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IS THE RISE IN ILLICIT OPIOIDS AFFECTING LABOR SUPPLY AND DISABILITY  
CLAIMING RATES?

Sujeong Park  
David Powell

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

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**ABSTRACT**

There is considerable interest in understanding the broader effects of the opioid crisis on labor supply and social insurance programs in the United States. This paper examines how the recent transition of the opioid crisis from prescription opioids to more prevalent misuse of illicit opioids, such as heroin and fentanyl, altered labor supply behavior and disability insurance claiming rates. We exploit differential geographic exposure to the reformulation of OxyContin, the largest reduction in access to abusable prescription opioids to date, to study the effects of substitution to illicit markets. We observe meaningful reductions in labor supply measured in terms of employment-to-population ratios, hours worked, and earnings. We also find significant increases in disability applications and beneficiaries. These labor supply and disability insurance shifts begin immediately after reformulation and are uniquely associated with pre-reformulation rates of OxyContin misuse, not rates of broader pain reliever misuse.

Sujeong Park  
RAND  
1776 Main St.  
Santa Monica, CA 90407  
sujeongp@rand.org

David Powell  
RAND Corporation  
1776 Main Street  
P.O. Box 2138  
Santa Monica, CA 90407  
dpowell@rand.org

# 1. Introduction

The opioid crisis in the United States is a national emergency. In 2017 alone, more than 70,000 individuals died of drug overdoses; almost 70% involved opioids (Scholl et al., 2019). There is widespread interest in understanding the broader effects of the opioid crisis beyond overdoses. In particular, policymakers and researchers have expressed concern about its economic consequences and its implications for labor markets and social insurance programs.<sup>1</sup> There is suggestive evidence that the link between the crisis and the labor market may be especially strong, and the Federal Reserve Chairman Jerome Powell recently claimed that the opioid crisis is having a “substantial” effect on the United States economy.<sup>2</sup>

Complicating efforts to quantify the broader effects of the opioid crisis, the epidemic continues to evolve. Before 2010, the “first wave” was driven primarily by misuse of natural and semi-synthetic opioids, such as OxyContin. In 2010, a pivotal transformation produced a second wave, a heroin epidemic, which transitioned in 2013 into the third wave—an illicit fentanyl crisis. These waves can be observed in fatal overdose trends by opioid type, which are presented in Figure 1. Recent research suggests that the transformation from prescription to illicitly-manufactured 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 prescription opioids as OxyContin was often the “drug of choice” for non-medical users (Cicero et al., 2005). Prior research has shown that states with higher rates of non-medical use of OxyContin experienced disproportionate growth in heroin overdose rates after reformulation (Alpert et al., 2018). In recent years, widespread substitution to illicit opioids has only increased in importance (Pardo et al., 2019), leading to disproportionately fast overdose rate growth in states more exposed to reformulation (Powell and Pacula, forthcoming).

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<sup>1</sup> 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

<sup>2</sup> <https://www.cnbc.com/2019/07/10/jerome-powell-says-economic-impact-of-opioid-crisis-is-substantial.html>, last accessed January 12, 2020.

A small, but growing, literature empirically analyzes how prescription opioid availability and overprescribing affect the labor market. Focusing on the overprescribing dimension, Harris et al. (2019) find that areas with more high-volume prescribers have lower labor force participation rates. Krueger (2017) and Aliprantis et al. (2019) suggest 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, comparing labor supply changes in areas with faster growth in opioid supply to those with lower growth. Currie et al. (2019) and Savych et al. (2019) also study geographic variation in measures of prescribing behavior over time. These papers rely on geographic-specific changes or cross-sectional variation in opioid prescribing or access, assuming that such variation occurs for reasons unrelated to labor market conditions. It is rare in this literature to leverage policy-driven variation. A recent exception is Beheshti (2019), who exploits the differential geographic impacts of the rescheduling of hydrocodone, finding convincing evidence that reduced medical access to hydrocodone improved labor force participation rates.

To date, the small literature on the labor supply impacts of the opioid crisis measures opioid exposure in terms of geographic prescribing rates and legal opioid supply. In contrast, this paper examines the labor supply and disability insurance effects of the transition from prescription opioids to illicit opioids, drugs that are not measured in prescriptions. This perspective is perhaps more relevant to the current state of the epidemic, potentially helping us to understand how labor supply effects may evolve as heroin and illicit fentanyl continue to flood many local drug markets. More broadly, it is rare to study the labor supply and social insurance consequences of a large shock to the size of illicit drug markets.<sup>3</sup>

We analyze how the reformulation of OxyContin affected labor market outcomes and applications for disability benefits. As the supply of abusable prescriptions opioids decreased, people switched to illicit markets and these markets grew disproportionately in areas where OxyContin misuse had been more prevalent. Given evidence that OxyContin reformulation transformed the opioid crisis, it is important to understand how that transformation affected disability insurance programs and the labor market. Estimates of the productivity costs of illicit drug use in the literature are large. Jiang et al. (2017) conclude that heroin use disorder alone in the United States results in over \$5 billion of productivity losses annually. In addition, a host of papers

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<sup>3</sup> In fact, it is rare to observe exogenous shocks to the size of illicit drug markets and study the ramifications on any outcomes. See Jacobson (2004) for an important exception.

have explored the association between personal drug use and labor supply outcomes (e.g., Kaestner, 1994; Zarkin et al., 1998; MacDonald and Pudney, 2000; French et al., 2001; DeSimone, 2002) or used policy variation to identify this relationship (e.g., Nicholas and Maclean, 2019; Sabia and Nguyen, 2018). We explore the direction and magnitude of effects from a broad market-wide shift from prescription opioids to illicit opioids.

There is also general interest, but limited empirical evidence, in understanding the broader effects of supply-side interventions designed to deter opioid misuse. A small literature has documented possible labor supply effects of prescription drug monitoring programs (PDMPs) (Kilby, 2015; Franco et al., 2019; Deiana and Giua, 2018). This paper also intersects with the literature studying the labor supply consequences of access to prescription drugs. Prior research found evidence that access to Cox-2 inhibitors increased labor force participation in the United States (Garthwaite, 2012) and decreased sickness absences in Norway (Bütikofer and Skira, 2018). In our context, we study the removal of an abusable formulation of a pain management therapy when more potent and dangerous pharmacological substitutes are available in illicit markets.

We use several complementary data sets to document labor supply outcomes including data from the Bureau of Economic Analysis (BEA), Current Employment Statistics (CES), American Community Survey (ACS), and Current Population Survey (CPS) to construct measures of labor supply such as the employment rate, hours worked, and earnings. We also rely on administrative data from the Social Security Administration (SSA) concerning applications and allowances for disability benefits through Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). We requested and were provided administrative data on allowances, benchmarked to the year of the initial application. We study these disability-related measures for two reasons. First, disability applications represent a dimension of labor supply not necessarily captured by other employment outcomes. Misuse of medical and illicit opioids may put people at risk of permanently reducing labor supply, making disability insurance applications and allowances especially useful proxies for longer-term labor force attachment in this context.

Second, there is significant policy interest in what drives increases in Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) applications and enrollment, especially when these mechanisms are not more traditional factors predicting disability enrollment

such as demographic shifts. At \$143 billion, SSDI represents 4% of the federal budget.<sup>4</sup> 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; Maestas et al., 2018). However, there is little work exploring how the growth of illicit drug markets affects enrollment in disability insurance programs.

At an individual level, substitution from OxyContin to heroin/fentanyl could affect an individual's ability to function in the labor force given heroin's potency and its associated health risks (e.g., due to higher rates of 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 risk of victimization. Heroin use – relative to prescription opioid misuse – could also increase the propensity to fail drug tests, hurting employment prospects. On the other hand, if reformulation induced some to stop misusing opioids entirely,<sup>5</sup> then we may observe beneficial outcomes on non-mortality dimensions such as labor supply.

At the market level, a broad transition from prescriptions opioids to heroin may cause illicit drug markets to expand beyond individuals previously misusing OxyContin. Powell and Pacula (forthcoming) find evidence consistent with substantial expansion. Drug market expansion could alter labor markets with general equilibrium ramifications on employment, induce crime, and systematically change population health. In the end, it is difficult to predict the effects of such a dramatic shift from prescription to illicit drugs since there is little evidence about what such an enormous transition will do to individual- and market-level behaviors.

We study changes in labor supply and disability outcomes in states with higher rates of OxyContin misuse before OxyContin reformulation relative to those with lower rates. Reformulation impacted the supply of abusable opioids nationally, but we can exploit that states with higher rates of OxyContin misuse were more exposed to the effects of reformulation, comparing more exposed to less exposed states. Our primary empirical strategy transparently traces the relationship between OxyContin misuse and these outcomes in each year, conditioning on state

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

<sup>5</sup> Cicero and Ellis (2015) suggest that this rate is low using results from a small survey, but this survey is for a very selected sample (153 participants enrolled in substance use treatment centers), which may not generalize to the broader population. It also does not capture reductions in initiation since all participants in the survey had prior OxyContin misuse.

and time fixed effects, while also accounting for the independent effects of pre-reformulation rates of pain reliever misuse more generally.

We observe large effects on labor supply and disability claiming. We estimate reductions in employment-to-population ratios in multiple data sets, beginning at the time of reformulation and growing over time. Additionally, we estimate even larger proportional reductions in hours worked, earnings, and total employee compensation, suggesting important “intensive margin” effects as well.<sup>6</sup> Our results also suggest that reformulation induced more people to apply for disability benefits, and many of these individuals met the SSA’s disability criteria and eventually received disability benefits. We estimate increases in disability applications, favorable determinations, and total beneficiaries. These relationships are unique to OxyContin misuse -- 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 (or even level differences). The evidence is consistent with reformulation as the driving mechanism and not other policies or confounding factors resulting from the Great Recession (see Section 5.6).

Our results imply that a state with a one standard deviation higher rate of non-medical OxyContin use prior to reformulation experienced a 7% relative increase in disability applications after reformulation. A back-of-the-envelope calculation suggests that the labor supply impacts of growth in illicit opioid markets are comparable to, but on the lower end of the distribution of estimated effects of, the consequences of legal opioid access increases found in the literature.

Although substance use disorders alone cannot be used as qualifying conditions for disability benefits, these results suggest that growth in illicit drug markets alters the labor market capabilities of those with other disabling conditions and/or worsens economic conditions for this population. The literature has provided evidence that economic conditions can drive application rates and alter the size of disability insurance rolls. This paper demonstrates that general equilibrium shocks to illicit drug markets can also affect demand for disability benefits.

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<sup>6</sup> These metrics encompass both intensive and extensive margin decisions, but we find larger responses on these dimensions than extensive-only metrics. The combinations of these results suggest meaningful intensive labor supply responses.

We discuss these interactions further in the next section while also providing background on the reformulation of OxyContin. We describe the data used in the analysis in Section 3, and our empirical strategy in Section 4. We present our results in Section 5 and discuss our conclusions 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 federal programs providing monetary and non-monetary support – such as health insurance – 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.<sup>7</sup> SSDI is generally available to individuals with a sufficient work history while SSI is intended primarily for individuals with little work experience and is subject to asset thresholds.<sup>8</sup>

When it began 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, with the introduction in 1997 of public law 104-121, substance use disorder beneficiaries became disqualified unless they also qualified under other disability requirements (Gresenz et al., 1998).<sup>9</sup> Since substance use does not *disqualify* individuals from receiving benefits, it may increase application rates if the substance use exacerbates other disabilities or reduces labor market opportunities. Brucker (2007) estimates that a “substantial portion” of SSI/SSDI beneficiaries struggle with substance abuse, at rates much higher than the general population for drugs, suggesting considerable scope for large transitions in substance use to affect claiming rates. “Substance dependence is a frequently present complicating factor” in Social Security determination decisions (Noblitt and Noblitt, 2012).

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<sup>7</sup> See Chart 12: [https://www.ssa.gov/policy/docs/statcomps/di\\_asr/2017/di\\_asr17.pdf](https://www.ssa.gov/policy/docs/statcomps/di_asr/2017/di_asr17.pdf), last accessed June 2, 2019

<sup>8</sup> Children are also eligible for SSI and evaluated based on medically determinable physical and mental impairments.

<sup>9</sup> Moore (2015) studied the long-term effects of disqualification for those affected by this reform.



The SSA outlines several ways in which substance use may appropriately lead to higher allowance rates.<sup>10</sup> For example, substance use may lead to a claimant acquiring a disabling impairment (e.g., contracting HIV through needle use) or it could cause permanent impairments (the SSA provides dementia and amnesic disorders as examples of such conditions), which qualify claimants for disability benefits. Alternatively, there is substantial heterogeneity in the judgment of disability examiners (Maestas et al., 2013) and substance use may add variability to this decision process, potentially altering allowance rates.

Similarly, shocks to the prevalence of substance abuse could change disability claiming rates for these same reasons or because of increased demand (e.g., due to poorer individual labor market prospects or broader labor market conditions) to reduce labor supply. It is difficult to find exogenous shocks to illicit drug use so existing evidence is limited, and it is difficult to forecast these effects. One important exception in the literature is Maclean et al. (2019), who find that state medical marijuana laws, which increase access to marijuana, increase disability insurance claiming rates.<sup>11</sup>

To apply for SSDI, an individual initially files an application at a Social Security office. The office screens applications to determine if an individual 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 state Disability Determination Services to determine whether the individual is disabled under Social Security guidelines and unable to work. Denials can be appealed. Individuals who are approved 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.

Disability benefit receipt and labor supply are not mutually exclusive. While receiving disability benefits reduces earnings (Maestas et al., 2013; French and Song, 2014), many recipients still work. SSDI recipients are restricted to earn less than “substantial gainful activity” (SGA), equal

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<sup>10</sup> See SSA’s “Policy Interpretation Ruling” for cases involving substance use, found here: [https://www.ssa.gov/OP\\_Home/rulings/di/01/SSR2013-02-di-01.html](https://www.ssa.gov/OP_Home/rulings/di/01/SSR2013-02-di-01.html), last accessed July 6, 2020.

<sup>11</sup> Also relevant, Ghimire and Maclean (2020) conclude that medical marijuana laws reduce workers’ compensation claiming.

to \$1,260 per month (\$2,110 for blind applicants), though there are exceptions to this rule and earning above this threshold is permitted for certain periods of time.

## *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 compared to previous drugs. However, crushing or dissolving the pill caused the complete dose of oxycodone to be delivered immediately, making OxyContin especially easy to abuse. Individuals could chew, snort, or inject the crushed pill for maximum euphoric effects. This type of abuse is arguably the most dangerous, as this high level of potency heightens risk for addiction and overdose death.<sup>12</sup>

OxyContin had more than \$3 billion in sale in 2010, making it one of the highest selling drugs in the United States (Bartholow, 2011). It was also one of the leading drugs of abuse (Cicero et al., 2005). Many experts have implicated OxyContin as a key driver of the opioid epidemic (e.g., Kolodny et al., 2015) and recent work concludes that its introduction explains a significant share of the growth in overdoses since 1996 (Alpert et al., 2019).

In April 2010, Purdue Pharma introduced a reformulated version of OxyContin 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. The change increases the costs of misusing OxyContin while maintaining the medical benefits of the drug. The reformulated version can still be abused orally (i.e., taking higher doses than prescribed) and some users have even found ways to counteract the abuse-deterrent properties of the new version.<sup>13</sup> However, reformulation has been shown to decrease abuse rates. In August 2010, Purdue Pharma stopped distributing the original formulation of OxyContin to pharmacies.

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<sup>12</sup> 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 statement that OxyContin had a lower potential for abuse. This claim was central to the marketing campaign (Van Zee, 2009).

<sup>13</sup> Cicero and Ellis (2015) noted that the significant time effort required should deter use of these methods.

Recent research has shown that replacing the original formulation with the abuse-deterrent version increased heroin overdose rates (Alpert et al., 2018; Evans et al., 2019), heroin-specific substance abuse treatment admissions (Alpert et al., 2018), and rates of infectious diseases (Powell et al., 2019; Beheshti, 2020).<sup>14</sup> Recent work suggests that the longer-term effects of OxyContin reformulation were especially important. Instead of just shifting the exposed population from prescription opioid overdoses to illicit opioid overdoses, Powell and Pacula (forthcoming) find that reformulation, over a longer time horizon, drastically increased overdose rates. While initially there was possibly just a shift to illicit markets, this shift caused those markets to grow and innovate over time. Powell and Pacula (forthcoming) tie reformulation to the rise in synthetic opioid overdose deaths as well as deaths involving non-opioids, such as cocaine.

The relationship between exposure to reformulation and overdoses has continued to increase over time. It is unlikely, given the evidence, that we are observing the initial set of people misusing OxyContin prior to 2010 gradually but increasingly dying of overdoses through 2017. That is part of the story – the introduction of fentanyl would increase the death rate among this group. However, it is also likely that illicit markets expanded, suggesting illicit drug exposure and overdose rate growth among new entrants.

While it is difficult to observe the number of consumers in illicit drug markets,<sup>15</sup> Powell and Pacula (forthcoming) find that exposure to reformulation is associated with increases in substance use treatment admissions for heroin (and opioids more broadly). This relationship is even stronger for those without any prior treatment episodes, suggesting an increase in initiation into dependence even in recent years. In addition, this work ties reformulation to growth in non-opioid overdose deaths, such as cocaine, which is also consistent with exposure to potent illicit opioids among a new population. Overall, the evidence suggests substantial market expansion.

Similarly, in this paper, we are not isolating the labor supply effects of reformulation on those misusing OxyContin prior to reformulation. Instead, we study geographic effects, which include the expansion of illicit markets and its broader economic consequences. These general equilibrium effects should be much larger than individual-level effects.

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<sup>14</sup> Evans et al. (2020) find that reformulation increased rates of child abuse and neglect.

<sup>15</sup> Caulkins et al. (2015) find that the NSDUH severely and uniquely undercounts people using heroin.

### 3. Data

We conduct our analyses at the state level given that all of our data sources are available at this level. While state boundaries likely do not appropriately define prescription or illicit drug markets, performing the analysis at the state-level should not induce bias. If we had more granular data, we would be able to exploit additional variation in exposure to reformulation, likely increasing power. However, aggregating to the state level is not problematic except for the loss of power, which is reflected in our standard errors. While we have individual-level data for some outcomes, it is more appropriate to aggregate to the state level since our treatment variables vary at that level (Bertrand et al., 2004). We select on 2001-2015 since all data sources are available for these years. We generally ignore the possibility of selective mortality given that the changes in overdose rates are orders of magnitude smaller than the estimated labor and disability outcome changes, but we discuss this issue in-depth in Section 5.6.

#### *3.1 Labor Outcomes*

We study several complementary measures of labor supply. First, we rely on data from the Bureau of Economic Analysis (BEA).<sup>16</sup> The BEA provides state-level annual employment figures using data from the Current Employment Statistics (CES), discussed below, while adding information on employment in industries excluded from the CES (e.g., agriculture). The BEA collects information from Internal Revenue Service data to estimate sole proprietorships and nonfarm partners such that the final employment numbers include the self-employed. Consequently, the BEA employment totals are much larger than those provided by the CES.

We will also use the BEA's measure of employee compensation. Total employee compensation provides an additional measure of labor which nests a host of individual and firm-level decisions such as hours worked and occupational choice (wages). The BEA compensation metric includes wages and salaries as well as the value of noncash benefits (e.g., employer contributions to health insurance and pension plans).<sup>17</sup>

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<sup>16</sup> BEA data are often used in economic analyses (e.g., Acemoglu et al., 2016; Gittell et al., 2017; Maestas et al., 2016; McCully, 2014).

<sup>17</sup> See <https://www.bea.gov/system/files/2019-12/Chapter-10.pdf> (last accessed February 28, 2020) for details.

We also study employment rates constructed using the CES data. The CES is a survey of establishments representing workers covered by unemployment insurance. Each month, the CES surveys about 145,000 nonfarm businesses and government agencies. By the nature of the survey design, the CES excludes some industries, such as agriculture, and the self-employed.

Both the BEA and CES are designed to provide employment totals based on place of work, not place of residence. For our purposes, it is not obvious whether we should prefer place of work or place of residence employment figures so we rely on multiple measures to provide complementary evidence. We scale these employment figures by the total resident population ages 16 and above with the understanding that people may reside in one state and work in another. For both the BEA and CES, it is more accurate to refer to their employment numbers as the number of jobs since a person may have jobs at multiple establishments. However, we will generally refer to these variables as “employment rates” throughout this paper.

We also construct labor supply measures from the American Community Survey (ACS), an annual household survey. The ACS is much larger than the Current Population Survey (CPS), though both include similar labor supply variables. The ACS is self-reported, but we are able to construct labor outcomes by state of *residence* and study some additional measures, such as usual hours worked per week.<sup>18,19</sup> Usual hours per week is equal to zero for non-workers and, thus, incorporates both intensive and extensive labor supply behavior.

We also study earnings in the ACS, which we define as pre-tax wage and salary income plus self-employment income from businesses and farms.<sup>20</sup> We rely primarily on the annual labor outcomes in the ACS, though we will also show some results referring to weekly labor supply. The annual labor outcomes refer to the previous 12 months so we use the 2002-2016 samples to construct labor supply metrics for 2001-2015.<sup>21</sup> Weekly outcomes refer to the same year as the year of the sample so we use the 2001-2015 samples for these outcomes. We will also study comparable weekly labor outcomes in the (monthly) CPS.

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<sup>18</sup> The CES includes data on hours worked (for all employees) since 2007, which does not provide enough of a pre-period relative to the ACS.

<sup>19</sup> The weeks worked variable changes during our time period and only provides broad categories so we do not study “total hours worked for the year.”

<sup>20</sup> The earnings variables in the ACS are topcoded. We use the values provided in the data. The BEA compensation measure does not involve any topcoding, which is another advantage of the BEA data.

<sup>21</sup> This creates some misalignment.

The ACS permit us to select on the 18-64 population for some analyses, which will be important for comparison purposes given that the disability outcomes are specific to this population. Overall, there are complementary benefits to each of these measures. In general, we do not find that our conclusions depend on the use of any specific data set.

### *3.2 Disability Insurance Measures*

To construct measures of disability claiming behavior, we use Social Security Administration (SSA) Fiscal Year Disability Claim Data, focusing on the adult population, defined as ages 18-64. These variables are available for fiscal years 2001-2015 at the state and annual level. We study the number of adult initial claims, which we will often refer to as applications for disability benefits though some applications are deemed ineligible in an initial screening and are never sent to a state agency. These applications are excluded from this measure. We divide the number of applications by the 18-64 population size. The data do not distinguish between SSI and SSDI benefits, but the majority of applicants in this age group are applying for SSDI. We study applicants because applying for disability benefits reflects demand to reduce labor supply. Note that the ACS and other surveys do not record disability insurance applications.

We are also interested in whether these applicants eventually qualify for benefits. Public data provide the number of allowances in that year, regardless of when the initial applications were filed. Disability determinations often occur in a very short timeframe, but they can potentially take years. In such cases, some “post-reformulation” allowances would refer to applications filed prior to reformulation.<sup>22</sup> By request, the SSA calculated and provided us with the “Total Allowances” (i.e., number of people approved for SSI or SSDI benefits) by state and year of application using data from the Disability Research File Reporting Service Cubes. Each allowance (i.e., new beneficiary) is benchmarked to the filing year. Benchmarking to the initial year of the application is beneficial because we are interested in the downstream consequences on disability insurance enrollment for the changes in application rates that we observe.

The number of total allowances reflects people granted benefits whether their applications were initially approved or after one or more appeals. According to conversations with the SSA, the number of total allowances could only be provided by *calendar* filing year. Because the number of

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<sup>22</sup> Autor et al. (2015) discuss the implications of long wait times in the disability insurance system.

applications are provided at the fiscal year (which ends in October) and the number of total allowances is provided at the calendar year, there is a slight misalignment when these measures are used together.

For the more recent years of data, some of the applications are still pending. We calculate that 5.8% of 2015 applications are still pending. This rate decreases to 1.7% for 2014 and less than 1% for all prior years. We will discuss the effect of these pending cases when presenting the results for this outcome.

We will also directly study the percentage of *applicants* approved for benefits, providing some evidence about the changing health of the applicants. The applicant pool may become less healthy because of the transition to illicit opioids, but the marginal applicants may also potentially be healthier on average. The net effect is an empirical question.

It is possible that state agencies alter their determination decisions in response to the changing environment. We cannot rule out this possibility, but the agents making these decisions have no 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 empirically rule out systematic reviewer-side behavioral changes.

Finally, we will study the fraction of the 18-64 population receiving disability benefits (as of December in each year), also provided in the SSA Fiscal Year Disability Claim Data. We highlight that changes in beneficiaries may reflect applications over, potentially, many years. Autor et al. (2015) report that for initial determinations, the median time to a decision is only three months. Thus, we would potentially expect to observe some evidence of immediate effects if application rates quickly respond to reformulation given that most of these applications will receive decisions before the end of the year. However, there is additional scope for lagged effects for this outcome since cases which are initially denied may take years upon appeal before receiving a favorable determination.<sup>23</sup>

We also study the number of beneficiaries by diagnostic group. We collected these data from the Annual Statistical Reports on the Social Security Insurance Program (see Table 10 of those

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<sup>23</sup> Autor et al. (2015) report that 10% of cases do not receive a final decision for over 37 months.

reports). These measures help provide some evidence about which conditions are affected by reformulation in terms of disability enrollment.

### *3.3. Nonmedical OxyContin and Pain Reliever Use*

To measure non-medical use of OxyContin and pain relievers, we use state-level data from the NSDUH, a nationally representative household survey of individuals ages 12 and older and the country's largest annual survey collecting information on substance use. The survey provides information on "non-medical OxyContin use" within the past year beginning in 2004 as well as "non-medical pain reliever use." 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 advantage of this measure is that it specifies both "nonmedical use" and "OxyContin." Substitution to illicit opioids should depend on the interaction of these two properties since OxyContin reformulation was specific to OxyContin and did not affect the medical capabilities of the drug.

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."<sup>24</sup> Given the sensitive nature of pain reliever misuse, NSDUH provides respondents with a private and confidential way to respond to questions in an effort to increase honest reporting.<sup>25</sup> Nevertheless, self-reported data on drug use are subject to some under-reporting error. When constructing the non-medical use variables, we combine the 2004-2009 surveys to reduce measurement error. 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.

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<sup>24</sup> 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."

<sup>25</sup> 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.



### 3.4. Summary Statistics

Figure 2 shows the time series trends in both the percentage of the 16+ population working and the percentage of the 18-64 population applying for disability benefits. There are large changes in both of these measures during the Great Recession, followed by partial convergence to pre-recession values by 2015.

In Table 1, we provide 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 before reformulation. Similarly, we observe little difference in pre-reformulation labor or disability outcomes based on OxyContin misuse. On almost all labor and disability-related outcomes, the two groups of states have nearly-identical rates before 2010 (the observed differences are not statistically significant for any measure). All dollar amounts in this paper are reported in 2015 dollars.

Our analysis uses an event study framework, discussed in the next section. Event studies are designed to help test for pre-existing trends; notably, we also have similar pre-existing levels for our outcomes. This property is an important feature for difference-in-differences designs according to recent work (Kahn-Lang and Lang, 2019) and reduces concerns about mean reversion as a potential driver of the estimated effects.

## 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

$$(1) \quad Y_{st} = \alpha_s + \gamma_t + \delta_t \times OxyRate_s^{Pre} + \theta_t \times PainRelieverRate_s^{Pre} + X'_{st}\phi + \varepsilon_{st},$$

where  $Y_{st}$  is a 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 fixed pain reliever misuse rate in state  $s$  in the pre-

reformulation period.<sup>26</sup> As shown in equation (1), the effects of both misuse variables are permitted to vary by year.

We include the pain reliever misuse variables to account for outcome changes related to pain reliever use more generally, isolating shifts that are unique to OxyContin misuse. We also include a set of time-varying controls: the percentage white and non-Hispanic (from the Surveillance, Epidemiology, and End Results Program, SEER), percentage Hispanic (SEER), five age shares (18-24, 25-34, 35-44, 45-54, 55-64; SEER),<sup>27</sup> four education shares (ACS),<sup>28</sup> percentage foreign born (ACS), and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries.<sup>29</sup> In Section 5.6, we will also implement lasso (with a broader set of controls) to systematize covariate selection. The results are similar using this approach.

The specification includes state and time fixed effects to account for fixed differences across states and national trends in labor outcomes. The assumption is that states with high rates of non-medical OxyContin use would have experienced similar outcome trends as states with low rates of non-medical OxyContin use in the absence of reformulation. This model permits us to trace the trajectory of the relationship between OxyContin misuse and the outcomes over time which offers some evidence about the appropriateness of the parallel trends assumption. We study pre-treatment trends while also studying the timing of any effect given the possibility of lagged effects in this context. We plot the  $\delta_t$  estimates with 95% confidence intervals, adjusted for state-level clustering.

Our main disability outcome – disability insurance applications – is provided at the fiscal year, which ends in October. The removal of the original formulation of OxyContin occurred in August. The introduction of the abuse-deterrent version, which also affected access to the original formulation, occurred in April. Thus, there is a possibility of observing partial effects in 2010.<sup>30</sup> The

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<sup>26</sup> 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.

<sup>27</sup> These demographic shares are measured as a percentage of the 18-64 population.

<sup>28</sup> No high school degree, high school degree (but no college experience), some college, and college (4 year) degree.

<sup>29</sup> PDMP and pain clinic information comes from Prescription Drug Abuse Policy Surveillance. Medical marijuana variables are provided by the RAND Marijuana Policy database (see Powell et al., 2018; Williams et al., 2019).

<sup>30</sup> It is possible that the removal of the original formulation had rather large immediate effects as people with dependence issues struggled to adapt.

other outcomes, including other disability insurance outcomes, are calculated by calendar year so there is a longer “treated” period in 2010, though we still expect a muted effect in this year relative to subsequent years.

To summarize our results, we will often present the average post-reformulation effect, which we define as starting in the first full year post-reformulation:

$$\frac{1}{5} \sum_{s=2011}^{2015} \hat{\delta}_s. \quad (2)$$

The appropriateness of the above metric can be observed (partially) by the event study results. We will also present the same metric for the pain reliever misuse variable (i.e.,  $\frac{1}{5} \sum_{s=2011}^{2015} \hat{\theta}_s$ ).

We will estimate equation (1) using the log of the employment rate or other outcome. We will also present results in which we use Poisson regression since it relaxes some of the assumptions implicit in a log-linear regression (see Santos-Silva and Tenreiro, 2006 for details) and does not impose additional assumptions on the relationship between the mean and the variance of the outcome like negative binomial regression and related estimators would.<sup>31</sup> When using Poisson regression, the outcome is total employment (or total applications) and we use population (for the relevant age group) as the exposure variable. In general, the magnitudes of the effects are much stronger when Poisson estimation is used. We also provide OLS estimates in which the outcome is expressed in levels, not logs.

Regressions are weighted by the size of the 16+ population unless the outcome is defined for a different age range. In those cases, we weight by the size of the state population in that age range. We adjust standard errors for clustering at the state level.

## 5. Results

### 5.1. *OxyContin Misuse and Growth of Illicit Opioid Markets*

In Appendix Figures 2 and 3, we show that reformulation triggered larger reductions in OxyContin misuse in high OxyContin misuse states. Appendix Figure 2 divides states into quartiles 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 present estimates from an event

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<sup>31</sup> Ciani and Fisher (2018) discuss the appropriateness of Poisson estimation in a difference-in-differences framework.

study with OxyContin misuse by 2-year wave as the outcome,<sup>32</sup> similar to a “first stage” relationship for the available data. We estimate that the 2012-2013 estimate is smaller and statistically different from the pre-reformulation estimates. These results show that the states with higher rates of non-medical OxyContin use before reformulation were most impacted by reformulation and experienced the sharpest declines in OxyContin misuse.

The states more exposed to reformulation also experienced larger increases in illicit opioid overdoses (Powell and Pacula, forthcoming). We show some of this evidence here. We estimate equation (1) for heroin overdoses per 100,000 people and present the event study estimates in Appendix Figure 4, Panel A. We observe a sharp rise in the relationship between pre-reformulation OxyContin misuse and heroin overdoses beginning with a partial effect in calendar year 2010 followed by more dramatic growth in subsequent years. We present the estimates for synthetic opioids in Panel B. For synthetic opioids, there is evidence of a delayed effect as illicit markets took time to grow and innovate. Illicitly-manufactured fentanyl was part of this innovation, and we estimate especially large effects in 2014-2015.

This evidence suggests that the illicit markets grew over the course of our time period and disproportionately in high OxyContin misuse states. Powell and Pacula (forthcoming) also find evidence that reformulation increased dependence among those without any prior substance abuse treatment episodes (even in recent years) as well as cocaine overdose deaths, further suggesting that illicit markets expanded faster in states more exposed to reformulation and beyond what we would expect if we were only observing substitution to illicit markets from the initial set of people misusing OxyContin prior to reformulation. We now proceed to study the labor supply consequences of this broad shift to illicit markets.

## *5.2 Labor Supply*

We consider the effect of reformulation on a wide range of labor supply metrics. We initially focus on the employment rate using BEA data. We flexibly evaluate the temporal relationship between OxyContin misuse and the log of the percentage of the population working. These estimates are presented in Figure 3, Panel A. Prior to reformulation, there is little evidence of any

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<sup>32</sup> 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 estimating 2-year effects) would be preferable even with more granular data.

systematic pre-existing trends. From 2008 to 2010, we observe an increase in the estimates. We will observe this type of slight increase across several outcomes. To the extent that this trend would have continued, many of our estimates will therefore be conservative.

Beginning in the first full year of reformulation, after three years of gradual increases, the estimates begin to decrease (the 2011 estimate is smaller than the 2010 estimate, though not statistically different) and continue to decrease through the end of the sample period. The 2013 and 2014 estimates are each statistically significant from zero at the 5% level. By 2015, we estimate a reduction of 4.9% for each additional percentage point of pre-reformulation OxyContin misuse (statistically significant at the 10% level). Since a one percentage point difference in OxyContin misuse is very large (equivalent to almost twice the national average), we will often report the effect of a one standard deviation differences in OxyContin misuse, equal to 0.23. Thus, each standard deviation increase in exposure to reformulation predicts an additional 1.1% reduction in employment. Evaluating at the pre-reformulation average, this reduction implies a relative employment rate decrease of 0.8 percentage points.

The downward trend begins around the time of reformulation, suggesting that the effects are not driven by confounding factors related to the Great Recession. Also, while we noted the slight increase prior to reformulation, the downward post-reformulation effect is unlikely to be mean reversion since the reduction far exceeds relative increases or decreases observed at any other point in the sample.

Next, we study per capita (ages 16+) employee compensation as a measure of labor supply. This metric includes both extensive labor supply responses (since non-workers do not receive any compensation) and intensive labor responses such as additional hours worked. These results are presented in Panel B. We observe a similar pattern as before with evidence that this trend begins in the partially-treated year of 2010 (the 2010 estimate itself is not statistically different from zero, however). Again, while there are periods of increasing trends and periods of decreasing trends prior to reformulation, the downward trend observed after reformulation is uniquely steep relative to any of these pre-period movements. Also notably, the post-reformulation reductions are much larger in magnitude than the estimated employment declines. The size of the employee compensation results – which nest both extensive and intensive margin responses (fewer hours worked, lower wages, etc.) – relative to the employment effects along are consistent with important intensive margin effects.

By 2015, the estimates imply that each standard deviation of OxyContin misuse predicts 2.6% reductions in employee compensation.

To summarize these results, we present aggregated estimates in Table 2, Column 1. In Column 2, we add region-year interactions (where region is defined as the four Census regions). The estimates for employment (Panel A) and compensation (Panel B) increase in magnitude when we account for differential secular trends across the country. In Column 3, we estimate an exponential model using Poisson regression. The estimates increase in magnitude using this approach. In Column 4, we use OLS but specify the outcome in levels instead of logs. The results generally imply similar proportional effects as those in Column 1.

As a complementary measure, we study per capita (ages 16+) employment using the CES. Despite the absence of some industries and the self-employed in the CES data, the pattern of estimates in Figure 3, Panel C is generally similar to the pattern in Panel A. The magnitudes are comparable as well; we present average effects in Table 2, Panel C. Finally, we study the percentage of people working in the ACS in Panel D of Figure 3. There is some evidence of a slight upward trend throughout the pre-period. This trend abruptly reverses in the first full year of reformulation. By the latter part of the post-period, we observe large and statistically significant reductions. The magnitudes are smaller for the self-reported ACS outcome than the previous outcomes, as shown in Table 2. However, overall, all four metrics provide similar evidence.

In Appendix Figure 5, we replicate Figure 3 but express the outcomes in levels instead of logs. The event studies are similar. Appendix Figure 6 presents the equivalent Poisson event studies. Again, the results are similar. In Appendix Figure 7, we replicate Figure 3 but the outcome is the log of total employment. Using total employment avoids committing to a scaling variable since the BEA and CES outcomes are not calculated for specific age groups (though we select the ACS outcome on the 16+ population). The estimates are generally similar (though, as we would expect, noisier) when studying total employment instead of employment rates.

In Appendix Figure 8, we repeat the main analyses for usual hours worked (equal to 0 for non-workers) and annual earnings using ACS data (equal to 0 for non-workers). For both outcomes, we observe declines beginning right at reformulation. The estimated decline in annual earnings compares to the decline estimated earlier for total employee compensation. The corresponding average effects are presented in Table 3. The results imply that each standard

deviation increase in exposure to reformulation predicts an additional 0.7% decrease in usual hours worked and 1.2% decrease in earnings (using the Column 1 estimates).

For most of this paper, we consider the relationships between the broader pain reliever misuse variable and the outcomes as nuisance parameters which help us to isolate the differential effect associated with OxyContin.<sup>33</sup> However, it is worth noting that in Tables 2 and 3 (and in many of the results of this paper) that we find that pre-reformulation level of pain reliever misuse predicts changes in labor and disability outcomes in the opposite direction of the nonmedical OxyContin use variable. This finding is consistent with analysis in Alpert et al. (2018), which concludes that states with higher levels of pain reliever misuse were systematically addressing opioid access and its downstream consequences around this time period.

We also examine *weekly* ACS outcomes in Appendix Figure 9. We study the percentage working last week, the percentage in the labor force, and the percentage employed last week (though they did not necessarily work). Weekly measures reflect a different margin of labor supply than annual measures. We observe declines for all three measures. Interestingly, the effect appears delayed by about a year relative to the annual measures<sup>34</sup> above but are generally supportive of the earlier findings. The labor force participation estimates decline for most of the post-period, but the magnitudes are smaller than those for other outcomes, suggesting that some of employment loss is not resulting in people leaving the labor force entirely. Note that disability insurance receipt, studied below, does not require labor force exit.

We study similar measures in the CPS in Appendix Figure 10. Specifically, we examine the percentage who worked in the previous week and hours worked in the previous week (equal to 0 if the person did not work). The findings are generally consistent with the ACS results. However, there is some evidence of a relative *increase* in labor supply in 2011. This increase would be consistent with a transitory benefit to reformulation due to reduced access to abusable opioids. We are hesitant to push this conclusion given that we only observe it in the CPS measures (and the CPS 2011 estimates are small and we cannot statistically reject that they are equal to zero). After 2011,

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<sup>33</sup> This relationship reflects a complicated system of policies and changing prescribing cultures over time which are disproportionately targeted to high misuse areas. It is difficult to interpret these estimates as reflecting the impacts of any specific policy or change.

<sup>34</sup> This delayed effect could potentially reflect that those switching to not working for the year were already not working each week so there is less scope to observe immediate effects on weekly labor supply compared to annual labor supply measures.

the estimates decline rather sharply (compared to the 2011 estimate) in a manner comparable to the findings from other data sets.

The labor supply measures analyzed in this section represent the labor market for a broad cross section of the population. We will next analyze our disability insurance variables. While interesting in themselves due to the social insurance implications, these measures also reflect a potentially relevant and different dimension of the labor force given that disability insurance enrollment often signals a near-permanent reduction in labor supply. Because the disability insurance measures refer to the 18-64 population, it is informative to study labor supply metrics specific to this population. We use the ACS to select on this age range and replicate our main event studies. We present these results in Appendix Figure 11. We generally observe similar evidence of labor declines on various measures when focusing on the 18-64 population.<sup>35</sup>

### *5.3 Disability Insurance*

While we find evidence of reductions in the fraction of people working due to reformulation, disability insurance outcomes represent a different dimension of labor supply. Disability benefits typically reflect a permanent reduction in labor supply while annual metrics of labor supply may indicate more transitory behavior.

Figure 4 shows event study estimates for the log of the percentage of the 18-64 population applying for disability benefits in Panel A. The estimates are relatively flat prior to reformulation – in the three years prior to reformulation, there is a slight and statistically insignificant decrease (from 0.017 to 0), consistent with the slight increases for most labor supply outcomes observed in the previous section. 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 increase further in 2014, the beginnings of the fentanyl crisis. Overall, we can statistically reject that the post-reformulation estimates are equal to the 2009 baseline. In 2014-2015, we estimate that each standard deviation increase in the misuse rate leads to

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<sup>35</sup> Appendix Figure 11 is equivalent to event studies shown in Figure 3 (Panel D) and Appendix Figure 8 but selected on this age range.



a 7% increase in applications, equivalent to an increase of about 0.08 percentage points (given a pre-reformulation mean of 1.2 percentage points).

Table 4 presents average effects for all outcomes studied in this section. We estimate a 24% increase (per percentage point of OxyContin misuse) over the full post-period for the application rate. This estimate decreases in magnitude when region-time interactions are included in the model but increases in magnitude when Poisson estimation is used. OLS estimation when the outcome is expressed in levels (not logs) produces similar implied proportional increases. The estimated effects suggest that the substitution to illicit markets has led to a meaningful increase in disability applicants.

As a complementary measure, we study the log of the share of the 18-64 population that eventually received an allowance for disability benefits. This metric provides additional information about whether the SSA state agency or appeals process determined that the person met the disability criteria and evidence on the downstream effects of changes in application rates. Using administrative data, allowances are benchmarked to the calendar year in which the person applied.

Figure 4, Panel B presents the event study results. As before, we observe little evidence of pre-existing trends, followed by increases beginning in 2010.<sup>36</sup> Jointly, the 2011-2015 estimates are statistically significant from zero at the 5% level. The estimates decline in the last couple of years, potentially reflecting the increasing frequency of undecided cases for these years, a fraction of which may still receive favorable determinations in the future.<sup>37</sup>

The post-reformulation estimates and implied level effects are smaller than the estimates in Panel A, suggesting that only a fraction of the new applicants due to reformulation were found to meet the SSA disability criteria.<sup>38</sup> In 2014-2015, the estimates imply that a one standard deviation higher nonmedical OxyContin use predicts an increase in favorable determinations by 5.3-5.7% (equivalent to an increase of about 0.03 percentage points). However, we can statistically rule out that *none* of the new applicants met the SSA disability criteria. The increase in allowances suggests

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<sup>36</sup> The 2010 increase here is larger than the 2010 increase observed in Panel A relative to the other post-reformulation estimates. This is likely because years are defined as calendar years in Panel B so more of 2010 is “treated.”

<sup>37</sup> As noted above, the rate of undetermined applications is low in our sample period so this factor likely only partially explains the slight decline in 2014-2015.

<sup>38</sup> Since we are estimating proportional effects, the smaller estimates combined with the smaller base for this outcome suggest smaller level effects.

that reformulation may have induced people with other disabling conditions to apply for disability insurance or it may have qualified some workers for benefits for reasons discussed in Section 2.1.

To better understand the Panel A and Panel B results, we study the fraction of adult *applicants* who eventually were approved for disability benefits.<sup>39</sup> This statistic is benchmarked to the year of application. One possibility is that reformulation increased application rates, but these new 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 were discussed above.

We present the event study estimates in Panel C. We do not observe a systematic relationship after reformulation for this metric. The results suggest that reformulation increased disability application rates with little meaningful change in the determination rate conditional on applying. Together, these factors generated an increase in the share of the eligible population receiving benefits.

Finally, we study the percentage of the 18-64 population receiving disability benefits (in December of each year). This outcome is not benchmarked to the year of application but, instead, reflects the downstream ramifications of the increased rate of applications and favorable determinations on the overall percentage of people enrolled in disability insurance. Panel D presents the event study estimates. We observe little evidence of any pre-existing trends followed by a gradual increase over time after reformulation.<sup>40</sup> The estimates imply that a one standard deviation higher rate of OxyContin misuse led to 2% higher rate of beneficiaries in 2015 (equivalent to a 0.1 percentage point increase of the 18-64 population receiving benefits).

#### 5.4 *Disability Diagnostic Groups*

Using data from the Annual Statistical Report on the Social Security Disability Insurance Program, we are able to study beneficiaries by diagnostic group to provide some evidence about which conditions are increasing in prevalence among the disability insurance population. We note

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<sup>39</sup> We construct a calendar year imputation of the number of applicants using a weighted average of the fiscal year numbers.

<sup>40</sup> There is some evidence of an immediate jump in 2010, which would be consistent with reformulation inducing a sudden and drastic shock to people misusing OxyContin, leading them to immediately apply for benefits. Given the average time to a decision (discussed earlier), many of these applicants would be beneficiaries by December. However, we note that the 2010 estimates in Figure 4 are (generally) not statistically significant from zero and imply very small effects.

an important distinction between this analysis and the previous results. The data listing the number of beneficiaries by diagnostic group include widow(er)s and adult children. Workers still make up the vast majority of the beneficiaries in the data (87% in 2015) so the population should be comparable to those in the earlier analyses. As with the previous (Figure 4, Panel D) analysis, beneficiaries are not benchmarked to year of application so some caution with interpretation is warranted. All outcomes refer to the values in December of that year.

We present results for four selected diagnostic groups due to their size and importance to the disability insurance system in the United States. It is difficult to hypothesize what types of diagnostic groups would be more or less affected given the possible channels through which illicit market growth can alter disability claiming behavior. For example, if illicit drug market growth leads to deteriorating economic opportunities, then all diagnostic groups are potentially impacted. However, these impacts may vary depending on how responsive applications for different groups are to economic opportunities. Prior research has found substantial variation in such responsiveness (Maestas et al., 2018).

We first present results for mental disorders (e.g., mood disorders, schizophrenic and other psychotic disorders). In aggregate, mental disorders were the most prevalent diagnostic group in 2015. Adults with mental health conditions are substantially more likely to receive opioid prescriptions (Davis et al., 2017) and misuse opioids (Feingold et al., 2018). We present the results in Figure 5A. We find some evidence of a downward trend in the earlier years of the sample, though it flattens for 2007-2009. We then observe a differential increase in beneficiaries with mental health diagnoses beginning after 2011. This relationship steadily increases through the end of the sample, though the total increase is relatively modest.

We also study beneficiaries with musculoskeletal-related diagnoses (the second largest category) in Panel B. Musculoskeletal conditions are often singled out as primary drivers of the rise of disability receipt (e.g., Liebman, 2015). Musculoskeletal conditions include spinal and soft tissue injuries, which are often treated with opioids for pain management. The relationship with nonmedical OxyContin use begins after reformulation and grows over time.

Next, we examine diseases of the nervous system, the third most common diagnostic category in 2015. Opioid misuse is associated with the development of short- and long-term neurological diseases (Finsterer and Stöllberger, 2016). We present the results in Panel C. The

pattern of estimates are similar to those observed for musculoskeletal conditions, though the magnitudes are smaller.

Finally, we study neoplasms (cancer). While cancer patients are prescribed opioids at high rates, we hypothesize that there is less scope for opioid use and growth in illicit opioids markets to affect disability claiming behavior for this diagnostic group. Our results are shown in Panel D. There is less evidence of growth in beneficiaries associated with reformulation here until the very end of the sample. Some growth could reflect responsiveness to overall economic conditions resulting from illicit market growth. However, the much smaller estimates are suggestive that some diagnostic categories were less affected than other and, generally, we find that reformulation seems to disproportionately affect categories that we might expect to be most affected.

## 5.5 *Discussion*

Our analysis explores many possible challenges to isolating the differential impact of reformulation while also presenting event study estimates to observe pre-reformulation trends based on exposure to reformulation. We do not observe meaningful changes in 2007-2009 for our labor supply and disability measures, despite the large national shifts in these metrics during this time period (as shown earlier in Figure 2). Thus, the timing of the effects is more consistent with reformulation than confounding factors related to the Great Recession (both the economic shocks of the Great Recession themselves and the subsequent economic policy adoption in response to the recession), suggesting that pre-reformulation OxyContin misuse did not predict differential “exposure” to the Great Recession. The next section provides additional evidence that underlying economic conditions are not confounding the results.

Instead, we generally observe small effects in 2010 followed by much larger effects beginning in 2011. This timing is consistent with the differential rise in heroin overdoses found in Section 5.1. We observe labor supply and disability insurance effects beginning rather soon after reformulation, implying that reformulation induced a quick but persistent and growing response. The timing also suggests that we are not observing the long-term effects of OxyContin misuse itself. OxyContin misuse rates are highly persistent at the state-level (Alpert et al., 2018). High misuse

states in 2008-2009 had high rates of misuse in 2004-2005 and likely before. It is unlikely that this misuse generated no independent effects for 5+ years, followed by sudden large effects in 2010.<sup>41</sup>

We leveraged information in several data sets to evaluate the labor supply consequences of the growth in illicit opioid markets. We find large effects in each data set which suggests that the results are not driven by specific characteristics of any particular data set such as relying on employment numbers benchmarked to the place of work (BEA) instead of place of residence (ACS) or using the number of jobs instead of number of employed people.

Our results are consistent with the broader transition to illicit markets worsening health and encouraging disability claiming for a segment of the population. Changes in disability claiming and employment are correlated outcomes but not mutually exclusive. For example, disability claiming could potentially increase among a population that would not have otherwise worked during that same time period.<sup>42</sup> However, our labor supply estimates imply employment reductions about ten times as large as the increase in disability applications (in percentage point terms).

Moreover, we estimated even larger proportional reductions in labor supply metrics which nest both extensive and intensive margin responses, such as hours worked and earnings/compensation, suggesting a role for large intensive margin effects. Maestas et al. (2013) find that disability benefit receipt reduces annual earnings and the propensity to earn above the SGA. It is possible that our labor supply reductions are partially *caused* by the increase in the disability insurance enrollment, but the relative magnitudes of our labor supply and disability insurance results suggest that this is not the primary driving mechanism.

We estimate that a one standard deviation increase in pre-reformulation OxyContin misuse is associated with a post-reformulation rise in disability applications of 7%. Evaluated at the pre-reformulation mean, this effect implies an additional eight adults per 10,000 applied for disability insurance because of the additional exposure to reformulation. Maestas et al. (2018) study the relationship between the unemployment rate and SSDI claiming rates during the Great Recession, concluding that each percentage point rise in the unemployment rate leads to a 3.3% increase in

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<sup>41</sup> Further note that if OxyContin misuse did generate this odd pattern independent of reformulation, then we would expect to see similar effects related to pain reliever misuse. We do not observe such effects, even attenuated ones. Instead, these estimates are generally the opposite sign.

<sup>42</sup> Mueller et al. (2016) find that most new SSDI recipients between 2005 and 2013 did not work in the year prior to receiving benefits.

claims. Thus, a standard deviation higher rate of exposure to reformulation led to disability claiming increases on the order of a 1.8 percentage point increase in the unemployment rate during the Great Recession.

The labor supply estimates in this paper are large and are consistent with reformulation leading to substantial illicit drug market growth, including spillovers into non-opioid markets. Similar to the conclusions of Powell and Pacula (forthcoming), the results here suggest that we are not simply observing the individual-level effects of some people misusing OxyContin and shifting into illicit markets beginning in 2010. Instead, the results suggest broader general equilibrium effects and growth in illicit drug markets affecting local economic conditions.

While the literature typically studies changes in *legal* opioid supply, it is useful to try to benchmark the estimates here with those found in the literature. Beheshti (2019) concludes that a 10% reduction in hydrocodone prescriptions leads to a 0.2 percentage point increase in labor force participation.<sup>43</sup> Krueger (2017) estimates that a 10% increase in opioid prescriptions decreases labor force participation by 0.11-0.14 percentage points while Aliprantis et al. (2019) find stronger relationships between 0.15 and 0.47 percentage points (depending on specification and sample).

For a comparable calculation, we focus on the rise in illicit opioids since the start of the fentanyl crisis. Our results suggest that each 10% increase in exposure to reformulation (and the accompanying rise in illicit drug markets) decreased employment-to-population ratios by 0.18 percentage points.<sup>44</sup> Note that a focus on the full 2011-2015 period would lead to more modest effect sizes as would using labor force participation rates instead of employment rates. Thus, we are generally finding results closer to the bottom of the distribution of estimates in the literature studying the effects of legal opioid supply differences.

## 5.6 Sensitivity Analyses

Our main results suggest that disability insurance applications and enrollment differentially increased in states with higher pre-reformulation rates of OxyContin misuse while labor supply

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<sup>43</sup> On page 19, Beheshti (2019) reports that a 22% decrease (one standard deviation) in hydrocodone prescriptions leads to a 0.44 percentage point increase in the labor force participation rate.

<sup>44</sup> For this calculation, we use the mean employment rate of 74.5% and that a 10% increase in OxyContin misuse is equal to 0.057 percentage points. The average employment effect for 2013-2015 is -0.043 (statistically significant at the 5% level).

decreased. These shifts happens exactly when we would expect if reformulation were the initiating force, and there is little evidence of confounding pre-trends.

We also find that these post-reformulation trends are uniquely associated with OxyContin misuse. Most policies targeted to address opioid prescribing or misuse affect pain relievers broadly. Likewise, underlying confounding predictors would likely be correlated with general pain reliever misuse, not OxyContin specifically. We find that variation in nonmedical pain reliever use typically predicts labor and disability outcome growth in the opposite direction as nonmedical OxyContin use. 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.

The main results are also robust to functional form considerations as the implied effects are similar whether we use a log-linear specification, a linear specification, or an exponential specification. Additionally, the opioid crisis has evolved quite differently in different regions of the country (Abouk et al., 2019). The estimates appear robust to including region-time indicators.

As discussed above, we ignore concerns related to selective mortality effects given the relative sizes of the mortality and disability insurance effects. Similar to the analysis in Section 5.1, if we estimate average effects for overdoses deaths involving heroin or synthetic opioids (ignoring the counteracting reductions in prescription opioid overdose deaths), we estimate that each standard deviation difference in exposure to reformulation predicts an additional 0.05 deaths per 10,000, less than one-hundredth the magnitude of the estimated change in disability insurance applications (which were small relative to the estimated labor supply reductions). Alpert et al. (2018) do not estimate statistically significant growth in overdose deaths by 2013, but we observe changes in disability application rates and labor supply before then, suggesting that mortality is not driving the observed changes in labor supply and disability claiming.

In Appendix Figure 12, we include additional sensitivity tests. We focus on the employment-to-population ratio (using the BEA data) and the disability application rate for these tests. First, we consider the importance of the selected time-varying covariates by systematizing covariate selection. We implement rigorous square root lasso (Belloni et al., 2011, 2012) to select the variables to include in each model,<sup>45</sup> followed by post-estimation OLS (Belloni and Chernozhukov,

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<sup>45</sup> We only penalize the time-varying covariates (including policy variables). We also add more covariates to the possible set of predictors including variables that overlap the covariates used in the main model such as

2013).<sup>46</sup> The event studies are provided in Panels A and B. The results are similar to the corresponding main event studies (Figures 3A and 4A), suggesting that our choice of covariates is not driving the results of this paper.<sup>47</sup>

Next, we consider the role of differential shocks to underlying economic conditions. Betz and Jones (2018) find that overdose death rates respond to labor demand shocks using a Bartik-style instrument (Bartik, 1991; Blanchard and Katz, 1992). We construct a similar variable, predicting the share working by interacting baseline (2001) state-specific industry shares with national (subtracting out each state's own growth) industry-level growth. We present the event studies in Panels C and D of Appendix Figure 12. In addition, Charles et al. (2019) consider the role of the decline in manufacturing employment in the United States and also find a relationship with overdose deaths. We construct a similar Bartik-type instrument for manufacturing employment specifically and control for this measure as well. These results are provided in Panels E and F. The Bartik predictions do not meaningfully affect the results,<sup>48</sup> suggesting that the main results of this paper are not driven by confounding economic shocks.

## 6. Conclusion

There is considerable interest in understanding the ramifications of the ongoing opioid crisis in the United States on labor supply and social insurance programs. This paper provides some of the first quasi-experimental evidence of this relationship with a focus on the recent and ongoing transition of the opioid crisis to illicit markets. Large literatures consider the determinants of labor supply and social insurance enrollment, but there is scant evidence about how widespread substitution from legal to illicit drug markets might affect these outcomes. Although federal social insurance forecasts typically internalize economic conditions and predictable 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. Policymakers and recent studies have suggested that the opioid crisis may have large effects on labor supply outcomes, but that work typically examines the effects of prescribing rates and not growth in illicit opioid markets.

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share white, share black, and share black and non-Hispanic. Including these additional predictors does not affect the results in a meaningful manner.

<sup>46</sup> This method is implemented using `lassopack` in Stata (Ahrens et al., 2019; Ahrens et al., 2020).

<sup>47</sup> The post-reformulation estimates are jointly significant at the 5% level in both Panels A and B.

<sup>48</sup> For example, the average post-reformulation effect reported above for disability applications was 0.235. When both Bartik predictions are included, the estimate is 0.224 (statistically significant at the 1% level).



We study traditional labor supply measures, but we also rely on metrics related to applications and determinations for disability benefits. Labor supply and disability metrics, while important on their own, act as proxies for overall population health, the health of the workforce, and the economy's workforce capabilities. We exploit prior work showing 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). There is limited evidence on the labor and social insurance consequences of such large supply-side interventions designed to curb opioid misuse.

We estimate meaningful labor supply reductions in response to reformulation. We find that states more exposed to reformulation experienced relative declines in the percentage of people working. Moreover, we find even larger labor supply reductions when using more comprehensive measures of labor supply such as hours worked, labor earnings, and employee compensation. We observe these reductions across several data sets.

Disability claiming rates are also of special interest given the importance of understanding drivers of disability applications due to their significance in terms of federal expenditures and because they represent sizable and near-permanent reductions in the size of the labor force. There is considerable demand from policymakers for evidence about the consequences of the opioid crisis on the social safety net, but the literature has rarely studied this relationship.

Our results suggest that the reformulation of OxyContin had strong effects on disability claiming behavior for the 18-64 population. We estimate that a state with a standard deviation higher pre-reformulation rate of non-medical OxyContin use experienced an additional 7% increase in disability applicants and a comparable increase in new beneficiaries after reformulation. The applicant pool did not appear to worsen or improve in health, on average, due to the substitution to illicit opioids. Thus, reformulation served to exacerbate the rise of applicants during the Great Recession and slow the decline since the recovery. While substance use itself is not a valid disabling condition, the evidence suggests that illicit opioid market growth interacted with qualifying disabling conditions or worsened economic conditions in a way that incentivized more disability applications. This evidence is generally consistent with prior work showing that disability insurance programs expand in response to diminishing economic opportunities since poorer labor market opportunities is one possible consequence of an increase in illicit drug use. However, the literature has rarely

provided direct evidence about the effects of exogenous shocks to illicit drug markets on disability insurance demand and enrollment.

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 fatal overdoses. This paper shows that reformulation impacted other important dimensions as well, decreasing labor supply, increasing the rate of applications for disability benefits, and increasing the rate of people found to have disabling conditions. Federal and states policies still often try to address the harms of the opioid crisis by limiting opioid access, and it is critical to recognize the possible unintended consequences of supply-side interventions given the availability of illicit substitutes. 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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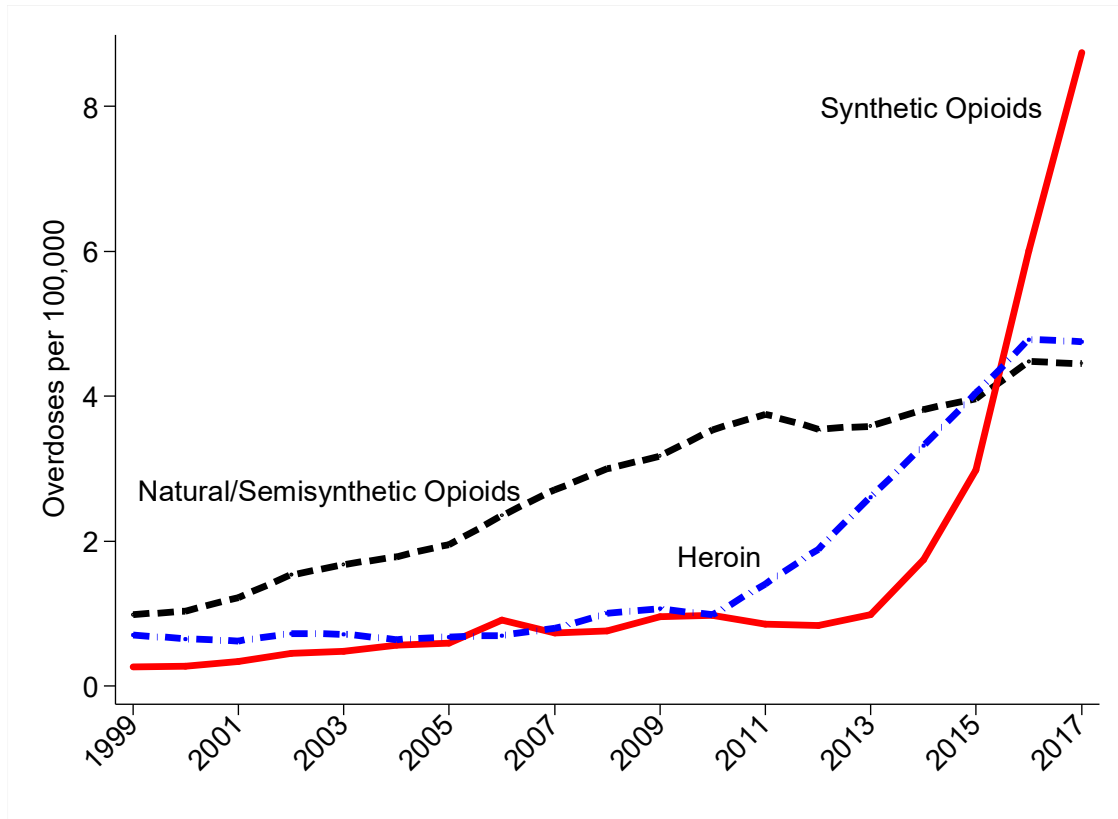
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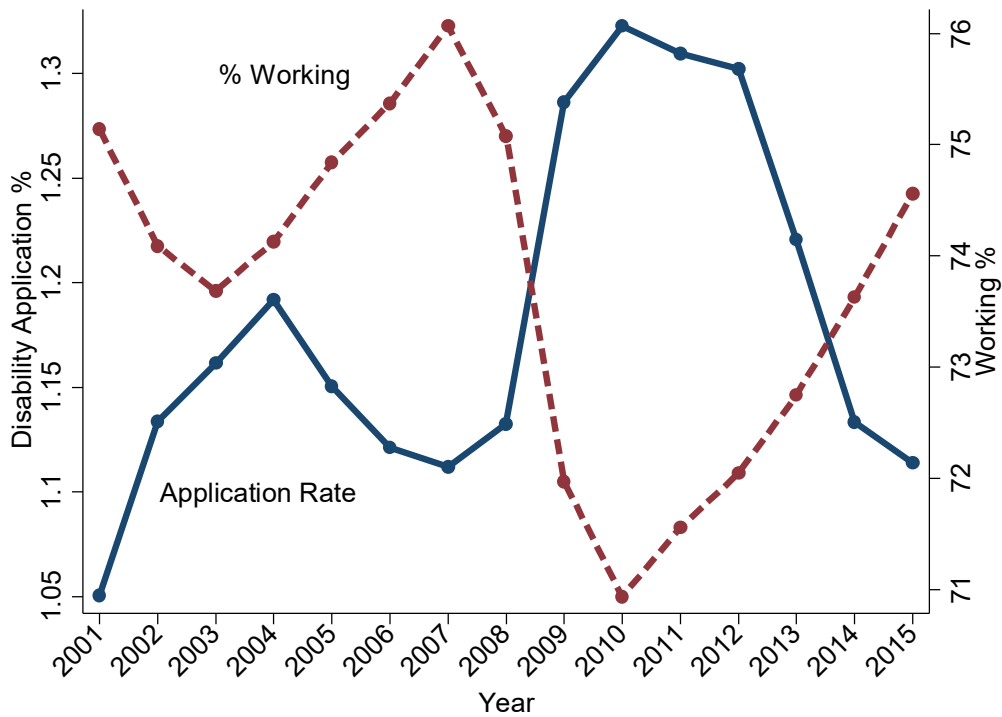
## Figures



**Figure 1: National Fatal Overdose Rate Trends**

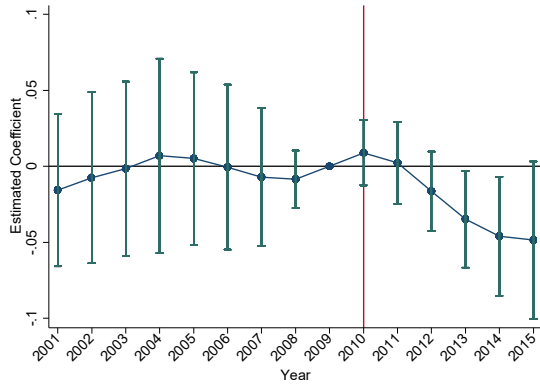
*Notes:* Figure 1 plots national annual fatal overdose trends in natural and semi-synthetic opioids (T40.2), heroin (T40.1), and synthetic opioids (T40.4) per 100,000 people for 1999-2017. These categories are not mutually exclusive and sum to rates higher than the overall opioid overdose rate. OxyContin is a semi-synthetic opioid.

*Source:* National Vital Statistics System

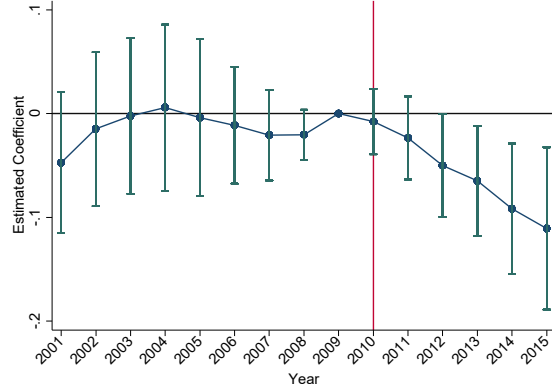


**Figure 2: National Time Series Trends in Percentage of Population Applying for Disability Insurance and Percentage Working**

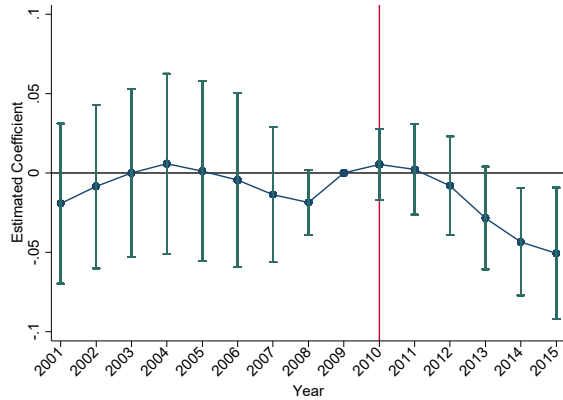
*Source:* SSA Fiscal Year Disability Claims Data and Bureau of Economic Analysis. The percentage of disability applications is scaled by the 18-64 population while percentage working is the number of workers divided by the size of the 16+ population.



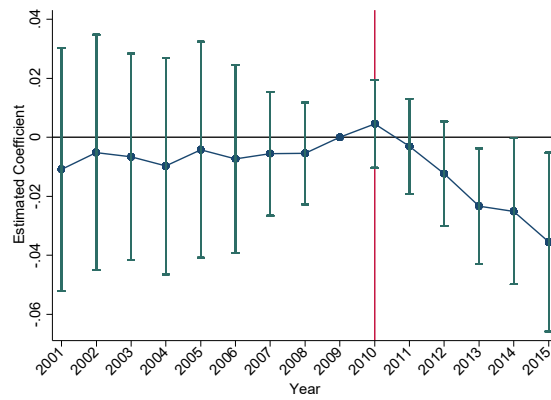
**A. Per Capita Employment (BEA)**



**B. Per Capita Employee Compensation**



**C. Per Capita Employment (CES)**

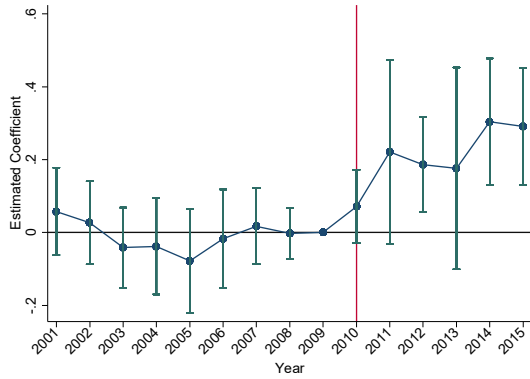


**D. % Worked (ACS)**

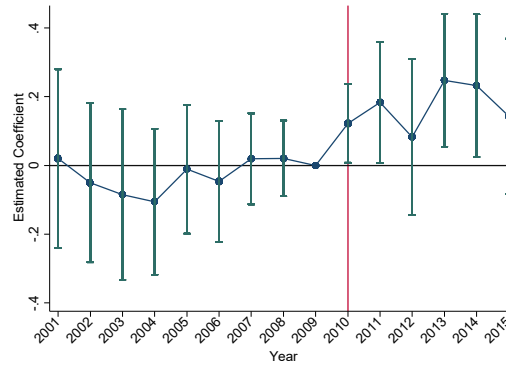
**Figure 3: Non-Medical OxyContin Misuse Event Study Estimates for Labor Supply**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of total employment scaled by the size of the 16+ population (Panel A) and the log of total employee compensation divided by the 16+ population (Panel B). These outcomes are calculated using data from the Bureau of Economic Analysis. The outcome in Panel C is the log of nonfarm employment divided by the 16+ population using CES data. The outcome in Panel D is the log of the percentage of the 16+ population in the ACS reporting that they worked during the year.

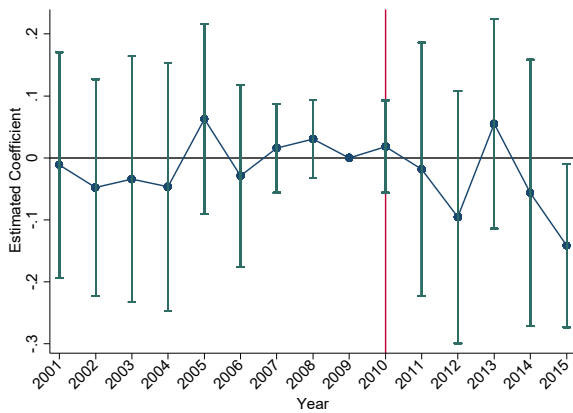
The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



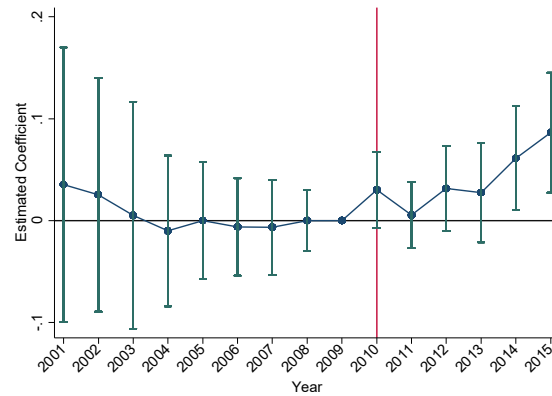
**A. Applications Per Capita**



**B. Favorable Determinations Per Capita**



**C. Favorable Determinations Per Application**

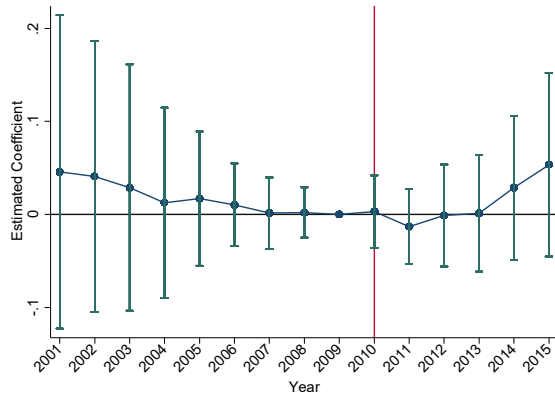


**D. Beneficiaries Per Capita**

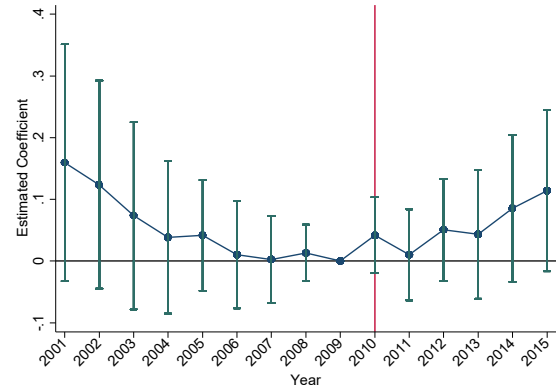
**Figure 4: Non-Medical OxyContin Misuse Event Study Estimates for Disability Outcomes**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of new applications scaled by size of the 18-64 population (Panel A). This outcome is calculated using the SSA Fiscal Year Disability Claim data set. The outcome in Panel B is the log of favorable determinations scaled by the 18-64 population, calculated using data from Disability Research File Reporting Service Cubes. The outcome in Panel C is the log of favorable determinations scaled by the number of applications. The outcome in Panel D is the log of the total number of beneficiaries scaled by the 18-64 population, using data from the SSA Fiscal Year Disability Claim data set.

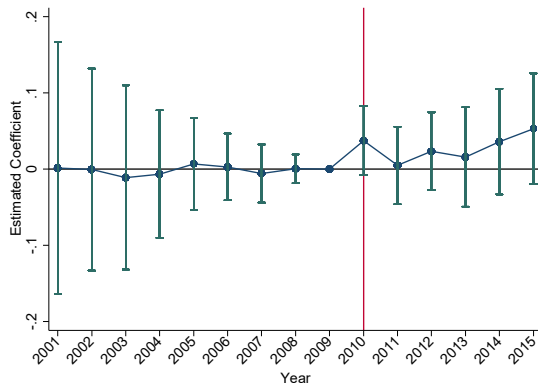
The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



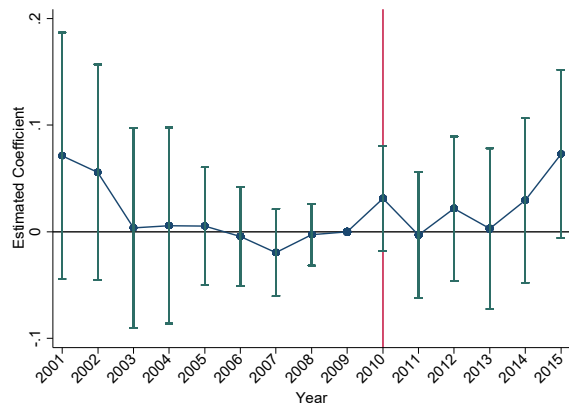
**A. Mental Disorders**



**B. Musculoskeletal System**



**C. Nervous System**



**D. Neoplasms**

**Figure 5: Non-Medical OxyContin Misuse Event Study Estimates for Percent Adults Receiving Disability Benefits for the Listed Diagnostic Condition**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is log of beneficiaries for the listed diagnostic condition scaled by the 18-64 population. These outcomes are calculated using data from the Annual Statistical Report on the Social Security Disability Insurance Program.

The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar 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 time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, 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 for 2004-2009**

| Variable (2004-2009)                  | All States | Below Median OxyContin Misuse States | Above Median OxyContin Misuse States |
|---------------------------------------|------------|--------------------------------------|--------------------------------------|
| <u>BEA Outcomes</u>                   |            |                                      |                                      |
| % Employed                            | 74.5%      | 74.4%                                | 74.8%                                |
| Per Capita Employee Compensation (\$) | 37,144     | 38,270                               | 34,567                               |
| <u>CES Outcome</u>                    |            |                                      |                                      |
| % Employed                            | 57.4%      | 57.3%                                | 57.4%                                |
| <u>ACS Outcomes</u>                   |            |                                      |                                      |
| % Working                             | 69.1%      | 69.2%                                | 69.0%                                |
| Usual Hours Work Per Week             | 26.8       | 26.9                                 | 26.6                                 |
| Annual Earnings (\$)                  | 27,412     | 28,140                               | 25,747                               |
| <u>CPS Outcomes</u>                   |            |                                      |                                      |
| % Working Last Week                   | 59.8%      | 59.8%                                | 59.7%                                |
| Hours Worked Last Week                | 23.24      | 23.26                                | 23.19                                |
| <u>Disability Insurance</u>           |            |                                      |                                      |
| % Apply                               | 1.2%       | 1.1%                                 | 1.2%                                 |
| % Favorable Determinations            | 0.6%       | 0.6%                                 | 0.6%                                 |
| Favorable Determination Rate          | 49.6%      | 49.3%                                | 50.4%                                |
| % Receiving Disability Benefits       | 5.5%       | 5.3%                                 | 6.0%                                 |
| <u>Demographics (%)</u>               |            |                                      |                                      |
| White, Non-Hispanic                   | 67.0%      | 63.0%                                | 76.3%                                |
| Hispanic                              | 14.5%      | 16.3%                                | 10.4%                                |
| Foreign-Born                          | 19.2%      | 21.0%                                | 15.1%                                |
| <u>Misuse</u>                         |            |                                      |                                      |
| OxyContin Misuse Rate (per 100)       | 0.57       | 0.45                                 | 0.84                                 |

*Notes:* All statistics are population-weighted. BEA, CES, ACS, and CPS outcomes scaled by 16+ population. Disability Insurance outcomes scaled by 18-64 population, except for Favorable Determination Rate (which is number of favorable determinations scaled by number of applications). All dollar amounts are expressed in 2015 dollars.

**Table 2: Average Effect Estimates for Aggregate Labor Supply Outcomes**

| <b>Panel A Outcome:</b> |  | <b>Percentage Working (BEA)</b>      |           |           |         |
|-------------------------|--|--------------------------------------|-----------|-----------|---------|
|                         |  | (1)                                  | (2)       | (3)       | (4)     |
| OxyContin Misuse        |  | -0.029*                              | -0.042*** | -0.046*** | -1.951* |
|                         |  | (0.015)                              | (0.015)   | (0.015)   | (1.098) |
| Pain Reliever Misuse    |  | 0.004                                | 0.010**   | 0.004     | 0.200   |
|                         |  | (0.004)                              | (0.005)   | (0.005)   | (0.327) |
| Region-Time Dummies?    |  | No                                   | Yes       | No        | No      |
| OLS/Poisson             |  | OLS                                  | OLS       | Poisson   | OLS     |
| Outcome                 |  | Log                                  | Log       | Total     | Level   |
| <b>Panel B Outcome:</b> |  | <b>Per Capita Compensation (BEA)</b> |           |           |         |
|                         |  | (1)                                  | (2)       | (3)       | (4)     |
| OxyContin Misuse        |  | -0.068**                             | -0.086*** | -0.107*** | -2469** |
|                         |  | (0.027)                              | (0.031)   | (0.025)   | (977)   |
| Pain Reliever Misuse    |  | 0.013*                               | 0.021**   | 0.012*    | 374     |
|                         |  | (0.007)                              | (0.008)   | (0.007)   | (264)   |
| Region-Time Dummies?    |  | No                                   | Yes       | No        | No      |
| OLS/Poisson             |  | OLS                                  | OLS       | Poisson   | OLS     |
| Outcome                 |  | Log                                  | Log       | Total     | Level   |
| <b>Panel C Outcome:</b> |  | <b>Percentage Working (CES)</b>      |           |           |         |
|                         |  | (1)                                  | (2)       | (3)       | (4)     |
| OxyContin Misuse        |  | -0.026*                              | -0.039**  | -0.040*** | -1.179  |
|                         |  | (0.015)                              | (0.017)   | (0.013)   | (0.851) |
| Pain Reliever Misuse    |  | 0.006                                | 0.012**   | 0.004     | 0.264   |
|                         |  | (0.005)                              | (0.006)   | (0.005)   | (0.262) |
| Region-Time Dummies?    |  | No                                   | Yes       | No        | No      |
| OLS/Poisson             |  | OLS                                  | OLS       | Poisson   | OLS     |
| Outcome                 |  | Log                                  | Log       | Total     | Level   |
| <b>Panel D Outcome:</b> |  | <b>Percentage Working (ACS)</b>      |           |           |         |
|                         |  | (1)                                  | (2)       | (3)       | (4)     |
| OxyContin Misuse        |  | -0.02*                               | -0.020*   | -0.027*** | -1.201* |
|                         |  | (0.010)                              | (0.011)   | (0.008)   | (0.695) |
| Pain Reliever Misuse    |  | 0.003                                | 0.004     | 0.004     | 0.181   |
|                         |  | (0.002)                              | (0.004)   | (0.003)   | (0.165) |
| Region-Time Dummies?    |  | No                                   | Yes       | No        | No      |
| OLS/Poisson             |  | OLS                                  | OLS       | Poisson   | OLS     |
| Outcome                 |  | Log                                  | Log       | Total     | Level   |

Notes: \*\*\*1% significance, \*\*5% significance, \*10% significance. All models include state and time fixed effects plus covariates mentioned in notes of Figure 3. Estimates presented are average effects for each pre-reformulation misuse variable using equation (2). Region refers to the 4 Census regions. When Poisson estimation is used, the outcome is the total and population is used as an exposure variable. N=765.

**Table 3: Average Effect Estimates for ACS Labor Supply Outcomes**

| Outcome:             | Usual Hours Worked  |                     |                      |                     |
|----------------------|---------------------|---------------------|----------------------|---------------------|
|                      | (1)                 | (2)                 | (3)                  | (4)                 |
| OxyContin Misuse     | -0.029**<br>(0.012) | -0.029**<br>(0.013) | -0.040***<br>(0.009) | -0.662**<br>(0.300) |
| Pain Reliever Misuse | 0.007**<br>(0.003)  | 0.008*<br>(0.004)   | 0.007**<br>(0.003)   | 0.136*<br>(0.072)   |
| Region-Time Dummies? | No                  | Yes                 | No                   | No                  |
| OLS/Poisson          | OLS                 | OLS                 | Poisson              | OLS                 |
| Outcome              | Log                 | Log                 | Total                | Level               |

| Outcome:             | Earnings            |                     |                      |                  |
|----------------------|---------------------|---------------------|----------------------|------------------|
|                      | (1)                 | (2)                 | (3)                  | (4)              |
| OxyContin Misuse     | -0.051**<br>(0.020) | -0.056**<br>(0.023) | -0.069***<br>(0.016) | -1819**<br>(682) |
| Pain Reliever Misuse | 0.01*<br>(0.005)    | 0.014*<br>(0.007)   | 0.009*<br>(0.005)    | 169<br>(158)     |
| Region-Time Dummies? | No                  | Yes                 | No                   | No               |
| OLS/Poisson          | OLS                 | OLS                 | Poisson              | OLS              |
| Outcome              | Log                 | Log                 | Total                | Level            |

Notes: \*\*\*1% significance, \*\*5% significance, \*10% significance. All models include state and time fixed effects plus covariates mentioned in notes of Figure 3. Estimates presented are average effects for each pre-reformulation misuse variable using equation (2). Region refers to the 4 Census regions. When Poisson estimation is used, the outcome is the total and population is used as an exposure variable. N=765. Outcomes are calculated from ACS and refer to hours and earnings in the past 12 months.



**Table 4: Average Effect Estimates for Disability Insurance Outcomes**

| <b>Panel A Outcome:</b> | <b>% Apply for Disability</b> |                    |                      |                      |
|-------------------------|-------------------------------|--------------------|----------------------|----------------------|
|                         | (1)                           | (2)                | (3)                  | (4)                  |
| OxyContin Misuse        | 0.236***<br>(0.078)           | 0.145**<br>(0.072) | 0.319***<br>(0.070)  | 0.237***<br>(0.080)  |
| Pain Reliever Misuse    | -0.053***<br>(0.018)          | -0.021<br>(0.017)  | -0.078***<br>(0.017) | -0.058***<br>(0.019) |
| Region-Time Dummies?    | No                            | Yes                | No                   | No                   |
| OLS/Poisson             | OLS                           | OLS                | Poisson              | OLS                  |
| Outcome                 | Log                           | Log                | Total                | Level                |

| <b>Panel B Outcome:</b> | <b>% Favorable Determinations</b> |                    |                      |                     |
|-------------------------|-----------------------------------|--------------------|----------------------|---------------------|
|                         | (1)                               | (2)                | (3)                  | (4)                 |
| OxyContin Misuse        | 0.178**<br>(0.086)                | 0.118<br>(0.098)   | 0.244***<br>(0.077)  | 0.033<br>(0.054)    |
| Pain Reliever Misuse    | -0.059***<br>(0.017)              | -0.037*<br>(0.020) | -0.057***<br>(0.017) | -0.031**<br>(0.012) |
| Region-Time Dummies?    | No                                | Yes                | No                   | No                  |
| OLS/Poisson             | OLS                               | OLS                | Poisson              | OLS                 |
| Outcome                 | Log                               | Log                | Total                | Level               |

| <b>Panel C Outcome:</b> | <b>Favorable Determinations / Applications</b> |                   |                   |                   |
|-------------------------|--|-------------------|-------------------|-------------------|
|                         | (1)  | (2)               | (3)               | (4)               |
| OxyContin Misuse        | -0.052<br>(0.084)                              | -0.032<br>(0.075) | -0.052<br>(0.084) | -2.768<br>(4.127) |
| Pain Reliever Misuse    | -0.005<br>(0.014)                              | -0.011<br>(0.014) | 0.017<br>(0.019)  | -0.150<br>(0.696) |
| Region-Time Dummies?    | No   | Yes               | No                | No                |
| OLS/Poisson             | OLS  | OLS               | Poisson           | OLS               |
| Outcome                 | Log  | Log               | Total             | Level             |

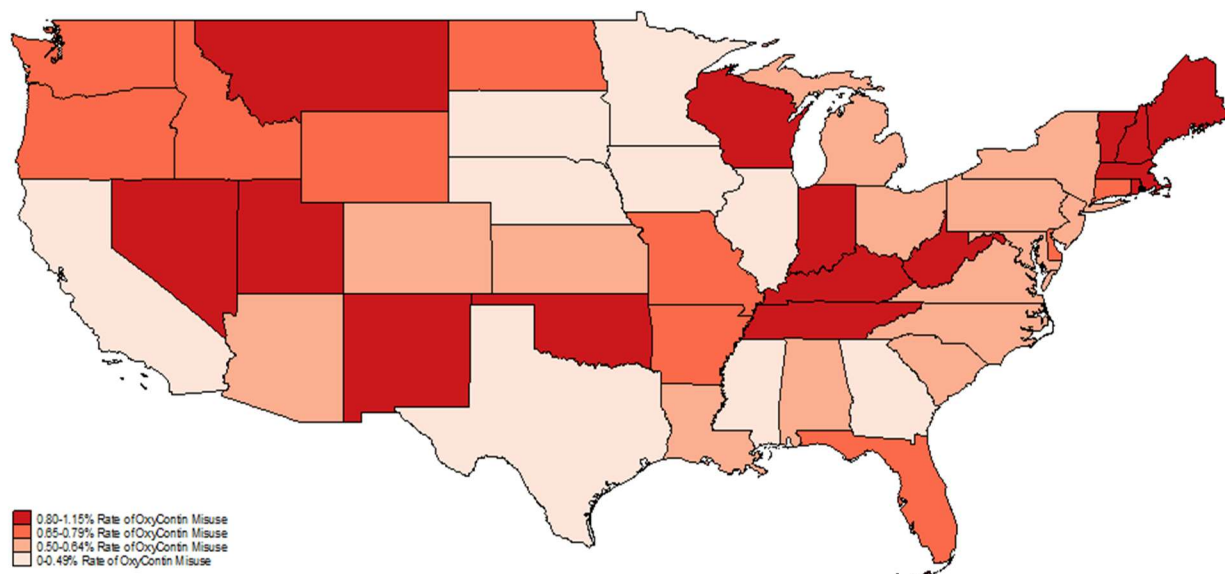
| <b>Panel D Outcome:</b> | <b>% Ages 18-64 Receiving Disability Benefits</b> |                    |                    |                   |
|-------------------------|---|--------------------|--------------------|-------------------|
|                         | (1)   | (2)                | (3)                | (4)               |
| OxyContin Misuse        | 0.042*<br>(0.022)                                 | 0.058**<br>(0.024) | 0.042*<br>(0.022)  | 0.004*<br>(0.002) |
| Pain Reliever Misuse    | 0.013*<br>(0.007)                                 | 0.007<br>(0.006)   | 0.018**<br>(0.008) | 0.001*<br>(0.000) |
| Region-Time Dummies?    | No  | Yes                | No                 | No                |
| OLS/Poisson             | OLS   | OLS                | Poisson            | OLS               |
| Outcome                 | Log   | Log                | Total              | Level             |

*Notes:* \*\*\*1% significance, \*\*5% significance, \*10% significance. All models include state and time fixed effects plus covariates mentioned in notes of Figure 4. Estimates presented are average effects for each pre-reformulation misuse variable using equation (2). Region refers to the 4 Census regions. When Poisson estimation is used, the outcome is the total and population (ages 18-64) is used as an exposure variable (for Panel C, the exposure variable is the number of applications). N=765.

# Appendix

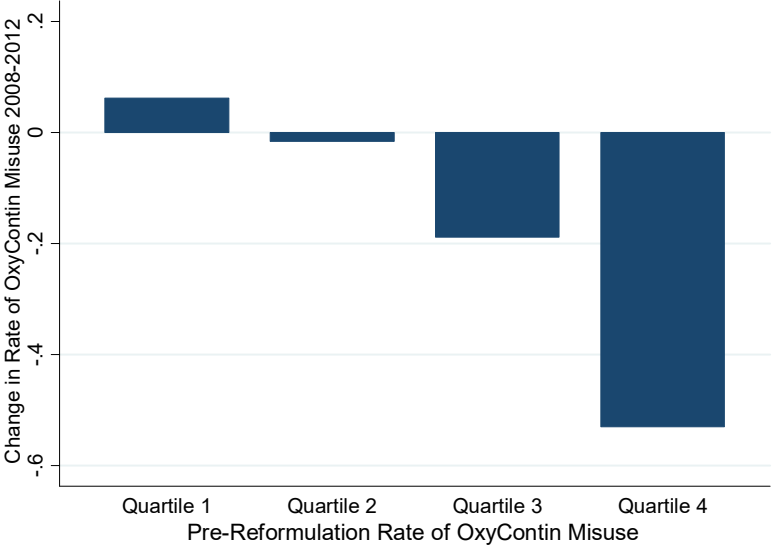
## Appendix Figures

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



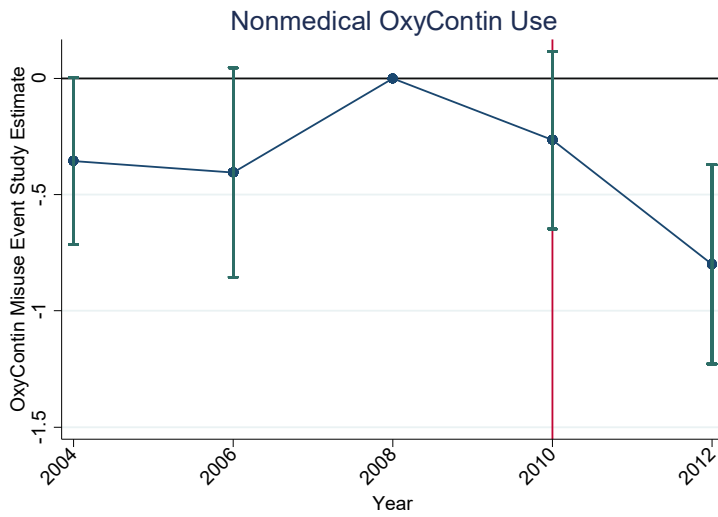
Source: 2004-2009 National Survey of Drug Use and Health

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



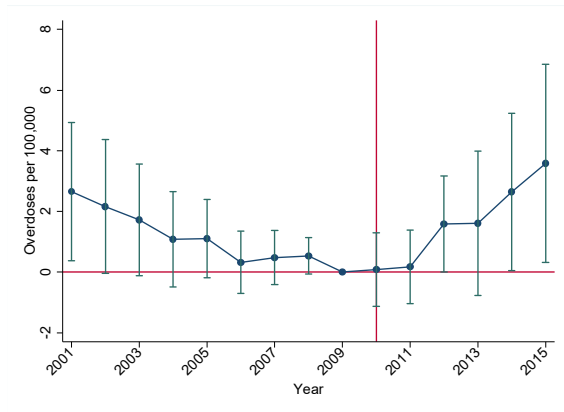
*Notes:* Quartiles represent states with the highest and lowest pre-reformulation rates of OxyContin misuse (Quartile 4 includes the 25% of states with the highest pre-reformulation rates of OxyContin misuse). The change in the rate of OxyContin misuse is weighted by state population.

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

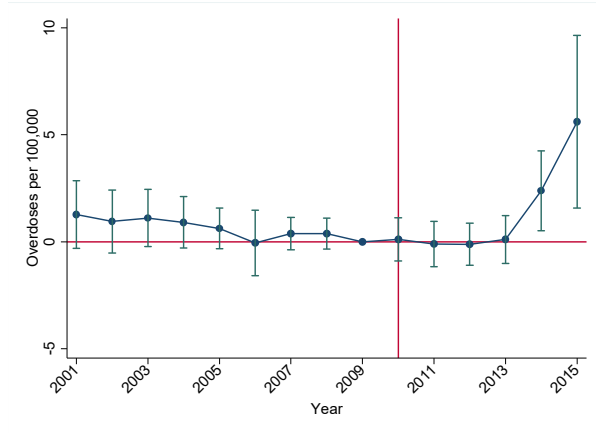


Source: Alpert et al. (2018)

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.



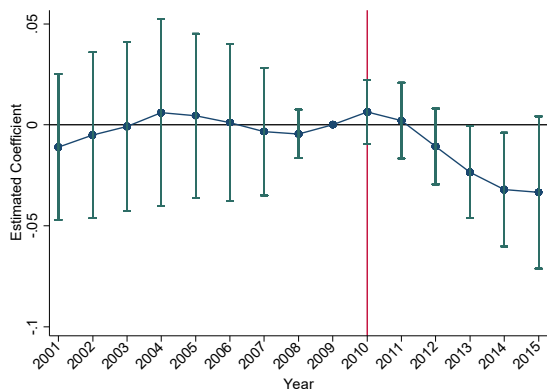
**A. Heroin Overdoses per 100,000**



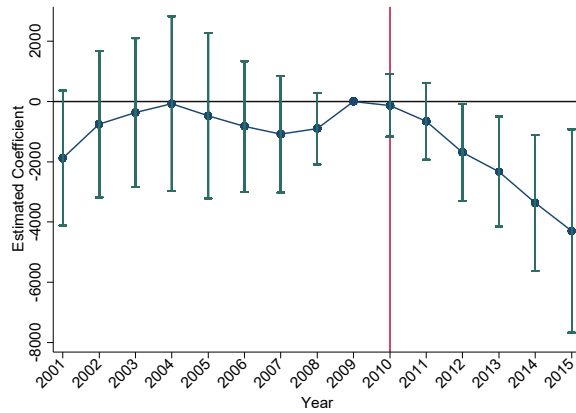
**B. Synthetic Opioid Overdoses per 100,000**

**Appendix Figure 4: Non-Medical OxyContin Misuse Event Study Estimates for Overdose Outcomes**

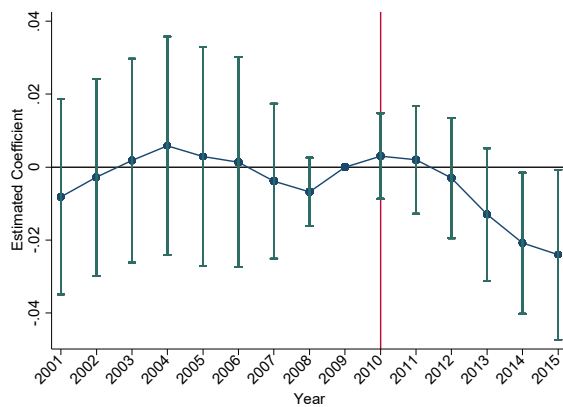
*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is heroin overdoses per 100,000 (Panel A) and synthetic opioid overdoses per 100,000 (Panel B). Both outcomes are calculated using the geocoded National Vital Statistics System. The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar 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 time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



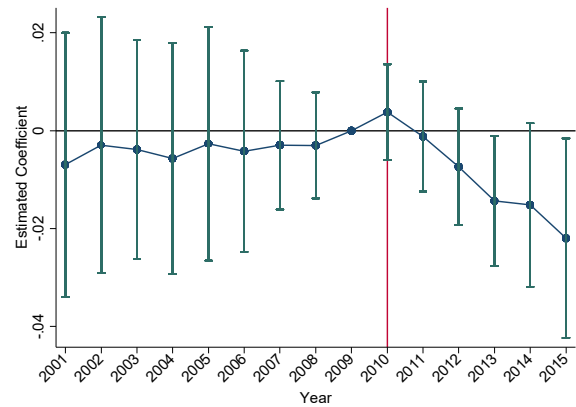
**A. Per Capita Employment (BEA)**



**B. Per Capita Employee Compensation**



**C. Per Capita Employment (CES)**

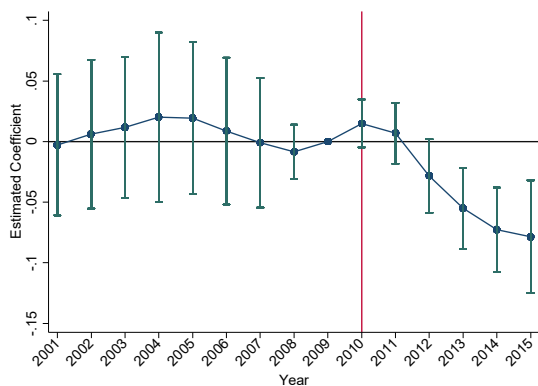


**D. % Worked (ACS)**

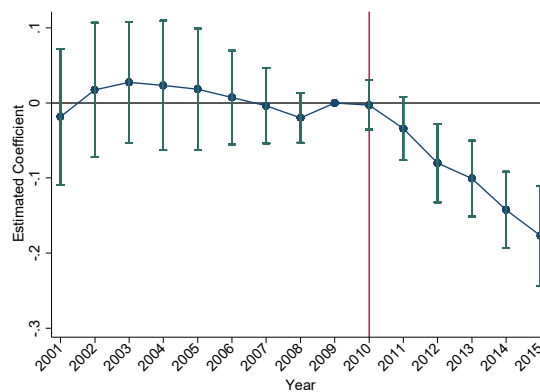
**Appendix Figure 5: Non-Medical OxyContin Misuse Event Study Estimates for Labor Supply, Estimated in Levels**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is total employment scaled by the size of the 16+ population (Panel A) and total employee compensation divided by the 16+ population (Panel B). These outcomes are calculated using data from the Bureau of Economic Analysis. The outcome in Panel C is nonfarm employment divided by the 16+ population using CES data. The outcome in Panel D is the percentage of the 16+ population in the ACS reporting that they worked during the year.

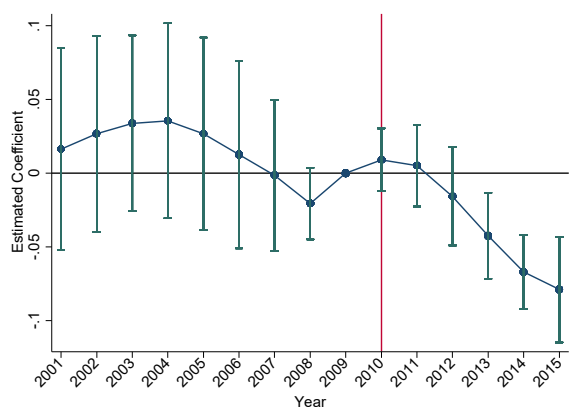
The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



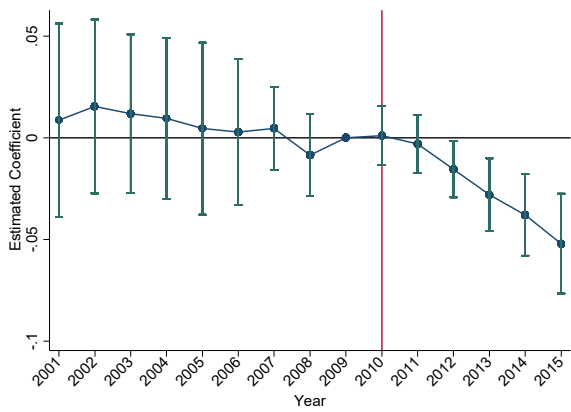
**A. Employment (BEA)**



**B. Employee Compensation**



**C. Employment (CES)**

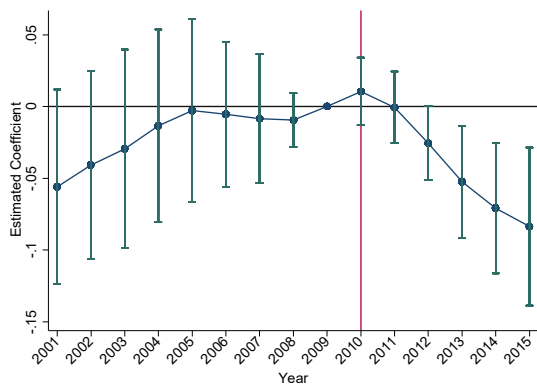


**D. Worked (ACS)**

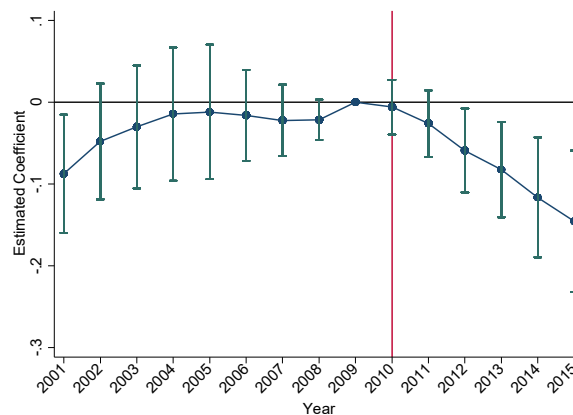
**Appendix Figure 6: Non-Medical OxyContin Misuse Poisson Event Study Estimates for Labor Supply**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is total employment (Panel A) and total employee compensation (Panel B). These outcomes are calculated using data from the Bureau of Economic Analysis. The outcome in Panel C is nonfarm employment using CES data. The outcome in Panel D is the number of the 16+ population in the ACS reporting that they worked during the year. We use Poisson regression with population (16+) as the exposure variable.

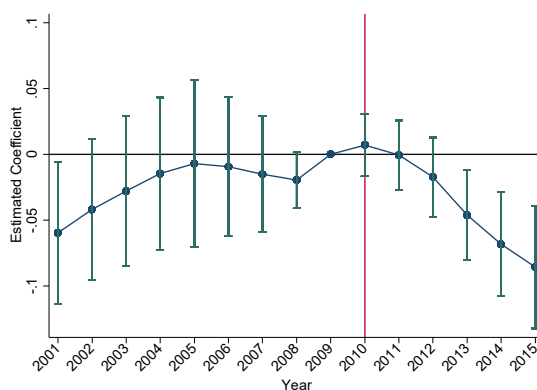
The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



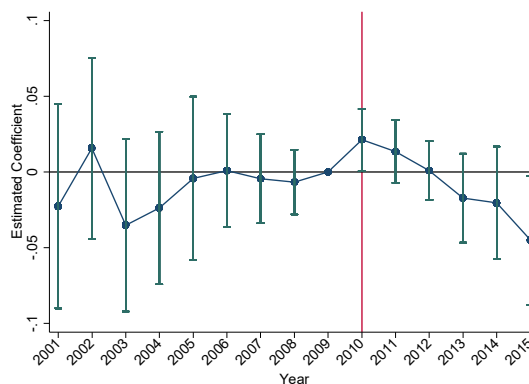
**A. Employment (BEA)**



**B. Employee Compensation**



**C. Employment (CES)**



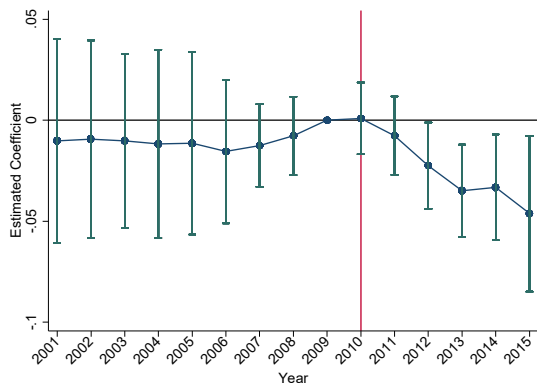
**D. Worked (ACS)**

**Appendix Figure 7: Non-Medical OxyContin Misuse Event Study Estimates for Labor Supply, Not Scaled by Population**

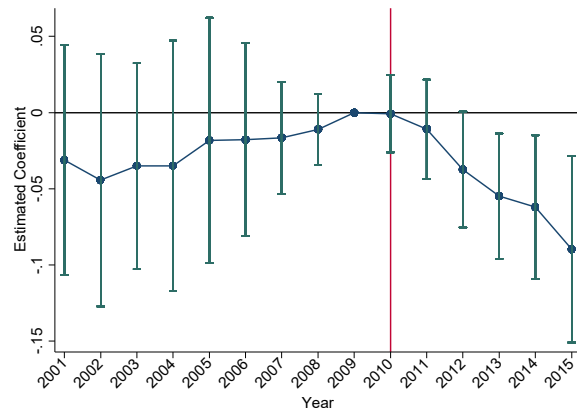
*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of total employment (Panel A) and the log of total employee compensation (Panel B). These outcomes are calculated using data from the Bureau of Economic Analysis. The outcome in Panel C is the log of nonfarm employment using CES data. The outcome in Panel D is the log of the number of the 16+ population in the ACS reporting that they worked during the year.

The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar 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 time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.





**A. Usual Hours Worked**

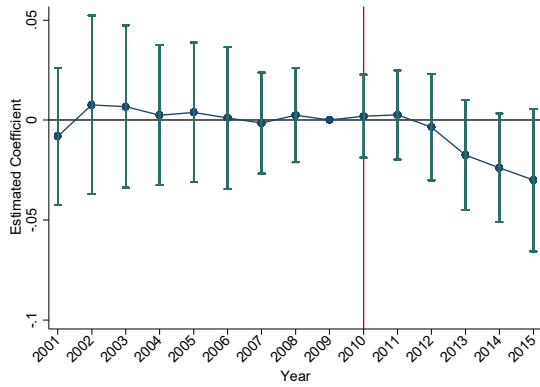


**B. Annual Earnings**

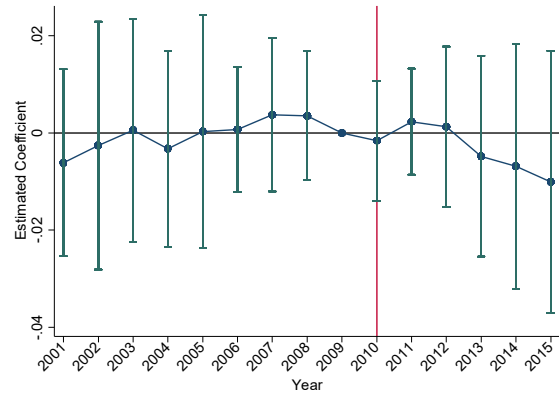
### Appendix Figure 8: Non-Medical OxyContin Misuse Event Study Estimates for Annual ACS Labor Outcomes

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of the usual weekly hours worked per person in the last year (Panel A) and log of total earnings in the last year (Panel B). Outcomes are calculated using the ACS for the 16+ population.

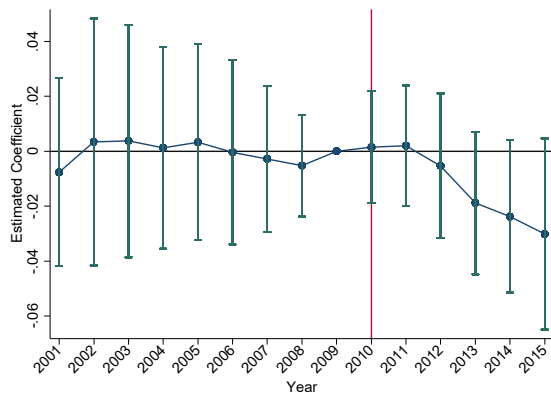
The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



**A. % Worked Last Week**



**B. % in Labor Force**

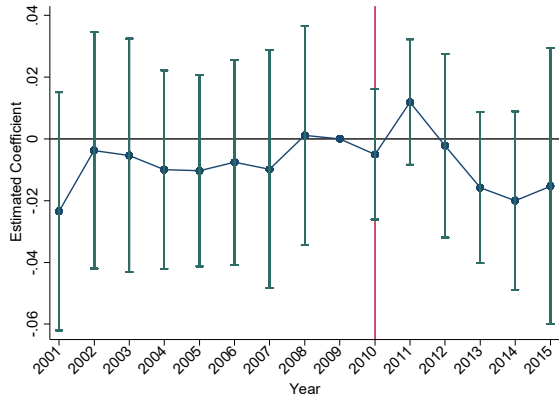


**C. % Employed Last Week**

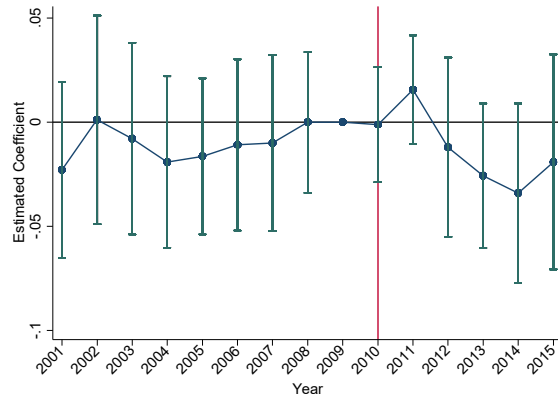
**Appendix Figure 9: Non-Medical OxyContin Misuse Event Study Estimates for ACS Weekly Labor Supply Measures**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of the percentage working in the last week (Panel A), log of the percentage in the labor force last week (Panel B), and the log of the percentage employed in the last week (Panel C). Outcomes are calculated using the ACS and refer to the 16+ population.

The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar 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 time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



**A. % Worked Last Week**

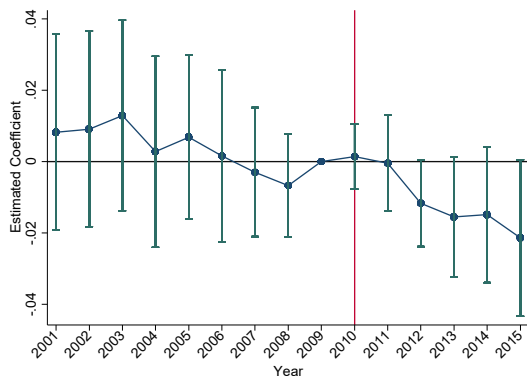


**B. Hours Worked Last Week**

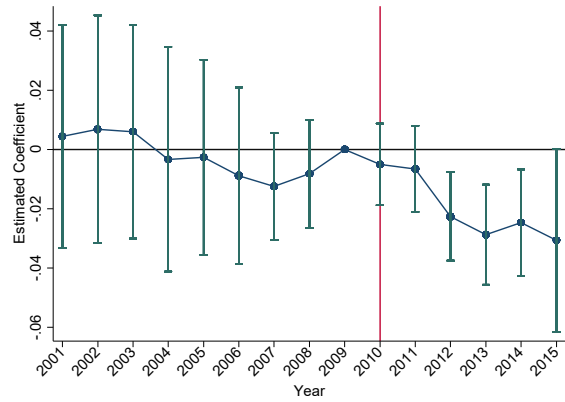
**Appendix Figure 10: Non-Medical OxyContin Misuse Event Study Estimates for CPS Labor Supply**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of the percentage working in the last week (Panel A) and log of hours worked last week (Panel B). Outcomes are calculated using the CPS and refer to the 16+ population.

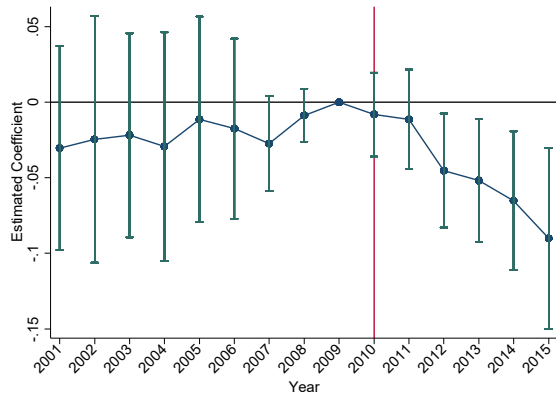
The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar year indicators. The 2009 interaction is excluded and the corresponding estimate is normalized to 0. The estimated specification is represented by equation (1). The specification includes state and time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.



**A. % Worked Last Year**



**B. Usual Hours**



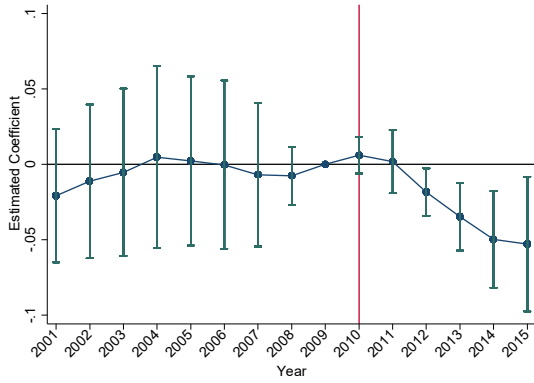
**C. Labor Earnings**

**Appendix Figure 11: Non-Medical OxyContin Misuse Event Study Estimates for ACS Annual Labor Supply, Ages 18-64**

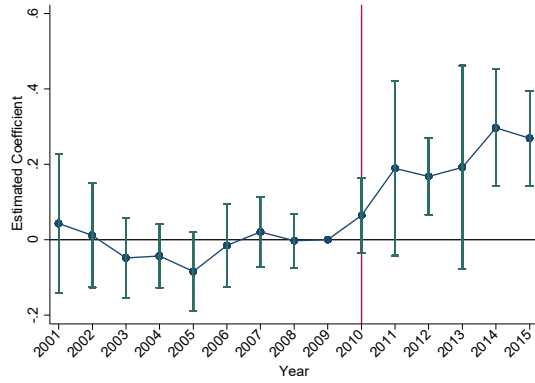
*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of the percentage working last year (Panel A), the log of usual hours worked (Panel B), and the log of total earnings in the last year (Panel B). Outcomes are calculated using the ACS for the 18-64 population.

The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar 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 time fixed effects as well as time-varying controls: five age share variables, % white and non-Hispanic, % Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries. We also jointly estimate effects for pain reliever misuse interacted with year indicators.

## Lasso Results

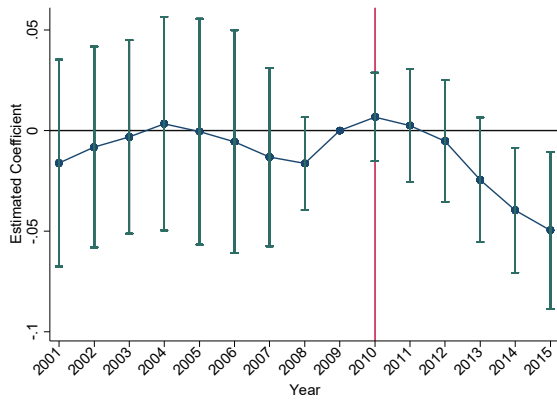


A. BEA Employment

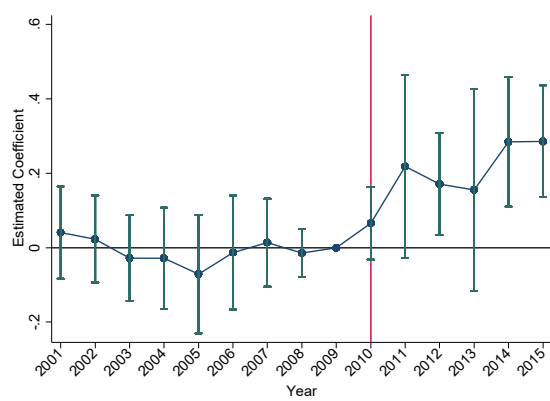


B. Disability Applications

## All Time-Varying Controls + Bartik Prediction



C. BEA Employment

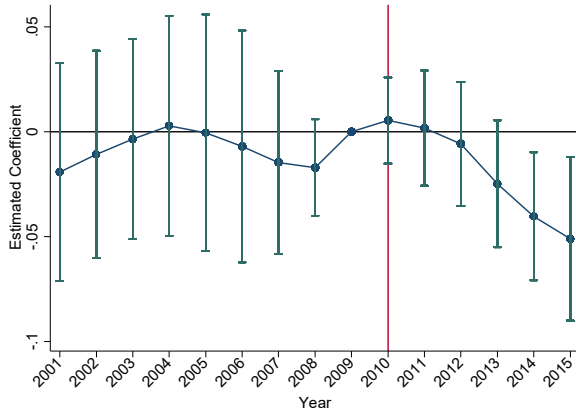


D. Disability Applications

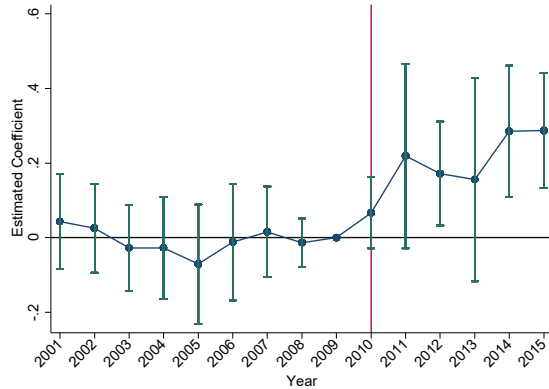
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**Add Bartik Manufacturing Prediction**



**E. BEA Employment**



**F. Disability Applications**

**Appendix Figure 12: Non-Medical OxyContin Misuse Event Study Estimates for BEA Employment Rate and Percentage Applying for Disability Insurance, Robustness Tests**

*Notes:* 95% confidence intervals adjusted for state-level clustering. Outcome is the log of the employment-to-population ratio (Panels A, C, E) and the log of the percentage applying for disability insurance (Panels B, D, F). Outcomes are calculated using the BEA for ages 16+ and SSA Fiscal Year Disability Claim data set for ages 18-64.

The estimates reported in the figures are the coefficients on the pre-reformulation non-medical OxyContin misuse rate interacted with calendar 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 time fixed effects. We also jointly estimate effects for pain reliever misuse interacted with year indicators.

In Panels A and B, we use rigorous square root lasso to select additional predictors. The possible set of predictors include five age share variables, % white and non-Hispanic, % Hispanic, % white, % black, % black and non-Hispanic, four education share variables, % foreign-born, and policy variables. Our policy variables are whether the state has a “must access” PDMP, pain clinic regulations, and legal and operational medical marijuana dispensaries.

In Panels C and D, we rely on the standard set of covariates for this paper (see Appendix Figure 11 notes). We also add a Bartik prediction as described in the text. In Panels E and F, we add a Bartik prediction specific to manufacturing to permit manufacturing labor demand shocks to have independent effects on state labor/disability outcomes.