, the 2011 estimate is statistically significant from zero at the 10% level. After 2011, the estimates remain positive but are closer to zero and statistically insignificant. Jointly, the post-reformulation estimates are not statistically different from zero.

Figure 2B shows the corresponding estimates for the share of the population not in the labor force. Again, we observe little evidence of pre-existing trends. After reformulation, there are no systematic movements until some evidence of a decline beginning in 2014. However, we cannot reject that the effect size is equal to zero (both jointly and for each post-reformulation estimate).

We include difference-in-differences estimates in Table 2 to summarize these effects and explore the robustness of the results. In Column (1), we do not include the pain reliever misuse variable. We estimate a small and statistically insignificant increase in the probability of working. When we add the pain reliever misuse variable, there is little change in the estimate.

In Columns (3) and (4), we add covariates and policy variables, respectively. These controls have little effect on the results. In Column (5), we include 2010 in the analysis and, in column (6), we extend the length of the pre-period. The results are not sensitive to the years included in the analysis. In Column (7), we limit the analysis to states west of the Mississippi. The motivation for

[16] The public NSDUH only provides data for 2-year waves. Given the small sample sizes of the NSDUH, using annual data would provide little benefit for the analysis in this Section and aggregating to 2-year periods (or, at least, estimating 2-year effects) would be preferable even with more granular data.

this test is due to the possible confounding effects of the rise and crackdown of the Florida pill mills which occurred during this time. While the Florida pill mills distributed many types of opioids, there is evidence that they disproportionately relied on OxyContin. Residents of other states often visited Florida for the pill mills, but it is unlikely that the pill mills had a meaningful effect on opioid use in states west of the Mississippi River. We replicate our main analysis while selecting only on these states, studying the effects of variation in OxyContin misuse for only these states. Here, we do find evidence of a small and statistically significant increase in the share working. The standard deviation of the non-medical OxyContin use variable is 0.23, implying that a one standard deviation increase in pre-reformulation misuse leads to an increase in the percentage working by 0.5 percentage points, a 0.7% increase from baseline. A positive effect on this dimension is consistent with a reduction in overall opioid misuse due to reformulation.

Finally, we study the log of the outcome to estimate proportional effects. Again, we cannot reject that there is no change in labor supply. Overall, we find limited evidence of effects on the fraction of the 18-64 population working. Table 2.B includes the same analyses for the fraction not in the labor force. As before, we find only limited evidence on any effects on this dimension, regardless of covariates, time periods, and functional form. The results generally point, as expected, in the opposite direction of those presented in Table 2.A given the nature of the outcomes.

We study labor outcomes for ages 18-64 to remain consistent with the disability variables used in the next section. However, we can repeat these analyses for the 25-54 population to focus more on the working age population. The event studies are provided in Appendix Figure 4. The results generally look similar to the Figure 2 event studies. The post-reformulation estimates are jointly statistically significant different from zero in these figures, but the direction of the effect varies throughout the post-period. Appendix Table 1 replicates Table 2 for the 25-54 age group and there is less evidence of any relationship. Even the West-only estimate, which suggested a positive relationship between reformulation and working, is not statistically different from zero. Appendix Table 2 dives into this change further by studying the 18-24 age group. This age group drives the relationship between OxyContin misuse and percentage working in the West. Given that the higher employment rates may potentially reflect changes in education attainment at this age group, we are less inclined to view the positive relationship in the West as possible evidence of beneficial effects of reductions in opioid misuse.

These labor supply measures represent the labor market for a broad cross-section of the population. We turn next to studying our disability insurance variables. While interesting in themselves, these measures also likely reflect a more relevant dimension of the labor force. Labor force participation may not respond to reformulation, but disability insurance applications provide a window into the effects for a segment of the population on the margins of the labor force. The implications of disability applications are also significant given that disability insurance enrollment is often an absorbing state and signals a potentially near-permanent removal from the labor force.

5.3. Disability Insurance

We study the percentage of the eligible adult population applying for disability benefits in Figure 3. From 2007 to 2009, the estimates are rather flat, though this period is preceded by a small (but statistically insignificant) increase. In 2010, we estimate a small increase followed by a much larger jump in 2011. The 2011 increase is uniquely large in the event study and represents the effect in the first full year after reformulation. The estimates then generally increase until the end of the sample period. Overall, we can statistically reject that the post-reformulation estimates are equal to the 2009 difference. We cannot reject that the 2006-2008 estimates are statistically different from this baseline.

In general, we are less interested in the relationship between the more general measures of pain reliever misuse throughout this paper. We believe that the estimates associated with this variable reflect the effects of a variety of policies targeting prescription pain reliever use, and the inclusion of these variables helps isolate the effects of interest. However, we show the event study estimated jointly along with Figure 3 in Appendix Figure A.5. We observe reductions in disability applicants throughout the post-reformulation period. This result is consistent with the corresponding findings in Alpert et al. (2018) that states with more (non-OxyContin) pain reliever misuse experienced better outcomes after reformulation due to systematic adoption of other policies.

The estimates in Figure 3 imply that a 1 percentage point increase in pre-reformulation OxyContin misuse predicts an increase in disability applications among eligible adults by about 0.3 percentage points (on average) per year. This estimate indicates that a standard deviation increase in the OxyContin misuse rate is associated with a rise in disability applications of 0.07 percentage points. Relative to a post-reformulation disability application rate of about 1.4 percentage point, this

growth is equivalent to about a 5% increase. Alpert et al. (2018) concluded that each standard deviation of non-medical OxyContin misuse was associated with a 47% increase in heroin mortality. In a proportional sense, our effects are smaller, suggesting that reformulation more directly affected substance use and overdoses relative to labor and disability outcomes. One possible reason is that non-medical OxyContin misuse may itself strongly push people towards applying for disability. Reformulation then may have a more marginal effect on this dimension relative to overdose propensities involving heroin. However, we note that on its own, the estimated effect is quite large, suggesting that the substitution to illicit markets has led to a meaningful increase in disability applicants than a counterfactual in which reformulation did not occur.

To summarize these results, we present difference-in-difference estimates in Table 3. We estimate that each additional percentage point increase in pre-reformulation OxyContin misuse leads to an additional 0.2 percentage point increase in SSDI applications per eligible adult. This estimate increases to 0.3 when we separately account for non-medical pain reliever misuse. Adding covariates (column 3) and policy variables (column 4) has minimal effects on this estimate. Including 2010 causes the estimate to decrease to 0.26 given that 2010 is partially-treated. Adding more years to the pre-period increases the estimate to 0.34. If we select only on states west of the Mississippi, we estimate an effect of 0.3, though the estimate is not statistically different from zero given the reduced sample size. However, below, we show the equivalent event study and find that the year-by-year post-reformulation estimates for the West are jointly statistically significant from zero. In the final column, we study the log of the outcome. The estimate implies similar level effects.

As a complementary measure, we study the share of the eligible adult population which received a favorable determination. Individuals can only receive a favorable determination if they apply, but this metric provides additional information about whether the SSA state agency determined that the person met the disability criteria. Other criteria must also be met to become a beneficiary, so this is not perfect measure of changes in beneficiaries, though it is certainly informative of the downstream effects of changes in application rates. In the data, the determination decision is benchmarked to the year in which the person applied.

Figure 4 presents the event study results. As before, we observe little evidence of pre-existing trends, followed by increases beginning in 2010. Each estimate from 2010 to 2015 is statistically different from zero. The post-reformulation estimates range between 0.1 and 0.2, smaller than the application rate estimates in Figure 3, suggesting that only a fraction of the new

applicants due to reformulation were found to meet the SSA disability criteria. However, we can statistically rule out that none of the new applicants were found to meet the SSA disability criteria, despite the rule that specifies that substance abuse condition alone cannot qualify one for disability benefits. This increase in favorable determinations suggests that reformulation may have induced people with other disabling conditions to apply for disability insurance or it may have worsened existing disabling conditions, qualifying some workers for benefits. Alternatively, reformulation may have more broadly affected local economic conditions which increased applications and, as a consequence, favorable determinations.

To summarize these results, we provide our difference-in-differences estimates in Table 4. When we include the pain reliever misuse variable, we estimate a post-reformulation effect associated with OxyContin misuse of 0.15, robust to including time-varying covariates and control variables. The result is also generally robust to changing the time period of the sample (columns 5 and 6). Interestingly, the point estimate increases when we select on states in the West. When the outcome is logged, the implied level effect is similar. As before, the estimate is robust to a variety of concerns. An estimate of 0.15 implies that a standard deviation increase in the non-medical OxyContin misuse rate is associated with an additional 0.03 percentage points in the percentage of eligible adults receiving favorable determinations, equivalent to about an 8% increase over baseline.

To better understand the Figure 3 and Figure 4 results (and equivalent results in Tables 3 and 4), we study the adult favorable determination rate, the fraction of adult applicants who received a favorable determination. As before, this statistic is benchmarked to the year of application, not the year of the determination. One possibility is that reformulation increased application rates, but these new "marginal" candidates were less likely to receive a favorable determination. Alternatively, it is possible that reformulation worsened the health of applicants and increased favorable determination rates. Other possible explanations are also possible and were discussed above. We present the event study estimates in Figure 5. We observe a small increase in the favorable determination rate post-reformulation. This result suggests that reformulation increased disability application rates and increased favorable determination rates conditional on applying, combining to increase the share of the eligible population receiving a favorable determination for the SSA disability criteria.

We present the corresponding difference-in-differences estimates in Table 5. The estimates are generally positive but not statistically different from zero. Overall, we take this as suggestive evidence that reformulation led to an increased rate in favorable determinations (per applicant),

though the primary rise in favorable determinations per eligible adult is driven by the increase in applicants, not a dramatic change in the decisions made for applicants. This decomposition is interesting because one hypothesis could be that reformulation drives more people to apply for disability benefits, but this marginal group is healthier on average so they are less likely to receive a favorable determination. The evidence suggests that this is not the case.

5.4 Discussion

Our main analysis above explores many possible threats to isolating the differential impact of reformulation while also presenting event study estimates to transparently observe pre-reformulation trends based on exposure to reformulation. For all of our labor supply and disability measures, we do not observe meaningful differential changes in 2009 despite the large shocks observed in the time series trend in disability applications shown in Figure 1. Thus, we do not believe that the Great Recession disproportionately affected high or low OxyContin misuse states. Instead, we generally observe a small effect in 2010 followed by much larger effects beginning in 2011, which is more consistent with OxyContin reformulation driving these differences, and not confounding shocks to economic conditions. This timing is consistent with the differential rise in heroin overdoses and other heroin-related harms estimated in Alpert et al. (2018). The timing of the effect is also interesting because we observe a rather immediate effect given that we see evidence of a partial rise in 2010, implying that reformulation induced a quick response on this margin. This short-term effect would be consistent with the harms associated with substitution to heroin by first-time, unsophisticated users of heroin.

Interestingly, we observe little evidence of reformulation effects for broad measures of labor supply. In fact, we can even rule out rather modest effects. Using our preferred model (represented in Table 2, Column 4), our 95% confidence intervals exclude reductions in the share working of 0.006 associated due to a higher OxyContin misuse rate of 1 (a large hypothetical difference equal to almost twice the national average), which would represent less than a 1% decrease relative to baseline. This lack of a labor supply effect is also suggestive that the Great Recession is not differentially affecting high OxyContin misuse states given that the Great Recession had large effects on this margin. However, we can more precisely estimate small increases in applications for disability benefits. By focusing on disability-related outcomes, we are able to hone in on more exposed populations and relevant shifts which are otherwise masked in metrics of overall labor force participation.

We estimate that a one standard deviation increase in pre-reformulation OxyContin misuse is associated with a rise in disability applications of 0.07 percentage points. This is smaller than the estimated effect of a 1 percent decrease in the unemployment rate in Autor and Duggan (2003) but still represents a meaningful increase in applications for reasons unrelated to economic conditions.

5.5 SSI Child Applications

While the focus of this paper is on the labor supply outcomes of adults, we are also able to study disability applications for the under-18 population. This population applies for SSI. It is less clear whether we should expect to observe effects for this population. The opioid crisis has impacted birth outcomes such as neonatal abstinence syndrome (NAS) rates, which could affect disability applications later in life, though possibly after our sample period. Parental use of heroin might alter propensities to apply for SSI on behalf of children or directly affect child outcomes. Growth of local illicit markets and changes in economic conditions could also impact children. We replicate our main analyses while using the eligible child population filing rate. The results are provided in Figure 6.

The estimates are noisy and it is difficult to precisely conclude that high OxyContin misuse states experienced increases or decreases in child disability application rates. With that caveat, the pattern of the results suggests a possible immediate decrease in child disability applications, which would be consistent with a decrease in parental non-medical use of (legal and illicit) opioids. This short-term reduction is followed by a steady rise in child disability application rates. Overall, the post-reformulation estimates are jointly statistically significant, implying that we can reject that all of the 2011-2015 estimates are equal to zero, but the signs of the estimates switch between suggesting falls and rises in disability application rates. Thus, while there is some suggestive evidence of a relationship with child disability outcomes, we are less confident in the (short-term and long-term) direction of this effect.

5.6 Sensitivity Checks

Our main results suggest that disability insurance applications and enrollment differentially increased in states with higher pre-reformulation rates of OxyContin misuse. This increase happens exactly when we would expect if reformulation were the initiating force and there is little evidence of confounding pre-trends. Our interest in the timing of the effect is partially due to the possible independent effects of the Great Recession. However, the time series trends show that the biggest

shocks due to the Great Recession occurred in 2009, prior to the differential effects that we estimate. This combination of results suggests that the time fixed effects are adequately accounting for the effects of the Great Recession.

We also find that this increase is uniquely associated with OxyContin misuse. Most policies targeted to address opioid prescribing or misuse affect pain relievers broadly. These findings reduce concerns that we are inappropriately associating the differential increase to OxyContin reformulation when it is driven by secular trends or other opioid-specific policies. We also find that our main results are generally robust to controlling for other policy variables.

In this section, we consider some other possible concerns with our main estimates. First, we consider whether our results are driven by modeling reformulation as affecting the level of disability applications. In Appendix Figure A.6, we re-estimate equation (1) but study the log of the share of the eligible adult population applying for disability benefits to estimate proportional effects (equivalent to the final column in Table 3). The pattern of estimates is similar and the magnitudes imply comparable level effects.

Next, we consider the role of covariates by excluding our demographic and policy variables. Our motivation for this test is that unobservable factors are less likely to be important if the observable factors have little effect on the main estimates. We present these estimates in Appendix Figure A.7. Again, the pattern of result is similar.

Finally, one concern with the timing of reformulation is that the rise and crackdown of the Florida pill mills also occurred during the time. We replicate our main analysis while selecting only on states west of the Mississippi River, studying the effects of variation in OxyContin misuse within this region. The results are presented in Appendix Figure A.8. While noisier due to the smaller sample, we observe similar effects (note the larger scale for this figure). The pattern is different as we estimate the largest effect in 2011. The lack of growth post-reformulation may reflect the different evolution of the opioid crisis in the West due to the relative lack of fentanyl for reasons discussed in Abouk et al. (2019). Overall, however, the evidence suggests that the main results are not driven by Florida and any spillovers from the pill mills into other states.

6. Conclusion

There is widespread interest in understanding the broader effects of the opioid crisis for the purposes of quantifying the optimal resources for combatting it and for comprehending the full

nature of the epidemic. Similarly, large literatures consider the determinants of labor supply and social insurance enrollment, but there is little evidence about the impact of widespread substitution from legal to illicit drug markets. While federal social insurance forecasts typically internalize economic conditions and predicted demographic changes, the growth of illicit drug markets is traditionally not part of the calculus and may have unpredictable effects on population health and federal government expenditures. A recent and growing literature has suggested that the opioid crisis may have large effects on labor supply outcomes, typically studying the effects of prescribing rates. In this paper, we study the transition from misuse of prescription opioids to the growth in demand for heroin and illicit fentanyl. Our estimates are likely more relevant to the future of the crisis.

We study traditional labor supply measures but also rely heavily on metrics related to applications and determinations for disability benefits. While the literature has studied the nature and causes of the transition from prescription opioids to illicit opioids, there is little work on its broader consequences beyond overdoses. Labor supply and disability metrics, while important on their own, also act as proxies concerning overall population health, the health of the workforce, and the economy's workforce capabilities. We exploit prior work which has shown that the reformulation of OxyContin, a substantial reduction in the supply of abusable opioids, drove the rise in heroin and synthetic opioid overdoses with disproportionate general equilibrium effects on areas with higher rates of OxyContin misuse (but not necessarily higher rates of pain reliever use overall).

We find little evidence of differential changes in labor supply. However, these aggregated metrics potentially conceal the effects on the population of interest – people at risk of switching to illicit opioids and possibly on the margins of the labor market. Disability claiming behavior, however, potentially reflects the affected population in a useful manner, acting as a valuable measure of a disproportionately affected group. It is also, of course, of special interest itself given the importance of understanding drivers of disability applications.

Our results suggest strong effects on disability claiming behavior for the 18-64 (i.e., SSDI) population. We estimate that a state with a standard deviation higher pre-reformulation non-medical OxyContin rate experienced a 5% increase in disability applicants and an 8% increase in favorable determinations after reformulation. Individuals applying for disability were more likely to receive a favorable determination after reformulation in high non-medical OxyContin use states, suggesting that the applicant pool worsened in health due to the substitution to illicit opioids. While substance

use itself is not a valid disabling condition, this evidence suggests the illicit opioid market growth exacerbated other qualifying disabling conditions or worsened economic conditions in a way which incentivized more disability applications.

Combined with prior evidence found in the literature, these results provide further evidence that the reformulation of OxyContin represented a pivotal transformation in the opioid crisis. The prior literature has focused on extreme measures such as heroin-related fatal overdoses and infectious diseases related to injection use. This paper shows that reformulation impacted other important but less extreme outcomes as well, increasing the rate of applications for disability benefits and the rate of people found to have disabling conditions. As heroin and illicit fentanyl availability continues to grow, we should expect the economic costs of the opioid crisis to grow as well.

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Figures

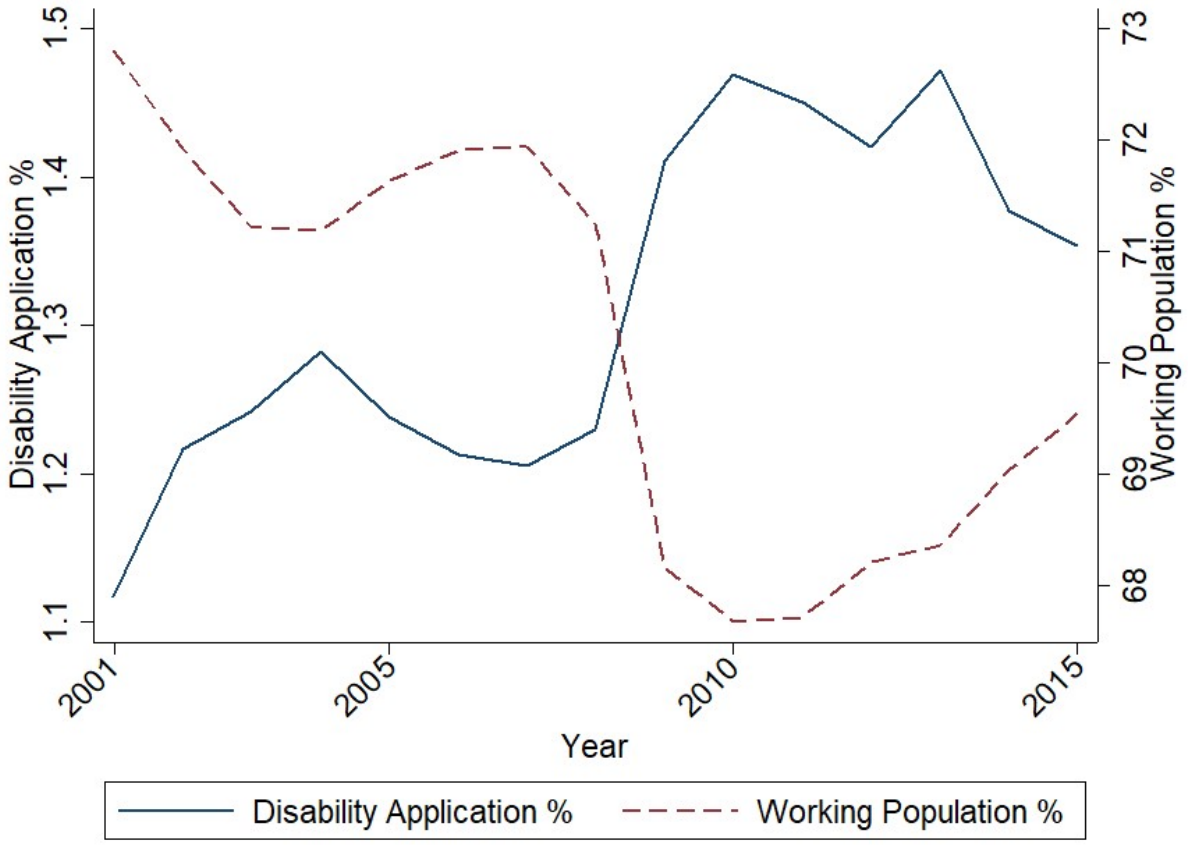
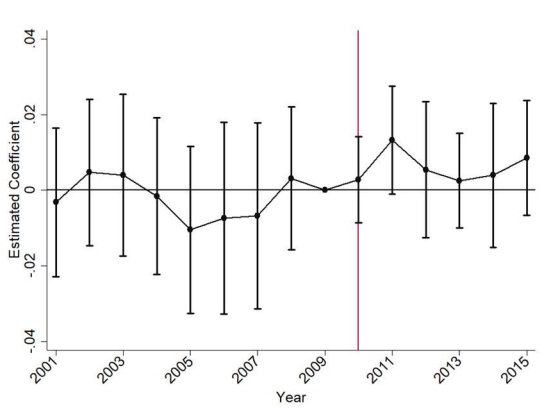


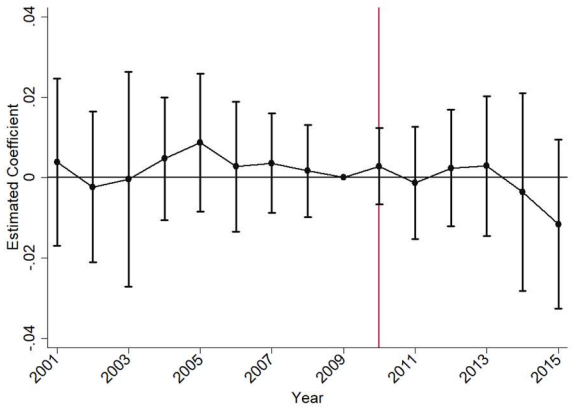
Figure 1: National Time Series Trends in Percentage of Eligible Adults Applying for Disability Insurance and Percentage Working (Ages 18-64)

Source: SSA Fiscal Year Disability Claims Data and Current Population Study



Pre-Period F-Statistic for 2006-2008 = 1.372
Post-Period F-Statistic for 2011-2015 = 2.181

A. % Ages 18-64 Working

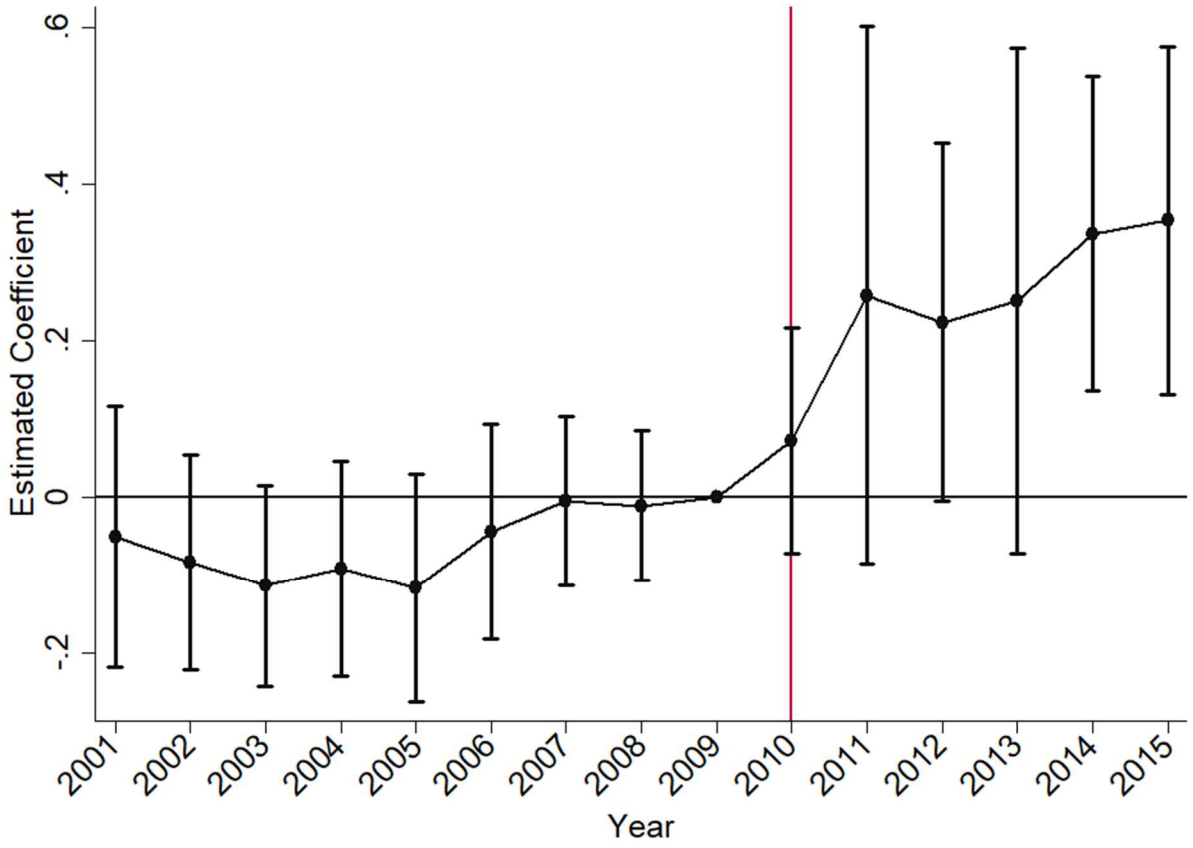


Pre-Period F-Statistic for 2006-2008 = 1.317
Post-Period F-Statistic for 2011-2015 = 1.054

B. % Ages 18-64 Not in Labor Force

Figure 2: Non-Medical OxyContin Misuse Event Study Estimates for CPS Outcomes

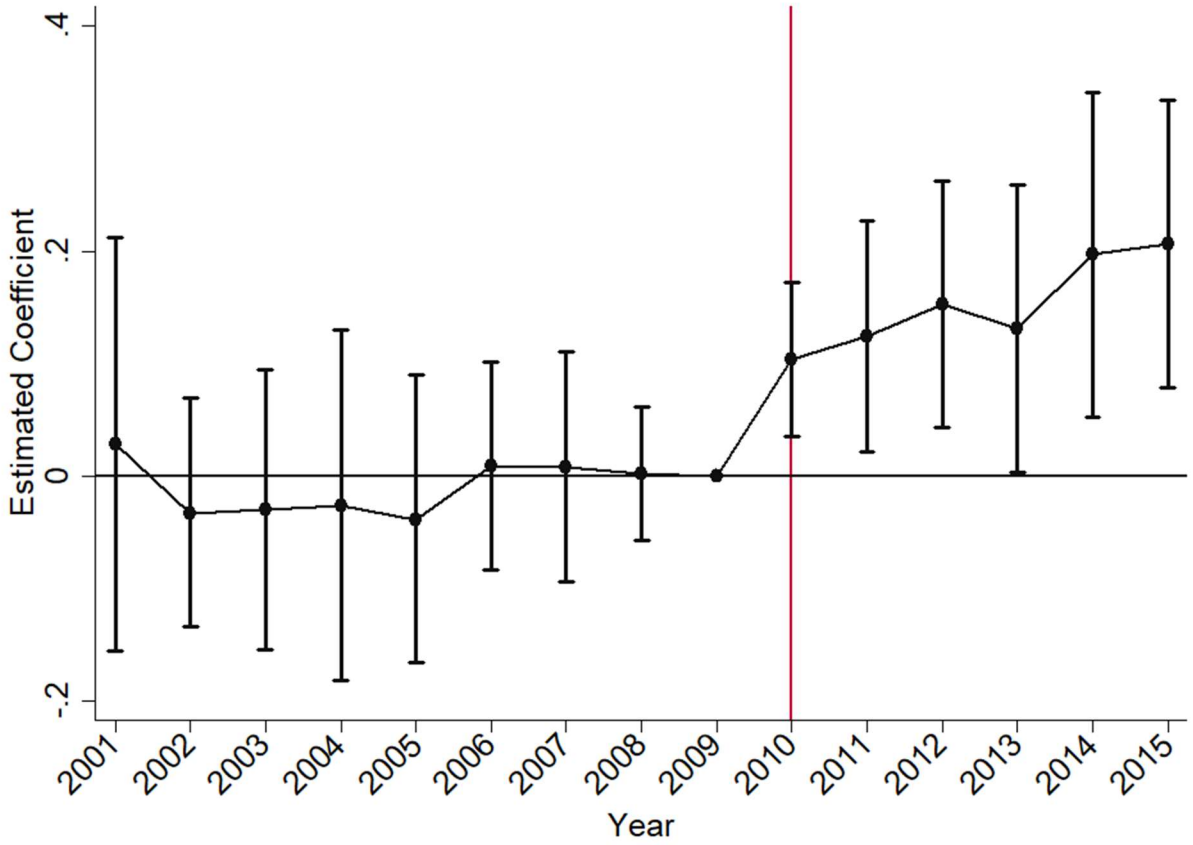
Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is the share of peoples ages 18-64 working (Panel A) and the share not in the labor force (Panel B). Both outcomes are calculated using the Current Population Study. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



Pre-Period F-Statistic for 2006-2008 = 2.000
Post-Period F-Statistic for 2011-2015 = 3.032***

Figure 3: Non-Medical OxyContin Misuse Event Study Estimates for Percent Eligible Adults Applying for Disability

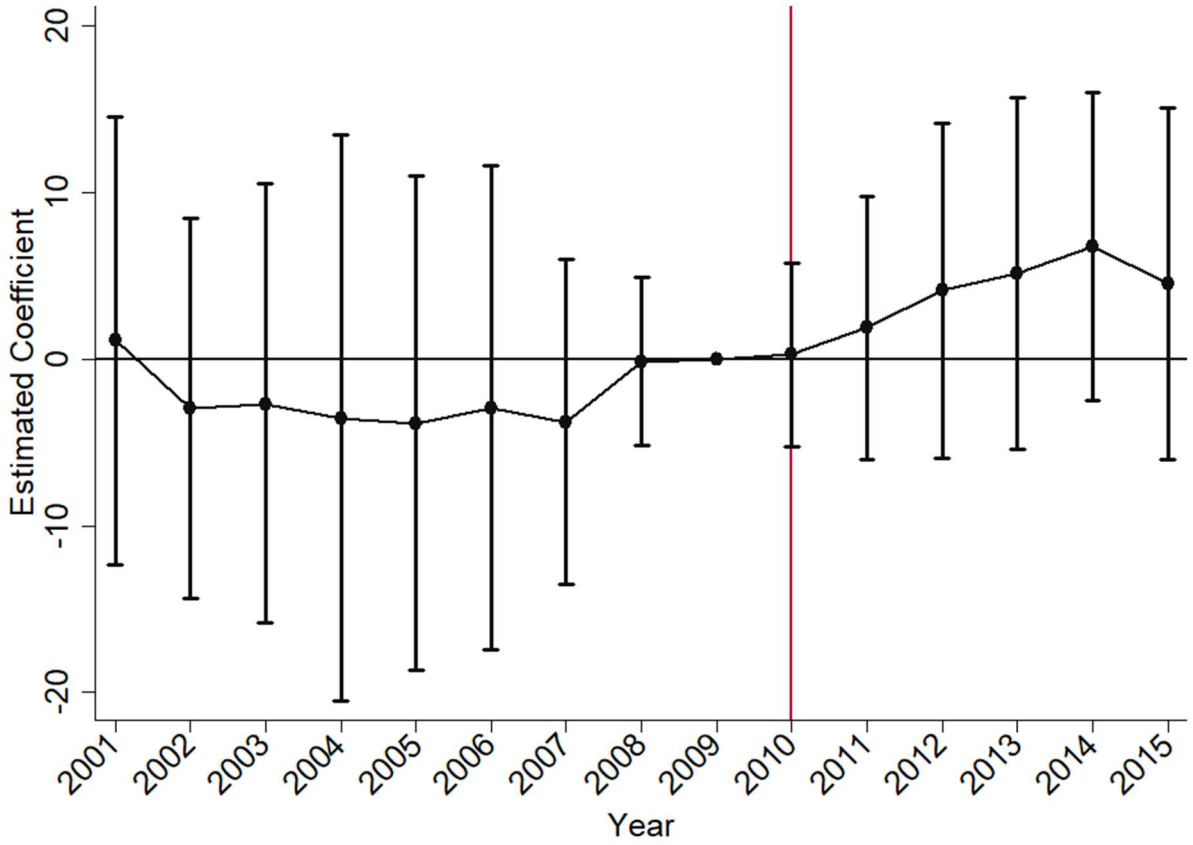
Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is % eligible adults applying for disability in the calendar year. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators. These estimates are provided in Appendix Figure 5.



Pre-Period F-Statistic for 2006-2008 = 2.169
Post-Period F-Statistic for 2011-2015 = 4.540***

Figure 4: Non-Medical OxyContin Misuse Event Study Estimates for Percent Eligible Adults Receiving Favorable Determination

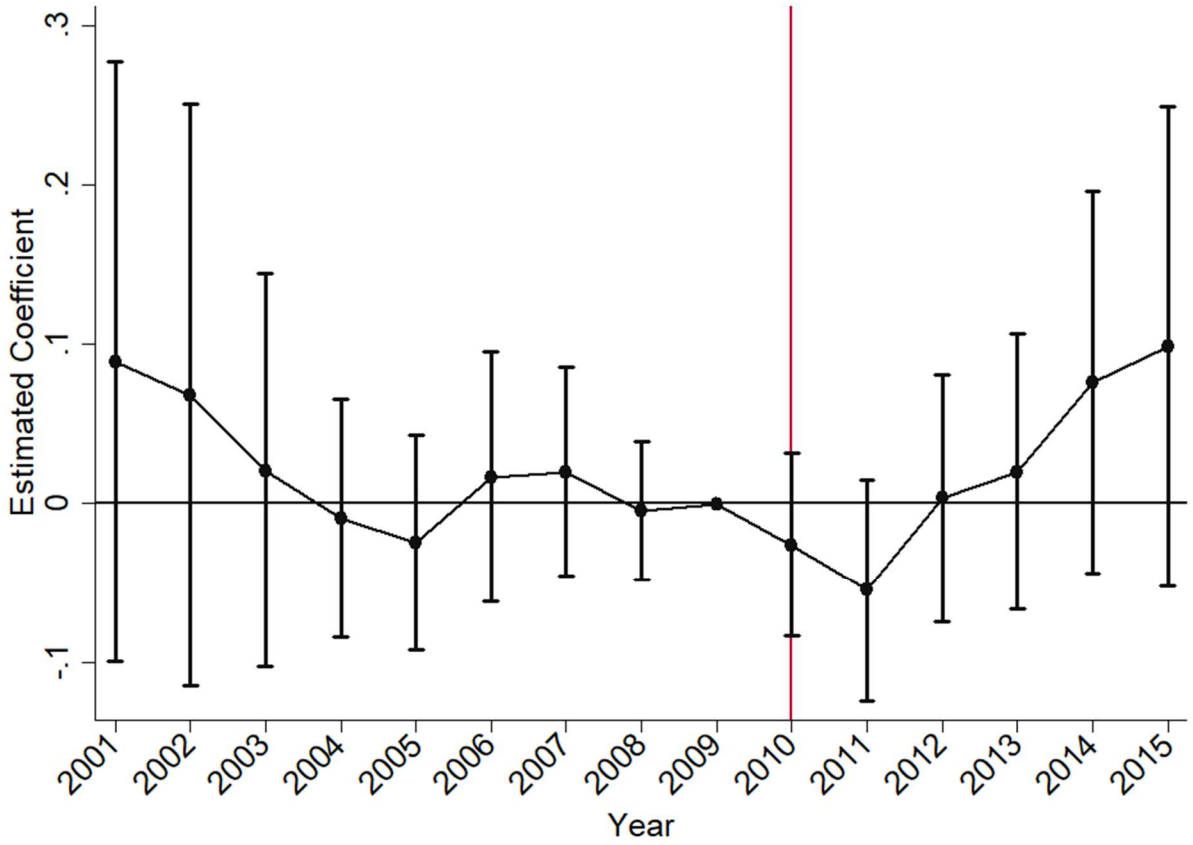
Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is % eligible adults applying for disability in the calendar year and subsequently receiving a favorable determination. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



Pre-Period F-Statistic for 2006-2008 = 1.698
Post-Period F-Statistic for 2011-2015 = 3.804***

Figure 5: Non-Medical OxyContin Misuse Event Study Estimates for Percent of Adults Applying for Disability Receiving Favorable Determination

Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is % of adults who applied for disability in the calendar year receiving a favorable determination. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



Pre-Period F-Statistic for 2006-2008 = 1.291
Post-Period F-Statistic for 2011-2015 = 2.835**

Figure 6: Non-Medical OxyContin Misuse Event Study Estimates for Percent Eligible Children Applying for Disability

Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is % eligible children applying for disability in the calendar year. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.

Tables

Table 1. Summary Statistics

Variable	All States	Below-Median Oxycontin Misuse States	Above-median Oxycontin misuse states
Outcomes (CPS) pooled from 2004 to 2009			
Population at work last week (%)	70.00%	69.75%	70.57%
Population not in labor force for any reason (%)	22.42%	22.64%	21.91%
Outcomes (SSDI) pooled from 2004 to 2009			
SSDI and SSI DC beneficiaries (%)	4.39%	4.22%	4.76%
SSDI adult beneficiaries (%)	5.51%	5.31%	5.96%
SSI DC child beneficiaries (%)	1.50%	1.48%	1.56%
SSDI and SSI DC applications (%)	1.05%	1.02%	1.12%
SSDI adult applications (%)	1.24%	1.20%	1.33%
SSI DC child applications (%)	0.59%	0.58%	0.61%
Eligible Adult Population Allowance Rate (%)	0.41%	0.41%	0.43%
Adult Favorable Determination Rate (%)	35.66%	36.19%	34.45%
Demographic Characteristic pooled from 2004 to 2009			
Percentage White	80.22%	77.82%	85.71%
Percentage Ages 25-44	27.55%	27.89%	26.78%
Percentage with college degree	18.82%	19.12%	18.16%
Population (unweighted)	5,877,760	8,341,671	3,508,615
Misuse			
OxyContin misuse rate	0.57	0.45	0.84
Number of States	51	25	26

Notes: All statistics are population-weighted (unless otherwise specified).

Table 2: Difference-in-Differences Estimates for CPS Outcomes

Panel A: Share Working

Outcome:	Share Working (ages 18-64)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	0.002	0.008	0.008	0.012	0.010	0.012	0.020**	0.020
	(0.009)	(0.012)	(0.011)	(0.009)	(0.007)	(0.008)	(0.008)	(0.014)
Pain Reliever Misuse x Post		-0.003	-0.003	-0.004*	-0.003	-0.004**	-0.008***	-0.006*
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Panel B: Not in the Labor Force

Outcome:	Share Not in Labor Force (ages 18-64)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	0.004	-0.004	-0.005	-0.006	-0.006	-0.005	-0.013***	-0.002
	(0.007)	(0.010)	(0.009)	(0.007)	(0.007)	(0.005)	(0.004)	(0.026)
Pain Reliever Misuse x Post		0.004**	0.004**	0.004***	0.003***	0.004***	0.005***	0.009*
		(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.005)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Notes: Standard errors in parentheses adjusted for clustering at the state-level. State fixed effects and region-time fixed effects (where region is East or West) included in all specifications. Post is equal to 1 for years 2011+. Time-varying controls are the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries.

***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 3: Difference-in-Differences Estimates for % Eligible Adults Applying for Disability

Outcome:	% Eligible Adults Applying for Disability							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	0.226**	0.302**	0.297**	0.283**	0.262**	0.341***	0.299	0.237**
	(0.105)	(0.125)	(0.122)	(0.115)	(0.100)	(0.123)	(0.197)	(0.090)
Pain Reliever Misuse x Post		-0.039	-0.041	-0.058*	-0.061**	-0.055	-0.077	-0.047**
		(0.028)	(0.031)	(0.031)	(0.028)	(0.033)	(0.051)	(0.021)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Notes: Standard errors in parentheses adjusted for clustering at the state-level. State fixed effects and region-time fixed effects (where region is East or West) included in all specifications. Post is equal to 1 for years 2011+. Time-varying controls are the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries.

***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 4: Difference-in-Differences Estimates for % Eligible Adults Receiving Favorable Determinations

Outcome:	% Eligible Adults Favorable Determinations							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	0.106*	0.151**	0.141**	0.143**	0.117**	0.172***	0.175**	0.394**
	(0.057)	(0.065)	(0.058)	(0.056)	(0.044)	(0.059)	(0.074)	(0.153)
Pain Reliever Misuse x Post		-0.023*	-0.024*	-0.031**	-0.027**	-0.033**	-0.043**	-0.073**
		(0.013)	(0.013)	(0.013)	(0.011)	(0.015)	(0.021)	(0.035)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Notes: Standard errors in parentheses adjusted for clustering at the state-level. State fixed effects and region-time fixed effects (where region is East or West) included in all specifications. Post is equal to 1 for years 2011+. Time-varying controls are the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries.

***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Table 5: Difference-in-Differences Estimates for % Applying Adults Receiving Favorable Determinations

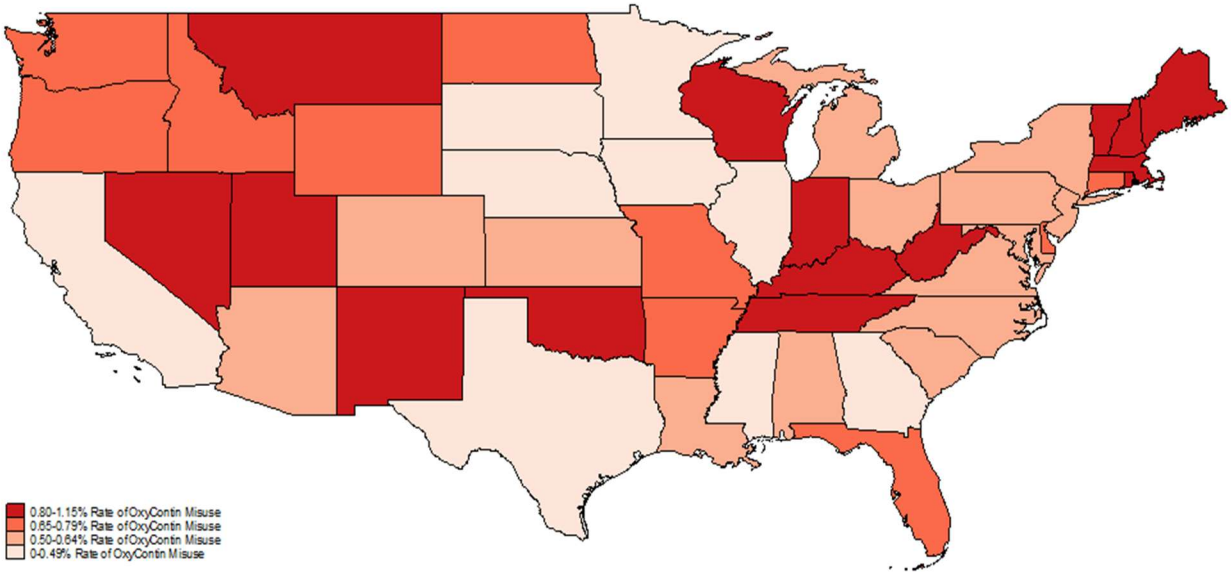
Outcome:	% Favorable Determinations of Adult Applicants							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	3.650	5.005	4.564	4.763	4.728	6.488	4.313*	0.140
	(3.041)	(3.694)	(3.295)	(3.656)	(3.014)	(4.042)	(2.286)	(0.109)
Pain Reliever Misuse x Post		-0.694	-0.849	-0.905	-0.851	-1.280	-1.990**	-0.032
		(0.735)	(0.709)	(0.763)	(0.629)	(0.877)	(0.800)	(0.023)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-201	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Notes: Standard errors in parentheses adjusted for clustering at the state-level. State fixed effects and region-time fixed effects (where region is East or West) included in all specifications. Post is equal to 1 for years 2011+. Time-varying controls are the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries.

***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

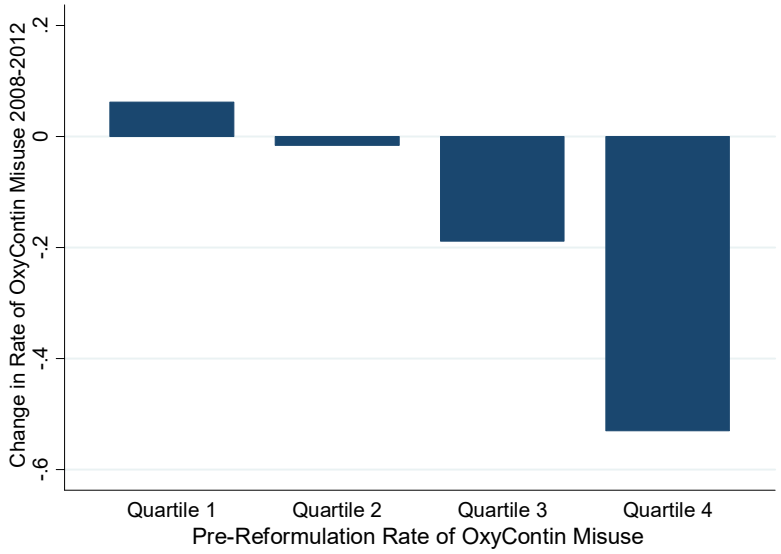
Appendix

Appendix Figure A.1: Geographic Variation in Rate of OxyContin Misuse, 2004-2009



Source: Alpert et al. (2018)

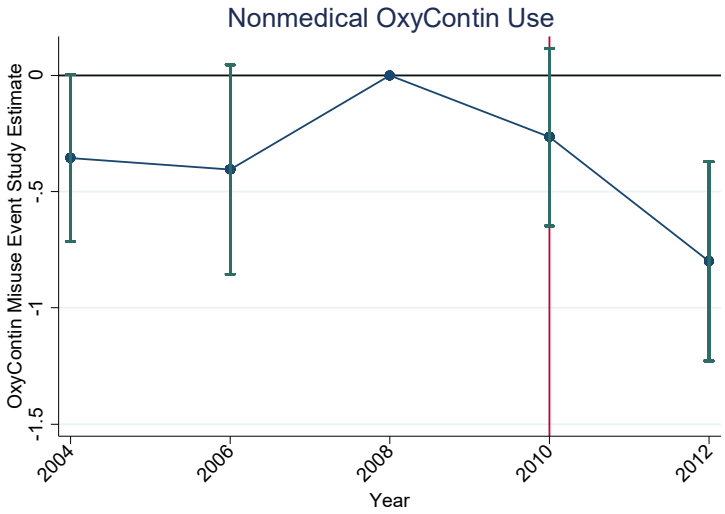
Appendix Figure A.2: Relationship between Pre-Reformulation Rate of OxyContin Misuse and Change Between 2008-2012



Source: Alpert et al. (2018)

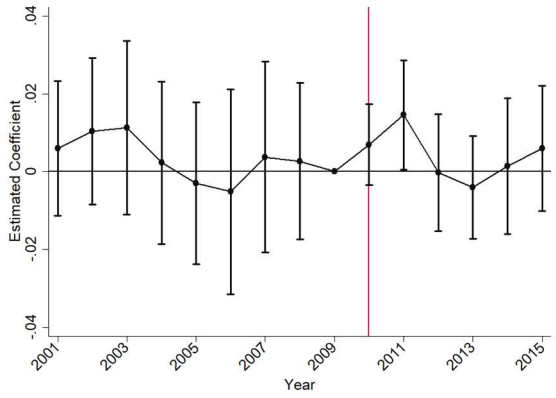
Notes: Quartiles represent states with the highest and lowest pre-reformulation rates of OxyContin misuse (Quartile 4 includes the 25% of states with the highest pre-reformulation rates of OxyContin misuse). The change in the rate of OxyContin misuse is weighted by state population.

Appendix Figure A.3: Relationship Between Initial OxyContin Misuse and Changes in OxyContin Misuse – Event Study Specification



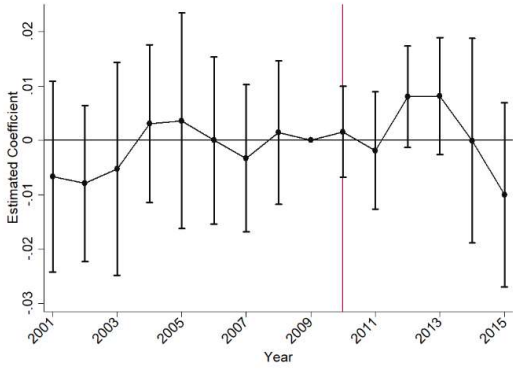
Source: Alpert et al. (2018)

Notes: Each year on the x-axis refers to that year and the following year since each NSDUH wave includes two years. Consequently, we should expect a partial effect in 2010 (which includes post-reformulation year 2011) and a full year effect for 2012 (and 2013). The graph reports point estimates and 95% confidence intervals (which are adjusted for within-state clustering) from the event study in Equation 1 using OxyContin misuse as the outcome variable. We can reject that the 2012-2013 estimate is equal to the 2004-2005 estimate at the 5% level, the 2006-2007 estimate at the 10% level, the (normalized to 0) 2008-2009 estimate at the 1% level, and the (partially-treated) 2010-2011 estimate at the 1% level. A joint test that the 2012-2013 estimate is equal to each of the pre-reformulation estimates (2004-2005, 2006-2007, and 2008-2009) rejects at the 1% level.



Pre-Period F-Statistic for 2006-2008 = 2.046
Post-Period F-Statistic for 2011-2015 = 6.102***

B. % Ages 25-54 Working

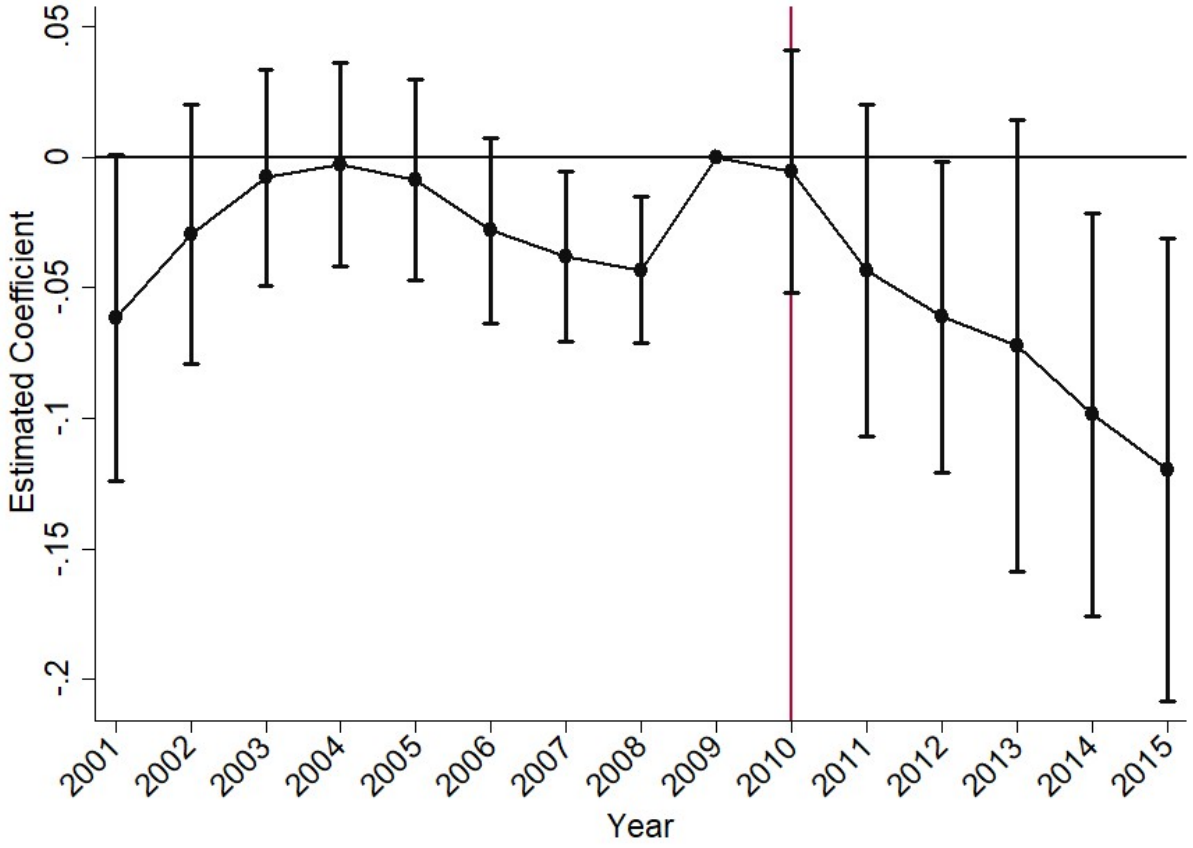


Pre-Period F-Statistic for 2006-2008 = 0.395
Post-Period F-Statistic for 2011-2015 = 3.455***

B. % Ages 25-54 Not in Labor Force

Appendix Figure A.4: Non-Medical OxyContin Misuse Event Study Estimates for CPS Outcomes (Ages 25-54)

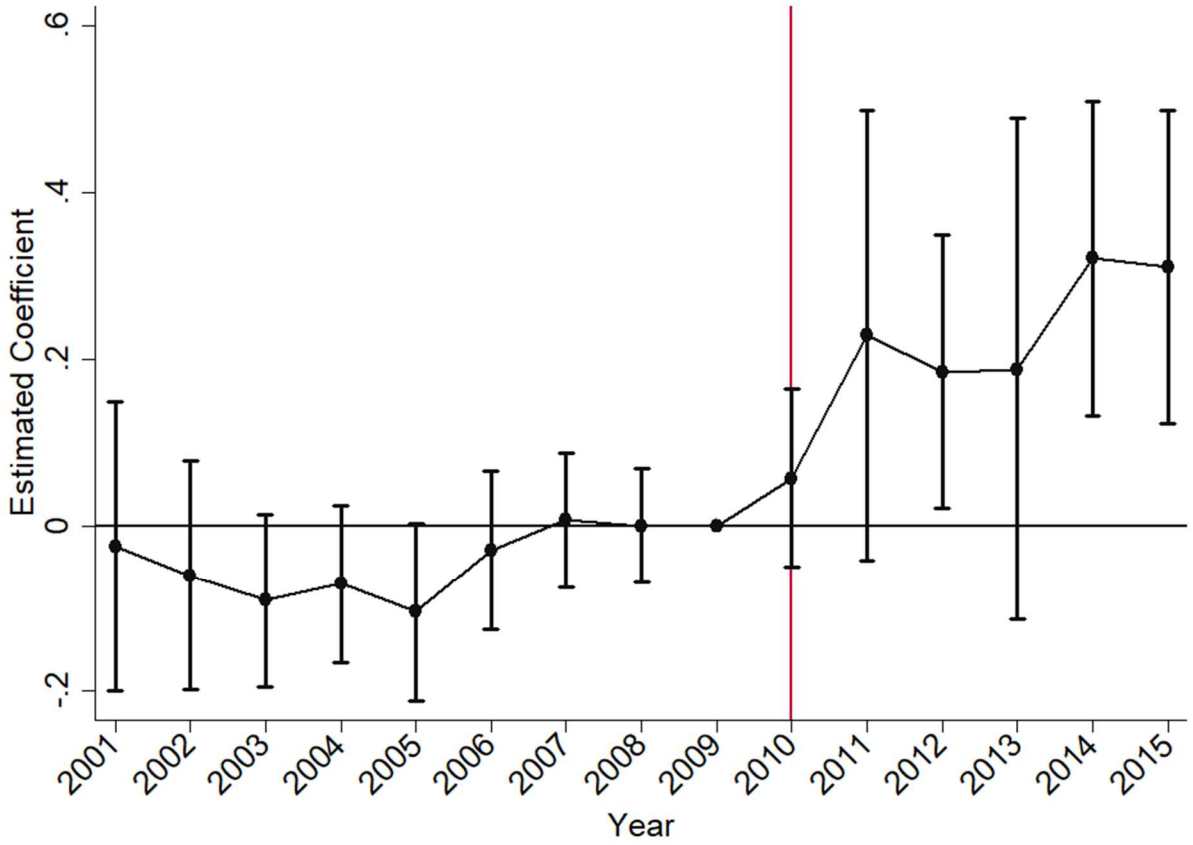
Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is the share of peoples ages 25-54 working (Panel A) and the share not in the labor force (Panel B). Both outcomes are calculated using the Current Population Study. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



Pre-Period F-Statistic for 2006-2008 = 2.465
Post-Period F-Statistic for 2011-2015 = 1.897

Appendix Figure A.5: Non-Medical Pain Reliever Misuse Event Study Estimates for Percent Eligible Adults Applying for Disability

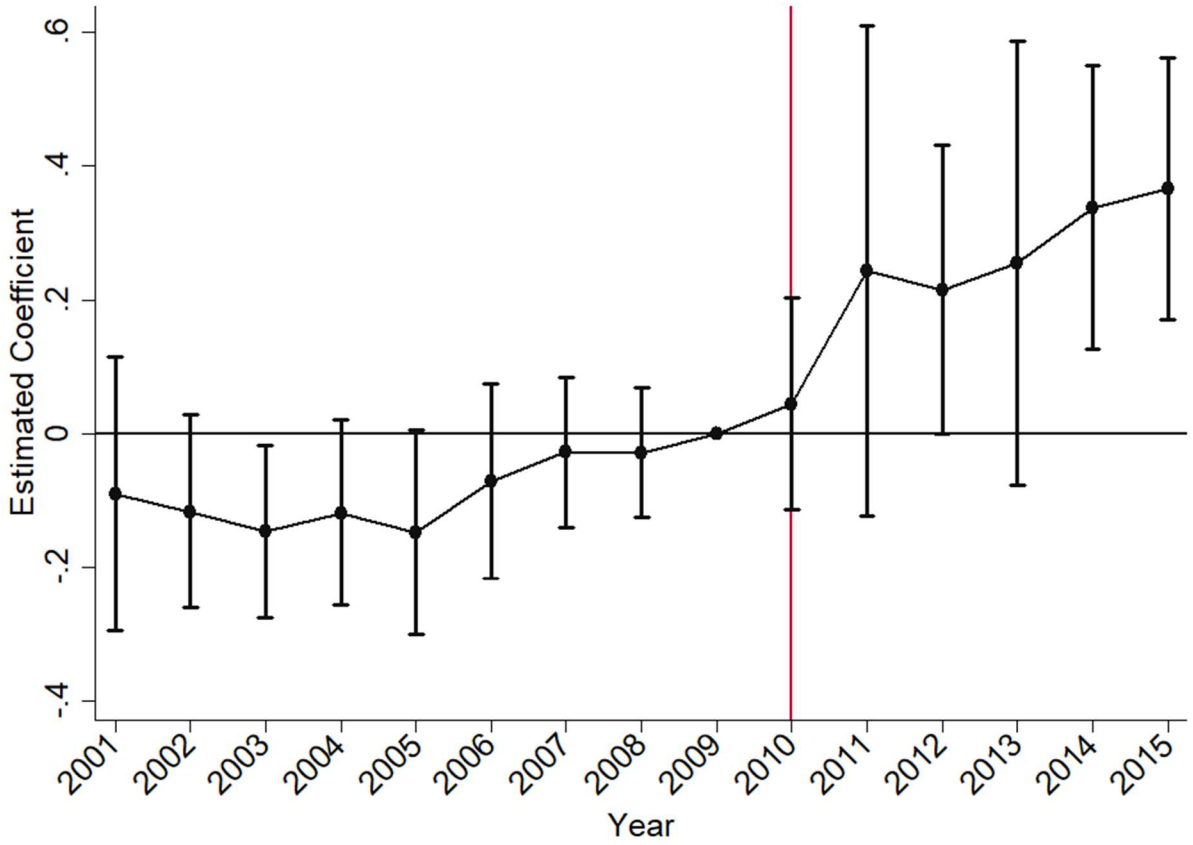
Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is % eligible adults applying for disability in the calendar year. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical pain reliever misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for OxyContin misuse interacted with year indicators. These estimates are shown in Figure 3.



Pre-Period F-Statistic for 2006-2008 = 2.419
Post-Period F-Statistic for 2011-2015 = 6.153***

Appendix Figure A.6: Non-Medical OxyContin Misuse Event Study Estimates for the Log of the Percent Eligible Adults Applying for Disability

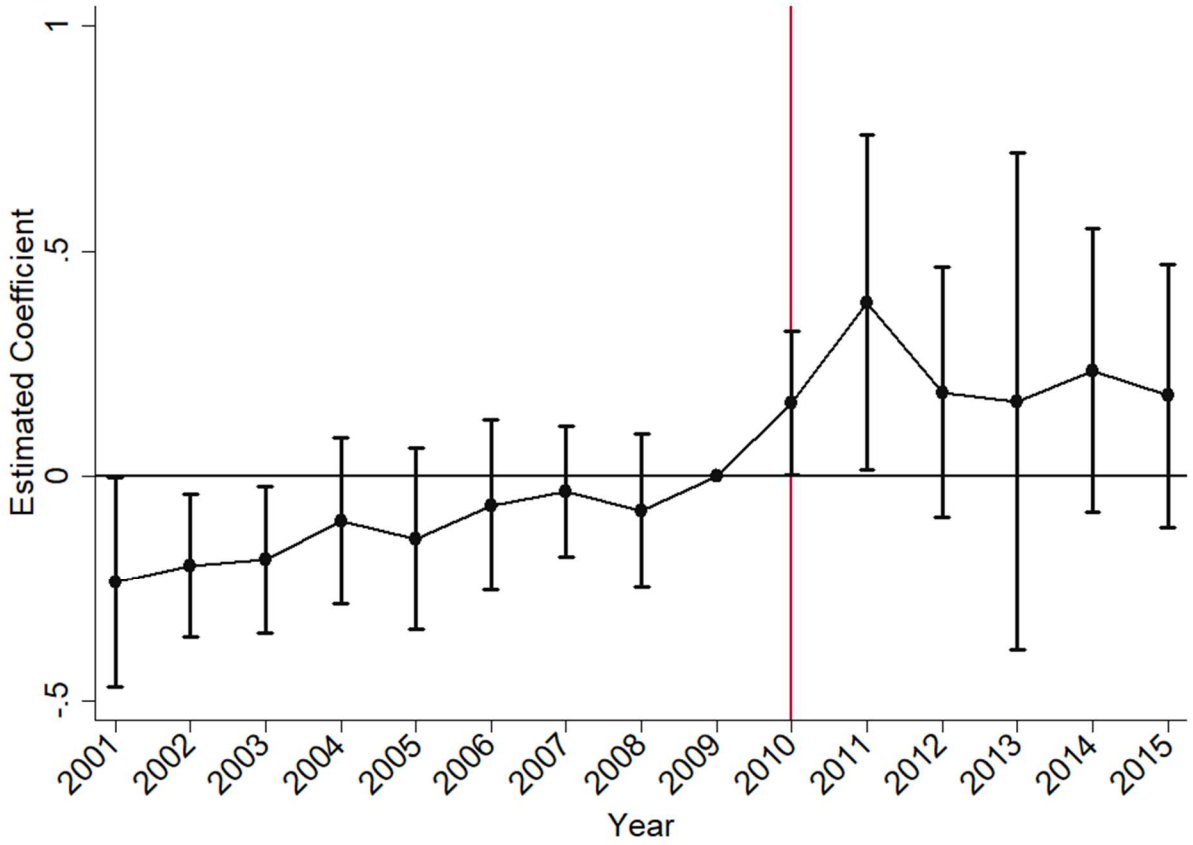
Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is the log of the % eligible adults applying for disability in the calendar year. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



Pre-Period F-Statistic for 2006-2008 = 1.975
Post-Period F-Statistic for 2011-2015 = 3.970***

Appendix Figure A.7: Non-Medical OxyContin Misuse Event Study Estimates for the Percent Eligible Adults Applying for Disability – No Additional Covariates

Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is the % eligible adults applying for disability in the calendar year. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West). We also jointly estimate effects for pain reliever misuse interacted with year indicators.



Pre-Period F-Statistic for 2006-2008 = 1.120
Post-Period F-Statistic for 2011-2015 = 4.434***

Appendix Figure A.8: Non-Medical OxyContin Misuse Event Study Estimates for the Percent Eligible Adults Applying for Disability – West Only

Notes: 95% confidence intervals adjusted for state-level clustering. Outcome is % eligible adults applying for disability in the calendar year. The estimates reported in the figure are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and region-time fixed effects (where region is East or West) as well as time-varying controls: the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators. We select on states in the West for this analysis.

Appendix Table 1: Difference-in-Differences Estimates for CPS Outcomes (Ages 25-54)

Panel A: Share Working

Outcome:	Share Working (ages 25-54)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	-0.003	0.002	0.000	0.006	0.003	0.004	0.009	0.009
	(0.008)	(0.010)	(0.010)	(0.009)	(0.008)	(0.009)	(0.009)	(0.013)
Pain Reliever Misuse x Post		-0.002	-0.002	-0.002	-0.001	-0.003	-0.007**	-0.004
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Panel B: Not in the Labor Force

Outcome:	Share Not in Labor Force (ages 25-54)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	0.009	0.001	0.001	-0.001	-0.001	0.002	-0.005	0.020
	(0.006)	(0.008)	(0.007)	(0.006)	(0.006)	(0.005)	(0.004)	(0.033)
Pain Reliever Misuse x Post		0.004**	0.004***	0.004***	0.003***	0.003**	0.005***	0.012*
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.006)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Notes: Standard errors in parentheses adjusted for clustering at the state-level. State fixed effects and region-time fixed effects (where region is East or West) included in all specifications. Post is equal to 1 for years 2011+. Time-varying controls are the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries.

***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.

Appendix Table 2: Difference-in-Differences Estimates for CPS Outcomes (Ages 18-24)

Panel A: Share Working

Outcome:	Share Working (ages 18-24)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	0.011	0.015	0.015	0.025	0.026	0.026	0.059**	0.058
	(0.023)	(0.029)	(0.028)	(0.025)	(0.019)	(0.024)	(0.025)	(0.049)
Pain Reliever Misuse x Post		-0.002	-0.001	-0.003	-0.002	-0.006	-0.010*	-0.007
		(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.009)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-201	2007-2015	2007-2015	2007-2015	2001-201	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Panel B: Not in the Labor Force

Outcome:	Share Not in Labor Force (ages 18-24)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OxyContin Misuse x Post	-0.008	-0.008	-0.007	-0.01	-0.01	-0.012	-0.045*	0.017
	(0.021)	(0.026)	(0.026)	(0.024)	(0.021)	(0.020)	(0.024)	(0.064)
Pain Reliever Misuse x Post		0	-0.001	0	0	0.002	0.007	-0.007
		(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.012)
State and Time-Varying Covariates	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy Variables	No	No	No	Yes	Yes	Yes	Yes	Yes
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2001-2015	2007-2015	2007-2015
Including 2010?	No	No	No	No	Yes	No	No	No
States	All	All	All	All	All	All	West Only	All
Outcome	Level	Level	Level	Level	Level	Level	Level	Logged
N	408	408	408	408	459	714	200	408

Notes: Standard errors in parentheses adjusted for clustering at the state-level. State fixed effects and region-time fixed effects (where region is East or West) included in all specifications. Post is equal to 1 for years 2011+. Time-varying controls are the fraction white, share of the population ages 25-44, fraction with a college education, and policy variables. Our policy variables are whether the state has a PDMP, a "must access" PDMP, pain clinic regulations, a medical marijuana law, and legal and operational medical marijuana dispensaries.

***Significant at the 1% level; **Significant at the 5% level; *Significant at the 10% level.